

Perspektiven der Anomalistik 4

Legitimacy of Unbelief

The Collected Papers of Piet Hein Hoebens

Gerd H. Hövelmann, Hans Michels (Eds.)

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Legitimacy of Unbelief

Perspektiven der Anomalistik

herausgegeben von

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**im Auftrag der
Gesellschaft für Anomalistik**

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Gerd H. Hövelmann & Hans Michels (Eds.)

Legitimacy of Unbelief

The Collected Papers of Piet Hein Hoebens

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In memory of P. H. Hoebens'
and the Editors' late friends,

Dr. John Beloff
(1920-2006)

Prof. Dr. Martin U. Johnson
(1930-2011)

Prof. Dr. Robert L. Morris
(1942-2004)

Prof. Dr. Marcello Truzzi
(1935-2003)



Piet Hein Hoebens (1948-1984), ca. 1983.

(courtesy of Lodewijk Hoebens)

CONTENTS

1	PREFACE — Gerd H. Hövelmann and Hans Michels	9
2	SKEPTICISM – THEORY AND PRACTICE	23
2-01	The Legitimacy of Unbelief	25
2-02	Time Machines, the Hume Game and a Successful Replication of a Classic ESP Experiment	39
2-03	Wrong Question!	55
2-04	In Praise of Meticulousness in Parapsychology	61
2-05	Is Interest in the Paranormal an Atavism?.....	69
2-06	The Ruthless Critics of Parapsychology	77
2-07	Parapsychology and Skepticism	81
2-08	How I was Debunked	89
2-09	Hello ..., Do You Read Me?	93
2-10	Professor Johnson’s Battle Against “Para-pornography”	97
2-11	Sense and Nonsense in Parapsychology	103
2-12	Reflections on Psychic Sleuths [with <i>Marcello Truzzi</i>].....	115
2-13	Parapsychologists, Critics Agree to Consensus Statement [as told to <i>Kendrick Frazier</i>]	131
2-14	Fringe Medicine and the Parapsychologist’s Responsibility	135
2-15	Fraud and Self-Deception in Parapsychology	147
3	RESEARCH AND DEBUNKING	165
3-01	The Mystery Men from Holland, I: Peter Hurkos’ Dutch Cases	167
3-02	The Mystery Men from Holland, II: The Strange Case of Gerard Croiset	177
3-03	The Mystery Men from Holland, III: The Man Whose Passport Says Clairvoyant	191
3-04	Gerard Croiset: Investigation of the Mozart of “Psychic Sleuths”	203
3-05	Croiset and Professor Tenhaeff: Discrepancies in Claims of Clairvoyance	215
3-06	Croiset: The Psychic Who Never Disappointed	225
3-07	Comparison of Reports of the “Denver” Chair Test: A Critical Examination of the Methods of W. H. C. Tenhaeff	229
3-08	Croiset: Double Dutch?	243
3-09	The Arsonist and the Psychic – or How Professor Tenhaeff “Edited” a Police Statement	249
3-10	W. H. C. Tenhaeff – Delusions of Scholarly Grandeur	255

3-11	Farewell to Pirmasens: A Critical Re-Examination of a Successful ESP Experiment	261
3-12	They Know the Unknown: Treacherous Facts	291
4	COMMENTS AND CONTROVERSIES	297
4-01	Ancient Astronaut in a Cactus – An Interview with Erich von Däniken	299
4-02	Critical Committee Puts “The Paranormal” to the Test	309
4-03	Cambridge Centenary of Psychical Research: Critics Heard, Encouraged to Cooperate	317
4-04	Skeptics United	321
4-05	Necessary Distinctions	331
4-06	People in Glass Houses	333
4-07	Spook Professor for Utrecht?	341
4-08	Netherlands: Psychic Surpluses and a Call for International Cooperation	347
4-09	“I Was Somebody Else” (Postscript: Stevenson Raises More Questions than He Answers, by C. T. K. Chari)	353
4-10	Some Further Reflections on the Mars Effect Affair	363
4-11	Speaking Animals Keep Scholars Talking	369
4-12	Book Review: <i>Para, Psi und Pseudo</i>	377
4-13	Book Review: <i>Flim-Flam!</i>	383
4-14	The Modern Revival of “Nostradamitis” (Addendum: Did Nostradamus Predict the sTARBABY Controversy?)	389
4-15	A Guide to Press Coverage of the Paranormal	401
4-16	Book Review: The Truth About Astrology	405
5	INSTEAD OF A POSTSCRIPT	409
	Singing Between the Stools – Personal Recollections of Piet Hein Hoebens, by <i>Eberhard Bauer</i>	
6	BIBLIOGRAPHIES	417
7	ABOUT THE EDITORS	457
8	NAME INDEX	459

PREFACE

Habent sua fata libelli – books have their own destinies.¹ This, arguably, is true for most or all books. However, as will become apparent, this old phrase appears to be true almost by definition for the book you are currently holding in your hands; it seems to be a particularly apt description of the history of this book.

Whereas, as we have painfully learnt, the publication on an academic level eventually may take incredibly longer than its authors or editors ever considered possible, or even imaginable, such long delays by no means are exceedingly rare events in academic circles, in any country – for other pertinent examples, one of them also from the Netherlands, see Feininger (1973) and Verster (1989). Even Charles Darwin was guilty of this sin (if a sin it was)² – the point being that some such delays probably were intentional and strategic, as in Darwin's case, while others just happened and continue to happen against their authors' or editors' better intentions.

Getting Involved

During the last 13 years of his lamentably short life, Piet Hein Hoebens (who died in October of 1984 at the age of 36) was a journalist and editorial writer with the widely circulating and strictly conservative Dutch daily newspaper, *De Telegraaf*.

In the late nineteen-seventies, Hoebens became first interested in and then quickly involved with the world of parapsychology and various other areas of what today we tend to describe as anomalistics and related skepticism. Within only a few years, he became an enthusiastic, extremely well-informed and well-known investigative author, reporting on the methodological quality, or the lack of it, of research into alleged paranormal phenomena. Within a matter of months, Hoebens not only had become the Dutch representative of the Committee for Scientific Exploration (CSICOP, cleverly renamed CSI about a decade ago), but also was on speaking terms and conducted an enormous cor-

1 This is a fairly popular (if notoriously incomplete and slightly mistranslated) quotation from the treatise *De litteris, de syllabis, de metris* by *Terentianus Maurus*, a late second-century Latin grammarian (Beck, 1993, verse 1286). For the curious history and the habitual misapplication of this phrase, see Milde (1988).

2 The latter was likely so in the case of Darwin, because he strategically delayed the publication of his *On the Origin of Species* for 22 years; it had been completed by the early fall of 1837, but was not published until 1859 (see Richards, 1983).

respondence with just about everyone, important or unimportant, who had even a feeble voice in the continuing international debates between the parapsychologists (so-called) and the skeptics (so-called).

In the *De Telegraaf* newspaper, Hoebens' involvement resulted in a regular column³ with a large variety of sometimes witty and sometimes harshly critical stories, unraveling yet another amazing paranormal craze or nailing down popular charlatans. That column also did not fail to include articles pointing out that there was proper research into such phenomena as well (see the virtually complete three-part bibliography of Hoebens' scientific and relevant journalistic writings at the tail end of this volume). In his newspaper column, that commenced in 1975, Hoebens used his clear journalistic writing style to make the points that he felt needed to be made on a given political, social or scientific issue. Very soon, however, Hoebens put his many talents to more serious use in publications that he wrote for and were quickly accepted by periodicals with a much higher level of aspiration and that, mostly, were more strictly scientific. These journals (and a little later a number of scientific anthologies) had either skeptical or anomalistic resp. parapsychological backgrounds or, in a few cases, were epistemologically neutral such as Marcello Truzzi's *Zetetic Scholar*. For a good general survey of Hoebens' publications, please take a careful look at the extensive bibliography in the final section of this book. In his contributions to various periodicals and magazines, as well as in various book chapters, Hoebens reported, in minute detail, on his investigations and explained why his findings were clear indications for methodologically unacceptable or even fraudulent activities in research done by some parapsychologists. Even more important in a long-term view were quite a number of theoretical articles and book chapters on the sociology of science, the repeatability issue, on questionable research practices, and on many other issues that kept us busy in the 1980s and are still closely in the focus of the field in our time. At the same time, however, he openly and frequently defended "the parapsychologists" against his nominal skeptical allies, "the critics", as long as, in his estimate, the former carried out research in responsible ways according to generally accepted scientific standards. The field of parapsychology still needs and deserves as much closely attentive, effective, balanced, open-minded and well-informed critical company as it did a few decades ago. This is what this book is about. And this is what makes Hoebens' work and clear thinking, his willingness to listen and his critical acumen permanently valuable. Much of his writing is as pertinent to the current situation as it was a few decades ago.

Almost unknown is that Hoebens, as a card-carrying skeptic and Dutch CSICOP representative, more than once organized substantial support, even collective support

3 The *De Telegraaf* column carried the sanguine title *Er is meer* [There is more].

and defense, from leading self-described skeptics (such as Paul Kurtz, James Randi, Ray Hyman, and others) for endangered parapsychological projects at various institutions. He never made a big fuss about such initiatives, but those in the field of parapsychology who profited from or at least enjoyed such measures of support, will not forget. Not many of these activities have become the objects of public knowledge, neither at the time nor later, but they are on record in a couple of archives and will be remembered.

Hoebens' combat against dogmatic or extremist points of view within both the skeptical and the parapsychological "camps" earned him many friends as well as probably as many enemies and detractors. He often emphasized, either implicitly or explicitly, that both "camps" and the demarcation line separating them were obsolete and ripe to disappear. He considered them to be mere obstacles, preventing many people from cooperating instead of wasting their time on fiercely attacking one another. After all, the goal on either side of that line should be one and the same: scientifically sound research to discover solid facts, whatever these facts should turn out to be. His strong plea for mutual cooperation was a position that Piet Hein Hoebens repeatedly emphasized and that he shared with both editors and also with Eberhard Bauer who generously contributed a final chapter to this book.⁴

The Hoebens Files

Most of those who knew him will be aware that Hoebens passed from life on 22 October 1984, less than a month after his 36th birthday (on 29 September). Three days previously Hövelmann (GHH, henceforth) had received one of his typically extensive letters. After talking business for a while, the letter changes its tone: "[I]t is time for me to make some arrangements for what is going to happen to my property after my death. For several reasons (e.g. your trustworthiness, your meticulousness, your polyglottism etc.) I think that you are the most natural choice for a bequest of my extensive files on anomalistics, including my professional correspondence. [...] They contain a lot of valuable information, the exact value of which can only be appreciated by someone like you." Hoebens then goes on to specify some conditions for the bequest, and his antepenultimate sentence reads: "I am sorry for the somewhat macabre tone of this letter, but then I realize that my files are of considerable interest to the serious student of the [p]aranormal, and that I have a responsibility to do whatever I can in order to prevent them from getting lost literally [*sic*] or figuratively."

For more than the past three decades, the Hoebens Files (4.5 running meters; several thousand letters, thousands of pages of publication manuscripts plus a reprint collection)

4 For these shared opinions, see, for example, Hövelmann (1988) and Michels (1989, 1990) and compare these sources to major sections of the present book.

have been in GHH's possession. Access is deliberately limited, in accordance with Hoebens intention, but basically in Hövelmann's discretion. Thus, as long as 25 years ago, GHH granted full access to the voluminous letter and document collection on the so-called Gauquelin / CSICOP / Mars Effect Affair to Suitbert Ertel and to Jim Lippard for their respective astrology and sociology of science projects (for instance, see Ertel, 1992). These Files contain a breathtakingly extensive collection of correspondences between Hoebens and all major and quite a few minor players in that influential affair. Everybody even remotely involved in the "Mars Effect" controversy is represented with their correspondences. The same counts for James Randi's so-called Project Alpha and various related topics.

The Hoebens Files also contain enormous (and enormously important) correspondence collections including, for instance, correspondents such as James Randi, Martin Gardner, Ray Hyman, Richard Kammann, James Alcock, Kendrick Frazier, Mark Hansel, Paul Kurtz, Chris Scott, Susan Blackmore, and many others from skeptical quarters, and with nearly everybody in the Parapsychological Association, the Society for Psychical Research and the Society for Scientific Exploration, including, e.g., Stanley Krippner, Robert Morris, Eberhard Bauer, John Palmer (whom Hoebens knew pretty well from Palmer's time at the University of Utrecht), Richard Broughton, Martin Johnson and Sybo Schouten, to name but a few.

All these materials are stored in the archives of Hövelmann Communication, and, upon request, most of them are accessible to those with a *plausible scientific or historical interest* (such as Ertel and Lippard years ago in the case of the huge "Mars Effect" collection).

Also in the Hoebens Files, to mention just a couple of other examples, are the surviving documents from Filippus Brink's 1958 Utrecht University dissertation project on the role of clairvoyance in criminal trials in the Netherlands and worldwide. Brink, shortly before his death, had bequeathed these research data, questionnaires and other materials to Hoebens. Also of major interest are Hoebens' correspondences with Eric J. Dingwall, C. T. K. Chari and Denys Parsons. Both are of scientific and historical interest and may stimulate and feed future publications in the relevant literature. Many other, often voluminous, correspondences in the Hoebens Files are awaiting eventual discovery and exploration.

There is only one major part of the Hoebens Files that necessarily will be exempt from public or private inspection: This one is the enormous, almost daily correspondence – often between 10 or 15 neatly typewritten pages at a time (real old-fashioned letters on real paper) – between Hoebens and American sociologist Marcello Truzzi. These files, important as they are, are tantamount to being personal diaries of both Hoebens and Truzzi over almost a decade from 1975 to October of 1984. They can not and will not be made available.

Archaic Book Plan

As early as a couple of months after the untimely death of Piet Hein Hoebens in October of 1984, Michels (HM), GHH and our colleague Jeff C. Jacobs, then a leading engineer at Philips, had begun exchanging their initial ideas on what was to become a reality only 30+ years later.

As far as the editors are able to ascertain, Piet Hein Hoebens' first publications on paranormal phenomena date back to 1977 (see bibliographies). That means that he had no more than about seven years to build his "career" in the communities of "the parapsychologists" and "the critics." Since Michels met Hoebens in person for the first time in April 1980, and Hövelmann's first communication with the author dates back to March 1981, they were in touch and worked with Hoebens for only a relatively short period of time. They valued his versatile contributions to the field of parapsychology to such a degree that they were of the opinion that his writings should not only be preserved, but, more importantly, be made available to a larger (and younger) community. After all, a vast number of Piet Hein's publications were unavailable in English at the time of his death, but had appeared only in Dutch or German, some in French.

Hoebens in his personal farewell-letter and elsewhere used to describe his relevant archives as his "files". That is why, throughout this book, we will refer to his complete archival collection and correspondences, which are stored and provisionally archived in almost 50 huge Leitz folders plus various maps and boxes at the Hövelmann Communication Archives, as "*The Hoebens Files*".

In his last communication with Hövelmann, Hoebens, as described, had decided that, with a few minor exceptions, his complete, extraordinarily huge correspondences, photo copies, notes, unpublished essays and several handwritten chapters of a book he was working on, should be handed to Gerd Hövelmann. Hoebens also granted permission to share major contents of the Files with others, who in GHH's estimate, would be both competent and serious enough, for publication. This a couple of years later was confirmed by Hoebens' widow, Liesbeth Hoebens-Hoedemaeker, an internationally recognized music librarian. In addition to Hoebens manuscripts for publication, all these files and material served as voluminous, rich and highly relevant sources of information for this book.

Letters exchanged between the two editors in the early planning stage reveal that the first discussions on the compilation of an overview of all publications by Hoebens and the plan to publish a book on his contribution to parapsychology must have taken place

well before March 1985. In a letter to GHH dated March 5, 1985, Michels⁵ refers to earlier personal communication with Hövelmann on a publication plan by Michels, Jeff Jacobs, and Brian Millar, that had already been discussed among those individuals, and he invites GHH to come to Eindhoven to discuss those plans in more detail. In his return letter of two days later Hövelmann⁶ confirms that he also has a preliminary plan for a book and summarizes some activities he has already started. In the months to follow, the preliminary setup and contents were discussed, refined, and exchanged regularly, and several colleagues who also had collaborated with Hoebens were asked to send us their comments. Most of those colleagues allowed us to refer to them as wholehearted supporters once we were to get in touch with any publisher willing to fund the production and publication of a book on Hoebens. Among those who kindly offered their support were Marcello Truzzi, John Beloff, Robert Morris, Stanley Krippner, James Randi, Kendrick Frazier, Ivan Kelly, and Jim Alcock.

Then, in May 1986, GHH approached Paul Kurtz of Prometheus Books in Buffalo, New York. He summarized the planning, introduced its originators, and emphasized the importance of making many of Hoebens' non-English writings accessible to a much larger readership, because they contain new and forceful arguments, both skeptical and non-skeptical, that were not present in the English literature at that time. Initially, Kurtz replied that he would love to publish Hoebens' collected papers in book form but, at that moment, did not see sufficient market options to make up for the costs. In February 1987, Prometheus emphasized that it still considered the contents to be important enough, but that it nevertheless had to decide to put the publication of this book on the back burner, because of the commercial risks involved. On the back burner it remained for many years, without a final consent or offer from Prometheus.

To cut a long story short, eventually we were offered the opportunity to re-ignite the back burner. During the 2005 conference of the WGFP (Wissenschaftliche Gesellschaft zur Förderung der Parapsychologie) in Germany, our long-time colleague Wim Kramer (Secretary of the Board of "Het Johan Borgman Fonds" [HJBF]) mentioned that he would be able to arrange some funding for the publication of a book on parapsychology. We decided to accept Kramer's offer. We soon informed Piet Hein Hoebens' widow, his brother, and his closest personal friend, Pieter Nouwen, all of whom immediately commented enthusiastically to the "restart" after so many years, and offered us their assistance if needed. The small but helpful grant from the HJBF defrayed some of the expenses of traveling between Eindhoven and Marburg.

5 J. A. G. Michels, letter to Gerd H. Hövelmann, 6 March 1985.

6 Gerd H. Hövelmann, letter to J. A. G. Michels, 8 March 1985.

We clearly realized that a project such as this would undoubtedly force us to use much of our already hardly available spare time. First of all, unlike the end of the eighties, nowadays both editors were used to working on various projects longer than the more or less standard eight hours a day. Secondly, we would have to dig up a vast number of letters, plannings, manuscripts, and many other documents we had hardly ever looked at for almost 20 years. On the other hand, we did not have useful tools, not even e-mail services, back then. We completed a preliminary synopsis and a first selection of the papers we wanted to incorporate, checked which papers or manuscripts still needed translation by us or by native English translators, and we compiled a first overview of all additional texts we had to produce. We decided that we could do many tasks separately in our home towns Eindhoven and Marburg, and that several “get togethers” of almost a week each were necessary. Our planning worked out well, with the exception that our daily jobs prevented us from completing the project as a whole earlier than we did. However, we both have always adhered to the principle that quality should prevail over time. So we tried to stick to that principle.

The Structure of this Book

The present book includes a total of 43 chapters, with addenda to chapters 4–09 and 4–14, grouped in three major sections: (2) *Skepticism – Theory and Practice*, (3) *Research and Debunking*, and (4) *Comments and Controversies*. Only 21 of these 43 journal or newspaper articles, book chapters, shorter notes or book reviews had originally been published in English (those in chapters 2–02, 2–05, 2–07, 2–08, 2–10, 2–11, 2–12, 2–13, 3–01, 3–02, 3–03, 3–04, 3–05, 3–07, 3–08, 4–03, 4–05, 4–10, 4–12, 4–14, 4–15). They here are reprinted, with the permission of the original copyright holders other than Hoebens and his heirs. Of these chapters, the chapters 4–09 and 4–16 were initially published in, respectively, Dutch and German, but shortly afterwards also appeared in English translations.

Two papers (chapters 4–01 and the addendum to 4–14) were never published in any form or language before. They here are printed from the original, unpublished manuscripts found in the Hoebens Files. Moreover, the paper in chapter 3–02 was not formally published before; so far it only had been accessible (full-length) in the two-volume *Proceedings* of the 1982 joint Centenary Jubilee Conference of the Parapsychological Association and the Society for Psychical Research as well as, as an abstract, in *Research in Parapsychology 1982* (Roll *et al.*, 1983). Chapter 3–06 was first published in French, the chapters 2–06, 2–09, 2–10, 3–09, 3–10, 4–02, 4–06, 4–07, 4–08, 4–09 and 4–11 initially appeared in Dutch versions, chapter 4–04 in Flemish. Finally, the chapters 2–01, 2–03, 2–04, 2–14, 2–15, 3–11, 4–13, and 4–16 originally were published in German.

Each of the 43 chapters in this book will open with *Editorial Introductions*. They have a triple function: First, these introductions are supposed to properly locate the respective text in the historical context in which it was written. Second, it provides all relevant bibliographic information on the original publications. And third, the intros shall explain why the respective articles or comments etc. are of long-term relevance even three decades after their author passed away. In addition and in a few selected cases, chapters also may have *Editorial Postscripts*.

Fortunately, in addition to their other academic and non-academic occupations, both editors have been professional translators for decades. And in some doubtful cases, we were happy to rely on the advice of our colleague Robin Moore, a British native speaker of English who lives in the Netherlands.

All the papers in the remaining 19 chapters (that is, chapters 2–01, 2–03, 2–04, 2–06, 2–09, 2–10, 2–14, 2–15, 4–06, 4–11, 4–12, 4–02, 4–04, 4–06, 4–07, 4–09, 4–11, and 4–13) were never published in English before. In a considerable number of cases, Hoebens himself had provided full English translations or English draft versions before publication for private circulation, especially for the information of his friend Marcello Truzzi and a couple of others who did not read Dutch, German or French, but who Hoebens wanted to keep informed of his latest writings – an admirable additional effort and service for the information of friends and colleagues. In more than a few cases this has made our editorial work quite a bit easier. So Hoebens often wrote English versions of articles that were not even intended for publication in English periodicals or books. Often we had several manuscript variants to compare with the finally published text versions.

Thus, the contents of all papers in this book always were checked very carefully against the final manuscripts in the Hoebens Files. As indicated before, in several cases checks against two or more preliminary manuscript versions were necessary and helped to clarify critical issues.

One more factor made our work somewhat easier: In several cases and chapters, Hoebens had insisted that GHH would be commissioned to translate Hoebens' original book chapters manuscripts into German. In cases of doubt we decided to always stick to the version that comes closest to the manuscript. A few other English Hoebens manuscripts, especially some early ones, had been translated into German by Eberhard Bauer for publication in the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*. Since Bauer was able to provide original English manuscripts of the papers that he had translated into German, we were spared the thankless task of re-translating any texts. Also, for a number of additional articles that appear in this book papers were translated specifically for this volume by the editors, with the welcome and able assistance of Robin Moore. So now,

much in this book contains additional information and many fresh arguments on parapsychological phenomena and research that had not been available or accessible before to an Anglo-American readership. They now are available in English for the first time.

Hoebens' work to us always has been a model of responsible skepticism, even if some readers (and the editors, by the way) may not fully agree with every detail, every single position or every statement that we encounter in Hoebens' writings. To those, German parapsychologists in particular, who previously had been routinely confronted by the kind of subterranean skeptical response represented by Otto Prokop, Wolf Wimmer or Herbert Schäfer, Hoebens writings came as a cast of fortune.

To close (almost) with a remarkable coincidence (if that's the right term) that is contributed by the second editor, Hans Michels. Michels got interested in so-called paranormal phenomena when, in 1968, he met Jeff Jacobs, then a student in Eindhoven, while Michels studied at the University of Tilburg. Initially, their main topic of interest were the strange, but apparently meaningful coincidences that sometimes occurred in their daily lives. Together with some other friends, all students by then, they regularly met to discuss why and how those remarkable coincidences occur. These meetings gradually evolved into plans to find out more about their backgrounds by means of studying them in more detail. This led to the founding of a formal body (the "Studiegroep Synchroniciteit") in 1969, to study the type of phenomena labeled "synchronicities" by the psychologist-psychiatrist Carl Gustav Jung. This group was the precursor of the SRU (Synchronicity Research Unit), founded in 1975, a group that took up systematic research into a range of so-called paranormal phenomena and to which both editors still belong as "Bestuurders" (i. e., Members of the Board of Directors).

Michels met Piet Hein Hoebens for the first time when the latter was invited to take part in a forum discussion on "coincidence" in Eindhoven on April 29, 1980. Piet Hein was to be the skeptic counterpart of an SRU associate who was to represent the point of view of "the parapsychologists". That evening marked the beginning of a closer cooperation and, eventually, friendship.

Within a couple of months lively discussions (both face to face as well as by correspondence) had started. These dealt with such varying topics as the suspicious research methods of famous Prof. Tenhaeff and the questions of how to get rid of the demarcation line between "the parapsychologists" and "the skeptics" (if such exists) or how to improve experimental setups to exclude fraudulent manipulations.

In 1983 Jacobs and Michels benefited from Piet Hein Hoebens' journalistic skills and his contacts in the media. Piet Hein published an article⁷ in a weekly paper distributed

7 See Mulder, B. [*recte*: Hoebens, P.H.] (1983).

in the Dutch province of Utrecht, enabling them to find the many subjects they needed there to be able to start a lengthy, substantial experiment⁸.

Over the years there was an ongoing exchange of opinions and critique on peculiar activities in the field of parapsychology or specific people active in its community, but just as well on important positive developments in research or attempts of theory building.

One of the activities for the preparation of this book were very time consuming searches through the large number of documents, letters and scribbled notes in Piet Hein's handwriting (notoriously difficult to decipher⁹), browsing through lots of things we were not searching for at that very moment. During one of those sessions Michels found a letter in which Piet Hein Hoebens accepted an invitation for a lecture in Michels' home town of Eindhoven. Nothing exceptional in itself, of course. In this case, it was a bit of a shock for Michels, however, since Piet Hein's letter (of June 10, 1981) was addressed to Michels' father, who had suddenly died on October 2, 1984, only 20 days before Piet Hein's death. It was a very strange experience to be confronted with that letter some 22 years later. Hans Michels had never known about any correspondence whatsoever between his father and Piet Hein Hoebens, and he never mentioned the name Hoebens to his father.

Acknowledgments

First and foremost we are very much indebted to Piet Hein's wife Liesbeth Hoebens-Hoedemaeker for her persistent support, and for her angelic patience ever since the editors, in early 1985, took the very first initiatives to publish a book on her husband Piet Hein, only a couple of months after his untimely death.

We are equally grateful to Piet Hein's siblings, his sister Henriëtte and his brother Lodewijk, for supplying much additional information, and photographs, and for their unremitting support.

The late Pieter Nouwen, a Dutch journalist, novelist and Piet Hein's friend since their adolescent years, and his wife Tine, provided enthusiastic support of our undertaking. Sadly, Pieter did not live to see the completion of the book, which he strongly believed in and endorsed.

The publication of this book greatly profited from a grant from our colleague drs. Wim Kramer, Secretary of the Board of the philanthropic Het Johan Borgman Fonds

8 See Jacobs, J. C. (1987).

9 See our 'Editorial Introduction' to Hoebens' joint chapter, with Marcello Truzzi, in Paul Kurtz's *Skeptic's Handbook of Parapsychology*, below.

(HJBF), Bunnik, and Kramer's fellow Board members, H. Verbrugh and H. Berends. Also we thank the Board of the Synchronicity Research Unit (SRU) in Eindhoven for their handling of this financial grant. René Paré's professional assistance in the design of the artwork, Robin Moore's skilled translations from Dutch into English (and, in several cases, his "last minute helpdesk assistance") also are much appreciated, as is the support of Brian Millar, a chemist and parapsychologist originating from Scotland, for his assistance in translations in the earliest phases of the project.

In addition, we are grateful to Eberhard Bauer (IGPP, Freiburg, Germany) for his assistance in tracing and supplying missing data details on several documents and, in particular, for contributing the final chapter to this book, his personal reminiscences and appreciation of Piet Hein Hoebens.

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PREFACE

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CHAPTER 2

Skepticism – Theory and Practice



CHAPTER 2-01

Editorial Introduction

This opening essay may be considered Hoebens' classical position statement justifying, in basically Lakatosian terms, an intuitively skeptical explanatory model as a rational approach to the reported empirical findings and the manifest theoretical problems of parapsychology. Hoebens discusses in some detail the role that both the fraud hypothesis and the repeatability problem (and their intricate relationship) take in the cautious skeptic's argumentation. He further elaborates on these questions in several later articles, also included in this book.

The paper originally appeared, in 1982, under the programmatic German title "Die Legitimität des Unglaubens" (transl. Eberhard Bauer), in the Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie (vol. 24, pp. 61-73), and it was reprinted, without the abstract, in a Festschrift for Hans Bender (Bauer & Lucadou, 1983, pp. 118-130) in the following year. The version below is based on Hoebens' final English manuscript that was counter-checked with the published German version for possible late revisions. It is published in English for the first time. (Eds.)

THE LEGITIMACY OF UNBELIEF

Abstract

Many parapsychologists profess to be genuinely puzzled by the fact that skeptics – even well-informed skeptics – refuse to be overwhelmed by the evidence for psi. There is, however, no unambiguous evidence in parapsychology. The data suggestive of a paranormal factor are also suggestive of alternative explanations in terms of error, artifact or even fraud. It depends on the "explanatory model" the observer prefers. It is difficult to decide post factum, which explanation is the "true" one for any given claim. What we need is an explanatory model that not only accounts for the known facts, but also is successful in anticipating novel facts. Parapsychology has as yet failed to transform its intuitive belief into something in the nature of a "progressive" Lakatosian research programme – which would require a satisfactory solution of the "replicability problem," prematurely pronounced dead by Timm. So the skeptical option remains a valid one, especially as the checkered history of psychical research provides several examples of what may be regarded as striking confirmations of skeptical predictions. The unbeliever is urged to stick to his guns – although he should beware of overstating his position.

In spite of numerous encyclicals issued by leading practitioners of the parapsychological craft and solemnly asserting the reality of the paranormal, a considerable number of scientists and informed non-scientists stubbornly continue to question the existence of telepathy, clairvoyance, psychokinetic spoon bending and similar oddities.

The phenomenon of skepticism has long intrigued parapsychologists, especially those who have pronounced their own conviction that psi is real to be unshakable. Perhaps Professor Bender was merely being polite when he recently informed me that he was genuinely puzzled by my apparent inability to share his belief.¹ Some of his published writings suggest that he has long ago solved the puzzle – at least to his own satisfaction. I suspect that he agrees with many of his colleagues who account for the skeptical presence by attributing to the skeptic an emotional attachment to a “materialist” world view and a depth-psychologically motivated unwillingness to come to terms with new realities.²

Undoubtedly there is an element of “emotional” resistance in the typical unbeliever’s philosophy.³ Does this disqualify skepticism? In this article I will argue that, to a certain extent, a pigheaded refusal to embrace the creed (even after perusal of the scriptures) can be a perfectly rational and legitimate reaction to the various claims of the paranormal.

“Facts” and Explanations

Isolated “facts” prove little or nothing: They acquire their evidential value from the context in which they are observed, and this, in turn, depends on the explanatory thinking model employed by the observer. Each model has its (implicit) a prioris – points of reference, fundamental assumptions which we select as the yardsticks with which we measure “reality.” A different choice of a priori assumptions will result in a “fact” being interpreted differently.

In theory, the number of models that fit “reality” is practically unlimited, as any concept could in principle be singled out as the unquestionable standard to which all information is compared. In practice, however, there exist useful rules-of-thumb as to the “objective” value of the myriad possible models.

In general, those models are held to be superior that do not only explain – as parsimo-

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- 1 Personal communication from Professor Bender, letter to the author, August 28, 1981. [The letter is preserved in the Hoebens Files; (Eds.)]
 - 2 Bender, H. (1981b). Pirmasens 1953, Retrospektive auf ein Platzexperiment mit Gerard Croiset. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 23, 219-230.
 - 3 I seem to have noticed an element of “emotion” in the position of certain parapsychologists too.

niously as possible – “reality” as it appears today, but that are also consistently successful in anticipating future developments.

The problem with parapsychology – and the reason that the pro and con debate often resembles a shouting match between pots and kettles – seems to be that the available data are compatible with two mutually incompatible explanatory models, both of which have a strong *prima facie* appeal, but neither of which has as yet established a decisive lead over its rival in terms of predictive power.

There are *reports* of occurrences which, according to most parapsychologists, point to the existence of a factor “psi” for which current scientific orthodoxy cannot account. Critics, however, point to the alternative explanation for the same data: “psi” may simply be the result of some flaw in the process of reporting and interpreting.

Whatever his Freudian motives, the skeptic has chosen to explore the possibilities of the model which is based on the initial assumption that psi is non-existent. When confronted with a “paranormal” claim he will raise the possibility of a “naturalistic” explanation. He will further use his initial assumption for formulating predictions as to the future development of the research.

If I may apply – somewhat carelessly – the methodological terminology of Imre Lakatos⁴ to the present debate: The skeptic works within the framework of a “research programme” the “hard core” of which consists of initial disbelief and the “protective belt of auxiliary hypotheses” of which consists of various strategies to shield this hard core from premature “falsification.” Lakatosian ethics permit him to defend his position tenaciously as long as his own “programme” continues to anticipate novel developments and the rival “programme” (parapsychology of the believers’ variety) remains unable to transform its conjectures into an explanatory system of vastly superior predictive power.

A Prioris

Several authors, notably Collins and Pinch⁵, have criticized the typical skeptical position for being aprioristic and unfalsifiable. “This type of criticism,” Collins and Pinch write

4 Lakatos, I. (1978). The methodology of scientific research programmes. In: Worrall, J., & Currie, G. (eds.), *Philosophical Papers. Vol. 1* (pp. 8-101). Cambridge: Cambridge University Press.

5 Collins, H., & Pinch, T. (1979). The construction of the paranormal: Nothing unscientific is happening. In Wallis, R. (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge* (pp. 237-270). Keele, Staffordshire: University of Keele. In the relevant section, entitled “The Tactics of the Critics,” Collins and Pinch quote a statement from Hansel as an example of dogmatic apriorism. Their quote, however, is spurious.

disapprovingly, “[...] draws its weight from the ‘ethnocentrism of now’ – nothing is true which conflicts with what it now knows.”

I can assure the reader that many skeptics are sufficiently sophisticated to understand that Nature is under no obligation to conform to what is deemed possible by the Vorsitzender Richter am Landgericht in Mannheim [Presiding Judge, District Court of Mannheim],⁶ and that many claims which conflict with today’s “knowledge” may yet be perfectly true.

I do not deny that most skeptics are suspicious of psi because it does not square with orthodox scientific theory. However, the sophisticated skeptic will regard the a priori certainty derived from orthodox “knowledge” merely as a *methodological instrument*. Psi is impossible within the framework of a thinking model which itself is *fallibilistic*. The sophisticated skeptic *assumes* that Nature does not allow psi, but acknowledges that the model based on this assumption may eventually become obsolete.

Surprisingly, Collins and Pinch seem not to realize that an aprioristic element is unavoidable in any form of thinking. Even more surprisingly, they seem to have overlooked the striking fact that skeptical “apriorism” is nicely mirrored by a different sort of “apriorism” on the part of the typical proponent of psi. Proponents tacitly assume that we know enough about human nature to be able confidently to reject certain “naturalistic” counter-explanations on the ground that these are incompatible with this psychological insight.

The highly significant results of the experiments by Dr. X., it is said, *cannot* be explained by the assumption that fraud took place because Dr. X. is a reliable man and because there were multiple witnesses all of whom hardly less reliable than Dr. X. The implication is obvious, if not to writers of pro-psi books: We are requested to accept certain assumptions regarding the personalities of certain human beings as an a priori reason for dismissing a hypothesis. Nothing is easier, of course, than for the mischievous skeptic to turn this piece of argumentative artillery 180 degrees around and fire it off in the direction of the proponents. For we may regard those reports on Dr. X.’s experiments as *possible refutations* of current thinking about human nature, rather than as refutations of those “laws of nature” cherished by Professor Prokop.⁷

6 This tacitly refers to Dr. Wolf Wimmer, Presiding Judge at the District Court of Mannheim, who had distinguished himself as one of the most vigorous, but not exactly the most competent, critics of parapsychology in Germany; e.g., see note 28. (Eds.)

7 Dr. Otto Prokop, professor of forensic medicine at Humboldt University in (then) East-Berlin arguably was the most prominent and influential critic of parapsychology in post-war Germany. Also see the discussion launched by Mildenerger (2013). (Eds.)

It all depends on what we choose as the “unproblematic background knowledge” on the basis of which we evaluate fresh data. The incompatibility of psi and current orthodox models may disprove psi. It may also disprove those models. All we can do is point to a conflict between assumptions and construct rival hypotheses. Which “a priori” is the “true” one can only be decided after the possibilities of the accompanying explanatory models have been thoroughly explored.

Repeatability

Closely related to the above considerations is a methodological issue usually referred to as the “repeatability problem.” In his delightful article “Wie unwissenschaftlich ist eigentlich die Parapsychologie?” [How unscientific is parapsychology in fact?] Dr. Ulrich Timm has pronounced this problem “endgültig gestorben” [dead once and for all].⁸ Fortunately, Dr. Timm has later written an equally delightful article on Life after Death,⁹ so presumably he will not faint from shock at seeing the corpse rise from its grave in the present article.

It is quite true that some skeptics have made a mess of the repeatability requirement by implying that a phenomenon which cannot be reproduced *ad libitum* is *ipso facto* a non-existent phenomenon. The real problem, however, is not so easily disposed of. This problem may be summarized as follows. Critical researchers are confronted with a number of reports of occurrences which purport to demonstrate the existence of a psi factor, but which in principle admit alternative explanations in terms of malobservation, fraud, artifact etc. Which explanation is to be preferred can only be decided if it is known what factors were operating while the reported event occurred. This, however, cannot be established *post factum*. The only way to find out is to construct a hypothesis as to the causative agens and next subject this hypothesis to the test. The final goal is to isolate the active ingredient in such a way that the conditions for the appearance of the phenomenon can be specified in advance. Before this goal is reached, attribution of the observed “fact” to any specific cause will remain a matter of speculation. Alternative explanations are not ruled out.

The repeatability criterion, as employed by cautious skeptics, does *not* require that successful psi experiments can be repeated by anyone at any time.

There may be perfectly good reasons why psi behaves as elusively as it has done in the past millennia. But such reasons belong to the realm of the necessary and sufficient

8 Timm, U. (1980a). Wie unwissenschaftlich ist eigentlich die Parapsychologie? *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 22, 103-116.

9 Timm, U. (1980b). Thanatologie, Parapsychologie und das Survival-Problem. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 22, 249-258. (Eds.)

conditions and so should be formulated as a hypothesis with testable implications. Parapsychology's inability yet to meet this requirement does *not* mean that it is unscientific or superstitious to believe in psi. It does, however, mean that parapsychology, as an explanatory thinking model, has not yet achieved the sort of breakthrough that would demonstrate its superiority over the skeptical rival. Before surrendering to Future Science, the skeptic may request that the proponent provide him with instructions as to what to do in order to be able to observe an experimental result *which only the proponent's model could have anticipated*. It is certainly ironic that the only science dealing with precognition is singularly unsuccessful in its attempts to predict the outcome of its own experiments.

Skeptical Strategies

As I have indicated above, all skeptical arguments are variations on a single theme: Reports of psi may prove no more than that there is a flaw in the reporting process. Some skeptics generally prefer to remain as non-committal as possible as to the specific nature of such flaws. They argue that it is extremely difficult, *post factum* to identify possible sources of error. Rather, they repeat the skeptical *ceterum censeo* that the burden of proof rests on the shoulders of the claimant who should solve the repeatability problem instead of praying at its premature grave.¹⁰ However, few unbelievers are satisfied with this passive attitude. Most of them feel compelled to think about alternative scenarios to explain the evidence offered by the proponents.

Some counter-explanations are relatively uncontroversial in that they do not question the claimant's honesty or sanity. Conjectures about possible artifacts can usually be discussed in an amiable way, especially when they concern new and insufficiently tested experimental procedures. Also the possibility of subject cheating can be raised in many cases without causing offense to the more level-headed of the proponents.

However, when skeptics openly suggest the possibility that the experimenters themselves may have been idiots or frauds they are likely to be accused of character assassination and mud-slinging. Rao¹¹ has condemned the fraud hypothesis as "the disgraceful argument of the dogmatic goat." Other parapsychologists have expressed themselves in similar or even stronger terms.¹²

10 This position is clearly laid out in Hyman, R. (1981). Scientists and psychics. In Abell, G.O., & Singer, B. (eds.), *Science and the Paranormal* (pp. 119-141). New York: Scribner's.

11 Rao, K. R. (1966). *Experimental Parapsychology*. Springfield, IL: Charles C. Thomas, p. 18.

12 None in stronger terms than the late Professor W. H. C. Tenhaeff, a fact which some readers may find somewhat ironical.

I am afraid that Rao *et al.* here confuse the rules of etiquette with those of scientific reasoning. The question is not whether the fraud hypothesis is polite, but whether it is rational. At this point I must emphasize that no skeptic I know of has ever suggested that fraud is the only alternative to psi. Deliberate deception is merely one of the many possible flaws in the proponents' evidence. Misleading reporting does not have to be the result of a conscious intent to bamboozle. There are morally respectable forms of bias which may be responsible for serious reporting errors. Yet I think the skeptic would be well advised to keep the fraud hypothesis on his repertoire. Experience teaches us that cheating is one of the commonest of human activities.¹³ All claims of the paranormal to this point depend, precariously, on the assumption that the claimants at the moment of claiming were not indulging in this common pastime. The reporter's good faith is an *essential* link in the proponent's chain of evidence, and therefore should not be exempt from critical examination. Is it entirely unreasonable of the unbeliever occasionally to resort to a line of reasoning suggestive of the *ad hominem* attack when he is confronted with so many arguments of the "*ex homine*" variety?¹⁴

C. E. M. Hansel, whose books¹⁵ contain the classical formulation of the fraud hypothesis, has recently¹⁶ been criticized on the ground that his position is essentially unfalsifiable. He rejects the significant results of a number of prize experiments (or at least denies them the status of conclusive evidence) because these results *could* have been brought about by cheating. He does not, however, demonstrate that fraud actually took place in all cases. According to Collins and Pinch, this strategy could be used to explain away even the best established of physical theories. No amount of evidence will ever *prove* that no cheating occurred.

For several reasons, I disagree with this objection. Hansel might, justifiably, claim that the empirical evidence that fraud actually took place is provided by precisely the same experiments which the parapsychologists cite to support their view that ESP actually took place. Statistics being unable to do more than confirm or disconfirm the null hypothesis

13 Müller, L. (1980). *Para, Psi und Pseudo. Parapsychologie und die Wissenschaft von der Täuschung*. Berlin, Frankfurt/M. & Wien: Ullstein Verlag. [See Hoebens' review of Müller's book in chapter 4-12 of the present book. (Eds.)]

14 *Ad hominem* attacks from parapsychologists on skeptical authors are not uncommon, although this seems to have escaped the notice of Collins and Pinch (see note 5).

15 Hansel, C. E. M. (1966). *ESP: A Scientific Evaluation*. New York: Scribner's; a revised and enlarged edition was published as: Hansel, C. E. M. (1980). *ESP and Parapsychology: A Critical Re-Evaluation*. Buffalo, NY: Prometheus Books.

16 Collins, H., & Pinch (1979), see note 5. Also cf. Hyman, R. (1981). Further comments on Schmidt's PK experiments. *Skeptical Inquirer*, 5, (3), p. 39.

the data are compatible with both explanations. Both explanations, moreover, are equally “unfalsifiable” as it is impossible ever to prove that no psi occurred. Even when fraud is as well demonstrated as in the case of Soal this can be explained in terms of ESP, as Pratt has shown.¹⁷

I think that Lakatosian methodology suggests a way out of the dilemma the falsifiability requirement (or at least Collins and Pinch’s curiously lopsided version of it) has got us into. According to Lakatos, theories should be rejected, not as soon as they are contradicted by “facts,” but only when they have degenerated to purely verbal strategies for explaining the data ad hoc and post factum, whereas a rival “programme” is able to explain the data “progressively,” i. e. by generating successful predictions. Collins and Pinch are trivially right to insist that the fraud hypothesis *could* be used to explain away any scientific discovery. Just claim that all physicists who pretend that the Earth is a sphere are engaged in a conspiracy!

I will not need to explain why the fraud hypothesis in *this* case is less plausible than the one provided by orthodox models. Here, the fraud hypothesis must be rejected not because it is intrinsically absurd or impolite, but simply because there is a *demonstrably better* hypothesis. Matters are different in the case of parapsychology. Proponents have not yet developed an independently testable theory to support their belief that a paranormal factor is the most likely cause for the anomalies which they report. That Hansel’s hypothesis, at this moment, remains “unfalsifiable” is, I perversely suggest, the proponents’ own fault. For in principle, Hansel can be “falsified” as soon as his detractors discover a way to perfect their explanatory model so as to generate successful predictions for which no amount of Hanselian ingenuity could possibly account.

What Does Skepticism Predict?

To this point I have argued that parapsychology (at least the “believers” explanatory model) has failed to demonstrate its superiority over its skeptical rival, the ability to anticipate novel facts being the criterion for superiority. What about skepticism? Does it have any predictive power at all? It certainly has, I am afraid.

From the skeptical model it follows logically that the “classical” proofs for psi must be flawed. These “proofs” being historical events, it will usually be difficult, positively to identify the sources of error. However, it stands to reason that in some cases traces of the original error have not yet entirely disappeared. Documents may still exist which

¹⁷ Pratt, J.G. (1978). Statement. *Proceedings of the Society for Psychological Research*, 56, 279-281.

undermine the accepted version of certain prize cases, witnesses may suddenly remember suspicious details, novel discoveries in the field of “orthodox” science may generate unexpected “naturalistic” explanations. Skepticism predicts that much of the “classical” evidence will evaporate in this way. And there have been surprising confirmations. A case in point is the Soal affair. The relevance of this exposure can hardly be overestimated.

Soal’s experiments were generally felt to be probably the best evidence parapsychology could offer. Not only because the scores were astronomically significant, but even more because the experimental design was seen as the ultimate in fraud control. Moreover, the experimenter had a solid reputation as a merciless critic of other peoples’ sloppiness. If there ever was a strong *prima facie* case against the fraud hypothesis, it certainly was the work of Soal. In spite of this, the suspicions of certain skeptics, voiced long before the final *démasqué*,¹⁸ were confirmed.¹⁹ On a more modest scale, some of my own investigations into much publicized claims have yielded results which confirm what one could have predicted on the basis of the Hanselian model.²⁰ There is a considerable amount of “backstage information” which strongly suggests that more embarrassing revelations will follow.²¹

I disagree with those of my fellow-skeptics²² who, in their published writings, convey the impression that skeptical inquiry will automatically result in unambiguous confirmation of skeptical expectation. Even after the most thoroughly critical examination of the available evidence, “loose ends” will remain; elements that ill fit the unbelievers’ explanatory model. Regrettably, such loose ends are too often glossed over in the skeptical literature. It is better, I think, and more fair, openly to admit that skepticism does not have an answer to every question. When faced with a considerable challenge, the skeptic

18 Markwick, B. (1978). The Soal-Goldney experiments with Basil Shackleton: New evidence of data manipulation. *Proceedings of the Society for Psychical Research*, 56, 250-277.

19 On the “Soal affair” also see chapter 2-15 and its editorial introduction. (Eds.)

20 Hoebens, P.H.: Gerard Croiset: Investigation of the Mozart of “psychic sleuths.” *Skeptical Inquirer*, 6, 1981, (1), 17-28; Hoebens, P.H.: Croiset and Professor Tenhaeff: Discrepancies in claims of clairvoyance. *Skeptical Inquirer*, 6, 1981-1982, (2), 32-40; Hoebens, P.H.: The mystery men from Holland: I. Peter Hurkos’ Dutch cases. *Zetetic Scholar*, No. 8, 1981, 11-17.

21 This very likely refers to magician James Randi’s so-called “Project Alpha,” about which Hoebens was informed (through Marcello Truzzi, as the Hoebens Files confirm) some time before it was disclosed. (Eds.)

22 E.g., James (“The Amazing”) Randi in his latest book *Flim-Flam!* New York: Crowell, 1980 (see my book review in *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 23, 1981, pp. 246-251 [reprinted here as chapter 4-13, Eds.]).

may resort to prevarication or to ad hoc strategies, provided that he does not do so surreptitiously. I much admire the frank statement by Christopher Scott: “Thus, faced with a piece of evidence for psi, the task for the skeptic is to find a plausible explanation consistent with, not necessarily the report as published, but at least a plausible distortion of the report as might represent the true events.”²³ I will put it even blunter: When faced with an apparently “inexplicable” piece of evidence, the skeptic may *assume* that there is a hidden flaw in that evidence, even if he does not have the faintest idea, what this flaw might consist of. Strict falsificationists²⁴ will find such stratagems absolutely horrifying. Yet I wish to defend the legitimacy of such evasive tactics. For they are rendered ultimately “falsifiable” by skepticism’s most audacious prediction: that parapsychology will never be able to solve the problems which have bedeviled it ever since the founding of the Society for Psychical Research. If parapsychology solves the “repeatability problem” skeptical ad hoc arguments will have been refuted. Unbelievers are willing to bet that this will never happen. Perhaps they are wrong, Until now, they have not lost their money.

Prejudices

Professor Bender and many of his colleagues have called for an investigation into the “prejudices” that afflict those skeptics who, even after having familiarized themselves with the results of the Pirmasens chair test, refuse to bow their heads and admit that there is more in heaven and earth than is dreamt of in Martin Gardner’s philosophy.²⁵ Notice the symmetry with statements by certain confirmed unbelievers who are convinced that the problem of psi can be solved by placing the proponents on the psychoanalyst’s couch. It would be more relevant, I humbly suggest, to recognize that “prejudices” on both sides play a necessary and by no means always uncreative role in a dispute which cannot yet be resolved unambiguously.

At the beginning of this article, I have frankly admitted that there is an element of non-rationality, of “emotion,” in the skeptical position. Why have we chosen to explore the “negative” thinking model in the first place? A subcutaneous hunch has probably warned us to distrust the Paranormal. We have a gut feeling that the suspicious features of the alleged phenomenon – its distasteful triviality, its marked preference for manifesting itself in the company of cranks and crooks, its elusiveness in the face of incredulity, its striking resemblance to known forms of illusion and deception, yes, even the strange

23 Scott, C. (1980). Commentaries on Dr. Beloff’s paper. *Zetetic Scholar*, No. 6, 110.

24 Popper never was one of them.

25 Bender (1981), see note 2.

compulsion felt by many of its proponents to construct breathtaking cosmologies on the basis of wafer-thin evidence – point to its nonexistence.

A basically metaphysical intuition tells me that, if we were able to journey back into time and be present at the miracles on which today's belief is based, we would eventually discover the non-paranormal causes for their occurrence. No amount of evidence can directly prove or disprove this intuitive feeling. In itself, it is "unfalsifiable." It does have, however, eminently falsifiable implications.

Stereotypes

In the foregoing pages I have, for the sake of argument, employed stereotypes, suggesting that skeptics and proponents are mutually exclusive groups, holding mutually exclusive ideologies. The reality of the continuing pro and con debate is far more complex.

Many registered "proponents" in fact more often than not employ a decidedly skeptical thinking model when confronted with outlandish claims, whereas some outspoken unbelievers are among the staunchest defenders of the legitimacy of parapsychology. As Professor Hyman has remarked, there are important overlappings of common interests and goals that cut across the "believer"–"skeptic" dichotomy.²⁶ In practice, I find myself in near-perfect agreement with some of those who nominally are my "opponents." What remains is little more than a difference in what Dr. Beloff has called "metaphysical predilection," a difference in expectations as to the final outcome of the controversy.

Thinking models do not need to be closed ideologies. It is possible and commendable, occasionally to try another one if only by way of intellectual calisthenics.

As a counter-balance to my skeptical predisposition I personally like to volunteer as a subject in psi experiments even though the outcome, in my case, is sometimes highly embarrassing for a registered unbeliever.²⁷ My initial remark that a pig-headed refusal to believe in psi may be a perfectly rational reaction to the claims of the paranormal should not be construed as a plea for closed-mindedness. I only wish that, as long as the matter has not been settled by a qualitative breakthrough unambiguously demonstrating the superiority of one particular explanatory model, all explanatory models should be adequately defended.

26 Hyman, R. (1978). Comments on Laurent Beaugard's paper. *Zetetic Scholar*, No. 2, 119.

27 See chapters 2-08 and 2-09 in this book. (Eds.)

Different Sorts of Skepticism

The skepticism defended in this article, the discerning reader will have noticed, is somewhat different from the brand offered in, for example, “Hexenwahn an Universitäten [Witch-craze at universities]”.²⁸ Some skeptics regard parapsychology as an intrinsically absurd pseudo-science which should be “exposed,” not invited for a dialogue. Such critics are apt to point to the social dangers of “Okkultgläubigkeit [occult belief]” being granted a semblance of respectability. Even these “super goats,” I suspect, will admit that the probability that psi exists is a fraction above zero.

However, they will argue that this is true for any claim, including the claim that the moon is made of green cheese. We all know of “crank” ideas that later proved sound. But we should not forget that “for every example of a crank who later became a hero there were thousands of cranks who forever remained cranks.”²⁹ In practice, it is a waste of time to shower “tolerance” on the unorthodox sciences. The chances that we are ridiculing a future Galileo or Pasteur are infinitesimally small. The chances that we are doing society a service by impugning noxious nonsense are accordingly large. Obviously, we cannot carefully examine and give the benefit of the doubt to each and every outlandish idea.

It would cost too much time, and too much money that would be more profitably expended on more plausible pursuits. One does not need to have read the Flat Earth literature in order to reject the Flat Earth theory. Some ideas are so ludicrous that they may confidently be dismissed prior to investigation. Why make an exception for parapsychology, where some of the leading practitioners have publicly espoused ideas compared to which the beliefs of the Flat Earth Society seem a model of scientific rigor?

I find it difficult to deny the logic of such arguments. All I can do is to point to “circumstantial evidence” supporting a different view and to admit, once again, that there is an element of “intuition” in my own preference for soft-line skepticism.

A century after the founding of the SPR the total evidence for psi, while inconclusive, is more suggestive than the 1882 skeptic would have predicted. The professional competence and the scientific sophistication of some of the proponents is impressive. (There is no Flat Earth equivalent to, say, Helmut Schmidt or John Beloff.) Skeptics have run into unexpected problems with many of their attempts, entirely to explain away certain claims. Even more important: Within the parapsychological community there is place for

28 Wimmer, W. (1980). Hexenwahn an Universitäten? *Zeitschrift für Allgemeinmedizin*, 56, 1390-1400.

29 Gardner, M. (1981). *Science: Good, Bad and Bogus*. Buffalo, NY: Prometheus Books, p. xiii.

the sort of rigorous self-examination and self-criticism seldom seen in other fringe areas of science. All this to me suggests that parapsychology is an exceptional case and deserves the sympathetic, if critical, attention from those who are concerned about the dangers of the Occult Explosion. Ultimately, however, my view that parapsychology should be taken seriously is based on an intuition which I cannot entirely explain. Let us call it what it is: a “prejudice” – albeit one unlikely to disturb Professor Bender.

CHAPTER 2-02

Editorial Introduction

Piet Hein Hoebens presented the following paper at a symposium on “The Case for Skepticism” at the combined Centenary-Jubilee Conference of the Society for Psychical Research (1882-1982) and the Parapsychological Association (1957-1982) that was held at the prestigious Trinity College in Cambridge in August of 1982. The symposium (chaired by criminologist D. J. West, with a subsequent open discussion moderated by sociologist Marcello Truzzi) had been convened by British statistician and parapsychologist-turned-skeptic Christopher Scott. Participants, in addition to Scott (“Why parapsychology demands a skeptical response”) and Hoebens, were psychologists Ray Hyman (“Does the Ganzfeld experiment answer the critic’s objections?”) and Susan Blackmore (“Prospects for a psi-inhibitory experimenter”).

While deplored by some extremist hard-liners in both the skeptical and parapsychological camps,¹ many were impressed by the constructive atmosphere that prevailed during the symposium as well as by the fact, apparently unexpected by some, that it had not only been accepted by the program committee but seemed to be well received by major parts of the audience.² In retrospect, this event may be considered something like a turning point in the relationship between (some) psi researchers and (some of) their critics. The symposium also marked a starting point for occasional constructive cooperation between nominal parapsychologists and nominal skeptics.³

All contributions to the symposium were abstracted in Research in Parapsychology 1982 (Roll, Beloff, & White, 1983 [with the abstract of Hoebens’ paper on pp. 15-17]. The full version of Hoebens’ presentation (which John Beloff repeatedly referred to in his Presidential Address at Cambridge) so far was only available in the first of two (unpaginated) volumes of the Proceedings of presented conference papers. Today these are difficult to find outside specialized libraries. (Eds.)

1 Gruber (1982) is an obvious example for the psi-proponents’ side.

2 During and after the conference, German arch-skeptic Thomas von Randow (1921-2009), a prominent science-journalist for the influential weekly *Die Zeit* (see Sommer, 2009) repeatedly assured one of the editors (G. H. H.) that he was very favorably impressed by the event, and he said as much in his conference report for *Die Zeit* (Randow, 1982); also, cf. Hoebens’ own conference report, reprinted as chapter 4-03 of the present book.

3 Cf. Hyman & Honorton (1986).

TIME MACHINES, THE HUME GAME AND A SUCCESSFUL REPLICATION OF A CLASSIC ESP EXPERIMENT

Sometimes, the best way to avoid misunderstandings is to begin whatever you wish to say with a disclaimer or two. Being something of a semi-professional goat, I have been invited here to defend the case for skepticism.

This does not mean that I regard myself as the representative of an organized movement, committed to a party line. Neither do I feel that it is incumbent on me to present a comprehensive view of the entire field of skepticism. Rather, it is my intention to say something in favor of certain skeptical pet opinions which I have recently seen challenged by well-informed proponents.

The very term “skeptical” is a source of much confusion. Etymologically, it refers to an attitude of agnostic doubt. However, many persons who profess to be skeptics and who are widely recognized as such, are hardly known for their willingness to suspend judgment on the subject of psi. James Randi, for example, has written an interesting and amusing book, *Flim-Flam! The Truth About Unicorns, Parapsychology and Other Delusions*. Someone who really *doubts* would never write a book with a title like that.⁴ Skeptics – in the original meaning of the word – search for the truth. They feel unable to claim that they have found it – and certainly not The Truth with a capital T. Some parapsychologists have written and said many things that I would call genuinely skeptical. Yet they are not usually thought of as skeptics.

In this paper, I will acquiesce in the fact that skeptics are no longer what they used to be and employ the word in such a way that Randi qualifies as a skeptic and Beloff does not.

Instead of a proper definition I will offer something in the nature of a thought-experiment from which such a definition could be inferred. Assume that someone discovers the secrets of time travel and constructs a time machine. Assume that you are invited to use this machine to go back in time till a time that a spectacular psi-event is said to have occurred. In the thought-experiment all experts agree that the case for psi stands or falls with the authenticity of this particular claim. Say, for instance, that one of D. D. Home’s levitations were selected.

Next assume that you have at your disposal sophisticated equipment that will enable you to record in a completely objective manner whatever you will witness. Finally, assume

4 Cf. Hoebens’ review of Randi’s book in chapter 4-13 of this book. (Eds.)

that you are invited to predict what you will, in fact, find and that you are offered one million pounds if your prediction is verified.

Then, you are a skeptic if you think that your chances of winning one million pounds are best if you predict that your equipment will not register a genuine levitation, but will instead reveal the naturalistic factors responsible for the erroneous reporting.

The definition implicit in this example has the advantage of being neutral. It does not say anything at all about the reasons participants would give for their decision. In many cases, people would probably bet on the skeptical horse for entirely illogical reasons. In other cases, it would be a rational choice.

However, all skeptics, whether they are rational or irrational, open-minded or fanatic, have one thing in common: They expect that if and when the matter is settled, it will not be settled in favor of the paranormal.

Time machines do not yet exist, so far as I know. Yet the thought-experiment I have just proposed is basically an idealized version of something which can be done in real life. I am now referring, approvingly, to what John Palmer⁵ has disapprovingly called “the Hume Game” – after [philosopher] David Hume, not Daniel Dunglas Home.⁶

In his celebrated essay *Of Miracles*⁷ David Hume advanced what he considered to be a watertight argument against superstitious belief in miracles. He states that a miracle is a violation of a law of nature, and that the only sufficient testimony for it would be testimony whose falsehood would be even more miraculous than the miracle itself. This argument is often employed by persons who wish to deny the reality of the paranormal. In fact, I suspect that most arguments against psi are somehow variations on Hume’s theme. That is probably why proponents of parapsychology are still attempting to demonstrate that Hume was mistaken.

More recently, K. Ramakrishna Rao delivered a paper entitled “Hume’s Fallacy”⁸ His objections against the famous argument are that it is tautological and that it is an a priori prescription that cannot possibly invalidate empirical claims.

5 Palmer, J. (1978). Extrasensory perception: Research findings. In Krippner, S. (ed.), *Advances in Parapsychological Research, Volume 2: Extrasensory Perception* (pp. 59-243). New York & London: Plenum Press.

6 D. D. Home pronounced his name >hume<. (Eds.)

7 Hume, D. (1963). *Essays Moral, Political and Literary*. Oxford: Oxford University Press.

8 Rao K. R. (1981). Hume’s fallacy. *Journal of Parapsychology*, 45, 147-152.

John Palmer⁹ has further criticized Hume's argument for being essentially unfalsifiable. "In other words," he tells us, "all a critic has to do to destroy the evidence for ESP (or any other hypothesis he or she doesn't like) is to demonstrate that for each relevant experiment there is some alternative hypothesis that doesn't invoke ESP." Since ESP is defined as a miracle by orthodox scientific assumptions, and because the supply of alternative hypotheses is all but inexhaustible the Humean observer always has to decide against the psi hypothesis. In other words: The skeptic cannot possibly lose the "Hume Game."

Or can he? I wish to propose that, with some modifications suggested by Hume himself, the celebrated and notorious argument against miracles can be used as a ground rule for an entirely fair contest between proponents and skeptics.

All we need to do is to inject some 20th century fallibilism into Hume's 18th century concept of natural law, and to be more precise as to what we mean by "testimony." Hume was not as doctrinaire and given to tautological debating tricks as Rao and Palmer apparently think. For the argument against miracles contains what nowadays would be called a "potential falsifier." Hume *does*, under certain conditions, accept "testimony" of events contradicting the Laws of Nature. His condition is that the testimony be of such kind its falsehood would be more incredible than the alleged miracle itself.

This enables the proponent of some "miraculous" claim to win the Hume Game – provided, of course, that this claim is true. What is required of him is that he strengthen his "testimony" in such a way that to deny it would entail rejection of a more fundamental principle of science than is violated by the original claim.

Now a very fundamental principle of science is that theories and hypotheses should be judged according to their explanatory and predictive power. In fact, it is this principle which underlies our confidence in what Hume called "laws of nature." These laws are theories which explain more facts in a more consistent fashion and also anticipate more novel facts than rival theories.

On the basis of these considerations I propose a modernized reconstruction of Hume's argument: "The reality of a 'miracle' can be established to the extent that the proponent is able to transform his belief that the 'miracle' occurred into a 'hypothesis' which successfully predicts future observations which rival 'hypotheses' cannot adequately account for, on the understanding that such observations can be repeatedly made even by adherents of the rival 'hypotheses.' As long as the proponent remains unable to meet this requirement (which in principle allows even unique events to qualify as an established 'miracle')

9 Palmer, J., *op. cit.*

the skeptic may cite *a priori* reasons for remaining skeptical about the empirical claim which he believes disregards the ‘laws of nature.’”

Using this ground rule we could easily validate the miracle of psi if only we had a time machine. All the proponent would have to do is to invite the skeptic to go back to Ashley House December 1868 and see for himself how D. D. Home defied the laws of gravitation. Very soon the anecdote about the levitation would be elevated to the status of fact, which the unbeliever could not possibly deny, except by sacrificing the very principles on which his skeptical position was originally founded. The claim would then be supported by a hypothesis of the *if, ...then* type. *If* you use the time machine, *then* you will witness, as many times as you like and assisted by all the recording equipment you can think of, the event which you believed was impossible.

As I have regretfully observed before, travel into the past does not seem possible. However, we cannot possibly avoid traveling into the future and this fact fortunately enables us to transform the imaginary wager into a less spectacular, but genuine contest.

The contest is about which explanatory model is most successful in anticipating future developments in the field of psychical research: the proponent’s model, according to which the existing evidence points to the reality of a paranormal factor, or the skeptic’s model, according to which the evidence merely demonstrates that, occasionally, something has gone wrong in the process of observing, reporting and evaluating non-paranormal occurrences.

At this point, I wish to emphasize that the matter of psi cannot be settled by arguments over the quality of the existing evidence, which basically consists of historical anecdotes, whether or not in the guise of scientific reports on laboratory experiments. One should never place too much trust in anecdotes, especially when these purport to establish the reality of an extraordinary event. It is often impossible, *post factum* to identify the source of error which led to an erroneous report.

This is a point frequently overlooked by over-zealous skeptics, as Ray Hyman has rightly reminded us.¹⁰ These super-goats appear to suffer from some sort of horror vacui: They cannot bear the thought that any claim of the paranormal should remain un-debunked. Presumably, they believe that failure on their part to offer an instant naturalistic explanation for the evidence suggestive of psi amounts to a capitulation to the forces of unreason. In their impatience, they sometimes embrace a definitive explanation that later turns out to be less than definitive, to the considerable embarrassment of the skeptical community.

10 Hyman, R. (1981). Scientists and psychics. In Abell, G. O., & Singer, B. (eds.), *Science and the Paranormal* (pp. 119-141, 375-378). New York: Scribner’s.

Investigating *faits accomplis* can be a very useful and amusing activity, but in general the results of such investigations have only heuristic value for the pro and con debate over parapsychology.

I believe it would be unwise for both proponents and skeptics to render their positions vulnerable to refutation-by-anecdote. I strongly recommend that they should resort to immunizing strategies to avoid instant falsification.

Quite a few knowledgeable parapsychologists would have been willing, twenty years ago, to bet their entire faith on the reliability of the Soal–Shackleton experiments. History has shown, how reckless this would have been. When the “time machine” arrived in 1978 the best available recording equipment registered evidence of data manipulation, not ESP. I fully agree with those parapsychologists who decided, after this, that the exposé of Soal was not fatal to the case for ESP.

On the other hand, the skeptic should not be afraid to maintain an attitude of “offensive incredulity” in the face of a seemingly credible miracle. As long as those miracles remain on the anecdotal level, the skeptic may regard them as temptations from the devil, and pray to the Holy Hume, beseeching him to strengthen his unbelief. I imagine that the saint will reply in more or less the following words: “Have you forgotten my Argument? What would be the greatest miracle? That the Laws of Nature should be violated, or that one skeptic should not be sufficiently ingenious to think of a rational explanation?” In short, the skeptic may *assume* that in any given piece of anecdotal evidence favorable to psi, there is a hidden flaw, even if he does not have the remotest idea, of what this flaw might consist.

Strict adherents to the falsificationist party line will find such stratagems absolutely horrifying. Yet I wish to defend the legitimacy of such evasive, elastic arguments. For they are ultimately rendered “falsifiable” by skepticism’s most audacious prediction: that parapsychology will never be able to solve the credibility problems which have bedeviled it ever since the founding of the Society for Psychical Research.

This is what the Hume Game is basically about. The skeptics predict that when the time machine travels farther and farther into the future, its cameras will never register the sort of evidence that will compel the unbeliever to surrender either his unbelief or his commitment to rational thinking. The proponents predict – implicitly or explicitly – that the problems with the evidence will be overcome.¹¹

11 The proponent is actually at an advantage when playing the Hume Game, for it is difficult to imagine him suffering a decisive defeat. On the other hand, it is difficult to imagine the skeptic celebrating a decisive victory. Psi can in principle be “proven” on the argument above, while it

In the meantime, skeptics and proponents can and should engage in a more informal version of the Hume Game by continuing the debate on the merits of the existing body of anecdotal evidence. This is a game of the no winners no losers type. Each party always has an “out”: the skeptic, when unable to explain away a certain type or piece of evidence, can fall back on the all-purpose argument that the unusual facts are the result of an unspecified hidden flaw. Similar strategies are available to the proponent.

This informal version of the Hume Game may seem futile, but it does serve a rational purpose. First of all, there is a very practical point: Quarreling over anecdotes can lead to the discovery of possible loopholes and other sources of error, which, in turn, can bring about improvements in experimental design. Second, as a result of the informal Hume Game, the relative strengths and weaknesses of various types of evidence will become apparent, a fact of which theoreticians of psi may take advantage. Imagine that some parapsychologist hits on an extremely plausible and promising theory of ESP which, alas, is incompatible with the assumption that the psychic feats of Peter Hurkos are genuine.

In that situation it will suddenly become of considerable interest that, on the anecdotal level, many of the relevant claims have been fairly thoroughly discredited. This will encourage the theoretician to continue to have confidence in his theory – something which he would not be able to have if his model had been incompatible with the assumption that the Helmut Schmidt experimental results are genuine.

Third, this game will reveal the relative strengths and weaknesses of the proponents’ and skeptics’ positions in themselves. Take skepticism: From its initial assumption (psi does not exist, reports of psi are the result of some sort of error) it logically follows that the existing evidence is flawed. Now it stands to reason that in many cases traces of the original errors have not yet entirely disappeared. Documents may still exist which undermine the accepted version of certain prize cases, witnesses may suddenly remember suspicious details, novel discoveries in the field of “orthodox” science may generate unexpected “naturalistic” explanations. Skepticism predicts that much of the classical evidence will evaporate in this way. The extent to which this prediction is verified can serve as a useful indication as to what side has the best chances of winning the Hume Game. If, for some reason, both the Schmidt experiments and the Ganzfeld work were to fall from grace it would probably cause many proponents to change sides. On the other hand: If some psychic were able to effect a complete auto-levitation in the presence of and

cannot be rigorously disproven. However, it is faintly conceivable that the skeptics will some day gain what amounts to a total victory. For this they will have to devise a repeatably testable theory which describes, comprehensively, the mechanisms which have led to the erroneous and persistent rumor that psi exists.

on conditions dictated by a committee consisting of Randi, Hyman and Truzzi, this anecdote would compel me to change my bet in the game and to become a proponent myself.

I do not wish to exaggerate the importance of my own investigations of specific parapsychological claims, but I do think that, in its modest way, my work will serve as a practical illustration of the theoretical issues discussed above. I was somewhat reluctantly drawn into the psi-controversy when, in 1978 and 1979, I published a series of articles in the Dutch daily newspaper, *De Telegraaf*, on the growing interest in matters occult. My articles were mainly concerned with debunking the more outlandish claims, and contained a few cautionary remarks on the need to keep an open mind regarding the serious attempts to investigate the paranormal with scientific rigor. In fact, the final part of the series was an extensive interview with Professor Martin Johnson, who aptly defended his field as “an attempt at science.”¹²

Nevertheless, my publications created an uproar among the Dutch “believers.” My editor, the publisher of the newspaper and myself started to receive numerous letters demanding my instant dismissal. Correspondents suggested that my disbelief could only be accounted for by the assumption that I was a communist or a madman.¹³ On a more dignified level, a university professor of parapsychology wrote a series of columns for *De Telegraaf* urging the readers not to listen to the “fanatic rationalists” but to accept the undeniable “fact” – confirmed by several Nobel Prize laureates – that the existence of ESP and PK had been demonstrated “beyond reasonable doubt.”

In almost all of these various reactions there was a common note: My critics challenged me to come to terms with the most compelling evidence parapsychology could offer. By this they meant “the rigorously scientific work of Professor Tenhaeff” with “the Caruso among the psychics,” Gerard Croiset.

At that moment I knew little about Tenhaeff and Croiset, except for what had been published in the pro-books and in the generally friendly mass media. I knew that Tenhaeff had been the first person in history to be appointed professor of parapsychology at an established western university, and that his reputation as a pioneer of psychical research was enormous, especially in his own country, in Germany and in Italy. About Croiset I knew that he was an internationally known psychic superstar, a man who was said to have assisted the police in five continents to find missing persons and solve crimes; he was also said to have excelled at a special form of precognitive ESP experiment, the

12 Published in English for the first time as chapter 2-10 of this book. (Eds.)

13 A selection of these correspondences is preserved in the Hoebens Files; they confirm Hoebens’ description. (Eds.)

so-called chair test. Prima facie, some of his achievements seemed impressive indeed, particularly as they had been reported not by a sensation-mongering tabloid writer but by a highly respected scholar. Such were the cards when, urged by my critics, I began to play the Hume Game with Professor Tenhaeff and Gerard Croiset.

If skepticism was right, it was to be expected that critical examination of the published evidence would reveal at least some reasons for caution. The skeptic would predict that a naturalistic mechanism would be found for the bulk of the Croiset material, and that, in the reports on the more incredible cases, suspicious features would be detected. Further it could be predicted that Croiset and his coterie would not be happy to cooperate in experiments designed to check skeptical suspicions.

The last prediction was fulfilled immediately when I contacted the psychic. He courteously but firmly declined any invitation to work wonders under skeptical scrutiny. The professor of parapsychology who had criticized me in *De Telegraaf* was furious that I had dared “to challenge Caruso to prove that he could sing.”

This professor of parapsychology insisted that the existing evidence was more than enough to convince any impartial observer of the reality of Croiset’s unique mediumship. He himself, as he told me, had been particularly impressed by one transatlantic chair test Tenhaeff had reported in several of his publications. This he compared to Swedenborg’s celebrated vision of the Stockholm fire.

I carefully examined the evidence in this case and discovered that Tenhaeff’s Dutch accounts of the experiment were highly misleading. The actual facts had been embellished and distorted in such a way that it was difficult to think of an innocent explanation.¹⁴ The real experiment had been inconclusive. Tenhaeff had transformed it into a miracle.

After having seen the manuscript of my report of this case, another Dutch parapsychologist agreed that Tenhaeff had committed serious errors. However, he warned against any attempt to generalize my criticisms to Tenhaeff’s entire work. The incident, after all, might have been atypical.

He wrote: “To make that generalization would require at least one other comparison. It is quite possible to check the stories of Tenhaeff against other sources such as in the police cases. If there the same discrepancies turn up I would be inclined to justify a generalization. Until that moment I think we should give him the benefit of the doubt.”

14 Hoebens, P.H.: Vom Lob der Genauigkeit in der Parapsychologie. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 22, 1980, 225-234. [For an English version see chapter 2-04 in this book. (Eds.)]

So next I investigated a number of prize cases in the field of psychic detection, as reported by Tenhaeff. In each and every one I discovered glaring flaws in the published accounts. In one case, Tenhaeff had cited a formal and signed statement from a police officer, confirming that Croiset had been completely successful in exposing a dangerous and elusive arsonist. Upon investigation it turned out that the formal and signed statement did not exist. The police officer who had been quoted by Tenhaeff accused the professor of having resorted to “outright falsehoods.” A tape recording of the original consultation proved beyond a shadow of doubt that Tenhaeff had falsified the psychic’s statements.¹⁵

So, by the criteria previously suggested by a representative Dutch parapsychologist, skepticism had won another round in the informal Hume Game. A “world figure in parapsychology,” as Tenhaeff was called in a *Parapsychology Review* obituary, had been shown to be an utterly unreliable source.

I do not know whether the parapsychologist who in 1980 had written that a least one other exposé of a Tenhaeff case was needed has stopped giving the professor the benefit of the doubt. Perhaps he was jesting when, in 1982, he proposed to the Dutch SPR that Tenhaeff should be honored by naming the Utrecht Parapsychology Institute after him.

The game went on, and the next move was made by Professor Hans Bender, the well-known parapsychologist at Freiburg University and for a long time a close associate of Tenhaeff. In the 3-4, 1981, issue of *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*¹⁶ he made a gallant attempt to contain the damage caused by my publications by regaling his readers with a re-appraisal of a classic chair test with Croiset, performed in 1953 in the German town of Pirmasens and originally reported by Bender in the very first issue of his journal.¹⁷

In a review in the same journal,¹⁸ I had pointed to some small but curious discrepancies between Bender’s and Tenhaeff’s accounts of that particular experiment. In 1981 Bender insisted that these discrepancies were inconsequential and in no way invalidated the results which, he solemnly asserted, would “astonish any unprejudiced observer.”

15 Hoebens, P. H. (1981-1982). Croiset and Professor Tenhaeff: Discrepancies in claims of clairvoyance. *Skeptical Inquirer*, 6, (2), 32-40. [Reprinted as chapter 3-05 of this book. (Eds.)]

16 Bender, H. (1981b). Pirmasens 1953 – Retrospektive auf ein Platzexperiment mit Gerard Croiset. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 23, 219-230.

17 Bender, H. (1957). Praekognition im qualitativen Experiment. Zur Methodik der “Platzexperimente” mit dem Sensitiven Gerard Croiset. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 1, 5-36.

18 Hoebens: Vom Lob der Genauigkeit (see note 15).

According to Bender, it had been Tenhaeff's enormous merit to have encouraged Croiset to perform this Herculean psi feat. The experiment had been supervised by Bender which, clearly, makes it invulnerable to any objection based on the untrustworthiness of Tenhaeff.

To summarize Bender's 1981 article: In June 1953 Gerard Croiset made a number of statements intended to apply to a person who a few days later was to occupy seat no. 73 in a Pirmasens classroom, where a demonstration of the chair test was scheduled. He gave a further reading intended to apply to the owner of an "inductor" object which the target person of the first test should pick up at random from several to be collected from participants prior to the demonstration.

According to Bender, both readings turned out to be amazingly apposite. For reasons of space, I will restrict myself to discussing a few hits scored in the first part, with the understanding that my own full report on the case, invited by the Editor of the *Zeitschrift*, will demonstrate that my arguments apply to the entire double experiment.¹⁹

Among other things, Croiset had made the following statements – paraphrased by me. The person in chair no. 73 would be a woman, about 30 years old. She had something to do with a man who looked like a particular German film actor and also like Churchill. She lived near a red building with tall columns and a high staircase. She often visited that building. She had had an emotional experience in a delicatessen shop, where she had bought or looked at a fruit basket. Croiset mentioned a box of dates. He also described a young man, 28-32 years old, with dark blond hair and wearing a pullover. This man had made a plan which the target person had prevented him from carrying out. She had turned her nose up at a green cigarette box.

We will probably never know, who, on the evening of the experiment, actually occupied seat no. 73. Croiset himself decided that the person he had paranormally observed was actually seated "two seats away from it." However, by that time Professor Bender was well aware of the frequency of "displacement effects" in the chair experiments, so the woman not sitting in no. 73 was accepted as the target person. According to the reports by Bender,²⁰ the young woman interpreted the statement about the man looking like Churchill as applying to her employer. She lived near a delicatessen shop, but could not

19 It was eventually published as „Abschied von Pirmasens. Eine kritische Nachprüfung eines erfolgreichen ASW-Experiments" in *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 26, 1984, pp. 4-28, and is reprinted in this book, in English, as chapter 3-11. (Eds.)

20 An English account of the case can be found in: Ebon, M. (1968). *Prophecy in Our Time*. New York: The New American Library. This account seems to be based on Tenhaeff's slightly embellished Dutch versions.

recall any emotional experience connected with it or with a box of dates. She did not live near any building like the one described by Croiset. However, there was such a building at the Pirmasens graveyard. The woman remembered an emotional experience there eleven years previously. On that occasion, she had worried about the fate of a friend who was away on the eastern front. Shortly before the experiment she had received a letter from this man, who suggested that they renew their relationship: This had irritated her. She stated that Croiset's description of the young man with the dark blond hair applied to this former friend. As a result of her refusal, his plan to renew the relationship had been frustrated. When they were still friends, the man had given her a yellow-green cigarette box. Croiset's statement about her turning up her nose at this object may have symbolized her indignation about the letter.

To me, one of the impenetrable mysteries in the history of psychical research is the fact that Professor Bender, one of the best known and influential parapsychologists in the world, has been telling us time and again since 1957 that this particular experiment provides us with compelling, astonishing evidence in favor of precognition.

As far as the experimental procedure is concerned, this chair test is a classical example of how psi experiments should *not* be conducted.²¹ No one had bothered to specify in advance the criteria for success or failure. Croiset had predicted that the woman to be seated in chair no. 73 lived near a red building with tall columns. A woman who was not sitting in chair no. 73 denied that this was the case. Yet Bender hails a striking hit. A further flaw is that the person who was asked to interpret Croiset's statements knew, prior to the "verification," that she was meant to be the target person. In 1953, the phenomenon of "personal validation" was well known. Soothsayers had taken advantage of it for centuries. Four years previously, Scott had extensively dealt with this pitfall.²²

There are also problems with the reporting. Bender has never published a full account of the "verification." He confines himself to a summary of the comments the target person had given not only during the actual experiment, but also during evaluation sessions which took place for several days afterwards. He hardly gives any information about possible differences in the various sets of answers. Nor does he tell us under what circumstances and as a reaction to what questions the young woman offered her comments.

21 In some later chair experiments, pioneered by Bender and Timm, far more refined methods were employed. A re-appraisal of these experiments is expected to be published in 1982 or 1983 [eventually published as part of "Abschied von Pirmasens" in 1984; for an English version, see chapter 3-11 of this book. (Eds.)].

22 Scott, C. (1949). Experimental object reading: A critical review of the work of Dr. J. Hettinger. *Proceedings of the Society for Psychical Research*, 48, 16-50.

In his 1981 re-appraisal²³ Bender does not mention the fact that in 1960 the German author Carl Pelz, in an extensive critique of the Pirmasens experiment,²⁴ had published extracts from the actual tape recordings. From these it becomes clear that Croiset himself had been allowed to enhance his “hits” by putting suggestive questions to the young woman. The target person, for example, had not been able immediately to recognize her employer in the psychic’s statement about the man resembling Churchill. She only succeeded in her interpretation after Croiset had modified his original statement by saying that any “sanguine” would do and had suggested several possible matches.

I do not wish to insinuate that Professor Bender has withheld important information on purpose. He has proven his good faith by making tape recordings of the verification available to Herr Pelz, and also by inviting me to come to Freiburg and check all the relevant material.²⁵

What utterly surprises me is that he has apparently not realized that details such as revealed by Pelz are crucially important. Pelz – and here I fully agree with him – has further called our critical attention to the ambiguity of the individual hits. Croiset’s statements had been far from specific in the first place. Even so, the presumed target person could rarely confirm them if taken literally. The red building is a case in point. Croiset had suggested that the target person lived near such an edifice, that she came there often, that it looked somewhat decrepit and that it had a hedge. The woman who was sitting two seats from seat no. 73 in fact denied all of this. However, she was able to relate this statement to an “emotional experience” connected with a visit to a graveyard eleven years previously. Now the skeptic will reply that given this freedom to interpret psychics’ statements, almost any reading can be matched to almost any personal history.

Bender recognizes this argument, but he rejects it on the ground that the chair tests reveal a meaningful pattern of emotionally charged associations and combinations – “gestalt-characteristics” which “cannot be detected by taking the medium’s statements to pieces.”

23 This re-appraisal is basically a re-statement of Bender’s 1957 position. It mentions recent interviews with the target persons, both of whom are still impressed by the appositeness of Croiset’s 1953 statements. According to Bender, they “were able to provide additional, correlative information” which fails to astound me and which, in one instance, contradicts the information given in 1953.

24 Pelz, C. (1959). “Herr Croiset, Sie können nicht hellsehen! (1. Teil)” *Kosmos*, 55, (9), 377-383.

25 At the time of writing (March 1982) no opportunity had yet presented itself for me to go to Freiburg. However, I feel free to present my conclusions at this point as my “naturalistic” explanation of this miracle. It does not question the veracity of Professor Bender’s report but is based on a different evaluation of the undisputed facts. [Hoebens eventually was able to inspect the Freiburg material a few months later, in the fall of 1982. (Eds.)]

I fail to understand why this should be an argument against the skeptic's objections. Bender assumes that the "meaningful pattern" he detects in the target person's interpretation was already present in the psychic's original reading. This strongly reminds me of a legendary chief of police in a town in southern Holland whose son went to a psychologist, was given a Rorschach test and interpreted the blots and dots as representing a homosexual couple engaged in unmentionable acts. This caused the chief of police to have the psychologist arrested for showing pornography to minors.

The error is obvious, if not to the Dutch chief of police and to Professor Bender: They failed to take into account that people tend to project meaning into random stimulus configurations. The vaguer the target material, and the greater the freedom to engage in "symbolic" interpretation, the greater is the chance that something with amazing "gestalt characteristics" will emerge. This sort of psi, like beauty, is in the eye of the beholder.

There is one thing that can be said in favor of the classical Pirmasens experiment, however: It has turned out to be repeatable. In the autumn of 1981, I let it be known at the office of *De Telegraaf* that volunteers were sought for a serious experiment in psychometry. A psychic who had been in close touch with the Freiburg parapsychology institute was to provide paranormal readings intended to apply to the owners of some inductor object. The first two volunteers were each given a set of statements and an instruction sheet in which the rationale of such experiments was explained. They were not aware that they had been handed transcripts of the 1953 Pirmasens readings.²⁶

The results of the replication were even more striking than those of the original experiment. I did not even need to invoke the useful displacement effect. The second set of statements had clearly been intended to apply to someone living in Germany – with references to hills which are not found in our flat little country. Nevertheless the target person had little trouble in finding enough emotionally charged associations and combinations to fill a Dostoyevskian novel.

The first part, however, proved the biggest success. The target person was able to confirm every statement except one very marginal one. Her interpretation was considerably more literal than that of the original target person in Pirmasens.

I will restrict myself to those statements already referred to in this paper. The target person is a woman about 30 years old. Her chief can be described as the prototype of a "sanguine." Croiset's description of the building with the columns immediately reminded

26 In one instance, I slightly changed the original statement. Croiset had predicted that the target person had read something about the Polish province of Silezia. In the autumn of 1981 this would have been too obvious.

her of two edifices: the post office next door to which she lives and where she goes often, and the office of *De Telegraaf* where she works daily. I had not even realized how strongly Croiset's statement applied to the building where I too go almost each day: a red building with very prominent columns and an enormous flight of steps. In contrast to the original target person, mine recently had many emotional experiences in a delicatessen shop. She is fond of the goodies found there, but recently had decided that she must go on a slimming diet with the result that most of her favorite delicacies were forbidden to her. So each time she passed the delicatessen shop she suffered all the emotional torments of the dieter. However, she continued to frequent the shop to buy low calorie tit-bits and also to buy boxes of dates for her mother, who relishes them. In Croiset's description of the young man, my target person effortlessly recognized her recently-acquired boy friend. Age, attire and color of hair fit completely. This young man had planned to go to America. He had to cancel his trip because he met the girl and fell in love with her. As far as the cigarette box is concerned: My target person is a non-smoker who cannot stand tobacco smoke and is irritated by the fact that her colleagues on adjacent desks smoke like chimneys.

One does not have to be a psychologist to detect the "gestalt characteristics" of this interpretation. The individual comments are all meaningfully related to the target person's life and preoccupations. Nor does one have to be a radical goat to wonder what is "paranormal" about a psychic reading that turns out to be applicable not only to a young woman in 1953 Pirmasens but also – and to an even stronger degree – to a young woman in 1981 Amsterdam.

The Pirmasens episode, of course, is no more than a minor skirmish in the ongoing debate between proponents and critics of psychical research. I wish to repeat my firm opinion that the matter will not be settled by disputes over the merits of individual anecdotes. However, if a prominent parapsychologist allows himself to claim that the Pirmasens miracle is of such a quality that any unprejudiced observer could not fail to be astonished by it, then surely a critic should be allowed to cite this very case as exemplifying his reasons for placing his bets in the Hume Game on the skeptical horse.

CHAPTER 2-03

Editorial Introduction

Philosopher of science Paul Feyerabend, during the summer term of 1984, organized and directed at the Eidgenössische Technische Hochschule (ETH) Zurich a series of eleven lectures and panel discussions centering around various “Grenzprobleme der Wissenschaften” [Problems of Scientific Frontiers]. In addition to topics such as “Reducing Psychology to Neurophysiology?,” “The Epistemological Significance of High Energy Physics,” “Conceptual Foundations of Science,” “Perinatal Medicine,” “Astrology,” “Love and Death in Science and the Arts,” and “Feminist Science,” there also was, on May 24, 1984, a session on “Parapsychology.” In order to stimulate controversial discussion Feyerabend invited, for each group of lectures with a subsequent panel discussion, three or four presenters who were known to hold opposing or supplementary views.¹ Hans Bender, Eberhard Bauer and Piet Hein Hoebens were invited for and participated in the session on parapsychology. Feyerabend also contributed a short presentation on “What does it mean to be scientific?”²

Hoebens presented his paper in English. It was subsequently translated for the published Proceedings by Feyerabend’s co-editor Christian Thomas. Since Thomas’ translation contains several nontrivial errors, which distort the meaning of one of Hoebens’ major arguments, the following version of the lecture is exclusively based on Hoebens’ original manuscript, which is preserved in the Hoebens Files. The published German version also contains a postscript with Hoebens’ commentary on a Japanese video film on a Croiset prize case that Hans Bender had presented in Zurich. Since, to be fully appreciated, that postscript requires at least some acquaintance with the film and Bender’s rendering of it, it is not included in the following reprint.³

1 The complete series of 43 presentations was subsequently published, in German, in Feyerabend, P.[K.], & Thomas, C. (eds.), *Grenzprobleme der Wissenschaften*. Zürich: Verlag der Fachvereine an den Schweizerischen Hochschulen und Techniken, 1985; for a detailed review, see Hövelmann (1986).

2 Feyerabend’s paper was published as „Was heißt das, wissenschaftlich zu sein? [What does it mean to be scientific?],“ *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 26, 1984, pp. 58-64. Eberhard Bauer’s contribution, characteristically entitled “Song between the stools – how does it feel to be a ‘parapsychologist’?”, appears on pp. 367-374 of the Feyerabend & Thomas volume, Hans Bender’s paper on “The scientific methods of parapsychology” is included on pp. 359-366.

3 Hoebens’ paper appears on pp. 375-383 of the Feyerabend & Thomas volume; its main part, which here is published in English for the first time, is from pp. 375-381.

Paul Feyerabend had set in advance one guiding question for each of the eleven events at the ETH Zurich. The guiding question for the May 24 session on parapsychology read: "Which objections might a proponent of strict scientific rigor [*strenge Wissenschaftlichkeit*] raise against parapsychology, and how does parapsychology respond?" (Eds.)

WRONG QUESTION!

With due apologies to my generous hosts I feel compelled to say that the Eidgenössische Technische Hochschule has gone to considerable expense to have us come to Zurich and answer the wrong question.

The problem is not that this question includes the controversial term "*strenge Wissenschaftlichkeit*" [strict scientific rigor]. I do not know what "*strenge Wissenschaftlichkeit*" means *exactly*, but I venture to insist that some forms of intellectual activity are more rigorously scientific than others, and that we may differentiate between research programmes on that criterion without running the risk of being overly dogmatic.

The problem is rather that the question includes the term "parapsychology" in such a way that the reader gains the impression that we are discussing a well-defined discipline that can be successfully or unsuccessfully criticized from the vantage point of "*strenge Wissenschaftlichkeit*." This impression would be completely erroneous. At its present stage, parapsychology is not, and does not claim to be, "a science" in the usual meaning of that word: i.e., a well-tested apparatus for solving puzzles and problems. Parapsychology is defined in terms of the questions it asks, not in terms of the answers it gives.

Now it seems obvious to me that "*strenge Wissenschaftlichkeit*" is meant to refer to the appropriateness of certain methods of inquiry and evaluation, *not* to the question whether it is likely that the application of these methods to any specific problem will result in extraordinary findings.

If we accept the original definition of "parapsychology" – parapsychology denotes an interest in certain questions and does not imply a commitment to a certain type of answers – then it logically follows that proponents of "*strenge Wissenschaftlichkeit*" cannot object to parapsychology without resorting to the use of arguments *not* derived from the doctrine of "*strenge Wissenschaftlichkeit*."

It is a very different matter that orthodox scientists may assign a very low a priori probability to the parapsychological programme eventually proving successful in terms

of important novel discoveries, and for that reason object to its being supported by academic institutions. That is a political and economical issue, and the argument itself is extra-scientific. If you are interested in my personal opinion: Although I doubt that “the paranormal” exists, I find parapsychology sufficiently intriguing and potentially important to cast my vote in favor of its being adequately funded – preferably at the expense of those neo-Stalinist indoctrination programmes currently being presented as “science” at some of our universities.

Now allow me to re-state the question to something like this: Can mainline science successfully challenge claims that parapsychological research has *already* yielded results that would compel the unprejudiced observer to admit that there is more in Heaven and Earth than is dreamt of in Professor Prokop’s philosophy?

Now if I insist that the answer is an almost unqualified “yes,” I am not reflecting skeptical prejudices. I am merely taking seriously what parapsychologists have traditionally claimed about their own field.

“Psi” is defined in terms of mainline science’s inability to account for it. It is an unknown, a residue that remains after the conventional methods of investigation and evaluation have been applied to certain puzzling phenomena without yielding a satisfactory solution. The authority of scientific orthodoxy is implicitly acknowledged in the traditional definition of “psi”: a phenomenon explainable in conventional terms *ipso facto* is not a psi phenomenon. “Psi” has no characteristics that would enable one to identify it positively whenever it chooses to manifest itself. It is defined negatively. It is not an explanation, but rather the exclusion of any other explanation.

This state of affairs has certain implications of a logical rather than methodological nature. Psi can only be demonstrated in a given instance if it is demonstrated that no conventional explanation applies.

Now here the proponent of a psychic claim is faced with a tough problem. He must be able to exclude any conceivable “naturalistic” counterhypothesis. This, however, will remain a virtual impossibility as long as all the evidence he has consists of reports of singular events in the past. There are innumerable ways that such evidence might be flawed without the flaws being always detectable at this point in time.

For this reason, sensible parapsychologists such as Martin Johnson, Stanley Krippner and Gerd Hövelmann have always warned against relying on so-called evidence from “spontaneous” cases or from data obtained in quasi-experimental settings, such as the work with Gerard Croiset and other so-called psychic detectives. “Evidence” of that type may seem quite striking, but it cannot possibly support the case for “psi.” By extolling the

virtues of the Croiset material Professor Bender will not succeed in convincing the critics – but he certainly will succeed in embarrassing his fellow-parapsychologists.

To a certain extent, the same objection applies to the experimental evidence in parapsychology. Although with that type of evidence the conditions under which the presumed miracle occurred are to a considerable extent known and controlled, the essential unrepeatability of the findings legitimize critical questions as to whether the claimed results might be in fact due to some sort of artifact. As long as this problem remains unsolved it remains a valid option to assume that the “paranormal” is merely a mis-identification of the non-paranormal.

Am I now attacking parapsychology? To the contrary: I have just been paraphrasing what leading parapsychologists have been saying for years. If skeptics claim the right to criticize parapsychological work from the vantage point of “*strenge Wissenschaftlichkeit*” it is because parapsychologists have traditionally accepted the skeptical criteria, either explicitly or implicitly.

This is one of the features that distinguish parapsychology from many of the New Age “sciences.” Mainline parapsychology looks for potentially revolutionary findings without resorting to revolutionary methods. The most frequently cited experimental evidence in psi research is based on the assumption that the orthodox concept of statistics is correct, which logically implies that the parapsychologists acknowledge the legitimacy of criticisms based on statistical orthodoxy. There is nothing particularly esoteric about the way parapsychologists criticize each other’s work in their professional journals: They employ essentially the same arguments as found in the writings of outsider skeptics. As a matter of fact, it is often difficult to differentiate between insider and outsider criticism in this field.

One of the most encouraging developments of recent years has been the emergence of a moderate center in the public debate on the paranormal. A center which includes both parapsychologists and outsider skeptics who, while they may disagree on intuitive grounds as to the most likely outcome of the search for “psi,” to a considerable extent agree on the criteria on the basis of which the success or failure of the parapsychological enterprise should eventually be decided. I am happy to say that even the controversial CSICOP, often accused of indulging in an inquisitorial crusade against deviant science, has now become far more moderate and cooperative than it was a few years ago and is now in the process of forming a parapsychology sub-committee which at least two respected parapsychologists have agreed to join.

Of course I know perfectly well that the very notion of “*strenge Wissenschaftlichkeit*” itself is controversial, and that, when challenged to make explicit their criteria for demar-

cating science and non-science, scientists will happily contradict each other and themselves.

However, the quarrels among the philosophers of science are only partly relevant to the discussion on the acceptability of parapsychology. Parapsychology is *not* incompatible with the orthodox notion of “*streng*e Wissenschaftlichkeit,” whereas a rejection of that orthodox notion would imply a rejection of the better part of modern parapsychology.

The problem is not that a group of outsiders wish to impose on parapsychology a rigorous methodology that is alien to the field’s own tradition. After all, some of the most uncompromising proponents of “*streng*e Wissenschaftlichkeit” come from within the parapsychological community.

Rather, the problem is that the conventional proto-science named “parapsychology” has certain historical, sociological and personal links to anti-conventional occultism and that the two traditions overlap to a degree that is puzzling and confusing to the casual observer. Occultism is a revolt against what it feels to be the oppressive regime of scientific rationalism and wishes to replace it with a cognitive system believed to be superior.

Occultism does not reject science *per se*: Science is accepted as an *ancilla doctrinae occulta*e, as a way of thinking that is often highly successful in its attempts to understand the lower aspects of reality, and sometimes may even be able to provide the ladder by which Man may reach the more elevated spheres of Higher Insight. It is in this context that we must appreciate the role of “scientific demonstration” in occultism: It is not meant to demonstrate the truth or falsity of a hypothesis but to illustrate a Truth which transcends science. Science is graciously permitted to support, but is not allowed to overrule occult beliefs.

I suggest that the acerbity characterizing much of the public debate on parapsychology becomes more understandable if we realize that several thinkers and researchers who label themselves “parapsychologists” are wholly or partly, openly or secretly, occultists. Skeptics (including skeptical parapsychologists) often find it an exasperating experience to engage in debate with such “parapsychologists,” as the latter seem to implicitly claim that the truth of their beliefs can be conclusively demonstrated within the framework of traditional science, yet when confronted with traditional scientific objections to their evidence change the rules of the game and seek cover under the umbrella thoughtfully provided by philosophers of science who challenge the pretensions of scientism.

The ensuing exchanges of insults are often highly entertaining, but they produce more heat than light and have the regrettable side-effect of providing a welcome excuse to lazy skeptics who for reasons of their own prefer to reject parapsychology in toto.

Of course, the scientific parapsychologists may be completely misguided, and their rejection of occult intuition as a reliable source of knowledge may doom their programme to failure. Perhaps a future lecture series could include a session on the question: “Which objections might a proponent of the occult *Weltanschauung* raise against rigorously scientific parapsychology, and how does the latter respond?”

But that is a question very different from the one we are discussing this afternoon.

CHAPTER 2-04

Editorial Introduction

The following article is a “first” in several respects. It was Hoebens’ first scientific publication in the German language; formally laid out as an essay review, it was his first major published critique of Professor Tenhaeff’s research and his standards of reporting; it was the first time he discussed the semi-experimental “chair test” design at some length (as he would do much more extensively on later occasions); and it basically was the first instance where Hoebens clearly spelled out his position of determined, but open-minded skepticism and at the same time separated himself from the kind of skepticism “practiced by some of my nominal friends.”

So far this essay was publicly available only in German.¹ It is reproduced here from the original English manuscript preserved in the Hoebens Files and takes into account some minor revisions that were made during the translation and editing processes. There are a number of original German quotes throughout the text, because Hoebens’ was writing for German readers. Since most readers of the present book, presumably, will not be sufficiently familiar with the German language, we have added English translations. (Eds.)

IN PRAISE OF METICULOUSNESS IN PARAPSYCHOLOGY

An essay review of the books *De Voorschouw – Onderzoekingen op het gebied van de helderziendheid in de tijd*. 4th, revised ed. (Den Haag: Leopold, 1979, 238 pp.) and *Ontmoetingen met paragnosten*. (Utrecht: Bijleveld, 1979, 224 pp.) by Wilhelm H. C. Tenhaeff.

In his review of Tenhaeff’s *Der Blick in die Zukunft*, W. P. Mulacz² noticed a factual discrepancy between the work under review and Tenhaeff’s contribution to the Schatz handbook.³ The discrepancy was quite minor, but Mulacz emphasized the need to avoid even

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- 1 Hoebens, P.H. (1980). Vom Lob der Genauigkeit in der Parapsychologie. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 22, 225-234. Although this is not mentioned in the published version (which may have seemed advisable for “political” reasons at the time), the text was translated into German by Eberhard Bauer.
 - 2 Mulacz, W.P. (1979). Rezension. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 21, 251-254.
 - 3 Tenhaeff, W.H.C. (1976). Zur Persönlichkeitsstruktur der Paragnosten. In Schatz, O. (ed.), *Parapsychologie. Ein Handbuch* (pp. 109-132). Graz, Wien u. Köln: Styria.

trivial mistakes, as they might make the reader distrustful of the author's work in general. Besides, such mistakes might be taken advantage of by mischievous skeptics. Mulacz's point was well made. Parapsychology simply cannot afford too many lapses, not only for reasons of public relations but also for reasons more closely pertaining to the substance of psi research. Parapsychology being basically an attempt to make scientific sense of a great number of odd and confusing data, it would be a pity if this attempt was partially wasted on spurious "facts."

The error Mulacz pointed out in Tenhaeff's work was insignificant and should in no way affect our opinion of the dean of continental psychical research. The apparent discrepancy may even have been due to a mis-translation, Tenhaeff's Dutch being somewhat recondite.

In the meantime, a new and completely revised Dutch edition of Tenhaeff's *De Voorschouw* has been published by Leopold in The Hague (1979). I understand this edition to be substantially identical to the German version published as *Der Blick in die Zukunft*. The revised *Voorschouw* was shortly followed by an entirely new work, *Ontmoetingen met paragnosten* (Utrecht: Bijleveld, 1979). This latter work was reviewed in the Dutch *Tijdschrift voor Parapsychologie*⁴ in almost ecstatic terms. The reviewer, W. de Roy, praised the author for "truthfully describing his experiments, reporting objectively both 'hits' and 'misses' without wanting to 'prove' anything."

In the present essay review of the two books, I will ignore Tenhaeff's theoretical speculations, concentrating instead on the way he presents the bare facts. I think this is of prior importance, as a questionable theory does not affect "good facts" while spurious factual material very much undermines any theoretical structure built on it. Tenhaeff basically seems to agree with this approach, as he personally told me: "My theories will probably be superseded in fifty years time, but the material in my files will survive."

Checking all the material presented in *Voorschouw* and *Ontmoetingen* would require years of intensive work, so I had to restrict myself to a sample.

While the error noted by Mulacz in the German translation of *Voorschouw* was not apparent in the Dutch version (here the text is consonant with what is reported in the Schatz volume), I unfortunately discovered a rather disconcerting number of other inaccuracies, omissions and discrepancies of a less trivial nature. I will present a few examples in some detail.

On pp. 96-99 of *Voorschouw*, Tenhaeff describes a very successful "chair test," the one conducted in Verona on March 3, 1956. Taken at face value, this was one of the most successful of all chair tests in the late Gerard Croiset's long career. The target person sitting in

⁴ Roy, W. de (1979). Boekbespreking. *Tijdschrift voor Parapsychologie*, 47, 158-159.

the chair indicated by the clairvoyant, Miss Rita Venturi, confirmed every single item in Croiset's reading. Some of those items were quite specific. The psychic, for instance, had "seen" the target person owned a drawing of a squirrel, which in fact she did.

Now, Tenhaeff must have known that in the *Journal of the Society for Psychological Research* Zorab,⁵ in his review of Pollack's book,⁶ singled out this experiment as "a beautiful example of how badly some of these tests have been conducted." Zorab had discovered that the circumstances surrounding the alleged success were highly suspicious. According to the protocol, only invited persons would be allowed to participate. Miss Venturi, however, was never invited. She was the only non-invited person present, having more or less forced her way into the audience hall.

Zorab points out that, at that time, Croiset, as a "magnetic healer," had formed a large clientele in Italy. If he had wanted to "plant" someone on the target chair he would have had an ample reservoir of potential confederates. Zorab does not actually claim Croiset resorted to trickery in that instance, but he made plain that the psychic "had every opportunity to do so" which would be more than enough to make the Verona experiment scientifically worthless.

In *Voorschouw* Tenhaeff does not even mention Zorab's criticism. His readers are left with the impression that the Verona chair test was quite flawless. Tenhaeff may have reasons to disagree with Zorab. If so, he should have attempted to refute him publicly. There is no justification whatsoever for flatly ignoring such highly damaging remarks, particularly as they were made by a psychical researcher of international stature. Tenhaeff's silence on this point has only served to increase my doubts about the Verona case.

On pp. 109-112 of *Voorschouw* Tenhaeff describes another "classical" chair test, the one conducted by him and Bender in Pirmasens on June 3, 1953. This case was reported by Bender in the present *Zeitschrift*.⁷ The way the two parapsychologists report the "verification" is identical – almost.

Croiset had "seen" the target person would be a young woman in a white blouse who "often wears a vest made of angora wool." The woman identified as the target person did not sit in the chair indicated by Croiset, but this was explained as a "displacement effect" by the experimenters.

5 Zorab, G. (1965). Book review. *Journal of the Society for Psychological Research*, 43, 209-212.

6 Pollack, J. H. (1965). *Croiset the Clairvoyant*. London: W.H. Allan.

7 Bender, H. (1957). Praekognition im qualitativen Experiment. Zur Methodik der „Platzexperimente“ mit dem Sensitiven Gerard Croiset. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 1, 5-35.

The angora vest was not much of a hit. The presumed target person admitted to owning one, but she denied she wore it very often. However, she remembered having opened her closet that day and having wondered if it might not be wiser to wear her warm yellow jacket that night. As the angora vest was on the same shelf as the yellow jacket, she had actually cast a glance at the garment supposedly “seen” by Croiset.

Bender’s résumé of the “verification”⁸ is as follows: “Als sie sie (die weiße Bluse; P. H. H.) aus dem Schrank nahm, sah sie ihre Weste aus Angora-Wolle und überlegte, ob sie sich wärmer anziehen sollte. [When she took it (the white blouse, that is; P. H. H.) out of the closet, she saw her angora wool vest and asked herself whether or not she should choose warmer garment.]”⁹ This is basically correct, although Bender might have pointed out that the warmer garment the woman considered wearing that night was not the angora vest. Tenhaeff, however, gives a version much more suggestive of at least a partial ESP hit: “As she took the blouse from the closet her eye fell on her angora wool vest. For one instant, she was about to put it on, as she feared she might be cold.”

In the same chair test, Croiset had wondered about the target person: “Hatte sie eine leichte Infektion an der rechten großen Zehe? [Did she have a slight infection in the big toe of her right foot?].”¹⁰ The “verification” according to Bender: “Am linken, nicht am rechten Fuß hatte sie kürzlich eine kleine Infektion [She recently had a slight infection on her left foot, not on the right one.]”¹¹ The “verification” according to Tenhaeff: “Shortly before, she had had a slight infection on both feet.” From the almost exact correspondence of the Dutch and the German texts it is obvious that Tenhaeff translated the verification part from Bender, or at least based himself on the same written source. Given Tenhaeff’s proficiency in German it is difficult to account for the two errors in translation, both of which amount to an “improved” version of the “hits.”

The most serious deficiencies I regretfully have to report concern Tenhaeff’s account of the “transatlantic chair test” in the two books under review. The experiment was conducted in Denver, Colorado, by Dr. Jule Eisenbud on January 23, 1969. Two weeks before, Croiset, in Utrecht, had given two series of statements meant to apply to two persons as yet unknown who would be chosen by lot from a group to be assembled in an audience hall in Denver. Both in *Voorschouw* and in *Ontmoetingen* Tenhaeff claims this chair test

8 Parts of the original transcripts were published in Pelz, C. (1959). “Herr Croiset, Sie können nicht hellsehen!” Der große Irrtum der Parapsychologie. (1. Teil). *Kosmos*, 55, 377-383.

9 Bender (1957, S. 259), see note 7.

10 Bender (1957, S. 258), see note 7.

11 Bender (1957, S. 260), see note 7.

was “very successful.” Indeed, from his reports the reader must conclude it was one of the most impressive ever.¹²

Almost by accident, I discovered that Eisenbud’s original report of the experiment had been published in the *Journal of the American Society for Psychical Research*.¹³ As Tenhaeff does not mention Eisenbud’s extensive article in any of his Dutch publications I had simply assumed it to be non-existent. From a detailed report¹⁴ I prepared for publication elsewhere I will take a few examples.

In *Voorschouw*, Tenhaeff states that Croiset “gave 21 precognitive statements in all.” He presents only a selection, but refers to an “exhaustive and detailed” discussion of the transatlantic test in the *Tijdschrift voor Parapsychologie*.¹⁵ There, we indeed find 21 statements, numbered 1-10 (for the female target person) and 1-11 (for the male). All this clearly implies that Tenhaeff claims his 1969 account contains the complete reading Croiset gave in Utrecht. However, in Eisenbud we find no less than nine additional statements intended to apply to the target persons and which are not incorporated in any way in Tenhaeff’s list. Most of these statements, omitted from all Dutch accounts, turned out to be quite wrong.

Tenhaeff gives a completely garbled account of the way the “verification” was conducted in Denver. He fails to mention the important fact that the presumed target persons dramatically increased their “confirmation score” at the successive stages of the “verification,” which suggests a possible non-paranormal explanation for at least a number of the apparent hits.

For the female target person Croiset had made the statement: “Did she recently experience an emotion connected with page 64 of a book?” According to Tenhaeff, Mrs. Olinger of Denver, whom he identifies as the female target person, confirmed this. She had bought a book about cats for her daughter living in Japan. However, she withheld

12 Apparently, the Denver experiment is one of Tenhaeff’s show cases. In his memoriam article on Croiset in *Esotera*, 32, 1980, pp. 816-827, he presents this case as “an example” of “a procedure ... which, in every respect, has firmly withstood the scrutiny of just scientific criticism” (p. 823).

13 Eisenbud, J. (1973). A transatlantic experiment in precognition with Gerard Croiset. *Journal of the American Society for Psychical Research*, 67, 1-25.

14 Hoebens, P. H. Comparisons of reports of the ‘Denver’ chair test: A critical examination of the methods of W. H. C. Tenhaeff. *Journal of the Society for Psychical Research*, 53, 1986, 311-320. [Published posthumously; see chapter 3-07 (Eds.)]

15 Tenhaeff, W. H. C. (1969). Experimentele voorschouw. Verslag van een tweetal te Denver (Colorado) geverifieerde stoelenproeven. *Tijdschrift voor Parapsychologie*, 37, 89-96.

the book because of a passage about the need to put old and sick cats to eternal sleep. She feared that passage might upset her daughter, as the Olinger family cat had recently been the recipient of euthanasia. The offending sentences were indeed on page 64. In *Voorschouw*, Tenhaeff writes: “At the meeting she could not remember the page number, but as soon as she got home she checked and found that this advice was on page 64.”¹⁶ This strongly implies that Mrs. Olinger during the meeting remembered the book (and said so) only to find out later that the emotion was indeed connected with page 64. This would certainly have been an impressive hit. Eisenbud, however, makes plain that Mrs. Olinger did not remember the book at the evening of the experiment. On the questionnaire all participants had to complete at that point she indicated the item “did not apply.” The first time she mentioned that particular book was on a duplicate questionnaire which she had taken home and mailed back to Eisenbud later. This makes the hit far less interesting, as in the meantime she had had the opportunity to search her library for a page 64 with sufficiently “emotional” material. In Tenhaeff’s account, the order of events is subtly changed, giving a much stronger intimation of paranormality than the actual “hit” warranted. Oddly enough, in his contribution to the Schatz handbook,¹⁷ Tenhaeff got his facts right on this point. The discrepancy is not only between Tenhaeff and Eisenbud, but also between Tenhaeff writing in Dutch and Tenhaeff writing in German.

I must also mention an article by Tenhaeff in the Dutch *Tijdschrift voor Parapsychologie*¹⁸ based on a lecture read at Imago Mundi conferences in Genoa and Königstein where a yet more startling version of the page 64 case is given: “[...] Croiset named the page of a book, which page contained a remark that caused the target person not to send this book to her daughter. At the verification it turned out that the target person had indeed withheld her book, bought for her daughter, because the content of page 64 could have reminded her of the loss of her cat [...]” Here, part of Mrs. Olinger’s comment has become part of Croiset’s original statement!

In *Ontmoetingen*, Tenhaeff relates another hit rather suggestive of ESP. Of the male target person, Croiset had remarked: “Does this gentleman have green socks with a hole in one of them?” According to Tenhaeff, Mr. Tuck from Denver, whom he identifies as the male target person, had commented: “On the evening of the experiment, I wore green socks. When I came home I discovered there was a hole in one of these socks.” Eisenbud

16 In the German edition this sentence is missing. Mulacz (see note 2, p. 253) notes that some cuts were made by the publisher of Tenhaeff’s book.

17 Tenhaeff, W.H.C. (1976, pp. 122-123), see note 3.

18 Tenhaeff, W.H.C. (1973). Anthropologische parapsychologie. *Tijdschrift voor Parapsychologie*, 41, 1-23.

gives a completely different version. According to the *JASPR* report, Croiset had referred to wearing green socks “last evening,” whereas Mr. Tuck had commented: “Some time before the meeting I do remember wearing a pair of green socks to work. There was a hole in one sock, in the heel.” I should add that Mr. Tuck at the evening of the experiment stated that this item “did not apply.” He “discovered” the match only in later stages of the “verification.”

In *Ontmoetingen*, Tenhaeff states explicitly that both Mrs. Olinger and Mr. Tuck were sitting in the chairs “indicated by Croiset.” From Eisenbud’s original account it becomes clear that this certainly was not the case. Croiset did not indicate any chairs at all. He gave statements intended to apply to persons who would be chosen by lot (the weather-key method was used). Mrs. Olinger and Mr. Tuck drew the wrong numbers. However, after the experiment was over, an error in the randomization procedure was discovered. Tenhaeff had instructed Eisenbud to dispense “24 white cards numbered 10-34.” From the holders of these tickets, the target persons would be selected by lot. There was a contradiction in the instructions given by Tenhaeff (who had remained in Utrecht), as 24 cards cannot be consecutively numbered 10-34, but only 10-33 or 11-34. Eisenbud had dispensed cards numbered 10-34, so a total of 25. Once such a mistake has been made, nothing can be done to correct it, as any “correction” would be arbitrary. However, Eisenbud (or someone else connected with the Denver side of the experiment) found that Mrs. Olinger and Mr. Tuck would have held the winning tickets if either no. 10 or no. 34 (one of which must have been the superfluous number if only 24 cards should have been used) were taken from its place in the original order after the shuffling had been carried out with 25 cards. Other, more logical attempts at a “solution” (like reshuffling the cards and trying again) did not yield Mrs. Olinger and Mr. Tuck as the target persons. Because the instructions had been contradictory in the first place, however, no satisfactory solution was possible, as Eisenbud clearly realized.

In the two books under review, Tenhaeff does not even mention the problem. In his 1969 report, he does devote a passage to the error, insisting that it was corrected “in time” and unequivocally. There, however, he claims Eisenbud had clearly been instructed to dispense 24 cards “numbered 11-34,” so denying the essential fact that his own letter of instruction (printed verbatim in Eisenbud) contained a contradiction and so largely invalidated the experiment.¹⁹ It is quite obvious that, particularly in reporting the Denver case, Tenhaeff made grave and embarrassing mistakes. I will not speculate about the most likely explanation here, but I do think the facts presented above are sufficiently serious to

19 On p. 147 of *De Voorschouw*, Tenhaeff states that 24 participants were given a card; in the German edition the number of ticket owners has risen to 25.

raise some questions about the accuracy of Tenhaeff's reporting in general. Errors of this order – even if they were committed in good faith – are very misleading.

Mulacz has conjured up the specter of the malicious skeptic discovering precisely such inaccuracies in a noted parapsychologist's work and using those findings as ammunition in a crusade against parapsychology. He may feel somewhat uneasy at the thought that the author of the present review is a registered skeptic and an active member of the Dutch section of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP) – a committee suspected of terrorist anti-psi activities by quite a number of psi proponents.

To avoid misunderstandings, a short "position statement" may be useful. Although my personal reaction to the various claims of the paranormal is generally one of polite disbelief I hold parapsychology to be a perfectly legitimate, and possibly important, field of scientific inquiry. It is quite easy to point to the serious flaws in the work of individual workers, but any attempt to "debunk" the whole field should take into account the activities of those proponents who are no less critical than the official "skeptics." Parapsychologists cannot collectively be blamed for the sins of some of their colleagues. "Guilt by association" is a criminological concept belonging to the era of the "Hexenhammer." I certainly would object if I were to be held responsible for the "skepticism" practiced by some of my nominal friends. It is only reasonable that parapsychologists, *mutatis mutandis*, would feel the same.

CHAPTER 2-05

Editorial Introduction

On May 5, 1984, Veritas, a well-known Dutch students' association, organized a one-day public symposium in Utrecht. The topic to be discussed by five invited speakers was "Ontevreden met de rede. Wetenschap, maatschappij en het paranormale denken [Dissatisfied by Reason: Science, Society and Paranormal Thinking]." Invited speakers and panelists were Prof. Henri van Praag (Tenhaeff's successor to the Special Chair of Parapsychology), journalist and magician Dirk Minnebo, social philosopher drs. Harry Kunneman, Piet Hein Hoebens, and American parapsychologist Dr. John Palmer who at the time was a Visiting Researcher at the Parapsychological Laboratory of the University of Utrecht. Lengthy Dutch abstracts of the presentations (and an English abstract of Palmer's talk, respectively) were published by Veritas in the same year.¹

*A full English version of his presentation that Hoebens had written around the same time was only published, with some very minor editorial revisions by Brian Millar, four years after his death in the undeservedly short-lived journal *Theoretical Parapsychology*.² This is the version that is reprinted below. In an "editor's introduction," Millar noted at the time: "Hoebens dignified the title of skeptic by his openness to new evidence. The paper published here for the first time encapsulates his thought."³ (Eds.)*

IS INTEREST IN THE PARANORMAL AN ATAVISM?

Abstract

Public discussion about the paranormal is dominated by the twin poles of traditional kinds of belief and skepticism. Neither of these views is adequate. As to whether psi exists, this paper concludes with Rushton that the evidence is too weak to be believed and too strong to be rejected. The roles of parapsychologist and skeptic overlap.

1 The abstract of Hoebens' paper appeared under the title „Is de belangstelling voor het paranormale een regressieverschijnsel?“. In Studentenvereniging Veritas (ed.), *Ontevreden met de rede. Symposium over wetenschap, maatschappij en het paranormale denken* (pp. 54-56). Veritas, 19e lustrum. No place [Utrecht]: Veritas, 1984.

2 Hoebens (1988).

3 Millar (1988, p. 1).

As to the social implications of the occult, the article attacks Voltaire's dictum that man will only stop committing atrocities when he has given up believing in absurdities. The clinching argument is that people are not monolithic creatures: they are credulous in very specific areas. Widespread belief in the occult thus presents little or no danger to society at large. But it does pose a great and serious danger to the parapsychological world. Any parapsychologist who declares that the reality of the paranormal is proven beyond doubt and in strict scientific fashion reveals that he is credulous upon just that territory where it is his task to be critical.

I propose that public discussion about the occult, the paranormal, is dominated by two fixed poles. The first pole is that the paranormal is a deeper dimension of reality, which is neglected or even denied by materialistic science. The second pole is that belief in the supernatural is a typical phenomenon of regression: a desperate attempt to defend, against the blessed light of reason, a magical world view to which people are attached on emotional or philosophical grounds. The first pole sees established science as the spiritual inheritor of the inquisitors who forced Galileo Galilei to recant his heretical views. The second pole sees parapsychologists, astrologers and flying saucer experts as a fifth column of the new middle ages: wizards and magicians in lab coats.

I dispute both views. I do not believe that the paranormal – if such a thing exists – is necessarily a deeper or a higher dimension, and I really become annoyed when I read about the cosmic implications of circus tricks such as those of Uri Geller and Gerard Croiset. But, on the other hand, I also have the tendency to fall flat on my back when I hear one of my fellow skeptics declare that the occult is nothing other than superstition and nonsense and that belief in telepathy, clairvoyance and flying saucers will ultimately lead to the downfall of rational western civilization.

My own opinion – and I value this view as being particularly reasonable – is that it is still quite uncertain whether the “paranormal” in the usual sense really exists; however the word “paranormal” circumscribes a complex of problems and puzzles that certainly deserve the serious and unprejudiced interest of the rational 20th century thinker. Furthermore, I am of the opinion that people can disagree with me without thereby automatically bringing western civilization into danger.

There are basically two ways to talk about the paranormal. We can regard the paranormal as a social phenomenon, and then we deal with the role that a particular sort of belief plays in diverse cultures, regardless of the possible truth of this belief. We can also regard the paranormal as a scientific and philosophical problem, and then we have to deal

with the question of whether paranormal phenomena exist, regardless of the popularity of belief in such phenomena.

I begin with the second approach. Question: does there exist such a thing as the paranormal? Answer: I don't know. It is often claimed that skeptics – and I am one – deny the paranormal *a priori*. Sensible skeptics do not do that. They see themselves in the role of lawyers who defend the interests of conventional science against the claims of a few self-assured newcomers. To be sure, every scientist is naturally well aware that, if phenomena such as telepathy and clairvoyance really exist, then it is in the interest of science that they should be recognized as soon as possible – but not before their genuineness has been established beyond reasonable doubt. What the skeptic contests, and that with great conviction, is the opinion that this proof has been long and richly delivered and that only blind materialistic prejudice prevents established science from admitting the fact.

Whoever reads the 1,000-page *Handbook of Parapsychology*, or several volumes of the *Journal of the Society for Psychological Research*, will only with difficulty be able to resist the impression that a century of organized parapsychological research has yielded an imposing quantity of evidence. Part of this material is also of such a quality that it is intuitively difficult to doubt it.

Nevertheless, careful study of the history, and critical analysis of the most representative literature, teaches us that there is still room for considerable doubt. The Achilles heel of parapsychology is and remains that the whole edifice of the evidence rests upon anecdotes, tales of remarkable happenings, that, according to eye witnesses, occurred at particular times. It is almost impossible, in retrospect, to establish whether these testimonies are in conformity with historical truth. There are two particular reasons for skepticism.

First, the phenomena reported seem to clash with what, for want of a better word, I shall call the laws of nature. This should not be taken too literally. I do not mean that laws of nature exist that *a priori* exclude paranormal phenomena. What I do mean is that phenomena such as psychokinesis and precognition (seeing the future) are difficult to reconcile with what the very pro-parapsychological British philosopher Broad called the “fundamental limiting principles” of science. The fact is that orthodox science, based on these limiting principles, is far from its last gasp: it succeeds rather well in describing the world without falling into problems that arise because the paranormal dimension is not taken into account. This argument suggests, *a priori*, that the occult should be approached with some suspicion. Anyone who has ever read a skeptical American or English book about parapsychology, will doubtlessly have come across the motto of the international skeptical movement: “Extraordinary claims demand extraordinary evidence.”

This brings me automatically to the second point – the strength of the currently available evidence. It is a full century since the founding of the famous Society for Psychical Research in Cambridge, which launched the systematic scientific study of so-called paranormal phenomena. In all this time parapsychology has never succeeded in laying the kind of evidence on the table that can be tested and found to be solid by even the most doubting outsider. In the whole of parapsychology there does not exist one “repeatable experiment.” In other words, no parapsychologist can tell the skeptic what precisely he must do (or not do) to observe even one trivial paranormal phenomenon.

Acceptance or rejection of the parapsychological evidence thus remains, in the last analysis, a question of trust or distrust. The skeptic is inclined to choose distrust. After all, the history of occultism and parapsychology shows that credulity, fraud and self deception have played a prominent role in shaping belief in the paranormal.

It is certainly not my intention to imply that parapsychology is one grand swindle. Researchers have been known deliberately to fake their experiments, and I have the impression that this plays a relatively greater role in parapsychology than in most of the established sciences. Nonetheless, I believe that those parapsychologists who have been caught faking the evidence – I name here Soal, Levy and Tenhaeff – are exceptions, and that the rule is formed by parapsychologists whose procedure conforms to their own norms of scientific integrity.

Honesty, in the sense of a subjective feeling of fidelity, does not, however, offer any guarantee of reliable research results. The bona fide parapsychologist will not play tricks himself, but he can well, without being aware of any problem, report cases in which he was fooled.

Much parapsychological work consists of testing so-called psychics and other individuals who declare themselves to be gifted with supernatural abilities. The history of psychical research teaches us that among these “paranormally” gifted there is a disproportionate number of brilliant fakes. Parapsychologists and other practitioners of science, however, lack the very specialized knowledge and experience necessary to be able to detect fraud based on conjuring tricks. And once the subjective conviction that a given wonder worker really has miraculous powers has taken root, then it is usual that the ability to detect indications of fraud rapidly declines. Not a few parapsychologists still believe that Uri Geller can really bend cutlery with pure mind-force and that Ted Serios is able to take polaroid photos of his own thoughts.

Bona fide parapsychologists are also continually exposed to the temptations of enthusiasm. Many of them have identified themselves to a considerable degree with the belief

that the paranormal really exists, and regard arguments to the contrary as existential threats. This again leads to the situation that they do not want so much to investigate the so-called paranormal as to defend and protect, or even propagate it. This mentality can have disastrous consequences for the quality of their work. Their enthusiasm inspires them to overestimate the strength of the positive evidence, but at the same time narrows their vision for alternative explanations of the material.

In this way even decent parapsychologists can come to make totally irresponsible statements about paranormal miracles. A British researcher, Manfred Cassirer, once beautifully characterized this phenomenon when he wrote: “The best way to deceive other people is to begin by deceiving yourself.”

These remarks might give rise to the misunderstanding that I want to paint bona fide parapsychologists collectively as a company of the well meaning but naive. However, this is decidedly not my intention. Such would also be in contradiction to the objectively observable reality, at least the reality observed by people who do not let themselves be misled by amateuristic and propagandistic writings.

Parapsychology is no sharply limited territory. The title “parapsychologist” is not a protected one. The same applies to the title “skeptic.” Here I do not want to argue about definitions. I only want to say that there is no clear historical division between parapsychologists and critics and that in many cases it is a question of personal taste whether a researcher considers himself a member of one group or the other.⁴ The important point is that among those who describe themselves as practitioners of parapsychology there are to be found people who think precisely or almost precisely the same about the matters treated above as the well informed skeptic. Conversely, among those officially registered as skeptics are to be found those who seriously defend the legitimacy of parapsychological research.

I shall give a few examples to illustrate the complexity of the situation that is so often seen in terms of a simple confrontation between believers and disbelievers. Three as inclusive as incisive criticisms of the pretensions of traditional parapsychology were published in the period 1982-1985 by three internationally known parapsychologists: Susan Blackmore, Gerd Hövelmann and Charles Akers. Blackmore’s attack on parapsychology was so radical that – believe it or not – thereafter parapsychology was defended by an active and highly visible member of the extremely skeptical CSICOP.

The journal of that committee, *The Skeptical Inquirer*, published a very positive review of the recent book by Professor Martin Johnson, Professor of Parapsychology at the

4 Also, cf. Hövelmann (1988). (Eds.)

University of Utrecht.⁵ Dr. Palmer, a worker at Johnson's institute and an internationally recognized parapsychologist, sees me, it would appear from a recent publication in the journal *Zetetic Scholar*, not as an opponent but as a colleague.⁶ When, a few months ago, there was a threat to the finances of the parapsychological laboratory at the University of Freiburg, the parapsychologists there received a formal motion of support from Professor Paul Kurtz, president of the skeptical society CSICOP. I could go on to give dozens of instances, but I think that these will suffice to convince you that the stereotype presentation of the parapsychologist as a credulous fool, and of the skeptic as a fanatical rationalist, is not reconcilable with the complicated reality of the current debate about the paranormal.

So much for science and magic; but what about the social implications of the occult? My opinions about this question can be summarized in what may seem a somewhat paradoxical proposition: widespread belief in the occult presents a great and serious danger for the parapsychological world, but none or hardly any for the rest of society.

In some radically skeptical writings, the following reasoning is to be found, a variant of the well-known statement of Voltaire that man will only stop committing atrocities when he has given up believing in absurdities. The reasoning is as follows: modern humane civilization is founded on a rational and scientific world view. Anyone who believes in irrational things is boring holes in the dam that illuminated spirits have thrown up against obscurantism. This was defeated by the scientific revolution, but it did not lay down its head and die: it waits, thirsting for revenge. It looks innocent enough to believe in horoscopes, flying saucers or paranormal healers; but those who do so switch off sound understanding and critical faculties and lay themselves open to irrational influences of a very much less innocent type. This is briefly the reasoning. The examples given are generally of the sort: the followers of Jim Jones became convinced of the divinity of the preacher. The result was massacre at Jonestown, Guyana. Or: astrologers, theosophists and clairvoyants undermined the critical faculties of the German people, for which reason they fell an easier prey to the pseudo-scientific racist theories of the Nazi movement.

It sounds very convincing, but is nonetheless fallacious, for a number of reasons. First, it seems to me totally incorrect *without more ado* to categorize belief in paranormal phenomena as "irrational belief."

Secondly, it should not be forgotten that the similarly widespread belief in established science is also often based on a totally erroneous idea of what science actually is. In

5 See chapter 2-11 of this book. (Eds.)

6 Palmer (1983, p.39 fn). (Eds.)

other words, science may well be a relatively rational enterprise, but her supporters often believe in this rational undertaking for the most irrational of reasons.

Thirdly, the reasoning can be reversed and used against the rational scientific approach. Belief in rational science could make many people susceptible to all sorts of culpable ideas dressed up in scientific guise. The scientific pretensions of communism have blinded numerous modern westerners to the abominable truth about Stalinism and Maoism.

Fourthly, I refuse to believe that irrationality and credulity are qualities that by definition characterize the whole of a man. On the contrary, I believe that in general such properties are compartmentalized. In other words, because someone is particularly gullible in one area does not say much about his credulity in another territory. I think that this is the crucial argument against the theory that occult magical thinking is an acute danger for rational modern society.

People are no monolithic creatures. They consist of all kinds of little compartments that certainly communicate with each other but which do not flow freely together. People are often described as vessels full of contradictions, but that is because time and again we try to apply stereotype metaphysical constructions to a complex reality. We then discover that they do not fit. When is somebody honest, when is he critical? I know people, and you know them too, who never tell a lie except when they fill in their tax declaration. There are also people who never invent tax deductions but who always lie when they come home late and are asked the reason for the delay. It is the same with credulity and skepticism.

I know people who have an indefatigable skepticism on paranormal matters, but who let their political insights be dictated by media religion. I know people who believe that proponents of pyramid power are heroes of the spirit, but who will never let themselves be taken in by propagandists of the model set by the Cuban revolution. Others again will never see a street light as a flying saucer, but immediately fall for it if someone of a certain sort proposes a profitable transaction. Or they believe in horoscopes but confidence tricksters have no chance. Or not in Uri Geller but absolutely in the Nicaragua committee.

I hope that what I want to illustrate with these examples is clear. People, at least most people, are not universally credulous. They are credulous in very specific areas. Susceptibility to nonsense in one area does not mean that a person is also prey to nonsense in another area. One can read horoscopes for 50 years and still believe in them without ever being tempted to run after a Messiah and kill oneself, upon his orders, in a South

American jungle. And a determined disbelief in horoscopes does not offer any protection against inculcation into some other absurdity.

For these reasons, I do not believe that belief in the occult is a snowball, which, if not arrested in time, will grow into an avalanche of obscurantism that will sweep our civilization away. And I do not believe that we, who enjoy bursting pseudo-scientific balloons, should be so pretentious as to believe that we are busy saving civilization.

Occult credulity has, of course, some dangerous points. Think, for example, about all those quacks who earn fortunes with worthless concoctions to cure serious conditions or about clairvoyants who give the parents of missing children false hope or unnecessarily scare them out of their wits. Or think about the risk of discrimination on astrological grounds by personnel bosses who believe in the stars.

But, all in all, I am inclined to interpret irrational belief in the occult as a rather innocent occupation. Contrary to many great religions occultism does not drive people to a fear of hell. Contrary to irrationalism à la Ayatollah Khomeiny, occultism does not demand of its followers that they be ready to be cut to pieces for some barbaric ideal. A consultation with a fortune teller costs less than a visit to a Freudian psychiatrist (and is probably less risky). And those who let their lives be regulated by newspaper horoscopes will not go so quickly astray as those who live according to the advice of media religion.

However, there is, as noted above, one territory where belief in the occult can have disastrous consequences, and that is parapsychology. A parapsychologist who declares that the reality of the paranormal is proven beyond reasonable doubt and in strict scientific fashion, proves *ipso facto* that he is credulous upon just that territory where it is his task to be critical.

The paranormal is a legitimate puzzle. Professor Rushton, former President of the Society for Psychical Research, expressed the dilemma in a way which has become classic. The evidence for the existence of the paranormal, he said once, is too weak to be believed and too strong to be rejected. Perhaps some day it will be shown that a core of truth resides in all these wonderful and unbelievable communications about miraculous happenings, and the discovery of that core will compel us to a fundamental revision of our scientific assumptions. It is also possible that the paranormal will one day definitively be shown to be a *fata morgana* – but in this case I believe that the unmasking will lead to new and surprising insights into human psychology. On intuitive grounds I am convinced that the final solution to the paranormal puzzle will add something worthwhile to our knowledge of man and nature. I am also convinced that this final solution will surprise parapsychologist and skeptic alike.

CHAPTER 2-06

Editorial Introduction

The following newspaper article by Hoebens is an early example (quite a few others were to follow) of a public exchange with a representative of what may be called the traditional (Tenhaeffian) school of Dutch parapsychology. Although the exchange, or Hoebens' part in it that is published below, was not exceptionally important at the time (or now), it is included here for three reasons. (1) It provides an idea of the general climate that prevailed in the country after Swedish psychologist Martin Johnson, Tenhaeff's vehement public protest notwithstanding, had been appointed Professor of Parapsychology at the University of Utrecht. (2) It demonstrates Hoebens' ability, especially in his journalistic writing, to adjust his tone and his rhetorical devices to that of his counterpart. And (3), more importantly, it indicates that Hoebens' views of parapsychology and skepticism developed very rapidly in the direction of what he would later describe as "ecumenicalism."

Hoebens had developed an interest in the scientific status of parapsychology only about a year before this article was written. Some of the views he expressed in this early newspaper article will not be found in his later writings. The difference may become apparent if the present short article is compared to the next chapter (2-07) on "Parapsychology and Skepticism" that was written about one year later.

This newspaper article was published in Dutch, under the title "De brutale critici in de parapsychologie," in De Telegraaf on July 7, 1979. Our English translation is based on (and fills the gaps in) a rough translation that Hoebens had written for the information of Marcello Truzzi at the time. In an accompanying letter to Truzzi (undated) he added the information: "Van Praag just challenged me to a TV debate. I have eagerly accepted." We found no evidence that that TV debate ever took place. (Eds.)

THE RUTHLESS CRITICS OF PARAPSYCHOLOGY

In a series of four recent contributions to the "Spiegel" column in this newspaper, Prof. drs. H. van Praag tried to protect parapsychology against the "fanatical rationalists" who express doubts about the pretensions of this ever-young boarder science. "These critics," according to Van Praag, are not critical, "but simply unwilling to be convinced by the facts." And on top of that they are ruthless! They throw "rude insults" at the Grand Old Man of Dutch parapsychology by speaking in friendly words about Prof. Dr. Martin

Johnson and send a “circus artist” to the Caruso among the Dutch paragnosts to challenge him to give a demonstration.

Since I am the perpetrator of the “rude insult” thrown at Mr. Tenhaeff, and I have regularly been seen in the low company of the aforementioned “circus artist” (i.e., the well-known illusionist James Randi) I feel that a rejoinder is in order.

From the fact that I restrict myself to a few main aspects, the reader should not conclude that I consider the remaining claims made by Mr. Van Praag as indisputable.

The logic applied by Mr. Van Praag in his “reckoning” with the skeptics is somewhat peculiar. Faraday is reprimanded because he refused to be present at experiments with [19th century medium D.D.¹] Home. On the other hand, Randi, who would have loved to be on the scene of the experiments with Croiset, “exceeds the limits of decency.” If I understand Van Praag well, skeptics are welcome at parapsychological séances, provided they guarantee in advance that they will be convinced afterwards. Because those who attend a séance, but leave afterwards without being convinced, are “worse than Faraday.”

Disbeliever

Mr. Van Praag suggests that critics have failed to take the trouble to investigate the facts personally, but his own knowledge of the skeptical literature is staggeringly poor. He presents Prof. Bart Bok as a “typical” disbeliever. In fact, however, Prof. Bok has never devoted a single word to parapsychology! (His well-known brochure² took issue with astrology.) Shall we really believe that Van Praag never heard of Prof. Hyman, Prof. Hansel, Prof. Kurtz, Prof. Truzzi or Dr. Evans? Even if he had only superficially studied the writings of these prominent scholars, he would have thought twice before putting skepticism on a par with prejudice and ignorance.

Mr. van Praag shares the opinion that the existence of the “paranormal” has been established in so convincing a way that only a stupid fanatic can still harbor any doubts. I would like to draw Van Praag’s attention to the fact that this approach also attacks several of his most prominent colleagues. The internationally renowned parapsychologist Dr. John Beloff also considers the currently available evidence insufficient. Prof. Johnson even wondered whether “there are any facts at all in our subject.” Does Mr. Van Praag

1 Added in translation. (Eds.)

2 *Objections to Astrology* (Bok & Jerome, 1975). (Eds.)

wish to qualify the opinions of these experts as being “a type of carelessness almost equaling insinuation”?

Conjurors

Mr. Van Praag ignores the role that magicians such as Houdini, Christopher, Randi and Berglas have played in investigating “spontaneous” paranormal phenomena. In his point of view, such circus-folk should stay away from the discussion. In my opinion, Mr. Van Praag, in whose published works many long-unveiled varieté tricks are still hailed as authentic miracles, should pay some more respect to the type of applied psychology known as conjuring.

For many years, scores of graduated academics have gaped in admiration at Uri Geller’s cutlery bending and the thought-photography of Ted Serios. These kinds of hocus-pocus were exposed by conjurors. Professional illusionists have achieved things at which Croiset would take off his hat. In 1957, Milbourne Christopher, for example, correctly predicted that the lottery ticket with number 20050 would win the first prize in the Cuban State Lottery. Trickery, of course. But let’s assume that Christopher had presented himself as a “paragnost.” In that case, Mr. Van Praag would probably have been very angry had I dared to suggest the possibility of clever manipulation!

“Spontaneous paranormal phenomena” show a striking (not to say suspicious) resemblance with conjuring tricks. That is exactly why parapsychologists should seek close cooperation with experienced illusionists. In doing so, they could spare themselves a severe loss of face. However, unlike Prof. Johnson, Mr. Van Praag holds the opinion that he needs the assistance of “circus artists” as much as he needs a hole in the head.

He completely agrees with Croiset (of whose successes in solving crimes his hometown police is completely ignorant, by the way) in not taking up Randi’s challenge. After all, in his point of view, tracing fraud and naivety is no more than “prejudice.” Van Praag keeps tactful silence about the spectacular successes of such investigations (some of which were carried out by some parapsychologists).

Reliability

The “facts” that impress Mr. Van Praag so much that he forbids any kind of doubt, are in reality no more than testimonies by witnesses of one-time phenomena. Not only the rude skeptics, but also a number of unsuspected parapsychologists take the view that one

should be extremely careful with regard to such cases. As Dr. Beloff wrote, the human abilities to observe and remember are utterly unreliable, especially in the case of “mysterious” events. “Under such conditions, the tendency to exaggerate and glamorize is almost irresistible,” said this colleague of Mr. Van Praag.

Beloff and Johnson frankly admit that parapsychology has not yet succeeded in converting belief in the supernatural into a coherent theory, the merits of which could be tested by “disbelievers” as well. For this reason, they still consider it unreasonable to require laymen to simply accept the “evidence.”

We should not forget that accepting the paranormal hypothesis probably would force the academic society to reconsider a number of “laws of nature” in a very radical way, though those laws have perfectly borne the scrutiny of criticism till now. Although science can be seen as systematical attempts to replace old theories by new, improved ones, nobody can expect a physicist to accept some curious anecdotes from Tenhaeff’s files as a refutation of generally accepted and solid theories on matter, space and time.

Mr. Van Praag concludes that a single event is a “proof of existence,” and he also holds that, from the beginning, this tautological claim knocks the bottom out of any criticism of details. He compares Croiset’s “chair tests” with the well-known experiment by Michelson and Morley (who intended to measure the effect of the “ether” on the velocity of light, but found the latter to remain perfectly stable) to show that “repeating” experiments is unnecessary. In fact, this example clearly speaks AGAINST his methodology in every conceivable way (see Lakatos, 1970). I will here restrict myself to mentioning only one crucial difference between the chair tests and the experiments dealing with the velocity of light. Mr. van Praag notes that Einstein never felt the need to ask someone to repeat the Michelson-Morley experiment.

What is really important, however, is that Einstein COULD have asked for such a replication at any given moment. In that case the outcome would undoubtedly have been exactly the same. Rest assured that Michelson and Morley would have responded to such a request in quite a different way than by shouting out in indignation about so much “resistance to learning.” To the contrary, both scientists doubted the outcomes of their own experiments to such a degree that they repeated them many times, even though they knew that light waves – unlike paragnosts – cannot lie.

CHAPTER 2-07

Editorial Introduction

In this invited article for the SRU Bulletin, Hoebens attempts to portray for his readers the species and sub-species of the “genus skepticus” and their respective motivations and approaches, and he tries to locate and explain his own “ecumenical” position within the framework of that classification. He clearly sympathizes with the skeptical “soft-line” approach. This essay was written in the summer of 1980.

The SRU Bulletin (eds. J.C. Jacobs and J. A.G. Michels) was published from 1976 to 1992 by the Synchronicity Research Unit in Eindhoven, The Netherlands. Initially conceived as a journal for the presentation of empirical parapsychological research and other technical papers by core members of SRU, in later years it also published invited papers, in Dutch or (mostly) English, by recognized scholars working or interested in parapsychology and related areas. Gradually their daily jobs left the editors too little spare time to continue solid experimental research that deserved publication. Therefore, the members of SRU decided to discontinue publication of the SRU Bulletin in 1992.

Hoebens’ essay, which here appears in English for the first time (in the editors’ translation), was published in Dutch as “Parapsychologie en skepticisme” in SRU Bulletin, 5, 1980, 108-112. (Eds.)

PARAPSYCHOLOGY AND SKEPTICISM

The editors of the SRU Bulletin invited me to write a short introduction to skepticism for the small fraction of their readers who might not yet be familiar with the skeptical species.

I am considered to have some expertise in this field, since I am a member of the Dutch section of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), and because I am the author of a critical regular column on parapsychology and related topics in the Dutch daily newspaper *De Telegraaf*.

I want to acquit myself of my task in a constructive way. In the world of parapsychology the debate between the “believers” and the “unbelievers” has not always been a paragon of reciprocal tolerance. In my opinion the main reason for this is that the extremists on both sides have had too many opportunities to present themselves as the exclusive representatives of their respective orientations. Although I enjoy a certain notoriety, at

least in some particular circles, as a “debunker” of parapsychological claims, in reality my inclinations are rather ecumenical.

I assume that the typical reader of the *SRU Bulletin* is deeply persuaded of the fact that the world of parapsychology in no way resembles a religious community with a pre-established ideology and a uniform credo. The differences between parapsychologists are so substantial that only a distinctly demagogic critic would find it tolerable to lump all of them together in the same box. If someone has any doubts about this, I recommend that he contrast, for example, Martin Johnson’s inaugural speech with any document by Henri van Praag, or, even easier, to compare this *SRU Bulletin* with the latest issue of the *Tijdschrift voor Parapsychologie*.

In what follows I wish to emphasize that the differences within the community of the skeptics are no less substantial¹ – a fact that those parapsychologists conveniently overlook who prefer to send their critics collectively to a psychiatrist (Tenhaeff) or even to hell (Rhea White²).

There even are card-carrying skeptics who display a more tolerant attitude towards parapsychology than some of the parapsychologists themselves. (In many respects, skeptic Truzzi, for example, is less skeptical than parapsychologists Breederveld and Millar.) This might be a bit confusing, and for this reason I think it is useful to insert a paragraph on definitions, which is inspired by Beloff.

If I may paraphrase him in this way, Beloff argued that, based on the current status of the available evidence, acceptance or refusal of the psi hypothesis is, *au fond*, a matter of “metaphysical preference.”

It is my conviction that this kind of metaphysical predilection is the one and only criterion that can serve to separate between proponents (“believers”) and skeptics (“unbelievers”) as two distinct groups. The proponent intuitively feels that psi exists. The skeptic just as intuitively feels that psi does not exist. This definition does not specify anything about the vehemence with which both species stick to their metaphysical predilections; in both groups we find “verligtes” and “verkrampes”.³ The moderates on both sides can

1 For the apparent difficulty, or even the impossibility, of identifying even a single person as either a parapsychologist or a skeptic, see, in the same journal, Hövelmann, G.H. (1988). Parapsychologists and skeptics – problems of identification: Some personal comments evoked by J.C. Jacobs. *SRU Bulletin*, 13, 125-132. (Eds.)

2 White (1980, p. 9). (Eds.)

3 “Verligtes” and “verkrampes” both are Afrikaans words. They characterize “open-minded” and “closed-minded” individuals, respectively. (Eds.)

be recognized by the “falsifiability” of their points of view: They know that their views are preliminary, and they are willing to change their minds if this is required by new evidence. Except for their differing expectancies with regard to the eventual outcome of the debate, they can agree on almost every important topic.

After this unavoidable introduction, I am going to present a brief overview of the sub-families of the “*genus skepticus*.” I shall also introduce one or more examples for each of those sub-species.

The First Sub-family: The Extremists

In this group we find a number of “unbelievers” whose abhorrence of hocus-pocus clearly shows ayatollah-like features. The extremist is not only fully convinced that the paranormal does not exist, but he also considers everybody who might have a different idea to be a threat to social health.

The prototypical representatives of this view live in Germany, and their names are Prof. Dr. Otto Prokop, Dr. jur. Wolf Wimmer and Dr. jur. Herbert Schäfer. This triumvirate is of the opinion that occidental civilization is acutely threatened for no other reason than the sheer existence of a parapsychological institute at the University of Freiburg.

Characteristic of the extremists’ approach is a paper entitled “Hexenwahn an Universitäten?” [Witch craze at the universities?] that Wimmer (1980) published in the German medical journal, *Zeitschrift für Allgemeinmedizin* [Journal for General Medicine]. Its motto clearly sets the tone: “Der Mensch hat fünf Sinne; der sechste ist der Blödsinn” [Human beings have five senses; the sixth sense is nonsense⁴]. Wimmer presents parapsychology as merely a version of medieval witch belief in “scientific disguise.” Parapsychologist Bender and his colleagues are held responsible, retroactively, for the horrible prosecution of witches in the era of Sprenger and Institoris.⁵

„Für okkultistische ‚Magia Naturalis‘ hat Europa mit Millionen Scheiterhaufen bereits teuer genug bezahlt. Es reicht jetzt. Wir sind gewarnt. Keiner sollte später sagen können, er habe es nicht gewußt! [With millions of stakes Europe has paid far too dearly for occult ‘Magia Naturalis.’ We are fed up with this. We have taken the warning. No-one shall ever

4 The German word “Blödsinn” has the context-dependent double meaning of “nonsense” and “idiocy.” (Eds.)

5 Jacobus Sprenger and Henricus Institoris were the authors of the infamous *Malleus maleficarum*, or “Hexenhammer,” which was first published in 1487. (Eds.)

be able to say that he was unaware!],” writes Dr. Wimmer, Presiding Judge at the District Court of Mannheim.

In my point of view, Wimmer’s words sufficiently demonstrate that the skeptics’ community has its Tenhaeffs as well: demagoguery instead of discussion, and insinuations instead of arguments.

The Second Sub-family: The Hard-liners

This group comprises the skeptics who have hardly any doubt that psi is just an illusion, and that parapsychology should be ranked as a pseudo-science. Their tone is often polemic, but they distinguish themselves from the extremists through the quality of their critique, and because they are prepared to look the “enemy” straight into the eye. One of the best examples for this is the well-known Canadian-American illusionist and escapologist James Randi, author of *The Magic of Uri Geller* (Randi, 1975), *Flim-Flam! The Truth About Unicorns, Parapsychology and Other Delusions* (Randi, 1982b), and of a large number of other very entertaining popular stories on occult topics. In his 1975 book on Geller, Randi defined parapsychology as “an art, not a science.” He is convinced that a fraud-proof experiment with any kind of so-called paranormally gifted subjects inevitably must result in a negative outcome. According to Randi, the current “evidence” for the existence of ESP, PK and RSPK is the product of fraud, credulity, inaccuracy, coincidence, artifacts or any combination of those. It goes without saying that not all parapsychologists receive Randi’s contributions to the debate with gratitude. One cannot deny that Randi demonstrates a certain level of prejudice and that his use of factual material is sometimes stretching a point. Nevertheless, I am gladly breaking a lance for Randi. As soon as he steps down from his soapbox, he proves to be an interesting and intelligent party in any discussion. Few people have more experience with and knowledge of the various methods for tricking scientists than Randi. He is always willing to assist a parapsychologist who wants to devise an almost fraud-proof experiment. His well-known challenge to the paranormal community (\$10,000 for the first paragnost who can demonstrate paranormal ability to Randi⁶) may be a stunt, but at the same time it shows Randi’s willingness to stick out his neck. Incidentally, dozens of miracle workers have taken on the challenge, but in all cases their supernatural powers have failed them. Geller, Serios and Croiset did not show any interest in Randi’s challenge.

6 The “prize money” was raised to one million dollars in the 1990s – and it may still be considered a “stunt,” as Hoebens surmised. (Eds.)

The Third Sub-family: The Almost Hard-liners

This sub-family has a strong resemblance with the hard-liners, and many people consider both groups as one and the same. I think they are different, however, even though I must admit that the differences are not very substantial. Unlike Randi, let alone the extremists, the almost hard-liners demonstrate, or at least verbalize, that they are willing to take parapsychology seriously.

As is clearly demonstrated by the title of his latest book, *Flim-Flam! The Truth About Unicorns, Parapsychology and Other Delusions*, Randi (1982b)⁷ lumps parapsychology together with “unicorns and other delusions.” The almost hard-liners are a bit more cautious. Martin Gardner may serve as an example. In his thrilling book, *Fads and Fallacies in the Name of Science* (Gardner, 1957), he volunteers extensive excuses for the fact that he mentions an earnest scientist such as Rhine in a book on crackpots such as Hörbiger, Abrams and Velikovsky. Gardner is a keen and ironic critic, but he grants his opponents the benefit of the doubt. When British mathematician Taylor mutated from a credulous admirer of Geller into a super-skeptic, Gardner protected parapsychology against the convert’s criticisms (Gardner, 1979-1980).

The best-known exponent of an almost hard-line approach undoubtedly is British psychologist Mark Hansel. His book, *ESP: A Scientific Evaluation* (Hansel 1966), and its revised and expanded re-edition *ESP and Parapsychology* (Hansel, 1980), caused something like panic in the field of parapsychology. Hansel’s sophisticated plea boils down to the claim that the statistically significant results of classical ESP experiments (Pearce-Pratt, Pratt-Woodruff, Soal-Goldney, etc.) do not prove the reality of ESP but only refute the null hypothesis. Every non-coincidental explanation is equally supported by the statistical figures. If the setup of an experiment does not exclude fraud, fraud is such an alternative non-coincidental explanation. Under such circumstances a significant outcome is an indication of ESP as much as it is an indication of fraud. Which of these two hypotheses is to be preferred cannot be decided on the basis of statistics alone. According to Hansel, within the context of contemporary science fraud is a priori more likely than ESP. While it is difficult to integrate ESP into specific, rather fundamental scientific knowledge, fraud is a very common phenomenon.

Some parapsychologists found that it is easier to contest Hansel’s arguments if they are slightly distorted beforehand. Thus, Hansel’s opinion is summarized as: “ESP does not exist, therefore all those experiments must be fraudulent.” Allow me to elaborate on

7 See Hoebens’ review of that book, reprinted here as chapter 4-13. (Eds.)

this with two examples. On page 167 of their book *Mind Reach*, Targ and Puthoff (1977) wrote: “He [Hansel] began his examination of the ESP hypothesis with the stated assumption, ‘In view of the a priori arguments against it we know in advance that telepathy etc., cannot occur.’” Searching for this quotation in Hansel’s writings is a useless endeavor, however. Tenhaeff was even more impudent. In his own book, Hansel had made some critical remarks on Tenhaeff’s protégé, Gerard Croiset. On several occasions, and again in the latest issue of the *Tijdschrift voor Parapsychologie* (Tenhaeff, 1979c, p. 196), Tenhaeff attacked Hansel, but he did so based on arguments that Hansel never used. Instead, they were made up by Tenhaeff himself.

Nothing of this sort will change the fact that some of Hansel’s “reconstructions” of historical experiments are susceptible to discussion. I am not one of those skeptics who believe that Hansel has once and for all exposed parapsychology as a pseudo-science. It is funny, by the way, that, while some of Hansel’s fellow skeptics classify him as a dogmatic, parapsychologist Breederveld speaks very positively about him!

A third typical example of the almost hard-line approach was the late Christopher Evans, a British psychologist who was the author of, among others, the book *Cults of Unreason* (Evans, 1973). Charming Evans was praised as an open-minded critic by many parapsychologists who at the same time considered Hansel to be utterly fanatic. This is somewhat peculiar, since Evans completely agreed with Hansel.

Although I do not know for sure, I would go so far as to suppose that the average supporter of CSICOP can be ranked with the almost hard-liners.

The Fourth Sub-family: The Soft-liners

In this group we find those skeptics who do not believe that psi really exists, but who have a weakness for parapsychology nonetheless. Typical soft-liners like to meet with parapsychologists. They happily publish in parapsychological journals and, despite their unbelief, they permanently refuse to commit themselves. They are the critical allies of the parapsychologist. The soft-liners catch the eye because of their philosophical approach to the problem. Frequently, they consider the debate on parapsychology as an illustration of the much more general debate on the nature of science.

Sometimes they distance themselves openly from their nominal confederates who are going much too fast. If they ever ruthlessly attack a specific parapsychologist, they always explain, politely, that their critique is not meant to apply to parapsychology as a whole.

Among those typical soft-liners we encounter statistician (and former employee of the Society for Psychical Research) Christopher Scott, Denys Parsons, both from Great Britain, magicians Milbourne Christopher and David Berglas (the latter being a doubtful case, by the way, because he is linked with CSICOP, but at the same time tends to believe in dowsing and poltergeists), American psychologist Ray Hyman and, particularly, sociologist Marcello Truzzi from Michigan. Truzzi is an extreme soft-liner. On the one hand, he does not (yet) believe in psi, but on the other hand he is an important defender of parapsychology. In the mid-1970s Truzzi was one of the founders of CSICOP and of its journal, *The Zetetic*. After a while Truzzi left in a row. The approach taken by CSICOP and its chairman, Paul Kurtz, he deemed too militant. Truzzi started a new journal, *The Zetetic Scholar* (CSICOP's *The Zetetic* was renamed *Skeptical Inquirer*), which since then mainly focuses on stimulating dialogues between proponents and skeptics. Both journals are worth reading.⁸

8 Marcello Truzzi's *Zetetic Scholar* ceased publication at the end of 1987; however, there were plans for a relaunch as late as 2002 (see Hövelmann, 2005a, p. 7, fn. 3). The *Skeptical Inquirer* is published to this day and, at the time of this writing, it is about to enter its 39th volume under the continuous editorship of Kendrick Frazier. (Eds.)

CHAPTER 2-08

Editorial Introduction

In his essay “The Legitimacy of Unbelief” (see chapter 2-01), Hoebens had noted: “As a counter-balance to my skeptical predisposition I personally like to volunteer as a subject in psi experiments even though the outcome, in my case, is sometimes highly embarrassing for a registered unbeliever.”

In the two short reports to follow in this and the next chapter (2-09), Hoebens describes his experiences as a subject in two parapsychological experiments and his impressions of the work of the respective experimenters. This first paper, originally published in volume 4 (1979-1980) of CSICOP’s Skeptical Inquirer ([2], 64-66), provides instructive inside views of the methodology and evaluation in one of Richard Broughton’s experiments at the Parapsychology Laboratory, University of Utrecht. (Eds.)

HOW I WAS DEBUNKED

One day last April, I suddenly fell under the suspicion of being psychic. The suspicion was voiced by a computer hidden in the basement of the psychology faculty at Utrecht University in the Netherlands. A few weeks earlier I had participated in a sophisticated PK experiment devised by Richard Broughton,¹ a parapsychologist working in Holland.

Of hundreds of subjects trying their hand at the experiment, only one seemed to produce results that were truly extraordinary – me. Not that my scores were particularly high, but the “variance effects” I produced were, well, just about the last thing you’d expect from chance alone.

My performance was remarkable for yet another reason: I happen to be a card-carrying skeptic. A native-born Dutchman, I belong to that visionary race that gave the world Hurkos and Croiset, but personally I prefer to question the existence of all paranormal phenomena, from poltergeists to pyramid powers.

As we all know, skepticism inhibits psi. This was clearly proved by Croiset, who early in 1977 predicted a major disaster in the Bermuda Triangle nine months hence. He did so

1 A couple of years later, Dr. Richard S. Broughton moved to the United States to serve as a research associate at, and later as the Director of, the FRNM (now called “Rhine Research Center”) in Durham, NC, and still later he returned to the UK to work at the University of Northampton. (Eds.)

at a public session that unbelievers were allowed to attend. At the appointed time, nothing whatsoever happened in that murderous part of the Atlantic. Since Croiset normally can read the future as you and I read a city map, this case of ESP-missing strikingly demonstrates the disturbing influence exuding from the skeptical presence.

For a while, however, my accomplishment at Utrecht University seemed to refute this cherished assumption. I didn't believe in PK, and yet I performed a neat mind-over-matter feat! Most embarrassing for believers and skeptics alike.

Fortunately for both parties, Richard Broughton is a cautious scientist. He works at the Parapsychology Laboratory of Professor Martin U. Johnson, whose name is anathema to the local occultists. (In a recent interview for *De Telegraaf*, Johnson told me ESP was not an established fact, and he expressed eagerness for close cooperation with professional magicians in his battle against "parapornography").² Johnson and his collaborator, Sybo Schouten, hate jumping to conclusions, and so does Broughton. His experiment *really* was sophisticated: it was self-debunking.

The experiment was called "The Head of Jut," after a popular game where one is meant to hit a lever with a heavy mallet, sending a weight up a wire to ring a bell at the top.

Broughton's Head of Jut (to give a short description that does scant justice to the refinement of the design) consisted of a column of 32 small lamps mounted in an aluminum frame. Controlling it was a device that interpreted signals from the computer program that ran the experiment. The number of lamps that would actually be lit was decided by a random-event generator coupled to the computer. The subject was asked to try to influence this random process psychokinetically, "willing" as many lamps as possible to light up in the column. High scores were rewarded with the sound of a bell. The gamelike arrangement was meant to be motivating, and hence psi-conducive. Well, I certainly felt motivated.

Yet I was exposed by the very contraption that almost proved my supernatural abilities. For Richard Broughton, distrusting the kind of evidence so often hailed as "conclusive" by the true believers, had used the so-called Edinburgh Split for analyzing the results. Basically, the Edinburgh Split is a computer-conducted separation of the data, allowing the first part of the result to be called the "pilot" and the second part the "confirmation." To quote Broughton's report, which was presented at the recent SPR conference in Edinburgh, and published in the *European Journal of Parapsychology* (Broughton, 1979, 2, pp. 337-357): "The pilot part can be analyzed with all the freedom the experimenter wishes. Based upon his findings in the pilot part, the experimenter then formu-

2 See chapter 2-10 of this book. (Eds.)

lates specific hypotheses regarding the effects which he believes to be in the data and then rigorously tests for these in the confirmatory part.” Failure to confirm must be accepted as a conclusive refutation, and may under no pretext be explained away. Of course the confirmatory data remain hidden from the experimenter until he has formulated his predictions.

Well, the Edinburgh Split ended my short career as a psychic. Having scored significantly in the pilot part (see the tables in the *EJP* [*ibid.*, pp. 346-350]), my remarkable variance pattern was not replicated to any degree in the second part. My psychokinetic feat had been entirely spurious. Chance does odd things once in a while.

Richard Broughton thinks his Head of Jut should serve as a warning to other psi researchers. With the increased use of mini-computers, game-like PK tests will become routine. He fears spurious effects, parading as psi evidence, will soon abound if parapsychologists do not guard against statistical booby traps. He strongly recommends the use of the Edinburgh Split as a mine-detector.

I find Broughton’s determination not to be fooled by his own experiments entirely admirable. It should assure him of sympathetic attention from the skeptics if he ever comes up with positive findings.

A true believer would never have allowed the confirmation part to spoil those promising pilot results. An expert in “astral bodies and hidden knowledge” with whom I discussed the Head of Jut was angry with Broughton for having surrendered so easily. Of course, I had real PK powers! It was very simple. My skepticism had initially forgotten to exercise its inhibiting influence. It had reasserted itself just in time, and that’s why Richard Broughton had to conclude his report with the anticlimactical sentence: “Accordingly this experiment was judged to have failed to demonstrate PK effects.”

CHAPTER 2-09

Editorial Introduction

While the previous report on Hoebens' experiences as a subject in Richard Broughton's Utrecht experiment (chapter 2-08) was published in English in an American skeptical magazine, this second impressionistic "experimental report" so far was only available in Dutch. It appeared in the issue of May 10, 1980, of the daily newspaper De Telegraaf. The editors' translation for this book has greatly benefited from an almost-literal English summary that Hoebens had written at the time for the information of Carl Sargent who was the experimenter.

Please also note the "Editorial Postscript" to this chapter. (Eds.)

HELLO ..., DO YOU READ ME?

MY ADVENTURE AS A PARAPSYCHOLOGICAL GUINEA-PIG AT THE UNIVERSITY OF CAMBRIDGE

Each time someone plans to run a well-designed parapsychological experiment within reasonable distance of Amsterdam I volunteer as a subject. My intuition tells me that telepathy and clairvoyance are just nonsense, but if someone wants to demonstrate the opposite in a sensible way I'll be glad to lend him an astral hand. After all, my intuition could easily be dead wrong.

My most recent adventures as a parapsychological guinea-pig I experienced last week at the prestigious University in Cambridge, where the young English psychologist Dr. Carl Sargent has been causing a stir with a remarkable series of psi experiments.

Sargent uses the so-called Ganzfeld technique, pioneered in the United States. Basically, that technique means that a "sender" tries to paranormally transmit a target picture to a "receiver" who has been cut off from normal sensory contact with the outside world.

Susceptible

The "receiver" is lying down on a comfortable mattress. Earphones play white noise in his ears. His eyes are covered with half ping-pong balls, fastened with sellotape.

Over his head is a lamp, emitting soft red light. In this “Ganzfeld” condition you are supposed to be more susceptible to paranormal communication.

During the session, the subject tells the experimenter (who is in a different room) by means of a one-way intercom whatever crosses his mind. Everything he says is registered. In the meantime, the sender, in yet another room, concentrates on the target picture. He has paper at his disposal to write down his thoughts and associations. Doodling is permitted as well.

Target

After the session is over, the experimenter shows the subject four different pictures. One is the duplicate of the target, but according to the procedure no one except the sender may at that stage know, which one.

With some assistance of the experimenter, who reads his notes aloud, the subject checks item after item, to decide which of the pictures best fits his impressions during the Ganzfeld session. As soon as the subject has made his final choice, the sender is invited to disclose the target.

According to probability theory, if there are four pictures you could expect a hit in about 25 percent of the trials by chance alone. After 200-or-so trials, however, Sargent reports a success rate closer to 40 percent.

It is very unlikely (odds of about 100,000 to one) that this result is due to chance. Apart from the statistics some subjects from time to time get “impressions” that later turn out to show striking similarities to the actual target. It has happened, Sargent told me, that one subject started to refer to a certain drawing by William Blake. You guessed it: that particular drawing was the target picture!

For more than only one reason, my own experiences as a “receiver” were curious, to put it mildly. The night before I traveled to Cambridge I happened to have a dream about the experiment in which I was to be a subject. One of the many things I dreamt, was that in my case the target picture would be a painting by Magritte. I wrote this “prediction” on a piece of paper, put it in a closed envelope and took it with me to the laboratory.

“Ganzfeld” is an entirely pleasant sensation. Isolated from the environment by the monotonous noise from the headphones I bathed in the soft red light that penetrated the ping-pong balls over my eyes. Fully relaxed, I let my imagination go.

Pleasant

It was as if the mattress started to rock gently, like “an air mattress on the sea. I thought about a forest, crossed by a river. Like a late-medieval painting. It was warm, summer. A late sun was shining on a fortress. People on horses. I got the impression of hovering over the landscape, like I was floating through the air, still on my mattress.

Suddenly I thought I saw letters in the soft red field in front of my eyes. Letters that became words. First: ‘Hello,’ then: ‘Italy.’ And all the time, this soft rocking feeling.” Very pleasant.

When I had returned to the normal world, Sargent showed me four pictures. One of those caused me to say immediately: that seems to me to be the target. It was a picture of a painting by Dali (Swans reflecting Elephants), a painting that seems to be inspired by some late mediaeval artists. A late summer sun shines over a broad landscape. A river running into a calm sea.

In the background, the painting shows a sort of fortress on a hill. Trees, swans, a floating man. Less eerie than one would expect from Dali. The dominant impression is rather one of warmth and peacefulness.

Let’s not exaggerate, however. The match was far from perfect. I had “seen” quite a number of things Dali had never painted, and vice versa. Yet the similarities were clear enough to me to bet on this picture, without any hesitation.

Full hit

Excited with expectation Sargent and I went to the room where the “sender” (psychologist Trevor Harley) was. When we entered his room, Harley turned around and showed us the target. It was the Dali all right. A hit. Sargent beamed with pleasure.

The “prediction” I noted down on a piece of paper after my dream in the night before I traveled to Cambridge was dead wrong. Except for one thing: I had mentioned a painting by Magritte. Dali and Magritte are both surrealists. Their works of art resemble each other so much that inexperienced museum visitors often find it hard to tell the difference.

The final surprise came when Harley showed the paper he had been scribbling on during his transmission time of the Ganzfeld session. There it was, in between the doodling, in large letters. The word “Hello.”

Remarkable, isn't it? But of course, it would be very unwise to draw sweeping conclusions from such an isolated case. It could have been sheer coincidence, and with those ping-pong balls covering my eyes I was hardly in a position to observe Sargent and Harley. My firm belief that it was all entirely honest basically proves nothing.

I should add that the Parapsychology Laboratory of Utrecht University is trying to replicate the Cambridge experiments. I have been a subject in these Utrecht tests as well. There it does not seem to have become a resounding success. However that may be, it was an impressive experience. Honestly, I think I would only be half surprised if Sargent's work turned out to be more than yet another parapsychological red herring. I am waiting. Skeptical, but curious.

Editorial Postscript

Hoebens, alas, was spared the half-surprise he was looking forward to in the penultimate sentence of his article. In fact, in the early 1980s, rumors spread (cf. Blackmore, 1980, 1983, 1987; Parker & Wiklund, 1982) "that some possibly fatal errors had been made in the Cambridge experimental programme." Hoebens was invited (and agreed) to join, as an outside observer and consultant, the committee that the Parapsychological Association had established to investigate allegations against the main experimenter.¹

For obvious reasons, Hoebens' observer's report to the committee (written in Amsterdam on May 28, 1984, but sent from Athens, Greece, later the same day) is confidential.² However, it ends on a note that may be quoted in full: "If you think it will serve any purpose you are free to report to whom it may concern that as a CSICOP-related observer in your committee I did not detect the slightest hint of any attempt to 'cover-up' the case. I was impressed by the committee's determination to get at the (possibly unpleasant) truth. I am convinced that, had any unambiguous evidence of fraud on CS's part been forthcoming, you and your fellow committee members would not have hesitated to expose the culprit in public. The very fact that an outsider skeptic (and a CSICOP associate at that) was invited to sit on the committee is ample proof of the committee's objectivity."³ (Eds.)

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- 1 The committee was chaired by Prof. Dr. Martin U. Johnson. Members of the committee were Dipl.-Psych. Eberhard Bauer, Dr. Rolf Ejvegaard, Prof. Dr. Erlendur Haraldsson, Dr. John Palmer, Dr. Sybo A. Schouten and Ms. Kathy Wilson. In addition to Piet Hein Hoebens, Dr. Gudmund W. Smith (a law expert) was asked for advice as an external observer and expert.
 - 2 It is preserved in the Hoebens Files and is duly reflected in the final report of the PA Committee (Parapsychological Association, 1984).
 - 3 Hoebens Files; Piet Hein Hoebens, letter to Martin U. Johnson, May 28, 1984.

CHAPTER 2-10

Editorial Introduction

In his 1985 obituary, Brian Millar noted: “Hoebens came upon a curiously split parapsychology in Holland. On the one hand is that indigenous to the country, heavily influenced by Tenhaeff. On the other is the [then] relatively new laboratory of Johnson, a Swede, which is oriented towards the outside world of international research. In the beginning this laboratory was regarded as a foreign imposition, about as welcome as a wart, and Johnson himself became the butt of much abuse. Thankfully these attitudes have undergone considerable change in the course of time and Hoebens played his part in this. His low opinion of Tenhaeff was counterbalanced by admiration for Johnson’s ‘no nonsense’ approach. Hoebens’ articles have done much to make Johnson’s work known to the Dutch public.”¹

The following newspaper article, originally published on March 17, 1979, in De Telegraaf under the title “Prof. Johnson’s strijd tegen ‘para-pornografie,’” is a pertinent example. The article, which here appears in English for the first time, was largely based on interview material thus giving Johnson an opportunity to explain his views and general approach to the Dutch public.

*For years, Hoebens and Johnson had a cordial relationship and saw each other on a more or less regular basis, both professionally and privately. It was Hoebens too who saved Johnson’s book *Parapsychology: Research in the Border Areas of Experience and Science* [Parapsychologie. Onderzoek in de grensgebieden van ervaring en wetenschap] (Johnson, 1982) from an unmitigated fiasco. Originally published in Swedish² two years previously, the Dutch translation, arranged by the publisher, turned out to be almost incomprehensible when Hoebens was shown the page proofs. Over a weekend Hoebens turned the book into an eminently readable volume. The publisher afterwards offered a modest honorarium for this linguistic rescue operation. Hoebens accepted it and donated it to a charity.³*

In a letter to one of the editors (G.H.H.), a week after Hoebens’ death, Johnson wrote: “I feel very sorry for the sad news about Piet Hein’s death. He and his very nice wife, Liesbeth, had dinner with me only a few days before he departed.”⁴ Johnson at that time had

1 Millar (1985, p. 127).

2 Johnson (1980).

3 The relevant correspondence and documents are preserved in the Hoebens Files.

4 Martin U. Johnson, letter to Gerd H. Hövelmann, October 30, 1984.

*already drafted an obituary for the European Journal of Parapsychology which in the end he did not publish when G.H.H. had submitted his obituary.*⁵ (Eds.)

PROFESSOR JOHNSON’S BATTLE AGAINST “PARA-PORNOGRAPHY”

PARAPSYCHOLOGIST DESCRIBES HIS OWN PROFESSION AS “NOT YET A TRUE SCIENCE”

Professor Dr. Martin U. Johnson is a maverick in the small but generally very frenetic world of parapsychology. This likable but imperturbable Swede does not come across as a fiery believer in the paranormal. In fact, he even maintains that parapsychology has yet to become a true, fully-fledged science. He is not your man if you want exciting stories about telepathic feats, poltergeists and astral bodies. A solution to the World Riddle is beyond him as well.

Ever since he was appointed professor of parapsychology at Utrecht University, he has frequently felt compelled to publicly calm down some of his overly energetic professional colleagues.

Johnson and his assistant, Dr. ir. Sybo Schouten, see themselves as the “conservative right-wing element of parapsychology.” They enjoy exchanging pleasantries with hostile skeptics, and for that reason alone are viewed with deep suspicion by the enthusiastic seekers of the Hidden Truth.

“It’s not easy to be a parapsychologist these days,” says Johnson with a sigh.

“You have to continuously do battle on two fronts. On the one hand, against the type of skeptics that condemn us without even knowing exactly what our claims are. And against ‘fanatical’ supporters on the other hand.” Your own friends can sometimes deeply embarrass you by suddenly turning up with reports of flying saucers, Bermuda Triangles and magical pyramids. That kind of susceptibility to cock-and-bull stories hardly contributes to the plausibility of parapsychology.

5 Hövelmann (1984a).

Stories of the Occult

Johnson abhors the nonsense that is so often presented to the public as "new scientific discoveries in parapsychology." He calls this type of occult sensationalism "para-pornography." He is particularly apprehensive of the stunts performed by "supermen" like Ingo Swann and Uri Geller. "I feel that parapsychologists should be very careful in how they react to these stunts," he says.

"That they generally do not do so is unfortunate in the extreme. Without wanting to speak ill of my colleagues, they do tend to get carried away now and then. I was almost stoned to death, figuratively speaking, when I had the audacity to be critical of Geller in 1973. Information about Geller that has come to light since then has confirmed that my suspicions about him were justified."

He was very disappointed that a business trip prevented him from meeting James Randi, a Canadian-American illusionist and fervent adversary of pseudo-science, when the latter visited the Netherlands. "I would have loved to meet him. I have always strongly supported collaboration between parapsychologists and professional illusionists. Particularly in the case of 'spontaneous' paranormal phenomena, where there is much more room for deception than in laboratory experiments. In fact, the assistance of an expert illusionist is a primary requirement when dealing with a phenomenon like Uri Geller."

Johnson knows from experience how difficult it is for parapsychologists to distinguish between a demonstration of dexterity and a supernatural event. On the occasion of the 19th Annual Meeting of the Parapsychological Association in Utrecht, he asked Ulf Mörling, a Swedish amateur illusionist, to give a small demonstration. A sketch, which Johnson had drawn on the previous evening (in the faculty toilet), placed in two thick envelopes and kept hidden in the leg of his trousers ever since, was reproduced by Mörling, who pretended to be using "telepathy," with astounding accuracy.

Mörling emphasized that this was a trick. But in vain! Johnson: "Something incredibly embarrassing occurred after the demonstration. A considerable number of the parapsychologists present started to discuss the possibility that Mörling really was clairvoyant without being aware of it. I was shocked that so many colleagues were capable of putting forward a paranormal explanation of what they had witnessed, and in utter seriousness at that, even though Mörling had sworn that it was just a trick!"

“I think I would call parapsychology an attempt at science,”⁶ he says. “It is not a science in the truest sense of the word, as physics is. Our profession has yet to make the major breakthrough, even though some would claim that it already has.”

Exaggeration

“Many grossly exaggerated claims are made in the name of parapsychology. Grossly exaggerated because of the obscure, ambiguous, ‘soft’ facts that we have at our disposal. If they are even worthy of being called ‘facts’ at all. I find it a pity that so many parapsychology works carry triumphant titles like ‘Extending the Boundaries of the Mind’ and so on; they sound like War Office communiqués! You know, people often say that the existence of ‘psi’ (a collective term for the entire range of paranormal phenomena from clairvoyance to the family ghost) has already been extensively proved beyond all reasonable doubt. But I want to emphasize that individual experiments cannot provide incontestable proof if you don’t understand the results and if you are unable to achieve the same results when you repeat the same experiment.”

“We don’t have any repeatable experiments yet in parapsychology. If we did, the controversy between the ‘believers’ and the ‘unbelievers’ would immediately cease to exist. People would be able to test it for themselves. I know that there are some colleagues who believe that a remarkable aversion to repeatable experiments is one of the inherent characteristics of ‘psi’, but that’s a supposition that gets you nowhere.”

“Moreover, there are faint indications, and I am expressing myself very reservedly here, that the work of people like Helmut Schmidt and Charles Honorton in recent years has led to results that may be a foreboding of the long-awaited breakthrough. Some successes are very impressive at any rate, if, that is, we can be sure that no errors have crept into the research. Unfortunately, at this stage, you can never be completely sure that that is the case.”

Sir Karl Popper

Johnson’s skepticism vis-à-vis his own profession derives from his admiration for the great Austrian-English philosopher, Sir Karl Popper. Popper’s philosophical thinking basically states that a practicing scientist should systematically detect his own errors.

6 This, in fact, is what the original Swedish title of Johnson’s book (Johnson, 1980) says: parapsychology is a “försök till forskning” – an attempt at science. (Eds.)

Theories should be formulated in as vulnerable a way as possible so that their possible erroneousness can quickly be demonstrated by experimentation. Above all, says Popper, do not try to protect your favorite ideas by continually dreaming up special pretexts if your experiments do not deliver the results that your theory predicts. Admit that you are wrong and think of something better.

"Reading Popper for the first time was a revelation for me," says Johnson. "Ever since, I have continually encouraged my colleagues to read his books. A critical attitude like this is precisely what is needed in our profession, as parapsychologists have too often tended to come up with 'special pretexts' if the observable facts did not meet their theoretical expectations. Of course, this occurs in other branches of science as well, but I find it a totally reprehensible practice."

Speculations

So what does Johnson believe deep down in his heart?

"I would have to speculate. You must understand that I have no objection to speculation as long as it is clearly identified as such. Well, I think it perfectly possible that in amongst all that paranormal 'nonsense' there is a genuine 'something', which cannot be explained by our current level of scientific understanding. I have no idea whether this 'something' will prove to be of any significance. But 'abnormal facts' are sometimes the key to interesting discoveries. 'Psi,' if it exists, may very well throw new light on the age-old juxtaposition of mind and matter. Maybe some mental processes are less time-bound and space-bound than we presently assume. But I repeat; this is just vague speculation and many facts point in a totally different direction. For example, if the mind, as many parapsychologists believe, is capable of functioning independently of the body, how can minor brain damage cause permanent changes in mental behavior? No, if you are asking me what I truly believe, I would have to say that I believe it is worth carrying out further research."

CHAPTER 2-11

Editorial Introduction

In the article to follow, basically an essay review of three major European books on parapsychology that were all written in languages other than English, Hoebens tries to differentiate between “sensible” and “nonsensical” ways parapsychological research is conducted, evaluated and presented to the scientific world and to the public. The paper originally appeared in the Winter 1983-1984 issue of the Skeptical Inquirer (vol. 8, pp. 121-132) and is reprinted here with the kind permission of CSI (formerly CSICOP) and of Kendrick Frazier, then and now the editor of the Skeptical Inquirer.¹ (Eds.)

SENSE AND NONSENSE IN PARAPSYCHOLOGY

PARAPSYCHOLOGY SPANS A SPECTRUM FROM SOPHISTICATED SKEPTICISM TO PRO-PSI GULLIBILITY

Parapsychology is indistinguishable from pseudo-science, and its ideas are essentially those of magic.

Parapsychology is a farce and a delusion, along with other claims of wonders and powers that assail us every day of our lives.

These somewhat unflattering remarks are taken from the concluding paragraphs of two recent books in which the pretensions of parapsychology are examined from a skeptical point of view. The first is from James Alcock's *Parapsychology: Science or Magic?* (Alcock, 1981) and the second from James Randi's *Flim-Flam!* (Randi, 1982b)

It was to be expected that such sentences would provoke the indignation of the parapsychologists – who in fact were quick to point out what they perceived as gross unfairness on the part of both authors. The complaint most frequently heard was something

1 Hoebens' paper was reprinted in Frazier, K. (ed.), *Science Confronts the Paranormal* (pp. 28-39). Buffalo, NY: Prometheus Books, 1986. In the following year, a Danish version appeared as a little booklet: *Fornuft og ofornuft i Parapsykologi. Parapsykologi spænder over et spektrum fra spidsfindig skeptisisme til forudindtaget lettroenhed* (= Skeptica Reprint-serien Nr. 6). Kilde: Para-nyt, 1987. As the Hoebens Files confirm, Hoebens himself had authorized the translation into Danish in the Spring of 1984, a few months before his death.

to the effect that both Alcock and Randi have overstated their case by generalizing their (often justified) criticisms of a subset of paranormal claims to the entire field of parapsychology – thereby tarring all proponents with the same brush. It is argued that, in their eagerness to exorcise the demons of the New Nonsense, the skeptics have failed to take into account the differences between “serious parapsychology” and the less than serious variety.

In his witty (and by no means unsympathetic) review of *Flim-Flam!* in *Theta*, Douglas M. Stokes (1981) writes: “In fact, almost all of the phenomena and claims Randi critiques in the book would be equally quickly dismissed by any competent parapsychologist as well. Only the lunatic fringe is going to be outraged by Randi’s exposure of Conan Doyle’s pictures of fairies, the underwater pyramid and road near Bimini, the space voyages of Ingo Swann and Harold Sherman, the Sirius ‘mystery,’ ancient astronauts, ‘transcendental levitation,’ biorhythms, N-rays, psychic surgery, or the oversexed spirits of Kübler-Ross.”

This quote is interesting not only because it reveals what Stokes thinks of several of the best-known practitioners of Future Science but also because it implies the existence of a class of persons deserving the label “competent parapsychologists” and easily distinguishable from the crackpots who believe in the Cottingley Fairies, psychic surgeons, and the cosmic outings of Mr. Swann.

I suspect that both Stokes and the skeptics somewhat oversimplify matters: the skeptics, by underestimating the internal differences within parapsychology; Stokes, by projecting an all-too-neat competent/incompetent dichotomy on the complex and confusing reality of modern psychical research.²

It simply will not do to reproach the critics for discussing certain outlandish claims in the context of a critique of “parapsychology” or for attacking “weak” and “unrepresentative” cases, since the “parapsychological community” itself cannot agree on the criteria for “strength” and “representativity.” We are faced with a similar problem if we want to decide who does or does not belong in the “community.” Several proponents have suggested that membership in the Parapsychological Association (PA) and/or a record of publications in the PA-affiliated journals be regarded as a suitable criterion. However, membership in the PA and a record of publications in the serious journals does not guarantee the absence of the sort of beliefs Stokes thinks characterize the lunatic fringe. In parapsychology, the chaff and the wheat overlap to such an extent that a neutral observer often finds it hard to tell the difference.

2 Elsewhere in his review, however, Stokes (1981) writes of “a wide continuum of parapsychologists, ranging from the skeptical to the credulous, with no clear line of demarcation separating the two groups.”

This of course does not justify overgeneralizations on the part of the critics. Precisely because parapsychology is an ill-defined field lacking a shared “paradigm,” it would be unfair to hold each “parapsychologist” individually co-responsible for everything that is claimed by his or her nominal colleagues.

My purpose in this essay-review is to illustrate the previous points by comparing three recent books written by prominent European parapsychologists. One of these books is a clear refutation of the claim implicit in some critical publications that parapsychology is ipso facto antagonistic to skeptical inquiry. The two other books demonstrate with equal clarity that the sort of parapsychology skeptics rightly find objectionable is not confined to the National Enquirer and the ad pages of Fate.

Martin Johnson

Martin Johnson, the Swedish professor of parapsychology at Utrecht State University, is a somewhat controversial figure in the Netherlands – because local “believers” suspect him of being a closet skeptic. When, around 1973, the university authorities announced their intention to appoint Johnson “professor ordinarius,” the Dutch Society for Psychical Research, dominated by the redoubtable Dr. Wilhelm Tenhaeff (who, much to his chagrin, had never been promoted from his second-rate status as a “special professor”), initiated an unprecedented and outrageous press campaign against that “Nordic woodchopper” who, because of his “gross incompetence,” would “destroy the life’s work of the nestor of Dutch parapsychology.” (For more about Tenhaeff, see my two-part article on Gerard Croiset in *Skeptical Inquirer* Fall 1981 and Winter 1981-82.³) Newspaper offices were flooded with angry letters. Questions were asked in Parliament. With a few notable exceptions, the Dutch media supported the “genius Tenhaeff” against the intruder from the Lapp tundra. The university was forced to accept a compromise. Johnson was appointed ordinarius but in addition Henri van Praag was appointed “special professor” to guard Tenhaeff’s heritage.

Since then, the “special professor” has kept Dutch occultists happy with breathtakingly uncritical books, articles, and lectures on Sai Baba, flying saucers, Rosemary Brown, reincarnation, fairies and leprechauns, Uri Geller, Ted Serios, psychic surgery, and the imminent Age of Aquarius, while Johnson quietly established what has now become one of the most prestigious and respected parapsychology laboratories in the world.

Parapsychologie (originally published in Swedish) is Martin Johnson’s first book (Johnson, 1982). It is intended as a general introduction to this controversial field. In

3 Both parts of that article are reprinted (as chapters 3-04 and 3-05) in this book. (Eds.)

refreshing contrast to most such introductions, it contains no pompous statements to the effect that the existence of psi has been demonstrated beyond any doubt and that only blind materialist prejudice keeps the scientific community from joining the parapsychological revolution.

To the contrary: Johnson agrees with the skeptics that the evidence for psi is weak and ambiguous and quite unable to support the grandiose cosmological claims others have tried to base on it. On the other hand, he believes some of the evidence is sufficiently suggestive to warrant further research based on the reality of psi as a working hypothesis. While Johnson personally is inclined to predict that future investigations will vindicate the psi hypothesis, he insists that the hoped-for breakthrough can only result from applying more rigorous research methods and from exercising more self-criticism.

Johnson agrees with his colleagues that there is considerable empirical support for the claim that something in the nature of ESP and PK (psychokinesis) exists, but he does not believe that this empirical support amounts to anything like proof positive. In a concise survey of the evidence presented hitherto he notes some promising developments (such as Helmut Schmidt's work with random-event generators and some research into psi and personality) but concludes that even with the most sophisticated experiments potentially fatal problems remain. In this context, he is remarkably candid about the role unconscious manipulation and deliberate fraud may play in his field. In this book, we are spared the ritual complaints about C.E.M. Hansel's supposed pig-headedness. Instead, Hansel's critique is welcomed as an interesting contribution to the debate.

There is an intelligent discussion of the replicability problem that has bedeviled parapsychology ever since its inception. Johnson points out that the concept of a "repeatable experiment" is more complex than is often assumed by critics. In mainstream science, opinions wildly differ as to the level of replicability required for academic respectability, whereas history has shown examples of perfectly repeatable observations based on collective misconceptions. However, Johnson does not invoke these methodological subtleties in order to excuse parapsychology's shortcomings. He is quite firm in stating that replicability in parapsychology is insufficient, especially given the extraordinary nature of the claimed phenomena. A fairly long chapter deals with the numerous attempts to make theoretical sense of psi. Johnson concludes that almost all such attempts precariously depend on "more or less fantastic auxiliary hypotheses" and usually raise more questions than they answer.

In a hilarious chapter on "Miracle Men" Johnson practices some hard-line debunking at the expense of the Uri Gellers and the Sai Babas – and of the parapsychologists who

have uncritically endorsed these psychics. “Personally, I am amazed that an intelligent and honest man such as Erlendur Haraldsson [the Iceland parapsychologist who published some remarkably naïve eyewitness-accounts of the Indian saint’s feats] seriously considers the possibility that Sai Baba, or the Babas of lesser caliber, could be anything but ordinary frauds,” he writes.⁴

The section on Uri Geller and other metal-benders is devastating – and should make some of Johnson’s fellow parapsychologists blush with embarrassment. At the occasion of the 1976 Utrecht parapsychology conference, which he hosted, Johnson invited a Swedish amateur magician, Ulf Mörling, to demonstrate “psi” for the benefit of the assembled participants. From the outset, Mörling clearly stated that he did not claim any paranormal ability whatsoever and that all his feats were based on conjuring tricks. Alas, Johnson writes, after the performance was over several prominent parapsychologists became “skeptical” and started to speculate seriously about whether Mörling might be a genuine psychic without being aware of it. The PA member who most staunchly defended this theory was – the reader will have guessed – Ed Cox, former associate of the late Dr. Rhine and a self-proclaimed foolproof expert on magic.

Johnson is appalled by the credulity some of his colleagues exhibited at the height of the Geller psychosis. He believes that this greatly contributed to the skeptical backlash of the second half of the seventies.

Johnson is not overly optimistic about his field’s immediate outlook: “I think that parapsychology is presently in a critical stage. More unambiguous and robust findings will have to be presented if we want to justify its continued presence at the universities.” And: “Time will tell whether psi research will bring about a conceptual revolution – or will languish in the backyards of the established sciences.”

Having read *Parapsychologie* several times I am struck by the remarkable similarities between Martin Johnson’s views and those of Ray Hyman, the skeptical psychologist who, among other things, is a member of the Executive Council of CSICOP. The book hardly contains a single statement to which a skeptic could reasonably object – unless he resorts to the *a priori* argument that the inherent absurdity of the concept of psi renders any serious attempt at investigation a waste of time.

4 Dr. Haraldsson kindly sent me his comment on Dr. Johnson’s criticisms. He argues that Johnson implicitly adopts a criterion that would justify summary rejection of *any* anomalous claim. [Haraldsson’s letter is in the Hoebens Files. (Eds.)]

Hans Bender

Professor Dr. med. Dr. phil. Hans Bender is a big name in international parapsychology. Arguably, he is the most renowned representative of the field in continental Europe. His credentials are impressive. He is a (now retired) professor at the Albert-Ludwigs University in Freiburg, a former president of the PA, founder of the serious *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, a onetime host to international conferences, a contributor to John Beloff's state-of-the-art volume, *New Directions in Parapsychology* (Beloff, 1974b), and author of numerous papers published in reputable journals. By any definition, he belongs to the core of the international parapsychological community.

Bender too is a controversial figure at home. While thousands of Germans adore him as a prophet of the New Age of post-mechanistic spirituality, the highbrow media in West Germany derisively refer to him as “der Spukprofessor.” Bender has frequently and bitterly complained that he has been the victim of unfair criticism. He certainly has a point here: The average postwar German skeptic is hardly noted for polemical subtlety. Bender's enemies have mercilessly exploited an embarrassing incident that took place a few years ago. (The magazine *Der Spiegel* alleged that, for three decades, Bender had falsely sported a “Dr. med.” degree. The professor was unable to produce evidence to the contrary. Recently, he obtained a genuine medical degree on the strength of an extremely curious thesis on poltergeists.) All too often, they have indulged in ad hominem attacks and in misrepresentation of the claims they had set out to debunk. The anti-parapsychological writings of the Mannheim jurist Dr. Wolf Wimmer in particular contain a number of deplorable examples.

A closer examination of Bender's publications, however, may to a certain extent explain why parapsychology continues to arouse such hostile feelings among German rationalists.⁵

Like Martin Johnson's *Parapsychologie*, Hans Bender's *Unser sechster Sinn* (“Our Sixth Sense”), a revised and enlarged edition of which became available in 1981, is a general introduction intended for a lay public (Bender, 1981a). The authors of such books bear a special responsibility, since they must assume that, for the average reader, *this* book

5 To a certain extent only. Too many German critics have ignored the fact that, after the emergence of the “new conservatism” in German parapsychology, the field is no longer monopolized by Bender [and now Benderian thought; eds.]. Ideologically, these new conservatives (e. g., Eberhard Bauer, Gerd H. Hövelmann, Klaus Kornwachs, and Walter von Lucadou) are close to Martin Johnson. For an excellent survey of [then; eds.] recent developments in Germany see Bauer & Lucadou (1983).

will be the most authoritative source of information on parapsychology he will ever be exposed to. General introductions, especially if written by university professors, decisively influence opinions and beliefs. That is why we may demand that the authors carefully refrain from overstating their case and give a fair presentation of the pros and the cons. Johnson's book adequately meets this criterion, as did a small number of earlier publications, such as those by West (1954) and Beloff (1973).⁶ *Unser sechster Sinn*, I am afraid, does *not* belong in this category. The purpose of this book is propagandistic rather than informative. The reader is urged to accept Bender's beliefs as scientifically established facts and is not alerted to possible rational objections to the author's views. The weaker points of parapsychology are carefully glossed over. Instead, we are regaled with the success story of a triumphant new science with revolutionary implications for our views of God, Man, and the Universe.

I believe that I am not the only reader to gain the impression that Bender basically is not interested in evidence, except when it can be used to illustrate a transcendent Truth that he personally would be happy to embrace without any evidence at all.

From Bender's discussion of so-called spontaneous phenomena, the casual reader will never guess why informed critics (including several prominent parapsychologists) resolutely refuse to accept such anecdotes at face value. Examples of seemingly perfect cases that were later conclusively exposed as due to error or fraud are conspicuously absent, although such examples are essential for understanding the controversial status of psi. The "normal" psychological factors that may lead to an "occult" interpretation of non-paranormal events are hardly mentioned at all. Alternative hypotheses to account for the data are either ignored, dismissed, or presented as applying only to an untypical subset of cases.

I have reasons to take Bender's anecdotes with a grain of salt. The fact that he repeats the long-discredited claim that Jeane Dixon "predicted the assassination of John F. Kennedy" should suffice as a warning.

A similar bias is apparent in the sections on the mediumistic phenomena that were the main subject of pre-Rhine psychic research. Eusapia Palladino is discussed without any mention of the numerous occasions she was caught in fraud. The exceptionally important writings by the great German skeptics who flourished in the first decades of this century are ignored, except in one instance where Bender gives the wrong author for the chapter on the Schneider brothers in the classic "Drei-Männer-Buch"

⁶ Including a remarkably well-balanced chapter on parapsychology.

(Gulat-Wellenburg, Klinckowstroem & Rosenbusch, 1925)⁷ and dismisses the critics' arguments without even telling us what these arguments amounted to.

The classic laboratory experiments of the Rhine era are dealt with in a similar spirit. Hansel's criticisms of the celebrated ESP tests with Hubert Pearce are summarily dismissed as having been conclusively refuted by Honorton and Stevenson. The naïve reader gains the impression that there never was any *serious* dispute over the work at Duke University.

Inexcusably, Bender has chosen to leave the section on Soal's experiments with Basil Shackleton virtually unchanged in the 1982 "revised edition." These experiments are presented as having provided extraordinarily strong evidence for ESP. Only in a later chapter – in a totally different context – does Bender casually remark that "tragically, doubts later arose as to the accuracy of some of Soal's protocols." Given the well-nigh incontrovertible evidence that this psychical researcher faked the most sensationally successful experiment in the history of parapsychology, Bender's discussion of Soal's work is – to put it mildly – utterly misleading.

The sections on the Rhine/Soal type of ESP and PK experiments performed in Freiburg present us with problems of a different nature. Bender claims fantastically significant results, but such claims are meaningless unless complete reports are available for skeptical scrutiny. The exact conditions prevailing during these experiments are anybody's guess. Inquiries in Germany revealed that no detailed reports were ever published. We have no means of knowing to what extent possible skeptical counter-hypotheses are consistent with the data. Could the significant results of the Achtert-Zutz experiments have been brought about by a coding system? Could the high-scoring subjects in the Pinno-Czechowsky experiment have filled in their scoring sheets *after* the random-event generator had produced the targets? Bender cannot blame the critic for being suspicious, especially since, on p. 62, he himself compares some of the Freiburg experiments to the Soal-Shackleton series.

In the (new) section on Gellerism, Bender alludes to attempts on the part of certain anonymous magicians to expose the Israeli metal-bender as a trickster, but he typically fails to provide the sort of details that might persuade the intelligent reader to agree with the prosecution. His conclusion is that Geller may on occasion have resorted to trickery ("as do almost all mediums when they are unsuccessful") but that "in *The Geller Papers*

7 This is the classic critique of the so-called "physical phenomena." It soon acquired the sobriquet "Drei-Männer-Buch" (Three men's book).

the physicist Charles Panati⁸ has published experimental results that *prove* psychokinesis” (emphasis added). No mention is made of the devastating criticisms of *The Geller Papers* by, among others, Martin Gardner (1981) and Christopher Evans (1977). We are not even allowed to know *which* “experimental results” Bender thinks have proved Geller’s PK.

The metal-bending star-subject at the Freiburg Institute – the Swiss Silvio M. – is introduced as a genuine psychic who has been able to perform his feats while observed by an acquaintance who is a member of the Berne magic circle. Bender does *not* tell us that Silvio was unable to demonstrate any PK while observed by the prominent German magician Geisler-Werry and by Freiburg’s own trick expert Lutz Müller or that he was caught cheating on several occasions. While I do not deny that there may be an as yet unexplained (as opposed to inexplicable) residue in the Silvio evidence, I object to Bender’s suppressing facts that might cause his readers to doubt the authenticity of the Silvio phenomena.⁹ Similarly, I object to Bender’s uncritical endorsement of Ted Serios’ “thoughtography.” *Unser sechster Sinn* is completely silent about the serious doubts that have been raised by skeptics and critical parapsychologists alike concerning the paranormality of these feats.

The sections on Bender’s favorite clairvoyant – the late Gerard Croiset of Holland – are nothing short of disastrous. Bender credits Croiset with having paranormally located the remains of a missing Scottish woman in the early seventies, whereas in fact her body has never been found. He further enthusiastically relates the astonishing results of the 1953 “chair test” in Pirmasens, where Croiset is supposed to have given a highly accurate precognitive description of two persons who, at a specified moment in the future, would happen to be seated in specified chairs. Not only is Bender’s “paranormal” interpretation of this case absurd; his account also abounds with factual errors. Bender cannot claim ignorance in this instance, as he himself had been the chief experimenter, and the “raw data” are kept at his own institute. Pirmasens is one of Bender’s prize cases, and he has referred to it in numerous books, articles, and lectures. My own investigations into this alleged miracle have raised serious doubts about Bender’s credibility as a reporter of unusual events.¹⁰ In his evaluation on the 1969 “transatlantic chair test” U.S. para-

8 Panati (1976). (Eds.).

9 Silvio’s cheating is documented in the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, vol. 23, no. 2, 1981 (Mischo et al., 1981). In recent years, this journal has adopted an open policy. Skeptical contributions are welcome. Here, the growing influence of the “new conservatives” is felt.

10 My exhaustive critical analysis of this alleged miracle is scheduled for publication in the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*. [See “Farewell to Pirmasens,” chapter 3-11 of this book. (Eds.)]

psychologist Jule Eisenbud made with Croiset, there is a curious discrepancy with the original 1972 edition. In 1972, Bender called this experiment “successful.” In 1981 he calls it “controversial.” What has caused Bender to change his mind? We are not allowed to know. Needless to add that Bender refrains from informing his readers of the reasons [why] many of his fellow parapsychologists now regard the late Wilhelm Tenhaeff, Croiset’s chief chronicler, as a disgrace to the profession. Instead, Tenhaeff is hailed as one of the pioneers of psychical research.

The section on poltergeist phenomena naturally gives pride of place to the celebrated Rosenheim case of 1967-68, which was investigated by Bender and his team. Rosenheim is generally considered one of the most striking ghost stories of all time, and not without justification. From the available material it seems difficult to think of a nonparanormal scenario to account for the data without leaving an uncomfortable number of “loose ends.”

However, the case is certainly not as strong as Bender suggests. No full report of the investigations has ever been published, so we are in no position to check to what extent the parapsychologists have been successful in excluding naturalistic explanations. A case in point is the heavy (about 175 kilograms) cabinet that is said to have been moved 30 centimeters away from the wall by a paranormal agency. It is implied that Annemarie S., the young office-girl who was seen as the focus of the disturbances, could never have achieved this by normal means. However, in Bender’s accounts one searches in vain for the answers to such essential questions as: Did anyone witness the actual movement of the cabinet? Did the cabinet weigh 175 kilograms when empty or is the weight of the files that were kept there included? Did the cabinet have handgrips? What experimental evidence has convinced Bender that 19-year-old girls cannot move 175-kg cabinets? (Experiments with my own 230-kg piano suggest that they can.)

Worse is that Bender omits from his account the highly significant fact that Annemarie was caught in fraud by a policeman. Neither does he mention the inconclusive but curious discoveries reported by the Viennese magician Allan after a visit to the Rosenheim office during the poltergeist outbreak (Allan, Schiff & Kramer, 1969). He states that it was possible to capture a “phenomenon” (a painting turning around “120 degrees” – that is 200 degrees less than was claimed in Bender’s first report!) on Ampex film. He does *not* tell us why persons who know something of the background of that incident refuse to be impressed with this piece of evidence.

In none of his publications of which I am aware has Bender ever referred to the suspicious features of the case.¹¹ Presumably, his silence has misled Eysenck and Sargent

11 It is only fair to mention at this point that inadequate reporting on poltergeist cases is not

(1982), in their militantly pro-psi book *Explaining the Unexplained*, to claim that “despite the fact that many people – highly trained in different disciplines – were looking for evidence of fraud all the time, no hint of it was ever sniffed.”

The publication of Bender’s most recent book, *Zukunftsvisionen, Kriegsprophezeiungen, Sterbeerlebnisse* (“Precognitive Visions, War Prophecies, Death Experiences”), has done little to restore my faith in the nestor of German parapsychology (Bender, 1983a). A detailed examination of this incredible work would be beyond the scope of this article. I will restrict myself to exposing what to the uninitiated reader must appear to be a perfect proof of the reality of precognitive ESP. On the first page of the book, under the chapter heading “Visions of the Future from a Scientific Perspective,” he tells the story of the American student Lee Fried, who, Bender says, in 1977 dreamt about a recently deceased friend who showed him a newspaper bearing a future dateline. The headlines referred to a collision of two 747’s over Tenerife with 583 people dead. Fried informed the president of his university of his premonition. Ten days later, the terrifying dream came true to the letter. According to Bender, “the opponents” will try to explain away such miracles by questioning the accuracy of the facts but in the case of the Tenerife prediction they stand no chance, for the documentation of the facts cannot be faulted. (In the chapter on parapsychology that Bender and his apprentice Herr Elmar Gruber contributed to *Kindlers Handbuch Psychologie* [Bender & Gruber, 1982], it is stated that the Fried prophecy is “reliably documented.”) All the stubborn skeptics could possibly do, Bender says, would be to resort to the preposterous hypothesis that the perfect match between premonition and actual disaster could have been brought about by chance.

Alas, the paranormal warning-system does not seem to have worked for Hans Bender when he wrote down those paragraphs. His version of the facts would indeed seem to preclude a naturalistic explanation. However, he managed to get all the crucial facts wrong. The Fried “prophecy” is a well-known, much-publicized, well-documented, and confessed hoax. In Bender’s account, the facts have been distorted almost beyond recognition.

Fried of course never *told* the president of the university of the impending Tenerife disaster.¹² What he did do was to put an envelope, said to contain an unspecified “predic-

Bender’s monopoly. In 1978 the criminologist Dr. Herbert Schäfer told the press that Heiner Scholz, focus person of the celebrated Bremen case of 1965-66, had made a complete confession. Bender and his colleague Johannes Mischo have pointed out serious flaws in the fragmentary press accounts of this exposé (Bender & Mischo, 1978). Schäfer never published a complete report of his findings, nor has he publicly replied to the parapsychologists’ counter arguments.

12 Personal communication from Terry Sanford, president of Duke University, July 1983. [Sanford’s letter is preserved in the Hoebens Files. (Eds.)]

tion,” in a locked drawer in the president’s office. When the catastrophe had taken place, the envelope was opened and a piece of paper with the words “583 Die in Collision of 747’s in Worst Disaster in Aviation History” was produced. Soon after, Lee Fried frankly admitted that he had planned the prediction as a stunt. The slip of paper containing the “prophecy” was inserted only *after* the disaster, by sleight of hand.

According to *The Second Book of the Strange* (Gadd, 1981) the Fried episode has shown that “the credulousness of at least a proportion of the news-consuming public is almost unlimited.”

We cannot really blame the public for occasionally failing to distinguish between fact and fraud. However, we are entitled to expect better from the most prestigious representative of scientific parapsychology in Germany.

Discussion and Conclusion

Hans Bender has stated that his conviction that the paranormal exists is “unshakable.” Furthermore, he has repeatedly affirmed his belief that a wider acceptance of psi will be highly beneficial to mankind. Parapsychology is the supreme weapon against the “mechanistic world-view” of the intellectual establishment – a world-view that Bender holds responsible for many of modern society’s most serious problems. From the vantage point of the moralist, such considerations would justify a certain nonchalance vis-à-vis the scientific facts. As soon as one’s convictions become unshakable, evidence ceases to be relevant – except as a means to convert the unbelievers – and factual inaccuracies in the parapsychological propaganda are excusable in the light of the Higher Truth. I do not wish to impugn Bender’s integrity. I am satisfied that his public statements and actions are consistent with his personal values. These values, however, are clearly incompatible with the spirit of scientific inquiry.

It is typical of the pre-paradigmatic status of parapsychology that Bender continues to be regarded as one of the foremost representatives of the field. Alcock has posed the question: Is parapsychology science or magic? No unambiguous answer can as yet be given. While Martin Johnson has shown that at least *some* parapsychologists are engaged in activities virtually indistinguishable from what critics mean by “skeptical inquiry,” the case of Hans Bender demonstrates that the demarcation line separating scientific parapsychology and fringe occultism is by no means as sharply drawn as some proponents have optimistically claimed.

CHAPTER 2-12

Editorial Introduction

The following paper demonstrates that Hoebens must be ranked with those who, like Sybo A. Schouten¹ in the Netherlands and Marcello Truzzi² in the United States, have pointed out the need for a theoretical framework for the evaluation of the claims of so-called psychic detectives. Some circumstantial information on the history and development of this paper may help readers to fully appreciate its gist.

(1) Hoebens' paper was originally published in A Skeptic's Handbook of Parapsychology (Buffalo, NY: Prometheus Books, 1985, pp. 631-643) edited by CSICOP-chairman Paul Kurtz. That book was explicitly conceived by its editor and publisher as providing a skeptical counter-balance to the prestigious, but only marginally influential Handbook of Parapsychology (Wolman, 1977) that had been published several years previously. However, during the planning stage for the book, it was Hoebens who had suggested to Kurtz that, in order to prevent a pronounced one-sided presentation, he also invite several representatives of parapsychology to contribute chapters to this skeptical anthology, and he had added a short list of possible candidates who then in fact were invited by Kurtz to submit chapters on certain pre-specified topics.³ This is how several contributions by nominal (then-)parapsychologists (Charles Akers, John Beloff^a, Susan J. Blackmore, Gerd H. Hövelmann, Betty Markwick, D. Scott Rogo, Douglas M. Stokes) made their way into the Skeptic's Handbook.

1 See Schouten (2002-2004).

2 See Lyons & Truzzi (1991), a book dedicated to the memory of Piet Hein Hoebens; also, see Truzzi (1990-1991).

3 The respective correspondences by most individuals involved are preserved in the Hoebens Files.

4 When, in 1990, Dr. John Beloff (1920-2006) decided to reprint his chapter from the Kurtz *Handbook* in his own book, *The Relentless Question*, a collection of his theoretical reflections on parapsychology, he added an additional preface to the chapter in appreciation of the role Piet Hein Hoebens had played in it. The most relevant portions read: "I had been told that Piet Hein Hoebens had persuaded Kurtz that his handbook would be improved if he included contributions from 'believers' as well as skeptics in contrast to the Wolman handbook, which had no contributions from skeptics. [My paper] represents my response to Kurtz' welcome invitation. I was fortunate that Hoebens himself offered to read my original draft and it was his valuable comments that led me to rewrite it drastically in the form in which it was published. This gifted Dutch journalist, though an avowed skeptic whose investigative journalism had been put to good skeptical use, was also a highly respected figure in the parapsychological community. His suicide in October 1984, at the early age of 36, was a devastating blow to all who knew him [...] I duly dedicated my own contribution to his memory." (Beloff, 1990, p. 147)

(2) As Hoebens mentions in his paper, Marcello Truzzi's Michigan-based Center for Scientific Anomalies Research, in 1980, initiated, with Hoebens' assistance, a long-term research project into the gifts of psychic detectives. The voluminous, diary-like correspondence between Hoebens and Truzzi runs to many dozen pages devoted to the details and rationale of this project that eventually culminated in the book *The Blue Sense* (Lyons & Truzzi, 1991).

(3) Hoebens' untimely death in October of 1984 precluded the completion of his own chapter for the *Skeptic's Handbook*, which was to be published in 1985. At the end of his farewell letter to his friend Marcello Truzzi, Hoebens shortly before his death – “which I intend to take place in two days time” – added a “final request: please complete my chapter on psychic sleuths. I enclose the completed [type-written] first half of the ms. plus the sketches for the rest. Tell Paul [Kurtz] that the chapter will be ready as promised.”⁵ Those who ever had a chance to see Hoebens' characteristic, almost undecipherable handwriting – swiftly floating notes that leave out many individual letters, much like an idiosyncratic shorthand system – will immediately understand that this “final request” created a slightly awkward situation for Marcello Truzzi and his attempts to eventually complete Hoebens' chapter on psychic sleuths. Thus, in a letter of June 6, 1985, Truzzi wrote to G.H.H.: “I am having a devil of a time deciphering Piet Hein's handwriting for the section of his paper for the Kurtz volume. I enclose xerox copies of his originals along with a copy of the deciphered parts I feel reasonably sure about. Since you also knew Piet Hein's handwriting, perhaps you could read over what he wrote and see what you can make of it. I suggest that you first try to decipher a paragraph from his handwriting without looking at what I came up with for it. I may have misdeciphered and this might lead you astray.”⁶ This resulted in a list of 57 tentatively deciphered portions of text going back to Truzzi.⁷ Remaining contingencies were “negotiated” during a couple of phone conversations. Readers must be aware, therefore, that Truzzi – and G.H.H., to some degree – had some input especially in the latter half of the following paper and that they may not have been fully successful, in each and every case, in reconstructing the wording that Hoebens himself would have chosen.

The use of psychics for police investigations continues to be a much-debated topic to this day. In addition to the literature referred to in the following article, studies from various perspectives (most of them written after Hoebens' death) that interested readers might usefully consult include Leeflang (1980), Hibbard & Worring (1982), Lucas (1985), Neu (1985), Boerenkamp (1988), Gerding et al. (1989), Lyons & Truzzi (1991), Sweat & Durm (1993),

5 Letter Piet Hein Hoebens to Marcello Truzzi, undated (October 20, 1984). Copies of the materials sent to Truzzi also are preserved in the Hoebens Files.

6 Letter Marcello Truzzi to Gerd H. Hövelmann, June 6, 1985.

7 Letter Gerd H. Hövelmann to Marcello Truzzi, June 18, 1985.

Nickell (1994), Schouten (1994), Truzzi (1995), Wiseman et al. (1996a, 1996b), O'Keeffe & Alison (2000), Schetsche & Schellinger (2007) and, in particular, Schouten (2002-2004).

Hoebens' paper is reprinted here with the kind permissions of the late Paul Kurtz and Marcello Truzzi. (Eds.)

REFLECTIONS ON PSYCHIC SLEUTHS⁸

If the voluminous “psi” literature on the subject is to be believed, the Watsons in the world’s police departments owe the solution of some of the most baffling mysteries they have ever confronted to the intervention of the psychic counterparts of Sherlock Holmes.

The blurb on the cover of Colin Wilson’s *The Psychic Detectives* (1984) neatly summarizes the claimant’s position: “No established psychological or criminological science can explain it ... the astonishing and recurrent phenomenon of those who can simply touch a garment or some other item to trigger their extraordinary psychic powers to solve a crime, identify a murder, locate a corpse, even predict where a killer will strike again.”

The employment of sensitives for police purposes has for many decades been a highly controversial topic. Professional opinion is sharply divided. Some police officers have championed the cause of their paranormally gifted colleagues, whereas others have acted as the spokesmen of implacable skepticism. The controversy of course is intimately linked to that surrounding the existence of extrasensory perception in general. If such faculties as telepathy and clairvoyance indeed exist, it is only natural to expect that, at least in some cases, they might be of practical use. On the other hand, if the “paranormal” is a delusion, it would seem slightly foolish to believe that adding “third eyes” and “sixth senses” to the contents of the police officer’s toolbox would serve any useful purpose.

It should be made clear from the outset that the author of this essay does not think that the question of the reality of psi can ever be settled by disputes over anecdotes pertaining to historical – and necessarily irreproducible – incidents. All accounts of psychic successes in police investigations are in this category. Psi can only be demonstrated to the satisfaction of the skeptical observer if the parapsychologists succeed in their attempts to distill from their vast collection of miraculous anecdotes at least one hypothesis that can

8 This essay was edited by M. Truzzi from the notes left him by Piet Hein Hoebens, whose untimely death precluded its completion. © Copyright 1985 by the Center for Scientific Anomalies Research.

be checked and rechecked by independent researchers, with results that consistently confirm what parapsychology predicts. The field's signal failure to meet this demand is at the bottom of what impatient proponents deplore as the persistence of skeptical obstinacy.

However, there is no denying that the claims surrounding psychic detectives (and the many other "spontaneous" or "real life" manifestations of psi) are of considerable interest to both the believing parapsychologist and his incredulous counterpart. Weak as the evidence may be, it does lend substance to the anemic experimental findings of parapsychology, and its heuristic value can hardly be overestimated.

A second reason such cases are of interest to the debate over the paranormal is that they can provide the sort of evidence (whether positive or negative) that would compel the Bayesian observer to modify his or her expectations as to the most probable outcome of the psi controversy. Although accounts of isolated incidents can never conclusively demonstrate the reality of ESP, I can conceive of a series of such incidents sufficiently striking and sufficiently resistant to debunking attempts that I personally would find it judicious to change my bets and provisionally join the moderate wing of the believer's party.

A third reason – which for the sake of argument assumes the nonreality of psi – is that a scientific examination of the claims of psychic detection may throw fresh light on the mechanisms of human error and human credulity – and perhaps also on some human abilities that, while not of a paranormal nature, are sufficiently unusual and unexpected to make it understandable that they have so often been taken for the miraculous.

Basic Questions

In discussing psychic detectives, it is essential to make a clear distinction between two very different questions: (1) Have persons claiming extra-sensory abilities in fact been successful in solving police cases? and (2) Is ESP the most likely explanation for what we have reason to believe are the facts of psychic detection?

This distinction (rarely made in the popular or quasi-scholarly literature on this topic) is important, as it is quite conceivable that a psychic could score a remarkable success in paranormally obtaining accurate information that does not, however, lead to the actual solution of a police case. On the other hand, it is conceivable that a psychic could be successful in solving a crime or locating a missing person but not obtain the relevant information by nonparanormal methods.

In this essay, I will attempt to clearly differentiate between the reliability of the claims and the inexplicability of claims presumed to be factually accurate.

Sources, Theories and Countertheories

A major problem confronting the critical investigator of psychic detectives is that the subject has been virtually ignored by the leading practitioners of parapsychology. The literature is vast, but very little of it can in fairness be said to be representative of parapsychological thinking at its best.

In short, there is no accepted parapsychological theory of psychic detection that the skeptic could juxtapose with his own. This is regrettable, as the critic naturally prefers to focus his criticisms on the cases that are deemed “strong.” To the extent that the relevant literature may claim to be scientific or scholarly, it is almost exclusively written by skeptics or by police officers who tend to approach the topic from the vantage point of the pragmatic law-enforcer rather than from that of the scientific researcher primarily interested in to what extent any factor unknown to conventional science might be involved.

The most authoritative source of information on psychic detectives remains the work of the pre-war German Landgerichtsdirektor Albert Hellwig, notably his magnum opus *Okkultismus und Verbrechen* (Hellwig, 1929). My own investigations into the claims surrounding the best-known Dutch “paragnosts” may be seen as a contemporary footnote to Hellwig, confirming his essentially negative conclusions. Experimental and semi-experimental findings supporting the skeptical position have been published by M. Reiser (Reiser *et al.*, 1979, Reiser & Klyver, 1982) and F. Brink (1960). At the moment of this writing, the long-term Psychic Sleuths Project, under the auspices of the Center for Scientific Anomalies Research (CSAR) and directed by the skeptic Marcello Truzzi, is under way.

On the psi side, the work of the Dutch parapsychologist Wilhelm Tenhaeff has long been hailed as exemplifying scholarly psychical research, but it has now been largely discredited. The literature in English is dominated by popular and sensationalist books like the monographs by F. Archer (1969), P. Tabori (1974), J. H. Pollack (1964), and C. Wilson (1984), and the autobiographies of noted psychics.

A renewed interest in practical applications of psi (exemplified by the California-based Mobius Society and the Canadian Psychic Systems Research group) may eventually lead to the emergence of a modern pro-literature of better quality.

The Skeptical Explanation

The standard skeptical explanation for the alleged successes of psychic detectives is that these sensitives offer their consultants the verbal equivalent of a Rorschach test. Their statements are typically vague, rambling, and verbose.

The accuracy of the “readings” is evaluated post factum: “Good” sitters retroactively interpret their ambiguous and often contradictory statements in such a way that they fit the true facts and obligingly forget the many details that were too wide of the mark.

Complete failures are ignored or suppressed.

The possibility that some of the paranormal information could have been acquired by normal means is quietly discounted.

Occasional lucky guesses (consistent with the chance hypothesis) are enhanced by selective reporting and editorial embellishment.

Cautious parapsychologists usually concede that this explanation is adequate in the vast majority of cases. However, they point out that, in some instances, a sufficient number of “loose ends” remain even after a thoroughly skeptical examination of the relevant data to justify a suspension of judgment.

The CSAR Project. In 1980 the Michigan-based Center for Scientific Anomalies Research initiated a long-term research project into the gifts of psychic detectives. At the time of this writing [Fall 1984], the mass of documents carefully collected by the CSAR investigators does not yet justify an unambiguous verdict on all cases. While the findings generally support the case for skepticism, Truzzi and his collaborators (the author of this chapter included) prefer to suspend judgment in a few instances that, prima facie, seem to be exceptions to the negative rule.

The Reiser test. In 1978, M. Reiser (Reiser *et al.*, 1979), director of the Los Angeles Police Department’s behavioral science section, conducted a controlled test with 12 carefully selected sensitives who were asked to look at evidence from four crimes.

According to the report, little if any useful information was elicited. A follow-up study showed that psychics are not better at making good guesses than are detectives and students. As the Reiser tests were rather limited in their scope, proponents of psychic detectives may be excused for declining to be entirely convinced until the results have been replicated in an in-depth series of similar experiments.

Reiser reports that the Behavioral Science Services staff of the LAPD “has received several first-hand accounts of reported success with psychics from several other departments.”

He has also suggested, however, that “perhaps the compelling manner in which self-identified ‘psychics’ tend to print the information may account for some of the positive beliefs about psychic abilities on law enforcement.”

The Reliability of the Reports

One of the most damning arguments against psychic detection is that, of the reports of “prize cases” in the pro-literature, many upon critical examination turn out to be grossly misleading or even fraudulent. One may be forgiven for wondering why, if there are convincingly “genuine” cases, proponents delight in regaling us time and again with pseudo-miracles. Here follow a few brief examples (recently published in a more extensive form) of much publicized, apparently inexplicable successes subsequently shown to have been spurious. A somewhat more detailed section will be devoted to three classic cases from pre-war Germany that feature in Tabori’s (1974) *Crime and the Occult*. To my knowledge, these cases of misreporting have not been exposed previously.

Claim: In 1951, the Dutch/American clairvoyant Peter Hurkos identified as the perpetrator of a series of arsons near Nijmegen the 17-year-old son of a respected local family whom the police believed to be beyond suspicion. To the utter amazement of the police chief, the boy confessed to the crime when confronted by Hurkos (Browning, 1970; Hurkos, 1961).

Facts: The 17-year-old arsonist, a mentally deranged farmer’s son, had been the prime suspect almost from the beginning. He was arrested after the police at the site of one of the fires found candy wrappings of a brand the boy had recently bought in large quantity at the local confectioner’s shop. Hurkos’ attempts to solve the case began only on the day after the suspect had been arrested and the case had for all intents and purposes been solved.⁹

Claim: Nutley, New Jersey, psychic Dorothy Allison told press reporters that she had given the name “Williams” to the Atlanta police long before a man of that name was arrested and convicted for the series of murders of black children (Allison & Jacobson, 1980). Moreover, she claimed, she had previously named “Williams” as the perpetrator of a series of homicides in Columbus, Georgia, and requested that the Columbus police forward this information to their Atlanta colleagues as soon as she heard of the slayings in that city. A spokesman for the Columbus police was cited in the press to the effect that Wayne Williams had indeed become a suspect in the Columbus murders before he was arrested by the Atlanta police.

9 For further details, see chapter 3-01 of this book. (Eds.)

Facts: It appears that, during her much publicized visit to terror-stricken Atlanta, Allison mentioned just about every name in the phone directory. James Randi (1982b) quotes Atlanta's Sergeant Gundlach as saying that Allison had given 42 names for the murderer but (almost surprisingly) not the correct one.

Truzzi spoke to two police officers Allison had mentioned to him as witnesses. One could not confirm having heard the name "Williams." The other one recalled that, at one time, Allison had mentioned a number of names, one of which was "Williams." Wayne Williams was not a suspect in the Columbus case, and a spokesman for the police denied that his department had ever had any reason to forward Allison's information to the Atlanta task force (Randi, 1982b; Truzzi, 1982a).

Claim: In 1979, the Dutch clairvoyant Gerard Croiset was consulted by the police commander of Woudrichem about a mysterious outbreak of arson. Croiset's description of the perpetrator would only fit the most unlikely suspect: a police sergeant in the commander's own police department. The commander was dumfounded when he later discovered that the psychic had been right (Tenhaeff, 1980b).

Facts: A tape-recording of the actual consultation proved that Tenhaeff, professor of parapsychology at Utrecht University, had fabricated the psychic's amazing "hits." The "protocol checked and signed" by the Woudrichem commander who Tenhaeff (1953b) claimed was basis of his report simply never existed.¹⁰

Claim: In 1958 the Dutch psychic Marinus B. Dykshoorn, now a United States resident, in a remarkable instance of long-distance clairvoyance, solved a theft case in Duisburg, Germany, while speaking on the telephone from his home in Breda, Holland. Dykshoorn identified the thief and gave very precise indications of where the booty had been hidden. The police confirmed to newspaper reporters that Dykshoorn had solved the case (Dykshoorn, 1974; Tabori, 1974).

Facts: The Duisburg police have repeatedly and flatly denied the claim. They insist that the case was solved by normal investigating methods. The accounts by Dykshoorn and Tabori contradict each other at several points. Dykshoorn refused my request to be supplied with documentary evidence supporting the claim.¹¹

10 See chapter 3-05 of this book. (Eds.)

11 See chapter 3-03 of this book. (Eds.)

Tabori's German Prize Cases

In *Crime and the Occult – How ESP and Parapsychology Help Detection*, Tabori (1974) relates a number of classic instances in which crimes were supposedly solved by means of clairvoyance. Of particular interest is the case of the occult detective August Drost, whose trial in 1925 received wide publicity in Germany and abroad. According to Tabori, Drost, a teacher, had a considerable reputation as a clairvoyant. He had “succeeded in a number of cases where the police had given up or had been unable to produce quick results.” A few of these cases will be critically examined presently.¹²

The Ballenstedt incident. According to Tabori, Drost had attempted to shed light on a burglary that had been committed in the house of a distinguished physician in Ballenstedt. The clairvoyant went into a self-induced trance, but the séance produced nothing very helpful. “Yet,” writes Tabori, “something strange *did* happen: Drost declared that the thief had taken from the Sanitätsrat’s desk a ‘greenish book’ – something ‘with which one makes money.’ When he was asked what he meant by this he said: ‘Something with which one collects money.’ Thereupon the victim went to his desk and found that his cheque book was missing – something he hadn’t known himself. Two police officials who were present confirmed this incident under oath.” According to Tabori, this is one of those cases where “coincidence can be excluded.”

Although Tabori claims that all the material in the chapter “has been taken from police archives or the personal memoirs of officials,” his version of the event actually appears to be a translation of a story published in the Berlin newspaper *Vossische Zeitung*.

In *Okkultismus und Verbrechen*, the classic work on psychic detectives, which is not even mentioned in Tabori’s book, Hellwig (1929) has discussed this case and shown it to be a perfect instance not of clairvoyance but of pseudo-ESP, recalling the far better known case of Sir Edmund Hornby. First it needs to be pointed out that Tabori is in error when he describes Drost as a clairvoyant. Drost was a hypnotist who worked with female mediums. It is true that the victim of the burglary (which took place in November 1922), Sanitätsrat Danziger, testified during the 1925 trial that Drost’s medium, while unsuccessful in her attempt to solve a crime, during a séance in 1922 had spoken of the theft of a checkbook – a fact that at the time had not yet been discovered.

12 The case of August Drost indeed was very widely discussed in the mid-1920s in German newspapers, general-interest magazines, established scientific and legal periodicals and in the parapsychological (*Zeitschrift für Parapsychologie*) and the reasonably skeptical journals (*Zeitschrift für kritischen Okkultismus*). Major parts of these discussions were summarized and evaluated in Hellwig (1929) which Hoebens refers to on several occasions. (Eds.)

Hellwig, however, was able to prove beyond doubt that Danziger was mistaken. He had been able to consult the original police files and had discovered that Danziger had already reported the loss of this checkbook 19 days prior to the séance. Drost had been handed a list of all the items reported stolen.

When confronted with this evidence, Danziger admitted that he must have been the victim of an extraordinary lapse of memory. He then recalled that he had made his statement to the authorities as a result of a suggestive question posed to him by the hypnotist. Drost had asked him: “You remember, do you, that you went to your desk and said: ‘Yes, it is true!’?” Danziger’s recollection of the actual event was not good, as he had never attached much importance to the loss of his checkbook – an item that the thief would find of no use whatever. Hellwig points out that the case is more remarkable in that Danziger was very skeptical of the paranormal in general.

The Schade case. Drost had a quite undeniable success, Tabori claims, in his attempts to solve a burglary of which a watchmaker and jeweler named Schade had been the victim. One of Schade’s assistants, Walter, was suspected at first but proved his innocence. Schade turned to Drost for help. This time Drost worked with the medium Louise Rennecke – who happened to be Walter’s fiancée.

At the séance, Drost and Rennecke identified a man named Franz as the thief. Drost added that the stolen goods were hidden under a layer of straw in a house on the Grosse Wasserreihe in Bernburg. Soon after, the police arrested a man named Franz Müller. The stolen goods were found where Drost had said they had been hidden.

In discussing this case Hellwig (1929) quotes the testimony of the police commander Heilman, who had handled the case. Heilman had told him that Schade (not a jeweler, incidentally, but the manager of a chemical plant) had initially suspected not only Walter but also a second employee of his firm, a certain Franz Montag, who lived on the Grosse Wasserreihe. This was the Franz who was later convicted of the crime. Walter never “proved his innocence;” although legal proof against him was lacking, Heilman was sure he had seen an accomplice. According to the police commander, the medium, since she was Walter’s fiancée, knew exactly what had happened and where the goods were hidden. In order to protect her lover, she had pointed only to Montag. Drost later told Hellwig that the medium, while in trance, had implicated not only Franz but Walter as well. At the medium’s request, the hypnotist had kept this secret.

The Rockmann case. According to Tabori (1974), “One of the most dramatic cases in which Drost had been involved concerned a murder and robbery in the house of a farmer named Rockmann in Calbe.” In February 1923 intruders into the Rockmann farmhouse

had been surprised by a certain Schlosser, who was brutally battered to death. There were no clues. Drost was called in, and he stated that the crime had been committed by two people, “Eddie” and “Aefer.” This was a hit; the culprits were soon arrested. Their names were “Ende” and “Schaeffer.” The clairvoyant, according to Tabori, had “read” the names, “but phonetically rather than visually, which explained the discrepancy.”

Hellwig’s investigations revealed that two individuals, named Ende and Schaeffer, had already been suspected prior to the séance. Moreover, the case was hardly as dramatic as Tabori invites us to believe. Ende and Schaeffer had merely stolen some linen belonging to Rockmann. The brutal murder of Schlosser is a pure invention.

Inexplicable and Unexplained Cases

The examples above have in common that they are representative of the genre at its best and that they have been explained. Not only have skeptics found a plausible scenario to account for the data but the skeptical explanation has for all intents and purposes been shown to have been correct. The specific errors in the claims could be identified. Such debunkings are often the result of sheer luck.

Enthusiastic skeptics are fond of stating *ex cathedra* that there is not a shred of evidence to support the claim that psychics have been successful in assisting the police. In fact, this is an overstatement. Several such shreds exist and are available for critical examination. Contrary to what is often believed by insufficiently informed skeptics, police officers *have* testified that they have been very satisfied with the services of sensitives. Such testimonies cannot in all cases be dismissed as the products of a delusion and may legitimately be cited as evidence supporting the proponent’s position.

One example: In response to an article in *Police Chief* magazine mentioning the Psychic Sleuths Project of the CSAR, a New Jersey chief of police [name here withheld but available to researchers from CSAR – M.T.] wrote to Marcello Truzzi to confirm that Peter Hurkos “had furnished us with a preponderance of leads that we could not have obtained using the standard investigative techniques.” In this homicide case, he further wrote, “Mr. Hurkos, using his unbelievable psychic powers, named for us (by first and last name) the actual perpetrator. It was not until two years later that my officers were able to establish sufficient corroborative evidence to make the arrest of the person named.”

While the police chief’s statement certainly does not constitute conclusive proof, it *does* constitute evidence of the sort that many ultra-skeptical publications have implicitly claimed does not exist.

In contrast to such “inexplicable” cases, I call cases “unexplained” when they continue to resist attempts at specific debunking even where the circumstances (the availability of documents and eyewitnesses, and so on) are such that a critical investigation might normally have been expected to yield information supportive of a specific naturalistic counterhypothesis. It is tempting, especially if one is a psi proponent, to present such cases as prima facie evidence for the reality of the ESP phenomenon. However, in evaluating historical incidents, great caution is called for. Such cases may have a naturalistic explanation. Apparent success of psychic detection may in fact have been due to naturalistic causes whose traces have been irrevocably erased, removed, or can no longer be identified.

Perhaps the most amusing psychic failure on record involves the well-known Dutch paragnost Cor Heilijgers, who phoned me to reveal that he had “seen” where the body of a missing railway employee was buried. The next day, the newspaper reported that the buried man had been found alive and well and living in Antwerp, almost next door to where Mr. Heilijgers had recently lived.

The Value of Police Opinion

Police investigators and psychical researchers are interested in paranormal detective work for different reasons, and this may on occasion cause some confusion. The parapsychologist is primarily interested in deciding whether or not the psychic demonstrated genuine ESP, whereas the police officer wants to know whether or not the psychic actually helped in solving the case. The two groups use different criteria for measuring success. The law officer may have a low opinion of a sensitive who did not succeed in tracing a missing body but who may have nevertheless scored ESP hits that would enrapture the dedicated psi researcher.

It is important, however, to note that in assessing the paranormality of a given psychic’s feat the average police officer is a layman whose judgment has no more intrinsic value than that of the average citizen. Police officers do not need to be familiar with the numerous techniques of simulating telepathy or with the psychological principles underlying “cold reading” and other soothsayer’s tricks. In fact, police officers may be at a disadvantage, as they are trained to quickly discover meaningful patterns in apparently chaotic data. They are conditioned to help the conveyor of information and to encourage the witness. They may be ideal victims for the talented psychic who relies on the cooperation of a sitter. This can be a serious problem as soon as a police officer momentarily steps outside his or her role as an expert in detection and starts playing the amateur parapsy-

chologist. Quite a few police are personally interested in the paranormal and are every bit as desirous of witnessing the “inexplicable” as are other people. This may influence their testimony (and their recollections) if they are requested to comment on the genuineness of psychics – apart from the question of whether these sensitives are of actual practical use to the police.

Affidavits from police departments. Statements by police spokesmen to the effect that a psychic has been successfully consulted should be treated with some caution. Police officers as a rule take a purely pragmatic interest in paranormal detectives and rarely attempt (if they are competent to do so at all) to establish scientifically to what extent the sensitive’s helpful perception has likely been of extrasensory nature. Positive statements usually mean no more than that the police confirm that an individual provided accurate information in a given case. As a rule, they are not capable of deciding whether sensitives are correct in attributing their successes to paranormal gifts.

The trigger hypothesis. Psychic detectives typically confront the police with a great number of ambiguous, contradictory “impressions.” Although perhaps containing actual information, these may on occasion act as a trigger. Police officers are forced to rack their brains to discover some signal in the noise and, presumably, will be in a state of intense concentration. A chance remark by a psychic may produce a chain of associations resulting in a lucky guess. The relevant piece of information is already stored inside the police officer’s mind; the psychic merely helped to retrieve it by presenting the verbal equivalent of a Rorschach tableau. In cases where the investigation has become bogged down in a morass of false trails and vicious circles, such “brain-washing” applied by a loquacious sensitive may actually contribute to the solution of a case.

Psychics are allies. An individual policeman who has a theory of his own concerning an unsolved police case but who fails to convince his superiors of the plausibility of his conjectures may take a psychic into his confidence, on the assumption that a supposedly independent paranormal confirmation of his theory may persuade the authorities to change their minds.

Laundering of information. In certain circumstances, criminals may use psychics for “laundering” information on fellow criminals. Here, the sensitive serves as a screen to protect the informant from retaliation. It is also conceivable that in rare cases the police, in order to protect actual sources, will pretend that the informative help vital to the identification of the criminal was obtained from a psychic.

Disinformation. Since a criminal is likely to be as superstitious as the next person, the police may deliberately leak the (false) information that a highly experienced psychic is

involved in the investigation, in the hope that the unknown perpetrator of a crime may get nervous and make an injudicious move.

Bribes. In (it is hoped) atypical cases, a psychic in need of favorable publicity may obtain confidential information by bribing a member of the police force and relay this information (this time purportedly received by paranormal means) to other police – who will be duly amazed by the accuracy of the psychic’s “vision.”

Nonpsychic Alternatives

Ordinary methods. One of the most obvious (if rarely discussed) naturalistic explanations for apparent psychic successes is that many such psychics employ the same methods as do private detectives, police detectives, and Pinkerton employees: collecting evidence, reconnoitering, questioning witnesses, making informed guesses, and practicing the art of deductive thinking. In his autobiography, *Mijn Dubbele Leven* (My double life), the noted Dutch psychic Cor Heilijgers (1976) claims that he presented the police with highly accurate descriptions of the murderers of “Black John” at a time when the authorities had no clues whatsoever. However, Wim Jongsma, an Eindhoven police officer responsible for public relations, told me that everyone in the village where the crime had occurred had known exactly who had done it. “And they knew it before Heilijgers appeared on the scene. The psychic spent a few days in the village, heard all the gossip, and later claimed a reward to having helped trace the culprits.” Heilijgers’ unquestionable successes in locating the bodies of missing persons (confirmed by the police) do not seem necessarily psychic in nature. Clairvoyants may become experts in a limited area of (conventional) police work, to the extent that they can outperform professional police. For example, the late Gerard Croiset’s enormous experience in searching for missing children presumed drowned had probably never been equaled by any professional police officer. Such experience makes it easier to see similarities between different events and to make an educated guess as to the most probable solution.

Unusual nonparanormal abilities. Recently, there was a lot of publicity surrounding an American physician who claimed to be able to identify a piece of music (and sometimes even the orchestra playing it) by merely looking at the grooves of a gramophone record (the label of which had been covered). Initial suspicions of a hoax were dispelled when James Randi, the magician and debunker of spurious psychic claims, tested the claimant and found his ability to be perfectly genuine. “Record reading” is certainly not an example for extrasensory perception but no less baffling for that. The example is relevant to this discussion as it reminds us that laymen and scientists alike are prone to underes-

timate the range of *sensory* perception. Sensory abilities vary enormously in individuals, and some have developed the ability to perform apparent miracles by using their eyes, ears, or noses. Some successful psychic detectives may actually be able to detect traces too faint to be discerned by the average person and erroneously attribute this ability to ESP.

Stacking effects. If many psychics attempt to solve the same case, the “stacking effect” well known to statisticians may easily produce spurious “success.” As a rule, the media will only report “hits” and keep silent about psychics who failed. In 1972, the Dutch paragnost R.G. was in close touch with the parents of 12-year-old A.P., who had disappeared on her way to school in Rotterdam. According to the report in the daily newspaper *De Telegraaf*, about 100 sensitives tried to be helpful. It seems almost a statistical oddity that none of the 100-plus “visions” (some psychics, such as the famous Gerard Croiset, tried twice) appreciably correspond to what later turned out to be the tragic truth, that A.P. had been murdered and that her body had been left in a pool near the airport. (If the psychics involved had each agreed to select a different 100th part of the town, at least one of them would have been assured a “hit,” with odds against chance of 100 of 1.)

CHAPTER 2-13

Editorial Introduction

As is apparent from several other chapters in this book, Piet Hein Hoebens had repeatedly commented on the particularly acrimonious, often witch-hunt-like¹ style of the German debate on parapsychology that basically prevailed from the 1960s through the 1990s. This awry situation also formed the wider background of the meeting that is described in the following report and of the resulting “consensus statement.” In fact, under the sobriquet “Marburg Manifesto,” this statement became more widely known in the United States than ever it was in the country from which it originated.

The following text was basically written by Hoebens and submitted for Kendrick Frazier’s regular column “News and Comments” in the Skeptical Inquirer, where eventually it was published, with minor modifications and with Frazier’s author’s initials (Skeptical Inquirer, 7, 1983, (4), 4-6]. We are especially grateful to Mr. Frazier for his kind permission to reprint this report in the present book

The document, in its penultimate paragraph, expresses the “hope that such talks and the agreed-upon statements might serve as a model for other dialogues between parapsychologists and their outside critics.” And, at least in one case, it did in fact have the desired effect: In early 1984, the Parapsychological Association established a temporary “Position Papers Committee” that was active for several years. Chaired by Stanley Krippner, its members were Charles Honorton, Gerd H. Hövelmann, Ephraim Schechter and Rhea A. White. Its first position paper, “Terms and Methods in Parapsychological Research” (Parapsychological Association, 1988), that was commissioned by the PA Board of Directors and approved by the PA membership during one of its annual conventions, made use of several source documents, one of them being, pursuing Stanley Krippner’s suggestion, the “consensus statement” or “Marburg Manifesto” that is reprinted here. (Eds.)

1 Articles such as Wolf Wimmer’s influential anti-parapsychological diatribes about what he chose to describe as a “witch-craze at universities” (Wimmer, 1970, 1979, 1980) show that this is quite literally true.

PARAPSYCHOLOGISTS, CRITICS AGREE TO CONSENSUS STATEMENT

The debate over parapsychology in West Germany, even more so than in the United States, has proceeded along lines best described as acrimonious. To try to see if some constructive dialogue might be achieved, an informal meeting to discuss the future relations between parapsychologists and external critics of parapsychology was held last November in Marburg, West Germany.

Three young German parapsychologists – Eberhard Bauer, Gerd H. Hövelmann, and Walter von Lucadou – met with Irmgard Oepen, professor of forensic medicine at Marburg University and one of the best-known skeptics in West Germany. Acting as “amicus curiae” at the meeting was Piet Hein Hoebens, the Amsterdam journalist who frequently reports on parapsychology controversies. (He wrote the investigative articles on the Dutch “clairvoyant detective” Gerard Croiset in our Fall 1981 and Winter 1981-82 issues and the report on the Cambridge psychical research centenary meetings in our Winter 1982-83 issue.)

There was one specific dispute at immediate issue. It concerned a recently published doctoral thesis that warned against medical pseudoscience.² Hövelmann had taken issue with the thesis’ discussion of parapsychological topics;³ Oepen had taken issue with Hövelmann’s criticisms. At the Marburg meeting the participants expressed appreciation for the thesis’ attempts to warn the medical world and the public against certain pseudo-scientific movements that try to take on scientific respectability by mimicking scientific terminology.

The larger purpose of the meeting, however, was to see if some of the apparent hostility between parapsychologists and their critics in Germany might be a result of misunderstandings that could constructively be dispelled. The participants worked to recognize common interests despite their differences in perspective.

“We reached agreement on a surprising number of issues,” says Hoebens. In a report of the meeting to the German *Zeitschrift für Parapsychologie*,⁴ Hoebens listed a number of statements all the participants found they could agree to.

2 See Gzara (1980). (Eds.)

3 See Hövelmann (1981). (Eds.)

4 That German report remained unpublished for reasons that today can no longer be reconstructed. However, it was mentioned in some detail elsewhere in the German literature (Oepen, 1986, p. 79). (Eds.)

They were in agreement on nine basic points:

1. A commitment to the study of parapsychology does not necessarily entail a commitment to the belief in the reality of “paranormal” factors.
2. “Psi” is a *hypothesis*. Given the present state of the parapsychological evidence it cannot be rationally maintained that it is a fact established beyond scientific doubt.
3. Even if “psi” exists – which can be doubted on rational grounds – very little if anything is known about its operations and its limitations. For this reason, claims of practical applications of “psi” should be treated with extreme caution.
4. Such considerations apply a fortiori when claims are made for medical applications of “psi.” As here the health of human beings is at stake it seems better to err on the side of skepticism than to err on the side of credulity. In general, people should be discouraged from believing in miracle cures, especially of serious afflictions for which competent medical treatment is urgent.
5. While, on a scientific level, parapsychologists are only responsible for their own work and their own statements, they have a special social responsibility in that it is incumbent on them, unambiguously and openly, to disassociate themselves from pseudoscientists, occultists, crackpots, and charlatans who claim scientific parapsychological support for their questionable claims.
6. In the interest of mental hygiene and with specific reference to the social and medical risks of uncritical acceptance of “paranormal” claims responsible parapsychologists should stress the *speculative* nature of many of their concepts, be candid about the controversial status of paranormal claims, and, in making factual assertions, not go beyond what is warranted by the evidence.
7. Responsible parapsychologists and responsible critics should seek cooperation in exposing fraudulent or otherwise irresponsible claims.
8. The debate over parapsychology should be conducted in a spirit of fairness and truthfulness. Each side should attempt to defend its point of view on the basis of accurate information. Misrepresentation of the other side’s position should be avoided. Polemical exchanges are to be welcomed, but they should be devoid of demagoguery, dogmatism, and cheap insults.
9. Participants in the debate should try to keep their own house in order. This implies a moral and intellectual obligation to criticize not only the “opponents,” but also the “allies,” whenever they depart from the rules of rational discourse.

The agreement over these statements may well be an important achievement. Almost all are points critics and observers of parapsychology have in one way or another pressed for in the past.

Hoebens says the discussions were amiable. He and the others hope that such talks and the agreed-upon statements might serve as a model for other dialogues between parapsychologists and their outside critics. He expressed hope that the outcome of the meeting will be favorably received by “all those who are sincerely interested in the search for the truth – whatever the truth in the matter of parapsychology may turn out to be.”

Finally, Hoebens told the *Skeptical Inquirer*: “I think that it is of considerable interest that three respected parapsychologists – all members of the Parapsychological Association – have now formally committed themselves to the position that psi is basically no more than an unproven hypothesis.”

- K[endrck] F[razier] -

CHAPTER 2-14

Editorial Introduction

The 1983 Marburg “consensus statement” by German parapsychologists Bauer, Hövelmann and von Lucadou and German skeptic Irmgard Oepen, reprinted as the previous chapter (2-13), also had served to intensify Piet Hein Hoebens’ contacts with Frau Oepen – the Hoebens Files contain several dozen letters exchanged between them over a period of little more than a year. Frau Prof. Oepen was (and still is) one of the most influential German critics of all conceivable varieties of alternative or complimentary medicine, with a huge number of publications to her credit. When, in the fall of 1983, Frau Oepen and her famous East-Berlin colleague and mentor Prof. Otto Prokop (1921-2009) threw up plans to edit an anthology on the dangers, supposed or real, of alternative medicine and “other parascientific currents,” it seemed a natural choice for her to invite Hoebens to contribute a chapter on “fringe medicine” and on the question what parapsychologists might conceivably have to do with it.

Hoebens wrote and submitted his chapter in January of 1984 and he insisted that Gerd H. Hövelmann be asked to translate it into German.¹ The editors agreed as did the Wissenschaftliche Buchgesellschaft, one of Germany’s most prestigious publishers of science books. The book was virtually completed by the late summer of 1984. However, it only appeared in print almost two years later, a year and a half after Hoebens’ death.

The main reason for this delay deserves mention. The editors, Oepen and Prokop, both were widely-known professors of forensic medicine as were most of the other 13 contributors to the book. The only non-physicians among the book’s authors were Hoebens and notorious skeptic Wolf Wimmer (1935-2004; see footnote 1 in the previous chapter), the Director and Presiding Judge of the District Court of Mannheim. Wimmer, having read the page proofs of Hoebens’ chapter, complained that Hoebens was much too friendly towards parapsychology and that a nominal parapsychologist had even been allowed to take responsibility for the German translation. He therefore took legal measures to prevent the publication of Hoebens’ chapter in the context of this anthology. He even threatened to sue the main editor (Frau Oepen) and the publishers in case they would not oblige. These moves, predictably, were not crowned with success, but they delayed the book’s publication for many months.

*Hoebens’ paper eventually appeared, posthumously, in Oepen, I., & Prokop, O. (eds.), *Außenseitermethoden in der Medizin. Ursprünge, Gefahren, Konsequenzen* (pp. 83-95). Darmstadt: Wissenschaftliche Buchgesellschaft, 1986. The following chapter presents the*

1 Letter Piet Hein Hoebens to Irmgard Oepen, 31 January 1984 (Hoebens Files).

final version of Hoebens' original English manuscript on which the German translation (which Hoebens approved) was based. (Eds.)

FRINGE MEDICINE AND THE PARAPSYCHOLOGISTS' RESPONSIBILITY

Introduction

Of all the “alternative sciences” which tradition has nonchalantly lumped together under the chapter title “occultism,” parapsychology is without doubt the one most deserving the careful attention of the skeptic. If it is a pseudo-science, then a pseudo-science *sui generis*, very different from horoscology, numerology, the cosmic billiards of Dr. Velikovskiy or the “astro-archaeology” popularized by Herr Von Däniken.

No sane man will seriously expect that the universities of the future will offer courses in the Science of Unlucky Number Thirteen or introduce their students to the art of Palmistry. With parapsychology, even diehard unbelievers are not always so sure.

The parapsychological propaganda has always laid great stress on the fact that the field has enjoyed the enthusiastic support of many Nobel Laureates and other persons whose important contributions to orthodox science are undisputed. As (1) eminent scientists can behave as utter fools when they venture outside their own domain of expertise and (2) an absurdity does not cease to be absurd when believed by a Nobel Laureate I consider this propagandistic argument to be of marginal relevance. What I *do* find significant is the fact that numerous highly qualified critics who have investigated the claims of the parapsychologists with relentless skepticism, who are exceedingly well aware of the many traps and pitfalls of the occult and who after years of careful study remain entirely unconvinced by all those marvelous tales of clairvoyance and psychokinesis yet insist that parapsychology should be granted the benefit of the doubt.

This was the feeling of some of the great pre-war German skeptics such as Max Dessoir and Carl Graf von Klinckowstroem, and has recently been restated in a most eloquent fashion by the British philosopher Anthony Flew, one of the original fellows of the skeptical Committee for the Scientific Investigation of Claims of the Paranormal: “Parapsychology ... is a horse of quite another color ... I concluded there that, although there was no repeatable experiment to demonstrate the reality of any of the putative psi phenomena,

and although the entire field was buried under ever-mounting piles of rubbish produced by charlatans and suckers; nevertheless one could not with a good academic conscience dismiss the case as closed. Too much seemingly sound work pointing to the genuineness of at least some of these phenomena had been done. Too many honest, tough-minded, methodologically sophisticated and often formidably distinguished persons had been involved in this work." (Flew, 1980, p. 100)

Now it will be obvious that the "parapsychology" professor Flew is referring to is very different from the "parapsychology" we encounter in newspapers such as *Bild* or *The National Enquirer*; in specialized magazines about "Die Wunderwelt an den Grenzen unseres Wissens" [The miraculous world at the frontiers of knowledge] such as *Esotera* and in those books bearing exciting titles such as "How to Get Rich by E.S.P." or "Teach Yourself Astral Projection."

For this type of parapsychological literature Martin Johnson, professor at Utrecht State University and author of a no-nonsense book on parapsychology (Johnson, 1982), has coined the word "parapornography." The pervading presence of parapornography has considerably complicated the scientific debate over "the Paranormal" – to the point where proponents and opponents frequently exhibit nothing but utter confusion when asked to explain precisely what they disagree about.

Strictly speaking, "parapsychology" (the word was coined by the skeptic Dessoir!) means little more than the systematic study of a special class of anomalous *claims* with the purpose of determining whether or not these claims justify the introduction of entirely new scientific concepts. More in concreto: parapsychology wants to find out whether the *apparent* cases of "paranormal" interaction between living organisms and their environment (telepathy, clairvoyance, psychokinesis, precognition) are due to an as yet unknown factor ("psi") or to a misidentification of non-paranormal factors.

Taken in this sense, a commitment to parapsychology does *not* entail a commitment to the belief that there is such a thing as "psi." Indeed, by this original definition well-known skeptics such as C.E.M. Hansel, James Randi, Martin Gardner and Otto Prokop would qualify as "parapsychologists."

However, "parapsychology" thus defined and the intellectual movement *historically* associated with the term overlap only to a limited extent. In fact, parapsychology has soon after its inception acquired the secondary meaning of "the science that studies paranormal phenomena" – which presupposes that such phenomena are part of reality. For this reason, skeptical investigators of occult claims as a rule dislike being referred to as "parapsychologists;" they prefer the term to be exclusively used to refer to persons who

believe, or are strongly inclined to believe, that the reality of psi phenomena has been demonstrated beyond reasonable doubt.

There are, however, important exceptions to this rule. The British psychologist Susan Blackmore still considered herself a “parapsychologist,” even after she had completely lost faith in “psi” and turned into a confirmed skeptic (Blackmore, 1985). On the other hand, some people considered to be “skeptics” have accepted some “paranormal” phenomena as real or very likely real. Examples are Carl Graf von Klinckowstroem in Germany and Eric John Dingwall in Great Britain. To complicate matters even further, the term is also historically (though not logically) associated with a particular semi-religious philosophy according to which the purported phenomena are signals of a radically different reality, impervious to all attempts to explain them in terms of natural science.

“Psi” and (the Philosophy of) Science

Most philosophers of science would agree that science refers to a method of investigation rather than to a body of established knowledge – which implies that it is, in principle, possible to hunt for chimaeras in an impeccably scientific way. (It is tacitly assumed that, if chimaeras are non-existent, a rigorous application of the scientific method will guarantee their non-discovery.)

However, it would be a terrible waste of time to apply scientific methods to an area of investigation without taking into consideration earlier discoveries arrived at by the application of these self-same methods. In other words: the battle-tested results of the scientific enterprise of the past centuries provide us with tentative a priori rules as to what to expect from excursions into new areas of research. (The rules will necessarily be tentative, as “laws of nature” discovered *empirically* can never be said to be, on *logical* grounds, resistant to all attempts to have them repealed.) An expedition to the Moon to test the claim that the lunar surface is made of Emmenthaler cheese would be deemed a folly as we practically know a priori that the hypothesis has no chance at all of being confirmed.

Is the “psi” hypothesis comparable to the Emmenthaler Moon hypothesis? May we say that the laws of nature tell us beforehand that parapsychology will never be able to make its case and will only lead to negative or spurious discoveries?

My own intuitive opinion is of considerably less relevance here than the fact that the scientific community itself is sharply divided over this question. Whereas both skeptics and “believers” have pronounced “psi” to be incompatible with natural science (the “believers” obvious motive being the wish to replace “materialist” science with something

more to their mystical liking) others – both critics and proponents of parapsychology – disagree. In recent years there has been a lively discussion among qualified physicists about the potential relevance of quantum physics for psychical research. Some highly speculative, bizarre, but intellectually respectable interpretations of the quantum paradoxes would seem to allow the reality of curious phenomena akin to some of the “psi” phenomena claimed to be discovered by the parapsychologists (Collins & Pinch, 1982).

However that may be: it would be foolhardy to claim at this moment, as some of the less sensible proponents have done, that “quantum physics confirms parapsychology.” The skeptical parapsychologist Prof. Martin Johnson of Utrecht University has pointed out that all attempts to “explain” the paranormal in this way precariously depend on “more or less fantastic auxiliary hypotheses,” and it would be prudent to remind ourselves of the utter failure of earlier attempts to “explain” psi in the then fashionable terms of “magnetism,” “electricity” and “electro-magnetism.” Each time physical science advances, parapsychologists look if they can use the new discoveries to their own advantage. Whether they will be successful this time remains to be seen.

At this moment, skeptics and proponents alike are well-advised to stick to the rule that “psi” phenomena are extremely difficult to reconcile with several fundamental assumptions underlying modern science, and that they therefore must be seen as extraordinary claims which require extraordinarily strong evidence.

The Quality of the Evidence

For reasons given in the preceding paragraph it would be an irresponsible overstatement to claim that “science,” by means of a priori arguments, can *guarantee* us that “psi” is nothing but nonsense. This, I would contend, legitimizes attempts, scientifically to test the hypothesis that “psi” exists. Such attempts have been made now for over a century. While every unprejudiced connoisseur of the relevant literature will agree with the skeptics Prof. Ray Hyman and Prof. Antony Flew that too much impressive work has been done for us to be allowed to dismiss the case as closed, even cautious and knowledgeable “believers” (such as Dr. John Beloff) admit that the case is far from being proven either.

To put it in a nutshell: one hundred years of psychical research has resulted in an enormous pile of more or less spectacular anecdotes which, in principle, could all be “explained away” by skeptical counter-theories without providing us with a single “repeatable experiment,” i. e. a set of rules telling the skeptic what conditions must be satisfied in order to guarantee the manifestation of *any* “paranormal” phenomenon.

This state of affairs is absolutely fatal to the often heard claim that “psi” has been “proven beyond reasonable doubt.” The “inexplicable” events and the “successful” experiments reported in the psi literature cannot be said to be “inexplicable” or “successful” as long as parapsychologists remain unable to transform their collection of anecdotes into a set of “if – then” statements that can be independently checked by other researchers. Claims that Mr. Daniel Home effected an auto-levitation in London on 16 December 1868, that Mr. Hubert Pearce correctly guessed so many Zener cards between August 1933 and March 1934 that the odds against a similar success arising from chance is 10^{22} to one, that Mr. Uri Geller paranormally bent cutlery on 24 April 1974 in a New York City apartment or that Mr. Gerard Croiset clairvoyantly solved a crime on 15 November 1979 are basically meaningless unless we know the exact conditions under which these alleged miracles occurred, were observed and reported.

Many persons who style themselves parapsychologists stubbornly refuse to understand why this is so. They do not tire of telling us that the discovery of a single black swan disproves the theory that all swans are white and that it would be irrational to insist on replication as the phenomena are most sensitive to as yet not entirely understood psychological conditions. Once again, I will make an attempt to enlighten these believers.

Until the parapsychologists can tell us what “psi” is (which implies that they must be able to tell us about the necessary and sufficient conditions for its manifestation) “psi” remains defined negatively. To say that an event is caused by “psi” only means that it is not caused by any other *known* factor or factors. Ergo: “psi” can only be “proven” in a given instance if all conceivable other causative factors are disproven. From this, it logically follows that “psi” cannot be demonstrated in an individual case unless all factors that could conceivably have contributed to the claimed result are known – and fully known. In practice, this will be impossible unless we dispose of a Time Machine that can take us back to the time the alleged event occurred, enabling us to check our skeptical suspicions repeatably and on the spot.

This being a practical impossibility, it will remain a valid option to assume, when confronted with a psi-anecdote, that the alleged miracle was in fact the result of some error in the observation and reporting of a non-paranormal event.

The history of psychical research teaches us that such alternative, naturalistic explanations are not only rational, but in many cases also demonstrably *true*. The scientific literature provides us with many striking examples of seemingly fool-proof psi events which later were conclusively exposed as the result of malobservation, error of recall, mis-identification, hallucination, trickery on the part of the tested “psychic” or dishonesty on the

part of the investigator. And in many more cases where a conclusive exposure was not possible, there are compelling reasons to believe that the claims have been spurious.

A Few Examples:

- Most of the classical “mediums” – the study of whom was the main occupation of parapsychology in its pioneer years – have at one time or another been caught in fraud. Some have confessed and – which is even more important – explained by what methods they had been successful in fooling otherwise sober observers (Gulat-Wellenburg, Klinckowstroem & Rosenbuch, 1925; Brandon, 1983; Podmore, 1902).
- The successful work of Dr. Samuel G. Soal, until recently hailed by most parapsychologists as the most convincing single series of psi experiments in the entire history of the field, has now been shown to have been almost certainly the result of dishonesty on the part of Dr. Soal (Markwick, 1978).
- Expert magicians have discovered many ways to simulate by trickery the allegedly psychic feats performed by Uri Geller and Ted Serios and have presented strong and convincing arguments for their suspicion that these miracle men are indeed nothing but cheats (Müller, 1980; Randi, 1982a, 1982b).
- The evidence purporting to prove the authenticity of the mediumship of the famous Dutch “clairvoyant” and “psychic detective” Gerard Croiset has been shown to be totally unreliable.²
- A critical examination of the published experiments of prominent researchers such as Targ, Puthoff and Sargent has revealed numerous defects in their experimental designs, potentially fatal errors in their evaluation of the data and in some cases even signs of irregularities in their reporting (Akers, 1984; Marks & Kammann, 1980; Hyman, 1985).
- Real-life demonstrations have shown that many professional parapsychologists are unable to distinguish between a paranormal event and a magic trick – and even resent the skeptical explanation when the “psychic” explains to them that the “miracle” has been faked (Hövelmann, 1984b; Johnson, 1982; Randi, 1983a, 1983b).

Indeed, the often hilarious history of parapsychology teaches us, if anything, that there are hardly limits to human fallibility. When under the spell of the occult, people will talk nonsense, lie, cheat, make fools of themselves and exhibit a credulity which they themselves would find reprehensible in any other context. Given the human pro-

² See “In Praise of Meticulousness” (chapter 2-04 of this volume) and “Gerard Croiset: Investigation of the Mozart of ‘Psychic Sleuths’” (chapter 3-04.). (Eds.)

clivity to err, it would seem reckless to put much trust in anecdotes of “the Paranormal” – and parapsychology alas remains unable to present evidence of a fundamentally non-anecdotal quality.

The Responsibility of Parapsychologists

In spite of my skepticism, I cannot consider the case against parapsychology as closed. I think it is a matter of fairness, even of intellectual integrity, to admit that some non-skeptics have presented interesting, intelligent, rational and sophisticated arguments to support their view that there is sufficient *suggestive* evidence to justify a further scientific exploration of the psi paradigm (Beloff, Bauer/von Lucadou, Johnson, West). However, the skeptics may make some demands of the proponents in return for skeptical tolerance. And this is where my general discussion of parapsychology touches the specific theme of this volume: heterodox healing methods and the health – both physical and mental – of human beings.

Whether they like it or not, parapsychologists bear a special social and moral responsibility in this area as parapsychology – seen as a historical phenomenon – has traditionally provided almost every form of quackery with a semblance of intellectual respectability.

Some seemingly absurd diagnosing or healing techniques could – theoretically – be legitimized if some of the phenomena which parapsychologists are interested in were to be proven genuine. To all intents and purposes, medical science has exposed “iriscopy” as a nonsensical fad. But what if “telepathy” and “clairvoyance” were to exist? In that case, the iriscopist would have a point if he explained to a skeptical customer that the iris basically served as an “inductor” to focus his extrasensory abilities. And this is precisely what advocates of such practices claim when unable to meet rational objections otherwise.

Whatever medical experts may say about the absurdity of quack claims; the quack can always take advantage of the loophole provided by “the science of parapsychology”: it could be psi! Parapsychology furnishes the terminology with which the practitioners of pseudo-medicine can cover-up the flaws in their doctrines. Terms such as “psychokinesis,” “materialization” and “decline effect” create an illusion of scientific precision, likely to overawe the naive patient. In fact, these words are nothing but provisional labels used to describe what are at best hypothetical constructs. When used as if they described scientifically demonstrated and sufficiently understood *phenomena* they become dangerous weapons in the hands of irresponsible witch-doctors.

The most dreadful example I could cite here concerns the notorious Philippino “psychic surgeons,” local spiritualists who claim to be able to “operate” patients with their bare hands and extricate tumors. The evidence that this “paranormal surgery” is nothing but fraud is so compelling as to be practically conclusive (Nolen, 1976; Oepen & Scheidt, 1984). The surgeons have been caught in trickery many times; on several occasions examination of the tissue or the blood said to come from inside the patient’s abdomen has revealed it to be of animal origin; the “psychic surgeons” have consistently refused to allow testing under skeptical conditions and the best known of them all, the late Tony Agpoo, went to a London hospital, not to a fellow “psychic,” when he himself became seriously ill.

Each year, thousands of citizens of western countries – notably the German Federal Republic – travel to the faraway Philippines in order to be cured from their diseases. The cost in money, but even more in human suffering, is enormous.

Now what is particularly relevant to the theme of this essay is that many individuals in the West have taken it upon them to defend this swindle as a legitimate form of alternative medicine, and have based this defense on arguments directly borrowed from parapsychology (Stelter, 1973).

The opening and closing of the abdominal cavity without the use of surgical utensils is “explained” in terms of “psychokinesis.” The suspicious discovery of animal tissue in the “tumors” supposedly extricated from the patient’s body is not taken as a clear indication of fraud, but as evidence for a secondary miracle: “psi” has not only “materialized” the tissue, but also “transformed” its nature. The inability of the “surgeons” to be successful when scrutinized by skeptical experts is explained away as being due to yet another “well known phenomenon”: the sensitivity of “psi” to “psychological atmosphere” and its reluctance to exhibit itself to the unbelievers.

In this way, “parapsychological” arguments are used to systematically discredit all attempts on the part of responsible persons to warn the public against this dangerous folly. What are, in the most optimistic case, interesting speculations are presented as scientific findings to silence all rational opposition to this swindle.

It is, I contend, the special responsibility of the parapsychologists to protest, openly and vehemently, against this scandalous misuse of their terminology. Their protest, moreover, will only have impact if they also refrain carefully from overstating the case for “psi.” It is not only a demand of intellectual honesty, but also of social responsibility, that they stress the speculative nature of their theories, admit that “psi” remains an unproven concept and emphasize that, even if “psi” exists, nobody at this moment knows enough

of its properties and limitations to be able to say anything definite about its potential practical applications. They should by all means discourage the public from believing that parapsychology has proven the reality of miracles.

Parapsychologists as Allies?

In the preceding section, I have implied that some parapsychologists could be allies of the skeptics in their campaign against quackery. Are there parapsychologists who would qualify for that part? The suggestion will be met with some skepticism, especially in Germany, where the most visible representative of parapsychology is hardly noted for his scientific caution. At this point I have to warn the reader against some skeptical oversimplifications which have spontaneously arisen from the heat of the psi controversy.

It is true that the sort of “parapsychology” with which the general public is most likely to be familiar is best described as a sensationalist pseudo-science. It is also true that very well known parapsychologists, associated with prestigious academic institutions, have made irresponsible claims for their field, and have thereby aided and abetted the popularity of “parapornography” with all its noxious implications for society. I am not making excuses for the writings of Prof. Dr. med. Dr. phil. Hans Bender who has pronounced his belief in “psi” – largely based on his determined credulity in the face of transparently phony miracles – to be “unerschütterlich” (“unshakeable”). Nor will I make any attempt to defend Bender’s colleague and admirer Professor Henri van Praag of Utrecht who, as late as September 1983, dared to publish an article confirming the authenticity of “paranormal surgery” while completely and deliberately ignoring the utterly damaging evidence contra.

However, I must insist that these two gentlemen cannot in fairness be presented as being representative of *all* parapsychology. Critics of parapsychology too often forget that some of the very best debunking work in the history of occultism has been done by persons who belong to the group of psychical researchers rather than to the group of outside critics. Frank Podmore, author of some of the most brilliant books exposing the delusions of the Paranormal, was a research officer of the earliest parapsychological association, the Society for Psychical Research. The *Journal* and the *Proceedings* of this Society have a long tradition of publishing criticisms. The notorious Madame Blavatsky met her Waterloo in the *Proceedings* (Hodgson, 1885). The first scientific journal to publish a detailed expose of the manipulations of Professor Tenhaeff was the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*. Carl Graf von Klinckowstroem, whose name is written in golden letters in the annals of German skepticism, was a parapsychologist in the sense

that he systematically investigated the allegedly paranormal phenomena and at one point became convinced that at least some of them were genuine. Dr. E. J. Dingwall, a sharp and perspicacious critic if there ever was one, also takes "the paranormal" more seriously than his early membership of the skeptical "Committee for the Scientific Investigation of Claims of the Paranormal" would lead us to expect. Excellent books debunking "Raudive voices" and "astral journeys" have been published by the SPR-sponsored young parapsychologists David Ellis (1978) and Dr. Susan Blackmore (1982). Leading parapsychologists such as Dr. John Beloff and Professor Martin Johnson have repeatedly and publicly warned against uncritical belief in "psi" and have urged their colleagues to seek more cooperation with skeptical scientists and skeptical experts on trick magic (Johnson, 1982).

Also – a most encouraging development – in recent years a group of young German parapsychologists, notably Dipl.-Psych. Eberhard Bauer, Gerd Hövelmann, Dr. Klaus Kornwachs and Dr. Walter von Lucadou, have opted for a "new conservatism" in parapsychology, dissociating themselves unambiguously from the disastrous heritage of the Bender era (Hövelmann, 1983).

Thus, there exists a critical (minority) tradition within parapsychology, less visible to the readers of *Bild* or *Esotera*, but all the more respectable in the eyes of the unprejudiced rationalist. Given the expertise, the detailed knowledge of the parapsychological literature and the specialized experience to be found in this group it would be a pity if the skeptics were to decide they could well dispense with the advice of the researchers.

Personally, I am willing to predict that the critical parapsychologists – precisely because they are critical – will fail in their search for solid evidence in favor of "psi." My intuition tells me that "psi" does not exist and cannot therefore be found. However, I do not wish to be dogmatic on this issue and am willing to contemplate the possibility that totally unexpected developments in science will eventually prove me wrong. In the meantime, let us not forget that, in matters of such great potential importance, it is better to err on the side of skepticism than to err on the side of credulity. And here, I am merely paraphrasing the words of a prominent parapsychologist.

CHAPTER 2-15

Editorial Introduction

Early in 1984, German philosopher-anthropologist Hans Peter Duerr, a close and long-time friend of Paul Feyerabend (see Duerr, 1980-1981; Feyerabend, 1995), came up with the idea of editing an anthology on "Authenticity and Fraud in Anthropology." In addition to questions of authenticity, fraud and (self-)deception in anthropology proper, the book also was to cover similar problems and phenomena in a number of related fields such as literary frauds (Macpherson's Ossian [Gauger, 1987] and the Finnish Kalevala epos [Honko, 1987]), the infamous Piltdown Forgery [Poirier & Gorzitze, 1987] and the possible role of deception and self-deception in a marginal scientific field such as parapsychology. While Piet Hein Hoebens, who was invited to contribute his reflections on the latter question, had delivered his chapter within a few weeks time, most of the other contributors took quite a bit longer. This is the reason that the book eventually appeared more than two years after Hoebens' death in Duerr, H.P. (ed.), *Authentizität und Betrug in der Ethnologie*. Frankfurt/M.: Suhrkamp, 1987, with Hoebens' chapter on pp. 308-330.

Again (see chapter 2-14), Hoebens had suggested to the editor that Gerd H. Hövelmann be asked to translate his chapter. As it happened, Duerr already had independently commissioned G.H.H. to translate a couple of other chapters for this very book, so he immediately agreed. Since there was a delay of more than two years before the book was published, G.H.H. suggested to Duerr that he write a translator's postscript to Hoebens' chapter to update some of the information on the cases of Soal, Levy and Sargent contained in the chapter – information that had become available only after Hoebens' death and that he, conceivably, also would have wished to incorporate. That postscript, which was appended to the Hoebens chapter (Hövelmann, 1987), is not reproduced here. The following chapter is based on Hoebens' original English manuscript. We are grateful to Prof. Duerr for his kind permission to use Hoebens' chapter for this book.

Readers may be interested to know that, at the very time we are editing this chapter (September 2009), an Australian psychologist has submitted a re-examination of the Soal case that was to retrieve the allegedly fraudulent Soal-Goldney target series from their reported source. It presents results that seem to confirm S.G. Soal's claims about the sources of his target series (in the meantime, that paper was published as Garton, 2010). This may, in effect, reopen the Soal case and put it back, after more than three decades, on the parapsychological and skeptical agendas. (Eds.)

FRAUD AND SELF-DECEPTION IN PARAPSYCHOLOGY

It has become something of an “opinion chic” that fraud is rampant in the Temple of Science, previously believed to be a sanctuary of intellectual integrity in a corrupt world. Philosophers such as Thomas Kuhn and Paul Feyerabend have divested the “scientific method” of its superhuman pretensions. Historians and sociologists have completed the demolition job by revealing the often shoddy reality behind the smooth marble façade erected by the propagandists of the scientific ideology.

The growing public awareness of “orthodox” misbehavior has provided consolation to the parapsychologists, who for decades have had to suffer the indignity of being widely suspected of having obtained their incredible findings by illegitimate or even outright fraudulent means. Many (though not all) proponents of parapsychology now like to argue that skeptical mainline science has forfeited the right to impugn the integrity of parascience by suggesting “fraud” as a naturalistic explanation of parapsychological data. The idea is that, after the recent revelations of the ubiquity of deception and self-deception in science, it would be an act of gross discrimination to put a special emphasis on the fraud theme in discussing the claims of parapsychology.

In this chapter I will argue that parapsychology is a science *sui generis*, exhibiting some unique features that make fraud and self-deception a *potentially* far more serious problem here than is the case in any of the established sciences.

As the claims of parapsychology are often thought to have revolutionary implications for our views on God, Man and Universe, the scientific debate over the legitimacy of these claims has been unusually acrimonious. Parapsychologists have bitterly complained about what they perceive as an inexcusable lack of open-mindedness on the part of the defendants of orthodoxy.

It seems only fair that I declare my own bias at this point, and soothe parapsychological feelings by inserting a few qualifying remarks. As a habitual skeptic, I suspect “paranormal phenomena,” as conventionally understood, of non-existence. However, I am clearly aware of the dangers of skeptical dogmatism and prefer to see the debate over “psi” as an intellectual game the outcome of which cannot yet be predicted with any degree of confidence. The skeptical position is based on certain assumptions that may eventually become obsolete.

I further concede the point that several well-known critics of parapsychology are insufficiently familiar with the subject and seriously underestimate the case *for* parapsychology. For the purpose of the present essay I will resign myself to the traditional, stereo-

typical proponent/skeptic dichotomy which is in fact a gross simplification of the actual state of affairs. The critique offered here should not be construed as implying that I subscribe to the belief that parapsychology is a fraudulent science. I only wish to argue that the “worst case scenario” has a logic of its own, and should not be summarily dismissed.

Lest I become too repetitious, I will not always make a clear distinction between deliberate fraud and unintentional (self-)deception. In terms of ethics, the difference of course is all-important. In practice, the effects are often the same. Both with deliberate fraud and non-reprehensible self-deception some psychological mechanism lures the scientist into making claims that, by the standards he is expected to adhere to, are not just erroneous but misleading.

Do Psi Phenomena Exist?

Or don't they? It is very difficult to tell. For over a century, parapsychologists have accumulated an impressive mass of evidence which *prima facie* seems to support the belief that phenomena such as telepathy, clairvoyance, precognition, psychokinesis and even poltergeists or communications with the disembodied denizens of the Beyond do indeed occur. Even persons intuitively averse to “occult” notions have commented favorably on the quality of some parapsychological work.¹ However, the scientific community and rationalists in general have been remarkably reluctant to accept the claims of parapsychology. The field is often unceremoniously classified as a pseudoscience, on a par with traditional astrology and iriscopy.²

The arguments the skeptics have advanced for disputing the reality of “psi” can be summarized as follows:

1. The entire realm of “paranormal phenomena” clashes with the “basic limiting principles” of science.³ Accepting “psi” would imply a drastic revision of some assumptions considered more fundamental than even the specific “Laws of Nature.” Thus the reality of the phenomena cannot be seriously considered unless exceptionally strong positive evidence is presented.
2. The actual parapsychological evidence is hopelessly inadequate for supporting a scientific revolution of the required magnitude. The data are basically little more

1 Flew, A. (1980). Parapsychology: Science or pseudoscience. *Pacific Philosophical Quarterly*, 61, 100-114.

2 Prokop, O., & Wimmer, W. (1976). *Der moderne Okkultismus*. Stuttgart: Gustav Fischer Verlag.

3 Broad, C.D. (1949). The relevance of psychical research to philosophy. *Philosophy*, 24, 291-309.

than anecdotal reports relating to singular past events. After more than one hundred years, parapsychology cannot yet point to even a single hypothesis that can in principle be tested and re-tested by anyone at any time with an outcome guaranteed to be supportive of the hypothesis. In the absence of a repeatable experiment a strong case can be made for rejecting the evidence on the ground that the reported data may prove no more than that something has gone wrong in the process of generating, observing, describing and evaluating non-paranormal events.

Apologists for parapsychology have complained that the second argument is “unfalsifiable” and therefore a breach of Popperian etiquette. They point out that it could in principle be employed to question *any* scientific claim, however well established.

This objection ignores the “falsifier” implicit in the skeptical argument: The skeptic acknowledges that the matter can be settled in the parapsychologists’ favor if the demand for a replicable experiment is fulfilled. It should be noted, moreover, that “psi” is defined *negatively*: in terms of mainline science’s supposed inability to account for certain puzzling data. “Psi” has no characteristics that would enable us to identify it positively whenever it chooses to manifest itself. It is not an explanation, but rather the exclusion of any other explanation. This state of affairs has certain implications of a logical rather than a methodological nature. “Psi” can only be demonstrated in a given instance if it is positively demonstrated that no conceivable conventional explanation applies. This will remain a virtual impossibility as long as parapsychology can only point to essentially anecdotal reports of singular historical events (whether or not supposed to have taken place in a scientific laboratory). Thus, the validity of the skeptical option is tacitly conceded in the parapsychologists’ own definition of “psi.”

Fraud and Other Sources of Error

The skeptical scenario does not require us to assume that all or most parapsychological claims are fraudulent. There are numerous innocent (even highly respectable) sources of error that might mislead a researcher into falsely concluding that his observations can only be accounted for if the reality of a “paranormal” factor is accepted. There is a distinct possibility that some parapsychologists have unwittingly discovered shortcomings in the standard methods they have faithfully applied in their experiments. The identification of such previously unsuspected artifacts might be of considerable importance for mainline science. However, the skeptic should not dissimulate that, in several cases, fraud and (self-)deception are the most parsimonious counter-explanations for the claimed observations. What in my view makes the case of parapsychology unique is the *potential* relevance of these counter-explanations.

The fashionable distrust of the propaganda of establishment science does not go so far as to justify rejection of “science” as a supremely useful instrument for explaining and understanding the universe, or rejection of all or most scientific discoveries. Especially in the “hard” sciences there are numerous “facts” of sufficient robustness that our acceptance of them is not affected by any suspicions we may harbor concerning the fact-finder’s integrity. To give a childishly elementary example: Revelations of large-scale fraud in marine biology would not justify our giving up our belief in the existence of fish. In parapsychology the situation is dramatically different. The field cannot pride itself on a single “fact” that, by the proponents own criteria, is above suspicion. Several leading parapsychologists admit the theoretical possibility that the evidential base for the belief in “psi” may entirely collapse if the fraudulent data were exposed and removed.

The following considerations may assist the reader in forming an opinion as to whether it is even remotely plausible that an entire science might ultimately rest on a foundation of (self-)deception.

- a. Parapsychologists admit that they know next to nothing of the nature of “psi,” of its properties and of its range and limitations. This means that it is virtually impossible to validate – even provisorily – a claim by pointing to the tell-tale marks of genuineness. In the mainline sciences, certain forms of deception are for all intents and purposes ruled out as they would result in “findings” that clash with well-established assumptions and would therefore immediately invite suspicions. In the absence of any known “positive” characteristic of “psi,” no observation or experimental result in parapsychology can be said to be intrinsically unlikely. The parapsychologist finds himself in the unenviable position of a biologist who cannot, by mere observation, tell a man in a Mickey Mouse suit from a legitimate rodent. (Ironically, the major exception to this rule is that parapsychologists have learned to distrust claims that are “too good.”)
- b. “Psi” has the annoying tendency to mimic known forms of deception. Conditions assumed to be psi-conductive are suspiciously often the same as those known to facilitate legerdemain (e.g. the presence of skeptical magicians is frequently felt to be a psi-inhibitory factor). Magicians such as Houdini, Berglas and Randi have demonstrated that most if not all “psychic effects” can be convincingly replicated by means of trickery.⁴
- c. Fraud in parapsychology is hard to detect as one of the most obvious methods for checking unusual claims – replication – is tacitly assumed not to be applicable. Parapsychological findings are not expected to be repeatable, and failure to obtain

4 Randi, J. (1975). *The Magic of Uri Geller*. New York: Ballantine Books.

similar results under similar conditions may not even be cited as a ground for suspicion, or so proponents tell us.

- d. Positive results can only be obtained by certain parapsychologists, whereas their “psi-inhibitory” colleagues (to say nothing of disbelieving outsiders) seem constitutionally unable to create the conditions apparently necessary for paranormal manifestations. This odd and unsatisfactory state of affairs is compatible with the hypothesis that the ability to observe the paranormal is somehow linked to the psychological peculiarities of the individual experimenters.
- e. The “psi” effects reported by parapsychologists are typically so faint and marginal that it would usually require only a very minor manipulation to bring about equally “significant” but spurious results.
- f. Bona fide researchers may unwittingly become the conveyors of fraudulent data when out-witted by their (human) subjects. Much of the parapsychological evidence is based on experiments with “specially gifted” individuals who, in addition to being proficient in telepathy and psychokinesis, with distressing frequency turn out to be also specially gifted in the art of trickery. High-scoring subjects being a rare commodity in parapsychology, these individuals are often in a position to impose their own conditions on the experimenter (who may not even be aware that he is being manipulated). Breaches of protocol are not always detected and, if detected, not always reported.^{5,6}
- g. More often than not, both the parapsychologist and his subjects have a personal interest in positive results – even if we disregard the obvious psychological rewards. A record of successful experimentation is generally considered an asset for a parapsychologist who wishes to achieve prominence in the field. With a few laudable exceptions, parapsychological publications prefer reports of experiments confirming the psi hypothesis. Subjects sometimes stand to gain financially by scoring well.
- h. Several parapsychologists espouse ideologies which, while not condoning deception, seem to discourage attempts at demystification. Certain suspicious features of “psi” are widely believed to have been adequately explained by their contribution to postulated “properties” of the phenomenon (e.g., the presumed sensitivity of “psi” to unfavorable psychological conditions created by the presence of unbelievers.) In extreme cases, even evidence of blatant trickery is “explained” in

5 *Ibid.*

6 Marks, D., & Kammann, R. (1980). *The Psychology of the Psychic*. Buffalo, NY: Prometheus Books.

this fashion (e.g., “paranormal transformation” as an explanation for the embarrassing discovery that the tumors supposedly removed from the patient’s abdomen by “psychic surgeons” were in fact of animal origin.⁷) A currently popular theory associated with the name of Kenneth Batcheldor⁸ even justifies the faking of “paranormal” events in the initial stages of an experimental investigation. The idea is that the participants in a Batcheldorian séance will thus become convinced of the reality of the paranormal, which in turn will result in the production of genuinely psychic feats – belief being a necessary condition for the manifestation of “psi.” This philosophy might be construed as providing a moral justification of deliberate fabrication of experimental data as a means of convincing the skeptical establishment – whose conversion will in turn remove the psychological obstacle that prevents genuine “psi” from manifesting itself on a more regular base.

- i. Parapsychology is a notoriously stagnant science, where very little substantial progress has been made for the past century. Broad and Wade consider progress to be “the ultimate gatekeeper of science”: “In the end, bad theories don’t work, fraudulent ideas don’t explain the world so well as true ideas do.” Parapsychology’s conspicuous failure to generate ideas that progressively explain the “occult” means that, by Broad and Wade’s main criterion, the field has failed to clear itself of suspicions of impropriety.⁹

Possible and Actual Fraud

The above considerations suffice, in my view, to convince the impartial reader that fraud is a *potentially* fatal problem in parapsychology. Several parapsychologists, however, consider it illegitimate to even mention the possibility of fraud unless *positive* evidence of dishonest manipulation in *specific* experiments can be presented. This view is reflected in many of the proponents’ comments on Hansel’s *ESP and Parapsychology: A Critical Re-Evaluation*,¹⁰ which may be regarded as the classical statement of the fraud hypothesis. Hansel “debunked” a number of representative psi experiments by demonstrating that the positive results *could* have been faked, but did not provide conclusive evidence that in all cases fraud actually took place.

7 Stelter, A. (1977). *Psi-Heilung*. München: Wilhelm Goldmann.

8 Batcheldor, K.J. (1979). PK in sitter groups. *Psychoenergetic Systems*, 3, 77-93.

9 Broad, W., & Wade, N. (1982). *Betrayers of the Truth*. New York: Simon and Schuster.

10 Hansel, C.E.M. (1980). *ESP and Parapsychology: A Critical Re-Evaluation*. Buffalo, NY: Prometheus Books.

The standard objection to Hansel's view sadly ignores the elegant logic of the fraud argument. If fraud is a *possible* explanation of a highly successful parapsychological experiment then the probability that it *actually* occurred is given by the very "odds against chance" the parapsychologists cite in support of their belief that "psi" actually occurred. The statistical reasoning on which the scientific case for "psi" rests only justifies a decision for or against the null hypothesis but does not, in case of rejection of the null hypothesis, justify a decision for or against any of the possible non-chance hypotheses. In other words: The case for "psi" and case for "fraud" are equally supported by the "astronomical odds" one reads so much about in the parapsychological literature. Statistics will only begin to lend unambiguous support to the psi hypothesis as soon as the non-chance findings occur in the context of an experimental programme generating repeatable and progressively predictable data. Until a "repeatable experiment" establishes the superiority of the psi-model it remains a rational option to suggest "fraud" as an alternative explanation for the claims of parapsychology (at least in the relatively rare instances where no other non-paranormal explanation seems remotely plausible) even in the absence of conclusive evidence that fraud indeed occurred in a representative number of cases. However, the ultimate objective of the psi debate is to decide what is *true* rather than what is *conceivable*. If "psi" is a myth, and fraud and self-deception play a vital role in perpetuating this myth, then one may expect that traces of *actual* cheating (or of the sort of sloppiness most easily explained as the product of self-deception) will relatively frequently be discovered upon critical examination of the relevant evidence. While a dispassionate study of the field's history does not bear out the ultra-skeptical claim that parapsychology is a proven swindle the annals contain an impressive number of examples of skeptical suspicions being soundly confirmed.

Actual Examples

I intend to focus on the problem of fraud and self-deception in experimental parapsychology as, with such work, the margin of honest error is much smaller than with the reporting of observations made under non-experimental or quasi-experimental conditions. If "propriety" in this context may be defined as the intention to report faithfully all relevant data, then impropriety can be established much more unambiguously in cases where the relevance or irrelevance of facts is objectively defined by the logic of the experimental design. In non-experimental parapsychology, the criteria for relevance are fuzzy, and there is often room for legitimate disagreement over the question, what data should be reported and what data may safely be omitted.

The publications by the well-known German parapsychologist Hans Bender are a case in point. A critical examination of several of his reports of “psi events” observed under loose conditions has revealed numerous omissions, inaccuracies and errors that, by skeptical criteria, completely invalidate the claims these reports are supposed to support.¹¹ However, Bender may reject the skeptical criteria and claim, post factum, that by *his* criteria the errors concern trivial details.

Only in exceptional cases, non-experimental parapsychologists may safely be charged with deliberate fraud. For example, the well-known Dutch parapsychologist Wilhelm Tenhaeff, in a report on the clairvoyant Gerard Croiset’s alleged success in paranormally identifying an elusive arsonist, quoted a police report which alas proved to be non-existent.¹²

“First degree fraud” – intentional wholesale fabrication of data by the experimenter – is probably a relatively rare occurrence in experimental parapsychology. Far more common is the sort of misrepresentation (tidying up data, selective reporting of favorable findings, suppression of suspicious details expediently judged “irrelevant,” incomplete reporting of experimental conditions, etc.) that in some cases may be indulged in with a comparatively clear conscience. The distinction between minor dishonesty and innocent self-deception is usually difficult to draw.

Of all the proven cases of “first degree fraud” in parapsychology that of Soal is by far the most serious, as it involves an experimental project long believed to have provided the strongest evidence for the reality of “psi” in the history of the field and – ironically – hailed as the *nec plus ultra* in fraud-proof testing. The facts, briefly, are these. From 1941 to 1943, Samuel Soal, the foremost experimental parapsychologist in Britain, investigated the alleged psychic abilities of the photographer Basil Shackleton. He and his co-experimenter, Mrs. K. M. Goldney, reported the spectacular results of these sittings in the *Proceedings of the Society for Psychical Research*.¹³

The experimental procedure adopted by Soal was complex. Its basic feature was that Shackleton was asked to guess the identity of cards bearing animal symbols seen only by an agent situated in an adjoining room. The target sequence was decided by a prepared list

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- 11 Hoebens, P. H.: Abschied von Pirmasens. Eine kritische Nachprüfung eines erfolgreichen ASW-Experiments. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 26, 1984, 4-28. [For an English version, see chapter 3-11 of this volume. (Eds.)]
 - 12 Hoebens, P. H. (1981-1982). Croiset and Professor Tenhaeff: Discrepancies in claims of clairvoyance. *The Skeptical Inquirer*, 6, (2), 32-40. [See chapter 3-05. (Eds.)]
 - 13 Soal, S. G., & Goldney, K. M. (1943). Experiments in precognitive telepathy. *Proceedings of the Society for Psychical Research*, 47, 21-150.

of random numbers. The sheets containing these numbers included a “guess” column in which Shackleton’s calls were entered. In a review, the British philosopher and proponent of parapsychology C.D. Broad called Soal’s results “outstanding”: “The precautions taken to prevent deliberate fraud or the unwitting conveyance of information by normal means are described in great detail, and seen to be absolutely water-tight.”¹⁴ Similar sentiments were to be expressed by various other commentators, including virtually the entire elite of parapsychology. Many reviewers stressed the point that Soal had impressive credentials as a critic of other parapsychologists and as a “debunker” of spurious “phenomena.”

Well after the Shackleton series had been canonized as one of the classic proofs of parapsychology, suspicions began to arise as to the accuracy of the reports. In a controversial critique, George R. Price described six different methods that Soal could have employed had he wished to fake his results.¹⁵ When in 1956 C.E.M. Hansel wished to inspect the original record sheets for possible signs of tampering, he was told by Soal that the documents had been lost, having been left on a train in 1946.¹⁶ This faintly suspicious incident had not been published at the time, and seemed to be contradicted by Soal’s 1954 statement that all records could be re-checked at any future time.¹⁷ In a letter printed in 1960 in the *Journal of the Society for Psychical Research* (and apparently written after some pressure from critical researchers), Soal and Goldney revealed that Mrs. Albert, one of the agents in the experiment, had claimed to have seen Soal altering figures on the score sheet.¹⁸ That Mrs. Albert’s allegations had been more specific than is apparent from the 1960 letter was only revealed in 1971 by R.G. Medhurst: Mrs. Albert had claimed to have seen Soal altering 1’s to 4’s and 5’s several times.¹⁹ Mrs. Albert’s observations suggested the possibility that Soal might have cheated by initially placing an excess of 1’s in the list of prepared random numbers together with a deficiency of 4’s and 5’s. Some of the 1’s would then later be changed into 4’s and 5’s in order to conform, *post factum*, to Shackleton’s guesses. In 1974, C. Scott and P. Haskell showed that this procedure would have caused certain peculiar features to appear in the data (such as an excess of hits on 4’s and

14 Broad, C.D. (1944). Discussion: The experimental establishment of telepathic precognition. *Philosophy*, 19, 261-275.

15 Price, G.R. (1955). Science and the supernatural. *Science*, 122, 359-367.

16 Hansel, C.E.M. (1980), *op. cit.* (see note 10).

17 Soal, S.G., & Bateman, F. (1954). *Modern Experiments in Telepathy*. New Haven, CN: Yale University Press.

18 Soal, S.G., & Goldney, K.M. (1960). Letter. *Journal of the Society for Psychical Research*, 40, 378-381.

19 Medhurst, R.G. (1971). The Origin of the “prepared random numbers” used in the Shackleton experiments. *Journal of the Society for Psychical Research*, 46, 39-55.

5's) and that the data indeed showed these peculiarities.²⁰ Scott and Haskell presented very strong evidence for the hypothesis that something was terribly wrong with Soal's results, but many parapsychologists preferred to take comfort in the reflection that the findings, while damaging, were not conclusive.

In the issue of the *SPR Proceedings* where the Scott/Haskell critique appeared, several leading parapsychologists, such as John Beloff,²¹ Gaither Pratt²² and Ian Stevenson,²³ came to Soal's defense. In the monumental *Handbook of Parapsychology*, published in 1977, Soal's contributions to the field are extensively discussed, but no mention is made of the critics' discoveries.²⁴

In 1978, the fraud hypothesis was dramatically confirmed by the British statistician/parapsychologist Betty Markwick, who in the aforementioned *Proceedings* published the results of an extensive critical investigation.²⁵ She found that, contrary to the stated procedures, Soal had repeatedly used the same sequences of digits but had, at various points, interpolated extra digits. These interpolated digits almost invariably corresponded to "hits." In the series for which such irregularities could be detected, the "psi effect" dropped to chance level when these suspicious hits were removed.

Since the publication of the Markwick findings, virtually every parapsychologist agrees that the work of Soal is now irrevocably discredited. Even Pratt, who preferred to believe that Soal "used precognition when inserting digits into the columns of numbers he was copying down, unconsciously choosing numbers that would score hits on the calls the subject would make later" conceded that "we are compelled to consider all of the records to be invalid as evidence ..."²⁶ The only parapsychologist who, to my knowledge, has chosen to regard the Markwick findings as too irrelevant to warrant the attention of

20 Scott, C., & Haskell, P. (1974). Fresh light on the Shackleton experiments. *Proceedings of the Society for Psychological Research*, 56, 43-72.

21 Beloff, J. (1974a). Why I believe that Soal is innocent. *Proceedings of the Society for Psychological Research*, 56, 93-96.

22 Pratt, J.G. (1974). Fresh light on the Scott and Haskell case against Soal. *Proceedings of the Society for Psychological Research*, 56, 97-111.

23 Stevenson, I. (1974b). The credibility of Mrs. Gretl Albert's testimony. *Proceedings of the Society for Psychological Research*, 56, 117-129.

24 Wolman, B.B. (ed.) (1977). *Handbook of Parapsychology*. New York: Van Nostrand Reinhold.

25 Markwick, B. (1978). The Soal-Goldney experiments with Basil Shackleton: New evidence of data manipulation. *Proceedings of the Society for Psychological Research*, 56, 250-277. [Markwick further elaborated on her findings in Markwick (1985). (Eds.)]

26 Pratt, J.G. (1978). Statement. *Proceedings of the Society for Psychological Research*, 56, 279-281.

his readers is Bender, who in the 1981 “revised edition” of his *Unser sechster Sinn* still presents the experiments with Shackleton as having provided exceptionally strong evidence for “psi.”²⁷

It is difficult, to exaggerate the importance of the Soal affair for a rational evaluation of the claims of parapsychology. Consider the following points:

1. Soal was a highly respected investigator with an excellent record as a critic of other investigators’ sloppiness;
2. The precautions against fraud he had taken were such as to elicit cries of admiration from the luminaries of parapsychology;
3. The data on closer examination revealed all sorts of “meaningful patterns” which the parapsychologists interpreted as further proof of authenticity;²⁸
4. The parapsychological establishment was reluctant to become convinced by the mounting evidence until Soal’s dishonesty had for all intents and purposes been *proven*. Soal’s prominent defenders occasionally came up with arguments that seem unconvincing *only* in the light of later discoveries;
5. The discovery of the “smoking gun” was as much due to sheer luck as to Markwick’s perspicacity and determination. If Soal had not carelessly used the same sequences of digits repeatedly, the interpolated extra digits would have escaped detection.

The case of Soal is of exceptional relevance as it provides a dramatic “falsifying instance” to any general psychological theory proponents of parapsychology might wish to appeal to in order to buttress their belief that “fraud” is an unlikely explanation for the claims of the paranormal. To put it in a nutshell: If the Soal case was possible, then *anything* is possible.

Minor Cheating and Self-Deception

The Soal case is highly relevant, but there is no need to assume that it is also *typical*. Soal’s experimental procedure had been sufficiently stringent to minimize the possible effects of minor errors whether deliberate or inadvertent. Having thus deprived himself of the advantages of expedient “mistakes” he was forced to resort to gross deception when “psi”

27 Bender, H. (1981a). *Unser sechster Sinn*. (Rev. and enlarged ed.). München: Wilhelm Goldmann.

28 *Ibid.*

stubbornly refused to manifest itself. In most other psi experiments with comparably significant outcomes, conditions were such as to allow more scope for lucky misadventures. In some classic psi experiments such as the Pearce-Pratt series precautions against subject deception were less than perfect.

In a major methodological critique,²⁹ Akers reviewed 54 post-war experiments collectively representing “the best case for psi” and found so many potentially invalidating flaws (target randomization failures, loopholes allowing sensory leakage or subject cheating, recording errors, statistical violations, reporting failures, etc.) that he had to conclude: “When all 54 experiments are considered, it can be stated that the research methods are too weak to establish the existence of a paranormal phenomenon.”

In a similar review, focusing on a much publicized experimental programme employing the so-called Ganzfeld technique (where sensorily isolated subjects are requested to describe randomly selected target pictures) Hyman reached similar conclusions.³⁰

The methodological weaknesses discovered in these experiments increase the possibility that some of the claimed results may have been affected by minor dishonesty or self-deception. Moreover, a failure to take certain precautions may itself be a form of “fudging.”

The Case of J.B. Rhine

Even though gross fraud à la Soal *may* be more common than I prefer to think likely it seems reasonable for the skeptic to assume that the production of spurious psi evidence typically takes place in the twilight zone between intentional deception and honest error.

A historical, previously unpublished, example involving the best known parapsychologist in this century was kindly brought to my notice by the Dutch parapsychologist Heyme Breederveld.³¹ On 12 March 1966, Breederveld wrote to J. B. Rhine mentioning the results of an as yet incomplete series of “psychokinesis” experiments with dice under systematically varying conditions. A crucial sentence in the letter reads: “The length of the runs, the length of the series and the methods of statistical analysis were planned beforehand.”

29 Akers, C. (1984). Methodological criticisms of parapsychology. In Krippner, S. (ed.), *Advances in Parapsychological Research*. Vol. 4 (pp. 112-164). Jefferson, NC & London: McFarland.

30 Hyman, R. (1985). The Ganzfeld / psi experiment: A critical appraisal. *Journal of Parapsychology*, 49, 3-49.

31 Breederveld, H. (1981). Personal communication.

The hit/miss ratio in the eleven series completed at that point showed something suggestive of the “decline effect” dear to Rhine: a presumed tendency for positive scoring in psi tests to decrease in time. The “decline effect” observed in Breederveld’s scores would have been significant had it not been for the tenth series (showing an *increase* in the number of hits) and for the fact that the experiment was not yet completed and a decision to terminate the project at that point would require the experimenter to correct for optional stopping. Due to the specific conditions (involving the use of a tranquilizer by the subject) the unobliging tenth series had been shorter than the other ten. However, this had been decided beforehand.

In his reply, dated 4 April, 1966, Rhine enthusiastically welcomed Breederveld’s results and invited his junior Dutch colleague to write a full and formal report for eventual publication in the *Journal of Parapsychology*. Rhine was especially impressed with “the significant CR [critical ratio] of the difference between the first half and the second half of the ten complete series, omitting your number ten as incomplete.” He continued: “If this is the stage at which you left this project I would suggest not continuing it, for the reason that you would involve a different psychological interest now at this stage, after discussing it and considering the writing of a paper.”

What Rhine was in fact suggesting to Breederveld was that the latter post hoc change the agreed-upon rules of the game and publicly present as “significant” results that were in fact conform to chance expectation given the logic of the experimental design. The reward: an opportunity to publish in the prestigious *Journal of Parapsychology* which at that time, as a matter of stated policy, only accepted reports of experiments with significant results.

Breederveld has taken a dim view of Rhine’s work ever since. I have discussed this case at some length precisely because it is rather unspectacular and may therefore be far more typical than the scandals involving Soal, Levy (see below) and Tenhaeff. There is no need to assume gross dishonesty on the part of Rhine: He may have sincerely believed that his suggestions were ethically and scientifically justified. However, in the light of Breederveld’s experience it seems legitimate to wonder to what extent Rhine’s own experimental data may occasionally have been prepared according to the recipe he recommended to his younger fellow-parapsychologist. Rhine’s work has come under heavy attack,³² but in general the criticisms have focused on the possibility of inadequate experimental design and sloppy reporting. Plausible charges of outright fraud have not, to my knowledge, been hurled at him. However, the complete history of the Rhine epi-

³² Hansel, C.E.M. (1980), see note 10.

sode (including a full account of some puzzling incidents that have taken place at his celebrated Durham laboratory) has yet to be written.

In a 1974 article on “Security versus Deception in Parapsychology,”³³ Rhine described twelve cases of fraud within parapsychology, from the 1940s and the 1950s, without identifying the culprits. Discussing the current scene, he confidently concluded that “we have been able to do quite a lot to insure that it is impossible for dishonesty to be implemented inside the well-organized laboratory today.” Only a few months later,³⁴ Rhine had to report that the acting director of his own laboratory, Walter Levy, had been caught faking his data in experiments with paranormally gifted mice. Rhine has been praised for his promptness in publicly revealing this embarrassing scandal. However, there is considerable internal controversy over the question to what extent Rhine’s apparent candor may have been the reluctant result of pressure from irate associates.³⁵

As I have attempted to demonstrate, it remains a valid option for the skeptic to assume that “psi” is a non-existent phenomenon and that the inexplicable residue of the parapsychological evidence (i.e. the evidence that continues to resist other “naturalistic” counter-explanations) must be the result of fraud, minor cheating or self-deception.

The fraud argument, however, should be used with great caution. It can be successfully employed to deflate the claim that parapsychology has proven beyond reasonable doubt that “psi” exists. It cannot, in fairness, be used to deny parapsychology the status of a legitimate protoscientific enterprise.

Several influential critics of parapsychology alas delight in misrepresenting the field. According to their polemical writings, parapsychology is medieval superstition parading as modern science. Its ultimate objective is the overthrow of the reign of Reason and a restoration of the dark ages of magical belief. In order to conceal their irrationalist aims, the parapsychologists have concocted transparently phony “proofs” which of course will evaporate instantly as soon as a card-carrying skeptic takes the trouble to subject them

33 Rhine, J. B. (1974a). Security versus deception in parapsychology. *Journal of Parapsychology*, 38, 99-121.

34 Rhine, J. B. (1974b): A new case of experimenter unreliability. *Journal of Parapsychology*, 38, 215-225.

35 See Rogo’s controversial chapter in the *Skeptic’s Handbook* (Rogo 1985), which indeed should not be taken at face value. There was a protracted controversy in 1984 and 1985 between Scott Rogo (Rogo 1984) and Rhine’s FRNM (Broughton & Rao, 1984) over the justification of Rogo’s charges that Rhine had fired Levy only after pressure from Rhine’s collaborators and some outsiders. Both 1984 documents referred to as well as Rogo’s 1985 *Handbook* article were written after Hoebens’ death so he could not possibly have known and considered them. (Eds.)

to a cursory examination. The parapsychologists are unable to see the obvious, as they are blinded by metaphysical prejudice.³⁶ Although several individual parapsychologists convey the impression of being obsessed by the desire to conform as closely as possible to the above stereotype it would be grossly unfair to claim that this is true for the parapsychological community as a whole.

The fundamental error in the propaganda of the extreme skeptics (apart from their tendency, seriously to underestimate the scientific case *for* “psi”) is their tacit assumption that a commitment to parapsychology implies a strong belief in supernatural forces and that, therefore, only outsider skeptics are capable of a critical assessment of claims of the paranormal.

In fact, the constitutions of the major parapsychological organizations have traditionally been non-committal as to the authenticity of the paranormal phenomena. While most parapsychologists accept “psi” as real or very probably real, others have dissented without their dissent leading to excommunication. Some of the very best “debunkings” of parapsychology or major aspects thereof have been published by “insiders.” In recent years, the parapsychological community has formed an increasingly effective internal “police force,” and fraud is discussed more candidly than presumably is the case in any other science or proto-science.³⁷ Recently, I had the privilege of being invited to act as an informal counselor to a parapsychological ad hoc committee investigating serious charges of fraud against a prominent investigator. No details of this case may yet be published, but I was impressed by the parapsychologists’ determination to get at the truth of the matter.³⁸

The reality of the psi debate is far more complex than is apparent from the polemical writings pro and con, with their monotonous emphasis on the stereotypical believer/skeptic dichotomy. Three of the sharpest critics of parapsychology alive today, Charles Akers, Susan Blackmore and Gerd Hövelmann, are themselves members of the Parapsychological Association and, ipso facto, recipients of ultra-skeptical abuse. On the other hand, radical skeptics in the German-speaking world revere the memory of the great skeptic Carl Graf von Klinckowstroem – whereas Klinckowstroem believed in “psi” and Blackmore does not.

36 Wimmer, W. (1980). Hexenwahn an Universitäten? *Zeitschrift für Allgemeinmedizin*, 56, 1390-1400.

37 Millar, B. (1979). Psi, experimenter-effect or experimenter-fraud? Paper presented at the Conference of the Society for Psychical Research, Edinburgh.

38 Cf. the Editorial Postscript to chapter 2-09 in this book. (Eds.)

To compensate for the mainly destructive nature of the arguments advanced in this essay, I wish to conclude with a few constructive suggestions for the future of the psi debate. Reasonable proponents and reasonable skeptics can agree that the subject matter of parapsychology – the myriad reports of occurrences suggestive of something “paranormal” – is of sufficient interest to warrant the close attention of the scientific community.

The question whether or not these reports are indicative of a paranormal reality is empirical and cannot be settled by a priori argument. At the present stage, neither proponents nor skeptics can point to empirical findings *definitively* vindicating or invalidating the respective positions. “Belief” and “disbelief” are not based on unambiguous evidence, but rather depend on individual expectations as to the eventual outcome of the search for such evidence. Rather than indulging in the futile pastime of proclaiming the superiority of one’s own insight, believers and unbelievers should view their expectations as stakes in a game-like, Lakatosian duel of “research programmes.”³⁹ Here, the proponent predicts that increasingly sophisticated research will result in a progressive accumulation of findings supportive of the psi hypothesis, whereas the skeptic predicts a progressive erosion of the parapsychological evidence as it becomes increasingly amenable to “naturalistic” explanations. In this game, the fraud argument is a powerful weapon against proponents who prematurely claim total victory – but not powerful enough to secure total defeat.

39 Lakatos, I. (1978). The methodology of scientific research programmes. In: Worrall, J., & Currie, G. (eds.), *Philosophical Papers. Vol. 1* (pp. 8-101). Cambridge: Cambridge University Press.

CHAPTER 3

Research and Debunking



CHAPTER 3-01

Editorial Introduction

*Traditionally, the Netherlands seem to have been blessed with an above-average number of psychic detectives. It is hardly surprising, therefore, that Hoebens, when he began to develop a skeptical interest in parapsychology and its public repercussions, devoted much time and attention to the tales about occult crime-busters in his native country. This has resulted in a number of publications that were to re-investigate and evaluate relevant cases that had acquired particular prominence both in scientific circles and in public perception. Hoebens' investigative methodology was basically modelled, with various modifications, on the example that Albert Hellwig's voluminous study *Okkultismus und Verbrechen* [Occultism and Crime] had set, in the late 1920s, for the German scene (Hellwig, 1929). Elsewhere (cf. chapter 2-12 in this book), and shortly before his death, Hoebens modestly stated that, in retrospect, his "investigations into the claims surrounding the best-known Dutch 'paragnosts' may be seen as a contemporary footnote to Hellwig, confirming his essentially negative conclusions."*

*Three such examinations (on alleged psychic success cases of Peter Hurkos, Gerard Croiset and Marinus Dykshoorn) were written, at Marcello Truzzi's invitation, for Truzzi's periodical, the *Zetetic Scholar*. They were published in the years 1981 and 1982, respectively, and form "The Mystery Men from Holland" series. That series started with a critical study of the Dutch cases of Dutch-American psychic Peter Hurkos. The article appeared in *Zetetic Scholar*, No. 8, 1981, pp. 11-17. (Eds.)*

THE MYSTERY MEN FROM HOLLAND, I: PETER HURKOS' DUTCH CASES

The minuscule kingdom of the Netherlands has produced what seems a disproportionate number of occult detectives – individuals who profess to assist the police by paranormal means in locating missing persons and solving crimes.

Foreign newspaper reports have conveyed the impression that psychics are employed as a matter of course in Dutch police investigations. Some spectacular cases continue to be cited in American and English publications, usually accompanied by the assurance that the author has personally verified the evidence.

The purpose of the present article is not that of evaluating the whole problem of occult crime-busting, but that of critically examining a number of Dutch miracles *as they have been reported in the Anglo-Saxon world*. I have selected those reports with the strongest claim to respectability, and compared them to whatever authentic sources I could trace in Holland.

Peter Hurkos' Dutch Years

Of all Dutch "paragnosts" (as they are called in Holland), none has achieved more fame in the US than Pieter van der Hurk, alias Peter Hurkos. The Radar Brain Man was born in Dordrecht on May 21, 1911. He spent the major part of his professional life in the US, and is now an American citizen, but his relatively short "native" period is by no means unimportant. All his biographers, himself included, agree that Hurkos amazed the Dutch before he went on amazing the rest of the world.

The principle sources on Hurkos' Dutch years are his autobiography *Psychic, the Story of Peter Hurkos* (Hurkos, 1961); an authorized biography by Norma Lee Browning, *The Psychic World of Peter Hurkos* (Browning, 1970); an interview in the book *Psychics* (Editors, *Psychic Magazine* 1973) and a chapter in Fred Archer's *Crime and the Psychic World* (Archer, 1969).

Some of the claims made in these books are so extraordinarily vague that it is quite impossible even to attempt to check their accuracy. For example, on page 71 of *Psychics* Hurkos states: "It [helping the police solve crimes] started when I was in Holland. I was asked to help locate a little girl who was missing. It was sad: some woman had killed her and threw her in the water. I found the girl's body and helped solve the case." No names, dates or places are mentioned, except that the event occurred in the Netherlands, early in Hurkos' career. The case – his first success as a psychic sleuth – is unaccountably ignored both in the autobiography and in Mrs. Browning's book. Dutch files which I was able to consult do not contain the smallest hint as to what event the clairvoyant may be referring to.

Undated too is the case Hurkos relates on pp. 66-67 of his autobiography. "One day" he found the body of the son of one Captain Folken. The boy had drowned in Rotterdam harbor. "The police were as amazed as I was at the accuracy with which my extra-sensory perception had been employed in this case," Hurkos claims.

I am grateful to Mr. Bouwman of the Rotterdam Municipal Police for having attempted to verify this tantalizingly concise story. Mr. Bouwman looked through numerous volumes of police reports and contacted several retired policemen who

might remember such an incident. In spite of his determined efforts, no trace was found of a document pertaining to a case resembling the one Hurkos reports. No member or former member of the corps remembered anything. The enthusiastic newspaper reports Hurkos mentions seem to have disappeared mysteriously from the files of the local papers. Mr. Henk Schröder, who kindly offered to search the press archives, drew a blank.

While this failure to unearth corroborating evidence does not actually disprove the claim it certainly cautions us against accepting it at face value.

If I do Mr. Hurkos an injustice by casting doubt on the veracity of his report, he himself is to blame. Fortunately, in relating some other cases Hurkos and his biographers have included enough details to allow the investigator to draw firmer conclusions.

The Psychic War Hero

As is well known, Hurkos acquired his uncanny gifts as the result of a fall from a ladder in the summer of 1941. At that time, Holland was an occupied country, and the Dutch police was controlled by the German invaders. In those circumstances it would have been unpatriotic to assist the authorities, so Peter Hurkos decided to use his ESP to further the aims of the resistance movement. In his autobiography, he portrays his own role in World War II in heroic colors. He states he had been a member of an underground group led by “a man named Hert Goozens, one of the bravest men in the entire system of secret fighters.”

Mrs. Browning (1970) reports that after the occupation was over, Hurkos was received at the Royal Palace by Queen Wilhelmina and was presented by Her Majesty with a gold medal and a charter proclaiming his valorous deeds in the service of the Fatherland.

Some of Hurkos’ reported activities as a war hero are for all intents and purposes in the psychic sleuths category and deserve to be mentioned here.

On pp. 17-20 of his autobiography, Hurkos relates how, when he was still in hospital recovering from his fall, he was visited by a mysterious stranger who was about to be released after an emergency appendectomy. As they shook hands, the newborn psychic “knew” that the other man was a British agent who was destined to be killed by the Germans on Kalver Street a few days later. In vain, he tried to prevent the doomed stranger from leaving the hospital. “He will be killed on Kalver Street! Stop him! Stop him!” he cried. The doctor and the nurse thought he had a raging fever. Two days after his release, the

British agent was in fact shot dead by the Gestapo on Kalver Street. Hurkos learned this while he was still in hospital. According to an official report cited by Mrs. Browning, the sensitive had been admitted to Zuidwal hospital on July 10, 1941, and had been released on August 5 of the same year. So the Briton must have met his dramatic death some time between these dates. Given the unusual circumstances (the Nazis certainly did not make a habit of executing captured enemy agents on busy streets), I expected this incident to have been extensively documented.

If the event had really occurred, the State Institute for War Documentation (known as RIOD) in Amsterdam should certainly know about it. Its archives are by far the most complete of their kind. I made inquiries with the RIOD and a few months later received a letter, dated February 1981, from drs. C.J.F. Stuldreher, whose help I gratefully acknowledge. Concerning the murdered Briton, drs. Stuldreher writes:

It is not known to us that in the summer of 1941 a 'British' agent (either of Dutch or British nationality) has been shot by the Gestapo in the Kalver Street in Amsterdam or any other Dutch town. It is very improbable that this occurrence really took place.

On pp. 40-43 Hurkos relates his most daring war time feat. A friend had been arrested by the Germans and taken to a camp in the town of Vught (which Hurkos erroneously thinks is only a few miles from The Hague – the actual distance is closer to 60 miles). Peter got hold of a German officer's uniform, went to Vught, introduced himself (in "flawless German") as "Wehrmachtskapitän Robert Fischner" and told the camp commandant that the spy was needed at headquarters for questioning. The Germans readily believed him and took him to the barracks where his friend, Yap Mindemon, was held. The moment Hurkos entered he knew, by ESP, that Yap was going to spoil the entire plan. The prisoner would assume that Hurkos had gone over to the enemy side. "In a moment, I could feel, he would shout and denounce me to the Germans..." Hurkos had no choice. Cursing – in flawless German – he hit and kicked Yap until the poor fellow was knocked out. The soldiers carried the unconscious prisoner to the staff car that the camp commander had kindly put at "Robert Fischner's" disposal. "Arrogantly I slid behind the wheel and drove through the gates of the camp as fast as I could go," the psychic recalls. Thus, Yap Mindemon was rescued from the clutches of the Nazis.

The files of the Vught camp are kept at the RIOD, and drs. Stuldreher kindly checked them for any evidence of this heroic deed. He found nothing whatsoever. His verdict: "The story seems to me a product of the imagination."¹

1 Hoebens here actually missed an opportunity to point out an additional (and not exactly

On pp. 126-130 we find what must have been one of the most dramatic cases in which Hurkos ever was involved. After having complained that “in a world of skeptics and fakers, it is not easy for a psychic to establish a reputation for truthfulness and accuracy...,” Hurkos recounts how, “one day,” he was invited to give a séance in the house of Mr. R. “one of Holland’s richest, most influential, and renowned patriots.” The guests were impressed, but Mr. R. himself remained incredulous. Then came his turn to hand Hurkos an object to “psychometrize.” He chose a cigarette case. The psychic touched it, and suddenly was hit by a terrible vision. “Sixteen Dutchmen – Sixteen Dutchmen!” he exclaimed. “What sort of man are you! Sixteen men – shot!”

Mr. R. choked, and in a desperate voice he gasped, “[y]ou are insane! Give – me – that...” It was too late. The man collapsed on the floor and lay still. The guests sat frozen in their chairs. Hurkos could not restrain himself. “He is a traitor!” he cried. “He was honored by our country as a patriot, and he betrayed us – sixteen men – shot – sixteen Dutchmen shot – and all his fault! He made a deal with the Nazis; they ran his factories but he controlled them.”

Mrs. R. then became hysterical. “He’s dead, he’s dead – and you killed him!” she screamed. “Liar! Liar!” But Hurkos had told the truth. After “five long, lonely, haunted years” it was finally established that Mr. R., the honored patriot, had indeed been a collaborator with the Germans. He had betrayed sixteen members of the underground to the Gestapo. All had been shot. Unfortunately, the enormous files of the RIOD do not contain the slightest indication that this drama, or anything like it, ever took place. Nothing is known there about “one of Holland’s richest, most influential, and renowned patriots” who has posthumously been exposed as a traitor. Drs. Stuldreher has the impression that the story falls in the same category as the Vught case. I do not know where Mrs. Browning checked her claim that Peter Hurkos after the war was decorated by her Majesty. The RIOD “has no information on any underground activities of Mr. Hurkos, alias Pieter Cornelis van der Hurk,” Drs. Stuldreher wrote me. Mr. Hert Goozens, “one of the bravest men in the entire system of secret fighters” must have been extraordinarily fond of secrecy, for even now nothing is known about him or his group.

clever) untruth on Hurkos’ part: There in fact never was the military rank of a “Wehrmachtskapitän” in Germany. This is not really surprising since “Wehrmachtskapitän” is contradictory in terms. “Kapitän” was a rank reserved for the German Navy, whereas “Wehrmacht” was the name of the armed (land) forces during the Third Reich. So had Hurkos, at the Vught camp, really introduced himself as a “Wehrmachtskapitän” we probably never would have heard from him thenceforth. This error arguably also throws some light on Hurkos’ alleged fluency in German. (Eds.)

Psychic Detective

After the Liberation, Peter Hurkos put his uncanny gifts at the disposal of the Dutch authorities. An early success is related on pp. 64-65 of the autobiography and on page 181 of Archer (1969). In the autumn of 1946 a young coalminer in the province of Limburg was murdered by his stepfather, Bernard van Tossings, who was known to have a jealous passion for his stepson's wife. The police were sure of his guilt, but they did not have conclusive legal evidence. For that, they needed to find the weapon with which the crime had been committed. Hurkos appeared on the scene, handled the victim's coat, gave an accurate description of the suspect (moustache, spectacles, wooden leg) and urged the police to "take a look at the roof of the murdered man's house." There, a revolver was found. The stepfather's fingerprints were on the butt, assuring his conviction.

This claim received a certain amount of publicity in Holland in 1958, when word got around in the province of Limburg that Hurkos was planning to shoot a motion picture in the coalmine area, featuring the psychic himself solving local mysteries. From what they had heard of the cinematographic project, the Limburgers feared that they might be used as "witnesses" of dubious occult successes. The Amsterdam newspaper *De Telegraaf* then phoned Hurkos in the US and asked him what cases would be highlighted in the film. Hurkos mentioned the Van Tossings affair. The journalists checked with the Limburg authorities and learned that Hurkos had indeed made some statements concerning the murder of a young coalminer that had taken place in October 1946 in the municipality of Spekholzerheide. The suspect had been arrested immediately after the crime, as it was known that he had quarreled with the victim. After having been handed a photograph, Hurkos had stated that the weapon would be found *in a brooklet*. The police dragged in vain. The revolver was found the next year, not in a brooklet, but on the leads of a house. The murder had not been one of the "crime passionelle" type, and the victim's wife had played no role whatsoever in the tragedy.

Hurkos had told *De Telegraaf* yet another story. In 1955, 43 coalminers had been trapped inside a Limburg coalmine, as a result of a failure in the lift system. After seventeen anxious hours they had been rescued. Investigation showed that the cause of the accident had been sabotage. The miners vowed to go on strike unless the culprit be found. At that moment, Peter Hurkos appeared on the scene. The chief of the mine police implored him to help. The clairvoyant obligingly psychometrized the wardrobe of all the personnel and picked out the working attire of an elderly employee. Guided by his paranormal intuition, he walked straight to the man's house. Inevitably, the culprit confessed.

The real facts, *De Telegraaf* learned from the authorities, had been slightly less dramatic. The failure had occurred in an *unmanned* lift, and the cause had been wear, not sabotage. No suspect was ever arrested for the simple reason that no crime had been committed (De Telegraaf, 1958).

To the north of Limburg lies the city of Nijmegen, the scene of what is reported as Hurkos' most impressive success in his native country. The relevant part of the autobiography (pp. 89-93) can be summarized as follows.

In August 1951, an outbreak of arson occurred in the area around Nijmegen town. Farmers were terrorized. The damage amounted to hundreds of thousands of dollars. Two hundred men patrolled the countryside, but could not prevent the pyromaniac from striking time and again. At the request of an industrialist friend, Hurkos offered his assistance. At first, the police were reluctant, but they rapidly changed their minds after the psychic had demonstrated his uncanny powers by accurately describing the contents of police chief Cammaert's pockets. Hurkos was taken on a tour of the burned-down farms. In the fourth ruin he visited he found a key. The moment he touched it he "saw" the arsonist. He told the police that the criminal was a boy of fifteen or sixteen years of age. The suspect was tall, had worked in a bakery but had been fired because he had tried to set fire to the place. The police told Hurkos they had pictures of all the town boys and asked him if he thought he could pick out the suspect. Hurkos was sure he could. In the police station, an officer "pulled out the highschool yearbooks of the schools for the past five years." The psychic began leafing through one of them and suddenly recognized the boy he had seen in his vision. The police were incredulous, as Hurkos had picked out Piet Vierboom, the seventeen year old son of a rich and respected Nijmegen family.

"I don't believe it," Captain Cammaert is quoted as saying, "It can't be. The family is one of the finest in Nijmegen." Shaking his head doubtfully, Captain Cammaert nevertheless fetched the boy for interrogation. Piet denied. Then Hurkos took over the questioning. Piet was lost. "I cannot lie to *you*. Yes, I did it," he exclaimed – so we are told in the autobiography.

This case has become something of a classic. It may have served as Hurkos' visiting card to the US, as it was featured prominently in the June 1956 issue of *True* that brought the sensitive to the attention of the American public. The story, titled "Man with the X-RAY MIND" and written by John Kobler (1956), is basically identical to Hurkos' own version. In addition, it mentions the name of "Baron Speyart von Woerden," chief of police for the region, as a witness. The "amazing story of extrasensory perception" is accompanied by an endearing painting by William A. Smith. There, we see Hurkos in a prophetic posture confronting Piet Vierboom, looking for all the world like Huckleberry

Finn. The lad, white as ash, is backing away, while the Nijmegen police, dressed as officers of the pre-war Royal Bulgarian Army, sternly look on.

The case is described by Archer (1969) and Browning (1970) too. Mrs. Browning's account is slightly different from that in the autobiography. For example, she states that the police asked for Hurkos' assistance, whereas Hurkos says he volunteered. In Browning (1970), the clairvoyant is not leafing through highschool yearbooks (which, incidentally, did not exist in Holland at that time), but is concentrating on photographs spread upside down on a desk. Mrs. Browning compares this psychic achievement with Swedenborg's celebrated vision of the Stockholm fire.

Unambiguous contemporary accounts of Swedenborg's feat are sadly lacking, but in the Nijmegen case we are more fortunate. The outbreak of arson and Piet Vierboom's arrest were widely reported in the Dutch press, as was Hurkos' involvement in the case. In addition to several newspaper reports, I have been able to consult a letter dated June 19, 1956, and signed by Baron Speijart von Woerden,² at the appropriate time Public Prosecutor in the Arnhem district.

From these sources, the following facts can be established. The arsony started on August 12, 1951, in the Ooijpolder, an agricultural area near Nijmegen. Soon, the police began to suspect Piet Vierboom, the son of a Ooijpolder farmer. The boy, who was mentally deranged, had been employed at Van Mook's bakery in the village of Bommel. On June 22, 1951, a mysterious fire had raged in the bakery. The local police thought Piet had been responsible, but the evidence was not strong enough to warrant an arrest. The authorities, however, remembered the incident and discovered that the Ooijpolder fires coincided with Piet's holiday, which he spent at his parental home. On August 14, at the site of one of the fires, candy wrappings were found. Investigation showed that Piet had recently bought a considerable quantity of this particular candy at the local sweet shop. The boy was quietly arrested on August 17. He was taken to Nijmegen and interrogated by Speijart von Woerden, who was convinced of his guilt.

At the same time, Hurkos happened to be in Nijmegen where he was to give a public séance. According to Speijart, this "stage telepathist" needed some publicity and offered his assistance to the police. In the afternoon of August 18, *the day after Piet Vierboom had been arrested*, the clairvoyant arrived at the police office. A policeman *who had been present at the interrogation* showed him a group picture of the Vierboom family. With an iron hook, Hurkos made "passes" over the photograph. At one moment, he pointed at one of the boys and stated that this was the pyromaniac. Those familiar with the telemetry

² This name is written in several different ways in relevant documents. (Eds.)

techniques of muscle-reading (in which Hurkos may be a professional expert) will not be surprised that the psychic picked out Piet (whose arrest had not yet been made public). "I cannot imagine that Peter Hurkos' performance had any scientific value whatsoever," Speijart concludes his letter.

I can add that I have a photostat of a statement by the State Police of the Nijmegen district dated June 23, 1956, in which it is categorically denied that psychics were ever successfully employed in criminal investigations. The statement is signed by Mr. A. Cammaert ...

Acknowledgement

I am grateful for the help of Dr. [Filippus] Brink in obtaining file material relating to the Nijmegen arson case.

CHAPTER 3-02

Editorial Introduction

The second part of “The Mystery Men from Holland” series considers several much-discussed cases associated with the well-known Dutch “paragnost” Gerard Croiset, the star psychic, as it were, of the doyen of Dutch parapsychology, Prof. W. H.C. Tenhaeff. As readers will not fail to recognize from other chapters of the present book, Gerard Croiset also has been the subject of several other of Hoebens’ critical re-examinations of alleged prize cases in psychical research. The following paper was published in the Zetetic Scholar, No. 9, 1982, pp. 21-32. (Eds.)

THE MYSTERY MEN FROM HOLLAND, II: THE STRANGE CASE OF GERARD CROISET

With few exceptions, educated citizens of the Netherlands have always tended to dismiss their former fellow-countryman Peter Hurkos as a typical Hollywood character: good enough for gullible Californians, but far too implausible for sober Dutchmen. Gerard Croiset, however, is a different kettle of fish. This remarkable clairvoyant, who died in July 1980, was taken fairly seriously in his native country, even by many persons who otherwise professed a strong disbelief in the occult. The case of Gerard Croiset is a strange and complex one. To a certain extent, he is a genuine challenge to the skeptic. My private belief is that he had no more than five senses. This opinion, however, may be strongly influenced by what Dr. Beloff would term my “metaphysical predilection” for the non-existence of psi.

On the basis of the evidence which others and I¹ have uncovered, I may certainly urge the reader, at the very least to suspend belief in Croiset’s paranormal powers. Even the most copper-bottomed of “proofs” I have found, are not above suspicion. It is true that Gerard Croiset, virtually alone among the internationally famous psychic detectives, has been vouched for by a prominent parapsychologist. However, the work of Dr. W. H.C. Tenhaeff has now been shown to be flawed in unsuspected ways. It seems perfectly rational to expect that the entire Croiset phenomenon in due time will be explained in terms of erroneous reporting, personal validation, coincidence and fraud. Yet I prefer

1 See chapter 3-04 in this book. (Eds.)

not to draw premature conclusions. I must point out, for example, that Mr. George Zorab (Zorab, 1965), who for years has drawn the attention of his fellow psychical researchers to serious shortcomings in the published evidence and who from personal experience is convinced that Croiset was at least a part time cheat, yet continues to feel that the subject of this article was a genuine sensitive. The question as to whether Croiset had any powers of extrasensory perception (if such exist) will not be settled here. I will restrict myself to presenting further reasons for extreme caution in accepting the proponents' reports at face value. As in my earlier article on Hurkos,² I will critically examine supposedly respectable accounts of the psychic's feats *as they have been published in English*.

Myths

My statement that Croiset "was taken fairly seriously in his native country" should not be misunderstood. It is true only if we compare his local reputation with Hurkos' or Dykshoorn's. Contrary to popular mythology abroad, however, Croiset most certainly was not the psychic stand-by of the Dutch police. Although incidental cases of co-operation are known, the police in the Netherlands have traditionally been skeptical of paranormal detectives. Reports published abroad often convey a highly misleading impression. With sensationalist newspapers such as *National Enquirer* this is to be expected. However, the mis-information is not restricted to the tabloids.

Mr. Roy Stemman, co-editor of the now defunct magazine *Alpha* and an experienced reporter on the occult, furnishes a typical example in his 1981 article "Croiset: The Psychic Detective" (Stemman, 1981). This article is accompanied by a photograph showing Dr. Tenhaeff, the clairvoyant and a uniformed individual whom Mr. Stemman identifies as "the Utrecht chief of Police." According to the caption beneath, "they were a regular team, Croiset helping the police in their search for missing persons and Professor Tenhaeff monitoring the clairvoyant's progress." Untrue, I'm afraid. The uniformed gentleman is *not* the Utrecht chief of police. And neither did Croiset, Tenhaeff and the Utrecht chief of police form a "regular team." In fact, the successive Utrecht chiefs of police have been notoriously skeptical of Gerard Croiset. One of them, Mr. Th. van Roosmalen, was the author of one of the most devastating "debunkings" of that psychic ever published (Roosmalen, 1960). As late as 1980, the official spokesman of the Utrecht corps told me that none of Croiset's attempts to locate missing persons or solve crimes in his home town had ever been successful.

² See chapter 3-01 in this book. (Eds.)

Mr. Stemman's article concludes: "Gerard Croiset died on 20 July 1980, at the age of 71. But the records on file at Utrecht University will continue to intrigue and baffle scientists for many years to come." I am afraid that scientists who wish to be intrigued and baffled will come to Utrecht University in vain. The whereabouts of Dr. Tenhaeff's celebrated files are a mystery, even to Dr. Tenhaeff's successor as "special Professor of Parapsychology" (Professor Henri van Praag, personal communication, 1981). Some backstage information enables me to make an educated guess as to what had happened to these precious documents. In any case, they are not available for examination. The desire of certain persons to avoid further embarrassment may have contributed to this sad state of affairs.

Tenhaeff and Pollack

As I have argued elsewhere,³ the decisive factor in Croiset's rise to international fame has probably been the fact that his powers of extrasensory perception have been vouched for by a prominent psychical researcher. Professor Doctor Wilhelm Heinrich Carl Tenhaeff enjoyed a considerable reputation, especially on the European continent. He held the first chair of parapsychology ever established at a major western university. His German colleague Professor Hans Bender has praised him as one of the great pioneers of parapsychology. When Tenhaeff died in July 1981, Professor Andreas Resch, the catholic parapsychologist of the Innsbruck Imago Mundi Institute, wrote an extensive obituary for the German magazine *Esotera* (Resch, 1981), entitled "Search for the Truth," in which the deceased was listed with the "great researchers of the soul in the history of psychology and parapsychology." The *Parapsychology Review* called him a "noted world figure in parapsychology" (*Parapsychology Review*, 1981). Given the chief chronicler's credentials, it is hardly surprising that writers on the occult, particularly if they were both foreigners and "believers," were only too happy to accept at face value what they were told about "The Dutchman with the X-ray Mind." After all, there was a body of "official" evidence, collected, verified, and published by a respected University Professor.

To a certain extent the book *Croiset the Clairvoyant* by the American journalist Jack Harrison Pollack (Pollack, 1964) forms part of this official evidence, as it was written under the personal supervision of Tenhaeff, who double-checked the manuscript and who openly endorsed the book – which has been translated into German and French. *Croiset the Clairvoyant* is an important source, as few of Tenhaeff's own publications are available in English. In this article, I will critically examine two prize cases as described

3 See chapter 3-04 in this book. (Eds.)

in the book. In addition, I will analyze two important cases of which reports by Tenhaeff himself have been published in this language.

Conversation With a Teacher

The claim (pp. 108-109 of the Bantam edition of *Croiset the Clairvoyant*): On February 21 [1951], a seven-year old child disappeared in Utrecht. Police could find no trace of him. Three days later Croiset, then living in Enschede, was telephoned by the child's schoolteacher, Miss H.M. "I have a clear picture of the child," the psychic is reported to have said, "I see military barracks and a shooting range. There is grass around it. In the grass is a small hill. There I see water also. In this water, the child fell and drowned. He is there now. His body will be found by a man in a small boat. This man wears a colored band around his cap. When you come from Enschede toward Utrecht, it is on the left side of the road."

On March 1, Tenhaeff asked Croiset whether he had more information. The clairvoyant answered without hesitation: "Yes. As I told his teacher, the child had definitely drowned in the water by Fort de Bilt (outside Utrecht). His body will soon be found." On March 5, the boy's mortal remains were discovered "precisely where Croiset had said" by a skipper of the harbor service who wore a colored band around his cap.

The claim is of some interest, as it is the only report I have been able to locate concerning a supposedly successful attempt by Croiset to solve a police case in the major town of Utrecht. Curiously, it must have happened practically under my two-year old nose, as at the appropriate time, I lived a few hundred yards from Fort de Bilt. My investigations, in 1981, soon revealed that vital bits of information are missing from Pollack's account. The same case is reported by Tenhaeff in the Dutch *Tijdschrift voor Parapsychologie* (Vol. 19, 1951, p. 199). This report mentions a few relevant details that are lacking in *Croiset the Clairvoyant*. First: the schoolteacher, Miss H.M., *knew Croiset well*. Second: before the phonecall on February 24, Miss H.M. had already called Croiset *twice*. On February 22, the clairvoyant had told her that "*there is no reason to worry*." He had added that he would be ready to go and search for the child in case he hadn't surfaced by next Saturday. On Friday night, Mrs. Croiset told Miss H.M. that her husband, who was sleeping, now was "less optimistic." He had "the impression that the boy was no longer alive." Tenhaeff's account of the telephone conversations of February 24 and March 1 essentially confirms Pollack's.

There is something odd about his story. If – as is claimed – Croiset on February 24 knew exactly where the boy was at that moment, then why was the dismal discovery not

made until March 5? Is it conceivable that, for ten days, no one would have searched the location indicated by the psychic? Strangely enough, neither Pollack's report nor Tenhaeff's mentions any attempt on the part of either the schoolteacher or the Professor to relay this information to the police.

On September 2, 1981, the Vice Superintendent of the Utrecht police wrote me to say that the department's files do not go back as far as 1951. Thanks to Dr. F. Brink,⁴ I was able to contact Mr. Wielinga, a retired police officer who, in February and March 1951, was on duty in Utrecht. Mr. Wielinga distinctly remembered the tragic incident. He did not remember that Croiset or any other psychic⁵ had furnished useful information to the authorities. He strongly doubted the story.

A search through contemporary newspaper files dissolved whatever mystery may have remained to this point. Both Pollack and Tenhaeff fail to mention the important fact that the victim, Appie Verbeek, lived in the Gildstraat *in the immediate vicinity of Fort de Bilt*, one of several military installations in the eastern part of Utrecht. Shortly before disappearing, the boy had been seen walking in a nearby street. In the area there is a canal, known as the Biltse Grift, which runs from the Griftpark to De Bilt, passing the barracks of the Fort. When a seven-year old child disappears and does not return for several days, the odds are that he is dead and that his remains are not far from where he was last seen. Any location in the close vicinity of the Gildstraat would also be in the close vicinity of Fort de Bilt. As for Pollack's claim that the body was found "precisely where Croiset had said": the body was *not* found in the waters by Fort de Bilt but in the Biltse Grift next to the Museum Bridge, inside Utrecht, about half-way between the Gildstraat and the Fort. Nearer-by are several highly visible landmarks such as a graveyard, a rotunda, a park and two palace-like buildings. A "precise" description would have included the elements "bridge" and "graveyard," *not* the Fort, which is outside town.

I do not know whether the skipper who found the body wore a cap with a colored band around it. The newspaper reports do not mention this detail and *neither does Tenhaeff*. It seems unlikely that the Professor would have accidentally overlooked this "hit." Suffice it to say that caps with colored bands are far from rare in Holland. To summarize: Croiset, when consulted by a person he knew well, first said that the child was

4 Author of *Enige aspecten van de paragnosie in het Nederlandse Strafproces*, a critical work on psychic detection. Utrecht: Drukkerij Storm, 1958.

5 From contemporary newspaper accounts it appears that several clairvoyants and dowsers attempted to shed light on this case. None was successful, although Cor Heilijgers (Heilijgers, 1976), in his autobiography *Mijn Dubbele Leven* [My Double Life] claims to have had a quite accurate vision which, alas, was supplanted by a second vision which was wrong.

alive. He changed his mind only when the boy did not return after a couple of days and the police told the press that an accident was likely. He later mentioned a landmark in the immediate vicinity of where the child had lived. Ten days later the body was found at a different location. It is not entirely clear to me how this case can honestly be presented as an example of successful psychic detection. It is important to note that Tenhaeff saw and approved the manuscript of *Croiset the Clairvoyant*.

A German Child Disappears

Summary of Pollack's account (pp. 113-115 of *Croiset the Clairvoyant*): Late in 1957 five-year old Bernard Schlegel from Buxtehude, Germany, vanished. The police were inclined to think that the child had been kidnapped and possibly murdered. In any case, there was a "general belief that the child had not drowned." Dr. Hans Bender, parapsychologist at Freiburg University, suggested that Croiset was consulted. In co-operation with the police Heinz Metzger, journalist with the *Hamburger Abendblatt*, visited Croiset in Holland in late January. The psychic "had heard nothing of the boy's disappearance" yet knew immediately what the reporter had come for. Croiset is quoted to have said: "This child has something to do with a kiosk (a sort of magazine stand with open slides and a roof, usually of canvas). I see a shop in the neighborhood. It has a striped awning with a tear on the lower right side ... The child is dead. I have no doubt. The child must have drowned." Herr Metzger told him that the "Oste river" which runs through Buxtehude, had been dragged but that nothing had been found. Croiset then described a factory, drew a sketch and stated that the body was lying about 400 meters "behind the factory." The police would be able to find him, but it would take a lot of time.

About three weeks later, the body was found in the "Oste river," "near the factory Croiset had described and corresponding to his sketch." Pollack concludes: "So once again, on a case he had known nothing about, Gerard Croiset's paranormal pictures led to the discovery of the body, the more remarkable in the face of a general belief that the child had not drowned. The German police's faith in the Dutch sensitive's powers became stronger when they checked his impressions and found them correct. One detail that deeply impressed them was Croiset's specific image of the striped awning, torn on the lower left side."

In 1981 I collected a considerable amount of information concerning this case. Some of this was given in confidence, but what I am at liberty to make public is sufficient to demonstrate that Pollack's report is misleading in the extreme.

The report suggests that there was an "official" element in Croiset's performance as the psychic was consulted on the advice of Dr. Bender and in co-operation with the police.

In fact, Dr. Bender has stated that he heard of the case only afterwards. And in a letter to DEGESA (The German Society against Superstition) dated February 18, 1958, the Landeskriminalpolizei points out that the only witness of the consultation in Utrecht had been Herr Metzger. The police “could not confirm whether Mr. Croiset’s statements were correct and how they were arrived at.”

Heinz Metzger, a crime reporter, had covered the case of Bernard Schlegel from the beginning. In an article in *Hamburger Abendblatt* he states: “I told Croiset all I knew concerning the boy. I described to him all possibilities and outlined all surmises.” Only then did Croiset mention the kiosk, the journalist replying that, indeed, “the spoor had ended at the station’s kiosk” (Metzger, 1958). No tape-recording of the entire conversation exists. It would have been interesting to check in how far this was an instance of information furnished by an unwitting client being fed back as a “telepathic impression.” Herr Metzger’s statement about having previously told all he knew suggests a non-miraculous explanation. Of course, the possibility that Croiset had been informed of the case prior to Herr Metzger’s visit should not be overlooked. The boy had been missing since Christmas. Numerous articles had appeared in the German press.

Pollack’s claim about the “general belief that the child had not drowned” is simply false. The Schlegel boy lived about 50 yards from the river Este (not Oste) and the police had assumed from the start that he had fallen into the water. This is stated by Pelz (1959-1960) and is confirmed in Metzger’s report in *Hamburger Abendblatt* of January 28, 1958.

On November 16, 1981, I had a revealing telephone conversation with Herr Metzger, presently chief editor of the major Berlin daily newspaper *B. Z.* Pollack’s chief witness surprisingly turned out to be a complete skeptic as to Croiset’s clairvoyant powers. He explained the “hits” not accounted for by the possibility of prior information as the result of post factum interpretation of an ambiguous psychic reading. Finding matches between some of Croiset’s statements and actual Buxtehude locations proved easy, due to the vagueness and generality of the former.

The striped awning “with a tear on the lower right side”⁶ is a case in point. According to Pollack, Croiset had mentioned a shop. From the January 27 report in *Hamburger Abendblatt* we learn that the clairvoyant had in fact referred to a pub. Not surprisingly, there was a pub near the station. The awning, however, belonged to a near-by shop. The police had little reason to be “deeply impressed.” Awnings are a common sight in Euro-

6 At the end of the relevant chapter, Pollack mentions “the lower left side.” Presumably this was a typing error.

pean towns and many are torn at the sides. As a photograph published by Pelz⁷ clearly shows, the Buxtehude awning had tears on both sides, not just the right one. (*Hamburger Abendblatt* on January 28 published only the right half of the same picture.) The shop with the awning had played no role in the drama. The odds against chance entirely depend on whether or not it is likely that a slightly damaged awning is found somewhere in the central area of a small German town.

The worst error in Pollack's version is the claim that the body was found "near the factory Croiset had described and corresponding to his sketch." "Absolutely untrue," Herr Metzger told me on November 16. The real facts are these. Two branches of the river Este flow through Buxtehude. One is known as the "Gestaute Arm," the other as the "Umfluter." As is apparent from the original reports in *Hamburger Abendblatt*, Croiset had finally decided that the body must be lying in the "Gestaute Arm," about 400 meters behind the factory. This is where a dam closes off the branch. And this is where the police suspected that the body would be. A search had been impossible due to the fact that the water was frozen. Both Croiset and the police were wrong. Bernard Schlegel's body was found in the "Umfluter," two and a half kilometers from the factory.

To summarize: Gerard Croiset had simply confirmed what everybody had assumed from the start. His only original contribution to the solution of the case consisted of a guess that proved to be dead wrong.⁸

In Tenhaeff's Own Words

One of the few authoritative English language publications on Croiset's work as a psychic detective – apart from Pollack's book – is the article "Aid to the Police" which Tenhaeff wrote for *Tomorrow*, the "World Digest of Psychological Research and Occult Studies" published by Eileen Garrett. The article⁹ is based on a paper which Tenhaeff read

7 Pelz writes at great length on this striped awning, both in his *Kosmos* article and in an article submitted for publication in *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie* on 3-28-1958 and rejected 4-10-1958.

8 Tenhaeff's own account of this case, in his article "Über die Anwendung paranormalen Fähigkeiten" (Tenhaeff, 1958, pp. 13-14) is comparatively accurate. At least he mentions the fact that the body was found far from the factory.

9 Tenhaeff (1953b). This is the second part of a double article by Bender and Tenhaeff. When, in his 1961 book *Hellseher, Scharlatane, Demagogen*, Wilhelm Gubisch (1961) criticized Bender for some remarks made in the first part of the article, the German parapsychologist [and professor of philosophy at the University of Munich (Eds.)] Anton Neuhäusler (Neuhäusler, 1964, pp. 102 & 113) claimed that the article in *Tomorrow* had not been written by Bender and

at the Parapsychology Foundation's First International Conference on parapsychological Studies held at Utrecht State University in the summer of 1953. This was an important occasion for Tenhaeff, for it provided him with one of the first opportunities to inform his colleagues of the results of his work with his favorite sensitive.

He begins by pointing out that the consultation of clairvoyants by the police "is a more complex affair than many an uninitiated may assume." Often, the information provided by such psychics did not advance police investigation as such, but still "proved interesting in terms of parapsychological research." "Nevertheless, some cases can be cited where the contribution of Mr. Croiset was of concrete use to the police and the courts of law." Of the examples he then describes, three stand out because 1) they concern attempts to solve crimes by ESP, 2) they seem fairly striking, and 3) they are reported in sufficient detail to enable the critical investigator at least to identify the incidents to which they relate.

One of these cases will not be dealt with in this article. It is the celebrated affair of the Wierden Hammer Assault, discussed by Hansel (Hansel, 1966, pp. 197-203; Hansel, 1980, pp. 262-268). After reading Pollack's account of this case, Hansel made inquiries with the Wierden authorities and was told that Croiset's efforts had been of no use to the police. A complete analysis of this case and of the controversy surrounding it would require far too much space but may be published separately in the future. Suffice it to say at this point that the ESP hypothesis is not supported by the facts.

The other two cases, however, have never before been the subject of critical examination.

The Coffee Smugglers

A summary of the account in the *Tomorrow* article (pp. 13-14): On April 11, 1953, a Mr. A. M. Den Hollander, an official of the Customs' Department at Enschede, had provided Tenhaeff with an extensive report of a meeting with Croiset on the previous November 10. Den Hollander had showed the psychic the photograph of a man whom he suspected of fraudulent dealings in coffee. "Mr. Croiset did not know the man, nor did the official volunteer any information," we are assured. The clairvoyant made a number of statements about the suspect, almost all of which were correct. Remarkably, Croiset told Den

Tenhaeff. According to Neuhäusler, it was a condensation of the papers read at the Utrecht conference, made by a journalist and without the authors' fiat. *Tomorrow* clearly mentions Bender and Tenhaeff as the authors. To avoid any possible misunderstanding, I consulted the stenciled Proceedings of the 1953 conference, a copy of which is kept at the Royal Library in The Hague. As far as the two cases dealt with in the previous case are concerned, the versions in *Tomorrow* and in the Proceedings are identical.

Hollander about some details that were at that time unknown to the police but which were subsequently verified. An example: “The coffee has not disappeared across smugglers’ trails, but normally through the custom’s barriers,” Croiset had said. Tenhaeff quotes Den Hollander’s comments: “Unknown during consultation. Afterwards, it was discovered that part of the coffee went over the border through the barriers. The coffee had been hidden in a limousine.”

From a number of details in Tenhaeff’s report, the case can be positively identified as the smuggling affair in which a Mr. G. Hasperhoven, director of a coffee-roasting factory in Enschede, was involved. Prior to the consultation, the case had received nationwide publicity. The name of the prime suspect (certain details in the *Tomorrow* account strongly suggest that this was the man whose photograph had served as the “inductor”) had been mentioned in the local press. Croiset, who at that time lived in Enschede, must have been aware of it. Local gossip (Enschede is a border town and was a center of smuggling in the early fifties) may very well have provided him with bits of information the authorities were not aware of. Even if we assume that Croiset had never seen the man on the photograph (How could Tenhaeff have known this, incidentally?), we must admit that he could safely have guessed that the consultation was somehow related to the smuggling affair. After all, his client was an official of the Customs’ Department! Conjectures apart, there remains something unsatisfactory in the evidence. Some essential questions are not answered. Who took the initiative in the consultation? Were Croiset’s statements recorded immediately and in full? Were there other witnesses? Pollack (1964), who describes the case on pp. 90-91 of his biography, insists that on April 11, Den Hollander *wrote* to Tenhaeff “thanking him for the invaluable help of Gerard Croiset in cracking this case; in disclosing exactly how the smuggling ring operated; and for furnishing key information that the customs department didn’t have.” Tenhaeff, however, does not mention a letter. He states that Den Hollander *told* him about the case, which suggests an oral report. The Dutch version of Den Hollander’s comments, published in *Beschouwingen* (Tenhaeff, 1957), bears the unmistakable marks of Tenhaeff’s own solemn and verbose style. If Mr. Den Hollander is still alive, I have been unable to locate him. I would have liked to ask him if he had indeed told Tenhaeff that at the time of the consultation (Nov. 10, 1952) the authorities did not yet know that the coffee had been smuggled not across smugglers’ trails, but normally through the customs’ barriers, hidden in a limousine.

I seem to notice a discrepancy with the fact that already on Monday, October 27, the Enschede newspaper *Tubantia* had mentioned the limousine and that, on November 5, the same paper had reported that the customs department had staged a reconstruction of the way the coffee had been smuggled. That report was accompanied by a photograph on which both the car and the customs barriers can be seen clearly.

The Woerden Case

On the final page of his *Tomorrow* paper, Tenhaeff relates Croiset's involvement in the solution of a spectacular crime that had occurred less than one year before the Utrecht lecture was read. This account deserves to be quoted in full.

In October 1952, a sensational attempt was made to murder a policeman on patrol in the municipality of W. The day after the news had been published in the papers, Mr. Croiset informed me that while reading the news, the image of a well-known shop in Utrecht had forced itself upon him. In this shop stage properties are sold and hired. A suit of ancient armor has stood for many years in one of the windows. The image of this suit of armor had forced itself upon him very distinctly. Besides, Mr. Croiset had the 'impression' that the guilty man had formerly worn a uniform.

Mr. Croiset suspected, on the basis of these 'impressions,' that the criminal must be somewhere in the vicinity of this shop.

Ten days after this telephone conversation, I was in the law court in Utrecht with Croiset. On the table was a parcel of objects belonging to the policeman who had been attacked. While it was still unopened, Mr. Croiset was able to inform us that there was a revolver in the parcel (which turned out to be correct). He, Mr. C., then began to communicate 'impressions' about the criminal. He was able to say, for instance, that this man liked fishing, and kept a little boat. The image of an iron eel-pot also forced itself upon him. He exclaimed:

'Now I understand the image of the armor. Such armor is made by a metal worker and that eel-pot is also made by a metal worker. This man (the criminal) is acquainted with a metal worker who has made it. It may also be that the man himself is a metal worker.'

After Mr. Croiset had communicated to those present further 'impressions' in connection with this case, the investigating judge told us that a metal worker – who possessed a small boat and an eel-pot and formerly wore a uniform – had been arrested on suspicion.

When we know that Mr. Croiset's parents were connected with the stage and that his brother Max, like his father, is a well-known reciter, we can understand why the image of the armor in the window forced itself upon him when he heard about the attempt on the policeman's life. Apparently Mr. Croiset already knew unconsciously, thanks to his psychic gifts, that a metal worker was in some way involved in the attack. Because of his interest in the stage, partly connected with youthful experience, the word metal worker was associated by him with the familiar suit of armor.

In 1957 Tenhaeff related the case again in his Dutch book *Beschouwingen over het gebruik van paragnosten* and again one year later in his German “Über die Anwendung paranormaler Fähigkeiten” and, finally, in 1960, in his English “The Employment of Paragnosts for Police Purposes.” These three versions being practically identical, I will restrict myself to referring to the English source (Tenhaeff, 1960).

There are interesting discrepancies with the 1953 *Tomorrow* version. There, Tenhaeff claims that Croiset had “seen” the uniform *before* any suspect had been arrested. In the 1960 article, however, we are told that this hit was scored ten days later – *after* the arrest had been made. In 1953, Tenhaeff creates the impression that the metal worker “seen” by Croiset had actually been involved in the assassination attempt. Surprisingly, in the 1960 account we learn that this was not the case.

“For the sake of completeness,” we read, “it should be mentioned that the arrested tinsmith had been suspected wrongly; he was set free soon after the consultation. The case can thus serve as an example of a consultation which failed from the police angle (but succeeded from the parapsychological angle). It is also of interest that Alpha (Croiset’s code-name) ‘saw’ the breastplate at a time when the sheet metal worker had not yet been arrested. One may surmise that the paragnost had obtained an impression of a future mistake on the part of the police.”

The 1960 account gives some additional details. At the consultation in the room of the law-court, Croiset had not only “seen” the revolver, but had also received an impression of “spokes.” “The presiding judge, who was present at the inquiry, said that the picture was correct. When the policeman was shot down, he not only dropped his revolver but also his bicycle. One of the wheels of the bicycle came to lie on top of the revolver.” The name of the municipality is now mentioned in full: Woerden, in the province of South Holland, not far from Utrecht.

What is implicitly denied in the 1953 account is admitted in the 1960 version: Croiset had utterly failed in his attempt to help the police solve a major crime. Yet Tenhaeff insists that the case was highly successful from the point of view of the psychical researcher. The psychic had picked the wrong man, but he had paranormally seen and described a suspect in specific detail. He had mentioned this man’s profession, his fondness of fishing and the fact that he had worn a uniform.

Striking as this may seem, it will hardly do as evidence for ESP. For Croiset had “seen” the metal worker only after the latter had been arrested. The Professor does not tell us what precautions had been taken to keep the clairvoyant from learning of this arrest by normal means. Prior to the arrest, Croiset had got no further than making vague state-

ments about a Utrecht shop and a suit of armor displayed in one of the windows. Tenhaeff is deeply impressed with the armor, which he invites us to believe, was a striking hit somewhat distorted by the unconscious processes inside Croiset's brain. But, of course, given this freedom to indulge in post factum "interpretations," *any* psychic reading can be made to fit *any* conceivable suspect. The "impressions" would have been at least as apposite if, for example, the suspect had been a soldier or someone somehow connected with the stage, if he had lived near the street mentioned by Croiset, or near the statue of a man wearing a mediaeval suit of armor. No doubt Tenhaeff would have hailed a remarkable hit if the suspect's name had been "Smit" ("smith") or "De Ridder" ("knight"), both very common names in Holland. And this by no means exhausts the supply of possible "matches."¹⁰

So even if we accept Tenhaeff's 1960 version of the facts the case is unconvincing. However, worse is to come. It will be recalled that, in his *Tomorrow* report, Tenhaeff spoke of a "sensational" crime. This caused me to wonder whether it might be worth the trouble to search the newspaper files for information relevant to the present inquiry. My visit to the archives of *De Telegraaf* proved highly rewarding.

The assassination attempt, so I learned, took place not in October but in November 1952, in the early morning of Friday 14. The victim, policeman Van Eck of Woerden, died before he arrived at the hospital. That same morning, *De Telegraaf* carried the story prominently. That report mentions the fact that Mr. Van Eck was riding a bicycle when he was shot. To the critical reader, this may suggest a possible non-paranormal source for Croiset's "impression" of "spokes," received ten days later. (The "vision" of the revolver is hardly more striking. Apart from the possibility that the shape of the parcel may have inspired Mr. Croiset, I must point to the fact that the policeman had been on his way to a burglary alarm and so had been armed as a matter of course.)

In all his published accounts, Tenhaeff states explicitly that Croiset received his "impressions" of a "metal worker" who was fond of fishing at a séance that took place *ten days* after he had phoned his mentor. The phone call had been "on the day after the news had been published in the newspapers," so the consultation in the court room has to be dated Tuesday, November 25.

De Telegraaf confirms that a metal worker was arrested. However, this metal worker was not the one who had "formerly worn a uniform." As it happened, there were two suspects. One of these was the 36 year old metal worker K. V.; the other one 31 year old

10 Similar examples of psychoanalytical acrobatics are found in Tenhaeff's often hilarious reports on the celebrated "chair tests."

D. van H., a civil servant who had formerly been a member of the Woerden police. The two men, who were both said to be poachers, had gone out together on the night of the murder. Both later proved to be entirely innocent.

The crucial fact is that the arrest of the two suspects took place on Wednesday, November 19, and was reported in the national daily newspapers on the 20th. On that day, *De Telegraaf* published the initials of the two men, mentioned their professions and former professions and did not neglect to remark on their fondness of fishing!

The details Croiset paranormally perceived during the consultation in the court room *had all been published in the papers five days previously*. By entirely suppressing this essential bit of information, Tenhaeff was able to present this non-event as a convincing example of extrasensory perception.

Conclusion

A critical and detailed examination of four cases of psychic detection has led to the discovery of glaring flaws in the published evidence. It is of utmost importance to note that these were prize cases involving one of the best known occult sleuths in history and reported either directly by or under the supervision of “a noted world figure in parapsychology.” As the motto of his book, Mr. Pollack had chosen Charles Richet’s celebrated dictum: “I will not say that it is possible. I only say that it is true.” As far as the four prize cases analyzed in this article are concerned I prefer to say: “*Je ne dirai pas que cela est impossible. Je dis seulement que ce n’est pas vrai* [I will not say that it is impossible. I only say that it is not true].”

CHAPTER 3-03

Editorial Introduction

This third and final part of “The Mystery Men from Holland” series is devoted to the colorful career of Dutch “paragnost” Marinus Dykshoorn. Less famous internationally (and even nationally, it must be said) than his well-known countrymen Hurkos and Croiset, his reported gifts as a “psychic detective” resembled theirs in many ways. This concluding part of the series was published in Zetetic Scholar, No. 10, 1982, pp. 7-16. (Please also note the editorial postscript.) (Eds.)

THE MYSTERY MEN FROM HOLLAND, III: THE MAN WHOSE PASSPORT SAYS CLAIRVOYANT

He has solved some extremely complex crimes, has located graves that have been ‘lost’ since 1917, foretold a great many events that defied probability, and once tracked a thief in a distant country by telephone. His fame is solidly established in his native Holland and in a number of European countries. He has actually been licensed by the Dutch government authorities as a ‘practitioner of the psychic arts.’

Thus, in his 1974 *Crime and the Occult*, Paul Tabori summarizes the extraordinary career of Marinus Bernardus Dykshoorn, the man whose passport bears the entry “Occupation: Clairvoyant.”

Unlike his famous countryman Peter Hurkos, who received his clairvoyant abilities as a result of an accident, Marinus Dykshoorn was “born psychic.” This happened in 1920 in the little town of ’s-Gravenzande near The Hague. Young Marinus was troubled by his unusual gift, the nature of which was a mystery to him and his community. The word “surprise” had no meaning for him. He would know beforehand what his parents would buy him for Christmas. He would often be punished for “eavesdropping” because he knew things he was not supposed to know.

He occasionally caused great embarrassment to his parents by revealing intimate information about visitors to the Dykshoorn house. He was a living lie-detector. “...I could not understand why anyone would say something that was patently not true. Surely everyone else realized there was no truth in what was being said?” he later recalled in his autobiography. The turning point in his life came in 1938 when a German scientist diagnosed his deviation as ESP.

Soon after, Dykshoorn decided to become a professional clairvoyant. He practiced in his native Holland until 1960, when he moved to Australia. The Australian episode was a frustrating one. The local police were strongly prejudiced against psychics and refused to even listen when Dykshoorn wanted to tell them where they could find the body of their Prime Minister who in 1967 had disappeared while swimming in the sea. “Mr. Holt’s body, of course, was never found,” the sensitive regretfully records in his memoirs. In 1970 Dykshoorn moved to the far more hospitable shores of the United States where he has become something of a celebrity.

In this article, I will restrict myself to Marinus Dykshoorn’s pre-1960 exploits. As in the earlier articles on Peter Hurkos and Gerard Croiset,¹ I will critically examine a number of prize cases as they have been published in English. “For the rest, the reader will have to believe that a few represent the many,” to borrow Mr. Dykshoorn’s own words.

The Sources

The principal source on Dykshoorn is the autobiography *My Passport Says Clairvoyant* (“As Told to Russell H. Felton”) (Dykshoorn, 1974). There are sections on the psychic in Tabori’s *Crime and the Occult* (Tabori, 1974, pp. 143-145) and in Laile E. Bartlett’s recent *Psi Trek* (Bartlett, 1981, pp. 76-81). Of considerable interest is an extensive feature article by Dan Greenburg in the February 1976 issue of *Playboy*, entitled “I Don’t Make Hocus-Pocus,” based on a lengthy interview with the sensitive.² After reading (“in a single sitting”) *My Passport Says Clairvoyant*, Dr. Gertrude Schmeidler wrote: “It is a fascinating account of almost unbelievable successes in tracking criminals, finding buried treasure, and similar clairvoyant or even precognitive feats.”³ “Almost unbelievable” claims

1 See chapters 3-01 and 3-02 in this book. (Eds.)

2 Greenburg (1976). Marinus Dykshoorn is best known for his supposed proficiency in psychic detection and private psychic counseling. His autobiography is almost exclusively concerned with successes in these fields. I first learned from the *Playboy* interview that Mr. Dykshoorn also claims to be a Dutch Uri Geller. He reportedly told the interviewer that he had psychokinetically stopped clocks “hundreds of times in laboratories,” that computers get upset “when I get very cranky” and that “everybody around me gets sick, really” when he is in a bad mood. Apparently, one is well advised never to pick a quarrel with Mr. Dykshoorn. A big Australian fellow who threatened to give the psychic “one good lick” next moment found himself lying on the ground, paralyzed. “Did you touch him at all?” Mr. Greenburg asked. “No,” said the clairvoyant. Unfortunately, the “hundreds of times” Mr. Dykshoorn worked PK miracles under laboratory conditions do not seem to have resulted in a commensurate number of scientific reports.

3 Quoted on cover of Dykshoorn (1974).

demand almost unbelievably strong evidence. In the light of this criterion, how does the case for Marinus B. Dykshoorn stand?

The blurb of the Dutch version of *My Passport*⁴ guarantees that “all claims in this fascinating book can be checked.” Unfortunately, when I actually tried to check some of the claims with Mr. Dykshoorn himself, the latter declined to cooperate. No reply was received to a letter sent to him on May 6, 1982. On July 14, I phoned the psychic at his New York office. He flatly refused to give me any of the information requested. He suspected that I wanted to filch from him material he was going to use in a second book. I pointed out to him that I merely wished to check some claims made in the first book, but to no avail. In spite of his claimed proficiency at “Long Distance ESP” (see section below), Mr. Dykshoorn repeatedly asked about my personal background.

Local Game

According to Tabori, Dykshoorn’s “fame is solidly established in his native Holland.” The clairvoyant himself conveys the same impression when, on p. 16 of his autobiography, he states that his work was “public knowledge” and that he was “accepted by the press, the police, the public, and finally the government...” There are no objective and unanimously agreed-upon criteria for celebrity, so I could not possibly disprove such claims. However, while Peter Hurkos and Gerard Croiset are household words in the Netherlands, questions about Marinus B. Dykshoorn are liable to be greeted with the counter-query “Marinus Who?” Having spent months in attempting to track the psychic’s record in this country, I know what I am talking about. Dykshoorn claims that most of his work for the Dutch authorities was done in strictest confidence. If so, the Dutch authorities must be commended for knowing how to keep a secret. Neither the files of *De Telegraaf* nor the invaluable private archives of the late Mr. Ph. B. Ottervanger in Bussum contained more than a handful of clippings relating to the man whose passport says clairvoyant.

Little of this press material can be said to be favorable to Dykshoorn. Consider the following story, taken from the weekly *Privé* of May 6, 1978. Young Truus van der Voort from Voorburg disappeared on June 28, 1975. About one year later her parents consulted Dykshoorn, who was then visiting his native country. This psychic took a pendulum, watched its movements, and cheerfully announced that the girl was alive and would be heard from “in three months time.” *Privé* quotes the parents as remarking bitterly that, as

4 “Translated and adapted” by Louis Rebcke, entitled *Mijn Beroep is Helderziende* (“My Profession is Clairvoyant”), Haarlem, Gottmer, 1976 (Dykshoorn, 1976). The “adaptation” has resulted in a few minor but curious discrepancies with the original English version.

late as 1978, no trace of their daughter had been found. (The body of Truus van der Voort was discovered in 1981 in a plane wreck in the Swiss Alps.)

Marinus Dykshoorn Tested by European Scientists?

“In the Netherlands and in Belgium I was tested many times at universities – among them the universities of Amsterdam, Delft and Utrecht – and was found to be a bona fide, or genuine, psychic. The researchers concluded that, although my abilities could not be explained, they could be seen to work, and I was allowed to practice as a professional clairvoyant,” Dykshoorn states on p. 16 of his autobiography.

As Mr. Dykshoorn refuses to disclose the names of the European scientists who are supposed to have tested his abilities, it is impossible to verify this claim. The parapsychological literature is curiously silent about these experiments.

The only European researcher of the paranormal named in *My Passport* is “Professor Greven, a professor for psychology and parapsychology from the University of Cologne” whom Dykshoorn met “one evening in early 1938.” Professor Greven, described as a totally blind septuagenarian, immediately recognized the Dutchman’s extraordinary gift. “He told me that I might be able to perform very valuable work, for my friends about whom I was constantly worried, and for the community. He told me that I was lucky to have been born in the Netherlands, where the attitude towards ESP was considerably more enlightened than in most countries” (p. 28). Professor Greven boldly predicted that the young sensitive would “meet skepticism and hostility.”

In order to find out more about this remarkable scientist, I consulted several reference books and made inquiries with the parapsychology institute in Freiburg im Breisgau. Strangely, no trace of a “Professor Greven, professor of psychology and parapsychology from the University of Cologne” could be found. The name is not listed in the index of *Handbook of Parapsychology* (Wolman, 1977) or any comparable work. Dipl.-Psych. Eberhard Bauer, an authority on the history of psychical research in Germany, had never heard of such a person. He kindly offered to contact Cologne University. At his request, Frau Lichtenfeld, Dekanatssekretärin of the Philosophy Department (of which the Cologne Psychology Institute forms part), consulted the complete Index of Lectures for the years 1937-1940. “Professor Greven” remained as elusive as ever. The exhaustive *Kürschners Deutscher Gelehrten-Kalender* only mentions a Dr. theol. Joseph Greven who in 1929 was appointed Professor Extraordinary at Bonn University. This Professor Greven was a theologian not a (para)psychologist. Moreover, he was not older than 56 in 1938.

Official Recognition?

According to Dykshoorn, the investigations by (anonymous) university researchers resulted in his being “allowed to practice as a professional clairvoyant.” Tabori and Greenburg claim that the psychic was “licensed” or “endorsed” by the Dutch government and had to undergo a most difficult examination before being granted this distinction. The theme of “official recognition” recurs throughout the Dykshoorn literature.

Mr. Dykshoorn has ignored my request to be shown an official document supporting the official endorsement claim. It is, to put it mildly, unlikely that such a document exists. Contrary to what seems to be widely believed abroad, the Dutch authorities have never licensed anybody as “a practitioner of the psychic arts.” The “psychic arts” are free in Holland, and there is no legal ground for “endorsing” (or, for that matter, for refusing to “endorse”) anyone who claims to be a practitioner. The official examinations mentioned by Tabori and Greenburg (and at least strongly hinted at by the psychic himself) must be the products of somebody’s fertile imagination.

Then, of course, there is the matter of the passport entry, referred to in the title of the autobiography. This is what Dykshoorn has to say about it (p. 70): “In any event, I believe that my work in this area (psychic detection) led to my claim being endorsed by the Dutch government when I was issued a passport listing my occupation as *helderziende* – ‘clairvoyant.’ As far as I know, I am the only psychic ever to have been so honored.”

In July 1982, I made inquiries with the Ministry of Foreign Affairs in The Hague, which is competent in all matters concerning passports. The official spokesman, Mr. Schutter, bluntly told me that the claim was “apekool” – rubbish. Professions are no longer listed in Dutch passports, but before new regulations went into effect Dutch citizens were entirely free to state any profession they happened to fancy. The entry in the passport does not imply any sort of “recognition” on the part of the government. Thus, if I had ever wanted *my* passport to say Clairvoyant, all I would have had to do would have been to convey this wish to the passport office clerk.

Occult Historian

Dykshoorn’s first prize case involved royalty. According to Bartlett (1981), “He reconstructed the assassination of Willem the Silent (first Prince of Orange, and founder of the Royal Dutch dynasty, no less), a murder that had taken place in 1584, over three and a half centuries before. The Director of the Prinsenhof Museum in Delft wanted to know whether Dykshoorn could fill in any of the details of the assassination, or of the particu-

lar people involved.” On pp. 43-45 of his autobiography (Dykshoorn, 1974), the psychic vividly recalls the scene: “We went into the chamber where the killing was known to have taken place, and I concentrated on the action. Immediately, I knew what had happened. Willem had been shot once in the throat, and another shot had missed. Both bullets had lodged in the stone wall. On the wall of the chamber was a small glass-fronted case protecting two neat holes from the potentially damaging fingers of sightseers. ‘These holes were originally much lower down,’ I said. The director smiled. ‘If you are a trickster,’ he said, ‘you have certainly done your homework. How else has the room changed?’ ‘The floor was much lower,’ I said. ‘This is not the original floor. The level we are on would have been at about chest height in those days.’ ‘Excellent!’ he beamed. ‘You’re absolutely right!’ But I was much more interested in testing my gift than impressing the director. I set out to reconstruct everything that had happened on that dark day for Holland in 1584. ‘Gerard really did do it,’ I said. ‘Philip had promised him instant elevation to the Spanish nobility if he succeeded...’” (Dykshoorn is referring to the assassin Balthasar Gerard and to Philip II, King of Spain, against whose tyranny the Dutch revolt had been directed – PHH)

“‘Gerard gained an appointment with Willem,’ I said, ‘to request permission to leave the country for Spain. Without such a permit he could not have escaped to collect his reward, so he waited until Willem had signed before firing the shots.’ I walked over to the wall. ‘There was a doorway here, lower down. Gerard escaped through it and hid under a dung heap outside. When the guards found him, they brought him back inside and walled him up in another chamber. They hoped to preserve him from the mob, at least until he could be tried and made to confess to having acted for Philip. But some of the crowd noticed the new brickwork. They tore down the wall and took him.’” So the hope of preserving the assassin from the mob had been in vain. On p. 43 Dykshoorn writes: “They dragged him into the open square and roped each of his limbs to a different horse. Gerard was torn apart.” The director of the Prinsenhof Museum paid Dykshoorn “on the spot” the sum of one hundred guilders for “clairvoyant services rendered.”

There is something fishy about this story. The reward was paid “on the spot” so the director could not possibly have had the opportunity to check any of the supposedly fresh information given to him by Dykshoorn. One hundred guilders was a considerable sum at that time (Dykshoorn mentions that it occurred in 1948). Is it conceivable that anybody – let alone a director of an important historical museum – would be so reckless as to pay a small fortune for an unverified psychic statement?

What Dykshoorn reports having said about the assassination would have been known to any Dutch schoolboy, except for two details: 1) that Willem of Orange had been shot in the throat and 2) that Gerard had been lynched by the mob before he could be brought

to justice. On both points, Dykshoorn was dead wrong. Willem the Silent was not hit in the throat but in the chest. The autopsy report has been preserved and can be seen in the State Archives in The Hague. Balthasar Gerard was not lynched by the mob but arrested, tried by a special Commission consisting of members of the High Court, the Court of Holland and the City Court of Delft and sentenced to death on July 14, 1584. The execution took place the same day. The horrible sentence has been preserved and can be seen at the Algemeen Rijksarchief in The Hague (3rd Dept., Archives of the States of Holland After 1572, brown cabinet no. 44.) The way Dykshoorn describes Gerard's fate vaguely suggests that he may have confused Willem's assassin with the De Witt brothers, prominent Dutch politicians who were lynched by a mob in The Hague in 1672. A Dutch schoolboy who would have made a similar mistake would have been punished by the history teacher instead of receiving a reward of a hundred guilders.

If Marinus Dykshoorn ever attempted to give a psychic demonstration at the Prinsenhof Museum the event does not seem to have made a lasting impression. Neither the present director, drs. R. A. Leeuw, nor his predecessor, drs. D. H. G. Bolten, could recall ever having heard the story. At drs. Leeuw's suggestion, I contacted the art historian Dr. Anne Berendsen who had been the custodian of the Prinsenhof since 1949. In her reply she wrote that she had never heard of Dykshoorn's alleged feat. It is unlikely that such a visit would never have been discussed afterwards," she added. According to Dr. Berendsen, the published account is "worthless."

Long Distance Clairvoyance

Writers on Dykshoorn seem to agree that the Duisburg Long Distance affair must be regarded as the psychic's chef d'oeuvre. According to Tabori, it was "Dykshoorn's most spectacular case." According to Bartlett, "solving a robbery case in Germany by telephone from Holland established Dykshoorn's international reputation." The case is the subject of a special chapter in the autobiography.

A summary of the claim: On March 25, 1958, Dykshoorn, in Breda, received a phone call from Franz-Joseph Becker, the captain of a Rhine barge. Becker was calling from Ruhrort near Duisburg in West-Germany. He reported the theft of his launch. It had been missing for two days, and the Duisburg river police had been unable to find a trace of it.

Dykshoorn replied that the boat had not been stolen, but had been set adrift by mischievous teenagers. It would be found "about two-and-a-half kilometers downstream" (says the autobiography) or "six miles upstream" (says Tabori). Becker reported Dykshoorn's statement to the police who found the launch where Dykshoorn had said

it was – wherever that was. On March 28 (says the autobiography) or “a few hours later” (says Tabori), Herr Becker called again. This time, a considerable amount of money had been stolen from the barge’s cabin. The police had been notified. In fact, they were listening on another line. Dykshoorn reported a vision of a 17-year old member of the crew of a fifteen-tonner moored near Becker’s barge. The lad had stolen the money and put it in his travel bag. He planned to leave for vacation the next morning. The captain and the police immediately set out for the ship indicated by the psychic. Sure enough, they found the 17 year old scoundrel and the travel bag full of money. After the police had confirmed to newspaper reporters that “Yes, Dykshoorn solved the case by telephone – long distance!” the story was picked up by the press and radio. Dykshoorn suddenly became a celebrity in Germany.

Critical evaluation of the claim: It is true that the affair received a certain amount of publicity at the time. I have a copy of an article that appeared in the *Frankfurter Abendpost* on May 13, 1958. This (popular) paper basically confirms Dykshoorn’s and Tabori’s accounts, except that it ignores the missing launch. From the newspaper article it would appear that Becker first contacted the Dutch psychic after the money had been stolen. The *Abendpost* has the police “smiling” at Becker’s request for permission to call Dykshoorn. The police would have had little reason for being ironical if, two days (or a few hours) previously they had been witnesses to a remarkable instance of psychic detection. However that may be, *Abendpost* has the Duisburg river police confirming the claim – which would seem the most confidence-inspiring feature of the case.

At the time, the *Abendpost* article caught the attention of the then active *Deutsche Gesellschaft Schutz vor Aberglauben* (“German Society for Protection against Superstition”), a group of (mostly) scientists strongly opposed to any sort of “occult” belief. The society made inquiries with the *Wasserschutzpolizeidirektor* [chief of the water police] von Nordrhein-Westfalen, whose reply, dated June 19, 1958, is quoted in the Society’s *Mitteilungsblatt* (No. 10, August 1958, pp. 12-13). The chief of the water police wrote: “Our inquiries have revealed that the story in the newspaper sent to us does not conform to the actual facts. In the relevant instance, the evidence against the offender was produced by normal police methods.”

From the brief note in *Mitteilungsblatt*, it is not entirely clear whether the letter from the police chief has been reproduced in its entirety. Especially as German proponents of parascience have frequently complained about what they perceived as a penchant for quoting-out-of-context on the part of the Society, an attempt at double-checking was made for the purpose of the present article. The files of the now defunct Society could not be located. In the spring of 1982 Herr Gerd H. Hövelmann of Marburg at my request

contacted the Wasserschutzpolizei, the Public Prosecutor in Duisburg and the municipal police of Duisburg-Ruhrort. His letters contained an accurate outline of the claims in Dykshoorn's and Tabori's books.

Both the city police and the Public Prosecutor denied having any information on the matter. However, in May a most helpful reply was received from Herr Kriminaloberrat Kitschenberg of the Wasserschutzpolizei. Herr Kitschenberg unequivocally denied that in 1958 his department had cooperated with any clairvoyant. After having read Herr Hövelmann's letter, he had spoken to the officer who had handled the Becker case. He had been assured that the case had been solved by normal means. This was confirmed by the documents in the police archives.

Herr Kitschenberg further wrote that the claim that the Wasserschutzpolizei had "admitted" the psychic's success is untrue. He reminded us that, at the appropriate time, a state law forbidding the police to employ clairvoyants was still in force. In a second letter, dated June 3, the Herr Kriminaloberrat added that the police files do not contain any indication that, in the Ruhrort affair, a psychic was consulted by a private party. He repeated that the extant documents show that the case was solved without paranormal assistance.

What happened in 1958? Is the claim a complete invention? Or is the police covering-up the fact, that, a quarter century ago, they disobeyed a state law by cooperating with a clairvoyant? A reconstruction of the actual events seems impossible at this time. My private guess is that a victim of a theft privately consulted Dykshoorn and was treated to the customary diffuse and ambiguous "psychic statements."

After the police had solved the case, the captain selectively remembered and subjectively validated Dykshoorn's utterances, convinced himself that the clairvoyant had scored a few remarkable hits and informed the press accordingly. In the journalistic process the account underwent further embellishments. All this is conjecture. The repeated statements from the Wasserschutzpolizei – cast in the role of chief witness in both Dykshoorn's and Tabori's reports – are clear and unambiguous. The principal claimant has refused to provide me with solid documentary evidence to the contrary.⁵

Two Further Claims

One of the most intriguing episodes in Dykshoorn's career is described on pp. 73-77 of *My Passport Says Clairvoyant* (Dykshoorn, 1974). On Tuesday, February 12, 1952, the

5 The full correspondences with the German authorities still are in Hövelmann's archives. (Eds.)

psychic underwent tests “at a Dutch provincial university that shall remain nameless here.” The anonymous researchers required him to state whether smears of blood on glass slides came from a man or a woman.

The psychic set to work, until he was given a sample of blood “from which I received a very strange psychic impression.” At first, he was surprised, then enraged. He stood up and told the scientists that he did not like being trapped. “You are playing games with a very serious subject, and I deeply resent the implication that I am merely a fraud who has never been exposed. My abilities and the way I use them are public knowledge, and until you can disprove my abilities, please do not degrade them. This blood sample has been taken from a female. A pregnant female. A pregnant female dog – or maybe a fox; I don’t know. Now if you will excuse me...”

In an angry mood, the orator went home. As he entered his Breda apartment, the telephone rang. The caller’s daughter and the daughter of a neighbor had disappeared in nearby Tilburg. Half an hour later, the man came to collect the clairvoyant. They set out for Tilburg, went to the police station, and then to the banks of the Wilhelmina Canal. “My gift led me to the exact spot from which the children had fallen into the water and then, immediately, to the body of the first child. There had been no foul play ... a few minutes later the police recovered the second tiny body.” The next morning, one of the university parapsychologists phoned to say that he “had read in the newspaper about my help in finding and recovering the children’s bodies...” and to apologize for what had happened at the laboratory. The researchers had not been aware that not all the blood samples had been human. A naughty laboratory assistant had taken blood from a pregnant fox and slipped it in among the human samples.

It is of course vaguely suspicious that Dykshoorn does not mention the name of the university where the remarkable experiment is supposed to have been conducted. He refers to “a Dutch provincial university,” but such institutions did not exist in 1952. It is curious that the amazing demonstration of ESP never seems to have been reported in the parapsychological journals.

The blood sample test also features in Tabori’s book – but in a completely different context. According to Tabori, the experiment was part of the examination Dykshoorn had had to take in order to get his government license. “A dog,” he said. He was wrong – it was a fox. But that did not prevent him from getting his license.” To complicate matters even further, the Dutch version of *My Passport Says Clairvoyant* has it that Dykshoorn correctly guessed that the blood sample came from a pregnant fox terrier – a dog owned by the parapsychologist who called to apologize on February 13. Regarding the case of the missing girls, I wrote to the Tilburg Police Superintendent on May 2, 1982. I further

contacted the Tilburg municipal archives, where a complete collection of the local newspapers is kept.

On June 18, Superintendent T. P. de Vries replied. He confirmed that on February 12 two children had drowned in the Wilhelmina Canal: a boy and a girl, both three years old. Contrary to what is suggested in the Dykshoorn autobiography, the cause of the disappearance had been clear from the start: the boy's sister had witnessed the tragic accident.

A quote from the original police report: "About 17.00 hours Mrs. van Z. (mother of one of the victims) told me (mother of the second victim) that her daughter J. had come home reporting that her little son A. and my little daughter P. had fallen into the water near the boat-house." So the location of the accident was known exactly, which throws a dubious light on Dykshoorn's claim that "my gift led me to the exact spot from which the children had fallen into the water."

The police report in no way mentions assistance from Dykshoorn or from any other psychic. It will be recalled that the enigmatic parapsychologist who apologized to Dykshoorn on the 13th had "read in the newspaper about my help in finding and recovering the children's bodies." However, the personnel of the municipal archives have ascertained that none of the local papers contained any mention of Dykshoorn's role.

It is unlikely that the parapsychologist could have read in *any* paper about the recovery of the two bodies on February 13. Both the police report and the newspaper accounts reveal that, while the girl was found late in the evening of the 12th, the body of the other victim was only recovered *four days later*. Mr. Dykshoorn's memory must have played a nasty trick on him – and on his readers.

Conclusion

Marinus B. Dykshoorn is the third of the famous Dutch "paragnosts" whose alleged feats are critically examined in this series. As with Peter Hurkos and Gerard Croiset, the successes in psychic detection ascribed to this sensitive do not bear skeptical scrutiny. As far as the claims discussed in this article are concerned, the facts flatly refuse to corroborate what Mr. Dykshoorn's passport says.

Acknowledgement

I am indebted to Mr. Gerd H. Hövelmann for his help in checking the Duisburg case.

Postscript

After the manuscript for the above article had been type-set, I accidentally discovered what must be the solution to the “Professor Greven” mystery. In the thirties there existed in The Hague an obscure “Society for Philosophy and Parapsychology” led by a Dr. E. Greven. This Dr. Greven was a Dutchman with strong Germanophile leanings. The Hague daily newspaper *Het Vaderland* on March 6, 1942, reported that Greven had been appointed Professor of Parapsychological Philosophy at Leiden University. Presumably, Professor Greven never actually lectured there, as the previous year Leiden University had been closed down by the Nazi invaders after the outcry of professors and students against the dismissal of the Jewish personnel. According to Mr. George Zorab (who in the *European Journal of Parapsychology*, Vol. 1, No. 3, November 1976, erroneously stated that Greven was appointed already in 1940 [Zorab, 1976]), the appointment had been a personal favor from the Nazi Governor, the notorious Dr. Seyss-Inquart. Greven acquired the status of “Professor” only as a result of the unusual political circumstances of the time. After the war (if he survived at all), he immediately relapsed into obscurity. It is quite possible that Mr. Dykshoorn some time during the thirties met this gentleman.

Editorial Postscript 2009

A quarter of a century after Piet Hein Hoebens, with only partial success, searched for traces of the once-elusive Professor E. Greven, his fellow Dutchman drs. Wim Kramer (2006) was more successful and managed to unearth much additional information. Prof. Greven, as it turns out, in fact held the first Dutch professorship for parapsychology, even if only for a few years and under somewhat peculiar circumstances. He in fact survived the war. But this is only part of the story. For the full history, a truly fascinating tale of a very unusual episode in the history of Dutch (and international) parapsychology, see Kramer (2006). (Eds.)

CHAPTER 3-04

Editorial Introduction

In a two-part article for CSICOP's magazine, the Skeptical Inquirer, that ran virtually at the same time as, and in parallel to, the three-part "Mystery Men from Holland" series in the Zetetic Scholar (see chapters 3-01 to 3-03), Hoebens tried to trace additional alleged "psychic sleuth" feats of Dutch "paragnost" Gerard Croiset. In particular, he tried to demonstrate that the very key to the Croiset mystery lay with his mentor, Prof. Wilhelm Tenhaeff, and his intricate reporting policy rather than with the psychic himself.

Arguably, both parts of this article, the first one of which is reprinted below, were much more influential – at least more frequently cited – than the contemporaneous Zetetic Scholar series. To a certain extent this may have been due to the very fact that, especially in the second part of the paper, Croiset's mentor Prof. Tenhaeff, rather than the psychic himself, increasingly moved into the focus of Hoebens' critical examination. However, the main reason probably has been the fact that the Skeptical Inquirer's circulation always used to be much wider (wider by orders of magnitude, in fact) than that of the Zetetic Scholar. It thus was likely to receive far greater national and international attention. This also may be the reason why the two SI articles were more frequently reprinted, with or without permission, than any other of Hoebens' publications.

The first part of the article appeared in the Skeptical Inquirer 6 (1981), (1), 17-28. It was reprinted on pp. 122-132 of the book Science Confronts the Paranormal, edited by Kendrick Frazier (Buffalo, NY: Prometheus, 1986), and, without authorization, in Indian Skeptic, 3 (1990), (3), 9-19; a Dutch version (transl. Karel Beckman) was published, under the title "Gerard Croiset: Onderzoek naar de Sherlock Holmes onder de paragnosten", in Skepter, 1 (1988), (3), 12-17, and a Danish version formed the first part of the little booklet En psykisk detektiv: Om mirakelmanden Gerard Croiset (transl. Gerda Volf, introduction Willy Wegner). Hjallerup: Skeptica skriftserie, 1986. (Eds.)

GERARD CROISSET: INVESTIGATION OF THE MOZART OF "PSYCHIC SLEUTHS"

Critical examination of the evidence surrounding the cases of supposed crime-solving by the celebrated Dutch "clairvoyant" finds extraordinary differences between the claims and the facts.

The Dutchman Gerard Croiset, who died unexpectedly in July 1980, was undoubtedly one of the psychic superstars of the twentieth century. His mentor, Professor Wilhelm Tenhaeff, has called him the clairvoyant equivalent of Mozart or Beethoven. Tenhaeff's German colleague, Professor Hans Bender, recently admitted that Croiset had been instrumental in transforming his belief in ESP into "an unshakable conviction." The obituaries published in the European press reflected the sensitive's unique reputation. According to the Amsterdam weekly *Elsevier*, the deceased had heralded a "new awareness of cosmic solidarity." The German parascientific monthly *Esotera* ran a cover story lamenting the death of "the clairvoyant who never disappointed" (Tenhaeff, 1980a, 1980b). A professor from the papal university delivered the funeral oration. Croiset's career in the supernatural has been distinguished indeed. According to his biographers, he has solved some of the century's most baffling crimes, traced countless lost objects, and located hundreds of missing persons. His paranormal healing powers are said to have been on the Caycean level. He "excelled" at precognition and is credited with having accurately foretold future events on numerous occasions. Most of his remarkable feats, it is said, were performed under scientific supervision, which supposedly would make Croiset one of the most thoroughly tested sensitives since Mrs. Piper.

Gerard Croiset was respectable. Many educated Dutchmen who profess disbelief in ESP have managed to hold the simultaneous conviction that Croiset, for one, was genuine. This miracle man is the subject of a full-length biography by American journalist Jack Harrison Pollack (1964), who claims to have spent five years checking and double-checking the psychic's record. Pollack's verdict: "Unbelievable, but true." Unbelievable, indeed. But true?

Psychic Detectives

The practical achievements of Gerard Croiset and other sensitives who claim to assist the police share most of the features of "spontaneous cases."¹ Such cases typically occur under uncontrolled conditions and are by their very nature unrepeatable. This means that the only evidence we have usually consists of whatever witnesses are able to remember or care to report. Before reaching a verdict, the critical investigator has to address two crucial questions:

1. Are the reports free of omissions, errors, and deliberate distortions?
2. Does whatever remains after the first question has been answered admit no more plausible an explanation than ESP?

1 This article and the one to follow are exclusively concerned with Croiset's activities as a paranormal sleuth. In that role he became best known abroad. About the experiments with him I will have more to say later. [See chapters 3-07 and 3-11 in this book. (Eds.)]

The Sources

Studying Croiset has been the virtual monopoly of Wilhelm Heinrich Carl Tenhaeff, the Dutch parapsychologist who in 1953 was appointed to the first chair of psychical research ever to be established at a regular university (Utrecht). Tenhaeff's books and articles (Tenhaeff, 1953b, 1955, 1957, 1960, 1979b, 1980a, 1980b) constitute the principal source of information on Croiset, whose case may be said to stand or fall with the reliability of his learned mentor.

Unfortunately, little of Tenhaeff's work has been translated into English, which leaves Pollack's *Croiset the Clairvoyant* (Pollack, 1964) as the main reference in this language. Pollack is a journalist, not a scholar. Yet his biography may be regarded as an authoritative document, since it was written under personal guidance of Tenhaeff himself. "He indefatigably double-checked the facts in my manuscript," Pollack states in his acknowledgement.

In Tenhaeff's *Tijdschrift voor Parapsychologie* (the official journal of the Dutch Society for Psychical Research), the professor has proudly confirmed this (Tenhaeff, 1955). According to Tenhaeff, *Croiset the Clairvoyant* "was written on the basis of information which I supplied and also under my supervision."

Police Records

According to Pollack (1964), Croiset won plaudits not just from parapsychologists but from policemen all over the world for his achievements in psychic detection. "I checked documents in case after case in police records," the biographer assures us. I am not quite certain what he means. Most of the documents he refers to must have been in Dutch, and I doubt that he ever familiarized himself with the language. The only Dutch expression I found in the book is the equivalent of "thank you," and even that solitary example contains an error. Presumably, Pollack relied on summaries or translations of the relevant documentation, prepared for him by Tenhaeff and other acquaintances in the Netherlands. He must have felt it was quite safe to do so. After all, his material would be double-checked by a distinguished scholar, a professor at a state university, a pioneer whom the American psychiatrist-parapsychologist Dr. Berthold Eric Schwartz had compared to Copernicus, Freud, and Einstein.

The Boy on the Raft

It is time to take a closer look at one of Croiset's most impressive successes. It is the "Boy on Raft" case, and will be found on pages 106 and 107 of the Bantam paperback edition of *Croiset the Clairvoyant*. This case has often been mentioned in the psi literature, and Tenhaeff himself has indicated more than once that it is one of the classics. This is how Pollack reports it (*italics added*):

Ten year old Dirk Zwenne left his home in the dunes city of Velsen near the North Sea canal on Saturday, August 29, 1953, at about two P.M. to play. When the boy had not returned home by early evening, his parents began to grow uneasy. They telephoned the local police without success. When no trace of the missing boy had been found in two days, Dirk's uncle telephoned Croiset, whose phone number and address in Enschede, 115 miles away, had been given to him by a police superintendent. Among the clairvoyant's *immediate images* was that Dirk had drowned: "I see a *small harbor*, a *small raft* and a little sailboat. The boy was playing on the raft. He slipped and fell into the water. As he fell, his head struck the sailboat and he received *an injury on the left side of his head*. I am very sorry. There was a strong current in the harbor. *The boy's body will be found in a few days in another small harbor which is connected with the first harbor.*" Unhappily, five days after he had disappeared, the body of Dirk Zwenne was found in this second harbor. And, *just as Croiset had seen*, the boy had a wound on the left side of his head. The raft and small sailboat were recovered in the first harbor – again *just as the sensitive had described*. "It is very likely that everything had happened as the paragnost had seen it," summarized Professor Tenhaeff.

This seems a striking case indeed. Oddly enough, until now nobody seems to have thought of comparing this version with a letter that was published in *Tijdschrift voor Parapsychologie* in 1955 (vol. 23, no. 1/2). It was written by Mr. A.J. Allan, the uncle who had consulted Croiset. From this report (embedded in an article by Tenhaeff, 1955) we get an idea of what really happened. On Monday, August 31, Mr. Allan phoned the clairvoyant, who was at that time living in the eastern Dutch town of Enschede. He acted on the advice of Haarlem police superintendent Gorter, who happened to be the second secretary of the Dutch Society for Psychical Research (SPR) and an acquaintance of both Tenhaeff and Croiset.

The sensitive, after having made clear that he knew what the call was about, told the uncle: "You must look near a *gasholder*."

Allan: "A gasholder?"

Croiset: "Yes. It might be a tank or a boiler or something like that. I see a road and a small ditch. I also see a small bridge and a small water. Do I speak to the boy's father?"

Allan: "No, you are speaking to an uncle."

Croiset: "All right, I can speak freely. The child has drowned. He is dead. I also see a jetty and a rowing boat or something like that. That's where the body must be."

Allan: "Could it be the North Sea canal?"

Croiset: "No, that is too broad. I don't see so much water."

Allan: "Then where is it?"

Croiset: I don't know Velsen, but you have to look near that gasholder or tank. It is to the right of it. *To know for sure I ought to come to Velsen. Call me again if that's necessary.*"

End of conversation.

Holland being a country full of roads, ditches, small bridges, small waters, jetties, rowboats, and objects that could be described as gasholders, tanks, boilers, or "something like that," Croiset's impressions had hardly been specific. His description could apply to any number of locations.

According to Mr. Allan, the police, "after having considered several possibilities," decided that Croiset must have "seen" a small harbor near a water purification plant. This is a rather surprising interpretation, as that "harbor" (really a recess) is *part of the North Sea canal*. The psychic had been specific on only one point: the water was *not* the North Sea canal. To me, this strongly suggests that the police had reasons of their own to regard the small harbor as a likely place.

The police decided to drag the harbor the next day. On Tuesday, they heard that Dirk, shortly before disappearing, had told one of his friends about "having found a nice raft."

Croiset, who was phoned again later that day, *now started to receive impressions of a raft also*. Mr. Allan suggests that this was due to telepathy, but the skeptical reader may be able to think of a more naturalistic explanation.

Nothing was found in the small harbor, and the next day Allan asked Croiset to come to Velsen. The clairvoyant arrived that same evening, in the company of Tenhaeff.

The psychic was taken to the small harbor, and there he started to get "strong emotions." He stated that the boy had been playing with his raft, had lost his balance, and had bumped his head on a hard object. "According to him [Croiset] *this had been fatal*," Allan notes. Croiset predicted the body would *not be found before Monday, September 7, or Tuesday the 8th*, and would show an injury "*on the left side of the forehead*." The clairvoyant was then taken to a second small harbor that also forms part of the North Sea canal. *There, however, he felt "no emotions."*

The next morning, Thursday, September 3, the body of Dirk Zwenne was found in the canal near the entrance of the second harbor. The head showed bruises, *but not at the*

location Croiset had indicated. Where and in what circumstances the boy had fallen into the water appears never to have been ascertained.

Now please compare this long and tedious story with Pollack's "summary" and be surprised at the magical metamorphosis an entirely unspectacular event has undergone in the process of summarizing. Croiset's impressions had been vague and for the most part wide of the mark, and yet this case is cited as a classic instance of successful psychic detection.

How could this fantastic distortion ever have survived the checking and double-checking by an experienced American journalist and a distinguished university professor? The answer to this question may contain the key to much of the Croiset mystery. For it was Tenhaeff himself who concocted the fake version. Having published Allan's account in his *Tijdschrift*, destined for the home market, the professor prepared a special version for export.

All Pollack had to do was to paraphrase the version Tenhaeff had already published in German (Tenhaeff, 1958) and in English (Tenhaeff, 1960). This latter version reads as follows:

When no trace of the child had been found by 31st August an uncle of the missing child rang up Mr. Alpha [Croiset's code name], whose name and address he had obtained from a police superintendent. According to the paragnost the child had drowned. Among the "pictures" which presented themselves to Mr. Alpha were a few *which concerned a small harbor*. In this small harbor he "saw" a *small raft* and a *little sailing boat*. According to the paragnost the child had been playing on the raft. He supposed him while at play to have slipped and to have fallen into the water. In doing so he appeared to have incurred a *wound on the left side of his head* where he struck the sailing boat as he fell. *In consequence of a current in the harbor, so the paragnost said, the body would be found in another small harbor, which was connected with the first. On 3rd September, just as the paragnost had "seen," the body of Dirk Zwenne was in fact found in the second harbor with a wound on the left side of his head.* [Italics added.]

On all essential points, this version is identical to Pollack's. In this form, the "Boy on Raft" case has become the "believer's favorite." It was featured in the cover story devoted to Croiset in the September 1979 *Holland Herald* (an English-language magazine mainly concerned with "selling" the Netherlands) and found its way into Ryzl's *Parapsychology: A Scientific Approach* (Ryzl, 1970) and numerous other publications. It is clear that in this case, Professor Tenhaeff "cooked the books." His probable reasons for doing so will be discussed later.

Pollack as a Witness

Jack Harrison Pollack (1964) can hardly be blamed for the serious errors in his report of the "Boy on Raft" case. A journalist may be forgiven for accepting a university professor's word, but he must be held responsible for his reports of what he claims he personally witnessed.

In *This Week* of February 19, 1961 (a slightly elaborated version will be found on pp. 25-26 of the paperback edition of the biography), Pollack recalled being present when, on May 21, 1960, Croiset was phoned by a neighbor of an Eindhoven family whose four year old son had been missing for 24 hours. According to the article, the police "*had no clues.*"

"The outlook isn't good," Croiset is quoted as saying. "Search the area immediately. But I'm afraid in about three days the child's body will be found in the canal close to the bridge."

Pollack continues: "Three days later, I checked up. The police of Eindhoven had just found the child's body next to one of the piers of the bridge over the canal – exactly as Croiset had predicted."

Something seems to have gone wrong when Pollack checked up. In 1981, I made inquiries with the Eindhoven police. Mr. W. Jongsma of the Information Office kindly offered to check the original police report. These are the real facts: The victim, three year old Anthonius Thoonen, while playing with a friend, fell into the Dommel river on May 20. The accident was witnessed by the other boy, who told Anthonius' mother about it when she came looking for him. Mrs. Thoonen saw something floating on the water. Presumably, this was the body. It had disappeared when the police arrived. On May 23 (two days after the telephone conversation), Anthonius' remains were found in the river, near the Gestel playground.

The police report does not mention Croiset. Neither does it mention a bridge. (There are so many bridges over the Dommel that there is always one nearby.)

The authorities from the very beginning knew that the boy had drowned in the river. Pollack's claim that the police "had no clues" is utterly misleading. No one needed a clairvoyant to say that "the outlook isn't good" or that the area should be searched immediately. Yet, by overlooking some crucial facts, Pollack is able to present this case as "an amazing demonstration."

Search for a Child

Pollack's book (Pollack, 1964) and numerous other English-language publications convey the impression that psychics are employed as a matter of course in Dutch police investigations. Some journalists seem to think that a special hot-line connects Tenhaeff's office with police headquarters in every major town. Perhaps the language barrier may have been responsible for this exaggeration. In fact, Dutch police authorities tend to be skeptical of clairvoyants.² Their typical reply to questions about Croiset is something to the effect that ESP may exist but Croiset was never of any use to them. However, there are a few exceptions.

Notable among the exceptions is a report by Inspector G.D.H. van Woudenberg, published in *Algemeen Politieblad* (Woudenberg, 1964). Van Woudenberg, at that time serving with the Voorburg police, relates an apparent success Croiset achieved in searching for the body of six year old Wim Slee. The child was reported missing on April 11, 1963. A thorough search was organized the same day. A police dog led the way to a certain spot on the bank of a canal locally known as De Vliet. There were good reasons to assume the dog was right, as it was known that Wim often went there to play. No body was found, however. The next day the case was mentioned in the press and on radio and television. A number of psychics volunteered with perfectly worthless information. In the meantime, an uncle had rung Croiset's phone number, to be told that the psychic had gone abroad. The uncle did not get through to Croiset until the 16th. The clairvoyant then told him that the boy had drowned in De Vliet. The body would surface in a couple of days near a bridge, a sluice, "or something like that," to the left of the spot where the accident had happened. Croiset asked to be called back in case the child had not been found by Friday the 19th.

That Friday, with still no trace of Wim Slee, Croiset came to Voorburg. He had with him a sketch of the location where the body had fallen into the water. He invited the police to get into his car and then drove to De Vliet. He stopped near the spot indicated by the dog and stated that he now experienced "strong emotions." Van Woudenberg (1964) noted "striking similarities" between the sketch and the actual location. Croiset said the child had drowned there but would surface on Tuesday morning near a bridge some 800 yards downstream. This was to the right of the indicated spot (as seen from Voorburg), but the clairvoyant explained that "to the right" really is the same as "to the left" if you look at it from the other side. Near the bridge, van Woudenberg continues,

² This is confirmed by a very detailed historical and systematic survey by Schouten (2002-2004). (Eds.)

"we saw there were points [on the sketch] that correspond with what the uncle had been told earlier that week."

As it happened, Wim Slee's body was found the following Tuesday near the bridge. Presumably, the remains had been tangled up in refuse on the bottom of the canal.

Unless we want to make unfounded conjectures about a possible lapse of memory on van Woudenberg's part, the verdict must be: a hit. Yet I wonder if ESP is the only plausible explanation. Croiset gave his first "impressions" on the 16th, five days after Wim Slee had been reported missing. The case had received considerable media coverage. The police suspected that the body had drowned in De Vliet and the dog had even indicated a likely spot. However, Croiset's initial "images" were vague, and he did not specify what bridge, sluice, or "something like that" he meant.

His description (as reported by Woudenberg [1964]) would fit a good many bridge-like structures. He stated that the body had floated "to the left" but did not say from what vantage point. Moreover, from his request to be called back in case the body was still missing on Friday the 19th, we may surmise that he expected that the boy would have been found by that date. Friday the 19th would have been eight days after Wim Slee disappeared, and van Woudenberg tells us that "most bodies come to the surface in a maximum of nine days."

Croiset scored a hit only when he tried again. The accuracy of the sketch he showed on the 19th is not surprising. He had simply drawn the area where, according to the police dog, the child had fallen into the water. The possibility that he had obtained his information by normal means should not be ruled out. Van Woudenberg (personal communication) thinks this hypothesis somewhat unlikely, as the sketch contained a few details of the location not visible from the public road. I venture to suggest that the inspector *may* have underestimated the resourcefulness of a highly experienced psychic.

What remains is that Croiset, in his second series of "impressions," received eight days after the accident, correctly predicted both the date and the spot where the body would be found. Striking enough, but I doubt whether the odds against such a hit arising from chance alone are really astronomical.

Van Woudenberg (personal communication) is still impressed by Croiset's success, although he does not think it falls into the "conclusive evidence" category. "The weakest part of the case," he told me in February 1981, "is that it seems to be pretty unique. It happened 17 years ago and continues to be cited as possibly the best case that ever happened in Holland. One cannot help wondering why there seem to be so few comparable successes."

Failures

As an isolated case, Croiset's achievement in Voorburg is fairly impressive. However, we must guard against a common fallacy in assessing such apparently compelling "proofs." The chance hypothesis can only be ruled out if we know the hit/miss ratio in the psychic's total score. On this point, no statistics are available, but there are a number of reliable indications.

In his English-language *Proceedings* (1960) and in a number of other publications, Tenhaeff has admitted that the number of successful consultations (successful from a practical point of view) is limited. The bulk of the material in his *Beschouwingen over het gebruik van paragnosten* (1957), his major Dutch work on psychic detection, concerns cases where the psychics supposedly demonstrated ESP without actually solving any crime or finding any missing person. There are very few prize cases, and these are cited time and again. Some of these "successes," as I have shown, are striking only when the facts have carefully been doctored. In his book *Ontmoetingen met Paragnosten* (Tenhaeff, 1979b) Tenhaeff quotes Croiset as stating that he was consulted by relatives of missing persons on an average of 10 to 12 times a week. That is something like 500 times a year, and Croiset has been in the business since the forties!

All this strongly suggests that thousands of Croiset's attempts have ended in failures – even if we generously use standards that allow, for example, the "Boy on Raft" case to be judged a success. Given so many misses, an occasional lucky hit is hardly surprising. The miracle van Woudenberg thought he witnessed may simply have been one of those successful guesses we can expect once in a while if the number of trials is sufficiently large. Tenhaeff is remarkably reticent about the many failures, except when he feels able to explain them in terms of misdirected ESP. The complete disasters that cannot be rescued even by parapsychological special pleading are conveniently ignored. Examples, however, are numerous.

In May 1956 the public prosecutor in Amsterdam revealed that three psychics had earlier that year attempted to shed light on the disappearance of a 31 year old inhabitant of Rossum. Croiset had stated that the man was alive and staying in Germany. Shortly thereafter, the body was found in a canal in the municipality of Ootmarsum, Holland.

In 1969, Croiset went to Viareggio, in Italy, to look for 13 year old Ermano Lavorini. He "saw" that the boy had fallen into the water while playing. In fact, Ermano had been killed by a friend during a quarrel. The body was found in the dunes.

In 1966, Croiset journeyed to Adelaide, Australia, to search for three missing children. A local "committee" paid the expenses. The clairvoyant was sure the children

were buried under a new warehouse. He advised demolition. The "committee" collected 40,000 Australian dollars to have the building pulled down. The soil under the concrete floor was removed to a depth of four yards. No bodies were found. Croiset urged them to dig one more yard, "and the children will be found." He was wrong. This costly mistake did not affect his reputation. Three years later the Amsterdam paper *Het Vrije Volk*, quoting an AP Telex, claimed that the Australian authorities had "refused permission to search on the spot."

In June 1973, Croiset was consulted by the relatives of a murdered Chinese from The Hague. The clairvoyant indicated that a Mr. Senf knew more about the crime. The relatives then abducted Senf and tortured him for three hours to get a "confession." Senf, however, had nothing to confess. He happened to be innocent. The following week, Croiset visited Senf, who was in the hospital recovering from his ordeal. He brought flowers and assured Senf that he now was quite convinced of his innocence.

In the police journal *Algemeen Politieblad* of January 9, 1960, Utrecht Superintendent Th. van Roosmalen (1960) published a catalogue of psychic blunders. In December 1957, he revealed, the 14 year old son of the E. family disappeared from his parental home in the Utrecht River district. The house was near one of several canals in that part of town. After a couple of days, the parents contacted Croiset. The psychic came and led Mr. and Mrs. E. to the quay, where he stopped and pointed. "This is where your son got into the water and drowned," he said. "I am desolate that I have to be the first to offer you my sympathy for having suffered such a grievous bereavement." The police learned from neighbors that the parents next day had contacted an undertaker to arrange for the funeral. A few days later the boy was found, alive and well and hiding in a haystack.

In the light of such occurrences – and I could quote many more – *Esotera's* description of Croiset as "the psychic who never disappointed" seems to contain an element of poetic license.

Note: While this article was in press, we were informed that Professor Tenhaeff died on July 9, 1981. Professor Tenhaeff had received prior notice of the results of Mr. Hoebens' investigations but declined several invitations to offer specific counterarguments. – *Ed.* [of the *Skeptical Inquirer*]

CHAPTER 3-05

Editorial Introduction

The paper to follow is the sequel to the first Skeptical Inquirer article on the psychic feats of Dutch clairvoyant Gerard Croiset and their representation (or “selling”) by Prof. Wilhelm Tenhaeff. Most of what was said in the editorial introduction to the first part also applies to this sequel. It was published in the Skeptical Inquirer 6 (1981-1982), (2), 32-40. Reprints appeared on pp. 133-141 of Kendrick Frazier’s book Science Confronts the Paranormal (Buffalo, NY: Prometheus, 1986), as well as, unauthorized, in Indian Skeptic, 3 (1990), (3), 20-28; a Dutch version (transl. Karel Beckman) was published, under the title “De professor en de helderziende: onder een hoedje gespeeld?,” in Skepter, 1 (1988), (4), 28-33; a Danish version formed the second and final part of the booklet En psykisk detektiv: Om mirakelmanden Gerard Croiset (transl. Gerda Volf, introduction Willy Wegner). Hjallerup: Skeptica skriftserie, 1986. (Eds.)

CROISSET AND PROFESSOR TENHAEFF: DISCREPANCIES IN CLAIMS OF CLAIRVOYANCE

It appears that Professor Tenhaeff, whose works are the principal source of information on the Dutch clairvoyant, fraudulently reported his results.

According to Professor Wilhelm Heinrich Carl Tenhaeff (1960), the majority of the hits scored by “psychic” detectives “appear to be of value solely from the parapsychological angle.” They are of no use to the police, but to the experienced psychological researcher they constitute interesting examples of ESP. The psychic is supposed actually to have “seen” particulars relating to a given police case but to have been unable to get his vision into focus. Only post factum can the clairvoyant’s impressions be declared hits. This, however, requires the facts to be subjected to a positively Procrustean form of “interpretation.”

An anecdote cited by American journalist Jack Harrison Pollack is an almost burlesque example of the lengths to which determined believers will go to make the outcome fit the prediction. Pollack is the author of a full-length biography of the Dutch clairvoyant Gerard Croiset (Pollack, 1964), which Tenhaeff helped him with and vouched for. Consulted in a 1950 Arnhem rape case, Croiset “saw” that the rapist had “an abnormally big genital organ.” When the police arrested a suspect, they had a good look at his private parts but found them to be standard size. Never mind, says Pollack, “They learned

that he was a twenty-year-old cook who occasionally used a big, red basting syringe in the kitchen, which prompted Croiset's image of an abnormally large genital organ." Both in the police cases and in the experimental "chair" tests, Croiset's typical ESP hit was on a comparable level. Of course the common willingness to believe that a post factum "explanation" reveals what the psychic really meant in the first place is at the bottom of the astounding success of hundreds of soothsayers, I Ching experts, tea-leaf readers, and other augurs. Tenhaeff, however, thought that those who make this objection suffer from "Gestalt blindness."

Professor Tenhaeff, who in 1953 was appointed to the first chair of psychical research ever to be established at a regular university (Utrecht), had a virtual monopoly on the study of Croiset. Tenhaeff's books and articles (Tenhaeff, 1953b, 1955, 1957, 1960, 1979b, 1980a, 1980b) constitute the principal source of information on Croiset, and it may be said that the case for Croiset's clairvoyant abilities stands or falls with the reliability of his learned mentor. (Tenhaeff died on July 9, 1981, while this two-article series was in press. Professor Tenhaeff had received prior notice of the results of my investigations but declined my invitation to offer specific counter-arguments.)

Critics

Th. van Roosmalen is not mentioned in Pollack's *Croiset the Clairvoyant* (Pollack, 1964) and neither are several other authors who have occasionally cast doubt on the psychic's achievements and their documentation. In the index to Pollack's book, one looks in vain for such names as George Zorab, the parapsychologist who first discovered Croiset and who could have told Pollack some interesting facts about both the psychic and the professor; Spigt, the historian who showed that Tenhaeff's inaugural address in 1953 was based entirely on a spurious source; Filippus Brink (1958), the criminologist who wrote a major work on occult detectives; Pelz (1959-1960), the Hamburg police officer who in 1959 published a scathing report, titled *Herr Croiset, Sie können nicht hellsehen* and Ph. B. Ottervanger, the Dutch skeptic who in the fifties fired many a well-aimed shot at Tenhaeff and his protégé.

Pollack may never have heard of the critics; presumably Tenhaeff did not encourage him to contact them. They might have persuaded the American journalist to correct at least a few of the most outrageous errors in his manuscript and to include some material that, while not flattering to the subject, might have improved the book.

In the same *This Week* article in that we find the Eindhoven case (described in Part I of this series), Pollack praises Tenhaeff as "a stickler for complete scientific proof."

Similar laudatory phrases are found in the biography. Pollack might have given a different description had he been familiar with van Roosmalen's article in *Algemeen Politieblad* (Roosmalen, 1960). There, the Utrecht superintendent reports his meeting with Tenhaeff, which was arranged by the examining judge in Utrecht. On that memorable occasion, van Roosmalen flatly told the professor that he did not believe in paranormal sleuthing. "Superintendent," Tenhaeff replied, "If you like, I will tell you of a few cases where the police failed and where Croiset was successful." Tenhaeff then related, in great detail, two ironclad cases. The first concerned a murder in the municipality X. After months of fruitless investigation, the police consulted Croiset. The psychic gave such a clear description of the murderer that they were able to make an arrest. The second case concerned a theft in a factory in the town of Y, where Croiset had identified the thief. Van Roosmalen decided to check these claims. The police officer in X, when asked about the murder case, was puzzled. He said it was somewhat unlikely that Croiset had been successful in identifying the murderer because they had no record of such a crime having been committed! Van Roosmalen's colleagues in Y admitted that a suspect had been arrested on Croiset's advice. However, the alleged thief proved to be entirely innocent and had to be released with profuse apologies. Van Roosmalen was urgently advised not to mention the name of Croiset if he should visit the police in Y.

Pollack might also have found an interview with Filippus Brink enlightening. Brink, a police officer, in 1958 completed a doctoral thesis, *Enige Aspecten van de Paragnosie in het Nederlandse Strafproces* (Brink, 1958, 1960), in which he reported the results of a series of experiments with occult detectives and of inquiries to police authorities in both Holland and abroad. Brink had tested four well-known "psychics" – one of whom was Croiset – by handing them photographs and other objects and requesting them to give their "impressions." Some of the materials were related to police cases, others were not. The experiments were extensive and lasted for more than a year. The results were nil. Looking at the picture of a murderer, the psychics clearly saw that the man was innocent; handling a weapon that came straight from the factory, they got visions of murder and hold-ups.

In "Aid to the Police," one of his few articles published in English, Tenhaeff (1953b) had assured his readers that Croiset "does not 'fish' for information." Brink (1958) observed nothing *but* fishing.

The results of Brink's police department inquiries (Brink, 1960) were hardly more comforting to the proponents of paranormal detection.¹ With very few exceptions, all

1 For his dissertation, Brink (1958, 1960) had conducted a questionnaire study on the potential use of psychics in criminal investigations with the police authorities or the ministries of the interior in all (then) 57 ICPO countries around the globe (ICPO = International Criminal

Dutch and foreign authorities stated that psychics had never been successful in furthering any police investigation. (Incidentally, this was the reply even from the Haarlem police district, where Mr. Gorter had been superintendent, as reported in the previous paper.) The exceptions concerned highly ambiguous successes.

Brink recently told me: "I dare to say that, barring an occasional lucky guess, no clairvoyant has ever been able to solve a police case by paranormal means in the Netherlands." My recent inquiries to a number of Dutch police departments suggest that little has changed since Brink's 1958 publication.

Caught in Fraud

For a number of reasons that will be discussed later, these criticisms were not seen as fatal to Tenhaeff's reputation as a careful and honest scholar. His proponents privately admitted that the professor occasionally suffered from bouts of absentmindedness and might even sometimes have been led astray by his own enthusiasm. However, they insisted that the substance of his work was unassailable. Some of them began to lose faith only in 1980, when I caught the professor red-handed in patent fraud. This time, it was difficult to think of innocent explanations.

In the course of my investigations into psychic claims, I have always concentrated on what Tenhaeff himself regarded as prize cases (to avoid the charge of biased data-selection). Given Croiset's international reputation as a psychic crime-buster, I was surprised at the scarcity of cases that would qualify him as such. Almost all the reports of his works with the police were about cases that did not result in the arrest of the actual culprit.

The Wierden affair (where Croiset is supposed to have identified the assaulter of a young girl by simply handling the hammer with which the crime had been committed) has been cited time and again by Croiset proponents, but it lost much of its appeal after C.E.M. Hansel (1966) reported that he made inquiries to the local authorities and was told that Croiset's endeavors had been of no use. Then, in the September 1980 issue of the German monthly *Esotera*, Tenhaeff (1980a) published a report of a case that seemed ironclad. To summarize this report: On November 15, 1979, a state police officer, Commander Eekhof, had visited Croiset and asked him to help identify a mysterious arsonist who had terrorized the Woudrichem area for months and had completely escaped

Police Organization, a precursor of Interpol) and, in addition, with 14 police directors of big European cities. When Brink died in the early 1980s, Hoebens inherited Brink's collection of (mostly) the original responses from the police authorities in all those countries. These documents from the 1950s have survived in the Hoebens Files. (Eds.)

detection. Eekhof did this “in the hope that he [Croiset] would be able to provide the authorities with definitive information concerning the culprit.”

A few weeks before Croiset’s sudden death in July 1980, Tenhaeff had visited the Woudrichem police “to learn from Eekhof in full detail how successful his visit to Croiset had been.” Tenhaeff (1980b) wrote: “Everything Commander Eekhof told us was videotaped. The tapes were protocolled and the protocol was checked and signed by Mr. Eekhof.”

According to Tenhaeff, Croiset was consulted at a moment when all official attempts to identify the perpetrator had proved fruitless. The clairvoyant described the arsonist as a man who “sometimes wore a uniform,” “lived in an apartment building,” and had “something to do with toy airplanes.” Asked by Eekhof whether it would be “model airplanes,” Croiset replied “in the affirmative.” Eekhof allegedly “was shocked” by the clairvoyant’s statements, “for Croiset’s description fitted a police sergeant in his own police group.”

According to Tenhaeff, at first the commander was incredulous, but “sometime later he saw himself compelled to admit that Croiset had been right: the police sergeant was a pyromaniac.”

In order to check this remarkable claim, I contacted Commander Eekhof and showed him the *Esotera* article, which he had neither seen nor heard of. After having carefully read and re-read the report, he stated positively that it contained “outright falsehoods.” He invited me to listen to the tape-recording of what Croiset had really said.

The grossest inaccuracies are corrected here:

- The consultation took place on November 15, 1977, a full two years before the date given by Tenhaeff. The police sergeant was not arrested until March 1980.
- Croiset at no time mentioned a “uniform,” which would have been the most striking hit.
- Croiset did not mention “toy airplanes,” although he did speak of “airplanes” – “sitting in airplanes,” “airfields,” and “airplane construction.” When asked by Commander Eekhof whether it could be model airplanes, Croiset first said yes, maybe, but then retracted and said, “No, these are big airplanes.” It is quite true that the police sergeant liked to build model airplanes. But these were first mentioned by Mr. Eekhof and not by Croiset. So if this was an ESP hit, then it was scored by the policeman and not by the clairvoyant!
- Croiset, who in earlier attempts to “see” the culprit had put the police on a false track, had finally identified the arsonist as a person in a photograph shown to him by Eekhof. This person – whom the police already considered a possible suspect – was not the police sergeant, who later admitted the crimes.

- Eekhof certainly was not “shocked” by Croiset’s statements, because he could not possibly have recognized his fellow policeman in the psychic’s confused “images.” The police sergeant, who did not live in an apartment building, began to be suspected only months later, for reasons entirely unconnected with Croiset’s “vision.”
- Tenhaeff’s claim that a protocol was “checked and signed” by Commander Eekhof is categorically denied by Eekhof, who told me he had not even seen a protocol.

Before exposing this quite extraordinary case of fraudulent reporting in two Amsterdam newspapers, *De Telegraaf* and *Courant Nieuws van de Dag* (October 18, 1980), I naturally invited Tenhaeff to comment. The “stickler for complete scientific proof” flatly refused to answer any questions, shouting a number of insults before slamming down the receiver.

The reader will not be surprised to learn that Tenhaeff’s Dutch version of the report, published at about the same time in his *Tijdschrift voor Parapsychologie* does not mention either “uniforms” or “signed protocols.” The worst distortions were prepared for export only. If I had not sent him a copy, Eekhof might never have seen the fairy tale in *Esotera*.

Methods

As I have demonstrated, at least to my own satisfaction, Tenhaeff’s reports – our principal source of information concerning Croiset – are utterly unreliable. It is therefore hazardous to suggest possible “naturalistic” explanations for any “facts” presented in those reports. One may easily waste one’s ingenuity on entirely spurious data. It is with this proviso that I will briefly offer a few conjectures about how the clairvoyant might have achieved what appear to be paranormal successes by perfectly normal means.

Croiset, a skilled hypnotist, was an expert muscle reader and a master of suggestive questioning. He was therefore well equipped for wresting shreds of information from unwitting clients and feeding these back as “telepathic” impressions.

It is possible that he occasionally resorted to cruder methods, such as using spies. In the pro-Croiset literature we find surprisingly little mention of the psychic’s “assistants” and “secretaries,” such as Dick West. Zorab (personal communication) has evidence that Croiset sometimes employed confederates in his experiments. Apparently the clairvoyant’s professional ethics were not such as to forbid a little trickery now and then.

Even more important, Croiset knew how to make friends and influence people. He maintained very cordial relations with a number of journalists and law officers. (Some policemen were patients of “Dr.” Croiset.) I do not need to point out the risks of such

familiarity. Croiset was an engaging man who disarmed visitors with a convincing display of sincerity and simplicity. He was not the sort of person in whose company one felt the need to be on guard.

Discussion

The standard skeptical explanation for the alleged successes of psychic detectives is that these sensitives offer their consultants the verbal equivalent of a Rorschach test. Their statements are typically vague, rambling, and verbose. The accuracy of the readings is evaluated post factum: “Good sitters” retroactively interpret the ambiguous and often contradictory statements in such a way that they fit the true facts and obligingly forget the many details that were too wide of the mark. Complete failures are ignored or suppressed. The possibility that some of the paranormal information could have been acquired by normal means is quietly discounted. Occasional lucky guesses are enhanced by selective reporting and editorial embellishment.

The results of the present investigation suggest that this standard hypothesis does not need to be revised in order to explain the Croiset phenomenon. What always set Croiset apart was probably not the degree of his supposed paranormality but rather the success of the propagandistic efforts on his behalf. Unlike other psychics, Croiset had the extraordinary luck to find an impresario who enjoyed a fairly solid reputation as a scientist and scholar. The “Miracle Man from Holland” would never have achieved his status without the indefatigable help of Wilhelm Tenhaeff.

The fact that this mentor was an authentic university professor has always protected Croiset. Croiset was widely believed to be the most honest of men because Tenhaeff said he was. As I have suggested, the key to the Croiset mystery lay with Tenhaeff. The question now is: Why did Tenhaeff act as he did? Why risk an academic reputation by engaging in palpable fraud? The answer, I think, can only be that Tenhaeff *had to deceive* and thought that he would be able to get away with it. Soon after he made Croiset’s acquaintance in 1947, the professor must have realized that the psychic’s successes in occult detection were highly ambiguous. Straight and full reports of the “police cases” would never convince those not committed to a strong prior belief in the paranormal. Tenhaeff, whose ambition had always been to become the Sigmund Freud of psychical research, devised something he was pleased to label a “theory” that enabled him to explain all but the worst failures in terms of ESP. The red basting syringe is a typical example. Yet the professor needed at least a few ironclad proofs to underpin this odd theoretical structure. The extreme scarcity of authentic miracles forced him to fabricate them.

What now remains to be explained is how Wilhelm Tenhaeff got away with this game for so many years. I can only offer a few suggestions.

1. His status as an “official professor”: Tenhaeff was hailed as the first professor of parapsychology in history, and this carried much prestige. “He must be right or else they would not have appointed him, would they,” his admirers were wont to say when confronted with an unbeliever. For many, such considerations effectively settled the matter.
2. The confusion he created with his publications: To the delight of the true believers, who find obscurity sure proof of profundity, Tenhaeff’s writings are chaotic, verbose, and abstruse. A thick fog seems to exude from his books. Scanning these thousands of convoluted sentences for contradictions requires many hours of exceptionally dull work. Most skeptics sensibly give up after a few pages. I myself only embarked on this dreary task as the result of a challenge. After I had published some critical remarks on psychical research in 1978, local proponents defied me to come to terms with the best evidence they thought parapsychology had to offer: the “rigorously scientific work” of Wilhelm Tenhaeff. Critical investigation is further complicated by the fact that the professor cleverly took advantage of the language barrier. The completely fraudulent versions of the “Boy on the Raft” and the Woudrichem cases were concocted for export only. To compensate, Tenhaeff occasionally did the reverse: in the German *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie* (No. 4, 1980)², I have shown how he dishonestly “edited” a report by Dr. Jule Eisenbud of an ambiguous success in a transatlantic ESP experiment with Croiset for home consumption in Holland.
3. The ambiguous feelings within the psi community toward cheating colleagues: The psi community has never completely freed itself from the pernicious *idée fixe* that overt criticism of a colleague may damage the cause and play into the hands of the enemies of parapsychology. Some psychical researchers began to suspect Tenhaeff long ago. Seldom, however, did they voice their doubts openly. And, when they did, some sociological mechanism seems to have prevented an adequate follow-up.
4. Tenhaeff’s mastery of propagandistic techniques: Tenhaeff has always been a master of propagandistic manipulation. He deftly used his excellent relations with the media to persuade a sizeable segment of public opinion in Holland that he was a prophet of a new, nonmaterialistic science, who therefore had to suffer the irrational hatred of those whose world-view was threatened by the glorious discoveries of parapsychology. He never failed to remind his audiences of the religious implications of his work or to allude darkly to possible bolshevik influences in skeptical

2 Reprinted, in English, as chapter 2-04 of this book; also see chapter 3-07 (Eds.).

circles. His favorite trick was to tell the public that his critics were suffering from a Freudian complex and needed psychiatric treatment rather than to reply to their impertinent questions.

Ironically, Tenhaeff's bizarre behavior convinced a number of skeptics that he was a gullible victim of a devious "psychic" rather than a deceiver in his own right. His apparent credulity served as camouflage for his dishonesty.

Conclusion

My purpose has not been to deny Croiset's paranormal powers *ex cathedra*. Rather, I wanted to draw the reader's attention to some false notes in the "psychic Mozart's" scores, dissonants that seem to have escaped the notice of other biographers.

Of course *if* such a thing as ESP exists, it may well provide the most economical explanation for some of the coincidences that have been reported in connection with Gerard Croiset. On the other hand, it would be rash indeed to conclude the existence of a paranormal faculty on the basis of the Croiset material. If Croiset's amazing talents of clairvoyance had been genuine, then why would Tenhaeff have felt the urge to manipulate reports and present them fraudulently as prize cases?

I certainly do not wish to be dogmatic about psychic detectives: positive evidence of a much stronger nature may yet turn up. Until it does, the "believer" is well advised not to base his belief on the "Psychic Who Never Disappointed."

Acknowledgement

I am grateful to Mr. and Mrs. Ottervanger of Bussum, Holland, for having allowed me to make use of the extensive archives of the late Mr. Ph. B. Ottervanger.

Editorial Postscript (December 2014)

It is a shame and a tragedy, to say the least, that within days (some say: within hours) of Wilhelm H.C. Tenhaeff's death, his assistant of many decades and female companion, Nicky G. Louwerens, burnt to ashes Tenhaeff's complete archives and collected correspondence of more than fifty years, presumably from the 1920s to 1980. Thus, a wealth of information on the careers of the first parapsychology professor and of his psychic protégé is irretrievably lost. At the same time, Louwerens also concealed her own trail

(including, presumably, that of her empirical work) and preserved it from future scientific scrutiny.

Hopefully, some crucial documents on Tenhaeff and Croiset's almost symbiotic relationship of so many years are preserved in the archives of self-declared clairvoyant, healer and psychic detective Gerard Croiset. This extraordinarily huge collection of documents, letters, newspapers, clients files – including, for instance, missing-person case files and patient records, an enormous assortment of film and audio recordings, and much more – was recently acquired by drs. Wim H. Kramer of the Het Johan Borgman Fond and its impressively successful archiving project. In our estimate, these sources will be certain, one way or the other, to form the basis for studies on the history of Dutch parapsychology and the field's first university chair. For the time being, Wim Kramer and drs. Maurice van Lujtelaar will be busy sorting and indexing this wealth of relevant material. Watch out for more. (Eds.)

CHAPTER 3-06

Editorial Introduction

The following short article may be considered a by-product of (and was completed very shortly after) the two-part series on Gerard Croiset and Prof. Wilhelm Tenhaeff that was published in the Skeptical Inquirer (see the two previous papers, chapters 3-04 and 3-05), and it covers basically the same ground. It is reprinted here nonetheless because it contains some additional bits of information and a couple of arguments that were not included with the SI articles.

The present paper was solicited by the editors of the French magazine Les Cahiers Rationalistes. It was written in English, translated into French by Claudine Briane, and published, in 1981, on pp. 245-250 of the magazine's no. 369 issue under the title, "Gerard Croiset, le médium qui n'a jamais failli." Before its publication, Hoebens had approved the French translation. The following chapter is reprinted from Hoebens' original English manuscript. The article was never published in the English language before. (Eds.)

CROISET: THE PSYCHIC WHO NEVER DISAPPOINTED

Gerard Croiset, the Dutch clairvoyant who died on 20 July 1980, was widely regarded as the occult equivalent of Sherlock Holmes. His many devotees called him "The World's Greatest Psychic"; "The Man with the X-Ray Mind" and "The Wizard of Utrecht."

A wizard he must have been indeed, if the hundreds of enthusiastic press accounts of his supernatural feats are even half true. A typical example of the kind of media coverage Croiset was used to receiving is provided by the English journalist Derek Shuff in the tabloid *The Sun* of 26 November 1979. In a two-page "exclusive" Mr. Shuff informed his readers that, "over the past 40 years, former grocer Gerard Croiset has used his talent as a clairvoyant to help police around the world with hundreds of crimes." "His most famous case was to describe the killer of a young Dutch girl who was found dead beside her bicycle." The journalist had traveled to Utrecht to consult the oracle about the notorious Yorkshire Ripper, whose identity was at that time a complete mystery. Croiset obligingly got a vision of the criminal, and described him in some detail.

The girl beside the bicycle is an enigma. Somehow, this "most famous case" must have managed to escape the attention both of the Dutch police and the Dutch press. Concerning Croiset's description of the Yorkshire Ripper I can only say that, if the psychic was right, then the British police probably have made a bad mistake by arresting that lorry driver.

Of course, it would be highly unfair to evaluate Croiset's alleged psi-powers on the basis of this and hundreds of similarly sensationalist press reports. Our judgement must take into account the evidence that has been collected and published by a respected scholar who, for over 35 years, has closely followed the clairvoyant's career. Professor Dr. W.H.C. Tenhaeff is regarded as one of the pioneers of European parapsychology. He was the first psychical researcher to be appointed to a regular institute of higher education – Utrecht State University. His work has won him praise from such diverse people as the Pope, who gave him a medal, and American psychiatrist Berthold Schwartz, who compared him to Galileo, Freud and Einstein.

Tenhaeff has repeatedly congratulated himself with his “well-known meticulousness” and his “rigorous methodology.” The American author Jack Harrison Pollack, who wrote a Croiset biography (Pollack, 1964), has called the Professor “a stickler for complete scientific proof.” Tenhaeff “approaches each case as if it were untrue. Only when it is documented to his satisfaction does he give it credence.” (*This Week*, February 19, 1961.)

Let us follow this admirable example, and critically examine one of Croiset's prize cases as reported by meticulous Professor Tenhaeff.

In the September 1980 issue of the widely read German monthly *Esotera*, the Dutch savant devoted a long In Memoriam to his deceased star-subject (Tenhaeff, 1980a). The article was announced under the title *The Psychic Who Never Disappointed*. On pp. 825 and 826 of the same issue, Tenhaeff (1980b) illustrated Croiset's proficiency as a psychic detective by presenting the case of the Woudrichem arsonist. As described by Tenhaeff, the case seems iron-clad indeed. To summarize the report: on November 15, 1979, a State Police officer, commander Eekhof, invoked Croiset's assistance in the search for an arsonist who had been creating havoc in the Dutch community of Woudrichem. Eekhof did this “in the hope that he [Croiset] would be able to provide the authorities with the definitive information concerning the culprit.” A few weeks before Croiset's unexpected death Tenhaeff had visited the Woudrichem police office “to learn from Mr. Eekhof in full detail, how successful his visit to Croiset had been.” “Everything commander Eekhof told us was videotaped. The tapes were protocolled and the protocol was checked and signed by Mr. Eekhof.”

According to Tenhaeff, Croiset was consulted at a moment when all official attempts to identify the perpetrator had proven fruitless. The clairvoyant described the arsonist as a man who “sometimes wore a uniform,” “lived in an apartment building” and had “something to do with small aeroplanes, with toy-aeroplanes.” Asked by Mr. Eekhof whether it could be “scale models of aeroplanes,” Croiset replied “in the affirmative.” Mr. Eekhof “was shocked” by this statement, “for this description by Croiset fitted a quarter-master in his own police station.”

At first, the commander was incredulous, but “some time later he saw himself compelled to admit that Croiset’s description had been right: the quarter-master was a pyromaniac.”

In order to check Tenhaeff’s remarkable claim, I contacted commander Eekhof and showed him the *Esotera* article, which he had neither seen nor heard of. After having carefully read and re-read the report he stated positively and indignantly that it contained “downright falsehoods.” He invited me to listen to the tape recording of what Croiset had really said.

To mention only the worst distortions:

1. The consultation took place on November 15, 1977, a full two years before the date given by Tenhaeff. (The quarter-master was arrested only in March 1980).
2. Croiset at no time mentioned a “uniform” – which would have been the most striking “hit.”
3. Croiset did not mention “toy aeroplanes” although he spoke about “aeroplanes,” “sitting in aeroplanes,” “airfields” and “aeroplane construction.” When asked by Mr. Eekhof whether it could be scale models Croiset first said yes, maybe, then retracted and said: “No, these are big aeroplanes.” It is quite true that the quarter-master liked to build scale models of aircraft. But they were first mentioned by Mr. Eekhof and not by Croiset. So if this was an ESP-hit, then it was scored by the policeman and not by the clairvoyant!
4. Croiset, who earlier by telephone had given a different “profile” that set the police on a false trail finally identified the “arsonist” as someone on a photo shown to him by Mr. Eekhof. This “suspect” – to whom the “profile,” aircraft included, was meant to apply – was not the quarter-master.
5. Mr. Eekhof certainly was not “shocked” by Croiset’s description, as it is absolutely untrue that he had recognized or even could have recognized the quarter-master in the psychic’s confused statements. The quarter-master began to be suspected only months later, for reasons entirely unconnected with Croiset’s supposedly paranormal impressions.

In fact, the real arsonist (who did not live in an apartment-building – such buildings not being found at all in Woudrichem) was a member of the police team investigating the case. In the company of his colleagues he had listened to Croiset’s tape-recorded statements without anyone having pointed at him exclaiming: “That’s you!”

6. Tenhaeff's claim that a protocol was "checked and signed" by commander Eekhof is categorically denied by the latter, who has told me that he had not even seen any protocol.

Oddly enough, when Tenhaeff published the Woudrichem case in his own Dutch-language *Tijdschrift voor Parapsychologie* (Oct. 1980) he presented a completely different version. No mention is made of "uniforms," nor of "signatures" under "protocols." Although quite misleading and containing a number of fresh inaccuracies, the Dutch article is far more consonant with the true facts than the version the Professor concocted for the export to Germany.

Before exposing this palpable fraud in the Amsterdam newspapers *De Telegraaf* and *Courant Nieuws van de Dag* (which I eventually did on October 18) I naturally invited Tenhaeff to comment. The Einstein of parapsychology angrily refused to answer any question, shouting a number of insults before throwing down the receiver. My subsequent revelations were greeted with embarrassed silence on the part of Tenhaeff's own *Studievereniging voor Psychical Research*, the Dutch parapsychological society. A few members of the Council privately requested that the Professor show them his evidence. Tenhaeff refused. No action was taken. At the time of writing (January 1981), a proven falsifier is still Editor in Chief of the *Tijdschrift voor Parapsychologie*, devoted to the "scientific investigation of paranormal phenomena." The fraudulent Woudrichem report is not an isolated lapse on Tenhaeff's part. Fresh evidence of dishonest manipulation will be published in the near future.

A final question must now be faced: How will Tenhaeff's fall from grace affect psychological research in general? The existence of a psi-factor is a fiercely debated subject, and the exposure of yet another famous proponent will no doubt be welcomed by those who insist that parapsychology is nothing but a shady pseudo-science.

I feel compelled, however, to state that I do not subscribe to this view, and that I hope my criticisms of an individual proponent will not be seen as part of an attempt to debunk the whole field. Let there be no mistake: personally I do not believe that ESP and psychokinesis are "real" phenomena. Yet I am sufficiently impressed by the arguments put forward by a rational minority within the parapsychological community, not to dismiss the whole thing in a summary fashion. I fully agree with the American psychologist Professor Ray Hyman (himself an "unbeliever") that "the 'believer'-'skeptic' dichotomy tends to overshadow the fact that there are important overlappings of common interests and goals that cut across this dichotomy." This confession may set me somewhat apart from the "hard-liners" among my fellow-skeptics, but I think I owe it to those parapsychologists who, while they may pursue non-existent phenomena, at least do not attempt to prove their case by citing non-existent documents.

CHAPTER 3-07

Editorial Introduction

The once-famous and highly acclaimed “Denver” Chair Test with psychic Gerard Croiset was briefly mentioned in several of the previous chapters. Hoebens examined its methods and public representation in detail (and with some support from Denver psychiatrist Jule Eisenbud), and he wrote up his findings and conclusions in a paper that is to follow here. That paper was originally written and submitted, as early as 1981, to the Journal of the American Society for Psychical Research. That journal was the obvious place for Hoebens to submit his paper to because Jule Eisenbud’s own study on the “Denver” Chair Test, in which he was involved and which formed part of Hoebens’ “comparison of reports,” had been published in that journal eight years previously. After much twisting and negotiating and apparent interventions from “interested parties,” Hoebens’ manuscript was eventually rejected – for the flimsiest of reasons – as being “unsuitable” for publication in that once-prestigious periodical.¹

After Hoebens’ death, his original manuscript was very slightly revised and updated, based in part on material kept in the Hoebens Files, by Scottish-Dutch parapsychologist Brian Millar. It was eventually published – posthumously, in the fall of 1986 – in the British Journal of the Society for Psychical Research, 53, 311-320.

*A concise history of the chair-test methodology and its refinements can be found, in Dutch, in Bootsman (1995). Its origins can be traced back to experiments Eugène Osty performed, in 1926, with one of his star subjects, Pascal Forthuny, at the Institut Métapsychique Internationale (IMI) in Paris (see Osty, 1926). Chair tests then seemed to have been largely ignored or forgotten until Tenhaeff (1938) rediscovered them when he wrote a review of Pascal Forthuny’s book *Je lis dans les destinées* (Forthuny, 1937). Since that time, Tenhaeff appears to have been involved, in one way or another, with virtually all chair tests that were reported in the parapsychological literature. According to Bootsman (1995, pp. 33-37), the career of the quasi-experimental chair-test methodology was effectively terminated after Hoebens’ detailed critiques of the “Denver” (this chapter) and “Pirmasens” (see chapter 3-11) experiments. In fact, we have been unable to trace any chair-test reports published after 1980. (Eds.)*

1 The full uninspiring correspondences, which throw a rather dim light on the contemporary JASPR editorial board, have been preserved in the Hoebens Files.

COMPARISONS OF REPORTS OF THE “DENVER” CHAIR TEST: A CRITICAL EXAMINATION OF THE METHODS OF W. H. C. TENHAEFF

Editorial Note

The author of this posthumous article died prematurely on the 22 October 1984 at the age of 36. An obituary of him by his friend, Brian Millar, appeared in the June 1985 issue of the Journal. The author’s antagonist, Professor Tenhaeff, had already died on the 9 July 1981 but the article was written while Tenhaeff was still alive. He was, of course, invited to reply but declined to do so. We are indebted to Dr. Millar for editing this paper and bringing it to our attention.

– THE EDITOR (*Journal of the Society for Psychical Research*²)

Introduction

Gerard Croiset, who died in July 1980 in his home town of Utrecht, Holland, was widely regarded as one of the world’s most gifted sensitives. This “Dutchman with the X-ray mind” – to quote a typical newspaper headline – was credited with having successfully assisted police forces all over the world in tracing missing persons and solving mysterious crimes, and with having demonstrated his amazing precognitive powers under scientific scrutiny almost continuously since World War Two.

These claims are, however, still a matter of controversy. Croiset’s achievements as a psychic detective share many of the features of “spontaneous” cases, with all the uncertainties inherent in that type of evidence. A number of policemen are clearly on record as having expressed their amazement at certain of Croiset’s hits (e.g. van Woudenberg, 1964). Nonetheless, it is difficult to decide how much value to attach to such testimonies: policemen do not need to be aware of possible non-paranormal explanations for what seem to be instances of ESP. It should be pointed out, moreover, that some of Croiset’s most effective critics have been police authorities (Brink, 1958; Hansel, 1980; Pelz, 1959-1960; Roosmalen, 1960).

The experimental evidence for Croiset’s powers is almost entirely in the form of reports on so-called chair tests, of which several hundred were performed since 1947. In a chair test, Croiset was asked to give a precognitive description of the person who would, at

² John Beloff was the Editor of the *SPR Journal* at the time. (Eds.)

some given time in the future, be seated on a chair bearing a particular number. Tenhaeff and Bender have reported positive results with many of these experiments (Bender, 1957; Tenhaeff, 1953a, 1979a). The chair tests have been harshly criticized by diverse workers such as Zorab (1965), Gubisch (1961) and Pelz (1959-1960) for inadequate experimental design in such matters as insufficient safeguards against fraud, *post factum* interpretation of ambiguous statements and selective reporting. Staub (1978) has further stigmatized them as “quasi-experiments” from a methodological point of view.

The difficulties inherent in the treatment of psychic readings have been much discussed (e.g. Scott, 1949) and the objective testing of such material and its statistical evaluation (pioneered in the chair tests by Bender [1957] and Timm [1965, 1966]) is no easy matter. The crude methods particularly evident in the earlier work have been progressively refined over the years. However, it is not the purpose of this paper to enlarge further upon these methodological issues. From a more intuitive point of view, quite a few of Croiset’s reported achievements are striking indeed. This however leaves us with an important question: can we fully trust the reports?

The fact that for over 30 years Croiset’s chief chronicler has been a respected Utrecht State University Professor, Dr. W.H. C. Tenhaeff, has caused many otherwise skeptical observers to regard this question as a purely academic one. Tenhaeff’s status as a distinguished scholar, holding a chair at a prestigious institute of higher learning, is taken as sufficient guarantee for the accuracy of his reporting. In the course of preparing a review of Tenhaeff’s last two books (a revised edition of *De Voorschouw* [1979a] [*Precognition*], and *Ontmoetingen met Paragnosten* [1979b] [*Encounters with Paragnostes*]) I have taken a closer look at some rather spectacular claims made in both works.³ The results throw unexpected light on Tenhaeff’s methods.

In both books (1979a, pp. 146-151; 1979b, pp. 165-171) Tenhaeff describes a “very successful” transatlantic chair test. On 6 January, 1969, Croiset had given two series of statements meant to apply to two persons as yet unknown. They would be chosen by lot from a group to be assembled in Denver, Colorado, on 23 January, at a demonstration evening supervised by Dr. Jule Eisenbud. According to Tenhaeff, both sets of statements turned out to be startlingly accurate. Croiset had “seen” that the female target person had experienced some emotion connected with page 64 of a book. He had “seen” the male target person would be wearing green socks with a hole in one of them. Not a single one of his 21 statements was a clear miss, although many of the hits needed a considerable amount of interpretation before their applicability became apparent.

3 See chapter 2-04 in this book. (Eds.)

As reported by Tenhaeff, this particular chair test must be counted among the five or six most successful of the many hundreds Croiset had done since 1947. As Croiset (most untypically) did not personally take part in the questioning of the presumed target persons and these targets were selected by the weather-key method, conditions seemed to be more satisfactory than usual. In both works, Tenhaeff refers to an “exhaustive and detailed” discussion of the transatlantic tests in the *Tijdschrift voor Parapsychologie*, the journal of the Dutch Studievereniging voor Psychical Research. (Tenhaeff, 1969). Another reference can be found in the *Tijdschrift* (Tenhaeff, 1973), an article by Tenhaeff, based on the text of a lecture on “Anthropological Parapsychology” read at conferences in both Genoa and Königstein the year before. In addition the case is mentioned in the German-language “Zur Persönlichkeitsstruktur der Paragnosten” [On the personality structure of paragnosts], Tenhaeff’s contribution to Schatz (Tenhaeff, 1976). In none of his post-1973 Dutch publications does the author refer to Eisenbud’s own account of the experiment in the *Journal of the American Society for Psychical Research* (Eisenbud, 1973). It was by pure accident that I stumbled upon this key document: as Tenhaeff does not mention it in any of his Dutch writings I had simply assumed it to be non-existent.

Eisenbud’s lengthy report immediately makes plain that this was no sensitive quantitative test: the design was unsuited to such work and furthermore some errors had been made. Principal among these (discussed later) was that a fault in the randomization procedure made it impossible to know who the designated target persons actually were. Eisenbud nonetheless found some of the apparent correspondences so outstanding as to be worth reporting. The only criteria available are the rather subjective ones of whether Croiset’s statements constitute striking hits. It is on this basis that both Eisenbud and Tenhaeff discuss the case.

Comparisons

Comparison of Eisenbud’s report and Tenhaeff’s versions yields surprising results. In Tenhaeff’s accounts, important details mentioned by Eisenbud are entirely lacking, whereas Croiset is credited with a number of hits that were hardly hits at all if we are to trust the report in the *Journal of the American Society for Psychical Research* (JASPR).

Moreover, Tenhaeff’s subsequent accounts contradict each other at some points. Some of the discrepancies are minor – they mainly concern details of the experimental procedure. Unfortunately, however, many of the differences do show the results in quite a different light.

The discrepancies are subsumed below under nine headings.

1. *The number of items*

In his 1979a book Tenhaeff states that “Croiset gave 21 precognitive statements in all.” In his 1969 paper we indeed find 21 statements, numbered 1-10 (for the female target person) and 1-11 (for the male). All this clearly implies that Tenhaeff claims that his 1969 account contains the complete and unabridged reading Croiset gave on January 6.⁴ We might expect some discrepancy in the number of statements reported by different persons due to the arbitrary ways that verbal material can be cut up into items. However, in Eisenbud (1973) we find no less than nine additional statements (items 5, 6, 12 [female target person] and 3, 4, 6, 10, 11, 13 [male target person]) intended to apply to the target persons and which are not incorporated in any way in Tenhaeff’s list. Most of these statements (omitted from all Dutch accounts) turned out to be wrong.

2. *At what point were the statements confirmed or denied?*

Tenhaeff (1969, 1979a, 1979b) gives an “integrated résumé” of two “verifications” on subsequent occasions but he hardly enlightens his readers about any differences in the two sets of answers. Clearly any information obtained by the participants between times would be likely to alter their responses. The first “verification” of Tenhaeff refers to the comments given by the participants to Croiset’s statements on the evening of the 23rd. The second “verification” relates to interviews which, Tenhaeff reports, Eisenbud had with both target persons in their homes, a few days later.

Additionally, all participants who drew numbered tickets were given questionnaires to complete later and return by post. Tenhaeff relates only that “the returned questionnaires showed that practically all participants answered the questions in the negative” except, of course, the target persons.

Eisenbud’s report reveals that no less than three questionnaires were used, apart from several interviews with the presumed target persons.⁵ On the evening of the demonstration, all participants were given a questionnaire they were requested to hand in before leaving. They were also given a duplicate to send in in case they changed their minds about any of their responses. On 20 February yet another questionnaire was sent to the participants for a final comment. This one was accompanied by a note stating that an error had been discovered in the randomization procedure and that the two front runners who had been found at the demonstration might yet be the actual targets, rather

4 His 1979b book, Tenhaeff claims, gives Croiset’s statements “*expressis verbis*.” Compared to the 1969 version, however, parts of a few statements have disappeared.

5 Eisenbud refers to “several interviews” but fails to indicate when they were conducted.

than the two designated as target persons at the end of the evening. The participant in each case was asked to assign each item a weight-number 0 (“does not apply”), 1 (“possibly applicable”) or 3 (“highly applicable”). A table published in the *JASPR* reveals enormous differences in the subsequent stages of the “verification.”

On the first questionnaire, for example, Mrs. Olinger (identified as the female target person by Tenhaeff) checked 6 items as “possibly” or “highly” applicable, with a weighted total of 10. She heads the list, but at a nose length. Another participant checked 6 items as well, with a weighted total of 6. On the returned carbon copy, however, Mrs. Olinger had changed the number of more or less applicable items from 6 to 11 with a weighted total of 31. The same happened with Mr. Tuck, whom Tenhaeff identifies as the male target. His score changed from 12 possibly or highly applicable items (runner up: 8) with a weighted total of 15 (runner up: 9) to 15 items with a weighted total of 35.

While one could argue that these changes were associated simply with increasingly accurate verification another more disturbing possibility suggests itself – and Eisenbud is aware of it. Both Mrs. Olinger and Mr. Tuck knew they were the front runners when they filled in their carbon copies. This had become perfectly clear during the demonstration evening. Did they not then have a stronger motivation than the others to find some match? Might it not be generally true that the scores of front runners “stretch” in this way?⁶ This would have important statistical consequences. In any case the large changes between successive “verifications” show that most of Croiset’s hits must have been ambiguous. Tenhaeff furnishes his readers no information on these marked score increases.⁷

3. Page Number 64

The “hit” most suggestive of ESP concerns Croiset’s statement for the first (female) target person: “Did she recently experience an emotion connected with page 64 of a book?” She certainly did, according to Tenhaeff. In his 1969 paper we read: “At first, Mrs. O. thought she had to answer the question in the negative. Later, however, memories started to be aroused and she began to understand this impression applied to a book she had bought for her daughter living in Japan. The title of the book is ‘The Cat You Care For.’” Mrs. Olinger had decided not to send the book to her daughter after all, because of a passage about the need to put cats to eternal sleep once they are old and sick. As this had recently

6 The success of various “cold reading” techniques strongly suggests this is indeed an important factor.

7 Unfortunately, giving “integrated résumés” was standard practice with Tenhaeff and also with Bender.

happened to the Olinger family cat, Mrs. Olinger felt it would upset her daughter. The offending passage was indeed on page 64.

Of course it is of vital interest to know at what point Mrs. Olinger discovered the correspondence between Croiset's statement and her own experience with page 64. The hit would become far less striking if she had discovered the match only after she had gone home and had had the opportunity to search her library for a suitable page 64. If she, as is vaguely suggested in Tenhaeff's 1969 paper, spoke about that particular book on the evening of the experiment and only later discovered the upsetting passage was indeed on page 64, the correspondence would be very impressive.

In his 1979a book Tenhaeff strongly implies this was the case: "At the meeting she could not remember the page number, but as soon as she got home she checked and found that this advice was on page 64." In Tenhaeff's 1973 article, based on the lectures read in Genoa and Königstein, we find an even more impressive version: "... Croiset named the page of a book, which page contained a remark that caused the target person not to send this book to her daughter. At the verification it turned out that the target person had indeed withheld a book, bought for her daughter, because the content of page 64 could have reminded her of the painful loss of her cat ..." Here, part of Mrs. Olinger's comment has become part of Croiset's original statement!

Eisenbud's account is dramatically different. He makes it plain that Mrs. Olinger discovered the match only after she had left the meeting. On her first questionnaire she marked the item 0 ("does not apply"). Surprisingly, no less than 17 other participants at that time assigned the statement a 1 ("possibly applicable"). The first time Mrs. Olinger mentioned "The Cat You Care For" was on the carbon copy of the questionnaire, mailed after she had got home. The hit has suddenly become far less striking.

Eisenbud (personal communication⁸) still thinks the coincidence is remarkable as Mrs. Olinger could provide a plausible explanation for her claim, whereas none of the other participants could: "... this would still put her out in front of the crowd, since no one else, with equal opportunity to conduct such a search (for a correspondence [PHH]) sent in anything in response to later questionnaires."

This is only partly convincing. As mentioned above, Mrs. Olinger already knew she was the front runner, thus she had a stronger motivation to find some match. It seems a little surprising too that she should not remember anything at all when first confronted with Croiset's statement, given that the book incident had happened less than a month before.

8 Jule Eisenbud's letters to Hoebens, referred to here and in later sections of this paper, are preserved in the Hoebens Files. (Eds.)

Furthermore, if the loss of the cat was such a painful memory for the daughter, any book about cats would surely have reminded her of it, with or without explicit reference to what should be done to old cats.

Moreover, “page 64” sounds very specific, but I did not find it at all difficult to discover a plausible reason to apply Croiset’s statement to myself. The Winter 1979-1980 issue of the *Skeptical Inquirer*, which I received just as I started my research on the Denver case, contained an article that aroused quite an emotion in me. It was the first article by my hand to be published in the journal of a scientific organization⁹: it was right there on page 64 and it contained two references to Mr. Croiset!¹⁰

Whatever our evaluation of this instance, in Tenhaeff’s account an interesting correspondence has become a minor miracle. His Dutch reports are in varying degrees misleading. Curiously enough, in his German language 1976 article he correctly states Mrs. Olinger assigned the statement a 0, and discovered “The Cat You Care For” only after she got home. Here, Tenhaeff refers to Eisenbud’s *JASPR* report, of which no mention is made for his Dutch readers.

4. *Push or bump?*

A minor point, maybe, but telling. Croiset had asked whether the female target person had, on some occasion, “pressed her nose almost flat into a window.” Mrs. Olinger remembered, Eisenbud reports, having “ducked forward and bumped my nose quite severely on the glass” when trying to get a look at her newborn grandson in the nursery of a Denver hospital. As “pressing” and “bumping” (and their exact Dutch equivalents, “drukken” and “stoten”) are quite different things, the hit is far from perfect. Tenhaeff (1969, 1979b), however, spuriously claims Mrs. Olinger “pressed” her nose into the window.

5. *Mr. Tuck’s shoes*

One of Croiset’s statements was: “Does this gentleman have a scar on his big toe?” (Tenhaeff, 1969, 1979b; Eisenbud, 1973). Mr. Tuck, the presumed target person for this item, did not have a scar on his big toe. However, he had ingrown toenails, which caused

9 Hoebens here refers to the article „How I was debunked,“ reprinted as chapter 2-08 in this book. (Eds.)

10 The “*plaisir de se voir imprimé*” was not the only emotion connected with that fateful page. Soon after publication, Dr. Brian Millar pointed out to me that I had made a minor but embarrassing factual mistake in the article.

him to wear steel-toed shoes. He showed these shoes to the audience on the evening of the 23rd. On this, Tenhaeff and Eisenbud agree.

The correspondence is far from striking, as an enormous number of people have trouble with that part of their anatomy. "Scars" on toes or legs are standard items in the repertoire of every soothsayer or tea-leaf reader.¹¹ In Tenhaeff's 1973 article, though, the incident has undergone a startling metamorphosis. "Concerning the second target person," Tenhaeff there claims, "Croiset made the statement that this person had trouble with his toes and, as a consequence, had to have his shoes steel-reinforced." Again, the target person's interpretation has remarkably become part of the psychic's statement.

6. *Mr. Tuck's socks*

"Does this gentleman have green socks with a hole in one?" Croiset had wondered, according to Tenhaeff (1969, 1979b). In the same reports we are told the presumed target person answered: "On the evening of the experiments I wore green socks. When I came home I discovered there was a hole in the heel in one of these socks". Although green socks are not uncommon, and holes in green socks are hardly more uncommon, this hit, as reported by Tenhaeff, certainly smacks of ESP. But it is surprising that Mr. Tuck should not have checked the color and the state of his socks when he had to comment on this item in the first questionnaire. He showed the audience his steel-toed shoes. Why not his socks? If, for some reason, he had preferred not to inspect his footwear for holes there and then, he would at least have noticed whether they were green or not. Yet, according to Eisenbud, he assigned a 0 to this item on the evening of the experiment. But then, according to Eisenbud, both Croiset's statement and Mr. Tuck's response were quite different from Tenhaeff's version. Croiset had referred to wearing green socks "last evening," and Mr. Tuck had commented (in later stages of the "verification"): "Some time before the 23 January meeting I do remember wearing a pair of green socks to work. There was a hole in one sock, in the heel."

7. *Mr. Tuck's sense of humor*

Croiset had described the male target person as a man about 5 ft 9, with dark hair brushed flat back, a gold tooth in his lower set and of a humorous disposition. "This description is correct," Tenhaeff states (1969). Eisenbud confirms the first three parts of this description

11 In the course of his career Mr. Croiset himself has clairvoyantly seen a rather surprising number of "scars." [Also, the standard item of supposed problems with toes, feet or shoes reliably reappears in the "Pirmasens" case; see chapter 3-11. (Eds.)]

but adds: “After several interviews, I would say just the opposite (of humorous.)” Croiset was 75 per cent right, if Mr. Tuck indeed was the target person. Tenhaeff makes it 100 per cent.

8. *Who shook hands with whom?*

Croiset said of the female target person: “After she was seated she stood up again to shake hands with the lady sitting behind her” (according to the instructions, contrived by Croiset himself, physical contact between participants was forbidden, presumably to avoid psychic contamination). Tenhaeff writes (1969, 1979b): “Mrs. O. did not shake hands with the lady sitting behind her, but she did shake hands with a lady in the corridor ...” Not a straight hit, but Croiset had correctly predicted that the female target person (if this was indeed Mrs. Olinger) would disobey the instructions. Eisenbud gives a different story. The instructions about not shaking hands were indeed disregarded, but not by Mrs. Olinger. The offender was a woman who had come with Mrs. Olinger to the meeting.

9. *Who were the target persons?*

In his 1979b account, Tenhaeff states explicitly that both Mrs. Olinger and Mr. Tuck were sitting on the chairs indicated by Croiset. As Croiset did not indicate any chair, Tenhaeff probably means Mrs. Olinger and Mr. Tuck had taken the numbered tickets that were eventually selected by the weather-key method. So, according to the rationale of the chair test, they were the target persons.

In Tenhaeff (1969), however, we hear of a slight complication in the identification of the targets. “It soon appeared that the statements did not apply to the persons thus selected, but to two others, Mrs. Olinger and Mr. Tuck. This caused the suspicion that some sort of error had been committed. A meticulous search showed that 25 people had been allowed to take a ticket, instead of 24. As accurate notes had been taken it was possible to correct the mistake. Now, both Mrs. Olinger and Mr. Tuck turned out to have the correct number after all, so that the experiment could be judged a complete success.” Tenhaeff makes it clear that the “error” referred to was committed on the U.S. side of the Atlantic. He states Eisenbud was instructed to dispense only 24 tickets, numbered 11-34 (this was Croiset’s idea. According to him, 11-34 would be more foolproof than 1-24). Yet Eisenbud had allowed 25 participants to take a ticket. Tenhaeff does not inform us how this error is supposed to have been corrected.

Eisenbud’s version is strikingly different. He quotes in full the letter of instruction Tenhaeff had sent him on 7 January. There Tenhaeff writes: “You must take twenty-four

white cards numbered 10-34.” There was a contradiction in the instructions given by Tenhaeff, as 24 cards cannot be numbered 10-34, but only 10-33 or 11-34. Eisenbud had dispensed cards numbered 10-34, thus a total of 25. The discrepancy was discovered only after the experiment was over. The target number selected for the first series was 34 (Mrs. Olinger held 20). In the second series 20 (Mrs. Olinger’s number) was selected, whereas Mr. Tuck held number 28.

Now there are several things you can do in such circumstances, and Eisenbud discusses some of the options.

- a. You may decide the error invalidates the whole experiment.
- b. You may decide that, as Tenhaeff clearly instructed the experimenters to number the cards 10-34, he really meant a total of 25.
- c. You may reshuffle the cards, now reduced to 24, and try again. This was in fact done in Denver. The first reshuffle (with the numbers 10-33 in the deck) yielded number 30 as the target in the first series and number 28 in the second, Mr. Tuck’s number. The second reshuffle (with the numbers 11-34 in the deck) yielded 31 and 25 as the targets. In both cases, the original weather-key was used.
- d. Another possibility, “to be considered – more as a conceivable ‘unconscious’ rather than a logical design,” to quote the 1973 *JASPR* report, would be the elimination of number 34 from its place in the original order after the shuffling had been carried out with 25 cards, or, assuming that 11-34 was meant, the elimination of 10. Curiously, in both cases this would yield 20 and 28 as the winning numbers.

This last “solution,” which Eisenbud rightly finds highly dubious from a methodological point of view, is presented by Tenhaeff in his 1969 report as the unambiguous correction of an error of which he and Croiset were entirely innocent.

Eisenbud (1973) comes to the unavoidable conclusion that the problem is insoluble, and the question “Who are the target persons?” cannot be answered. Moreover, he notes so many ambiguities in the correspondences claimed by the presumed targets, so many uncertainties in the coincidences reported and so many ways to explain apparent hits without resorting to a paranormal hypothesis that he abstains from a clear final verdict. He certainly did not report a “complete success,” as Tenhaeff (1969, 1979b) implies he did.¹²

12 In his 1979a book Tenhaeff erroneously states that Eisenbud in a January 27 telegram judged both experiments to have been “very successful.”

Discussion

The question of whether Croiset displayed ESP in these tests can, in this instance, only be decided in a subjective way. I can only say that in comparison with the Tenhaeff reports I personally find the Eisenbud paper fails to give me the conviction that anything paranormal was involved. Others, of course, may form quite different opinions. What seems certain is that the hits are in reality not of such a striking nature as to be self-validating.

The purpose of this report, however, is not that of deciding whether Croiset here demonstrated precognition but is instead to draw attention to the discrepancies between Eisenbud's and Tenhaeff's papers.

An honest account of an apparently paranormal event should include all details that might suggest a normal explanation – even if those details could be used by mischievous skeptics for debunking purposes. In Tenhaeff's reports on the Denver chair test, such details are systematically lacking. Instead, many divergencies from Eisenbud's original report may be pointed out and almost all these divergencies tend to credit Croiset with more (and more striking) hits than he actually achieved. Eisenbud (personal communication) has suggested that the discrepancies might be accounted for if Tenhaeff had written parts of his 1969 paper from memory. He further proposes that a conceivable complicating factor may have been difficulties on Tenhaeff's part with his (Eisenbud's) English. There is indeed some evidence that progressive embellishment of memory may have played its part. In general, the earliest (1969) report is most consonant with Eisenbud's and, later, the discrepancies have considerably widened. An apparent exception lies in the German 1976 report, not for home consumption; but here Tenhaeff may well have been more concerned to ensure strict accuracy by consulting his primary sources.

Additionally, Eisenbud (personal communication) has expressed the view that, at least in Tenhaeff's 1969 report, the errors are "trivial and not particularly tendentious." Readers must of course form their own opinions of the magnitude of the discrepancies, but I strongly disagree with Eisenbud's mild judgment. The almost inconceivable degree of sloppiness which seems apparent on Tenhaeff's part indeed forces me seriously to question whether he has been guilty of deliberate manipulation in reporting one of his favorite sensitive's prize cases.

An exchange of letters, a number of telephone conversations and a personal meeting with the Professor did little to shed light on the matter. Tenhaeff refused to discuss the details, insisting that I either believe him or call him a liar.

He protested not only his truthfulness, but his "well known meticulousness" as well. He did, however, state unambiguously that his 1969 report (untranslated) was sent to and was approved by Eisenbud. Unfortunately, Eisenbud, perhaps not surprisingly in a case 11 years old, was unable to confirm or deny this. He could not recall ever having seen it (before I sent him a complete translation), and could not find it in his files. Tenhaeff, on the other hand, did not produce any tangible evidence to support his statement that Eisenbud had given his approval.

Thus this rather important question remains unsolved. However it may be, extreme carelessness or subtle deceit, both alternatives are damaging to Tenhaeff's claim to be a reliable source of information on the paranormal.

Conclusion

It is of the utmost importance to note that the Denver experiment is one of the relatively few instances where Tenhaeff's accuracy can be checked so thoroughly, because a full and independent account of what really happened was published elsewhere. In most Croiset cases, Tenhaeff's reports are the only publicly accessible sources of information.

The question arises as to whether the reporting deficiencies noted here are for some reason unique to the Denver chair test, or whether all Tenhaeff's work is equally suspect. Further investigation on some of the classic police cases reveals that the aberrant Denver reports are not isolated lapses on Tenhaeff's part. In one case (Hoebens, 1981-1982) the generally circumstantial material is supplanted by quite direct evidence of Tenhaeff's fraudulent report. The findings resulting from my research give additional weight to Zorab's caveat (Zorab, 1965) that Tenhaeff's goldmine "contains at least some amount of alloy."

CHAPTER 3-08

Editorial Introduction

Inspired by some (generally wide-spread) misconceptions and errors in an article on “Croiset: The psychic detective” (Stemman, 1981) in the popular British magazine The Unexplained, Hoebens wrote and submitted to the same magazine a short summary of his critique of Croiset and his mentor, Prof. Tenhaeff. This article is reprinted here because for the first time it mentions, even if in passing, several additional cases and experiments that he was to write about in much greater detail on later occasions. The article appeared, in 1983, in The Unexplained, 11, (132), pp. 2630-2633. Hoebens also was to publish several other short articles and correspondences in the same magazine (see the bibliography) before it ceased publication, apparently for economic reasons, at the end of 1983.

Curiously, the paragraph that explicitly refers to Stemman’s earlier article and that had prompted Hoebens to write this note in the first place, was left out by the editor, without explanation.¹ We here have re-inserted that paragraph based on the original manuscript in the Hoebens Files. (Eds.)

CROISSET: DOUBLE DUTCH?

Gerard Croiset, the famous Dutch “psychic detective,” is widely believed to have performed near-miracles in identifying murderers and finding their victims. But, as Piet Hein Hoebens discovered, hardly any of Croiset’s cases will bear close scrutiny. (Ed. [The Unexplained])

If international celebrity is a measure of greatness, Gerard Croiset must rank with the greatest Dutchmen in history. His fame may equal that of Rembrandt, Van Gogh and the boy who saved the land by putting his finger in the dyke. He was called, among other things, “the miracle man of Holland,” “the Dutchman with the X-ray brain,” “the Mozart among the psychics” and “the clairvoyant who never disappointed.”

When Gerard Croiset died on 20 July 1980, aged 71, obituary writers both in Holland and abroad recalled the psychic’s astonishing achievements. He had, they said, by paranormal means located hundreds of missing persons, dead and alive. He had served as an occult Sherlock Holmes to the Watsons of the Dutch police by solving dozens of baffling

¹ To judge by the correspondence in the Hoebens Files, this omission does not seem to have become an issue between Hoebens and the editor of *The Unexplained*. (Eds.)

crimes. In sharp contrast to virtually all other “psychic detectives” he had allowed scientists to monitor his work. He had scored incredible hits in numerous tightly controlled ESP experiments. Two of the most distinguished parapsychologists in Europe, Professor Wilhelm Tenhaeff of Utrecht State University and Professor Hans Bender of Freiburg in West Germany, had vouched for the authenticity of his mediumship.

[The testimonials have been so impressive that even persons not normally given to credulity have tended to accept the claims surrounding Croiset at face value. Non-Dutch authors until recently had little choice anyway: the language barrier would have effectively precluded any attempt on their part to examine the bulk of the evidence. This is why, I think, both in the UK and America otherwise critical students of the paranormal have unwittingly helped to perpetuate a myth (see Stemman, 1981).²]

In Britain, Gerard Croiset was best known for his work as a paranormal crime-buster and finder of missing persons. His attempts to help find the Yorkshire Ripper were widely publicized.

Late in 1979 *Sun* reporter Derek Shuff went to Utrecht to consult the “miracle man.” On Monday 26 November the paper announced that Croiset had “offered vital new information.” He had “seen” that the elusive mass murderer lived right in the center of Sunderland. He reportedly told Mr. Shuff that he was “confident” that his vision had been essentially correct. Perhaps an innocent man is presently serving a life sentence in connection with the Yorkshire murders. If not, Croiset was absolutely wrong. It is, to put it mildly, curious that the psychic had basically done nothing except confirm what was widely – and erroneously – believed at the time: that the Ripper lived in the Sunderland area. Croiset was a master in the stock-in-trade technique of the “prophet”: collect information by normal means and feed it back as “telepathic impressions.”

The most curious part of Derek Shuff’s story is where he writes: “His [Croiset’s] most famous case was to describe the killer of a young Dutch girl who was found dead beside her bicycle.” For some reason, this “famous case” seems to have completely escaped the notice of the Dutch police and the Dutch press. Certainly there had once been a case involving a girl and a bicycle – in Wierden, in 1946. However, the girl was not dead and the chief of the Wierden police has personally debunked some sensationalist accounts in the Dutch press of Croiset’s alleged success in helping to solve the case. Of course, it would be unfair to judge Croiset on the basis of a

2 This bracketed paragraph is included in Piet Hein Hoebens’ original manuscript but did *not* appear in the paper as it was eventually published in *The Unexplained* – conceivably because it included a mildly critical remark on an article (Stemman, 1981) that had been published in the same journal. (Eds.)

tabloid article. We must take into account what purports to be scientifically validated evidence of his unusual abilities.

Croiset's international reputation was largely based on the fact that Wilhelm Tenhaeff – the first professor of parapsychology in the history of western science³ – claimed to have closely examined and carefully documented the psychic's achievements for over a quarter of a century. Alas, Tenhaeff, who died in 1981, was not the cautious, dispassionate and honest scholar his many admirers believed him to be. He was an extremely sloppy investigator who not infrequently indulged in outright manipulation of the data.

For example, at the first International Conference on Parapsychological Studies, sponsored by Eileen Garrett's Parapsychology Foundation, in Utrecht in 1953, Tenhaeff presented a paper on his work with psychic detectives. One of his prize cases concerned an attempt on the life of a Dutch policeman; Croiset had astounded the investigating judge by paranormally "seeing," in specific detail, a metal worker who had been "in some way involved in the attack." In the context of a project on psychic detectives started by Marcello Truzzi's Michigan-based Center for Scientific Anomalies Research (CSAR) I myself have attempted to verify this claim. I discovered that all the specific details Croiset had "paranormally" perceived had been published in the national newspapers *five* days before the séance where the investigating judge supposedly was "astounded." At the 1953 conference, Tenhaeff also forgot to tell his colleagues that the suspect indicated by Croiset had soon proved to be entirely innocent.

Unbelievable – and Untrue?

In 1955, Croiset scored a curious, if ambiguous, success in helping to find a lost set of research papers belonging to a Dutch schools inspector and part-time parapsychologist, J.G. van Busschbach.⁴ Van Busschbach wrote an account of the incident, which was accepted for publication in the Dutch *Tijdschrift voor Parapsychologie* (Journal of Parapsychology). When Van Busschbach received the galley proofs he was amazed to find that the editor – Tenhaeff – had "embellished" the case: so much so that the "misses" had been changed into perfect "hits." For example, Croiset had said the documents would be found on a "three-leg table" in an office that he described in detail. The papers were, in

3 This seemed to be true in the eighties, but must be corrected after Wim Kramer's historical research on Prof. Greven (see Kramer [2006] and the editorial postscript to chapter 3-03). (Eds.)

4 J. G. van Busschbach is best known for his ESP experiments with young Dutch and American school children, see Busschbach (1959, 1961). (Eds.)

fact, found on an ordinary – four-legged – table in an office bearing only a vague resemblance to that described by Croiset. Angrily, Van Busschbach withdrew his article. This did not prevent Tenhaeff from later regaling American journalist Jack Harrison Pollack with the doctored version, which appears in Pollack's biography *Croiset the Clairvoyant* (1964). This book, subtitled *Unbelievable but true!*, abounds with errors, distortions and misrepresentations. It was written under the supervision of Tenhaeff, who checked the manuscript and subsequently repeatedly endorsed *Croiset the Clairvoyant* as a valuable contribution to the parapsychological literature.

At the 1953 Utrecht conference, Tenhaeff also claimed that Croiset had provided vital information concerning a coffee-smuggling affair in the town of Enschede in 1953. According to the professor, Croiset had paranormally “seen” particulars of this affair that were not known to the police but were later to be found correct. A striking hit? I discovered that some of the details “unknown to the police” had been published in the Enschede newspapers, pictures and all, a fortnight before Croiset was even consulted.

In September 1980, Tenhaeff published in the West German magazine *Esotera* a report of what seemed a cast-iron case. The previous year, the professor wrote, commander Eekhof of the Woudrichem police had consulted Croiset in connection with a mysterious outbreak of arson.

All attempts to find the perpetrator had been in vain. Could the psychic provide the vital clue? Eekhof initially refused to believe what Croiset told him, for the clairvoyant's description of the arsonist fitted only one man in the Woudrichem area: a quarter-master in the local police force. Later, however, the commander saw himself compelled to admit that, once again, Croiset had been right. The quarter-master was the arsonist. In his account in *Esotera*, Tenhaeff cited a “protocol checked and signed by Mr. Eekhof.” When I showed the article to the police commander, he positively stated that it was a “fairy tale” and that it contained “outright falsehoods.” Fortunately, he had kept a tape-recording of the consultation with Croiset and could prove that Tenhaeff had fabricated the psychic's amazing “hits.” The “protocol checked and signed by Mr. Eekhof” simply never existed. And when I telephoned Tenhaeff to ask him for an explanation he slammed down the receiver.

It is widely believed that Croiset was the psychic stand-by of the Dutch police. However, Dutch policemen have always been very skeptical of Croiset and other paranormal detectives. The above examples may suggest why this is so.

Apart from his activities as an occult policeman, Croiset's chief claim to fame was his alleged ability to demonstrate precognitive ESP in the so-called “chair tests.” In such

tests, the clairvoyant would try to describe a person who, at some specified time in the future, would happen to be sitting in a specified chair. This “target person” would then be requested to comment on Croiset’s statements. Hundreds of times, it is claimed, the psychic’s precognitive “impressions” turned out to be startlingly accurate.

The Faith that Fakes

But these experiments raised many questions. Usually, the only precaution against fraud was the experimenters’ firm belief that Croiset would not cheat. The well-known and respected Dutch psychical researcher George Zorab has evidence that the “Mozart among the clairvoyants” was not above taking advantage of this loophole now and then.

Croiset’s typical “readings” were sufficiently vague to allow for many possible interpretations. The experimenters, usually Tenhaeff and/or Professor Hans Bender, were wont to encourage the “targets” to keep searching for a correspondence – *any* correspondence. Frequently, the recipients of this encouragement finally came up with interpretations that hardly bore any resemblance to the original psychic statements. On one occasion, during the 1953 Pirmasens chair test – said by Bender to be one of the classics – Croiset had said, “Does this lady live near a red building? In front of this building I see high columns. I also see a high flight of stairs. Is this lady often in this building? I have the impression that there is a hedge in front of the house. The building is somewhat decrepit.”

The “target person” in fact denied *all* this. However, Tenhaeff and Bender hailed another striking hit for, next day, the woman told them that Croiset’s description vaguely reminded her of the chapel in the graveyard at Pirmasens where she had an emotional experience 11 years previously!

In 1981, I reproduced this classic experiment in Amsterdam. I gave my targets the complete set of statements Croiset had made for the 1953 Pirmasens test, pretending that these were fresh ones. Yet these results were even more striking than in the original experiment – the targets “recognizing” descriptions of themselves – which certainly confirms any suspicion that a typical “hit” in the chair test was the result of subjective validation rather than genuine ESP.

The Hot Seat Cools

Moreover, the Pirmasens woman who remembered the emotional experience in the graveyard chapel was *not* sitting in the chair for which Croiset had made his predictions.

This is another problem with these experiments: “displacement effects” were observed with amazing frequency. Sometimes, it was decided that Croiset had really “seen” not the person in the target chair – but a relative of that person, or someone who would have been sitting there if he had not chosen another chair, or a whole *group* of people, or someone who had accidentally touched the target chair. Given the elastic rules of the chair tests, it would have been hard for Croiset not to be a success.

A final reason for not being impressed with the “evidence” from the chair tests is the inadequacies of the published reports. In a couple of instances it has been possible to compare the published accounts with independent sources – and usually with the result that curious omissions and discrepancies have been discovered.

Tenhaeff repeatedly tried to impress the Dutch public with the astonishing results of the 1969 transatlantic chair test in which Croiset, in Utrecht, made the psychic statements, and Dr. Jule Eisenbud, in Denver, Colorado, interrogated the targets. In several Dutch publications, Tenhaeff reported “hits” that would seem to defy rational explanation. However, a comparison with Eisenbud’s original account revealed that Tenhaeff had suppressed a number of “misses” (while indicating that this report was exhaustive), that he had ignored some procedural errors that had largely invalidated the experiment and that he had invented a number of “hits” that Croiset, in fact, had never scored. For example: Tenhaeff claimed that the target person, during the evening of the experiment, had worn green socks with a hole in one of them – exactly as Croiset, thousands of miles away, had paranormally “seen.” But was this ESP? The truth is more prosaic: that particular evening, the target did *not* wear green socks with a hole in one of them.

Of course, it is impossible to disprove each and every one of Croiset’s alleged psychic feats. The “miracle man of Holland” may indeed have been a genuine clairvoyant – however I, and other investigators, have compelling reasons to urge extreme caution in accepting the “evidence” presented for his achievements at face value.

CHAPTER 3-09

Editorial Introduction

The following article, though originally published in the Dutch daily newspaper De Telegraaf of which Hoebens was an editorial writer, seems to be by far the fullest, even if journalistic, published account of the Woudrichem arsonist case. This Dutch article, entitled “De pyromaan en de paragnost. Hoe professor Tenhaeff een politie-verklaring ‘bewerkte,’” first appeared in De Telegraaf, 18 October, 1980, p. T5.

An English version of the article (translated by Hans Michels and Brian Millar) was published, in 1989, in the SRU Bulletin, 14, 93-96. This reprint is based on the latter source but was again counter-checked with the earlier Dutch version. (Eds.)

THE ARSONIST AND THE PSYCHIC – OR HOW PROFESSOR TENHAEFF “EDITED” A POLICE STATEMENT

A word of caution

I shall not mince words: This paper unmistakably shows that Prof. Tenhaeff faked his evidence. Unfortunately, the present case is not the only example of this kind of cheating. And I do not claim this without adequate justification.

For many Dutch people Tenhaeff has always been the personification of parapsychology. Just for this reason I want to add a word of caution to these revelations. Tenhaeff and his methods are not representative of (Dutch) parapsychology. There are parapsychologists who try in an honest and intelligent manner to make parapsychology into a real science.

It would be highly unfair to associate these honest scientists with abuses such as the Woudrichem case.

In 1977 and 1978 the charming village of Woudrichem, situated in the southern part of the Netherlands, was alarmed by a series of mysterious fires. It was clear these fires were the work of an arsonist, but for several months the police tried in vain to find the culprit. The eventual outcome was a sad surprise: in March 1980 a police sergeant was arrested and confessed he had raised the fires.

September 1980: The German popular monthly magazine *Esotera* unveils that the psychic Gerard Croiset, who died on the 20th of July 1980, played an important role in unmasking the arsonist. Not a single Dutch newspaper had published this news.

The author of the report in *Esotera* is not just anybody: it is Prof. Dr. W.H.C. Tenhaeff, professor emeritus at Utrecht University, honored by the pope and editor in chief of the *Tijdschrift voor Parapsychologie*, a man whose scientific achievements have been compared to those of Einstein by the American parapsychologist-psychiatrist Dr. Berthold Eric Schwartz.

According to Tenhaeff when the police-investigations had come up against a blank wall, police-officer Eekhof had taken up contact with Croiset “in the hope that he could provide the authorities with the crucial clue needed to find the arsonist.”

Statement

Some weeks before Croiset’s unexpected death, Tenhaeff, Croiset and his secretary Mr. D. West had visited the Woudrichem police station to “hear police officer Eekhof recount in full detail the success of his visit to Croiset.” West recorded Eekhof’s statement on videotape and a transcript was made, which was “checked by Eekhof and signed to indicate his approval.” Tenhaeff made an excerpt of the transcript, parts of which follow.

“Uniform”

“When Eekhof visited Croiset on the 15th of November 1979 the latter stated: ‘I get the impression that the culprit has something to do with little aircraft, toy-aircraft ...’. When Eekhof asked whether Croiset perhaps meant model-aircraft, Croiset confirmed this. Croiset also said that the man lived in a flat, approximately 200 meters from a food-producing plant. Lastly he saw that the arsonist sometimes wore a uniform.”

“Eekhof was shocked because this description fitted a Woudrichem policeman, who had shown particular diligence in the search for the arsonist. At first Eekhof thought that one person might have been mistaken for another, an error psychics sometimes make. Later on, however, he was forced to accept that Croiset’s description was right: the policeman was the arsonist.”

So much for Tenhaeff’s summary. He added that the policeman had indeed built model-aircraft. At first sight, one might say that this is convincing proof of Tenhaeff’s claim that psychics can sometimes solve crimes.

14th October 1980: In the police station of Woudrichem commander D. Eekhof reads the *Esotera* report for a second time. The look on his face clearly reveals his astonishment. At last he exclaims: "What kind of con-trick is this? This story contains downright falsehoods!"

What did really happen? In the autumn of 1977, when fires were being raised, anxious inhabitants of Woudrichem started to make plans to assist the police. Some thought it a good idea to consult clairvoyants.

Description

As a consequence of such a citizens' initiative Mr. Eekhof found himself in the consulting-room of Croiset in Utrecht, on the 15th of November 1977. Eekhof accompanied a private citizen from Woudrichem. Over the telephone the psychic had already given this person a description of the culprit: a man, aged between 18 and 20, approximately 1.72 m tall, dressed in a blue overall, black beret, blond hair, does many odd jobs, has a scar on the right, sorry, left eyebrow.

On this basis the police had kept an eye on a young man who, to some degree, fitted this description.

But it turned out that this man had a perfect alibi. This was told to Croiset on the 15th of November.

Aircraft

Only after being informed about this did Croiset say that the man he had described was not the arsonist, but that the man did know who the arsonist was. The real perpetrator worked on a farm, did not drive a car but a moped with a blue petrol tank, lived in a house "where there are milk churns" and his name was Johan.

Suddenly Croiset asked Eekhof: "There is no airport in that neighborhood where advertising planes are used, is there? Have you been in an aircraft recently?" Eekhof answered that sometimes crop-spraying planes were flown nearby. Croiset: "It must have something to do with aircraft or building aircraft. He sat in a plane once. So this man must have something to do with aircraft. It could be crop-spraying planes."

Commander Eekhof asked whether it could be model-aircraft. Croiset: "Yes, that's also possible, but in any case ..., no, they are large aircraft."

After this statement the clairvoyant looked at some photographs Eekhof had brought with him. Eekhof told him that one of these photographs showed a man whose name had earlier been mentioned in connection with the fires. Croiset then said that this man was the arsonist and gave some advice on how to get him to confess.

Croiset's suspect number 2 was not the police sergeant. The information the psychic gave to the police did not help them at all at that point.

Eekhof, who had recorded on tape the consultation with Croiset, labels the *Esotera* report a sensational cock-and-bull story. The *Esotera* report includes the following manifest liberties with reality:

- The visit to Croiset did not take place on the 15th of November 1979, but two years earlier. When Tenhaeff visited Eekhof on the 19th of March 1980, Croiset was not present, although his secretary was.
- During the visit to Croiset on the 15th of November Croiset did not mention “uniforms.” As a matter of fact he never used this word (which, had it been used, would have been a fine “hit.”)
- Croiset did not mention “toy aircraft.” When he was asked whether it might concern “model aircraft,” he first confirmed, but then denied this, saying they are “large aircraft.”
- Croiset's description did not astonish Eekhof at all, for he did not recognize the police sergeant from it. Only several months later was the police sergeant suspected for the first time and this had nothing to do with Croiset. When Croiset's tape-recorded statements had been played back at the Woudrichem police station it turned out that the police sergeant in question had been one of those present! In the description Croiset had given, practically nothing was right, apart from the fact that the perpetrator indeed built model aircraft and was interested in aviation. Concerning this specific topic, however, Croiset had, to put it mildly, not been very clear. It was not Croiset, but Eekhof who first spoke about “model-aircraft” and eventually the psychic denied it as a possibility.
- The *Esotera* report fails to inform us that Croiset had earlier given a description of the arsonist over the phone, which was completely false and had later pointed out a man in a photograph who was also not the culprit.
- Tenhaeff claims to have given an “excerpt” of a transcript signed by Eekhof. Commander Eekhof: “I never saw any transcript at all, let alone signed it.”

Conclusion: there is remarkably little in Tenhaeff's story which is true.

Falsified

Commander Eekhof: "I can at best say that among all the statements of Croiset there were a couple of which one might later say: 'Hey, that's odd.' Like the aircraft for example. Another thing: one of the locations he pointed out on the map was approximately the place the sergeant used to land his boat. But this is all after the fact. The information he gave us on the 15th of November in no way helped us to find the fire raiser. When the sergeant was already seriously suspected, but it still was difficult to prove that he really was the arsonist, some policemen of this station considered Croiset's activities a kind of moral support. One could then hear policemen say: 'That Croiset did tell us about little aircraft!' I was willing to talk with Tenhaeff and West because I thought it might be of scientific interest to them. It is a disgrace to find the whole story falsified in this outrageous way for the publicity!"

15th October 1980: One day after my conversation with Eekhof the *Tijdschrift voor Parapsychologie* drops on my doormat. This journal also contains the Woudrichem story. Author: Tenhaeff again. Nevertheless: a totally different version. No "uniform" is mentioned, I cannot find any "signatures" under "transcripts." What I do find is a lot of serious errors, but the version in the *Tijdschrift voor Parapsychologie* is a great deal more accurate than the German edition.

Communication by phone with Eekhof: "It is still a cock-and-bull story, but there are indeed less distortions in it than in the German version."

Later that same day a phone call to Mr. West. He has the videotape with Eekhof's statement. Would he mind answering some questions? West: "Given my position I can't help you. In some sense I am in the service of professors. I can't get involved in it. If you would like to see the videotape, I should need at least the written permission of Professor Tenhaeff."

A little bit later: Prof. Dr. W.H.C. Tenhaeff picks up the telephone. "I consider it useless to speak to you! Everything is exactly as told by the commander! There is a tape! I copied it literally!" "But, Professor, Mr. Eekhof can NOT confirm that."

Tenhaeff: "If you think I'm a liar, then you must publish that. I don't give a d... about what you'll publish. Thou art an INSINCERE human being!"

After which the Einstein of parapsychology smashes down the receiver.

Tenhaeff in his *Esotera* report: "In my files there are hundreds of such cases." I am afraid he is right.

CHAPTER 3-10

Editorial Introduction

In the following paper Hoebens presents critical commentaries on an article, “De paragnost Gerard Croiset,” that Prof. Wilhelm Tenhaeff had published, in 1981 (a few months before his death), in the respected Dutch medical journal Arts en Wereld [The Physician and the World]. The manuscript of Hoebens’ paper, entitled “De euvele moed van W.H. C. Tenhaeff,” was found, along with a cover letter to the editors of Arts en Wereld, in the Hoebens Files.

As far as can be ascertained on the basis of Hoebens’ comprehensive files, there is no indication that the editors of this medical periodical ever deemed Hoebens’ manuscript worthy as much as a simple response or acknowledgement. Inquiries we made in the mid-1980s and, again, in 2008 also received no response, and Arts en Wereld today may be defunct. For all we know, the manuscript has remained unpublished to this day. With the friendly assistance of Robin Moore, the editors translated Hoebens’ manuscript into English. Presumably, it is published here for the first time in any language. (Eds.)

W.H.C. TENHAEFF – DELUSIONS OF SCHOLARLY GRANDEUR

One of Prof. Dr. W.H.C. Tenhaeff’s favorite quotations, originally attributed to Professor H.J. Pos,¹ is “dat de ervaring ons heeft geleerd, dat in de tegenwoordige cultuur geen enkele geestelijke prestatie, hoe hoog ze ook boven de bevassing van de meest ontwikkelde liggen moge, gevrijwaard is van puur emotioneel gegronde verdachtmakingen [that experience has taught us that in today’s culture no intellectual performance, no matter how much it may defy the comprehension of the most erudite people, remains free from purely emotionally based insinuations].” The contexts in which Tenhaeff tends to use this piece of wisdom demonstrate that he considers his own parapsychological activities as intellectual performances on the highest level, and critique, correspondingly, as no more than irrational insinuations.

Recently, several media regularly offered Tenhaeff opportunities to address his critics as infamous liars, while continually adulating himself. I found a remarkable example of this in “De paragnost Gerard Croiset [The paragnost Gerard Croiset],” an article he published

1 Hendrik J. Pos (1898-1955) was a linguist and professor of philosophy at the University of Amsterdam; see Daalder & Noordegraaf (1990). (Eds.)

in a recent issue of the medical journal *Arts en Wereld*. In it, the professor first treats the reader to the unavoidable quotation by Pos, and then complains that now and then the daily newspapers present the “reverberations” of “the era of unreasonable critique and slanderous accusations.” From a recent radio broadcast I learned that Tenhaeff also classifies my own publications in, among others, *De Telegraaf*, as “slanderous accusations.”

I must admit: Tenhaeff has got guts! He knows perfectly well that in the meantime a considerable amount of evidence has been gathered that unmistakably demonstrates that his scientific pretensions are based on bragging and deception. Nevertheless, he continues, irritably, to present himself as the victim of a mean smear campaign. At the same time, he has his professorship and he is allowed to propagate his points of view in a respected medical journal such as *Arts en Wereld*, obviously speculating that this combination will impress the general public to such a degree that he will not have to expect any nasty questions from these quarters.

As an antidote against this deception of the public (and I am not at all reluctant to use this term), I want to hold up against the light some of the things Tenhaeff is claiming in his article.

The Dream

Tenhaeff tells his readers how Maetsuycker (the Dutch General Governor in the Indies in the 17th century) had a dream in which his friend Vlamingh died in a shipwreck. Maetsuycker, so the story goes, asked his private secretary to note down this vision and the dream. The document was handed to “de Heren Zeventien” (the Board of the East-Indian Company). Several months later it became clear that Vlamingh and his ship went down with all hands, “at the moment of the vision.” Tenhaeff writes: “Everything happened as it was seen in the vision. The document in possession of ‘de Heren Zeventien’ proves it. In this case we cannot speak of a fairy tale.”

In 1953, this same story was the topic of the professor’s inaugural address in Utrecht. However, in December of that very same year the historian Dr. P. Spigt, in the journal *De Nieuwe Stem. Maandblad voor Cultuur en Politiek* (Spigt, 1953), a monthly for culture and politics, published an article in which he demonstrated in sufficient detail that Tenhaeff’s inaugural address was mainly based on blunders. The “properly certified historical case of spontaneous ESP” (Tenhaeff) was not certified at all. The only source for the story is a short note in the chronicles of Valentyn, an author who is not particularly well-known for his accuracy. These chronicles were published no less than sixty years after the alleged vision in the dream. On top of that, Tenhaeff even managed to read the short note incor-

rectly. In his inaugural address, he attributed the dream to Springer, a bargeman from the village of Enkhuizen. In later versions this detail was tacitly corrected without mentioning his earlier mistake.

Tenhaeff briskly continued to repeat all the other boastings, however. Time and again he suggested that there was a “document” (the transcript of Maetsuycker’s dream), serving as evidence that this case is no fairy tale. He keeps silent about the fact that this “document” has never surfaced, and that no trace of it was ever found in the annals of the era in question. The claim that “a couple of months later” it was confirmed that the contents of the dream manifested itself in minute detail is totally unfounded. The date of Vlamingh’s death was never confirmed. It even remains uncertain whether he actually died in a shipwreck. Two other minor details: The quoted part of Valentyn’s chronicles does *not* mention that Maetsuycker dictated his dream to his private secretary, and the ‘Heren Zeventien’ are completely absent in those chronicles.

After being caught making these kinds of blunders, any earnest scientist would be so ashamed that he would rather sink into the ground. Not so W.H.C. Tenhaeff. Almost thirty years after Spigt’s revealing publication, Tenhaeff indefatigably continues to sing the praises of old nonsense tales, presenting them as “properly certified historical cases.”

The Chair Test

In 1969, clairvoyant Gerard Croiset took part in a transatlantic chair test. The tale was that he had given stunningly correct details about two persons as yet unknown who would, on a later moment in time, be chosen by lot to be seated in specific chairs in an audience hall in Denver. Last year, I did extensive research on this showpiece.

I cannot but conclude that Tenhaeff systematically distorted the facts found by the main experimenter, Dr. Jule Eisenbud. Tenhaeff made his Dutch readers falsely believe that this experiment was a huge success. This is not the place to go into the details. Those who are interested can find them in my “In praise of meticulousness.”²

The Slanderers

Without mentioning any names, Tenhaeff accuses people to be slanderers, who want to insinuate that “the police statements used in some of my writings” were the results

2 See chapter 2-04 in this volume. Many more details will be found in chapter 3-07, written and published long after the manuscript for the present paper was completed. (Eds.)

of bribing police officers. He refers to the statements dealing with Croiset's activities as a psychic detective. In 1969, in an article published in his own journal, the *Tijdschrift voor Parapsychologie*, Tenhaeff wrote that one of the sources of these insinuations is the British psychologist Prof. Dr. C. E. M. Hansel, author of the skeptic book, *ESP: A Scientific Evaluation*.³ The truth is that Hansel never wrote something that might justify Tenhaeff's accusations. Tenhaeff simply fabricated an accusation addressed to himself. The same goes for the large number of other remarks that Tenhaeff first attributes to Hansel and then "refutes" in his journal.

As for the police statements mentioned above: In September 1980, Tenhaeff claimed, in the German occult magazine *Esotera*, that the Croiset of blessed memory exposed the arsonist of Woudrichem by means of clairvoyance. He calls upon police commander Eekhof. According to Tenhaeff, Eekhof had told him that he confirmed Croiset's performance, and Eekhof was also prepared to confirm the minutes of that conversation in writing. In *De Telegraaf* of 18 October 1980, I revealed the ins and outs of the matter. Tenhaeff had distorted the facts in a really unimaginable way. The signed minutes did not exist at all! For this kind of deception of the public there is only one appropriate word: fraud.

It is very characteristic of the disgraceful and appalling courage Tenhaeff has shown during his entire career that this exposure was no reason for him to step down from active parapsychology once and for all. Worse still; his impertinence seems to be so strong as to claim, in a recent radio broadcast, that my exposure was based on "gross lies." The journalists of the broadcasting network NCRV considered it unnecessary to tell their audience that they had spoken on the phone to police commander Eekhof, and that the latter fully confirmed that the facts in my own report in *De Telegraaf* were correct.

Parapsychology and "Unbelief"

As a sly demagogue W. H. C. Tenhaeff has always tried to neutralize any critique by casting doubt on the good faith and the mental health of the critics. He likes to portray himself as the one and only representative of The Truth, continually working hard to achieve great mental performances and thereby causing feelings of displeasure in some *mala fide* individuals who consider such a high level of scientific serenity as a reprimand for their own banality. Many "negativists" were advised by Tenhaeff to consult a psychiatrist to be cured from their repressed complexes which are supposed to lie at the roots of their

3 Hansel (1966). (Eds.)

“reactions of unbelief.” There is no point in starting a discussion with these morally and mentally deficient persons, according to the Professor. “Dat deze negativisten associatief verwant zijn aan personen die ontkennen dat er in Duitsland ooit concentratiekampen zijn geweest of Jodenvervolgingen hebben plaatsgevonden, behoeft wel geen betoog” [It will be clear that these negativists are associatively related to those denying there have ever been concentration camps or persecution of the Jews in Germany], so he writes in his *Tijdschrift voor Parapsychologie* (Tenhaeff, 1980c, p. 131). Unbelievable? If you have any doubts, feel free to check it.

Most skeptics will not feel personally offended by such distasteful attacks, since they know from what quarter they originate. The real victims of Tenhaeff’s impertinence are the *bona fide* parapsychologists. For them, it must be an annoyance that a large part of the general public in the Netherlands still considers Tenhaeff to be a leading parapsychologist, and that his publications in respected journals such as *Arts en Wereld* contribute to the persistence of this misunderstanding.

For the time being, I have doubts as to the existence of phenomena such as clairvoyance. Nevertheless, I feel great sympathy for those parapsychologists who are trying, in honorable and intelligent ways, to transform their controversial field into a respected science. They do not deserve to be lumped in the same box with Tenhaeff.

CHAPTER 3-11

Editorial Introduction

The quasi-experimental Pirmasens chair test, conducted in 1953, seemed to make a stronger prima facie case for “paranormality” than the Denver chair test discussed in chapter 3-07 of this book. However, even Bootsman (1995, pp. 32-37), in the very journal founded by Tenhaeff and edited by him for decades, had felt obliged to admit that “de conclusies van Hoebens omtrent de twijfelachtigheid van Tenhaeffs beweringen ervoor gezorgd [hebben] dat Tenhaeffs werk [...] het laatste restje van glans verloor [Hoebens’ conclusions on the doubtfulness of Tenhaeff’s claims gave the final blow to the results of Tenhaeff’s research]” (p. 37). With the Pirmasens experiment, Tenhaeff had not been the only experimenter present and responsible for the test. Therefore, the main experimenter, Hans Bender, in 1981, had specifically challenged Hoebens to find faults with that test’s design or evaluation. The paper to follow here presents Hoebens’ findings in considerable detail.

The paper was originally written in English and translated into German by Eberhard Bauer. It was published, in 1984, under the title “Abschied von Pirmasens,” in the Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie (vol. 26, pp. 4-28).¹ Below it is published in English for the first time. The Hoebens Files contain about half a dozen very slightly varying (but mostly undated) versions of the original English manuscript. We have compared all of these variants to the published German text and also have taken into account the discussions about proper wordings found in the letters Hoebens and Bauer exchanged during the translation process. In (very few) doubtful cases, we have always chosen the English wording that we felt best fits the one that was eventually published in German.

Moreover, the published German paper, throughout the text, contains many citations of varying lengths from original German publications, tape recordings or transcripts. These are also quoted in their original German versions in the various stages of Hoebens’ English “Pirmasens” manuscript. The paper, after all, was written for a German readership so there was no need for Hoebens to provide English translations of such quotes even in his English manuscript. Since most readers of this book, presumably, will not be sufficiently familiar with the German language, we have added full English translations for all German quotations.

1 Bender (1984a) replied only very briefly, and in general terms, in the same issue of the *Zeitschrift*. Ulrich Timm (1984b) also entered the discussion, but more or less concentrated on a statistical re-check of the Pirmasens chair test based on the 1953 data that Bender had published in 1957.

What Hoebens termed his “counter-experiment,” that is presented in the final part of this paper and used Croiset’s original 1953 statements for substitute target persons in 1981 Amsterdam, has received some private criticism from Hoebens’ friends and colleagues in parapsychology, presumably because it mimicked or perpetuated, rather than improved on, the Benderian and Tenhaeffian methods he had set out to criticize in the first place. Readers are invited to decide for themselves whether (and, if so, in which respects) this detracts from the appositeness of Hoebens’ specific criticisms. (Eds.)

FAREWELL TO PIRMASSENS

A CRITICAL RE-EXAMINATION OF A SUCCESSFUL ESP EXPERIMENT

Abstract

Recently, Hans Bender effectively challenged his critics to refute his claim that the results of the 1953 “chair experiment” with Gerard Croiset in Pirmasens can only be accounted for if the hypothesis of a paranormal precognitive ability is accepted. A critical analysis of the experiment and a close examination of both the published and the unpublished documents do not support Bender’s assertion. The design of the test is shown to have been irredeemably flawed, the experimenters having failed to specify in advance their criteria for distinguishing “hits” from “misses” and having completely ignored the distorting effects of subjective validation. Comparison of the published reports with the “raw data” shows the former to contain an inexplicably great number of errors and omissions. A “counter-experiment” conducted by the author in 1981 in Amsterdam shows that, given the loose experimental conditions adopted by Bender and Tenhaeff, Croiset’s original “precognitive statements,” intended to apply to participants in a 1953 meeting in Pirmasens, twenty-eight years later are easily interpreted as applying to substitute target persons in a different country.

In a recent issue of this journal, Professor Dr. phil. & med. Hans Bender published a re-appraisal of the Pirmasens case, in his view one of the classics among the so-called chair tests with the well known Dutch sensitive Gerard Croiset.² The purpose of this re-appraisal was twofold. First, Bender wished to render homage to Croiset and his mentor, Professor Dr. W.H.C. Tenhaeff, both of whom had recently died. Second, he wished to

2 Bender, H. (1981b). Pirmasens 1953 – Retrospektive auf ein Platzexperiment mit Gerard Croiset. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 23, 219-230.

contain the damage possibly caused by the publication of my “Vom Lob der Genauigkeit in der Parapsychologie [In Praise of Meticulousness in Parapsychology].”³

In that essay-review I have drawn the readers’ attention to a number of regrettable inaccuracies in Tenhaeff’s reporting of supposedly evidential cases. My tentative conclusion that the flaws I had discovered might be more than isolated lapses on Tenhaeff’s part was amply confirmed by the outcome of further investigations.⁴

In his re-appraisal, Bender does not comment on the main part of “Vom Lob der Genauigkeit” – an analysis of the discrepancies in Tenhaeff’s various accounts of the celebrated transatlantic chair test of 1969. Instead, he concentrates on the few paragraphs where I point to what seems to be comparatively minor embellishments in Tenhaeff’s rendering of the Pirmasens case.

Bender does not dispute the accuracy of my criticisms. However, he insists that my findings (“kleine Unstimmigkeiten [minor discrepancies],” “bei Bagatellaussagen [concerning trivial statements]”) in no way detract from the value of the Pirmasens test. He firmly states that the results of that experiment were such as to “astonish any unprejudiced person.” The implicit challenge has not escaped my notice.

Professor Bender has greatly facilitated my work as a critic. First, he has unambiguously committed himself to defending the view that the Pirmasens case is a *representative* example of the sort of evidence that would establish the reality of precognitive ESP. Second, he has allowed me, even encouraged me, to examine what remains of the original documents in the case. At this point, I wish to express my appreciation for his courteous helpfulness.

In order to be consistent with what I have written earlier⁵ I must emphasize that I do not believe that the matter of “psi” can ever be settled in disputes over anecdotal

3 Hoebens, P.H.: Vom Lob der Genauigkeit in der Parapsychologie. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 22, 1980, 225-234. [English versions of that paper and of all the other Hoebens articles referred to in the present chapter are reprinted in this book. (Eds.)]

4 Compare my articles, Hoebens, P.H.: Gerard Croiset: Investigation of the Mozart of “Psychic Sleuths”. *Skeptical Inquirer*, 6, 1981, (1), 17-28; Hoebens, P.H.: Croiset and Professor Tenhaeff: Discrepancies in claims of clairvoyance. *Skeptical Inquirer*, 6, 1981-1982, (2), 32-40; Hoebens, P.H.: Mystery Men From Holland, II: The Strange Case of Gerard Croiset. *Zetetic Scholar*, no. 9, 1982, 21-32.

5 Hoebens, P.H.: Die Legitimität des Unglaubens. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 24, 1982, 61-73; also published in Bauer, E., & Lucadou, W. von (eds.) (1983), *Spektrum der Parapsychologie. Hans Bender zum 75. Geburtstag* (pp. 118-130). Freiburg i. Br.: Aurum Verlag.

evidence. A hypothetical failure on my part to “explain” or “debunk” Pirmasens would not amount to a vindication of the Paranormal. Such cases could, conceivably, be flawed in ways no longer detectable. On the other hand, convincing exposures of parapsychological prize cases are quite insufficient to discredit “psi” as a legitimate (proto-)scientific concept. Rather, such exposures serve as arguments for extreme caution in accepting “paranormal” evidence at face value. It is with this proviso that I have taken up Bender’s gauntlet. In this re-examination I will critically discuss (1) the design of the experiment, (2) the logic of Bender’s evaluation, (3) the quality of Bender’s reporting. In addition, I will present the results of a “counter-experiment.” I assume that the reader is familiar with Bender’s 1957⁶ and 1981 accounts. Nevertheless, a brief summary of the case and of the controversy surrounding it seems desirable.

Pirmasens Summarized

In June 1953, Gerard Croiset made a number of statements intended to apply to a person who, later that day (June 3), was to occupy seat no. 73 in a Pirmasens class room, where a demonstration of the chair test was scheduled.⁷ He gave a further reading intended to apply to the owner of an “inductor” object which the target person in the first test should pick up at random from several to be collected from the participants prior to the demonstration. According to Bender (1957), both readings turned out to be amazingly apposite, even though something in the nature of a “displacement effect” was noticed in the first part. Bender, the chief experimenter, published the results in the very first issue of *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*.⁸ A slightly different account was published by his co-experimenter Tenhaeff in the Dutch book *De Voorschouw*.⁹ This version seems to have been the model for an English account in Martin Ebon’s *Prophecy in Our Time*.¹⁰ In 1959, the German skeptic Carl Pelz criticized parts of Bender’s report in *Kosmos*.¹¹ In that magazine Bender published a brief, general

6 Bender, H. (1957). Praekognition im qualitativen Experiment. Zur Methodik der “Platzexperimente” mit dem Sensitiven Gerard Croiset. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie* 1, 5-35.

7 The demonstration was part of a series of lectures on parapsychology.

8 See note 6.

9 Tenhaeff, W.H.C. (1979a). *De Voorschouw – Onderzoekingen op het gebied van de helderziendheid in de tijd*. 4th, rev. ed. Den Haag: Leopold.

10 Ebon, M. (1968). *Prophecy in Our Time*. New York: New American Library.

11 Pelz, C. (1959). “Herr Croiset, Sie können nicht hellsehen!” Der große Irrtum der Parapsychologie. (1. Teil). *Kosmos*, 55, 377-383. Pelz discusses only a few items of the first part of the experiment.

reply.¹² He ignored Pelz' criticisms in his subsequent accounts of the case, notably in *Unser sechster Sinn*.¹³ In 1981, Bender re-visited both presumed target persons. The interviews were tape-recorded. Later that year, the re-appraisal was published, with an editorial postscript to the effect that the present author had been invited to publish a reply. In June 1982, I had the opportunity to travel to Freiburg and examine the documents preserved at the Eichhalde Institute.¹⁴ A brief account of my findings was included in my paper "The Hume Game", presented at the August 1982 SPR/PA Centenary Jubilee Conference in Cambridge.¹⁵

The Design of the Experiment

In my opinion, some glaring defects in the experimental design are in themselves sufficient to invalidate all claims that the Pirmasens séance may be regarded as a scientific demonstration of ESP. No unambiguously formulated hypothesis stood to be tested. No clear criteria of success or failure were agreed upon in advance. No precautions were taken to insure that only a "paranormal" factor could account for an apparently positive outcome. What the experimenters basically did was to wait and see and afterwards claim that what they had observed met their tacit criteria of success.

Even in 1953, Bender and Tenhaeff must have been aware of certain pitfalls in evaluating psychic "readings." For years, critical researchers had argued that non-paranormal psychological mechanisms could generate striking but spurious "hits" in such tests. First, there is the tendency of subjects to comply with the "demand characteristics" of the situation, that is, to try and live up to the experimenters' expectations.¹⁶ Second, there is the phenomenon now known as "subjective validation," that is, the common tendency to see meaningful patterns in random stimuli and then stick to the belief that those patterns are objectively part of the stimuli. In his impressive monograph on the work of Hettinger,

12 Bender, H. (1960). Parapsychologie, Wissenschaft und Aufklärung. *Kosmos*, 56, 195-197.

13 Bender, H. (1971). *Unser sechster Sinn*. Stuttgart: Deutsche Verlagsanstalt, pp. 81ff.

14 Now generally known as the IGPP, no longer situated on the once-famous and spectacularly romantic Eichhalde, in Freiburg, Germany. (Eds.)

15 Hoebens, P.H.: Time machines, The Hume Game and a successful replication of a classic ESP experiment. *Proceedings of the 25th Annual Convention of the Parapsychological Association, Cambridge, August 1982* [abstract published in Roll, W.G., Beloff, J., & White, R.A. (eds.), *Research in Parapsychology 1982* (pp. 15-17). Metuchen, NJ & London: Scarecrow Press.]

16 In the Pirmasens case, the participants had previously been subjected to lectures on parapsychology. Prior to the demonstration, Bender had explained the chair test method to them. It is clear what this implied for the demand characteristics of the situation.

Scott¹⁷ forcefully argued that no “hit,” however striking, can be reasonably attributed to ESP unless a number of elementary precautions are taken. To mention just the most important of these:

1. The target person should be identified only after every participant in the experiment has completed his or her attempts to find a suitable interpretation. After the target person is identified, *rien ne va plus*.
2. The questioning of the candidates must be handled by someone ignorant of the actual targets.
3. The target material must be thoroughly mixed up with a great number of control statements indistinguishable from the originals.

In brief, neither the target person nor the experimenter must know what can be done in order to help the psychic to obtain higher scores. Only if, in a repeated series of well controlled sittings, the actual target persons consistently select those statements in fact meant to apply to them, one may surmise that a paranormal factor may have been responsible for the results. None of these conditions were met in the Pirmasens chair test, even though Scott’s paper had been available since 1949.¹⁸

The Logic of Bender’s Evaluation

In both his 1957 and 1981 reports, Bender insists that the results of the Pirmasens chair test are so amazing as to be self-validating. That is to say that no amount of criticism of “Teilaspekte [partial aspects]” of the case could possibly affect the general conclusion that Croiset performed a genuine feat of precognition. In both articles, Bender introduces the stereotypical skeptic who, ignoring the “gestalt characteristics,” takes the medium’s statements to pieces and then proceeds to explain the individual pieces as “coincidence.” This skeptic is dismissed in a sentence that sums up Bender’s chair test philosophy and therefore deserves to be quoted in full: “Wer so argumentiert, verkennt in einer Art künstlicher Gestaltblindheit, daß hier Konfigurationen, Strukturzusammenhänge in Frage stehen, die nicht für beliebige Personen ausgesagt worden sind, sondern in einer spezifischen, eine paranormale Leistung fordernden und provozierenden Situation für Personen, die

17 Scott. C. (1949). Experimental object-reading: A critical review of the work of Dr. J. Hettinger. *Proceedings of the Society for Psychical Research*, 48, 16-50.

18 In some of the later chair tests, less crude methods were used, although Scott’s conditions were never fully met. See Timm, U. (1965). Die statistische Analyse qualitativer paragnostischer Experimente. *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, 8, 78-122.

bei einer bestimmten Veranstaltung auf einem bestimmten Stuhl oder zumindestens in dessen Nähe sitzen werden. [This argument, in a kind of artificial gestalt blindness, fails to recognize that here we are dealing with configurations, with structural correlations, that are not meant to apply to any person, but rather refer to persons who, in a certain situation that requires and stimulates a paranormal performance, are seated in or close to a pre-defined chair.]”¹⁹

In Bender’s “holistic” approach, the critic is denied the right to attack the experiment on the level of the individual hit (although the parapsychologist may cite individual hits to support his pro-ESP conclusions.)

An example: In the first part of the Pirmasens test the presumed target person was requested to confirm or deny the following statement: “Wohnt diese Dame in der Nähe eines roten Gebäudes, vor dem hohe Säulenträger sind? Ich sehe auch hohe Stufen. Kommt die Dame viel in dieses Gebäude? (etc.) [Does this lady live in the vicinity of a red building with tall front columns? I also see a high staircase. Does she often visit that building?]” The presumed target person in fact denied every part of this item. According to Bender, this is irrelevant, for finally she came up with a “symbolic” interpretation fitting the “meaningful pattern.” A skeptic who might wish to protest that, in this way, every conceivable psychic prediction could be made to match every conceivable target is rebuked for having failed to take into account the “meaningful pattern.” Bender’s reasoning is entirely based on a fallacy. He assumes that the “Strukturzusammenhänge” [structural correlations] he detects in the target persons’ *response* were already objectively present in Croiset’s *reading*.

This reminds me of a legendary chief of police in southern Holland whose son went to a psychologist, was given a Rorschach test and interpreted the blots and dots as representing two persons engaged in unmentionable sexual activities. This caused the chief of police to have the psychologist arrested for showing pornography to minors.

The error is obvious, if not to certain Dutch policemen and to certain German university professors. The catch is that people tend to project meaning on random stimulus configurations. The vaguer the target material, the greater the freedom to indulge in “symbolic” interpretation and the greater the experimenter’s willingness to be astounded, the greater the chances will be that something with remarkable “gestalt characteristics” will emerge. This sort of psi, like beauty, is in the eye of the beholder.

Although I have some reservations about Pelz’ general approach to parapsychological claims (for example, I do not quite understand his argument that Croiset’s more spectacular failures demonstrate the absence of unusual abilities) I believe that he intuitively

¹⁹ See note 6.

understood this problem far better than Bender ever did. Pelz was completely unimpressed with all this post factum talk of “Tracer-Aussagen [tracer statements]” and “affektive Komplexe [affective complexes].” He realized that the number of possible “meaningful configurations” depends on the elasticity of the material of which they are constructed. So he insisted, quite rightly, that the individual “matches” be critically examined before any attempts are made to assess the accuracy of the entire reading.

This seems to be a point which parapsychologists of the “qualitative” school find difficult to grasp, although it involves only the simplest form of “quantitative” reasoning. If one precognitive item may be interpreted in a hundred different ways, then a reading consisting of ten such items will yield hundreds of thousands of possible “complexes.” In the case of Croiset, a hundred possible “hits” per item seems a very conservative estimate, as the number of permissible “matches” was not decided upon in advance. The example of the “red building” suggests, that, in the case of Pirmasens, the tolerance of the experimenters knew no bounds. In this context, it seems relevant to refer to Marks and Kammann’s incisive discussion of the “multiple end points” illusion.²⁰ A coincidence may seem striking if we only wonder about the probability of *this particular coincidence*. However, this particular coincidence is just one of innumerable *potential equivalent coincidences* (or “odd-matches”, as Marks and Kammann prefer to say), several of which are bound to materialize by chance alone.

There is no reason for me to feel inhibited by Bender’s injunctions against overestimating the value of “mosaikartiger Akribie [mosaic-like meticulousity].” **Quite to the contrary:** I am convinced that a thorough analysis of the individual hits (and of the way they were arrived at) is a precondition for understanding Bender’s prize case. First, however, some remarks need to be made regarding the general quality of Bender’s reporting.

Deficiencies in the Reporting

Bender’s account of the “exploration” is based on what the presumed target persons told him during interviews conducted a day *after* the actual experiment. The comments given *during* the evening in the Pirmasens Volkshochschule [institution for adult education] are not reported. Thus, the reader is not alerted to possible differences between comments in the two phases. This is an important point, as at the moment they were interviewed both presumed target persons had had ample time to reflect on Croiset’s statements and to search for correspondences. In view of what Bender has to say about Croiset’s reacting

²⁰ Marks, D., & Kammann, R. (1980). *The Psychology of the Psychic*. Buffalo, NY: Prometheus Books.

to “affektive Komplexe [affective complexes],” one would expect the recognition of a “hit” to be instantaneous and spontaneous. “Matches” discovered only after a considerable interval should be evaluated with increased skepticism.

No records of the actual demonstration are available. However, the unedited transcripts of the verification make it perfectly clear that, between the evening of June 3 and the interviews, several apparent “misses” had turned into apparent “hits.” It is worth mentioning that among the dozens of reports on the chair tests that I am aware of²¹ only *one* adequately informs us of the differences between responses at different stages of the verification.²²

Bender’s description of the evening in Pirmasens is sketchy. We can infer that Croiset was present, but we are left in the dark as to his possible share in the “verification” procedure. Pelz has reported – and the unedited transcripts confirm – that the psychic was allowed to put suggestive questions to the presumed target persons without anyone keeping a record of what was being said. In a few instances, Croiset even seems to have changed the wording of his predictions after having heard the target persons’ initial responses.

Comparison of Bender’s published reports with the “raw data” (tape-recordings, transcripts etc.) reveals numerous errors and omissions. While the fact that Bender had made the original material available for skeptical inquiry testifies to his bona fides the deficiencies in the reports are such as to convey a misleading impression to the readers.

The First Part of the Experiment – General Remarks

The most conspicuous weakness of this experiment is, of course, that the “target person” was not sitting in the target chair, but “two seats away from it.”²³ Bender regards this as a minor imperfection. After all, Frl. B. was sitting “near by.” The problem is that the experi-

21 For the history of the quasi-experimental chair-test design, see our editorial introduction to chapter 3-07. (Eds.)

22 See Eisenbud, J. (1973). A transatlantic experiment in precognition with Gerard Croiset. *Journal of the American Society for Psychical Research*, 67, 1-25.

23 Stories grow in the telling. In his book *Er is Meer... Waarheid en leugen bij paranormale verschijnselen* (Haarlem: De Haan, 1981), the Dutch author W. Hogendoorn claims that the target person was indeed sitting in chair no. 73. As his source he cites K. Frischler’s *Geisterstunde der Gelehrten*. München: Herbig, 1972, p. 109. [Also cf. Hoebens’ Dutch review of Hogendoorn’s book, Hoebens, P.H.: Hoedt u voor namaak! [Beware of plagiarism!]. *De Telegraaf*, April 11, 1981. (Eds.)]

menters failed to state in advance their criteria for acceptable proximity. Would they have judged the experiment a failure if Fr. B. had been sitting three seats away from seat no. 73? Even if we accept “two seats away” as the maximum permissible error, this would mean that no less than 17 persons (or 25 if we also allow bent lines) would in principle have qualified as target persons. That is to say between 7 and 10 percent of the entire audience. (This would rise to a maximum of 17 percent if “three seats away” is accepted.)

It was Croiset himself who, after entering the hall, indicated that the person he had “seen” was slightly “displaced.” We may safely assume that the person actually seated in seat no. 73 was *not* an approximately 30 years old woman in a white blouse.

In his reports, Bender distinguishes between “Kernaussage [core statement]” and less important ones. Needless to say that the relative importance of the statements was only decided upon *post factum*. In his 1981 interview with Fr. B. Bender refers to several statements by Croiset as forming a “Komplex.” Again, this is *post factum*. Not the statements, but only the eventual interpretations of those statements can be reasonably said to form a complex. Unless the individual items are assigned a different weight in advance we must treat all items as equally important. After all, every statement is a potential hit with potential “gestalt characteristics.”

A rational assessment of an apparently successful psychic reading demands that we ask how many different outcomes would, given the rules of the game, have been judged *equally successful*. We have already seen that, in the first experiment, Croiset had at least 17 chances of a “hit.”

However, the experimenters had more “outs” than that. In the chair test literature we find several instances of “displacement effects” of an even more bizarre nature. Tenhaeff has reported that Croiset not infrequently received psychic impressions of several persons simultaneously, mistakenly believing that he had “seen” only one individual. In such cases, the experiment is judged to have demonstrated precognition if several participants could each identify a part of the reading.²⁴

In *Parapsychologie. Ihre Ergebnisse und Probleme* Bender²⁵ gives an example of the curious phenomenon of “psychic contamination”: When someone, not necessarily participating in the experiment, accidentally touches the target chair or the inductor object *that* person may be the one “paranormally” seen by Croiset! It has also been observed that Croiset’s impressions related to a person not present at the demonstration, but somehow “linked” to the person in the target chair. The complete absence of clear rules as to

24 Tenhaeff, W.H.C. (1953a). Experimentele proscopie. *Tijdschrift voor Parapsychologie*, 21, 49-109.

25 Bender, H. (1970). *Parapsychologie. Ihre Ergebnisse und Probleme*. Bremen: Schünemann, p. 54.

how the target person is to be identified is one of the most objectionable features of the chair tests.

The Appearance of the Target Person

Statements 1 and 2: “I see a lady, about 30 years of age or somewhat younger, who often wears a vest of angora wool. She wears a white blouse.”

Comments: It is hardly surprising that the age of the presumed target person and the color of the blouse she was wearing approximately corresponded to Croiset’s impressions. We may safely assume that Croiset picked out Frl. B. on the basis of this correspondence. The hit is, of course, far from striking. White blouses are very common, especially around June 3. We are not informed of the mean age of the audience. It is unclear what measures were taken to prevent Croiset from acquiring prior information about the composition of the audience. The experiment was part of a series of lectures on parapsychology. He may have been told that the previous evening the hall was packed with 30 year old ladies.

In “Vom Lob der Genauigkeit” I have already pointed to a curious error in Tenhaeff’s account of the “angora vest” item. For details, the reader is referred to that essay review.²⁶ Suffice it to repeat here that Tenhaeff slightly embellished the facts, suggesting that the angora vest was somehow related to a specific incident that had occurred on the very day of the demonstration.

By comparison, Bender’s account is accurate. In fact, Frl. B. could *not* confirm that she often wore an angora vest. In his 1981 re-appraisal, Bender remarks that this item was among the least important of the set – implying that, in criticizing Tenhaeff on this point, I had wasted my “meticulousness” on trivia while ignoring the really important parts.

I reply that: (1) The essay-review was not concerned with the evidential value of the Pirmasens experiment, but only with the accuracy of Tenhaeff’s reporting; (2) The statement about the angora vest was judged unimportant only in retrospect. The item would no doubt have been hailed as a “core statement” if Frl. B. had come up with a convincing incident in which this piece of clothing had played a key role; (3) Tenhaeff’s embellished account was such as to suggest to at least one skeptical author that “the only impressive hit, therefore, concerns the angora wool vest...”²⁷

26 See note 3. (Eds.)

27 Cohen, D. (1973). *ESP: The Search Beyond the Senses*. New York: Harcourt Brace Jovanovich, p. 132.

A Jovial Boss

Croiset's statement: "Bei dieser Dame sehe ich einen Mann, der mich an einen Filmschauspieler Georg ... erinnert, einen Wiener, der mit Martha Eggert zusammen gespielt hat. Auch eine Ähnlichkeit mit Churchill. [In this lady's company I see a man who reminds me of a movie actor, Georg ..., from Vienna. He played together with Martha Eggert. Also a likeness to Churchill]." Verification, according to Bender: "Diese Beschreibung bringt diese Dame in Verbindung mit ihrem Chef, einem – wie festgestellt wurde – heiteren Pykniker [The lady associates this description with her boss, a cheerful pyknic, as could be confirmed]."

Comments: Pelz has rightly called Bender's account of this part of the experiment misleading. It is distressing that, in none of his subsequent publications, Bender has even acknowledged Pelz' incisive criticisms.

In the transcript of the verification of June 4, we find the following, revealing dialogue:

Bender: "...Er wurde beschrieben von Croiset als sehr heiter, pyknisch – wer könnte das sein? [Croiset described him as very cheerful, pyknic – who could that be?]"

Frl. B.: "Diese Beschreibung hätte auf meinen Chef gepaßt, der dann nachher ins Lokal kam und wo Herr Croiset sagte, ‚Ja das stimmt, er sah so aus‘ [This description would have fit my boss, who later entered the pub, which prompted Croiset to state, 'Yes, that's right, that's what he looked like.]"

Bender: "Sie arbeiten in einer Apotheke? [Are you working in a pharmacy?]"

Frl. B.: "Ja. Er fragte erst, ob mein Vater klein sei, da sagte ich nein, dann sagte er, ob mein Chef klein sei und diese Figur habe, und da sagte ich, da paßt es, der hat auch dieses Temperament. [Yes. Initially, he asked me if my father was short built. I denied that. He then asked me if my boss was a short man with this kind of stature, so I said: It fits him, he also has that temper.]"

The reader of Bender's report has no means of knowing that this "hit" was obtained in a most suspicious way. Obviously, Frl. B. did *not* recognize anybody in the description when first she was asked to comment on this item. The "hit" emerged only after Croiset had been given the opportunity to put suggestive questions to the presumed target person.²⁸

28 Hogendoorn (see note 23) quotes the target person as commenting: "As far as that jovial, sanguine man who looks like a movie actor is concerned, that immediately reminds me of my employer."

The conversation between the psychic and Fr. B. should have been regarded as a clear breach of protocol and have been reported as such. Incredibly, Bender thinks otherwise. Instead of remarking that Croiset had surreptitiously changed the wording of his prediction after it had become clear that “Georg ...” and “Churchill” meant nothing to the presumed target person, the boss, the experimenter adopts the psychic’s tactics, reminding Fr. B. that “Croiset described him as very cheerful and jovial.”

To present this as a hit is preposterous. The incident, however, is highly revealing. It throws a very dubious light on the way these experiments were conducted and shows that Croiset was by no means averse to the “fishing” techniques dear to country fair soothsayers.

The Red Building

Croiset’s statement: “Wohnt diese Dame in der Nähe eines roten Gebäudes, vor dem hohen Säulenträger sind? Ich sehe auch hohe Stufen. Kommt diese Dame viel in dieses Gebäude? Ich habe den Eindruck, daß vor diesem Gebäude ein Zaun ist. Es macht einen etwas verfallenen Eindruck. [Does this lady live in the vicinity of a red building with tall front columns? I also see a high staircase. Does she often visit that building? I have the impression that there is a fence in front of this building. It appears somewhat decrepit.]”

Verification, according to Bender (1957): “Die Dame wohnt nicht in der Nähe eines Gebäudes mit Säulenträgern. Es fällt ihr ein, daß das einzige Gebäude mit Säulenträgern in Pirmasens die Friedhofskapelle ist, in der sie vor zwei Tagen anlässlich einer Beerdigung war. Sie dachte dabei lebhaft an eine Beerdigung, an den letzten Besuch der Friedhofskapelle im Jahre 1942. Sie war damals in Sorge über einen in Rußland stehenden Freund, von dem sie keine Post bekommen hatte. Einen Tag vor dem Experiment hatte sie einen Brief von diesem mittlerweile verheirateten jungen Mann erhalten, der die Beziehung mit ihr wieder aufnehmen wollte, was sie befremdete und ärgerte – Vor dem Gebäude ist kein Zaun, es macht auch keinen verfallenen Eindruck. [The lady does not live near a building with columns. She remembers that the only Pirmasens building with columns is the graveyard chapel where she had been for a funeral two days previously. At that moment she vividly remembered a funeral in 1942, which was the last time she had visited that chapel. Back then, she was worried about a friend who was in Russia as a soldier, and from whom she had not received a letter. One day before the experiment she had received a letter from that man who had married in the meantime. He wanted to resume their relationship which offended and annoyed her – There is no fence in front of the building, and it does not look decrepit either].”

Additional information given by the presumed target person in 1981 (Bender, 1981b): “Bei der Befragung habe ich eigentlich keinen Zweifel gehabt, daß es sich bei diesem Säulengebäude plus Zaun um die Friedhofskapelle in Pirmasens dreht ... [During the interview I did not really have any doubt that that building with columns plus a fence was the Pirmasens graveyard chapel ...]”. Comments: In spite of Bender’s determined attempts to present this as a hit the fact is that the presumed target person denied every single element in Croiset’s statement – which, in the unedited version, contains the additional question: “Does she pass this building every day?”

The interpretation arrived at is extremely weak and far-fetched. Croiset had clearly indicated that, for the target person he had in mind, entering, passing or seeing this building was a frequent if not daily occurrence. Fr. B. seems to have visited the Pirmasens graveyard chapel once every so many years. The picture of the Pirmasens chapel (published in this *Zeitschrift* to illustrate Bender’s 1981 re-appraisal) clearly shows that the columns are far from “tall.” Neither can the stairs be adequately described as “high”. (According to Fr. B.: “Ungefähr 7-8 Stufen schätzungsweise [about 7 to 8 steps, I guess]” – transcript verification).

In his 1957 account, Bender has the presumed target person remember that the building is red. The transcript of the June 4 interview does not contain such a statement. Fr. B. told the experimenters that she could not remember the color of the columns. Although the photograph in the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie* is in black-and-white, the part of the building shown there looks a bit too bright to be red. Of course, the chapel did not have a fence and neither was it decrepit. It should be remarked, moreover, that Croiset originally referred to the building as being “a house.” The relevant part of the statement has disappeared in the published version.

The target person’s 1981 statement that “at the verification” she had had no doubt that Croiset had meant the chapel is flatly contradicted by what she told Bender on June 4, 1953, about the Pirmasens chapel being a mere “vage Möglichkeit, nicht ganz bestimmt ... [a vague possibility, not really certain ...].” She had hit on this vague possibility only “nach längerem Überlegen” – after long reflection. It is quite possible that, while visiting the graveyard in 1942, Fr. B. was worried about her friend on the eastern front. Presumably, she was concerned about the young man not only during that burial service, but at other times as well. There is no reason to regard the chapel as being specifically related to Fr. B.’s friend. After all, *he* was not buried. Someone else was.

At this point Bender (1957) states that the presumed target person was “offended” by the former soldier’s attempts to renew their friendship in 1953. (We are not told about possible earlier attempts, but there is the suggestion that Fr. B. had not heard from the former soldier for years until she received that letter a day before the experiment). The

target person's "anger" is particularly relevant to Bender's interpretation of the final statement in the series – the one about the cigarette box, to be discussed later. The transcript of the 1953 verification contains not the slightest indication that Frl. B. felt offended.²⁹ In her 1981 talk with Bender she even specifically *denied* having been offended. In Bender's 1981 re-appraisal, the word "ärgerte [angered]" has been deleted without an explanation.

The Delicatessen Shop

Croiset's statement: "Hat die Dame in einem Delikatessengeschäft eine Aufregung gehabt? Kaufte sie dort einen Obstkorb oder hat sie danach gesehen? Es fällt mir eine Dose mit Datteln auf. [Was the lady excited when she was in a delicatessen shop? Did she buy a fruit basket, or did she look for one there? A box of dates attracts my attention.]" Verification: Croiset was wrong on every account. However, the experimenters find it worth mentioning that there is a delicatessen shop opposite the pharmacy where the presumed target person works. This is of course not what Croiset had predicted. I wonder, how many citizens of the Federal Republic do *not* live or work near a delicatessen shop. The statement, meanwhile, is infuriatingly vague and ambiguous, and I could think of a great many *potential* "hits" with exciting "gestalt characteristics." A hypothetical example: Target person goes to the delicatessen shop to buy a basket of fruit for her mother, who is living in a red building with tall columns. In the shop, a drunk who looks like Churchill throws a box of dates at her. When she gets home, she discovers that a date has stuck to her angora vest.

Incidentally: Croiset had described the shop as being fitted with "white tiles." This part of the statement has been deleted in the published accounts. Would it also have been deleted if the white tiles had been a "hit"?

Upper Silesia

Croiset's statement: "Hat sie vor kurzem etwas gelesen über Oberschlesien oder hat sie über Oberschlesien ein Gespräch gehabt? [Did she read about Upper Silesia recently, or did she have a conversation on Upper Silesia?]" Verification, according to Bender (1957): "Sie zeigt ein Buch *Biographie der Landschaft Schlesien*, das in Zusammenhang mit einem aus Oberschlesien gerade gekommenen Besuch seit drei Tagen auf dem Tisch liegt. [She

29 Transcript 1981 interview (unpublished).

produces a book, *Biography of Silesian Landscape*, that had been lying on the table for three days because someone from Upper Silesia had come for a visit.]”

Comments: Pelz (1959) already revealed that *Biography of Silesian Landscape* was merely the sub-title of the book shown by the presumed target person. The main title was *Schwarzer Adler unterm Silbermond*. A trivial embellishment, to be sure. Yet I fail to understand why this tiny error was stubbornly repeated in both the 1971 and the updated 1981 editions of Bender’s *Unser sechster Sinn*. In none of his reports does Bender mention that Frl. B. had specifically denied that she had either read anything about Upper Silesia or had a conversation about that region. Upper Silesia had only been mentioned in connection with a recently arrived visit.

Bender (1957) states explicitly that the visit had “just arrived from Upper Silesia.” Yet the target person had only told him (transcript verification) that she had *believed* that her visitor was *originally* from Upper Silesia. In fact, the visitor was *not* originally from that area. She was from the Mark Brandenburg.

The experimenters must have been aware of this as early as 1953, for the “Silesian” visitor *had accompanied the presumed target person to the Pirmasens Veranstaltung* [event]!³⁰ In any case, Frl. B. told Bender in 1981 in so many words that the visitor was *not* originally from Upper Silesia. This fact, however, is not mentioned in the subsequent re-appraisal.

Needless to say that Croiset’s statement had been sufficiently vague to allow for hundreds of possible “matches.” I wonder how many Germans could have been found in 1953 to whom the statement *in no way applied*. At that time, the problem of the expellees was acute, and the Silesian provinces, lost to Poland, must have been frequently mentioned in the press.

Frl. B.’s Toes

Croiset’s statement: “Hat sie eine leichte Infektion am rechten großen Zeh gehabt? [Did she have a slight infection in the big toe of her right foot?]”

Verification: No, she had not. Only a “little something” with her left foot. Bender’s version is accurate. Tenhaeff claims that the presumed target person admitted to have had a slight infection in *both* feet.

³⁰ Transcript 1981 interview (unpublished).

Comment: Sore toes are a standard item on the soothsayer's repertoire, and feature in Croiset's readings with dismaying frequency.

The Kite

Croiset's statement: "Ich sehe einen Drachen, der nicht aufsteigt. Das ist ein holländisches Sprichwort, das bedeutet: etwas erreichen wollen, was sich nicht erfüllt. Das hat etwas zu tun mit einem Mann von 28-32 Jahren, grauer Anzug, dunkelblondes Haar. Er trägt einen Pullover. Dieser Mann hat einen Plan gehabt, aber diese Dame ist dazwischen gekommen. [I see a kite that doesn't ascend. This is a Dutch proverb that means: to strive for something that remains unfulfilled. This has something to do with a man of 28 to 32 years, gray suit, dark blond hair. He wears a pullover. That man has had a plan, but this lady interfered.]"

Verification: Age and color of hair apply to the young man referred to in the section on the "red building" statement. It is not known "whether he wears a pullover." Bender (1957) also mentions: "Sein Plan scheiterte an der Empörung der Versuchsperson. [His plan failed due to the subject's indignation.]"

Comments: The last part of the "verification" must be seen as poetic license (see "Red Building" section). The description of the young man is about as general as you can make it. Croiset does not specify at what occasions this man is supposed to wear gray suits or pullovers. The wording of the statement ("dazwischen gekommen") suggests that Croiset meant that the young man's plan would have succeeded if the target person had not interfered. Of course, that hardly was the case. It is important to note that the young man figures only in this statement. Nowhere does the original reading contain even the slightest indication that Croiset had seen a connection with the statements about the red building and the cigarette box.

The All-Seeing Eye

Croiset's statement: "Ich sehe das Symbol des 'alles sehenden Auges' – ein Auge im Dreieck [I see the symbol of the 'all-seeing eye' – an eye inside a triangle]"

Verification: Croiset had previously told the experimenters that this statement was to be interpreted symbolically. The all-seeing eye stands for the target person's keen intelligence. The experimenters decided that, indeed, Frl. B. possessed a keen intelligence. Comment: superfluous. The item could have resulted in a nice hit if a non-symbolic

interpretation had been discovered. The presumed target person might have worn a piece of jewelry in the shape of an all-seeing eye – it might even have been a present from the young man!

The Cigarette Box

Croiset's statement: "Ich sehe eine grüne Zigarettendose. Sie rümpft davor die Nase [I see a green cigarette box. This made her turn up her nose]."

Verification, according to Bender (1957): "Der junge Mann hat ihr zur Zeit ihrer Freundschaft eine gelbgrüne Zigarettendose geschenkt. Daß sie davor die Nase rümpfte, mag ihren akuten Ärger über das Ansinnen ihres früheren Freundes symbolisieren. [During their friendship the young man had given her a yellow-green cigarette box as a present. That she turned up her nose may signify her acute anger about her former friend's request.]"

Comments: The statement would have allowed for a great many possible "matches," symbolical or otherwise. Examples: Target person is offered a cigarette out of a green box, she dislikes the brand. Target person wants to buy a cigarette box, but at the shop she goes to only green ones are available. She dislikes the color green. Symbolical: Target person strongly dislikes a relative who smokes cigarettes from a green box. *Etcetera ad infinitum et ad nauseam.*

The interpretation chosen by the presumed target person (according to Bender, that is) is particularly far-fetched. The cigarette box was given and accepted in friendship. The "turning up her nose" was concerned with something the giver had done a decade afterwards.

The tape-recording of the original statements contains a specification missing from the published version. Croiset had clarified that the target person turned up her nose because "sie fand sie nicht schön" [she didn't like it]. This additional statement makes the symbolic interpretation all the more implausible. More important: This interpretation is Bender's, not Frl. B.'s. According to the 1953 verification transcript the presumed target person said she could *not* explain the "turning up her nose" part. It was Bender who matched this part of the statement to Frl. B.'s alleged indignation about her former friend's intentions. However, as we have seen in previous sections, Frl. B. was *not* indignant. Exit psi. Minor point: The cigarette box was not "yellow-green" but yellow. It had yellow and green flowers painted on it.

In his 1981 re-appraisal, Bender seems quite excited about some additional statements Frl. B. made concerning this cigarette box. On the evening preceding the demonstration

the participants had been asked to take an “inductor” object with them for Croiset to psychometrize the next day. (It will be recalled that the event was part of a series of lectures on parapsychology.) Fr. B. had selected a bracelet she had once intended to give to the young soldier who had given her a personal present. Try as she would, she could not remember what this personal present had been.

Only after the chair test demonstration had been over she recalled: It was a cigarette box! Strukturzusammenhänge [structural correlations]! Without wishing to cast any doubt on Fr. B.’s veracity I must point out that such statements are entirely unverifiable. What I fail to understand is why, at the demonstration proper, the presumed target person denied ever having had a cigarette box. If, as she told Bender in 1981, she had been “mentally searching” for that cigarette box, if that cigarette box had been, so to say, on the tip of her tongue ever since she took the bracelet out of her jewelry box, then why didn’t Croiset’s statement instantly refresh her memory?

Bender attaches much value to the cigarette box. It is certainly ironical to reflect that, if Fr. B. would have persisted in her denial, the item would now have been dismissed as another “Bagatellaussage [trivial statement].”

The Second Part of the Experiment – General Remarks

Both Bender and Tenhaeff have suggested that, from a methodological point of view, there is something especially sophisticated about the way the second target person was selected. I fail to see why.

This time, no displacement effect was observed although, strictly speaking, Frau F. cannot be regarded as the second target person as her “inductor” object was picked up by someone who was not sitting in the target chair. It will be noticed that, in the second set of statements, Croiset gives no indication as to the sex or the age of the target person.

The House on the Hill

The majority of Croiset’s statements in the “Greifversuch [pick test]” concern a house, somehow related to the target person. According to the psychic, the house is built on a hill (1); it is white (2), it has a protruding, brown roof (3); over the front door is an oaken arch (4); in front of the house is a small gate also with an oaken arch (5); near the house is a small road leading uphill (6).

Only items (1) and (6) apply to the house where Frau F. lives. The odds against obtaining a similar hit/miss ratio by chance alone hardly seem astronomical. Please note that the psychic would have scored exactly the same number of hits if the house had *not* been built on a hill, but *had* been white, or brown-roofed etc. There are fourteen possible combinations of two hits and four misses. I would like to know how many houses in Pirmasens can *not* be “described” with the help of any of these fourteen combinations.

In his final statement, Croiset refers to an “Einbuchtung [recess] ... very well known to a female resident of the house.” There happens to be a recess near the house where Frau F. lives. Apparently, the target person was quite impressed with this hit. However, Croiset had not said that this recess was near the house. The fact that he had added “very well known to a female resident” rather suggests that he meant a recess not so well known to the other residents. Presumably, all residents of Frau F.’s house were very much aware of the recess at their front door.

In his 1981 re-appraisal, Bender marvels that, 28 years after the experiment, both target persons “remembered every detail.” I suggest that he compares Frau F.’s 1981 recollection of this part of the experiment with the original data. He will be surprised to learn that Frau F. remembered every detail – wrongly.

Radiation Treatment

Croiset’s statement: “Hat man dort im Hause über Kurzwellenbestrahlung gesprochen? Jemand hatte Schmerzen in der Lendengegend [Was there any conversation about short-wave radiation treatment in the house? Someone suffered from pain in the loin area].”

Verification, according to Bender (1957): “Die Mutter von Frau F. leidet seit 14 Tagen an einer Nierenentzündung. Der Arzt hat eine Bestrahlung empfohlen. [For 14 days, Mrs. F.’s mother has been suffering from nephritis. The physician recommended a radiation treatment.]” According to Tenhaeff (*De Voorschouw*) the doctor had even “verordnet [prescribed]” such treatment.

Comments: Hardly a striking hit. Croiset did not specify, when this incident should have occurred. Any conversation about radiation treatment, any case of pain in the loin area in the history of the house or its tenants would have matched the prediction.

Minor inaccuracy: According to the transcript of the June 4, 1953 verification, the target person did *not* tell the experimenters that the doctor recommended (let alone prescribed) radiation treatment. What she did tell them was: “Der Heilpraktiker, der im Hause war, hat auch von einer evtl. Bestrahlung gesprochen. [The non-medical practitioner

who happened to be in the house mentioned, among other things, a possible radiation treatment.]”

Both in Bender (1957) and in Tenhaeff the short-wave radiation treatment and the pain in the loin area are presented as being part of the same statement (statement 6). In the original transcript of the reading, however, the “pain” is part of the *following* statement: “Jemand hatte Schmerzen in der Lendengegend. Sind da heftige Worte über oder mit einer jungen Dame, die ich mit dem Rücken gegen die Haustüre sehe. Sie verläßt das Haus. Das ist wahrscheinlich schon längere Zeit her [Someone had pain in the loin area. Are there vehement words about or with a young lady whom I see with her back against the front door? She leaves the house. This probably happened quite some time ago.]”

This suggests that the incident with the pain in the loin area should certainly not have occurred as recently as two weeks previously in order to correspond to Croiset’s “impression.” The second part of the statement is ignored in Bender’s published accounts. However, Tenhaeff registers a hit: “Mrs. F. had fired a maid for misbehavior. This happened a few months ago.” The tape recording of the verification does not contain such a statement.

In any case: Croiset had specified that the young lady “was angry and ran away.” This part of the prediction (suggesting quite the opposite of someone being fired) is ignored by Tenhaeff. Curiously, in 1981 the target person told Bender that this statement concerned “a relative of my husband.”

Orthodox Former Tenants

Croiset’s statement: “Waren die früheren Bewohner dieses Hauses sehr orthodox? [Were the former inhabitants of this house strictly orthodox in a religious sense?]

Verification, according to Bender: “Dazu gab Frau F. an: ‘Einige Familienmitglieder des Inhabers des anderen Hausteils sind auf eine besondere Art fromm, man kann wohl sagen orthodox.’ [Mrs. F. stated: ‘Some family members of the owner of the other part of the house are pious in a special way one might describe as orthodox.’]”

Comments: All this circumlocution only serves to distract from the fact that Croiset had scored a “miss.” It is interesting to read in the transcript of the verification that the target person had learned to adopt the special language of the “qualitative school.” She mentioned the “Überlegung ... daß Herr Croiset die andere Hälfte des Doppelhauses mit einbezieht in sein Bild [idea ... that Mr. Croiset incorporates the other half of the double house in his image.]” Instead of warning Frau F. against such over-interpretation Bender encouraged her, saying that this was very interesting indeed.

The Mystery of the Ring

Croiset's statement: "Hat man dort einen Ring verloren, der nicht wiedergefunden wurde? Er ging vor ungefähr einem Jahr in einem Zimmer im ersten Stock verloren, rechts im Hause. Rechts um die Ecke. [Has someone lost a ring there which was never found? It was lost approximately a year ago on the first floor of the right half of the house. Around the corner to the right.]"

Verification, (Bender, 1957): "Frau F.: 'Damit konnte ich nicht einig gehen. Das einzige, woran ich mich erinnere, ist, daß meine Mutter im linken Haus, in dem wir wohnen, einen Ring verloren hat, den sie aber nach einigen Wochen durch Suchen wiederfand. Das war ungefähr vor einem halben Jahr. [Mrs. F. 'I could not agree with that. The only thing I remember is my mother losing a ring in the left part of the house, where we live. After some searching, she found it back within a few weeks, however. That happened approximately half a year ago.']"

This story has a most curious sequel. In 1961, the target person wrote to Bender to report that her mother's ring, lost since several years, had been found under the stove, in a space that "for some time, but not anymore" had been closed off by a piece of wood. This, she wrote, confirmed Croiset's statement that "Der Ring ist unter dem Holz. [The ring is underneath the piece of wood]." In 1981, she told Bender that the ring had been missing for a while when the Pirmasens demonstration took place. This time, she clearly remembered that Croiset had said: "Der Ring liegt hinter dem Brett [The ring is behind the board]." In Bender's re-appraisal one can read about the somewhat "emotional" background of the incident. Apparently, the "hit" with the lost and found ring gave Frau F. the "absolute conviction" that Croiset was a real clairvoyant. Predictably, Bender remarks on the "affective" chord which Croiset had touched with his statement.

Comments: An infuriating example of Bender's lack of methodological rigor. The fact that the target person's subsequent statements are full of contradictions does not disturb him at all. In his 1981 re-appraisal he mentions that in 1953 he "had understood" that the mother had already recovered the ring, which to the casual reader may suggest that some misunderstanding was involved. To the contrary: In 1953 the target person had unambiguously stated that the ring had been found after a couple of weeks. If, after several years, an entirely new version crops up this should make the experimenter wary. Instead, Bender waxes lyrical this fresh proof of ESP.

Of course, "Der Ring ist unter dem Holz / Der Ring liegt hinter dem Brett [The ring is underneath the piece of wood / The ring is behind the board]" was *not* part of the reading Croiset had given prior to the Pirmasens event. In 1981, Frau F. explained to Bender that

Croiset had made this additional statement in a personal conversation. Bender: “Ah ja, das hat er ihnen persönlich gesagt [Ah, so he told you this in personal conversation].”

In the 1981 re-appraisal we are told that the “Holz / Brett” was mentioned “während der Exploration [during the exploration].” So this must have been during the event at the Pirmasens institution for adult education. Bender could not recall having heard it himself.³¹

All this tells us something about the way these experiments were conducted. Croiset was allowed to hold “personal conversations” with the presumed target persons prior to the subsequent verification on which the published reports would be based and without any of the experimenters even keeping track of what was said (cf. the “heiterer Pykniker [cheerful pyknic]” incident in the first part of the séance). Whatever Croiset may have privately told Frau F. on June 3, this additional statement was *not* mentioned by her on June 4, which alone would be a sufficient reason for skeptical reserve.

False Notes

Croiset’s statement, according to Bender: “Steht im Hause ein verstimmtes Klavier? Speziell an der rechten Seite ist es verstimmt. Ich sehe besonders die rechten Tasten. Ist dennoch auf diesem verstimmten Klavier eine Sonate von Beethoven gespielt, aber abgebrochen worden, weil es so häßlich klang? [Is there an out-of-tune piano in the house? It is particularly out of tune at its right side. I especially see the keys on that side. Has someone played a Beethoven sonata on that out-of-tune piano, nonetheless, but broken it off, because its sound was so unpleasant?]”

Verification, according to Bender: “Im Zimmer, in dem die Exploration vorgenommen wurde, stand ein besonders im Diskant verstimmtes Klavier. Auf diesem hatte Frau F. vor kurzem eine Sonate von Beethoven angefangen, aber wegen des Mißklangs abgebrochen. Die von ihr gespielten Takte wurden auf Tonband aufgenommen. [In the room in which the exploration was done, there was a piano that was out of tune especially in the descant. Mrs. F. had recently started to play a Beethoven sonata on this instrument, but she stopped because of the dissonance. The bars she played were tape recorded.]”

Comments: *Prima facie*, this seems to be the best hit of the entire set. In this case, however, the appearances are misleading. On closer inspection, the prediction is far from specific. Many families have pianos. Most pianos are a little out of tune. Beethoven is an

31 This is clear from the transcript of the 1981 interview, but the readers of the published re-appraisal are not told about the doubtful status of the “Holz / Brett” statement.

immensely popular composer. Amateurs rarely play an entire opus. More often than not pieces (particularly of sonata length and difficulty) are broken off well before the end. I would have been more impressed if Croiset had scored a hit by stating that the owner of the inductor object had played a Fantasia by Bakfark on an Ondes Martenot. Anyway: The “verification” rests on the target person’s apparently uncorroborated testimony. No other witnesses are mentioned in the material available to me. The fact that Frau B. at the occasion of Bender’s visit on June 4, 1953, played a few bars of Beethoven’s Opus 14 no. 2 proves nothing whatsoever.

The case is even weaker than is apparent from Bender’s published accounts. For the chief experimenter has omitted a most revealing part of the target person’s comments. Worse, he has embellished the hit by changing – *post interpretationem* – Croiset’s original statement. According to the transcript of the verification, the target person said: “Ja, zuerst habe ich bei diesem verstimmtten Klavier und der Sonate an das Nebenhaus gedacht ... Ich habe mich inzwischen besonnen. ... [Well, initially this out-of-tune piano and the sonata reminded me of the neighboring part of the house ... In the meantime I have changed my mind ...].” Now this is a most interesting statement. It strongly suggests that, at the evening of the actual demonstration, Frau F. replied that this item applied not to her but to her neighbors. Presumably, these neighbors owned a piano, presumably out of tune, on which a Beethoven sonata had been played. Only after a considerable interval did the target person change her mind: She too owned a piano and she too had made an attempt to play Beethoven!

The unedited version of the verification transcript confirms my suspicion that Croiset’s prediction was of the sort that would apply to a great number of people. It applied to the target person, but also to the people next door! Curiously, the target person in 1981 wrote a newspaper article in which she recalls having been “flabbergasted” by this hit. If the hit had been so astonishing, then why did it take her so much time and two guesses to find the “real” correspondence? Some readers may feel that Croiset’s statement contained at least one specific detail in that he had said that someone had broken off the Beethoven sonata because it sounded so terrible. However, *Croiset never said that!* Bender changed the prediction *after* the target person had decided on her interpretation. In fact, Croiset had said: “Steht im Hause ein verstimmttes Klavier? Speziell an der rechten Seite ist es verstimmt. Ich sehe besonders die rechten Tasten. Etwas gell geworden. Ich bekomme den Eindruck, daß, ohne daß das gestimmt ist, auf diesem Klavier eine Sonate von Beethoven gespielt wurde. Die Dame hat sich über etwas geärgert. Darum sehe ich das. [Is there an out-of-tune piano in the house? It is particularly out of tune at its right side. I especially see the keys on that side. Became kind of shrill. I get the impression that, even though it is not in tune, someone played

a Beethoven sonata on it. The lady felt annoyed about something. That's why I am seeing this.]"

I find it difficult to understand why Bender, who, while writing his 1957 report had both a tape recording and a transcript at his disposal, misquoted Croiset – thereby generating a spurious “hit.” It certainly deserves mention that, for once, Tenhaeff got it almost right. The account in the otherwise unreliable *De Voorschouw* (Tenhaeff, 1979a) does not attribute to Croiset the statement that the sonata had been broken off.

Summing Up

A critical analysis of the individual “matches” does not support Bender’s assertion that Croiset’s precognitive statements were both specific and accurate. In each instance, the “hits” were the result of a laborious process of subjective interpretation. Bender has completely ignored the problem of what Marks and Kammann have labeled the “equivalent odd-matches.” He does not seem to realize that an interpretation is *not* a “hit” if an unspecified number of entirely different interpretations would also have resulted in (equally good or even better) “hits.” A gambling casino operating on Bender’s tacit criteria of “success” would be bankrupt within half an hour. The Pirmasens chair test is unconvincing even if the published reports are accepted at face value. However, an examination of what remains of the “raw data” reveals serious shortcomings in the reporting. The net result of Bender’s editing is that Croiset is credited with more and better “hits” than he actually scored. Bender’s remarks about “mosaikartige Akribie [mosaic-like meticulousity]” versus “due consideration to the gestalt characteristics” hardly strike me as a convincing excuse.

Counter-Experiment

To verify my suspicion that the Pirmasens miracle was the result of subjective validation rather than ESP I repeated the experiment in July 1981. I let it be known at the office of the daily newspaper *De Telegraaf* that volunteers were needed for a scientific experiment in psychometry. The first two volunteers were asked to put their signature on a piece of paper, which then would serve as an “inductor” object, to be “psychometrized” by “Mr. Parker,” a fictitious clairvoyant. After a while, the subjects were handed their “readings,” with an instruction sheet in which the rationale of such experiments was explained in Benderian fashion. The volunteers were under the impression that the “readings” had been generated the previous day, when “Mr. Parker” was supposed to have handled the

“inductors.” Actually, they had been given Dutch translations of Croiset’s 1953 statements. The verification took place on July 16. The protocols were signed by the “target persons.”

Results, First Test (Target person Miss E. V.)

(Statements are summarized to avoid overlap.)

1 – Woman, age 30, white blouse, angora vest

Verification: Entirely correct. “I often wear a vest made of angora wool.”

2 – Churchill look alike

Verification: Initially, target person was unable to interpret this statement. When, however, the experimenter suggested that the psychic might have meant a “heiterer Pykniker [cheerful pyknic]” it cost her little effort to recognize her boss, Assistant Editor in Chief Mr. J.L., who is a “heiterer Pykniker” if there ever was one.

3 – Red building, columns

Verification: “This reminds me of the *De Telegraaf* building, which is built of red bricks, has enormous pillars at the entrance and a high flight of stairs.” The statement also reminded the target person of the post office next door to where she lives. It is not red, but it has a high staircase and she comes there frequently. Neither building is particularly decrepit. (Note by the experimenter: Perhaps the psychic had received an impression of the future state of the building?) The *Telegraaf* building has a fence.

4 – Delicatessen shop

Verification: A great number of immediate associations. Target person often goes to a delicatessen shop. Recently, she had started a slimming diet. She had to restrict her purchases to fruit. The sort of emotions she experienced while looking at the temporarily forbidden other delicacies will be easy to imagine. She is fond of dates, and so is her mother. “Each time I go to the delicatessen shop I buy her a box.”

5 – Upper Silesia

Given the political situation in Poland in July 1981, the original statement would have been suspiciously obvious. So “Upper Silesia” was changed into “Bavaria.” On July 16 Miss V. commented: “No particular associations.” The next day she reported: “I had to visit the newspaper’s archives this morning to look for a particular file. When I opened the file the first thing that struck me was an article on Bavaria.”

6 – Toes

Verification: No trouble with Miss V.’s toes. (Perhaps Mr. Parker received an impression of future trouble?)

7 – Young man, frustrated plan

Verification: Target person immediately recognized her boyfriend. The description applies. Age, attire and color of hair are correct. This young man had made plans to go to America. He had to give up this plan because he (recently) met the target person and fell in love with her.

8 – All-seeing eye

Verification: No “literal” interpretation. If meant as a symbolic description of Miss V’s keen intelligence the statement is a “hit.”

9 – Cigarette box

Verification: “I strongly dislike tobacco smoke, and I am always annoyed when my colleagues light a cigarette when I am near.”

Results, Second Test (Target Person: Miss H. v. S.)

1 – Description of the house

Verification: These statements remind Miss v. S. of her paternal home. There was an arch over the front door. The roof, not brown, was slightly protruding. Before reaching the back entrance one had to pass another arch (stone, not oaken). Target person also recognizes a symbolic meaning in these statements, related to a “protected” upbringing and the somewhat stiffening atmosphere in the reformed school she visited. No special associations with country-roads or recesses. Of course, the country being flat Holland, the v.S. house was not built on a hill.

2 – Radiation treatment, pain in loin area

Verification: When young, the target person frequented the house of a befriended family, living in the same town as her parents. She often played the piano there. The woman living in the house had cancer. One may surmise that the possibility of radiation treatment was discussed. Taken together, statements 2 and 4 (orthodoxy) remind target person of a certain woman who for some reason had taken a violent dislike of her. This woman’s husband suffered from cancer in the abdomen. The couple was orthodox.

3 – Young woman leaving the house

Verification: This is a very appropriate impression. Target person left her parents’ home early, and went to live on her own.

4 – See (2)

5 – Lost ring

Verification: Target person recalls no such incident.

6 – Piano / Beethoven

Verification: Both in her parental home and in the house of her friends (see 2) a piano was available. Miss v.S. played it often. She cannot recall whether any of the instruments was particularly out of tune. However, she recently discovered that her HiFi installation was defective. The right loudspeaker was malfunctioning, distorting the sounds of a Beethoven work she was listening to.

Evaluation

One does not have to be a psychologist to detect the “gestalt characteristics” of these interpretations. The individual comments are all meaningfully related to the target persons’ lives and preoccupations. I maintain – necessarily on subjective grounds – that the replication has been at least as successful as the original experiment.

The first part even yielded remarkably superior results in the replication – and this without any “displacement effect.” Miss V. confirmed every statement except one. Her interpretation was considerably more “literal” than was the case with the original target person. No doubt a determined practitioner of “depth psychology” could distill far more “meaningful configurations” from the material I have presented than I have done. I did not ask the second target person about possible “symbolic” explanations of the statement on the lost ring, but I have reasons to believe that this apparent “miss” could easily be transformed into a “hit” by means of Benderian-Tenhaeffian logic. Aficionados of psychoanalytical jig-saw puzzles may like to speculate about the “affective complexes” linking the first target person’s comments on the post office, the slimming diet and the new boyfriend.

Both target persons knew that, in matters parapsychological, I am inclined to a certain skepticism. Although I have tried to be persuasive in my explanation of the rationale of such tests, the “demand characteristics” of the situation must have been different – and less psi-conducive – than was the case in Pirmasens.

Bender was “astounded” by the results of the original experiment. In a different context, he has said that his belief in the reality of the Paranormal is “unshakeable” – and that Gerard Croiset has been instrumental in bringing about this state of mind. However, a “goat” – even a moderate goat – might be excused for wondering what is “paranormal” about a psychic reading that turns out to be applicable not only to a young pharmacy assistant in 1953 Germany but also – and to an even stronger degree – to a young journalist in 1981 Holland.³²

32 I could make a very convincing case for applying both Croiset’s Pirmasens readings to Mrs. Hoebens and myself, but will not bother the reader with the details.

Conclusions

Bender (1981b, p. 229)³³ ends his re-appraisal as follows: “The Pirmasens case has sufficient hard facts for a paranormal interpretation to be impressive. It might be possible to organize an opinion poll as to how the evidential value is judged. I believe that a complete denial [of the psi explanation] should be followed by an examination of the prejudices underlying such an attitude.” In the case of the present writer, the examination should not take much time. I confess to being strongly prejudiced against badly designed, sloppily conducted, illogically evaluated and inaccurately reported experiments parading as revolutionary science.

By singling out the Pirmasens miracle as a particularly compelling demonstration of precognitive ESP Bender has implicitly defined his own concept of scientific parapsychology. After having carefully examined the case I cannot but draw unflattering conclusions.

In fairness to Bender, however, I must point to an extenuating circumstance. In the sharpest possible contrast to the abusive behavior of his late colleague Tenhaeff,³⁴ Bender has not only tolerated my skeptical inquiry: He has even encouraged it. It was his idea that I should come to Freiburg and be astounded by the evidence. Without his courteous help, this “debunking” of Pirmasens would have been impossible. These facts certainly do not excuse Bender’s scientific errors. However, they should be taken into account by those who might wish to indulge in speculations about Bender’s motives.

33 See note 2.

34 See my “Croiset and Professor Tenhaeff” (note 4).

CHAPTER 3-12

Editorial Introduction

During the early 1980s, one of the editors (G. H. H.) used to be a regular book reviewer for Psychologie heute, the German equivalent to Psychology Today. When Rüdiger Runge, then an assistant editor of Psychologie heute, invited him to review Hans Bender's recently released book on precognitive visions, war prophecies, death experiences and related phenomena (Bender, 1983a), G. H. H. ventured to make an alternative suggestion. He recommended that, instead of publishing a traditional book review, Runge invite Hoebens to write a feature article on Bender's book and give Prof. Bender the opportunity for an immediate rejoinder in the same issue. This is what Runge did, and Hoebens and Bender both accepted the invitation.

Hoebens prepared and submitted his review essay after his long paper on the Pirmasens chair test (see chapter 3-11) had been completed, but almost a year before it was eventually published. Runge translated Hoebens' English manuscript to German (and in the process committed a minor, but non-trivial translation error that was to cause some irritation with both discussants), and he invited Bender's reply which the latter duly submitted.

Since Hoebens had not provided a title for his review article, Runge thought up the headline "Die Erfasser des Unfaßbaren: Trügerische Tatsachen" (which we have translated into English for this reprint). So the title is Runge's, not Hoebens'. The review was published in the September 1983 issue, volume 10, pp. 64-67, of Psychologie heute with Bender's reply (Bender, 1983b), entitled "Fallstricke des Vorurteils [Pitfalls of prejudice]," immediately following on pp. 67-70. Hoebens briefly replied to Bender in the December 1983 issue ("Vernebelungstaktik [Smoke-screening]"), again translated by Runge (Psychologie heute, 10, [12], p. 19).

Ulrich Timm, Bender's collaborator during major parts of the 1960s and 1970s, eventually also supplied a brief commentary a few months later (Timm, 1984a). Finally, later in 1984, the entire exchange was reprinted in a special collection of selected highlight articles from Psychologie heute: Heiko Ernst (ed.), Grenzerfahrungen [Boundary Experiences]. Weinheim & Basel: Beltz Verlag, with Hoebens' review article on pp. 199-201.

Below only Hoebens' essay review of Bender's book will be reprinted. It was not published in English before. Again, this chapter is based on Hoebens' original English manuscript from his files. It therefore avoids Runge's infelicitous translation error mentioned before, which makes at least parts of the ensuing Psychologie heute discussions obsolete. (Eds.)

THEY KNOW THE UNKNOWN: TREACHEROUS FACTS

On March 29, 1983, the *Freiburger Zeitung* reported that the world-famous German parapsychologist Professor Dr. phil. Dr. med. Hans Bender had been awarded the Verdienstkreuz I. Klasse des Verdienstordens der Bundesrepublik Deutschland [Cross of the Order of Merit of the Federal Republic of Germany]. During the awarding ceremony the Minister of Science and Art of Baden-Württemberg had praised Bender for his contributions to the integration of parapsychology into mainstream science. Paraphrasing the minister, the newspaper continued: “Professor Bender is strictly concerned with demarcating facts that can be established scientifically from all shades of fraud, subtle deception and simple confidence tricks.”

Ever since its inception – now well over a century ago – parapsychology has been a highly controversial subject. While even diehard skeptics have frequently been impressed with the quality of the work done by at least a minority within the parapsychological community there remains widespread doubt as to whether phenomena such as “telepathy,” “clairvoyance” and “precognition” have been adequately demonstrated. Many scientists feel that “psi” contradicts our basic assumptions about “reality” and contend that the actual parapsychological evidence is far too shaky to support the scientific revolution the overthrow of the ruling “paradigm” would imply. In the past 100 years, many a supposedly flawless proof of “psi” has been exposed as the product of wishful thinking, sloppy experimenting methods, observation errors or even outright fraud. The unexplained residue may reflect no more than skeptical failure to identify, at this point of time, the non-paranormal factors causing the apparent miracle. Thus the parapsychological evidence, however striking *prima facie*, is taken with a pinch of salt.

Many (but not all) parapsychologists often complain about what they perceive as gross unfairness on the part of their critics, who, it is said, typically fail to distinguish between the occult garbage published by sensation-mongering “outsiders” and the sophisticated work done by responsible “insiders.” It is all right to dismiss out of hand a ghost story published in *Bild* or *Esotera*, but is it rational to remain incredulous when someone of the stature of Professor Bender has vouched for the paranormal facts?

Bender’s latest book purports to be a rigorously researched monograph on arguably the most subversive of “psi” phenomena: precognition. Bender firmly believes that this phenomenon exists and has been demonstrated beyond reasonable doubt. However, he thinks that the popular idea of “precognition” is wrong. There is no actual transfer of information from an objective, pre-determined future to the present. Rather, the “psychic” sees potential future states that may, or may not, come true. Thus there is no criterion for

distinguishing in advance between “hits” and “misses.” The accuracy of a given prediction can only be established *post factum*. This is not to say that precognitive successes are chance hits. Often, it is possible to demonstrate in retrospect that the match between prediction and outcome is too close to be attributable to chance. It is with such incidents that the major part of Bender’s latest book – *Zukunftsvisionen, Kriegsprophezeiungen, Sterbeerlebnisse* (Bender, 1983a) – is concerned. At this point, the habitual skeptic has to address two crucial questions: 1) Are the facts true as reported? and 2) Does whatever remains after the first question has been answered admit no more plausible an explanation than ESP?

Given the number of anecdotes in Professor Bender’s book, it is virtually impossible to check them all. For this reason, I will restrict myself to critically examining a few of the more striking claims.

Case 1: Bergengruen’s Uncle

Of comparatively minor importance is a curious case reported on p. 22. An uncle of the poet Werner Bergengruen had dreamt about a hearse and a coachman who had invited him for a ride. Some time later, the dreamer visited a department store in Paris where he was about to enter a lift when he recognized the attendant as the coachman he had seen in his dream. Realizing that he had received a warning, he declined to enter the lift. Immediately after, the lift’s cable broke, and it smashed into the basement. Several people were killed or injured. The case is honestly presented as an “anecdote,” but apparently Bender finds it sufficiently plausible to deserve mention in a book on “genuine” precognition. Unfortunately, the paranormal warning system does not seem to have worked for Professor Bender. He has failed to realize that the story is identical to the famous story told about the British Lord Dufferin in Camille Flammarion’s *Au tour de la mort*¹ and subsequently retold in dozens of publications, sometimes with different characters and different locations. The original account has been conclusively exposed as a fairy tale,² and there is no reason whatsoever to lend credence to any of the later versions.

1 Flammarion (1921, pp. 231-232). (Eds.)

2 When Bender used that tale for his collection he apparently was unaware that, in fact, it had long been exposed as an urban legend. As early as 1926, five years after Flammarion and more than half a century before Bender’s book was published, Carl von Klinckowstroem had described the tale as an “okkultistische Wanderanekdote” – a “travelling occult legend” (Klinckowstroem, 1926, p. 275). For a brief account of the legend’s history, see a paper by Bender’s Italian colleague Emilio Servadio (1994). (Eds.)

Case 2: The Pirmasens Chair Test

Connoisseurs will not be surprised to hear that Bender, in his new book, again regales his readers with a glowing account of one of his prize miracles: the so-called Pirmasens chair test of 1953, where the noted Dutch “clairvoyant” Gerard Croiset is supposed to have given a startlingly accurate precognitive description of two persons who would, at a later point, happen to be seated in specified chairs in a Pirmasens auditorium. Recently, I have made an exhaustive critical examination of this claim, a report of which has been submitted for publication in *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*³. In brief, I discovered fatal flaws in the design of the experiment, absurdities in the parapsychological assessment of the data and serious imperfections in Bender’s various published accounts of the facts. Contrary to what Bender tenaciously believes, Croiset’s “precognitive statements” were far from “specific” for the “target persons.” In 1981 I have replicated the experiment, using Croiset’s 1953 statements but with an entirely new (Dutch) target group. This time, the results were even better than in the original experiment – which nicely demonstrates that such “precognitive successes” are due to personal validation on the part of the target persons rather than to paranormal abilities on the part of the “psychic.” Bender received a preliminary report of my findings in the summer of 1982. In his 1983 book, some factual errors have been corrected – without any explanation being offered for the discrepancies with earlier versions. Other inaccuracies have been left unchanged. An example: Croiset had made some allusions to a “red building with tall columns.” Bender claims that the girl to whom this statement was meant to apply “immediately associated it with the chapel on Pirmasens cemetery” – where she had had an emotional experience. This claim is flatly contradicted by the original documents which I inspected in Freiburg in June of 1982.

Case 3: The Two Fieldpost Letters

The case of the “unknown French prophet” who, according to Bender, in 1914 predicted in amazingly accurate detail the fate of Germany until 1945 is the central part of the section on “War Prophecies.” At first sight the case may seem impressive, especially as the authenticity of the 1914 letters in which the prophecies are contained is hardly in doubt.⁴ However, a critical analysis of the claim reveals the fallacious nature of Bender’s reasoning.

3 See chapter 3-11 of this book. (Eds.).

4 The French “prophet’s” fieldpost letters of 1914 also are shown, described and transliterated in Bender (1980, 1984b). (Eds.)

First, the case is a prime example of a conclusion based on data which have been selected *post factum*. In the course of the centuries, thousands of “prophets” have attempted to predict the future. Most of them have failed miserably. However, given the number of participants in the prophetic lottery, chance alone would adequately explain a remarkable “hit” once in a while. Bender fails even to mention this vitally important point – assuming without further explanation that the French prophet’s apparent success must be due to a special ability.

Second: The “hits” in this case are not nearly as striking as the book suggests. Bender has been singularly unsuccessful in avoiding the pitfalls of what is known as “subjective validation.” With the wisdom of hindsight, he has projected a historical interpretation on the prophet’s vague, confused, garbled and contradictory statements and naively concluded that this projection reflects the prophetic text’s *actual* content. However, the two “fieldpost letters” would allow for many completely different interpretations and could – given Bender’s ultra-elastic rules of evidence – be matched to countless different historical scenarios.

Case 4: The Tenerife Dream

The most striking case in the book is related on the very first page – right under the chapter heading “Zukunftsschau aus wissenschaftlicher Perspektive [Prophecy from a scientific perspective].” It is about an American student, Mr. Fried, who, Bender says, in 1977 dreamt about a recently deceased friend who showed him a newspaper with a future dateline. The headlines referred to a collision between two Jumbo jets over Tenerife with 583 people dead. Mr. Fried informed the president of the University about this premonition. Ten days later, the terrifying dream came true to the letter. According to Bender, the incident has been “published frequently.” The case, incidentally, also features in a chapter on parapsychology which Bender (with his pupil Herr Elmar Gruber) contributed to *Kindlers Handbuch Psychologie* (Bender & Gruber, 1982), where we are assured that the amazing facts have been “reliably documented.” If true as reported, this incident would be astonishing indeed. Unfortunately, Bender has not done his homework. The Fried “prophecy” is a well-known, much-publicized and *confessed* hoax, perpetrated by an amateur magician.

Mr. Fried never told the University president of the Tenerife disaster before the fact. What he did do was to put a box, said to contain an unspecified “prediction,” in a safe. When the air disaster had taken place, the box was opened in the presence of witnesses and found to contain a piece of paper with the text: “583 Die in Collision of 747s in Worst

Disaster in Aviation History.” Soon after, Mr. Fried frankly admitted that he had planned the prediction as a stunt. Of course, the prophetic piece of paper was inserted only *after* the crash, by sleight of hand.

I have in my possession⁵ a copy of a letter Mr. Fried wrote to a fellow magician shortly after the stunt. There, he flatly denies having paranormal powers and complains that he has been embarrassed by people who asked him for occult advice. *The Second Book of the Strange* (Gadd, 1981) comments as follows on the Fried episode: “The credulousness of at least a proportion of the news-consuming public is almost unlimited.” We cannot really blame the “news-consuming public” for occasionally failing to tell fact from fraud. However, we expect better from a leading parapsychologist who is awarded a „Bundesverdienstkreuz” for his rigorous efforts to demarcate “facts that can be established scientifically from all shades of fraud, subtle deception and simple confidence tricks.”

Bender’s latest book drastically demonstrates that the dividing line between “occult sensationalism” and “respectable parapsychology” is not as sharply drawn as some proponents have optimistically claimed.

5 This letter (and Hoebens’ correspondence with Terry Sanford, then President of the University of Durham, NC) are preserved in the Hoebens Files. (Eds.)

CHAPTER 4

Comments and Controversies



CHAPTER 4-01

Editorial Introduction

Piet Hein Hoebens owned sizeable files of material, collected during the late 1970s and early 1980s, on what has become known as “astro-archaeology” or “Palaeo-SETI,” and he had read many of the relevant books. However, that field, almost single-handedly popularized in the West by Swiss author Erich von Däniken since the mid-1960s, rarely turns up in Hoebens’ newspaper articles, and it is hardly ever mentioned in his scholarly writings either.

Nevertheless, in the summer of 1979, Hoebens seized the opportunity to attend a conference of the Ancient Astronaut Society in Munich and to conduct an interview, in English, with Erich von Däniken. As far as we have been able to ascertain, the tape-recorded and transcribed text of the interview was never published in any language. Conceivably, Hoebens may have published an abridged Dutch version of the interview, but even after extensively searching his comprehensive files as well as potentially relevant newspapers and magazines, we have been unable to trace any. Also, knowledgeable long-term observers of the “astro-archaeological” scene,¹ such as Ingbert Jüdt and Jonas Richter, were unaware of this interview.²

The text of the interview that is printed below, presumably for the first time, was found in the Hoebens Files. We have tacitly corrected a number of typing errors, provided the basic structure, and added the title, references and footnotes. It goes without saying that the interview text itself literally follows the transcript. (Eds.)

ANCIENT ASTRONAUT IN A CACTUS – AN INTERVIEW WITH ERICH VON DÄNIKEN

Introduction

I had the pleasure to meet Mr. von Däniken at the 6th World Conference of the Ancient Astronaut Society in Munich where I also met astro-archaeological luminaries like Prof.

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- 1 Shortly after this introduction was written, an opportunity presented itself to pre-publish this interview in a special issue of the *Zeitschrift für Anomalistik* (Hoebens, 2012).
 - 2 Ingbert Jüdt, personal communication to Gerd H. Hövelmann, 2 May 2009; Jonas Richter, email to Gerd H. Hövelmann, 24 September 2009. (Richter is preparing a Ph.D. thesis on von Däniken in Religious Studies at the University of Göttingen).

Dr. Luis E. Navia, Dr. Gene M. Philips, Zecharia Sitchin, Dr. George Sassoon and Dr. Duncan Lunan – who is, in fact, a skeptic.

The conference was held at the Munich Sheraton Hotel, 14th to 16th June, 1979, and was attended by some 600 to 900 people. The faithful were kept happy with the promise that an AAS-sponsored expedition would soon return from the Brazilian jungle, where, according to “reliable sources,” lives an Indian tribe possessing a 12,000 year old library of extraterrestrial provenance and numerous alien artifacts. There was even talk of well-preserved little green bodies resting in a secret tomb. It sounded very much like a beautiful hoax perpetrated by a slightly malicious Indian chief. There was a lot of ritual shouting at the pig-headed skeptics, but I was surprised by the relative sophistication of some of the speeches. As Mr. von Däniken was in charge of the whole organization of the Conference he could spare me only an hour, although he invited me to come and visit him at his home in Switzerland. I got the strong impression that he is basically sincere: He truly believes the Gods were Astronauts. His enthusiasm is almost contagious. He is, however, only faintly aware of the difference between “Dichtung und Wahrheit”.³ He is so confused in his reasoning he doesn’t even understand what his critics have against him. His disregard for the rules of logic are, I think, clearly demonstrated in his answers to my well-prepared questions. I was somewhat dismayed when he started blaming his publishers for the glaring inconsistencies in his books. His version of what happened in the caves is less than satisfying. My private impression is that he is willing to resort to untruths in order to save a theory he really believes is true.

The Interview

Piet Hein Hoebens: Mr. von Däniken, I have taken it for granted that most of my readers will be familiar with the gist of your theories. They may be less aware of the criticism that has been leveled against it. For the purpose of this interview I have read or re-read almost everything you have published, and all the criticism I could lay my hands on. After all this reading I was left with quite a few questions. Let me begin with the so-called Sirius Mystery, which you and others consider to be one of the best pieces of evidence in favor of the Ancient Astronaut hypothesis. Surprisingly you never seem to have made any attempt to refute the criticism of this evidence by Mr. Ian Ridpath.

Erich von Däniken: Ridpath? Who is he?

3 *Dichtung und Wahrheit* [from *My Life: Poetry and Truth*] is a proverbial allusion to Goethe’s autobiographical writings, which comprised 20 books, arranged in four volumes, written between 1810 and 1831. (Eds.)

PHH: You should have known. He sent you a copy of his book *Messages from the Stars*,⁴ which you acknowledged ...

EVD: Can't remember, what does he say?

PHH: He says it's not necessary at all to postulate alien visitors to explain the Dogon mythology. There is no reason to suppose that those parts of the tribe's legends that suggest advanced astronomical knowledge are really very old.

EVD: Was he the one that spoke about this medieval university at Timbuktu?

PHH: No. That was Professor Ovenden.⁵ Ridpath disagrees with him. He says Mr. Temple⁶ used an old mythology, ignored those elements not consistent with his theory and interpreted some highly ambiguous facts in a rather selective and fanciful way. For instance those sand drawings. Temple claims one of those drawings clearly shows Sirius B circling Sirius A in an elliptical orbit, whereas the drawing shows both symbols *within* an egg-shaped object – if it is an object at all. And there are more symbols in it than just Sirius A and Sirius B. Well, his main point is: The Dogon myth is an oral tradition, and like all oral traditions subject to additions and embellishments. Let's assume the tribe really had an ancient myth somehow centering on the Dog's star. Since the beginning of this century, decades before Mr. Temple's oft-quoted anthropologists appeared on the scene, the Dogons had been in contact with western civilization, mainly through missionaries. It is only natural that they wanted to discuss their divine star with the white visitors, and it is equally natural that those visitors told them what *they* knew of Sirius. The missionaries may even have tried to combat local superstitions by giving the poor heathens the true facts about Sirius. Instead of converting, however, the Dogons happily incorporated some recent astronomy into their tribal lore. I find this entirely convincing, because the Dogons' astronomical knowledge is modern by the standards of the twenties, not by the standards of today.

EVD: Yes, but according to Robert Temple and Madame Griaule, I met her in Paris, the Dogons have their knowledge of the Sirius myth for at least 900 years! And not since recent times, because there are many many things ... They make these wooden masks, and the main dancer has for every Sirius ceremony another mask. And, as I have understood it, the mask of the main dancer may be used only once. Once the ceremony is over they hang the mask on a tree or whatever, and every fifty years, or sixty years, I am not sure now, there is only one of these masks. Well, there are so many of those masks,

4 Ridpath (1978). (Eds.)

5 Ovenden (1962). (Eds.)

6 Temple (1976). (Eds.)

the oldest one should be 900 years old. But there are many other reasons to believe the tradition is really old, but I don't remember these right now. But by the way, this Sirius Mystery is really Robert Temple's idea, not mine. I heard Temple defending himself at a meeting in the USA where he was heavily criticized, but he did it brilliantly. But I am not his defender, I don't know if he is right or wrong.

PHH: Well then, let's leave that subject. But just one thing. You write about this Sigui ceremony and claim this is performed once every fifty years, nicely fitting into the Ancient Astronomy hypothesis. But according to Robert Temple and the anthropologists he always quotes, the Sigui is celebrated once every *sixty* years. They admit this does *not* fit into the theory. Why did you change the number of years?

EVD: But I have my fifty years from Robert Temple! I know him. We met in London several times. He told me the Dogons have several ceremonies, one in every fifty years, one every sixty years, one is even every eighty years, because they have several planets. And he told me the fifty-year ceremony was the one related to Sirius B.

PHH: Well, I couldn't find this in Temple's book!

EVD: You should ask this to Temple. Maybe he is wrong, why not?

PHH: Well, let's continue with the theories that are really yours. You have often been criticized for being inconsistent in your handling of the evidence. I found what looks like a striking example of that in your books. It concerns your treatment of the Nazca Markings. In *Chariots of the Gods*⁷ you say they "remind you of landing-grounds." In your book *Kreuzverhör*⁸ [Cross-Questioning] you complain of having been wrongly quoted.

EVD: I never said they were landing-grounds – as a matter of fact, in my *Zurück zu den Sternen*⁹ [Return to the Stars] I said just the opposite: They are *not* landing-grounds.

PHH: Well. What do we read on the very last page of your *Beweise* (British edition: *According to the Evidence*¹⁰)? I quote: "So it *was* a landing-ground for the extraterrestrials. Wait and see." Is this a contradiction or not?

EVD: It is entirely correct what you say. And this is one of the mistakes in the books. It was wrong, wrong, wrong. I cursed against it. You know, this was inserted by my lector, you know, the man who edited the book for the publishers. I was not at home then.

7 Däniken (1968). (Eds.)

8 Däniken (1978). (Eds.)

9 Däniken (1969). (Eds.)

10 Däniken (1977). (Eds.)

I travel most of the time. When I returned I saw what they had done. Wrong, wrong, wrong! But it was too late to stop it. And the translations were published at the same time. I was very sad. It was wrong, and you are right.

PHH: Another contradiction. On page 290 of your *Beweise*, British edition again, you explain why the extraterrestrials are not officially contacting the Earth now. They're afraid of contagious diseases, you say. Why didn't they think of that when they visited us 400,000,000, 20,000 and 5,000 years ago?

EVD: Well, if there are really UFOs and extraterrestrials in them, it is by no means certain that they are the same who visited the Earth before. Maybe it's another group.

PHH: Why then don't they contact us by radio? No fear of bacteria there!

EVD: I don't know.

PHH: Your astronauts often strike me as quaintly old-fashioned. You quote the old book of Ezra, where the messengers of God – astronauts, according to you – instruct the prophet to collect all the scribes of the area in order to take down a message. Isn't that odd? Why should an advanced civilization need the help of scribes? Didn't they have type-writers, tape recorders or xerox machines? Or, at least, more durable writing materials than papyrus and parchment? Why so primitive?

EVD: Well, we don't believe that those visitors from outer space have been very, very advanced to us. We think they were only twenty or thirty years ahead of us. They are described as coming down with a lot of noise and smoke, and this doesn't sound very advanced. This is not my calculation, by the way, but Blumrich's.¹¹ Then, according to some mythologies, there was something like a fight between the gods. The ones that came down to Earth were something like rebels. Maybe they did not have all the technology they needed because they were cut off from the mother spacecraft. What do I know? Maybe they really needed scribes!

PHH: There is something else about these old books. On page 197 of *Beweise* you quote the Book of Ezra where the astronauts tell the prophet to keep secret almost all the books dictated by the Gods. One page earlier you had quoted the Book of Enoch, where the astronaut tells the prophet he may *not* keep secret any of the divine books. Your gods seem to be of two minds!

EVD: You're right. It is a paradox. But that's what the books say.

PHH: Of course, but I wonder how you can use two sources that flatly contradict each other in support of one and the same theory.

¹¹ Blumrich (1974). (Eds.)

EVD: Well, I see both texts as a clear indication that we have been visited by beings from outer space. Maybe the conflicting passages were inserted later.

PHH: Like what happened to your books.

EVD: Yes. You see, my critics always say, when I quote the Bible, that I only take what I like, and ignore the rest. That's correct. That's what I'm doing. That we call selection. Every scientist, every theologian in this case, uses exactly the same method. Absolutely the same thing. The Bible is like an onion. You peel and you peel. And layer after layer you throw away, until you get at a hard core – an old tradition that shows the people of that time knew something they could not have known. Most of the Book of Enoch is just rubbish. I can't do anything with it. But when it comes down to the astronomical part ... you know, "These are the Names of the Guardians in the Sky," etcetera, that's damn interesting. Who told them those names?

PHH: There have been allegations against you that much of the evidence you use is not genuine, and that you allow your imagination to run away with you. Some of your facts are not facts at all. An example. In *Chariots of the Gods* you mention a "calendar" found in the mud at Tiahuanaco. It proves, you claim, that the people who carved it had astronomical knowledge far ahead of ours. As Ronald Story¹² rightly wonders: Where on earth is that thing? Nobody except you seems to have seen it.

EVD: The calendar of Tiahuanaco I was referring to is in fact the gate of Tiahuanaco! The same thing! Mr. Story should read the book of Bellamy.¹³ Bellamy has, in a very convincing way, demonstrated that the gate of Tiahuanaco is in fact a calendar.

PHH: Why didn't you say so in your book? In *Chariots of the Gods* I couldn't find a single passage confirming what you just claimed. You treat the gate and the calendar as if they were two separate things. Bellamy's calculations, apart from being utter hogwash à la Hörbiger, concern the Great Idol of Tiahuanaco, which you clearly say is *not* the calendar.

EVD: Let me say this to you, concerning *Chariots of the Gods*. When I had written this book it was 411 pages. I sent it to twenty publishers, and nobody accepted it. Finally, I found a publisher who was willing to publish it, but only on the condition that I agree that it would be edited into a much shorter work. After all these frustrations I was so mad I agreed to everything. The publisher said it was too long, and too scientific, nobody would read it. Well, I agreed. And the publishing house cut it down to 210 pages. So *Chariots of the Gods* is not a scientific book. It is a provocative, explosive book, but in no

12 Story (1976). (Eds.)

13 Bellamy (1956). (Eds.)

way scientific. So it may well be, and I accept this kind of criticism, that there are some misinterpretations, and some wrong ideas and too much fantasy in *Chariots of the Gods*. By the way, Ronald Story is a funny man. All these critics are. We had in the US the first book against me, called *Crash go the Chariots*.¹⁴ Believe it or not, this Mr. Wilson ... he claims he is an archaeologist which he is not, he is a minister, a priest, from Australia, we know each other, we had public debate at the North Dakota University, for five hours ... well, this Mr. Wilson is like the other critics. First they attack me, and as soon as they are well-known they go on writing their own books – in favor of these theories. Mr. Wilson's second book was a flying saucer book.¹⁵ The same thing with Ronald Story. Story wrote this book against me, making a lot of money, quoting a lot of rubbish by the way, from *Playboy* etcetera, and now Story's second book is a UFO book. Pro UFO!¹⁶

PHH: Are you serious?

EVD: Oh yes!

PHH: Let's go to the South American gold caves. In your book *Kreuzverhör* you finally face an accusation you had ignored in *Beweise*: Mr. Móricz' claim that you have never set foot into those caves. I must admit I find your version of the facts a bit difficult to swallow. You claim in *Gold of the Gods*¹⁷ that you have "seen and photographed the incredible truth in person," and the only person alive who could have corroborated your story says you're lying. Now you claim you have been speaking the truth. You have the evidence, but cannot show it to us. You have promised not to reveal the secret, and you're afraid of powerful enemies in Ecuador in case you break that promise ...

EVD: That's true enough, by the way ... yes. Well, Mr. Móricz says I have never been in the caves. Yet I have published, in *Beweise*, a photograph where Móricz and I are sitting in front of the cave. How can he say we were not there?

PHH: That's not what he is denying. According to the interview in *Der Spiegel*,¹⁸ you have been shown a blocked side entrance, and have never been inside it.

14 Wilson (1972). (Eds.)

15 This is not quite correct. Among almost three dozen books by Clifford A. Wilson that we are aware of (almost all of them popular and sometimes lamentably superficial works), including several on flying saucers and related phenomena, there seems to be none that really argues „in favor of these theories,“ as Däniken maintains. (Eds.)

16 This time, Däniken was right. Ronald Story wrote or edited several books on UFOs from what may be described as a proponent's perspective. (Eds.)

17 Däniken (1972). (Eds.)

18 Móricz (1973). (Eds.)

EVD: But why the hell should I go with him for at least 36 hours just to sit before a cave and take a photograph? I have taken photographs down there which I have not published. We have a few here,¹⁹ we are showing them. Taken inside the caves.

PHH: Did you overcome your fear of your Ecuadorian enemies? I thought you were not allowed to show those pictures!

EVD: Different things again. I was not allowed to show the pictures taken inside the metal library. In other parts of the caves, where there are many objects, it was not so secret. Hundreds of objects there. Very impressive.

PHH: You often accuse your critics of being pig-headed, doctrinaire, intolerant and even ignorant. How could such terms refer to a man like Dr. Duncan Lunan, who came to this very conference, at your invitation, to serve as a panel member? He is not afraid at all to be associated with a conference like this. And yet he is a critic. He thinks the evidence you have collected is of poor quality, confused, ambiguous. It points too many ways to be convincing.

EVD: Well. I don't agree with him ... in this case. Because we know so many theories, about the beginning of the universe, the rise of life, mankind's anthropology etcetera etcetera, which are generally accepted by science, and if you question those theories you will soon find a lot of facts that speak against them. But nobody says so. In this case, my theory, we have so many indications, and all these indications form a puzzle we call the evidence. Now I have no doubt whatsoever that some of these indications may be wrong, clear, but this is true of any other science! What is to say against the idea that extraterrestrials were here in antiquity?

PHH: Well, nobody takes that line of criticism. Story doesn't. He clearly states there is nothing absurd about the idea of aliens having visited our planet in ancient times. He just claims the evidence you present is faulty. The same with Carl Sagan. He never said the idea is preposterous a priori. Maybe they have been here, maybe not. It's just the way you have tried to prove they have that makes no sense to your critics.

EVD: Ah, Sagan! Sagan! You know, he said about *Chariots of the Gods* it's "the most illogical book of the century." Well, in my newest book, *Prophet der Vergangenheit*²⁰ [Prophet of the Past], I say this is the most stupid argument of the millennium. Ha Ha. Now, he is arrogant, this Sagan. Absolutely arrogant. Many years ago I admired him. But I have now lost all respect. It's gone ...

19 At the Munich conference, that is. (Eds.)

20 Däniken (1979). (Eds.)

PHH: You seem to have a lot of trouble with the people you once admired! You admired Heyerdahl, and Heyerdahl attacked you...

EVD: Oh, I still admire Heyerdahl. I just disagree with him ...

PHH: Another unrequited love: I recently bought a book by Professor Hoimar von Ditfurth. On the cover was a glowing recommendation – by Erich von Däniken. In a recent series of TV programmes on the German network Hoimar von Ditfurth called you a crank, a mystery monger, a crackpot whose distortion-ridden pseudoscientific pamphlets constitute a danger to society!

EVD: What are you telling me now? Oh, yes, I do remember. Yes, I once did write a recommendation. That's what surprises me so much in these people. That suddenly they crash down on you without even talking to you. I never met Ditfurth, I never met Sagan²¹ ... I invited Ditfurth to come here and speak against us. He didn't come. I invited many critics, as we have very prominent critics. No one came!

PHH: Duncan Lunan came!

EVD: But he is not a critic.

PHH: Oh yes, he is. Haven't you read his book *Those Mysterious Signals from Outer Space*²²? He pokes fun at you, and tells his readers there is no reason whatsoever to believe a word of your theories. In his lecture here he called the sort of evidence you collected all but worthless ...

EVD: Oh? I had no time to attend his lecture ... [short pause] Ah, there are so many indications, so many ... Why is nobody explaining it? Just take the case of Enoch. Why is Enoch telling us, "These are the names of the 200 Watchmen of the Sky that have descended"? Why is nobody attacking that statement? Have you seen that picture of the sculpture at El Baul in Guatemala? The man with the space-mask, and the cylinder on his back? I showed it to archaeologists. One of them said: To me it looks like a maize-farmer with a maize-bier on his back. Another said it was a ball-player. Okay, that is possible, everything is possible. But what is the most logical? Look: the helmet totally closed, with this object on his back, the air coming out, this tube ... I don't know. 2,500 years ago the extraterrestrial said to the prophet Ezekiel: "You human beings, you have eyes to see, and

21 In April of 1987, one of the editors (G.H.H.) had an opportunity to spend an evening with Carl Sagan in Pasadena. On that occasion, Sagan reported that he and von Däniken had in fact met. This, of course, may have happened after 1979 when Hoebens and von Däniken had this interview. (Eds.)

22 Lunan (1978). (Eds.)

yet you do not see.” Certainly you can attack every damn piece of evidence, that’s possible. Every indication can have a different explanation. It is the total of indications that makes the puzzle. I find it fascinating. How many different interpretations have we heard, scientific interpretations, of the planes of Nazca? About seven! And every time we hear a new one they say: Now we know it. Well, nobody knows. I don’t know it either. But at least my speculation should have the same value as other speculations. Why are we not taken seriously?

PHH: It is not the hypothesis as such that is ridiculed, but the way you set out to prove it. Your critics are not used to your kind of arguing. They can’t know it was your publisher who made *Chariots of the Gods* from over 400 pages of sound science into just over 200 pages of incoherent speculation.

EVD: Yes, but I didn’t do that!

PHH: Of course, but haven’t your critics the right to criticize the books as they are published?

EVD: Absolutely!

PHH: And *Chariots of the Gods* as it is now is very odd indeed. Most arguments in the book are not arguments at all, but rhetorical questions.

EVD: Yes, there are 238 question marks in *Chariots of the Gods*!

PHH: Question marks are hardly a convincing way to prove your point. A question may be answered in more than one way. But on the one hand you ask a question, and on the other hand you make it clear that you will accept only one answer. Remember how you interpreted the Palenque Astronaut. A child can see it’s someone in a rocket, you say. Who could possibly doubt it? Well, I can!

EVD: Do you doubt it? Don’t you see it?

PHH: Yes. To me the Palenque Astronaut looks like an Indian chief who fell into a cactus and now is crying for first aid.

EVD: Are you serious? You are joking, aren’t you? Hahahaha!

CHAPTER 4-02

Editorial Introduction

The paper to follow is one of Hoebens' first newspaper articles on skeptical approaches to parapsychology and a variety of other areas that today often are lumped together under the heading of "anomalistics." It was published, under the title "Kritisch comité rekest af met 'het paranormale,'" in the Dutch daily newspaper De Telegraaf, of which Hoebens was a long-term editorial writer, on September 23, 1978, p. T25. As Hoebens was to note several years later, in a letter to the British chemist, film director, music theoretician and former psychical researcher Denys Parsons, only earlier in that same year, 1978, he had begun to develop an interest in such fringe areas and their possible scientific and social relevance.¹

This newspaper article was written a few months after Hoebens had established contact with the US-based Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP)² of which Hoebens soon was to become (and loyally remain until his death) the Dutch representative – despite his many demonstrable deviations from a strict CSICOP party line in the years to come. Readers may find it interesting to compare the contents and tone of this newspaper article with another CSICOP-related paper that Hoebens wrote several years later (reprinted here as chapter 4-04; also see chapter 4-10). Robin Moore's assistance with the translation of the Dutch text was invaluable and is gratefully acknowledged. (Eds.)

CRITICAL COMMITTEE PUTS "THE PARANORMAL" TO THE TEST

UFOLOGISTS, CLAIRVOYANTS, WONDER DOCTORS AND SPOON BENDERS UNDER THE MICROSCOPE

Mysteries and more mysteries! There is more between Heaven and Earth than anything you or I could ever imagine in our wildest dreams!

Unidentified flying objects, driven by pyramid power, cleave through hyperspace. The green-tinted occupants want to warn us about the dangers of materialism. They are greatly intrigued by our pavement tiles, which, according to recent reports from Uden

1 For more details, see the preface to this book.

2 Meanwhile, the Committee slimmed down its acronym to CSI – a clever move.

in the Dutch province of Noord-Brabant, they subjected to thorough investigation in the middle of the night using equipment similar to giant vacuum-cleaners. Some eye-witnesses were later visited by men in black suits who advised them to keep their mouths shut about what they had seen. Those who ignore these warnings sometimes die a mysterious death ...

Aircraft and ships disappear with disquieting regularity in an area of ocean that has been known as the Bermuda Triangle, the seafarer's nightmare, since Columbus' day. No trace of the wrecks has ever been found. Sometimes, long after the initial disappearance, vague but alarming radio transmissions are received, which sound as though they come from a different dimension ...

There are huge airfields on the coast of Peru, which must have been built thousands of years ago. Detailed illustrations of manned space capsules have been found in centuries-old Mexican graves ...

A young Israeli can bend spoons and forks just by looking at them ...

The Apollo astronauts are sworn to silence about what they really saw on the moon ...

And now the most surprising thing of all; the astonishing ease with which millions of twentieth-century earthlings, of apparently sound mind, are capable of talking this kind of total nonsense!

Defensive Measures

In the United States in particular (although also quite commonplace in Holland), the belief in the paranormal, the enigmatic, the existence of the "Astral Plane" has grown to such an enormous extent in the recent past that the exact sciences community has felt obliged to take defensive measures.

University professors are discovering to their bewilderment that more students are firmly convinced that spoon-bending and divining are no less scientific than the theory of relativity or the theorem of Pythagoras.

A juvenile court magistrate in Ohio recently admitted that he draws the horoscope of juvenile delinquents before passing sentence.

The parliamentary Environmental Committee of the State of Oregon has voted in favor of legislation designed to protect the Bigfoot, a type of bipedal hominoid that until now has mainly demonstrated its paranormal powers by disappearing into thin air as soon as a qualified zoologist comes on the scene. So how do (real) scientists react to this fad?

Some ignore this phenomenon and say they have better things to do than pay any attention to the confused babbling of con-men and scatterbrains.

Others want to pick up the gauntlet by investigating the paranormal. They want to avoid dismissing the reality of the reported phenomena out of hand, but do feel that supporters of all things mysterious should subject themselves to the rigorous research methods used by conventional science. Moreover, they want to provide well-balanced information in order to protect the public against the flood of nonsense that currently threatens to engulf it.

The latter group of scientists recently joined together to form a "Committee for the Scientific Investigation of Claims of the Paranormal" (what a mouthful!). This committee, which is based in Buffalo (New York), organizes lectures and conferences, undertakes experiments, issues bulletins to the newspapers, radio and television channels and publishes its own magazine called *The Skeptical Inquirer*.

Even though the fourth issue of the magazine has yet to appear, it has already fought battles and won resounding victories in the mysterious world of ufologists, clairvoyants, wonder doctors and spoon-benders.

CSICOP investigates all claims, which, if they are to be validated, have to be confirmed by independent observers, preferably under experimental conditions. If somebody claims that meditating when the moon is full aids gaining transcendental insight into the Astral Plane, the Committee will let him carry on without interference. After all, you can hardly push a mystic into a retort, expose him to moon beams and then establish that his astral insight really has improved.

CSICOP only goes into action when, for example, somebody claims to be able to nullify the force of gravity simply by staring at his navel. Something like that can be verified!

Transcendental Meditation

The elderly hippie guru Maharishi Yogi recently offered a course in hovering, and making oneself invisible. Several people paid a couple of thousand guilders in order to master these useful techniques under the guidance of the great holy man.

James Randi, a CSICOP representative, publicly challenged the Maharishi's supporters to come and hover for a while in front of a small audience of expert observers. He offered a reward of 10,000 dollars for a successful demonstration. He has vainly been waiting for a response up to this very day ...

Randi is not a scientist; he is an illusionist. Illusionists play a prominent role in CSICOP, as they can be considered to have a sharp eye for trickery and deception. Randi has helped to conceive paranormal experiments that completely rule out trickery.

Consequently, the Israeli spoon-bender, Uri Geller, who has now been unmasked as a fraud by his own manager, was never willing to undergo such a test.

Modern-Day Copernicus

Jean-Pierre Girard had no such misgivings. This Frenchman had succeeded in convincing managers at steel company Pechiney of his psychokinetic powers (the ability to move material objects using the power of thought alone).

Pechiney's research director, Charles Crussard, had irrefutably demonstrated Girard's paranormal gifts and proclaimed himself "the new Copernicus" as a result of this earth-shaking discovery.

Four lengthy tests (Girard had agreed to the arrangements beforehand) were a complete failure.

The new Copernicus' comment: Randi himself had paranormal powers and had used them to sabotage the experiment!

The English spoon-bender, Judy Knowles, was also unmasked as a fraud by CSICOP. She stared for hours at a spoon but was unable to get it to move by even a fraction of an inch. The investigators had coated the bowl of the spoon with soot to make it impossible for human agents to come to the aid of the supernatural without leaving any traces.³

Hearsay

Another CSICOP supporter, Ronald Story, has become a specialist in the claims made by Swiss hotelier Erich von Däniken.⁴ The latter propagates the theory that extraterrestrial beings landed on this planet thousands of years ago, used genetic manipulation and education to bring its wild and barely intelligent inhabitants to a higher plane of civilization, and then went back home. According to von Däniken, there are many archaeological finds that support this theory. Story has looked critically at each of these proofs, one by one, with crushing results for the author of *Chariots of the Gods*.

3 Randi (1978). (Eds.)

4 Story (1977); also see chapter 4-01. (Eds.)

For example, von Däniken describes large libraries in a gigantic network of tunnels that, according to him, extends for great distances under the surface of Ecuador and Peru, in which sensational information about our extraterrestrial visitors has been found. Later, he was forced to admit that he had never actually been in these tunnels and had never seen a “book.” He heard the story from somebody whom he met in a café, who had in turn heard it from somebody else!

Treasure Trove

Von Däniken claims that “the greatest treasure trove of South America” is to be found in the inner courtyard of the Church of Father Crespi in Cuenca (Ecuador). He indicated that this collection (“the greatest archaeological sensation since the discovery of Troy”) contained irrefutable proof of the presence of extraterrestrial beings in the far distant past. Story demonstrated that Crespi’s “treasure trove” is no more than a collection of tin and bronze souvenirs that were manufactured in a workshop close to the church. A mysterious column that von Däniken had seen in Delhi, and which he claimed was made of an unknown alloy that was probably extraterrestrial in origin, turned out to be made of nothing more unusual than earthly iron.

The “True Story”

Sometimes excessively imaginative proclaimers of the paranormal betray themselves by mentioning details that conflict with claims they have made in their own written work or with objective information from other sources.

A striking example of this is the sensational book, *The Amityville Horror*, by Jay Anson,⁵ in which the “true story” is told by the Lutz family who moved into a haunted house in Amityville on Long Island and were then confronted by all kinds of gruesome manifestations of the Beyond. The story reaches its climax during a 12-hour hurricane on 13 January 1976. American [para-]psychologist Robert Morris⁶ simply looked up the meteorological records for the date in question. There was not even a storm on that day, let alone a hurricane! The book also contained other “facts” that are demonstrably incorrect. Weather reports also play a significant part in research

5 Anson (1977). (Eds.)

6 Morris (1978). (Eds.)

carried out by Larry Kusche,⁷ who has focused his attention on the so-called Bermuda Triangle.

Tall Stories

Writers like Charles Berlitz⁸ have served up the tallest of stories about ships and aircraft that have vanished in thin air in this area of ocean off the East coast of America.

The disappearances are claimed to be connected with the sunken continent of Atlantis, with extraterrestrial spacecraft, with parallel universes, with eddies in the space-time continuum and whatever other fantastical things you can think of. Kusche demonstrates with merciless logic that not a single one of these stories is based on any factual evidence. Berlitz claims that an average of two aircraft per month disappear in the Bermuda Triangle.

However, the statistics of the American air traffic safety service, which records all instances of missing aircraft, indicates that on average two and a half aircraft disappear in the area in question every year! Moreover, small private aircraft are involved in almost every case.

Safe as Houses

Three times as many aircraft vanish into thin air above the American mainland. So in fact, the Bermuda Triangle is statistically one of the safest places on Earth! Berlitz continually claims that certain ships and aircraft have dissolved into thin air in excellent weather conditions, during the day and close to the coast.

Kusche has referenced the reports of the American meteorological service and the records of Lloyds (where all shipping insurance policies are registered). His findings are crushing. In cases where Berlitz writes of “good weather, bright daylight conditions and close to the coast,” almost all instances are actually associated with “hurricane weather conditions, in the middle of the night and far out in the ocean.”

Berlitz’s reports of sensational “last-minute radio transmissions” turn out to have never been sent. Aircraft that he claims were hijacked by flying saucers appear to have landed normally. Members of the scientific community to whom he ascribes certain

7 Kusche (1977). (Eds.)

8 Berlitz (1974). (Eds.)

statements deny that they ever said anything of the sort. The entire Bermuda Triangle is just about as mysterious as the Dutch province of Noord-Holland. The only thing that has disappeared into thin air are all the dollars that millions of gullible members of the public spend on Berlitz' books.

Other CSICOP representatives who have been in the news are, for instance, Philip Klass,⁹ who has nipped one UFO story after the other in the bud, Donald Goldsmith,¹⁰ who has surgically cut Velikovsky's claims to implausible ribbons, Martin Gardner,¹¹ who has a sharp eye for the statistical traps that believers in telepathy and telekinesis stumble into time and time again, and William Bainbridge,¹² who has unmasked the whole theory of Biorhythms as a pseudo-science.

Critical Notes

As one might expect, CSICOP's methods have attracted strong criticism. And this criticism is sometimes not totally unfounded. For example, UFO exterminator Philip Klass often tends to make no distinction between fanatical flying saucer maniacs and respectable people who merely suggest that there may be more to UFO sightings than pure trickery and self-deception. While there are a thousand and one reasons that indicate that the existence of UFOs is highly implausible, Klass seems to have overshot the mark by turning his skepticism into a kind of religious dogma. It is however true to say that most of the criticism of the Committee originates from people who are simply not interested in rational answers to paranormal riddles because they have an emotional or material interest in keeping the Mystery intact.

Authors like Berlitz and von Däniken have earned fortunes from the gullibility of the public and they are obviously not well-disposed to anything that might reveal them as liars and fantasists.

Immunity

The true "believers" became immune to scientific counter-arguments a long time ago. They want to continue to see the world as a riddle, as that perspective discharges them

9 Klass (1978). (Eds.)

10 Goldsmith (1977). (Eds.)

11 Gardner (1957). (Eds.)

12 Bainbridge (1978). (Eds.)

from the responsibility of having to use their brains. Proof is nothing to them. They are convinced that scientific skepticism is no more than a conceptual handicap that blinds those who suffer from it to true transcendental reality.

But there are others involved as well and it is they who are the object of CSICOP's missionary ardor. These are the millions of reasonable people who feel, in all honesty and sincerity, that "some of it must be true," who are overwhelmed by the sensational tales of the paranormal, which are often couched in pseudo-scientific terms, who do not entertain the possibility of trickery and deception, who are insufficiently informed to be able to see through the spectacular claims of the new prophets, but who are also open-minded enough to want to look at the other side of the coin as well.

As for the skeptics on the Committee, they not only want to be truly confronted by the extraordinary, they are also prepared to pay heavily for the privilege of being proved wrong.

\$10,000

There is still a sum of 10,000 dollars in the safe in Buffalo for anybody who, in the presence of suspicious academics and seasoned illusionists, is able to demonstrate that the paranormal is more than just a flight of fancy.

Would-be beneficiaries from the Netherlands are also welcome.

CHAPTER 4-03

Editorial Introduction

The following conference report that originally appeared in vol. 7, 1982-1983, of the Skeptical Inquirer (no. 2, pp. 2-4) is devoted to the joint SPR/PA Centenary Jubilee Conference that was held at Trinity College, Cambridge, in August of 1982. Major parts of the report are devoted to the presence, presentations and reception of card-carrying skeptics during the conference as well as on the (then) seemingly improving relationship between (some) parapsychologists and (some of) their critics. Hoebens' own paper, that he presented during the conference, "Time machines, The Hume game, and a successful replication of a classic ESP experiment," is reprinted as chapter 2-02 of this book.

While one parapsychologist (Stanley Krippner) privately complained (and probably had a point there) that Hoebens had somewhat over-emphasized the relevance of the (privately organized) "metal-bending parties" at the tail end of this article, many (including Krippner) considered his report a fair and constructive representation of the Cambridge Centenary Conference – an account that, incidentally, reached many of the same conclusions that were drawn by prominent German science journalist Thomas von Randow who had observed the meeting for Germany's influential weekly Die Zeit (Randow, 1982). As might have been expected, some others – such as Brian Inglis (mentioned in the report) and Elmar Gruber (1982) – had rather seen the skeptics banned from the conference.

The conference report was checked against the original manuscript and is reproduced here with the kind permission of Kendrick Frazier (Skeptical Inquirer) and Barry Karr (CSI). (Eds.)

CAMBRIDGE CENTENARY OF PSYCHICAL RESEARCH: CRITICS HEARD, ENCOURAGED TO COOPERATE

One hundred years after a group of Cambridge scholars inaugurated the scientific investigation of things that go bump in the night, the question of whether there *are* things that go bump in the night remains an open one. "It is a measure of our failure that the controversy still exists," John Beloff admitted in his presidential address¹ to the Centenary Jubilee Conference of the Society for Psychical Research (founded 1882) and the Parapsycho-

1 Beloff (1983). (Eds.)

logical Association (founded 1957), which met, quite appropriately, in Trinity College, Cambridge, from August 16-21, 1982.

It is typical of the ambiguous feelings the “paranormal” arouses even among the members of the parapsychological community that the case for skepticism became one of the dominant themes of the occasion.

Card-carrying critics, such as Christopher Scott, Ray Hyman, Marcello Truzzi, James Randi, and Piet Hein Hoebens, presented papers or otherwise contributed to the discussion, but (presumably to the chagrin of dogmatics on both sides) they were by no means the only ones to argue for extreme caution in accepting evidence for “psi” at face value. Indeed, Brian Inglis, editor of a series of books published on behalf of the SPR and a determined believer in the unbelievable, complained about the “disease” of skepticism infecting his fellow psychical researchers. He had cause for complaint. British parapsychologist Susan Blackmore dropped a little bombshell by announcing what amounted to a conversion to skepticism. For years, she has tried to catch a glimpse of the occult, but “whenever I started to look into psi seriously, the evidence started to disappear.” Her present work, she said, is concerned with identifying the nonparanormal factors that could account for the persistence of paranormal beliefs. It is a healthy sign that Dr. Blackmore was not instantly ex-communicated.

To the contrary, many of the leading parapsychologists at the Cambridge conference expressed themselves unambiguously to the effect that, given the present unsatisfactory state of the evidence, skepticism remains a rational and valid option. What is more, they indicated that they would welcome closer cooperation with the critics in examining the evidence and in designing better experiments. Ray Hyman, a member of the CSICOP Council and the new occupant of the Stanford University “spook chair,” cast a cool eye on the celebrated “Ganzfeld” experiments, where ESP subjects are placed in a state of sensory deprivation – presumed to be psi-conductive. According to Hyman, there is a strong association between the presence of loopholes and the chances of obtaining significant results. (Similar conclusions, incidentally, were reached by parapsychologists Parker, Wiklund, and Ballard.) Ganzfeld pioneer Charles Honorton disputed Hyman’s analysis, but the gratifying outcome of the exchange was that proponents and skeptics agreed to join forces in an attempt to “debug” the Ganzfeld work.²

2 This eventually resulted in a “joint communiqué” by Hyman & Honorton (1986) with additional, and sometimes extensive commentaries by K. R. Rao, R. Rosenthal, I. L. Child, J. E. Alcock, C. Scott, G. H. Hövelmann, J. McClenon, J. Palmer, R. G. Stanford, D. M. Stokes, and J. Utts in an issue of the *Journal of Parapsychology* that was specifically devoted to the “Ganzfeld Debate.” This, however, was not the end of the story which, for obvious reasons, cannot be retold in this place. (Eds.)

By and large, the visiting critics were favorably impressed, not only with the quality of some of the papers presented, but even more by the parapsychologists' willingness to look at the other side of the psi coin. (On the other hand, the parapsychologists were pleased to discover that at least some of the skeptics are – to quote Cambridge psi-researcher Carl Sargent – “almost human.”) Even so, parapsychology remains a bewildering field where scientific sophistication coexists with appalling credulity.

Jerusalem psi researcher H.C. Behrendt presented a film showing “A New Israeli Metal-Bender,” Rony M. “There is no reason for doubt,” Behrendt pontificated. In fact, the film was an embarrassingly silly affair, showing nothing but clumsy tricks by a second-rate Geller. Parapsychologist John Palmer called the presentation “rubbish” – and requested that he be quoted. Even more embarrassing was the presentation of the so-called SORRAT evidence. In a workshop session (for which the Program Committee disclaims all responsibility), the irrepressible W.E. Cox showed slides, purportedly of miraculous events inside a “minilab” (a sort of fish tank in which various objects are placed to be moved “psychokinetically”). In an uproariously funny film, Tony Cornell of the SPR demonstrated how such marvels could have been brought about by simple trickery. At which point Brian Inglis left the hall, furiously reproaching the audience for laughing at very serious matters.

George Hansen of the Foundation for Research on the Nature of Man (FRNM, the late J.B. Rhine's institute) reported that he and Richard Broughton had attempted to replicate Mr. Cox's experiments in “spirit writing” – where a sealed letter containing a question and addressed to a third party is placed near a “minilab,” disembodied entities obligingly taking care of the answer, the postage, and the mailing! Hansen and Broughton found that the “spirit letters” in their experiment had been tampered with.

On Saturday, August 14, the *Times* of London reported the rumor that James Randi, who had submitted a paper at the conference, was planning some sort of coup “to prove the SPR and the Parapsychological Association a gullible body of fools.” A small panic broke out among the delegates. They had not needed to worry. Randi's contribution (on the proper methods to test “metal-bending”) was generally praised as an example of constructive criticism.

Yet, if we were reminded of E. J. Dingwall's words, written in 1926³, “[t]he study of occultism has so odd an effect on the human mind that even after a few years, when the conviction of the reality of supernormal phenomena has become fixed, the most transparent deceptions are gravely cited as marvels of mediumship,” the parapsychologists themselves (or at least a number of them) must be blamed.

3 This very likely refers to Dingwall (1927). (Eds.)

From the point of view of public relations, the staging of two “metal-bending parties” on the hallowed grounds of Trinity College was a disastrous lapse. “M-B-parties” are said to be the latest craze in the Washington, D.C., area. Guests are handed spoons and forks, are exposed to a peptalk, are instructed to yell “Bend! Bend! Bend!” and then proceed to ruin the cutlery they hold in their hands. The idea, I gather, is that the physically applied force accounts for only *part* of the bending – the residue being attributed to psi. Initially, I had assumed that the party in Cambridge was intended as a joke. I was amazed to discover that many of the participants took this preposterous business quite seriously. One visitor actually fainted upon having twisted his spoon. Another told me that she had heard a paranormal “voice” telling her that the metal had become soft. Yet another reported having felt a mysterious “energy.” I, too, was handed a spoon, and bent it effortlessly. Never will I forget the spectacle of a certain PA member who, jumping up and down with excitement, exclaimed that, yes, even skeptics could do it. Of course we can, if we may use our hands!

The next morning, while bewildered charwomen were cleaning up the mess, several parapsychologists of the more serious variety implored me to make plain to the readers of this journal that they were horrified by this sudden relapse into the crudest form of Gelleritis.

Their disclaimers indeed deserve to be reported. There may be a farcical side to psychological research, but at least there are influential parapsychologists who, while maintaining some sort of belief in the unknown, have successfully resisted the temptations of unreason.

CHAPTER 4-04

Editorial Introduction

Hoebens attended the first International CSICOP Conference that was held in Buffalo, New York, in October 1983. After his return he was invited by the editors of the (undeservedly short-lived) Belgian quarterly journal Psi-Forum to (a) explain to their readers his views on the proper role of reasonable and responsible skepticism and (b) provide a summary report of the Buffalo conference. Psi-Forum was published, in Flemish (Belgian Dutch), by the members of the Werkgroep Parapsychologie Gent [The Working Group on Parapsychology in the city of Gent, Belgium] which sought to strike a balance between scientific interest in the paranormal and a responsibly skeptical general attitude. In this respect Hoebens and the leading members of the Werkgroep had much in common.

Within a few weeks time Hoebens had prepared his report, which was published, in two parts, entitled “Skeptici bijeen,” in the first two issues of the first volume (pp. 17-20 and 97-103) of Psi-Forum. Those two parts are combined here into one article, as it was originally conceived, and they are published in English for the first time. Our translation into English has benefited from partial translations Hoebens himself had prepared (found in the Hoebens Files) and from the valuable assistance of Robin Moore.

Regrettably, the ambitious Psi Forum project ceased publication after only two years – much like the German Zeitschrift für kritischen Okkultismus in the mid-1920s, which made it to three volumes – but lasted long enough to publish both a death notice (Gondry et al., 1984-1985) and two memorial articles (Hövelmann, 1985a; Eeman, 1985) on Hoebens.

As far as we are concerned, the particular relevance of this two-part article lies in the way in which Hoebens holds up the mirror to overzealous skeptical ambitions. (Eds.)

SKEPTICS UNITED

In the parapsychological literature of the militant apostolic genre, scientific research into phenomena such as telepathy, clairvoyance and precognition is generally presented as a revolutionary undertaking that has already resulted in the triumph of a new paradigm. In as much as this type of literature deigns to mention the skeptics, they are portrayed as pitiable beings who have not at all understood the “The Signs of the Times” and who desperately cling to an antiquated,

nineteenth century view of the world that is both materialistic and reductionist. These fossils lack knowledge and insight, which of course explains their predilection for polemic ruses such as insinuation, distortion, false incrimination, ridicule and character assassination. For an example, read what the late Wilhelm Tenhaeff, a well-known Dutch parapsychologist, wrote some three years ago in the *Tijdschrift voor Parapsychologie* about the “negativists.” Good taste prevents me from quoting the passage in question here. I will include no more than the reference: *Tijdschrift voor Parapsychologie*, volume 48, no. 3, October 1980, page 131.¹

Intelligent parapsychologists (such as Martin Johnson, John Beloff, Brian Millar, Eberhard Bauer, Douglas Stokes, Charles Akers, Sybo Schouten, Gerd Hövelmann, Susan Blackmore, Robert Morris, Walter von Lucadou and the SRU quintet in Eindhoven) do not allow themselves to be misled by stereotypes of this nature. They know that the skeptics are a motley crowd, just like the parapsychologists themselves. Enlightened minds and fanatical idiots and every variant between these two extremes can be found on both sides of the dividing line (if there is such a thing as a dividing line at all).

In 1976, the first truly international organization of skeptics was founded in Buffalo, New York: the “Committee for the Scientific Investigation of Claims of the Paranormal,” abbreviated to CSICOP. The founders were scientists, philosophers, journalists and illusionists who felt that the time was ripe for a critical and rational answer to the neo-obscurantist “Occult Explosion” – a social phenomenon that is and was inextricably associated with Uri Geller, the Israeli spoon bender.

The committee set itself the task of “encouraging critical research into paranormal and pseudo-scientific claims” and “investigating these claims objectively and conscientiously rather than rejecting them on a priori grounds.” A program that is essentially the same as that of the British Society for Psychical Research, which was founded in 1882 and is the oldest parapsychological society in the world, and with which any scientifically oriented parapsychologist can agree without reservations. However, are the ostensible objectives a true representation of the facts?

A violent controversy about this very question arose as far back as 1976 and continues to this day. According to the antagonists (whose written work has now become so copious that it would fill a small library), CSICOP is not so much interested in open-minded critical research as in anti-paranormal propaganda. Theodore Rockwell, an American parapsychologist, speaks of “a crusade against the paranormal”² while Rockwell’s Dutch

1 See Tenhaeff (1980c). (Eds.)

2 See Rockwell, Rockwell, & Rockwell (1978). (Eds.)

colleague, Dick Bierman, recently complained about how CSICOP systematically “out-laws” non-conformist research programs.³

Even though I, in my capacity as the “contact member for the Netherlands” (and de facto also for Germany), maintain a close relationship with CSICOP and also regularly act as its representative, I will not conceal the fact that *I have often had to accept that our critics are often right*. There is a powerful, radical fraction within the committee that treats “parascience” with unconcealed hostility. These radicals have a habit of equating respectable researchers like Gauquelin, Beloff and Hynek with wags, charlatans and pseudo-scholars like Von Däniken, Berlitz, Velikovsky, Holzer, the Flat Earth Society and the “creationists.” This is a polemic trick that is known as “guilt by association.” My good friend, James Randi, has been guilty of this kind of counter-propaganda on more than one occasion and has been less affected by my rebukes⁴ than is propitious for the purity of his soul.

An error that many skeptics make – and which has repeatedly caused problems within the committee – is to think that “claims of the paranormal” will disappear like snow in the sunshine as soon as they are exposed to the penetrating light of skeptic Reason. Blind confidence in the unassailable correctness of their own views tends to cause critics to underestimate both the quality of the proponent and the complexity of the issue that is under discussion. I am firmly convinced that the explanation of the so-called “Mars Effect scandal,” in which CSICOP was recently involved, can be attributed to this.

This debacle focused on the “neo-astrological” claims of French psychologist and statistician Michel Gauquelin, who had discovered a significant correlation between the time of birth of leading sportsmen and women on the one hand and the position of the planet Mars relative to the horizon on the other hand. An ad hoc committee of CSICOP skeptics assumed that Gauquelin had made a fairly elementary methodological error and proposed a “conclusive experiment.” This experiment was performed – and the results supported the claims of the “neo-astrologist”! Initially, the skeptics obstinately refused to swallow the bitter pill. They tried to divert attention away from Gauquelin’s victory using all kinds of highly dubious post hoc analyses (which equated to a retrospective change in the rules). The whole story, which is anything but edifying, can be read in issues 9, 10 and 11 of *Zetetic Scholar*, the magazine published by the Center for Scientific Anomalies

3 This refers to Bierman (1982). (Eds.)

4 These can be found, for instance, in Hoebens’ review of Randi’s book *Flim-Flam!* (see chapter 4-13 in this book) and, in particular, as the Hoebens Files reveal, in numerous private letters to the magician. (Eds.)

Research. This Center was set up by Marcello Truzzi, a very prominent skeptic and former founding member of CSICOP, after he left the organization in 1977 in protest against the growing influence of the radicals.⁵

At the climax of the “Mars” affair (end of 1981, beginning of 1982), we (i.e., the skeptics who supported Gauquelin) sincerely feared that the committee had become a lost cause. The moderates (at least those who had not already left)⁶ felt that they would have no choice other than to terminate their membership, leaving the crusaders against the Paranormal in full authority. I am glad to say that those fears were unfounded. Last year, an interesting reversal took place within CSICOP.

Due in part to external and internal criticism, those who were responsible for the “Mars” fiasco published a “Reappraisal” in which they admitted that they had made serious errors when investigating the correctness of Gauquelin’s findings.⁷ Gauquelin was very pleased with this declaration and expressed his readiness to collaborate further with CSICOP. At present, there is discussion about a new test of Gauquelin’s “neo-astrological” research, which will be directed by Ivan Kelly, a Canadian psychologist and statistician and chairman of the CSICOP sub-committee for astrology, who incidentally has also won a reputation as a fair critic in pro-astrological circles.⁸ (Readers of this magazine will be interested to learn that Kelly has invited Ronny Martens⁹ to join the sub-committee.)

My impression is that the leaders of CSICOP have learned from the experiences of the “Mars” affair, and not just in terms of the committee’s future attitude towards (neo) astrology. In the recent past, there have also been more and more signs of a readiness to at least give parapsychology the benefit of the doubt. Various members of CSICOP – including psychologist Ray Hyman and the author – had already explicitly pleaded in favor of a constructive and friendly dialogue with the critical parapsychologists of the ilk of Johnson and Beloff – who we strongly feel are allies rather than antagonists in the battle against irrationalism. This view now seems to enjoy increasing support within the

5 See Clark & Melton (1979) and Hövelmann (2005a). (Eds.)

6 Several prominent skeptics such as astronomer Dennis Rawlins, statistician Persi Diacoinis and anthropologist Eric Dingwall had left the Committee under protest by that time. (Eds.)

7 Abell et al. (1983). (Eds.)

8 See e.g. Kelly (1982). (Eds.)

9 Belgian biochemist Ronny Martens at the time was a leading member of the *Werkgroep Parapsychologie Gent* and one of the editors of the journal *Psi-Forum*, in which this two-part report was published. (Eds.)

committee. This was clearly apparent during the first international CSICOP conference in Buffalo, which was held in October 1983.

In the first part of this article, I devoted some words of constructive criticism to the Committee for the Scientific Investigation of Claims of the Paranormal. This criticism was intended to introduce a personal report (read on for more details) of the first international congress of this committee of skeptics, which was held last autumn in Buffalo, New York.

Even though ill-informed parapsychologists like Bierman like to describe CSICOP as a single-minded society of destructively inclined vigilantes that faithfully adhere to the party line as set out by Professor Hansel while attempting to outlaw all forms of neo-science, the conference in New York made it clear that the committee is a prime example of a pluralistic group where the differences of opinion among the members are sometimes greater than those in relation to the nominal “antagonist.”

It is a pity that only a few representatives of parapsychology put in an appearance in Buffalo. Dr. Stanley Krippner – who chaired the Parapsychological Association last year – was invited by CSICOP on an all-expenses-paid trip in order to give a speech at the conference. He refused however, because he – as he reported to the PA board – feared that his presence would be misused by the committee for propaganda purposes! The psi researchers involved in Project Alpha¹⁰ were equally unenthusiastic about sitting in on a panel with James Randi, which was to look at parapsychology and illusionism during the conference. Other than Charles Akers (a parapsychologist who has been associated with Rhine’s FRNM institute¹¹ in Durham for many years, but who now participates in CSICOP activities), Dr. Robert Morris¹² was the only prominent parapsychologist who agreed to a face-to-face confrontation with the skeptics.

For the radicals in his audience, Morris’ speech, “The Evidence for Parapsychology: Some Strategies for Research and Evaluation” will have been something of a surprise. Morris’ argument was a paragon of sober, modest, down-to-earth and critical reasoning that was intended to lead to constructive dialogue. Not a mention of airy-fairy old wives

10 For evaluations of Randi’s ”Project Alpha“ scam, see Randi’s own accounts (Randi, 1983a, 1983b) as well as Hövelmann (1984b), Truzzi (1987), and Thalbourne (1995). (Eds.)

11 FRNM Institute: Foundation for Research on the Nature of Man. The institute that J. B. Rhine founded after his retirement in order to continue his parapsychological work. There are no longer any official links to Duke University.

12 Soon afterwards, Robert Morris was to become the Koestler Professor of Parapsychology at Edinburgh University in Scotland (see Hövelmann & Schriever, 2004). (Eds.)

tales à la Van Praag, just a rational evaluation of the strengths and weaknesses of contemporary parapsychology. On the subject of the Achilles' heel of parapsychology – the “replicability problem” – Morris said that the situation was a little less hopeless than some critics would like to believe. Experiments that focus on establishing functional relationships between “psi” and other variables are difficult to repeat in the sense that more than one team of researchers are able to report significant results with any kind of regularity. According to Morris, this series of “process-oriented” experiments currently forms the strongest “evidence” that pleads in favor of psi and the critics should therefore concentrate all their acumen on these tests.

“We hope intelligent criticism will focus on these areas so that either (a) progress will be made in our understanding of these apparent new means of communication and how to enhance them or (b) these studies will be shown to be flawed and not replicable when adequately done.” Morris concluded: “We feel that active cooperation between critic and researcher, ideally with each readily assuming the role of the other upon occasion, is vital to the evaluation of psychic claims.”

In his speech “The Evidence for the Paranormal,” the Canadian psychologist Prof. James Alcock (author of *Parapsychology: Science or Magic?*¹³) defended the challenging thesis that the results of recent psi research, where very often highly sophisticated methods are applied, are more consistent with the hypothesis that “psi” is an artifact rather than the hypothesis that it is an “unknown form of interaction between organisms and their surroundings.” (One talks of an artifact when the paranormal results of a test are in fact caused by an error in the structure of the experiment, but are interpreted by the researchers as an indication of the reality of a paranormal phenomenon.) Alcock’s argument: recent psi research (particularly that based on the so-called “Observational Theories”) has uncovered increasingly eccentric characteristics of the psi phenomenon. Psi appears to a major extent to be unaffected by factors which one would definitely expect to be important, i.e. time, distance, the familiarity of the test subject with the nature of the experiment, etc. Paradoxical results like these are typical of experiments where artifacts play a major role: “The finding that psi effects turn up whether one uses cockroaches¹⁴ or college students, whether the effects are to be generated in the present or the future or the past, whether the subjects know that there is a random generator to be affected, whether a sender and a receiver are inches or continents apart, this generalizability of psi to, it seems, almost any situation in which one matches subjects scores against a list weakens rather than strengthens the case for parapsychology.”

13 See Alcock (1981). (Eds.)

14 PK experiments using cockroaches as subjects were in fact carried out by physicist Helmut Schmidt (see Schmidt, 1979). (Eds.).

I will not discuss the speech made by the well-known British skeptic Prof. Mark Hansel here. Firstly because my notes on the speech have mysteriously dematerialized and secondly because Hansel did no more than repeat the arguments that he had already published in his book, *ESP and Parapsychology, a Critical Re-evaluation*.¹⁵

The Canadian science philosopher Prof. Mario Bunge put the fear of God into his listeners with a fundamentalist, skeptical sermon against “pseudo-scientists,” which he feels includes both psychoanalysts and parapsychologists.¹⁶

According to Bunge, the pseudo-scientists have benefited from the confusion in the field of science philosophy for some considerable time. Bunge’s colleagues – dixit Bunge – have created excessively simplistic standards for determining the difference between genuine and fake scientific phenomena, such that the application of those criteria cannot fail to lead to inconsistencies. Instead of a definition of science, Bunge offered a kind of “checklist” of conditions that a “cognitive field” has to satisfy in order to be recognized in scientific terms. He demonstrated – to his own satisfaction at least – that parapsychology satisfies almost none of these conditions. (Regrettably, I have to comment that his argumentation was based on a caricature of what modern parapsychology in fact encompasses.) It is remarkable that Bunge’s condemnation of parapsychology was also an implicit attack on the CSICOP skeptics program. After all: the committee “does not reject claims on a priori grounds, antecedent to inquiry, but rather examines them objectively and carefully.” Bunge, however, said: “There is always the fear that some golden nuggets may lie hidden in pseudo-science: that the latter may be nothing but protoscience, or emerging science. Such fear is quite justified in the beginning, particularly since an extremely original theory or technique – an unorthodoxy – may smack of pseudo science just because of its novelty. But caution must be replaced with skepticism and skepticism with denunciation if the novelty fails to evolve into a full-fledged component of science at the end of half a century ... And no caution at all is called for even at the beginning if the new idea collides head-on with the scientific outlook, the scientific method and the best established (yet of course fallible) scientific theories.”

The famous science philosopher Prof. Stephen Toulmin (of the University of Chicago) had a completely different story to tell.¹⁷ He explicitly warned against the risks of the skeptical dogma and illustrated his arguments by referring to historical incidents: “We

15 See Hansel (1980). (Eds.)

16 Subsequently published in the *Skeptical Inquirer* (Bunge, 1984). (Eds.)

17 Toulmin’s presentation also was published in the *Skeptical Inquirer* (Toulmin, 1984), immediately following Bunge’s “skeptical sermon.” (Eds.)

have to be continually aware of the risk that later changes in our theoretical ideas may force us to call some of our judgments in question. So the problem is that the line along which the distinction between the normal and the abnormal cuts itself has a long and complex history, which must induce a certain modesty even about our skeptical doubts.” Toulmin reminded his audience that various significant scientific discoveries had been rejected in the past with exactly the same arguments that some skeptics use today in their battle against the Paranormal.

Your scribe pleaded for cooperation between skeptics and rational proponents of non-conformist sciences.¹⁸ I argued that the readiness to enter into a collaboration of this type is a question of intellectual integrity. Ultimately, CSICOP professes not to reject “claims of the paranormal” on a priori grounds and prior to thorough and impartial investigation. This claim imposes certain obligations, in particular the obligation to seriously allow for the possibility that those who advocate in favor of “psi” may very well be right.

Having stated that a tolerant attitude relative to the parapsychologists can also have practical advantages (sympathetic critics are more easily able to gain inside information than aggressive crusaders), I jokingly said something along the lines of: “So even if you were an extreme skeptic, it would be worth pretending to be a supporter of the moderate line.” Correspondence that has been intercepted by my extremely efficient secret service¹⁹ indicates that this latter utterance has been minutely examined by a number of parapsychological scholars and taken to be proof of their long-held belief that the apparently affable CSICOP representative in the Netherlands is really a wolf in sheep’s clothing.

A pleasant surprise (at least for those who have always seen him as a “hard-liner”) was provided by the speech of CSICOP’s chairman, Prof. Paul Kurtz. Kurtz pleaded eloquently for skeptical moderation and warned against the danger of critical doubt degenerating into dogmatic rejection. “Science surely is not to be taken as infallible and some of the defects found in the pseudo- and para-sciences can be found in the established sciences as well, though on a reduced scale. Scientists are fallible and they are as prone to error as anyone else [...] Similarly it would be presumptuous to maintain that all intelligence and wisdom is on the side of the skeptic [...] We have made mistakes and have sought to correct them. We should not trust anyone to have all the truth and this applies to ourselves as well.” Kurtz emphatically rejected the ultra-skeptic dogma that paranormal phenomena should be viewed as “impossible”: “Sometimes what appears to

18 An abridged version of Hoebens’ talk also was published in the *Skeptical Inquirer* in 1984. It is reprinted here as chapter 4-08. (Eds.)

19 We know, but will not reveal, the “interceptor’s” identity. (Eds.)

be bunkum because it does not accord with the existing level of ‘common sense’ may turn out to be true.” He implored his listeners to adopt an “open-minded” attitude towards the serious proto-sciences, which he felt to include parapsychology and also “some aspects of recent UFO and astrological research.”

My personal highpoint of the conference was the brilliant performance put on by psychologist and illusionist Prof. Daryl Bem, a professor at Cornell University.²⁰ Bem used the techniques of illusionism and mentalism to give his students object lessons in “information-processing strategies.” His favorite method is to simulate a number of spectacular “paranormal” effects so convincingly that his audience firmly believes it has witnessed a true miracle. Subsequently, Bem goes on to explain which “strategies,” which incidentally are of enormous use in everyday life, have in this case led to a totally erroneous interpretation of what has been perceived. Bem: “The acceptance of the paranormal in the absence of compelling scientific evidence is the result of neither irrationality nor gullibility. Rather, it is a normal by-product of everyday information-processing strategies that usually serve us well.” In daily life, we simplify a complex reality by assuming certain causal relationships and interpreting our perceptions in the light of that explanation. Normally, this strategy works to our full satisfaction, but can in certain situations lead to false cognitive perceptions that subjects obstinately defend.

Bem illustrated his argument with an entertaining variety act. He played the “clairvoyant” whose task it was to determine what personal belongings a female student from the University of Buffalo had placed in a sealed box. (The girl was not an accomplice.) Bem’s performance was a resounding success: he even “saw” that there was a ticket for the Indianapolis 500 in the box. The skeptics in the room all had a smile on their lips: they could pretty much see what technique the performer was using. It was obvious that Bem continually followed the eye movements of his test subject, sometimes holding her hand and occasionally making strange gestures. There could be no doubt: Daryl Bem was sophisticatedly applying the techniques of muscle reading and “neurolinguistic programming,” techniques from the highest levels of mentalism, which allow the user to uncover surprisingly detailed information by carefully studying the reactions of the test subject to suggestive words and gestures. Any “believers” who are witnessing this performance would immediately think about ESP, said the skeptics to themselves, but we know what he is doing: it is just a question of muscle reading and neurolinguistic programming! At the end of his show, Bem revealed all: the skeptics had fallen for it. The sealed box stunt was in reality a standard illusionist’s trick. Bem had already sneakily found out what the contents of the box was before the performance started. All that gesturing, all the

20 Daryl Bem is a prominent member of the Parapsychological Association. (Eds.)

“neurolinguistic” staring was for one single purpose: to suggest a pseudo-explanation to the skeptics in the audience and thereby also demonstrate that avowed believers could also be fooled by their own “information-processing strategies.”

CHAPTER 4-05

Editorial Introduction

During the less than seven years that Hoebens was actively involved with the skeptical scrutiny of parapsychology and other areas of “anomalistics,” he must have read a library full of literature on most of these respective areas, as well as quite a few contemporary works in the philosophy of science. Moreover, he corresponded (often extensively) with just about everyone who was even marginally involved in any of the relevant areas. We have not counted them, but the Hoebens Files, in addition to all the other assorted material, must comprise thousands of letters with, probably, a five-digit number of pages. None of these letters were intended for publication, but a couple of them nevertheless made their way into print.

This was the case with a letter that Hoebens wrote to Kendrick Frazier, the editor of the Skeptical Inquirer, after he had read, in the fall of 1980, an interview of parapsychologist Jeffrey Mishlove with Hoebens’ fellow-skeptic Ray Hyman, professor of psychology at the University of Oregon. Originally, the letter was meant to give Hoebens’ private comment on the interview, but when Frazier suggested that he might print it in the “From our Readers” column of the SI, Hoebens resubmitted a very slightly revised version of his letter, which eventually was published, in 1981, in the Skeptical Inquirer, vol. 5, no. 3, p. 78. The title was added by Frazier. (Eds.)

NECESSARY DISTINCTIONS

I want to register my wholehearted support for the views expressed by Professor Hyman in the interview with Dr. Mishlove (*Skeptical Inquirer*, Fall 1980) and in his postscript¹ to the interview.

1 Mishlove & Hyman (1980, pp. 66-67). Hyman had commented: “I believe that, if many of my fellow critics had been with me at the Parapsychological Association meetings [in 1979] and listened to the many papers, they would have to agree with me that the quality of the design and the sophistication of the statistics were generally quite high. The discussions and criticisms of each other’s papers by the parapsychologists were of high quality and quite penetrating. And the parapsychologists seem to take as much interest as we skeptics do in finding loopholes, and possible alternative explanations, in each other’s work. I am not making a case for less criticism of parapsychology. On the contrary, I believe that parapsychology needs *more* criticism. But I also think the criticism needs to be relevant and deal with the evidence at its best.” (Eds.)

Hyman is right in insisting that the skeptics (of whom I happen to be one) should deal with the evidence at its best when attempting to pass judgment on parapsychology as a whole.

Of course the critics (and the critical parapsychologists) must deal with the nonsense too. After all, much garbage is sold to the public as “scientific parapsychology.” But the responsible skeptic should take great pains to avoid giving the impression that he is holding the Helmut Schmidts and the Martin Johnsons responsible for, say, the contents of Puharich’s biography of Geller.²

It has always been my view that the CSICOP should not only debunk the preposterous claims made in the name of “future science” but also promote understanding of the sophisticated efforts by some parapsychologists to overcome the problems inherent in their field.

Some supporters of the skeptical cause, alas, fail to make the necessary distinctions. For example, I was appalled at Professor Hammerton’s letter (*Skeptical Inquirer*, Summer 1980)³ in which he implied that the very fact that Dr. John Beloff was invited to contribute an article to *Encounter*⁴ constituted a victory for the forces of unreason.

I am happy that Professor Hyman has once again emphasized the difference between critical investigation and ritual denunciation.

2 See Puharich (1974). (Eds.)

3 Hammerton (1980, p. 80), in a letter to the “From our Readers” column of the *Skeptical Inquirer*, had lamented, sandwiched between other invectives against parapsychology: “The usually admirable journal *Encounter* promises an article on progress in parapsychology, to be written by Dr. Beloff, whose convictions are known.” (Eds.)

4 This refers to John Beloff’s article “Coming to terms with parapsychology”, in *Encounter*, 54, 1980, pp. 86-91. (Eds.)

CHAPTER 4-06

Editorial Introduction

The following paper probably does not need much introducing, because its peculiar history is properly reflected in its introductory section. It is a response to an article on skeptical positions (“Hoe wetenschappelijk zijn onze critici? [How scientific are our critics?]”) that physicist and parapsychologist Dr. Dick J. Bierman of the University of Amsterdam had published in the Tijdschrift voor Parapsychologie in 1982. When the Tijdschrift, after much toing and froing, refused to publish Hoebens’ response, the board of the Synchronicity Research Unit in Eindhoven offered to print it in their own journal, the SRU Bulletin, where eventually it was published, entitled “De balk in eigen oog,” in vol. 9, 1984, pp. 5-12.

The “Introduction” was specifically written for the SRU publication so did not form part of the initial manuscript. The article never appeared in English before; the editors translated it for this book from the published Dutch version. Please also note the “Editorial Postscript” to this chapter. (Eds.)

PEOPLE IN GLASS HOUSES ...

Abstract

Bierman’s complaint (Bierman, 1982) that the position of “the critic” is characterized by a preference for “unfalsifiable” arguments is shown to be mistaken. Bierman misrepresents the views of those he attacks. His discussion of the “betting model” is shown to be based on a fallacy.

Introduction

This paper was originally meant to be published in the *Tijdschrift voor Parapsychologie* (*TvP*). In that remarkable journal the Dutch psi-investigator Dr. Dick Bierman recently published an (if possible) even more remarkable article dealing with a group he calls “the critics” (Bierman’s quotation marks). Bierman’s paper clearly shows that the strength of his aversion to these critics is inversely proportional to his knowledge of the critical literature.

To show my consideration for the average reader of the *TvP* (who, after all, cannot be blamed for being kept in darkness by the anthropological-parapsychological propaganda he has been exposed to over many years) I set aside my aversion to this journal of the Dutch SPR and gave permission for the editorial board of the *TvP* to publish my answer to Bierman.

My conciliatory gesture was not appreciated by the majority of the editorial board. On the 24th of January, 1984, my manuscript, submitted almost a year earlier, was definitively rejected.¹

The reasons for rejecting it were given: the board did not appreciate the “tone” of parts of my paper. From personal communication with some members of the editorial board I was led to understand that the board thought ill of the fact that I was not prepared to assist the SPR in trying to cover the awkward truth about the late Professor W.H.C. Tenhaeff with a mask of secrecy. Beforehand I had informed the editors that publication would be *conditio sine qua non* for peaceful co-existence between the *TvP* and the Dutch representative of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP). The rejection of my manuscript made a definitive break in diplomatic relationships inevitable.

I very much appreciate the willingness of the *SRU Bulletin*, true to its reputation of integrity, open-heartedness and open-mindedness, to publish what the readers of the *TvP* were not allowed to read.

Response to Bierman²

In an earlier episode in its history the *TvP* regularly published polemical papers attacking the “negativists.” These papers distinguished themselves mainly by their rude insulting tone and wantonness in distorting the opponent’s views. The author of these papers, a well-known practitioner of “anthropological parapsychology,” was of the opinion that the evidence (particularly that provided by himself) for the reality of paranormal phenomena was solid to such a degree that reactions of unbelief could only be the outcome of “stupidity,” “foolishness,” “cowardice” or other kinds of mental or moral inferiority.

The readers of the *TvP* were never told the exact contents of the “negativists” views, nor were the “negativists” ever afforded the opportunity to reply.

1 The respective correspondences are preserved in the Hoebens Files. (Eds.)

2 This sub-heading was added for the present chapter to mark the start of the version of the paper as originally it was submitted to the *Tijdschrift voor Parapsychologie*. It did not appear in either Hoebens’ manuscript or the published text in the *SRU Bulletin*. (Eds.)

In the near future I hope to publish an extensive article³ about the many papers of the genre, which were written by this parapsychologist.

One single quotation will be sufficient to give the reader an idea of the level on which this savant treated his opponents: “Dat deze negativisten associatief verwant zijn aan personen die ontkennen dat er in Duitsland ooit concentratiekampen zijn geweest of Jodenvervolgingen hebben plaatsgevonden, behoeft wel geen betoog [It will be clear that these negativists are associatively related to those denying there have ever been concentration camps or persecution of the Jews in Germany]”. (Tenhaeff, 1980c, p. 131.)

I was under the impression that this not particularly uplifting polemic tradition had died a natural death. The paper “Hoe wetenschappelijk zijn onze critici? [How scientific are our critics?]” by Dr. D. J. Bierman (1982) makes me think, however, that I have been too optimistic.

It is true that Bierman does not seriously offend against good taste (and in that way positively distinguishes himself from the author of the above quotation). Nonetheless his paper elicited in me the well-known and in this case irritating feeling of “*déjà vu*.” The article is in no way a rational contribution to a discussion, but simply a propagandistic exercise.

The level of Bierman’s paper is so low that a serious reaction could be regarded as an undeserved mark of honor. When I nevertheless take the trouble to carefully expose Bierman’s fallacies, it is mainly because of my supposition that the average *TvP* reader will not be familiar enough with the skeptical literature to be able to recognize as such all the misleading elements in Bierman’s paper.

I emphasize that my reply is directed exclusively against the anti-skeptical *polemicist* Bierman, a somewhat unhappily formed secondary personality of the highly reputable *experimenter* Dr. D. J. Bierman.

Bierman’s definition of the critics (why always “critics” between quotation marks; is it intended irony?) is: “Those who take part in committees such as the Committee for the Scientific Investigation of Claims Of the Paranormal (CSICOP) or who endorse the views of these committees”. According to Bierman these critics adhere to a uniform ideology, thought up by the British psychologist Professor C. E. M. Hansel.

If we are to believe Bierman, Hansel and his adherents hold the view that the chance that, at a given moment, an authentic paranormal phenomenon occurred is always

3 No such manuscript was found in the Hoebens Files except for a few rudimentary sketches. (Eds.)

(Bierman writes “ALWAYS”!) smaller than the chance that the supposed paranormal phenomenon is fraudulent. This view makes the skeptical standpoint unfalsifiable.

This complaint would have some basis if “Hansel and his adherents” were indeed to defend the viewpoint attributed to them by Bierman. This is not the case, however. In short, Hansel’s fraud hypothesis states that “as long as parapsychologists have not solved the repeatability problem, the results of *single* experiments may not be considered *conclusive evidence* as long as these results *could* also have been gained by fraud.” The “falsifier” in Hansel’s reasoning is that the fraud hypothesis becomes untenable as soon as parapsychologists succeed in proving the superiority of the psi hypothesis, by means of one or more replicable experiments.

This is a completely reasonable point of view, implicitly or explicitly shared by intelligent parapsychologists. This has nothing to do with “systematic criminalization.” One could just as easily claim that parapsychologists systematically criminalize paragnosts by insisting on paranormal achievements under fraud proof conditions. This kind of criminalization is unavoidable because at the moment psi is defined entirely by means of a negative: the absence of non-paranormal causes. This negative way of describing a concept has specific consequences: if one wants to prove the existence of psi, one has to prove the absence of non-paranormal explanations. The critics cannot be blamed for the practical impossibility of doing so in the case of anecdotal evidence.

I do not want to present “the critics” as more rational than they really are (like Bierman uses a too flattering image of the group of which he is part) and I immediately admit that some of my nominal allies often misuse the “Hanselian” strategy. Had Bierman limited himself to lamenting that some critics try to discredit parapsychology in an unfair manner, then I should not have felt the need to argue. Indeed I have made similar comments myself. It looks as if Bierman fears that the use of nuance might detract from the rhetorical effects he wants his paper to have. That is why he insists on blaming the critics *collectively* for the established offense against the spirit of the scientific ethic. I cannot but think that he does so deliberately despite the fact that he knows better.

It cannot have escaped Bierman’s notice that within the skeptical movement which he dislikes so much, there are fundamental disagreements about the scientific respectability of parapsychology. CSICOP, although dominated by somewhat radical disbelievers, counts “liberals” as well as hard-line proponents. It is noteworthy in this context, that in the *Skeptical Inquirer* two members of the CSICOP-Board, Professor Ray Hyman and James Randi, have strongly criticized Hansel’s theories. Nonetheless Bierman feels free to stigmatize “the critics” in general as adherents of the “everything is fraud” theory.

Where Bierman's paper deals with the "consequences of our critics' attitude" and the "real damage done by fraud" it is almost unintelligible. A fragment such as "This does not alter the fact that possible fraud can really be damaging. Not to science of course, but, on the contrary, to society," does not contain any reasonable message, no matter how many times I read it. The context around these sentences, however, makes me think that Bierman attributes to the critics a longing to exterminate serious investigations of anomalies. This is sheer nonsense. When, some years ago, Professor Wheeler suggested banning the Parapsychological Association from the AAAS, CSICOP publicly resisted the idea. Several CSICOP members contributed in a positive fashion (as acknowledged by most of the parapsychologists present) to the SPR/PA Centenary Jubilee Conference in Cambridge. Many registered skeptics have repeatedly and unmistakably expressed themselves against the prejudice that "psi" could never be a topic for decent scientific research. I for one (associated with CSICOP, and thus falling under Bierman's definition of "a critic") have very specific reasons to consider the above passage in his article a preposterous allegation.

I would not be surprised, if some of Bierman's parapsychological colleagues fall through the floor from shame when they read his paragraph about the betting model. Skeptics should not be allowed to ask wonder workers to repeat their tricks under skeptical control, because most parapsychologists never claimed these miracles to be repeatable. The critics, however, should, on penalty of losing their scientific credibility, take up challenges à la Eisenbud.

Bierman, then, wants the critics at all times to be able and willing to prove after the fact that there was no psi in a specific historical case. Bierman refers approvingly to Eisenbud, who challenged Randi that the latter would not be able to reproduce the "thought photographs" of Ted Serios *under the same conditions*. This bet is based on a fallacy because it assumes just what is under discussion: that the conditions Serios needs to produce "thought photographs" are known and that Eisenbud's description of these conditions is complete and accurate.

Concerning Bierman's own "challenge" (Prove that the Japanese film about Croiset was faked): the parapsychological colleagues of Bierman mentioned earlier will fall even deeper into the earth on reading the paragraph dealing with this topic. How can it be that someone who pedantically presents himself as a methodological expert can put something as silly as this on paper? Or is Bierman displaying a certain cunning? Japan is not as close as, let us say, Woudrichem. By the way, if Croiset's Japanese case were to be outright fraud (other non-paranormal explanations are also possible); if there still exists evidence (traces of fraud can be erased) and if the appropriate documents should fall into my

hands, I will not hesitate to take advantage of this knowledge. Bierman may know what I refer to.

I now want to add a few words about Bierman's remarks on the Ted Serios affair. According to Bierman *the* critics often refer to fraud detected in this case: the fraud-story apparently goes back to an article "in which a photographer sketches out a possible method for fraud and makes allusions to a movement by Serios which was considered suspicious by the author." Bierman also mentions a story in the journal *New York*, in which it was stated that Serios had made a kind of confession. Later, again according to Bierman, it became "as clear as daylight" that the story was an invention from A to Z. But, Bierman says, "the critic" does not know this or prefers to forget about it.

These remarks of Bierman must be commented on:

1. The relevant article "in which a photographer ..." is a report, signed by two persons, about a visit a team of experienced conjurors paid to Denver, to observe the performances of Serios at first hand. The visit was instigated by, among others, the *Scientific American*. One of the members of the team was Dr. Persi Diaconis, now assistant professor in statistics at Stanford University. Diaconis is considered a top expert in the art of conjuring. The team found that the conditions under which Serios worked offered ample opportunities for fraud. When Diaconis saw the miracle man make a suspicious movement and wanted to verify his suspicions, he was prevented from doing so by the experimenters. Searching Serios was not allowed. Eisenbud seized upon the absence of Diaconis' name in the heading of the report in *Popular Photography* to suggest that this prominent scientist apparently wanted to dissociate himself from its tendentious contents. In 1978 Diaconis published a further paper, in *Science*, under his own name (Diaconis, 1978), in which he confirmed the findings of the earlier article in *Popular Photography*.
2. The "confession" by Serios was published in the journal *New York* in 1973 (Tobias, 1973). The author, A. Tobias, quoted a witness who declared that Serios said to his mentor Eisenbud something like: "The game's up, doctor, they've rumbled it." Thereafter Eisenbud is said to have burst into tears and to have refused to believe his subject. Later Eisenbud emphatically denied this story. I recently asked Tobias' witness for his comments. He confirmed the version in *New York* but added he could never prove he was in the right, because of the absence of other witnesses. The same goes, I suppose, for Eisenbud's denials. It is a case of the word of one against the other. Personally I am inclined to doubt the authenticity of Serios' "confession" (as well as the authenticity of his "thought photographs" I should add). Bierman insinuates that *the* critic does not know or prefers to forget about the publications in which the confession story is denied. I can assure Bierman that

I know Eisenbud's counter statements. Had I preferred to forget about them, then I should not have mentioned them in my critical review⁴ of Randi's latest book (Randi, 1982b).

3. I agree with Bierman's remarks about selective publication. I advise him to take his own warnings to heart. It is very easy to discredit *the* critic by saddling him with given opinions about Serios and then contrasting these opinions with a so-called "clear as daylight" but in reality quite tendentious version of the "facts."

Bierman writes as if parapsychologists were prevented from doing their real work because the critics force them to discuss the existence of "psi" over and over again (That is why he wants to send the critics off to Japan). But as it happens, many parapsychologists (among them respected experts) do not need be encouraged by skeptics to be willing to discuss the existence problem. The symposium "The Case for Skepticism" at the Conference in Cambridge was initiated by Dr. John Beloff, president of the Parapsychological Association. In his presidential address Beloff (1983) re-emphasized the importance of these distracting discussions. Beloff is, by the way, highly respected by many skeptics. The fact that he always takes the trouble to examine the critics' views before replying in a fair way, is seen as proof of his intellectual integrity.

I agree with the broad lines of Bierman's remarks about theory development. Bierman, however, makes it sound like something which will be strongly combated by *the* critics. This is sheer nonsense. Most skeptics accept parapsychologists who use the concept "psi" within the context of a research program and put the existence problem temporarily aside. I, for one, am almost convinced that the existence of "psi" can only be proven in a roundabout way, with the aid of a theory which explains the phenomenon to a certain extent. In this context I greatly admire the work of Dr. Brian Millar, another top parapsychologist, who freely discusses the existence of psi without being forced to do so by belligerent skeptics.

According to Bierman the critic's purpose is the destructive pursuit of parapsychologists' investigations into paranormal phenomena. As has been stated above, I do not want to make Bierman's mistake; thus I avoid generalizing and will not claim that there is no critic who cannot be accused of hampering parapsychologists. Some forms of criticism are destructive indeed, in intention as well as in their consequences. An example is afforded by the tirades of Dr. Wolf Wimmer (see, for instance, Wimmer, 1979), the style of which strongly reminds me of certain polemical compositions in earlier issues of the *TvP*.

4 See Hoebens' review, reprinted as chapter 4-13 of this book. (Eds.)

Rational critics (a prominent group, whose existence is carefully concealed by Bierman) restrict destructive pursuit, however, to those parapsychologists who make premature claims of having unshakable evidence and those who, after having been fooled a couple of times by some crafty conjuror, announce a revolution in thought about Man and Cosmos. And of course the destructive pursuit includes also those parapsychologists who try to discredit the critics collectively by use of half-truth and insinuations.

Conclusion

Bierman's paper is deplorable. Not because the critics attacked will personally take offense at Bierman's opinion of them, but because this way of "counter-criticizing" can only damage the interests of legitimate parapsychology.

Dutch parapsychology has only just started to recover from a very unhappy period in its history. In the past decades psychical research in this country, especially as far as the broader public interest is concerned, was dominated by a movement whose most important purpose seemed to be to confirm the worst suspicions of the skeptics. To enhance the process of recovery we more than ever need constructive dialogue among all those who, irrespective of their metaphysical pre-dilection, want to work towards a scientific answer to the question of whether "psi" exists and, if so, what it means.

One prerequisite for this dialogue is that proponents and skeptics take the trouble to listen to one another's ideas. The paper "How scientific are our critics?" shows that at least one Dutch parapsychologist will have to do his homework again.

Editorial Postscript

In a later issue of the SRU Bulletin Dr. Dick Bierman (1984a) replied to Hoebens with "Een reaktie op 'De balk in eigen oog' van P.H. Hoebens [A reaction to 'People in glass houses...' by P.H. Hoebens], SRU Bulletin, 9, 69-72. This was followed by Hoebens' rejoinder ("Een antwoord aan dr. D. J. Bierman [Reply to Dr. D. J. Bierman]"), SRU Bulletin, 9, 72-75, and a long "Postscript" to the debate by Bierman (1984b) in the same issue (pp. 75-76). That issue of the SRU Bulletin was published in October of 1984, the month of Hoebens death, and he may not have lived to see it in print.

In the process, both Hoebens and Bierman implicitly agreed to abandon further rhetoric exchanges, and there seemed to be some rapprochement of positions that served as a face-

saver for both parties. At the end of his "Postscript", Bierman (1984b, p. 76) noted: "Het komt mij voor dat de standpunten van Hoebens en mij eigenlijk niet zover uit elkaar liggen [It seems to me that Hoebens' and my views in fact are not that far apart]." If that's the conclusion – which was hinted at by Hoebens when, above, he pleaded for "constructive dialogue among all those who, irrespective of their metaphysical pre-dilection, want to work towards a scientific answer to the question of whether 'psi' exists and, if so, what it means" – this exchange that seemed to start on rather irreconcilable terms yet may have been worth while. (Eds.)

CHAPTER 4-07

Editorial Introduction

In her recent Master's Thesis in "Historical and Comparative Studies of the Sciences and Humanities" at the University of Utrecht, Ingrid Kloosterman observed: "In 1951 Tenhaeff was appointed as a salaried lecturer at Utrecht University and from 1953 onwards he held the first chair in parapsychology in the world at the same university. Tenhaeff remained on this chair until 1978 when he was succeeded by Henri Van Praag (1916-1988). Tenhaeff was a controversial figure. Not only were his investigations with his favourite medium Gerard Croiset contested,¹ he refused to give up his chair in parapsychology for over a decade when the Utrecht University asked him to in 1964. Because of the refusal of Tenhaeff to give up his chair, the Utrecht University initiated a second chair in 1974 and appointed the Swede M.U. Johnson as professor in parapsychology. This led to a unique situation: from 1974 to 1986 Utrecht University had two professors in parapsychology and two corresponding research institutes. The second chair [Johnson's (Eds.)] and the accompanying institute were an integral part of the Psychological Institute of Utrecht University." (Kloosterman, 2009, p. 10)

This situation formed the background for the following newspaper article Hoebens wrote on one individual candidate (who neither was the first nor the last one, as soon became apparent) for the special professorship. Henri Van Praag in fact would remain the special professor in parapsychology from 1978 until 1986.² The article was published, in 1984, under the title "Spookprofessor voor Utrecht?", in the daily newspaper De Telegraaf and was translated into English for this book. (Eds.)

SPOOK PROFESSOR FOR UTRECHT?

Is the Netherlands soon to boast a university professor in the study of spooks, ghosts and apparitions?

The "Studievereniging voor Psychical Research," the oldest parapsychological society in the Netherlands, intends to put forward William Roll, an American who was born in Bremen, for the post of associate university professor in parapsychology at the State

1 Almost single-handedly by Piet Hein Hoebens, as several chapters of this book show.

2 The last (2009-2011) special professor in parapsychology was German psychologist Dr. Stefan Schmidt who succeeded Dutch physicist Dr. Dick J. Bierman.

University of Utrecht. He would succeed the current “stand-in pope” drs. Henri van Praag, who in turn succeeded the notorious Prof. Dr. Wilhelm Tenhaeff.

William (Bill) Roll, who in his field is viewed as a respectable but slightly marginal figure, specializes in research into spooks, ghosts and apparitions. He has written a complete book³ about the phenomenon of the Poltergeist. He has also enriched the English language by inventing the term Recurrent Spontaneous Psychokinesis (RSPK), which means just the same as “spook” but sounds much more scientifically acceptable.

Adolescents

Does Roll really believe in spooks and ghosts? In one sense he does and in another sense he does not. Roll does not think it likely that ghostly apparitions are caused by supernatural beings or by the souls of the peevish dead. He assumes that this type of phenomenon is caused by the minds of the living; generally adolescents having trouble adapting to their environment.

An occult event that recently made the headlines took place in the American town of Columbus, where a 14 year old girl, Tina Resch, seemed to be the focal point of a veritable eruption of RSPK. Telephones flew through the air, pieces of furniture raced each other in a mock sprint, and eggs managed to mysteriously find a way past the fridge door before smashing themselves to pieces against the ceiling. Roll went to have a look and became convinced that the phenomena were genuine and not some kind of deception.⁴ On the other hand, experienced illusionists, like James Randi, are sure that the whole Columbus affair was a set-up.

Mr. Roll’s appointment is by no means certain. Associated university professors do not earn a state salary and the SPR has no money to spare. So a solution for the financial aspects has yet to be found. Moreover, Roll has yet to receive his Ph.D.; he is working on a dissertation about the connection between epilepsy and spectral phenomena, but it may be a while before he is entitled to put a Dr. in front of his name.

3 This refers to Roll (1972). (Eds.)

4 Two decades later, Roll published an extensive review of the Tina Resch case and of the rather tragic post-poltergeist history of its presumed agent (Roll & Storey, 2004). (Eds.)

Cunning

Moreover, insiders reckon that Van Praag's move in putting forward Roll may just be a cunning way of gaining time. It is thought that the current professor may want to put forward his own candidate, an ambitious young pupil of his, called Douwe Bosga.

Until recently, Bosga was best known as a flying saucer expert, but since his "graduation" from a non-accredited university, he prefers to call himself a parapsychologist.

Roll is not the first candidate for the post either. When it became known about two years ago that the SPR was looking for a successor for Van Praag – a well-known popularizer of the occult philosophy, but not exactly seen as a representative scientist by his professional colleagues – two candidates applied initially: the chemist and mathematician Dr. Brian Millar from Scotland (but resident in Utrecht and fluent in Dutch) and the Amsterdam physicist Dr. Dick Bierman.

Reputation

Ultimately, both were rejected. That has nothing to do with their qualifications. Both Millar and Bierman have a good reputation and the Scot is even viewed as one of the most brilliant representatives of truly scientifically oriented parapsychology.

The problem was that Bierman or Millar were likely to intrude upon the activities of the conventionally funded (i.e. state-salaried) chair of parapsychology, which is occupied by the Swede, Prof. Dr. Martin Johnson. The university and the SPR want the new associate professor to carry out "anthropological" work. What that means exactly is anybody's guess, but the intention is that the work domains covered by the two chairs will complement each other without too much overlap.

The next candidate was Utrecht clinical psychologist, drs. Leo Pannekoek (who, according to local gossip, had already told bosom friends that they might congratulate him on his new appointment).

But Mr. Pannekoek was equally unsuccessful, probably because influential figures found his philosophical views to be too vague. The names of an Englishman, Julian Isaacs, and an American, Jerry Solfvín, were also bandied about at a later date, but eventually the SPR decided in favor of Bill "spook expert" Roll.

CHAPTER 4-08

Editorial Introduction

In his two-part article “Skeptici bijeen [Skeptics United]” in the Flemish journal Psi-Forum, reprinted here as chapter 4-04, Hoebens had reported in considerable detail about the first International CSICOP Conference that was held in Buffalo, New York, in October of 1983. A slightly abridged version of Hoebens’ own presentation to that conference was subsequently published in the Skeptical Inquirer, 8 (1984), pp. 227-229. That paper, which calls for international cooperation on various levels, is reprinted below with the kind permission of Kendrick Frazier and CSI. (Eds.)

NETHERLANDS: PSYCHIC SURPLUSES AND A CALL FOR INTERNATIONAL COOPERATION

The state of belief in that part of Europe I consider to be my *fief* does not, I think, fundamentally differ from the state of belief in other parts of the developed world.¹ There is a pattern of paranormal beliefs that is simply part of modern life. It is a universal phenomenon with basically insignificant local variations.

In the Netherlands people are no different from Americans, French, Italians, or Australians, in that they find it hard to resist the temptations of the unbelievable. However, they do not necessarily select the same phenomena as targets of their credulity. The supply of miracles also has characteristics particular to the area.

For example, Holland has a surplus of so-called psychic detectives, to the point that they have become something of an export item; but there is a definite shortage of poltergeist cases, and flying saucers are also sadly underrepresented.

Holland is probably unique in that back in the fifties the queen officially received George Adamski at the Royal Palace for an eyewitness report on current affairs on Venus; but no resident of the kingdom has ever been granted the privilege of a UFO abduction, and, for some reason, those extraterrestrials given to the disconcerting habit of cattle mutilation will not touch our cattle.

1 There are (at least) two book-length treatments, written from differing perspectives and with different aims, on the history of parapsychology in the Netherlands: Dongen & Gerding (1983) and, of more recent date, Kloosterman (2009). (Eds.)

Dutchmen think the British superstitious for believing in ghosts, and they think the Americans gullible for having swallowed Peter Hurkos. Ghosts and Peter Hurkos are nonsense, most Dutchmen think – unlike faith healing, Gerard Croiset, horoscopes, and the miraculous powers of copper bracelets. We may not have a chief of state who believes in astrology,² but Leiden University supports a professor of Blavatskian metaphysics.³

Instead of giving further examples of Dutch beliefs and unbeliefs, I wish to offer a few words about some problems – both philosophical and practical – facing those who are worried by the “occult explosion” and who feel the urge to actively promote the skeptical viewpoint.

Problem No. 1 is the danger of excessive polarization between skeptics and so-called believers. If you carefully look at the history of the paranormal controversy you will find that serious mistakes have been made on *both* sides. Some of the predecessors of CSICOP have fallen into the trap of skeptical dogmatism, and have thereby lost their effectiveness. The best way to avoid this trap, I would suggest, is to seek cooperation, whenever possible, with other groups – especially those that are the targets of our skeptical curiosity.

There are several reasons why we should seek to be on speaking terms with those proponents of paranormal claims who have indicated that they are willing to play according to the rules of the scientific game.

The first reason is simply intellectual honesty. CSICOP associated skeptics are committed to the point of view that claims of the paranormal may not be rejected on a priori grounds, antecedent to inquiry. This can only mean that we must seriously consider the possibility that the verdict of history will be that we were wrong and the proponents were right on a number of issues.

The second reason is a more practical one. If we want to debate the paranormalists fairly and effectively, we must take pains to ensure that we are criticizing what the other side is really claiming and not attacking straw men. The pro-paranormal literature is vast and complex, and few skeptics can afford to spare the time and the energy needed for keeping abreast of all developments in, say, parapsychology or astrology. It is very easy for

2 This alludes to former US President Ronald Reagan who, according to many sources (and quite a bit of credible backstage information), held strong beliefs in astrology. The leading German political magazine, *Der Spiegel*, devoted an extensive cover story to the astrological inclinations of the President and the First Lady (Anon., 1988), and even White House officials conceded the fact in the *New York Times* (Roberts, 1988). (Eds.)

3 Until recently, that chair was occupied by Dutch philosopher (and PA member) Prof. Dr. Hans Gerding, who had earned his Ph.D. in philosophy with a very substantial dissertation on *Kant and the Paranormal* (Gerding, 1993) from the University of Amsterdam. (Eds.)

a skeptic accidentally to overlook a possibly important piece of information that tends to support the nonskeptical hypothesis and thus expose himself to the charge of data selection or even misrepresentation. We can avoid such risks by keeping in touch with knowledgeable proponents and by consulting them before we attack certain claims in public.

The third reason is contained in a quotation from Ray Hyman: “The believer-skeptic dichotomy tends to overshadow the fact that there are important overlappings of common interests and goals that cut across this dichotomy.” Several proponents of paranormal claims are nonskeptics only to the extent that they predict that the application of scientific research methods will result in the vindication of certain extraordinary claims. However, the proponents I am referring to here are very conservative in their methodology and in their philosophy of science, and they will usually agree with the skeptics on such issues as to what constitutes acceptable scientific evidence. So these people are really on our side, and we should be careful not to lump them together with the real crackpots.

The fourth reason is practical and perhaps even a little Machiavellian. Proponents of the paranormal are often extremely good sources of information for skeptics. The skeptic who restricts himself to critically reading the literature may get a somewhat unrealistic idea of what is really going on in the alternative sciences. For proper perspective, we cannot dispense with the backstage information, with the gossip that only the insiders will be able to give us. However, these insiders will only part with this information if they know that they can trust us.

Problem No. 2 is far more practical than problem No. 1 and is basically about the accessibility of relevant sources. Much valuable information is contained in books and journals that have become very rare and difficult to obtain. Often this literature will give us clues to the solution of paranormal problems that puzzle us today. Some proponents like to buttress their case by citing spectacular anecdotes of ancient miracles – and are not contradicted for the simple reason that few if any skeptics are aware of the published criticisms. A related problem concerns the skeptical literature that continues to be cited by enthusiastic unbelievers who are not aware of the published rejoinders.

The fact that a book or article concludes the nonexistence of a paranormal phenomenon does not guarantee that it is reliably documented or that it reaches its negative conclusions on the basis of adequate evidence. It is always risky to rely on secondary sources. To locate the primary sources, however, involves a lot of work. In practice, this can only be done if skeptics from all over the world pool their resources. What we need is a comprehensive and critical bibliography of the literature – and here I mean the literature *pro and con*.

A final problem I want to discuss briefly is the language barrier, which often complicates or even renders impossible skeptics' attempts to obtain reliable information on certain much-publicized claims. Many such claims are effectively protected from debunking assaults by the simple fact that the relevant documents are only available in a language with which the debunker is not familiar. Thus I have found that many prize cases involving the internationally renowned Dutch psychics Hurkos, Croiset, and Dykshoorn continue to be cited as strong evidence for ESP in English-language publications, whereas in fact these claims are fantastic distortions of entirely nonspectacular incidents. The documents proving this, however, are in Dutch or in German and are not accessible to someone who does not read those languages.

Recently, the *Skeptical Inquirer* had an excellent article (Worrall, 1983) exposing iridology.⁴ It was, as I said, an excellent article but the author could have considerably strengthened his argument if he had been aware of the existence of the voluminous critical literature on this subject published in German.

Another example: An American skeptic who reads the recent book by Eysenck and Sargent (1982), *Explaining the Unexplained*, may be impressed by their claim that the celebrated Rosenheim poltergeist case had been the subject of a full and critical investigation and that – I quote – “despite the fact that many people – highly trained in the different disciplines – were looking for evidence of fraud all the time, no hint of it was ever sniffed.” Very impressive, and you have to be able to read German to be able to read the police reports where evidence of patent fraud *was* presented.

A final example of how the language barrier tends to protect false miracles. In a recently published book (Bender, 1983a), purporting to be a scientific evaluation of the phenomenon of precognition, the very famous German parapsychologist Professor Hans Bender, a former President of the Parapsychological Association, presents as one of the precognitive prize cases of all time the case of the American student who in 1977 told the president of Duke University about a dream he had had concerning an aviation disaster on Tenerife, with two 747s colliding and 583 people dead. The student had thus an unimpeachable witness when, ten days later, this disaster actually occurred. According to Bender, this claim had been reliably documented. He defied the skeptics to explain this one. It was more or less by accident, that this fake claim was quickly exposed, in spite of the language barrier.⁵ Normally, the English speaking skeptics would never have heard of

4 Hoebens (Iridology critiques in Germany. *Skeptical Inquirer*, 8, 1983-1984, 188-190.) commented on Worrall's article and supplied some of the information from the German sources he is referring to. That comment is not included in this book. (Eds.)

5 See chapter 3-12 in this book. (Eds.)

Bender's claim, published in German only, whereas the German speaking skeptics would have been quite unaware of the English publications in which this miracle is exposed as a stunt by a talented amateur magician.

There is a need for intensification of international skeptical cooperation, so that no questionable miracle is safe from skeptical investigations even if it takes refuge in a distant country.

CHAPTER 4-09

Editorial Introduction

*In 1981, Teleboek publishers in Amsterdam issued a Dutch translation (Stevenson, 1981) of Prof. Ian Stevenson's seminal book *Twenty Cases Suggestive of Reincarnation* (Stevenson 1974a). Hoebens used the release of the Dutch edition of Stevenson's book to describe for his readers, in a fairly long article in the Dutch daily newspaper *De Telegraaf*, several of Stevenson's cases "suggestive of reincarnation" and to explain some of the principal empirical and theoretical difficulties involved with this kind of research. Under the headline "Ik was iemand anders" (with the slightly quaint subtitle, "Biya, in 1939 aan een hartkwaal overleden, was in 1948 herboren als Swarnlata"), the article was published in the newspaper's September 12, 1981, issue (p. T29).*

*At the end of his article, Hoebens had briefly referred his readers to the fact that, on various occasions over the previous two decades, Indian philosopher-parapsychologist Prof. C.T.K. Chari¹ had suggested that Stevenson's cases might be more adequately explained in terms of Indian social psychology than as examples of reincarnation. Hoebens therefore had drafted an almost literal translation of his newspaper article and had sent it to both Prof. Stevenson (1918-2007) and Prof. Chari (1909-1993). In a couple of private letters (preserved in the Hoebens Files), Stevenson pointed out that he felt his cases generally were stronger than they appeared from the way Hoebens had presented them, but he applauded the apparent intention and the general tone of the article. Chari, on the other hand, drafted an English manuscript of his own and placed it at Hoebens' disposal for possible publication in the newspaper. Accordingly, Hoebens slightly edited and translated Chari's commentary into Dutch and published it, with an editorial introduction, in the December 19, 1981, issue of *De Telegraaf* under the title "Prof. Chari licht kritiek op boek over reïncarnatie toe."*

Our English version of Hoebens' article is based, with very few minor corrections, on the translation that Hoebens had provided for the information of profs. Stevenson and Chari. We are especially pleased that, when initial plans for the production of the present book arose back in the mid-1980s (for details, see the preface), the late Prof. Chari had granted his permission to also reprint his commentary along with Hoebens' article. Chari's comments, which here are appended to Hoebens' review, literally follow his original English manuscript. Hoebens' article and Chari's comments are published in English for the first time. Also see our "Editorial Postscript" at the end of this chapter. (Eds.)

1 Examples can be found, for instance, in Chari (1967, 1978, 1981).

“I WAS SOMEBODY ELSE”

BIYA DIED OF HEART DISEASE IN 1939 AND WAS REBORN AS SWARNLATA IN 1948

On the 2nd of March 1948, a daughter was born to the family Mishra, who lived in Shahpur in India. There was something remarkable about her. When Swarnlata was only a few years old, so the story goes, her father took her on a trip to Jabalpur. When passing the little town of Katni, the child suddenly asked the driver if it would be possible to take the road to “my house.” There they would be offered a “nice cup of tea.”

Swarnlata’s father found it not a little strange. When they were back home, his other children told him that Swarnlata had regularly spoken about her earlier life in Katni, as member of a family Pathak. She remembered all kinds of details about the house she used to live in, as well as the members of the family.

When Swarnlata was 10 years old, she learned that the wife of a professor Agnihotri, whom she had met in her new place of residence Chhatarpur, originated from Katni. She then claimed she had known this woman in her earlier life.

A Certain Biya

A well-known Indian parapsychologist, H. N. Banerjee, heard about the case. He recorded the claims in writing and traveled to Katni to find out whether someone had lived there who fitted the description Swarnlata had given of her previous life. He did find such a person: a certain Biya, daughter of the family Pathak, and married to a Mr. Pandney of Maihar. Biya had died from a heart attack in 1939.

Later that same summer relatives of the late Biya journeyed to Chhatarpur. Biya’s brother Hari Prasad arrived without prior notice at the Mishra home. Swarnlata is said to have recognized him and to have addressed him by his pet-name “Babu.” Nor did Biya’s husband introduce himself; but Swarnlata said she recognized him and demurely lowered her eyes, as good Hindu women are supposed to do in the presence of their spouse.

Moneybox

But she was not too shy to remind Mr. Pandney of the fact that he had one day taken 1,200 rupees from the box in which Biya kept her money. According to his son Murli, Pandney

later admitted that this was true. Murli himself tried to mislead the girl by claiming he was someone else, but she did not fall into the trap. Shortly afterwards Swarnlata and her family reciprocated with a visit to Katni and Maihar. The child recognized many of Biya's relatives and acquaintances and addressed some of them by their correct names. She identified the cowherd of the family Pathak, even though bystanders tried to fool her by saying that the cowherd was long dead.

For the Pathaks the affair was plain as a pikestaff: Biya, who died of heart disease in 1939, was reborn as Swarnlata in 1948. A clear case of the transmigration of a soul. From then on the girl from Chhatarpur was considered a member of the family. When staying in Katni she behaved like the elder sister of Biya's brothers, whose average age was 50! Nobody found that strange. Murli accepted her as the reincarnation of his mother, although he was old enough to be her father.

Evidence

This was the state of affairs when a renowned American psychiatrist-parapsychologist, Professor Ian Stevenson from the University of Virginia, visited India in 1961, searching for evidence of reincarnation.

He was given access to the reports of his colleague Banerjee and himself spoke with the witnesses. He was deeply impressed and devoted an extensive chapter to Swarnlata in his famous book, *Twenty Cases Suggestive of Reincarnation* (Stevenson, 1974a). A short time ago a Dutch version of this book became available.² I shall review the merits (and shortcomings) of this book later in this article. But first a few words about reincarnation in general.

From Ape Man to Lady-Reporter

From their youngest days Christians are (or were) taught that man has but a single earthly life, followed by an eternal stay in one of the two main departments of the hereafter. Many non-Western cultures adhere to a different view however. Hundreds of millions of people are expected to believe that the soul of a human being moves to another body after death. Souls are recycled. Death is only a passing interlude. Buried today, in a new cradle next year. And so on, until the Wheel of Rebirth finally comes to a halt. According to the Buddha this occurs when mankind discovers that the whole of existence is an illusion; a very profound idea I shall not explore further here.³

2 Stevenson (1981). (Eds.)

3 For an excellent and very extensive 870pp. historical survey of the concept of “Seelen-

Buddhists and Hindus do not need any scientific evidence to be able to believe in reincarnation. Many Westerners do, however. Does such evidence exist? Every Dutch bookshop boasts at least one shelf full of books that answer the question with an emphatic “yes.” This answer is justified by appeal to two kinds of parapsychological investigations.

Regression

The first one is the investigation of spontaneous cases such as that of Swarnlata. The second one is hypnotic regression, a technique in which a person is hypnotized and then tries to recall any memories of previous lives.⁴

Right now this “regression” method happens to be in fashion. One comes across more and more people who have personally taken advantage of this luxury device. Reincarnation hypnotists do good business. Emmy van Overeem, lady-journalist with *Elsevier*,⁵ always ready for an occult adventure, recently let an expert in Nijmegen take her back to a previous life as an ape-man. Thereafter she was, into the bargain, given a trip to a future incarnation! In the year 4400 everything will be peace and amity, a relieved Emmy reported after having returned from the trip. “The earth finally had a new face. Technology served man, not the other way around. We could, for example, communicate with one another by means of telepathy.”

There are sound reasons to assume that these kinds of recollections are the product of fantasy. Literature dealing with this kind of “evidence” can for the most part be best classified as cock-and-bull stories.

The material Stevenson uses in his book is of a quite different caliber, however. All of his “twenty cases” concern children (from India, Ceylon, Brazil, Lebanon and Alaska) who at some point, it seems, started to claim they had previously been “somebody else.” And they were able to give so many particulars about that “somebody else” that it was possible to identify the previous personality. Stevenson is a cautious man. In his book he does not exaggerate, nor does he force any opinion upon his readers. Reading between the lines one can easily see that he is a passionate supporter of the concept of reincarnation; a right one cannot deny him. *Twenty Cases* is a well-written and serious book, in which the author defends a fascinating theory with laudable care.

wanderung“ (reincarnation) in the European context, from antiquity to the late 20th century, see Zander (1999). (Eds.)

4 Even today, many books on hypnotic regression are modeled after the example set by Bernstein (1956); for a critical appreciation of Bernstein’s book, see Ducasse (1960). (Eds.)

5 *Elsevier* is a popular Dutch weekly magazine. (Eds.)

I should advise the reader, however, to be even more careful than Stevenson himself. I cannot escape the impression that reincarnation research has problematic features which are somewhat under-exposed in the book.

“Memories”

Take the case of Swarnlata, with which this paper started. At first sight it looks watertight. But ... reading Stevenson’s text carefully, one realizes that an important question remains unanswered: did Swarnlata ever mention the name “Biya” before Banerjee started his explorations and found that Swarnlata’s “memories” nicely fitted Biya? Is there a possibility that Swarnlata’s “recollections” were slightly “adjusted” afterwards? Did Banerjee present the case as more favorable than it really was? H. N. Banerjee is by no means irreproachable. The parapsychological journals have published strong indications that he tampered with the results of experiments.⁶

Did Stevenson always pose the right questions? Did he never, in his enthusiasm, forget important facts? Why has the prominent Indian parapsychologist Prof. C.T.K. Chari, who has himself investigated dozens of “cases of reincarnation,” always expressed himself so critically about Stevenson’s work?⁷

In this context it is worthy of note that Stevenson’s former fellow-worker Dr. David Read Barker (an anthropologist who speaks Hindi) now also has his doubts. In a recent issue of the *European Journal of Parapsychology* he reports another, at first sight very striking, case of reincarnation in India.⁸ However, he found that the crucial “facts” on which it all depended were not so solid after all. His conclusion: not a case of reincarnation but a “product of Indian social psychology and of the widespread belief in the possibility of remembering previous lives.”

A Pinch of Salt

It is remarkable that in his paper Barker so strongly emphasizes the similarity of Stevenson’s cases with the case he investigated himself. Can one conclude other than that Barker is warning us to take Stevenson’s work with a pinch of salt?

6 This tacitly alludes to the substantial critiques of Banerjee’s work that K. Ramakrishna Rao published in the *Journal of Parapsychology* (Rao, 1964a, 1964b). (Eds.)

7 See note 1. (Eds.)

8 See Pasricha & Barker (1981). (Eds.)

All this does not alter the fact that I think the Dutch version of *Twenty Cases* is one of the very few books in Dutch that everyone who is interested in parapsychology should read. Whether you believe in reincarnation or not, the book is a fascinating document.

PROF. CHARI EXPLAINS CRITICISM OF STEVENSON'S BOOK ON REINCARNATION

A few months ago, the Dutch publishing house, Teleboek, issued a Dutch translation of a sensational book, *Twenty Cases Suggestive of Reincarnation*, in which American psychiatrist, Dr. Ian Stevenson, attempts to scientifically prove the truth of the ancient belief in the migration of souls.

Stevenson has personally studied a significant number of cases where young children (from India, Brazil, and a number of other countries) started, apparently spontaneously, to talk of their experiences in a previous life.

Their stories about their previous life were often found to correspond to the actual events in the life of a deceased person in the most minute detail. The children were also able to recognize family members from their previous life and sometimes still knew their names. In some cases, they were even able to accurately indicate where their former family had hidden the family jewels. According to Stevenson, these cases are inexplicable unless we assume that these children are in fact reincarnations of the dead.

Revealing Criticism

I made a number of critical remarks in an article devoted to the book that appeared in this newspaper. One of the things I asked myself was whether Stevenson had always been completely thorough in his verification of the facts. I also referred to the memorable fact that Prof. Dr. C.T.K. Chari, India's most respected parapsychologist, had been fiercely critical of Stevenson's work. When an English translation of my review came into Prof. Chari's possession, he offered to write an article on behalf of this newspaper in order to explain his objections to Stevenson's book.⁹

With his permission, I have freely translated, edited and summarized his article. While I have done this as conscientiously as possible, I do feel it necessary to explicitly

⁹ The correspondence between Hoebens and Chari is preserved in the Hoebens Files. (Eds.)

state that Prof. Dr. Chari is solely responsible for the complete English version, of which a limited number of copies is available for those who are interested.¹⁰

STEVENSON RAISES MORE QUESTIONS THAN HE ANSWERS

By Prof. Dr. C.T.K. CHARI

This article is designed to follow on from the review in this newspaper that Piet Hein Hoebens wrote on the Dutch translation of Professor Stevenson's book, *Twenty Cases Suggestive of Reincarnation*. In his rather critical review, Mr. Hoebens pointed out that I, as a representative Indian parapsychologist, strongly dispute Stevenson's explanation of these suspected cases of reincarnation. I can confirm that I do indeed have serious objections to Stevenson's research.

Mr. Hoebens is correct when he writes that Stevenson, an experienced psychiatrist, does not at least fall prey to the irresponsible sensationalism that characterizes so many popular works on the reincarnation question. However, in my opinion, this in itself is not adequate justification of Stevenson's approach. I refer now in particular to his argumentation in the case of the Indian reincarnations. The problem here is that the Indian factual material was compiled and analyzed under American supervision, and that a book about a pre-eminently Indian theme has been written by outsiders to a previously defined plan.

Stevenson does not speak or understand any of the Indian languages or dialects, and in his fieldwork he was compelled to rely largely on what his English-speaking Indian employees, assistants and interpreters told him. Obviously, serious miscommunication can arise in such a situation. Other than my own articles, there is no independent and critical analysis of Stevenson's work by other members of the Asian scientific community.

Questions

Stevenson's approach to the theoretical aspects of reincarnation raises more questions than it answers. Based on his ever expanding mass of material, Stevenson claims that we

¹⁰ Prof. Chari's statement is reproduced here from his original English manuscript which is kept in the Hoebens Files. (Eds.)

are now in a position to try to demonstrate that reincarnation is a global phenomenon and that the cases he has handled cannot be dismissed as collective figments of the imagination of a given culture.

Stevenson argues that cases of suspected reincarnation have been reported by various different cultures, which are often totally isolated from each other. These cases are nevertheless very similar in nature. There is a kind of “archetype” that appears to be common all over the world. In Stevenson’s view, this is more than pure chance; there must be more behind this than mere socio-psychological causes.

Skepticism

I doubt Stevenson’s hypothesis for reasons which I have explained in specialized publications. For example: Neither Stevenson nor his employees have published information on representative cases of reincarnation from the four southern provinces in India. I myself have undertaken research in these regions, where the population believes in rebirth. I indeed came across a number of cases, but they were so drastically different from the type that Stevenson claims to occur all over the world that I was forced to consider a quite different, much less “paranormal” explanation than reincarnation!

Finally, I feel I must mention serious factual inconsistencies in Stevenson’s evidence. For example, I have thoroughly researched the case of Malikka, which is described in great detail in Stevenson’s book. I came to the conclusion that this is not a reliable case. Stevenson’s main witness, who was also his principal interpreter, refused to answer my questions. But Malikka’s father and her grandfather not only said that reincarnation was not involved here, they also denied the so-called facts on which Stevenson bases his arguments. A police report that came into my possession revealed even more, often essential, contradictions.

Clearly Stevenson has a right to interpret this case as he wishes. But it would have been more honest if, in the second edition of his book (on which the Dutch translation is based), he had at least mentioned the weaknesses of this particular case, which I had already pointed out in specialist publications at the time when the first edition appeared. He intentionally did not do so, however. In his published work, he continues to present the case of Malikka as a plausible, albeit less convincing, example of the reincarnation type. What this has to do with responsible parapsychological research escapes me.

I also have significant criticism of other cases that Stevenson treats in his book. Mr. Hoebens has already pointed out the weaknesses of the case made for the girl Swarnlata.

And Stevenson has yet to respond to my question of how it was possible that the girl Gnanatilleka from Sri Lanka could remember that a number of houses close to where she had lived in a previous life had been demolished and renovated. It is striking to note that the renovation took place after the death of her previous incarnation!

In yet another case, that of the girl Sukla, I know from the eye-witness accounts that have been sent to me, that the child was unable to recognize a likeness of her previous incarnation from a photograph. A striking example of “memory loss,” which Stevenson simply ignores.

I have no objection to Stevenson using reincarnation as a working hypothesis in order to explain his data. However, as a responsible parapsychologist, he should at least mention the contradictions that other, mainly Asian, researchers have uncovered and the objections that these colleagues have presented. This seems to be something he prefers not to do. I do not wish to appear unsporting. But is he only really interested in the opinions of Asians when they agree with him? If so, this would be a reprehensible characteristic in a serious scientist. Unless this changes, research into reincarnation is doomed to remain as controversial a subject in the future as it is today.

Editorial Postscript

The publication of Hoebens’ article and Chari’s commentary in De Telegraaf in the fall of 1981 marked the beginning of a pretty extensive three-way correspondence between Prof. Chari, Hoebens and Hövelmann. One of the issues under discussion was the fact that Chari frequently had made reference to documents he claimed to possess on quite a few of Stevenson’s reincarnation cases, but that he never actually had made those documents available to other interested researchers. Eventually, about two years later, towards the end of 1983, both Hoebens and Hövelmann received two identical sets of photocopies from Chari with what he suggested were the essential documents on the cases being discussed. Chari wanted Hoebens and Hövelmann to “see for [themselves].” Hoebens and Hövelmann agreed to write a joint article more or less independently of Chari, but using and evaluating the copied documents he had supplied. Due to Hoebens’ untimely death less than a year later this article in fact was never completed.

The only published account was a short casual reference to the Chari material Hövelmann made a couple of years later in the context of a different article (Hövelmann, 1985b, p. 673). It read in part: “[S]ome of these documents support Chari’s negative conclusions about a number of Stevenson’s reincarnation cases, while the case for the prosecution

appears not as strong as Chari claims [...] The documents available to me at this point are far from fatal to the whole of Stevenson's Indian case studies."

Again, almost a decade and a half after that article was published, philosopher Frank B. Dilley remembered having seen Hövelmann's note when he was reading the draft of a Master's Thesis on the status of reincarnation research his student John Hileman was preparing. He therefore encouraged Hileman to get in touch with Hövelmann for further details, which Hileman did. He asked Hövelmann for his view of the relevance of Chari's material. Hövelmann, in turn, based on the Chari documents and contemporary notes drawn up with Hoebens, supplied a rough two-page draft of the conclusions that they had arrived at after their initial study of the Chari documents.¹¹ This statement eventually was included full-length as an appendix to Hileman's Master's Thesis (Hileman, 2000, pp. 85-86).

The most relevant part may be quoted here: "I think that what I wrote in my brief note [of 1985 (Eds.)] that you are referring to still adequately represents my position. To be somewhat more specific, I personally consider the cultural artifacts hypothesis more plausible than its reincarnationist or survivalist competitors. The Chari material certainly is sufficient to document some flaws in some of Stevenson's Indian cases, and it shows cultural artifacts to be a more reasonable counter-explanation than Stevenson seems willing to admit. On the other hand, I can not see how the material might justify Chari's sometimes strong conclusions about the quality of the investigations and the evidence. I believe that Chari's documents do raise legitimate concerns about Stevenson's investigative methodology, reliance on interpreters, acquaintance with the foreign culture etc. [...] But I don't think that they are sufficient to definitely demolish even a single of Stevenson's cases. Therefore, I continue to believe that, on the whole, Stevenson's preferred hypothesis is a legitimate one even though I find it unconvincing." (Hövelmann in Hileman, 2000, pp. 85-86) (Eds.)

11 Gerd H. Hövelmann, letter to John Hileman, 14 April 2000.

CHAPTER 4-10

Editorial Introduction

The so-called “Mars Effect Affair” around the neo-astrological research findings by French psychologist and statistician Michel Gauquelin and the (at least temporary) scientific and public-relations mess that CSICOP had created with its re-investigations, has repeatedly been alluded to in previous chapters of this book (cf. chapter 4-04 for an example). Hoebens over several years had been in regular (and often voluminous) correspondence with all major (and most minor) actors in what easily could have turned into a dramatically scandalous affair, spreading like bushfire. In fact, it was Hoebens (even if not Hoebens alone) who eventually managed to have almost all participants talking to each other again and who got CSICOP to establish, at least temporarily, a new astrology sub-committee under the leadership of Canadian psychologist Ivan Kelly.

A few weeks after Michel Gauquelin (1928-1991) had learned that Hoebens had taken his life, he phoned one of the editors (G.H.H.) to communicate that, from his perspective, Hoebens very clearly had been instrumental in re-establishing, by 1984, civilized communication between most of those involved (sadly, Dennis Rawlins, George Abell and Richard Kammann, three of the major actors, didn't live to see that outcome) – and Gauquelin requested that he be quoted accordingly should an opportunity ever arise.

Much as Hoebens had been active behind the scenes of the “Mars Effect Controversy,” he rarely, and comparatively briefly (see his “Comments [on Curry's paper]”, Zetetic Scholar, No. 9, 1982, pp. 70-71), contributed to the printed exchanges in order to protect his role as a mediator who, although he never was given to mincing words, was respected and trusted by all parties involved. His major (and final) public comment in English that is reprinted below was published in Zetetic Scholar, No. 11, 1983, 25-28.

The Hoebens Files contain a huge section of mostly unpublished background material on the “Mars Effect Affair.” This was made available by Hövelmann, in respectively the late 1980s and the 1990s, to Prof. Suitbert Ertel for his own research on Gauquelin's “Mars Effect”¹ as well as to Jim Lippard for his specialized website on the history of the affair (see note 13, below). (Eds.)

1 See, for example, Ertel (1986, 1988a, 1988b, 1991, 1993a, 1993b, 2000) and Ertel & Irving (1996).

SOME FURTHER REFLECTIONS ON THE MARS EFFECT AFFAIR

In spite of several appeals for a truce, the controversy over the so-called Mars Effect shows few signs of abating. The five part Mars Effect section in *Zetetic Scholar* No. 10 has raised a number of important questions. The editor² has specifically invited persons associated with CSICOP to share their views with the readers of this journal. These comments are strictly *à titre personnel*.

Ad McConnell & Clark

Although I respect Professor McConnell and continue to believe that his intentions were honorable, I strongly object to the manner he has chosen to intervene in the controversy.³ His September 1981 letter “to all public supporters” of CSICOP can only be seen as a regrettable lapse. Apparently it has not occurred to him that his inquisitorial approach could have led to an “experimenter effect” largely invalidating his conclusions. McConnell believes that the table printed in *Zetetic Scholar* No. 10 reflects CSICOP reactions to sTARBABY.⁴ For all I know the table may just reflect CSICOP reactions to abusive letters. The December 1981 follow-up only added insult to injury.

To make matters even worse, McConnell may have helped to prevent a satisfactory solution to the problem. The controversy over M. Gauquelin’s findings is an extremely complex affair. Many of the “public supporters” in 1981 did not have the remotest idea what all the fuss was about. When the first rumors of a “scandal” reached them, they had to decide – necessarily on the basis of an intuitive assessment – whether a detailed examination of the claims, counter-claims, counter-counter-claims, etcetera, would be worth their trouble.

I cannot really blame those who, after having pursued McConnell’s *J’Accuse*, concluded that the matter was not sufficiently serious to warrant their attention. The valid points which McConnell undoubtedly had made were completely obscured by his intemperate rhetoric. At my newspaper we stick to a tacit rule: Letters purporting to reveal the

2 Prof. Dr. Marcello Truzzi. (Eds.)

3 This section is a commentary on McConnell & Clark (1982). In a letter to the Fellows and Consultants of CSICOP, Prof. McConnell had charged the organization with being “intellectually dishonest” and he had in fact asked them to withdraw their support for CSICOP. (Eds.)

4 See Rawlins (1981). (Eds.)

“worst scandal in history” (we receive about ten every day) have a 10,000 to one chance of belonging in the crank mail category.

Ad “The True Disbelievers”

De gustibus non est disputandum. I was puzzled when I learned that some of my friends in CSICOP find the style of Professor Kammann’s paper⁵ objectionable, inflammatory and undignified. I beg leave to express my dissent. I regard “The True Disbelievers” as an eminently fair, highly readable and – given the circumstances – remarkably restrained statement from a distinguished skeptic who has gone to almost incredible lengths in his attempts to help CSICOP free itself from its Martian predicament. It is true that Kammann’s verdict is hardly flattering to several prominent members of the committee, but that verdict was reached after an extensive and scrupulous examination of the evidence.

I do not think that Kammann has been excessively censorious. To the contrary: He has made a great effort to make the facts fit his “innocent mistakes” scenario. It is not his fault that the facts refused to co-operate. Even so, Kammann does not indulge in cheap moralizing at the expense of Professors Kurtz, Abell and Zelen. Rather, he portrays them as the victims of their inability to detect the pitfalls of rationalist irrationality.

Ad CSICOP

To some it may appear somewhat incongruous that the above paragraph was written by someone who, despite Professor McConnell’s exhortations, remains a “public supporter” of CSICOP.

I confess to having mixed feelings about the Committee. I agree with Kammann, McConnell, Curry⁶, Rawlins, Eysenck⁷ and Truzzi⁸ that CSICOP has made quite a mess of its dealings with that remarkable and courageous scientist, Michel Gauquelin. However, I do NOT think that CSICOP is beyond redemption; I do NOT think that the Mars Effect debacle was “the biggest scandal in the history of rationalism,” and I do NOT think that this affair is symptomatic of everything that is going on inside the Committee.

5 This is Hoebens’ commentary on Kammann (1982), a sharp insider’s critique of how CSICOP had handled the home-made “Mars Effect” affair. (Eds.)

6 Curry (1982a, 1982b). (Eds.)

7 Eysenck (1982). (Eds.)

8 Truzzi (1982b, 1982c). (Eds.)

According to some of its more outspoken detractors (and here, I am *not* referring to Truzzi and Kammann, who some supporters of CSICOP have falsely cast in the role of “enemies”), CSICOP has cynically and systematically disregarded the lofty principles proclaimed on the back-side cover of each issue of *The Skeptical Inquirer*. Having had access to many of the background documents, I have gained a somewhat different impression.

The more disturbing instances of skeptical misbehavior have been adequately exposed and analyzed. We should not ignore, however, those instances where CSICOP behaved far more creditably than participants in other scientific disputes have often done in comparable circumstances.

What first comes to mind is the comparatively respectful manner the Committee has treated the principal victim. In all fairness it cannot be maintained that Profs. Abell, Kurtz and Zelen have been guilty of a systematic campaign to discredit and vilify Gauquelin. Gauquelin was given the opportunity to argue his case in the skeptical periodicals, and the replies, while often unsatisfactory or even misleading, have been generally courteous. CSICOP and *The Skeptical Inquirer* have been fairly consistent in presenting Gauquelin’s work as sufficiently challenging to warrant serious investigation.

My second point concerns the way CSICOP has responded to internal and external criticisms. In general, this response has been tragically inadequate. Having been a direct witness to one of the crucial incidents in sTARBABY, I am less than satisfied with the Committee’s version of the events that led to Dennis Rawlins’ excommunication. Even so, the facts do not really fit the “worst scandal” theory, according to which the CSICOP leadership, in a determined attempt to cover up the unwelcome truth, engaged in a ruthless campaign to suppress internal dissent. I mention my own experiences only as an example. Since the autumn of 1981, I have repeatedly, both privately and publicly, expressed my misgivings about the way the Committee has handled the affair. The CSICOP leadership was well aware of my friendly contacts with both Rawlins and Gauquelin. Apart from some extremely odd communications from a well-known skeptic whom charity forbids me to name here,⁹ the response to my insubordinate queries has been remarkably courteous and rational. There was no noticeable pressure on me to conform to any party line, not even after I had made plain that I continued to find Dennis Rawlins’ criticisms more convincing than the purported refutations. My dissent was treated as entirely legitimate. Those who have read, for example, the correspondence between a one time chairman of the German Society Against Superstition and the

9 After more than three decades we feel free to disclose, less charitably, that this remark refers to the letters Hoebens received from CSICOP member and prominent UFO critic Philip J. Klass. These remarkable encyclicals are kept in the Hoebens Files. (Eds.)

eminent skeptic Carl Count von Klinckowstroem (who committed high treason by accepting some claims of dowsing) will perhaps understand why my verdict on CSICOP is comparatively mild.

Finally, there are the measures CSICOP has taken publicly to correct at least some of the past mistakes. Kendrick Frazier's decision to publish Rawlins' merciless "Remus Extremus" in *The Skeptical Inquirer*¹⁰ was an act of courage. As for the "Re-appraisal" Profs. Abell, Kurtz and Zelen¹¹ have published in the Spring 1983 issue of the journal: The least that can be said of this remarkable document is that it demonstrates that the CSICOP leadership is not entirely unresponsive to criticisms. Too little and too late? Maybe – but much more than many of us would have expected.

My generally skeptical view of human nature does not permit me to see the Mars Effect affair as merely a series of innocent errors. It is rather obvious, I should say, that at several points considerations of political expedience have prevailed over the demands of intellectual integrity. This is usually the case where a group of fallible human beings becomes involved in a protracted controversy.

Some critics have insisted that the Mars Effect fiasco is symptomatic of the way CSICOP deals with the anomalous claims it professes to "examine objectively and carefully" and that it has showed the Committee for what it is: a pseudo-rationalist pressure group, obsessed with discrediting – if needs be by hook and by crook – any scientific finding that offends orthodox sensibilities. While I agree that the Committee frequently fails to practice what it preaches (the "clear and present danger" Professor Truzzi saw in 1976 is no less clear and present today), I am not a little suspicious of the motives of some of its most vehement enemies. Compared to some of the published attacks on the Committee which I have seen, even Mr. Klass' CRYBABY¹² seems a model of dispassionate scholarship.

10 Rawlins (1981-1982). (Eds.)

11 Abell, Kurtz, & Zelen (1983). This article was still in print at the time Hoebens wrote his comment. (Eds.)

12 CRYBABY is Phil Klass' (Klass, 1981) unpublished, but privately distributed response to Rawlins' sTARBABY (Rawlins, 1981). To fully appreciate Hoebens' remark in the text, consider, for instance, the verdict on CRYBABY published by Richard Kammann, another of Klass' nominal allies: "Although it offered to refute the cover-up charge [made by Rawlins], it ignored practically every specific point that Rawlins had made. Instead it offered a blatant *ad hominem* attack on Rawlins' motives and personality, bolstered by rhetorical ploys – including crude misquotation." (Kammann, 1982, pp. 62-63). (Eds.)

A Modest Proposal

I wish to conclude with a somewhat quixotic suggestion. The Mars Effect affair¹³ has raised questions about CSICOP's credibility. The Committee, on its part, has protested its bona fides – and has publicly corrected at least some of the major mistakes. Doubts about CSICOP's ulterior intentions, however, will linger on. In my view, the most felicitous thing CSICOP could do to clear its name once and for all would be to become re-involved in the scientific debate over the claimed planetary effects and to propose to Michel Gauquelin (who has taken an admirably sober view of the entire business), Richard Kammann, Dennis Rawlins and others that they all join forces in a new test of cosmobiology. I suggest that, instead of the Mars Effect for sports champions, a different effect be chosen this time. I think it would be worth the trouble. CSICOP would have a chance to prove that the Mars fiasco has indeed been an isolated lapse. The advantages for Gauquelin are obvious. Finally, all of us would profit, for such a test would bring us closer to the answer to the only question that really matters: Do planetary effects exist, and, if so, how can they be explained? After all, this is what the controversy was about in the first place.

13 A very detailed chronology of this intricate Mars Effect affair, including a wealth of information from unpublished sources, will be found on Jim Lippard's homepage, <http://www.discord.org/~lippard/mars-effect-chron.rtf>. An abridged version of the chronology, covering the time until ca. 1995, is appended to Ertel & Irving (1996). (Eds.)

CHAPTER 4-11

Editorial Introduction

Unsuspecting readers may be surprised to learn that, at least since the 1950s, research into the supposed linguistic abilities of various species of animals used to be, and apparently still is, a widely accepted and appreciated part of mainstream science, whatever criterion one may decide to apply. It has profited from very substantial research grants from recognized institutions (such as the National Institute of Health [NIH] in the United States), and the research was published in periodicals as prestigious as Science and in many of the leading journals in psychology and a variety of related disciplines. Also, it appears as if current neuro-physiological and neuro-philosophical discussions largely seem to accept those “animal language” findings for their own recent theory building.¹ Apparently, all the many critical studies on “animal language research”,² that have demonstrated often fatal problems with the experimental designs and the methods of data collection and interpretation in this specialist area, have been in vain. At least they obviously have been insufficient to prevent current wide-spread scientific recognition of that research.

In the article to follow Hoebens had concentrated on the lighter, more entertaining and hilarious – but nevertheless characteristic – side of this issue (although he briefly refers to Patterson’s work with gorillas). The article was published in the August 6, 1983, issue (p. T25) of the Dutch daily newspaper De Telegraaf. It was stimulated by Hoebens’ (then) recent visit to Marburg where he had inspected Hövelmann’s collection of historical material on animal language studies, and by a TASS press-agency release that he had seen around the same time. The latter, again, had motivated the well-known semiotician, linguist and anthropologist Thomas A. Sebeok to devote a little poem to an allegedly “speaking Russian elephant” in the pages of the Skeptical Inquirer. Hoebens’ article was translated by the editors and is published in English for the first time.

The article also had stimulated the private exchange of spontaneously produced little poems on “talking animals” between Hoebens and Prof. Sebeok. At least one of them later was to appear in print (see our “Editorial Postscript”). (Eds.)

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- 1 For instance, see Perler & Wild (2005); for a critical commentary, see Hövelmann (2007b, pp. 189-190).
 - 2 Cf., for example, Sebeok (1979a, 1979b), Sebeok & Rosenthal (1981), Umiker-Sebeok & Sebeok (1980, 1981), Seidenberg & Petitto (1979), Terrace et al. (1980), Hövelmann (1984c, 1989a, 1989b).

SPEAKING ANIMALS KEEP SCHOLARS TALKING ...

RUSSIAN ELEPHANT INTERVIEWED AND A DOG WRITES LETTERS ...

A week or two ago the world rejoiced in a report that the Union of Socialist Soviet Republics now has at its disposal a *talking elephant*. The elephant, Batir, is kept captive in the zoo of Karaganda, in the republic of Kazakhstan. The state-owned press-agency TASS announced that the animal is able to produce complete sentences like: "Batir is nice" and "Do you have water for an elephant?"

The talkative pachyderm seems to have permitted an interview for a local radio-station, but when some Western journalists asked for the tape recording it was, alas, unavailable.

The Anglo-Saxon press published the unavoidable cartoons with dissident Soviet elephants playing the leading role.

Since then an American dog which was sentenced to death has driven the Russian elephant from the front pages.

A talking elephant! Is it possible? Are there any animals which can express themselves by means of *language* (not like parrots imitating words, without a clue about the meaning)?

Rhymes

A few years ago the old controversy about "thinking animals" flared up again after reports on gorillas and chimps which showed remarkable indications of the intelligent use of language. Francine Patterson, chairperson of California's Gorilla Society, even claimed her ape Koko had thought out rhymes all by himself.³

In 1980 an entire congress in New York was devoted to the phenomenon.⁴ The prophets of animal language were no match for skeptical scientists such as Thomas A. Sebeok, who argued that Koko's behavior did not differ in kind from that of the average dog which has learned to sit up and give a paw: it is no more than a question of training.

Batir and Koko have a legion of precedents. This article deals with some of them.

3 See Patterson (1978a, 1978b, 1981). (Eds.)

4 Sebeok & Rosenthal (1981). (Eds.)

In 1904 a Russian stallion, Hans, appeared on stage in Berlin. The animal was owned by the somewhat eccentric East-Prussian mathematics teacher Wilhelm von Osten. The horse was able to do calculations like addition, subtraction, powers and roots. He answered questions, read maps, remembered names and recognized photographs. The horse could not really speak, because horses do not have a speech organ. The horse “counted” by stamping on the floor and spelled words by pointing at letters on an alphabet-board with his hoof. “Der Kluge Hans” (Clever Hans) became a sensation. The international press treated him as a celebrity. Images of the animal appeared on matchboxes and beer-mats. Eminent scientists came to watch Hans’ performances and concluded that the horse was really able to think.

Signals

In 1904 the Prussian Ministry of Science and Arts set up a committee to subject the phenomenon to thorough investigation. One of the members of this committee was one *Herr cand. phil. et med. Oskar Pfungst*, a psychologist whose first love was the experimental method. Pfungst uncovered the secret of Clever Hans.⁵ A long series of ingeniously designed experiments led him to conclude the horse did no more than react to small signals, like movements the spectators made with their heads, normally not noticed by uninvolved bystanders.

Without realizing it, the spectators gave Hans signals as soon as he had stamped his hoof the required number of times for the outcome of a calculation or when his hoof reached the appropriate letter on the alphabet-board. For the horse it was rather important to perform well, because correct answers were rewarded with a carrot or a glass of beer, and Hans was very fond of both.

Pfungst had found a reasonable explanation for the phenomenon but at the same time he set the fat in the fire. Von Osten and his friends refused to accept the negative conclusions of Pfungst’s investigations. They joined forces, declared war on the academic establishment and started one of the most comical movements in the already rich history of the alternative sciences.

After Von Osten’s death the jeweler Karl Krall, who lived in Elberfeld, became intellectual leader of the new “animal psychology” movement. He inherited Clever Hans and carried out a large number of experiments with the famous animal. In his own stables he

5 Pfungst (1907/1911). (Eds.)

discovered several other talking horses, among which was the totally blind Berto,⁶ who performed even better than Hans.

Krall's conclusions were gathered together in a book of some 500 pages, entitled "Denkende Tiere" [Thinking Animals] published in 1912 in Leipsic.⁷ The book is a monument of unwitting humor.

The naive Krall was very proud that his horse Muhamed regularly mistook him for the German emperor. Krall was a tolerant teacher. When the horse should have spelled the word "grün" [green] Krall also accepted *düün*, *rgkünen*, *urnk*, *gürl*, *ürhnk* and the like. When in 1911 the horse Muhamed was asked to answer "what is white and good to eat?" he did no more than put his hoof on the "z." Krall considered this yet another staggering example of animal intelligence. It was perfectly clear, he wrote, that the "z" must be interpreted as an abbreviation of "Zucker" [sugar]. In 1914 a psychologist, von Máday, living in Prague, published a book in Leipsic (naturally also of 500 pages) in which he demolished Krall with Teutonic thoroughness.⁸

Furore

Nonetheless the "new animal psychology" remained in fashion. Respected academics and other intellectuals (for example the great poet Maeterlinck⁹) were converted to belief in the Elberfeld horses. In 1914 even a scientific journal was launched, entitled *Mitteilungen der Gesellschaft für Tierpsychologie* [Communications from the Society for Animal Psychology], edited by Professor Dr. H. E. Ziegler from Stuttgart.¹⁰ This journal, which evokes irrepressible laughter, was largely dedicated to praising Krall and his menagerie to the heavens. A second purpose, of about equal importance, was fulminating against doubting Thomasses.

6 Krall (1913). (Eds.)

7 Krall (1912). (Eds.)

8 Máday (1914). (Eds.)

9 Maeterlinck (1914). Maurice Maeterlinck was awarded the Nobel Prize in literature in 1911. For a critical (and in fact sharply satirical) assessment of Maeterlinck's views on "talking animals," see Sebeok (1982). (Eds.)

10 In addition to the *Mitteilungen der Gesellschaft für Tierpsychologie*, which was published, with some interruptions, between 1913 and 1933, there was a second, much shorter-lived periodical, *Tierseele* [Animal Mind], that only appeared for little more than a year (1913-1914) and was supported by essentially the same group of authors, most of them with impeccable academic credentials. (For details see Hövelmann, 1984c, 1989a, 1989b.) (Eds.)

Copies of the *Mitteilungen* are now very rare. (I am grateful to the Marburg philosopher Gerd H. Hövelmann for allowing me to consult his collection). What makes them absolutely unique is the history of the *thinking dog of Mannheim*. Rolf, the faithful four-legged friend of Frau Moeckel, was not just a thinker and talker, but also a diligent writer of letters.¹¹ Selections from Rolf's correspondence (with people as well as with other dogs) were published in Professor Ziegler's journal.

Rolf never held a pen "in his fingers." The dog dictated its letters by tapping a kind of simple Morse-code on the floor, using its right foreleg. The results were astonishing.

Some examples: In a letter to Rolf, Professor Gustav Wolf¹² had written: "Dear Lol (Rolf's nickname), I often think of you. How beautifully you performed and in such a friendly way. I told our dog, Pick, about it, who can't yet speak or read. I also have a dachshund, who is unfortunately ill at the moment. Yours sincerely. Professor Wolf." The dog answered the professor as follows: "Nice! Pick go to mother (Frau Moeckel). Study. Dachshund also come. Call doctor. Kiss. Lol." Karl Krall gave the dog of Mannheim a book for Christmas. Rolf answered that he had read the book with much pleasure. "Animals like studying. Christmas-child comes. Lots of kisses. Lol." In a letter to another dog, Rolf confessed, however, that he did not like studying very much at all. "Study is tiring. Headache. Prefer not study. Always say: tired. Then mother says: no. Then say: pain in stomach," and so on.

Rolf received many letters from scientists. A Dr. Gradenwitz invited him to Berlin for discussions. Rolf answered in a polite letter, reporting that he caught a cold because he had been swimming in the Rhine. Rolf's comment on the First World War: "Lol likes fighting. Mother forbids. But French fight Germans. Mother should forbid. It's quite indecent."¹³

It is unbelievable that the idea that they might be victims of a mystification or a hoax never crossed the minds of Professor Ziegler, Professor Wolf or Dr. Gradenwitz. Their belief in Rolf's epistolary abilities was unshakable. Try to imagine a university professor who in full German seriousness writes a letter to a dog, asks the dog's opinion about current politics and asks the dog to convey greetings to the wife and children.

11 E.g., see Mackenzie (1919). (Eds.)

12 The correct spelling of the professor's name is „Wolff“; e.g., see Wolff (1914). (Eds.)

13 All quotations are from various issues of the *Mitteilungen* (see note 8). (Eds.)

Paranormal

Something specially for humorless Prussians? No, the naive belief in thinking animals is not restricted to a specific nationality. Until his death, the famous American parapsychologist Dr. J. B. Rhine believed in the marvelous abilities of the mare, Lady, of Richmond, Virginia.¹⁴ This animal not only understood English and Chinese, but was paranormally gifted as well. In 1952 the newspapers reported that the horse had, clairvoyantly, solved a police case of a missing person. And Rhine's prominent colleague, Dr. J.G. Pratt, was firmly convinced of the telepathic talents of Chris, a dog from Rhode Island who was able to talk the hind legs off a donkey.

Around 1840 Antonio Blitz created a sensation. In the streets of London he conversed with coach-horses. On one occasion he attracted a massive crowd in a market place, because he was talking to a steamed kipper and succeeded in eliciting an answer from the corpse. In 1840 the technique of ventriloquism was still a well kept secret. Now no longer. Even attendants in Kazakhstan zoos can learn to use it.

Editorial Postscript

In the summer of 1983, Associated Press, based on a TASS press release, had reported: "Batir, a baby elephant at the Karaganda Zoo, talks – and zoologists have tape recordings to prove it, the official press Agency Tass said yesterday [...]. He puts his trunk in his mouth and you can hear what sounds like a low male voice."

Dr. Thomas A. Sebeok (1920-2001), "Distinguished Professor of Linguistics and Semiotics" at Indiana University, a prominent critic of "animal language" studies and a leading authority on animal communication,¹⁵ had been stimulated by this press release to submit a little poem on the supposedly talking elephant to the Skeptical Inquirer (vol. 8, 1983-1984, p. 110). Hoebens had taken him up on it and, in private correspondence,¹⁶ had ventured to reply to Sebeok with a poem of his own, sent in the name of "Batir," the elephant. Other correspondence was to follow, some of it in rhymes again.

Since one of the editors (G.H.H.) and Sebeok were working on a couple of joint projects in the mid-1980s, Sebeok immediately granted permission to use his poem and correspondence

14 See Rhine & Rhine (1929a, 1929b). (Eds.)

15 See Sebeok (1968, 1977, 1979a), Sebeok & Ramsey (1969). (Eds.)

16 The full correspondence between Hoebens and Prof. Sebeok is preserved in the Hoebens Files.

with Hoebens when plans for the production of the present book first were discussed. We therefore are happy to present here the late Prof. Sebeok's poem with "Batir's" [Hoebens'] indignant response immediately following. The latter eventually also found its way into print, disguised as a Letter to the Editor of the Skeptical Inquirer, "Batir responds," with a short introduction also written by Hoebens (Skeptical Inquirer, vol. 8, 1984, p. 300). (Eds.)

Batir, the "talking" elephant

*From the zoo in Karaganda,
Trumpets Soviet propaganda:
Sauce for goose is sauce for gander!*

*The Kazakhstan ungulata,
Just like Lana in Atlanta,
Talk while taking their siesta.*

*In the East-West gabfest derby
Batir (orphaned as a baby)
Tapes recordings – oh well, maybe.*

[Thomas A. Sebeok]

Batir Responds

The following message, apparently in response to Thomas Sebeok's contribution in our Winter 1983-84 issue, made its way to the Skeptical Inquirer through clandestine channels with the aid of Piet Hoebens.

*PATRIOTIC SOVIET ELEPHANT
RESPONDS TO U.S. IMPERIALIST SLANDER*

*Dear Professor Sebeok!
Reading your verse gave me quite a shock
Obviously you view as bunk
The wonders coming from my trunk
(Implying that my gift of speech
Is a trick by N. Ivanovich).
Your reference to "propaganda"
Is shameless anti-Soviet slander.*

*A cheap insult – you'll understand –
To that Glorious Fatherland
To which belong the Khazak moors*

Pachydermatously yours,

BATIR

*(Member of the All-Soviet Union
of Socialist Mammals)*

[Piet Hein Hoebens]

CHAPTER 4-12

Editorial Introduction

The concluding five chapters (4-12 through 4-16) of this book, all of them comparatively short, are devoted to some outstanding book reviews that Hoebens wrote between 1980 and 1984. Given the explosion of principally accessible scientific knowledge and the transdisciplinary character of parapsychology and various related fields in what now often is referred to as “anomalistics,” a convincing case can be made for the increasing relevance of in-depth book reviewing in the interest of individual and reciprocal information and education.¹ The book reviews that we have chosen for this section in our view are exemplary in that they provide both detailed representations of the contents of the respective books and the necessary contextual information that allows readers to judge these books’ relevance for (then) current and ongoing discussions, thus opening up perspectives for the readers’ own pursuit of the topics under discussion.

*This book review section starts with Hoebens’ review of a German book on the relationship between parapsychology and what the book’s author has termed the “science of deception.” The review originally appeared in *Zetetic Scholar*, No. 7, 1980, pp. 150-153. (Eds.)*

BOOK REVIEW: PARA, PSI UND PSEUDO

Para, Psi und Pseudo: Parapsychologie und die Wissenschaft von der Täuschung. By Lutz Müller. Berlin, Frankfurt, Vienna: Ullstein, 1980. 256 pages, illustr.

Although the “natural antagonism” between the two groups has often been exaggerated, the relationship between the parapsychologists and the magicians has traditionally been an uneasy one. This is not surprising. Psi phenomena and magic tricks have the somewhat embarrassing habit of looking like Tweedledee and Tweedledum and disputes over “who is competent to judge what” are bound to arise. Some parapsychologists resent what they perceive as the meddlesomeness of psi-investigating illusionists, whereas some magicians suspect behind every parapsychologist a credulous dupe who, rather than listen to the experts, invokes quantum physics to explain why handkerchiefs can turn

1 For detailed explications, see Hövelmann (2005b, 2007a).

into pigeons.² Researchers who may claim to be insiders in both groups are relatively rare.

Dr. Lutz Müller from Stuttgart, Germany, is one of those exceptions. A professional psychologist who has long been associated with Bender's psi institute at Freiburg University (he received his Ph.D. on a parapsychological subject), Müller is also an experienced magician, a member of the German Magic Circle and a frequent contributor to magic journals.

Para, Psi und Pseudo (the title behooves no translation) is a revised and enlarged version of his 1977 doctoral thesis, devoted to "parapsychology and the science of deception." As the language barrier will prevent many readers of this journal from personally acquainting themselves with this excellent book³ I will attempt a rough summary of its contents.

Müller regards the history of psychical research "as a history of the confrontation with trickery and fraud." From this assumption it logically follows that parapsychologists must seek close cooperation with experienced magicians. Such an interdisciplinary approach is the only way to avoid repeating the embarrassing mistakes of the (often very recent) past.

After an amusing survey of the most notorious cases, Müller severely criticizes the naive belief of psychical researchers like the German Gerda Walther⁴ that genuine paranormal phenomena can be recognized by their self-validating genuineness. The perfect deception, he asserts, is indistinguishable from the real article, as the subtle introduction of psychological elements that cause the observer to declare the phenomenon authentic is part of the trick. The only criteria for genuineness are "those criteria that exclude cheating." It is obvious, according to Müller, that only the real experts on trickery – the magicians – can with any degree of confidence decide whether these criteria have been met in a given experiment. Müller proposes a standard "deception analysis" as an essential preparation for experiments with supposedly gifted subjects. In this analysis, various aspects of the procedure must be inspected for the "degree of freedom" they allow the subject. For example, if a subject insists on being tested in familiar surroundings, refuses to be searched for gimmicks and requires total darkness, the "degree of freedom" must be judged "high" on those three points, and *a priori* suspicions of fraudulent intentions must be commensurate – regardless of how "reliable" the fellow looks. The main part of the

2 For a systematic sociological survey of the history of this often uneasy relationship and a plea for fruitful cooperation, see Truzzi (1997); also see Lachapelle (2008). (Eds.)

3 Much as this book would have deserved an English or American edition, it has remained untranslated. (Eds.)

4 See Walther (1950-1951). (Eds.)

analysis is to decide in what way the special conditions prevailing in a given experiment could be taken advantage of by a trickster.

The average scientist, Müller sadly notes, is singularly ill-equipped to perform such an analysis without the assistance of an experienced magician. Obviously, cooperation is called for. Yet trick-experts have relatively seldom been consulted in parapsychological experiments. Strong prejudices on both sides often prevent a fruitful contact. Many parapsychologists have misgivings about the “inhibiting influence” that presumably would result from the presence of a magician. On the other hand, many magicians have *a priori* reservations of a different kind. Müller cites the embarrassing instance of a French illusionists’ congress in November 1976 where magicians who testified to their having observed apparently paranormal metal bending were all but shouted down by angry colleagues. However, Müller sees some signs of a growing ecumenism. The Freiburg Institute has invited magicians to serve as consultants. Utrecht parapsychologist Martin Johnson has urged his colleagues to cooperate with the Magic Circles (and in 1976 the PA conference in Utrecht organized a magic demonstration with the embarrassing result that quite a number of leading parapsychologists started suspecting the magician of real paranormal powers).

A poll taken by Müller among the members of the German magic community suggests the trick experts are by no means as hostile as is often assumed. Although 81.6% thought ALL Geller’s feats were tricks, 72.3% said they thought psi was probably a real phenomenon. The respondents, however, were self-selected and only 283 of the 1,000 invited sent back their questionnaires, so Müller presents his findings with utmost caution. The questionnaire method, moreover, did not allow him to look for possible correlations between degree of belief and degree of competence, which could have yielded interesting results.

In a short, but important chapter on “Understanding Cheating,” Müller places the issue in a wider context. One of the most fundamental and pernicious deceptions, he states, is the idea that deception is a marginal phenomenon, an isolated disruption in an otherwise “objective” view of reality. He insists (as Hansel did) that cheating (including self-deception) is a vital ingredient of human psychology, a natural result of the way the mind perceives its environment. Müller regrets that this important aspect of human existence has not yet become the subject of a special “Science of Deception,” although the magical brotherhood has long been practicing an underground version of such a science.

Psychical researchers have often become the victims of hoaxers and tricksters, Müller states, not only because they knew too little about the technical possibilities of trickery,

but even more because they greatly over-estimated their own competence as observers, and underestimated their unconscious willingness to be led astray.

From the arguments Müller advances and from the fascinating examples he has chosen, the determined hard-line skeptic could construct a strong attorney's case against the pretensions of parapsychology in general. If so little is known about the psychology of deception, then how can we hope realistically to estimate the importance of the fraud factor in psychological research? If even Podmore could be tricked, then why should we put any trust in the judgment of persons less perspicacious than Podmore? If a number of Houdini's tricks are still unexplained, then how can we ever attempt to deduce the authenticity of "paranormal" events from their inexplicability?

Müller, however, is no hard-line skeptic. From the perspective of the extreme critic, he would even qualify as a "believer." In the introduction to his book he states his view "that, by and large, the existence of psi phenomena can be regarded as having been scientifically demonstrated." While discussing pseudo-psi he often seems to take the existence of the genuinely paranormal for granted, even where his own arguments would seem to throw doubt on even the best evidence.

It is my feeling that Lutz Müller has not been entirely successful in solving his own identity problem as an observer of the paranormal scene. The book at times reads like an uneasy compromise between the parapsychologist Jekyll and the trick-expert Hyde. Jekyll approvingly quotes Bender's attacks on the pig-headed skeptics, while Hyde almost out-Hansels Hansel in asserting the universality of cheating.

This ambiguity is probably responsible for the (very few) disappointments I experienced while reading the book. I had expected to find an extensive discussion of the poltergeist, which after all is one of the specialties of the Freiburg school. With his unique background as a magician and a former associate of Bender's institute, Müller would have been in an unrivaled position to throw some light on still controversial cases like the Rosenheim and Bremen RSPK outbreaks. Poltergeists, however, are not discussed at all.

Likewise, Müller is extremely reticent in reporting his own experiences during his stay in Freiburg. The reader is not informed about the experiments the author must have observed himself. Müller mentions the very interesting fact that Geissler-Werry, one of Germany's top magicians, has been a consulting trick-expert at the Freiburg Institute. The reader would wish to know more about Geissler-Werry's experiences there. Did he ever watch Bender's star spoon bender Silvio in action, and, if so, what did he think of it? The question remains unanswered in *Para, Psi und Pseudo*. (In a personal communication,⁵

5 The correspondence still exists in the Hoebens Files. (Eds.)

Dr. Müller notes that Geissler-Werry did attend at least one experiment with Silvio. No metal bending took place.)

Müller's reluctance, critically to discuss that part of the parapsychological world to which he himself has belonged for years, however, should be seen within the context of the pro- and con-debate in Germany. That country used to have an admirable tradition of responsible skepticism vis-à-vis the paranormal, exemplified by Dessoir, von Klinckowstroem and, to a certain extent, Gubisch. In recent years, however, skepticism in Germany has become increasingly identified with a small number of excessively hostile "disbelievers" – notably Otto Prokop, professor of forensic medicine in Berlin, and the criminologists Herbert Schäfer and Wolf Wimmer. These critics work from the unquestioned assumption that the Laws of Nature are sacred and that parapsychology is nothing but medieval superstition dressed up as "science." The presence of a psi institute at Freiburg University they see as one of the gravest threats to western civilization. Indeed, one of them (Wimmer, 1980), has managed to imply that Bender and his associates must share responsibility – in retrospect – for the witch hunts in the Middle Ages! According to Wimmer, "fairness has its limits" when dealing with aberrations like psychical research. The fairness of these three skeptics certainly has its limits. English-speaking parapsychologists who complain about the English-speaking skeptics should read Wimmer and Prokop's book *Der moderne Okkultismus* (Prokop & Wimmer, 1976). Then they will agree that, by comparison, James Randi is positively "sheepish."

Some passages in *Para, Psi und Pseudo* to me suggest that Müller may deliberately have wanted to avoid providing Prokop, Wimmer and Schäfer with fresh ammunition for their almost hysterical campaign against parapsychology. If my hunch is correct, I would question the wisdom of Müller's protective attitude. Parapsychology is best served by total frankness about its weak points. (Incidentally, this seems to be the attitude adopted by the Freiburg Institute in recent years. Its journal, now edited by Eberhard Bauer, has increasingly become a forum for "believers" and "skeptics" alike. Critical re-evaluation of psi claims is actively encouraged.)

Para, Psi and Pseudo is an excellent book, but it might have been even better if the author had been a tiny bit less discreet.

CHAPTER 4-13

Editorial Introduction

Magician James “The Amazing” Randi has been viewed by some (and maybe by himself) as the nemesis of parapsychology. Hoebens in the following review discusses the merits (and sometimes the lack thereof) of the first edition of Randi’s book *Flim-Flam!* (the book was later reprinted by Prometheus Books in 1982). The review (including its “Postscript [May 25, 1981]”) was published in German in the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, vol. 23, 1981, pp. 246-251, but was originally written in English (German translation by Eberhard Bauer). The following reprint is based on Hoebens’ English manuscript. Hoebens privately shared the manuscript with his fellow-skeptic Randi whose response was generally (and maybe surprisingly, given Hoebens’ many reservations) quite positive.¹ (Eds.)

BOOK REVIEW: FLIM-FLAM!

Flim-Flam! The Truth About Unicorns, Parapsychology and Other Delusions. By James Randi; with an introduction by Isaac Asimov. New York: Lippincott & Crowell, 1980, 340 + XII pp., illustrated.

As a rule, one should beware of books that promise to tell “The Truth” about something or other. Such grandiose claims almost invariably identify the author as a quack doctor, a crackpot or religious fanatic.

So, naturally, I felt somewhat alarmed when I learned that James Randi’s new book on occultism would bear the title *Flim-Flam! The Truth About Unicorns, Parapsychology and Other Delusions*. Mr. Randi and the present reviewer are both in some way associated with the U.S.-based Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP). The members of this group are frankly skeptical about the “paranormal,” but are formally committed to the view that such claims should not be rejected *a priori*, but should instead be examined “objectively and carefully.” Opponents of the Committee have repeatedly charged that we seldom practice what we preach. They have often portrayed us as dogmatic debunkers, motivated by an irrational aversion to anything that might undermine Official Science. Mr. Randi is probably the most visible member of CSICOP. So I was worried that the contents of his book might prove to be as arrogant

1 As is evident from the correspondence between Hoebens and Randi in the Hoebens Files. (Eds.)

as the title, thereby inviting further doubts about the sincerity of the Committee's stated policy.

Having now read *Flim-Flam!* (obtaining a copy proved to be extraordinarily difficult in Europe) I continue, alas, to feel somewhat uneasy. The book has considerable merits, and in fact I consider its publication an important event, but it is flawed by its author's frequent demonstrations of partiality.

Mr. Randi, a prominent conjuror whom I know as a highly intelligent and erudite man, has taken a look behind the scenes of para-science and reports a number of quite unsettling discoveries. Unfortunately, he has yielded to the temptation to generalize his findings, weakening the impact of his criticism in the process.

Mr. Randi's overall view of the problem has the charm of simplicity: on one side, there is "real science," on the other side, "pseudo-science." "Real science" is right, "pseudo-science" is wrong. This dichotomy serves as the starting point for Mr. Randi's excursion into the occult world. He implicitly assumes that competent observers will only observe what real science thinks possible. Those who claim to have observed the impossible must be victims of "delusions," "wishful thinking" or "faulty experimentation" – unless they are liars.

This, of course, is a version of Occam's Razor, but I am afraid that handling this instrument without cutting oneself requires greater methodological dexterity than Mr. Randi shows in this book.

Representatives of diverse "frontier sciences" have often complained that Mr. Randi is unfair to them. There is much in *Flim-Flam!* that suggests they have some reason to complain. Martin Gardner, in *Fads and Fallacies* (Gardner, 1957, p. 299), courteously apologized in advance for having to discuss Dr. Rhine and other reputable parapsychologists in a book mainly concerned with cranks such as L. Ron Hubbard, Velikovsky and Hörbiger. Mr. Randi in *Flim-Flam!* neither apologizes nor explains. Although he is convinced of the personal integrity of researchers like Schmidt and Tart he does not explicitly acknowledge that there is a difference, in terms of credibility, between psychical research, ufology, ancient astronauts, astrology, the Cottingley Fairies and the Bermuda Triangle. The reader is left with the impression that all these claims are equally hare-brained.

There is the subliminal suggestion that we should look askance at Dr. Beloff because Mr. Berlitz invented all those lost aeroplanes. I trust this was not Mr. Randi's intention, but he might have been more careful in choosing his words. Mr. Randi shows a marked lack of generosity by seldom or never acknowledging signs of sanity in his opponents. Irresponsible claims made by individual parapsychologists are presented as being char-

acteristic for the discipline as a whole, whereas debunking activities by “believers” do not receive the attention they deserve.

In his otherwise excellent chapter on “Medical Humbugs” Mr. Randi should, for balance, have mentioned that many parapsychologists have never been taken in by the Agpaos and the Arigos. Given the general tone of the book, many readers will conclude that “pseudoscience” believes that Arigo could work miracles, and that seeing through this mumbo-jumbo is a skeptical privilege. I must presume that Mr. Randi knows that the first thorough exposure of the Arigo myth was published by a parapsychologist, the Brazilian Professor Oscar González-Quevedo (1978). Prof. Quevedo is not mentioned in *Flim-Flam!*, though his portrait (knife under eyelid) is printed on p. 177. However, according to Randi’s caption, this face belongs to Arigo! While Mr. Randi systematically belittles the case for the defense, he sometimes overstates the case for the prosecution. A telling example may be found on p. 63. There, we read: “Recent attempts to test a ‘Mars Effect’ have shown that the Red Planet is just that, and not a magic influence that reaches across space to influence our lives. The Mars Effect was supposed to have been confirmed during investigations of the claim that prominent athletes were more apt to be born when that planet was influencing their sign. Careful tests have failed to support any such claim, though fancy excuses have been plentiful. But more money will go into similar projects. There are plenty of sponsors of such idiocy waiting”

This, I am afraid, is hardly a straightforward way to inform the public of “The Truth” about the CSICOP-Gauquelin controversy.² Before he completed the manuscript Mr. Randi must have heard that the methods by which some members of the Committee have attempted to refute Mr. Gauquelin’s findings (Kurtz *et al.*, 1979-1980) have been challenged not only by Gauquelin but also by confirmed skeptics. One of the CSICOP Council members, Mr. Dennis Rawlins, has insisted that the actual result of those CSICOP tests was an embarrassingly unambiguous victory for Mr. Gauquelin, a fact which the Committee is said to have tried to suppress (Rawlins, 1980). Mr. Rawlins is no longer on the Council: he told me he had been unceremoniously ejected.

Mr. Randi approvingly quotes Mr. Rawlins’ negative remarks on astrology in general. He certainly should have informed his readers of this prominent skeptic’s misgivings about the “careful tests” that supposedly refuted the Mars Effect “idiocy.”

It is also somewhat regrettable that *Flim-Flam!* does not contain a word on Professor Marcello Truzzi’s reasons³ for leaving the CSICOP and founding his own journal,

2 For the background and intricacies of the “Mars Effect” controversy, also see chapter 4-10 in this book. (Eds.)

3 See Clark & Melton (1979). (Eds.)

Zetetic Scholar.⁴ The fundamental disagreements within the skeptical camp are ignored.

Parapsychologists wishing to make fun of Mr. Randi are advised to compare a remark on p. 217 with a remark on pp. 211-212. On p. 217, Mr. Randi states: “I have never claimed to be able to prove a negative – an impossibility.” On pp. 211-212 we catch the author believing the impossible, for there he refers to the VERITAC experiments as having “proved – once again – that subjects do not have the ability to guess or to influence events any better than chance would have it.”

Having taken Mr. Randi to task for his shortcomings I will now extol his virtues. For in spite of its noxious side-effects, *Flim-Flam!* is an excellent treatment for that social ill which Professor Johnson has so aptly diagnosed as the addiction to “para-pornography.”⁵ Mr. Randi urges the reader not to take at face value what he is told about “the Wonderworld at the frontiers of our knowledge.” As a professional magician, he understands the psychology of deception better than almost anyone. In the first chapter, he has summarized “the hallmarks of paranormal chicanery” in twenty points. These points, I think, should be nailed to the doors of the Temple of Parascience for all to see.

Mr. Randi, moreover, has done more than issue warnings: he has extensively investigated the paranormal scene and he reports surprising examples of credulity, sloppiness and even gross dishonesty.

The victims of his onslaught may yet come up with convincing arguments to the contrary, but Mr. Randi has presented a strong *prima facie* case against some well-publicized and supposedly respectable claims.

Of particular interest is his discussion of the famous psi experiments at Stanford Research Institute. (The relevant chapter is entitled, “The Laurel and Hardy of Psi.”) The reports published by Russell Targ and Harold Puthoff have already been challenged by Wilhelm (1976), Marks and Kammann (1980) and Randi himself (Randi, 1975). *Flim-Flam!* presents fresh evidence that there is something decidedly wrong with Targ and Puthoff’s “positive proof that E.S.P. exists.”

Mr. Randi has succeeded in contacting a number of persons who were present at some of the SRI séances and their story contradicts that told by the experimenters.⁶

4 *Zetetic Scholar: An Independent Scientific Review of Claims of Anomalies and the Paranormal*, edited by Marcello Truzzi [the *Zetetic Scholar* ceased publication after double issue #12/13, 1987 (Eds.)].

5 See chapter 2-10 in this book. (Eds.)

6 However, see Hoebens’ postscript attached to this review. (Eds.)

For example, what Dr. Arthur F. Hebard, the builder of the magnetometer Ingo Swann is supposed to have influenced paranormally, told Randi about this experiment is very revealing. According to Dr. Hebard – as quoted in *Flim-Flam!* – nothing extraordinary took place. In several reports, the experimenters have created the misleading impression that the irregularities registered by the magnetometer's chart recorder coincided with Mr. Swann's attempts to bewitch the equipment.

I do not have sufficient documentation at my immediate disposal to clear up the following minor mystery: on p. 133 Mr. Randi quotes Targ and Puthoff as falsely claiming that the experiment had been successfully repeated the following day, while numerous other scientists were present. ("It's a lie," according to Dr. Hebard).

Although no exact reference is given it appears that, according to Randi, this was said at a conference in Geneva in August 1974. In *Mind Reach* (Targ & Puthoff, 1977, p. 25), however, it is stated quite clearly that this attempt at replication was unsuccessful. If the Geneva quote is correct, Targ and Puthoff have contradicted themselves and Mr. Randi has missed an opportunity to score a further point against the "Laurel and Hardy of Psi."

Of interest too is the testimony of Zev Pressman, the SRI photographer who shot the famous SRI film on Geller *et al.* From Mr. Pressman's statement it appears that, notwithstanding the experimenters' claim to the contrary, a number of die-throwing experiments with Geller were not reported. Moreover, the celebrated experiment shown on the film was actually a reenactment of the actual miracle, which Targ and Puthoff said they had observed after Mr. Pressman had gone home! There are more revelations like that, and the net result is that my own reluctance to take anything from those two experimenters at face value has considerably increased.

In the chapter "Off the Deep End," Mr. Randi continues his feud with Dr. Jule Eisenbud, the discoverer of "thought photographer" Ted Serios. Eisenbud is portrayed as a credulous dupe, and some evidence is presented to support that judgment. Unfortunately, there is not a word about Serios' alleged "confession" that was reported by E. Tobias (1973) (and gleefully repeated by Prof. Prokop and Dr. Wimmer, 1976, p. 106). From his silence on this point we may surmise that Mr. Randi doubts the authenticity of this "confession." He should, I think, have been more specific on this, as, after all, he himself was cited by Mr. Tobias as the source of the story.

In the same chapter, we find a discussion of Prof. Hasted's metal bending experiments. Prof. Hasted is accused of inexcusable naivety. For example, when told by a member of Granada TV crew that his subject Steven North had been caught cheating, the Professor

calmly replied that, as Steven North does not cheat, this must obviously have been a case of malobservation. This is Occam's Razor upside down!

Mr. Randi quotes a comment by Dr. Paul Horowitz of Harvard University to the effect that some of Prof. Hasted's experiments were perfectly worthless. It is to be hoped that Dr. Horowitz some day will publish a more detailed criticism.

To conclude: *Flim-Flam!* does not live up to its promise to tell all that is worth knowing about the paranormal. Mr. Randi prematurely claims total victory over his foes. His belligerence will embarrass those of his fellow-skeptics who prefer a dialogue to mutual recrimination.

By overstating his case, Mr. Randi provides a welcome excuse for those parapsychologists who would prefer to ignore his extremely damaging revelations about many aspects of their discipline.

Postscript (May 25, 1981)

Shortly after I submitted this review (May 1981) I learned that a highly critical review of *Flim-Flam!* by D. Scott Rogo would be published in the June 1981 issue of *Fate* (Rogo, 1981a). I obtained a copy of that article by rush delivery and feel now compelled to express further reservations. Dr.⁷ Rogo asserts that Mr. Randi has misquoted two of his chief sources and prints affidavits from the persons concerned to that effect. Dr. Hebard, although he largely confirmed Mr. Randi's story, told Rogo that the quotes attributed to him in *Flim-Flam!* contain non-trivial inaccuracies. Far more damaging is the affidavit from Mr. Zev Pressman, in which the latter flatly denies ever having spoken to Mr. Randi. He also denies that the SRI film showed a reenactment of the dice experiment. Obviously, someone is trying to pull our legs. Rather than incorporating these new allegations (about the background of which I have no first hand information) into my review I have suggested to the Editor of this journal that he invite Mr. Randi by registered letter to reply in this or the next issue to Mr. Rogo's accusations regarding the Hebard and Pressman testimonies. I am gratified that the Editor has acted on this suggestion and eagerly await Mr. Randi's explanation.⁸

7 D. Scott Rogo (1950-1990) held no doctoral degree. (Eds.)

8 Randi never submitted a comment or response for publication in the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, but he replied to Rogo in *Fate* magazine. For the full discussion see Rogo (1981a, 1981b, 1982) and Randi (1981). (Eds.)

CHAPTER 4-14

Editorial Introduction

Works on the vague and enigmatic prophecies of Nostradamus for a long time have been among the best-selling items on the vast esoteric book market. This makes them obvious candidates for the discussion of the interpretive fertility of occult belief systems and their proponents. It therefore does not come as a surprise that Hoebens has repeatedly tried his skeptical hand and journalistic talents on this late-medieval prophet and the huge flock of those who, even today, try to make sense of the “seer’s” rather obscure writings.¹ Hoebens’ detailed critique of one of the most prominent and successful books on Nostradamus’ prophecies and their interpretation is reprinted here. It originally appeared as a review essay, in 1982, in the *Skeptical Inquirer*, 7, (1), 38-45, and it was reprinted in the *Indian Skeptic*, 3 (1990), (5), 20-25.

Moreover, we are very pleased to present here for the first time, as an addendum, a brilliantly satirical rendering of “Nostradamitis,” which Hoebens wrote under a pen name. It was never published before and was rediscovered in 2008 in the family archives of Hoebens’ brother Lodewijk – see the editorial postscript. (Eds.)

THE MODERN REVIVAL OF “NOSTRADAMITIS”

TESTING A PHENOMENAL BOOK’S INTERPRETATIONS

Nostradamus: Historien et Prophète. By Jean Charles de Fontbrune. Monaco: Éditions du Rocher, 1980, 576 pp., illustrated.

Fontbrune’s *Nostradamus* is an extraordinary phenomenon. At the time of this writing² over 700,000 copies of the expensive original French edition have been sold, and translations into several languages have either been published or are being prepared. The book has been given front page treatment by the European pop media from Oslo to Lisbon, but it has also featured as the subject of cover stories in weeklies as respectable as *Der Spiegel*, *Die Zeit* and *Nouvelles Littéraires*. An opinion poll ordered by the French illustrated *Paris*

1 For instance, see Hoebens article, “Nostradamus zat er ook wel eens naast [Even Nostradamus was known to make the occasional mistake]” in *De Telegraaf* of March 13, 1982.

2 Hoebens wrote this article in May of 1982. (Eds.)

Match revealed that three-quarters of the French public are aware of the book's existence, while a quarter (some 17 million inhabitants of an advanced Western state) believe its horrifying message. Headline writers coined the word "Nostradamitis." Recent reports indicate that the virus is about to cross the Atlantic.

What is all the fuss about? Jean Charles de Fontbrune (pseudonym for M. Pigeard de Gurbert) claims to have discovered the hidden key to the enigmatic "prophecies" of the sixteenth-century French seer Michel de Nostredame, better known as Nostradamus. A careful analysis of these prophecies, he says, has revealed that Nostradamus foresaw in gruesome detail that no less than two major wars would take place before the end of this century. World War III, instigated by Khadaffi and to break out in the eighties, will see Paris and Istanbul utterly destroyed, the Pope assassinated, and Europe occupied by Russians and Muslims. After three years of ferocious fighting, the aggressors will be soundly defeated by Henry (the Happy) V, King of France, and his allies.

A period of relative peace and prosperity will follow. Paris having been reduced to radioactive ashes, Avignon will become the capital of France. This happy interlude will end around 1999 when the final war breaks out. The Chinese and (again) the Muslims will overrun the Occident; Rome will be destroyed. The last of the Popes will be captured. The papacy and the French monarchy will disappear. After 27 years of unmitigated disaster, the Millennium will have arrived.

In order to understand how this somewhat silly horror story could have caught the imagination of millions of Europeans, it is obviously necessary to consider the socio-political conditions prevailing in modern Europe. The Old World was ready for Nostradamus/Fontbrune's apocalyptic message. In a society disturbed by economical insecurity and political unrest, scared by crime and terrorism, and threatened by nuclear holocaust, prophets of doom and hope are assured a large audience. However, predictions of imminent Armageddon are a dime a dozen. There must be something special about Fontbrune's book to have persuaded 17 million Frenchmen to accept it as a prospectus of future history.

This special factor, I suggest, may well have been the carefully fostered impression that *Nostradamus: Historien et Prophète* is not a wildly speculative work but rather the result of dispassionate, sober and scientific analysis. A blurb, echoed in many reviews, interviews, feature articles and broadcasts, emphasizes Fontbrune's claims to serious scholarship: "... *une méthode rigoureuse ... le rationnel* always prevailing over personal judgment and imagination ... scrupulously respecting the prophetic texts ... unequaled precision ... unquestionable intellectual integrity ..." It is the purpose of the present review to examine these claims critically.

De Kerdéland (quoted in Gauquelin, 1979) has characterized the prophecies of Nostradamus as “an inexhaustible magic hat from which modern prestidigitators can pull innumerable rabbits.” The prophet himself has referred to his “cloudy manner, with abstruse and twisted sentences.” According to L. LeVert (1979), “he pushed the techniques of multivalence to an extreme not paralleled until the twentieth century.”

Blissfully unaware of the pitfalls of subjective validation, occultists ever since the sixteenth century have attempted to “decode” the garbled and confused presages. Their efforts have demonstrated – at least to the satisfaction of their authors – that Nostradamus had accurately foretold the death of Henry II, the Thirty Years War, the French Revolution, Napoleon, the rise and fall of Hitler, the deaths of John and Robert Kennedy, and even the fact that in March 1982 an exhibition of Flemish tapestry would open in the New Church on Dam Square in Amsterdam. The interpretations often reflected the political predilections of the interpreters. Fascist Nostradamologists had the prophet predict Axis victory; the anti-fascists discovered portents of Axis defeat.

Decoding the prophecies is a game without rules. The freedom to indulge in metaphorical and cryptogrammatical interpretation virtually assures a rich harvest of striking hits. Skeptical authors such as J.C. Adelung (1789), P.F. Buget (1860-1863), R. Baerwald (1926), C. von Klinckowstroem (1927), E. Leoni (1961), M. Gauquelin (1979), and L. LeVert (1979) have pointed to the Rorschach-like quality of the prognostications. They have also demonstrated that many Nostradamologists are well versed in the ancient art of cooking the books. Undeterred, the aficionados have continued to exhibit their prize rabbits.

Fontbrune is critical of many of his fellow interpreters. He pokes fun at an astrologer who in 1938 predicted, on the basis of the prophecies, that 1940 would be a year of French *grandeur*. He solemnly warns against charlatans and sensation mongers who lack intellectual integrity and *la méthode rigoureuse*. His own method is explained in the first chapter of the book. At least, that is what the title, “*De la Méthode!*,” seems to promise. Unfortunately, one searches in vain for anything even remotely resembling a clear statement of the methodological principles underlying the claim that Fontbrune has cracked the Nostradamus code and restored the “real” chronology of the verses.

We are not even told why the author accepts not only the famous “centuries” of quatrains, the “presages,” and the two prophetic letters but also the 58 sixains as authentic Nostradamus. The sixains are generally assumed to be spurious, and their inclusion would have required at least some explanation. What it all boils down to is that Fontbrune simply *asserts* that the prophecies contain a “real meaning” and that he, Fontbrune, has finally succeeded in matching the verses with the historical and future

events actually foreseen by Nostradamus. His “method” does not basically differ from that of the other Nostradamus believers: *cherchez la correspondance* – keep searching for similarities until a match has been made.

Fontbrune repeats the classic argument that the prophet was forced to resort to apparent multivalence (requiring apparently far-fetched interpretations) because the dreaded Inquisition would have accused him of sorcery had he been more specific. What is wrong with this argument is that sixteenth-century inquisitors could not possibly have known the difference between a “veiled” and a “specific” presage if it was concerned with an event sufficiently far in the future. If Nostradamus had written a quatrain containing the names “Thatcher,” “Galtieri,” “Haig,” “Pym” and “Falkland Islands,” this would have meant nothing to his contemporaries. To us, the message would have been clear. Why did Nostradamus write no such quatrains? Fontbrune does not provide an answer. Apparently he is not even aware of the problem. Instead, he invites us to be amazed by the fact that, in Century IX: 2, Nostradamus mentions “D’Arimin Prato,” which, because Mussolini was born in a village equidistant from Prato and Rimini, must refer to the fascist dictator. Would the Inquisition in France really have had reason to become suspicious if Nostradamus had written “Benito Mussolini” instead of “D’Arimin Prato”?

Although Fontbrune’s *méthode rigoureuse* is not made explicit, the quality of his scholarship can be inferred from the arguments he advances to support individual “matches.”³ Before I analyze some of his prize examples, however, I wish to commend Fontbrune for having avoided at least a few of the more outrageous bloopers found in the writings of other believers.

Unlike Erika Cheetham (1973), he has resisted the temptation to translate “Hister,” a name mentioned in several quatrains, as “Hitler.” (“Hister” is simply Latin for “Danube.” There is not the slightest doubt that Nostradamus was referring to a river, not a dictator. In Century V: 29, there is even a bridge over “Hister”!)

Unlike Brian Inglis (1977), Fontbrune does not claim that the quatrain containing an anagram of the name of Mazarin and a number of strikingly accurate statements on the latter’s career “defied rational explanation.” (Rational explanation, for the benefit of Mr. Inglis: the two Mazarin quatrains are found in a counterfeit edition of 1649, backdated to 1568. It was a celebrated hoax, perpetrated by the enemies of the Cardinal.) It is also praiseworthy that, on the basis of his interpretations of the verses he believes refer to

3 Some press reports have fostered the impression that Fontbrune employed a sophisticated computer program to crack the code. This is misleading. A computer was used only for counting, indexing and cross-indexing words and names mentioned in Nostradamus’ work.

future events, Fontbrune makes a number of quite specific predictions. So some of his claims at least have the merit of being falsifiable.

The central theme of the book is stated on page 13: “Nostradamus’ message was intended for the twentieth century: therefore the texts dealing with earlier centuries are included only to testify to the validity and the authenticity of the prophecies.”

For Fontbrune’s hypothesis it is essential that the prophecies extend no farther than the end of the present century, which will see the end of pre-Millennium history. Here, however, he is faced with a problem: in a letter to his son César the oracle himself clearly states that his prophecies extend to the year 3797. The manner in which Fontbrune turns his apparent refutation into a resounding confirmation of his hypothesis certainly deserves a place of honor in the annals of crackpot arithmetic. His “proof” starts with the traditional biblical chronology: 4757 years from Adam through Noah, Abraham, Moses, and David to Jesus Christ. Next he notices that there is a difference of 2242 years between 1555 (the year the letter to César was written) and 3797. He then adds 4757 and 2242, which gives 6999. Corrected for the Christian chronology, Fontbrune triumphantly explains, this yields the year 1999.

On a similar level of scholarship is Fontbrune’s demonstration that Nostradamus foresaw a French King Henry, nicknamed “the Happy,” appearing after the fall of the Fifth Republic. Many readers will be familiar with Century I:35, one of the most celebrated of all Nostradamus’ quatrains:

The young lion will overcome the old one
on the field of battle in single combat
He will put out his eyes in a cage of gold.
Two fleets one (or: in one of two combats), then die a cruel death.

Several authors (virtually ignored by Fontbrune) have demonstrated that the interpretation favored by the believers (the quatrain accurately predicts the death of Henry II of France, who died in 1559 after having been injured in the eye at a tournament) is based on a distorted version of the historical facts (Klinckowstroem, 1927; Leoni, 1961; Gauquelin, 1979).

However, the most compelling argument against this interpretation is found in Nostradamus’ own work. His introductory letter of June 27, 1558, is dedicated to Henry II, whom he calls, of all things, “invincible.” Fontbrune is aware of this obstacle, but instead of concluding that Century I: 35 cannot possibly refer to Henry II, he concludes

that Henry II cannot possibly be the king to whom the 1558 letter was addressed! He explains that “second” is a gallicized form of the Latin “secundus,” which can, among other things, mean “prosperous” or “happy.” Ergo: the letter is actually addressed to a future king. This notwithstanding the fact that, in the letter, Nostradamus reminds His Majesty of a previous meeting.

At about the time the new monarchy replaces the French Republic, Pope John Paul II will be assassinated in Lyon. Here is how Fontbrune demonstrates that Nostradamus predicted the Polish-born pontiff. In several quatrains, mention is made of “Pol mensolée,” “Mansol” or “saint Pol de Manseole.” *Pol*, says Fontbrune, has a double meaning. It refers both to the name of the present pope and to his land of birth. *Mansol* is a word concocted by Nostradamus from two Latin words, *manus* (work) and *sol* (sun). Together they mean “the work of the sun.” Now another famous prophet, Malachias, had predicted that the chosen device of the present Pope would be “*de labore solis*,” which also means “work of the sun.”

This is tripe. Given the context in which “Mansol,” etc., is mentioned there is precious little doubt that Nostradamus was referring to a geographical location near the Rhône in southern France. There is such a place: Saint Paul de Mausole, just outside Nostradamus’ birthplace, St. Rémy, a few miles from the Rhône. Saint Paul de Mausole was a well-known monastery that later became a lunatic asylum. Its most famous inmate was Vincent Van Gogh. Fontbrune ignores this obvious explanation. However, because Nostradamus insists that “Mansol” is near the Rhône, his interpreter is forced to have the Pope die near the river.

It will be clear by now that Fontbrune’s predictions are based on a very silly sort of logic. However, they might in principle become true, thereby retroactively justifying to some extent Fontbrune’s reasoning. The fairest way to assess his “method” is therefore to examine the way Fontbrune seeks to demonstrate that some of Nostradamus’ prophecies have already been validated by history. I will analyze two instances that are presented in the book as particularly striking hits.

On page 107, Fontbrune quotes from the Letter to Henry II (or V, as he prefers): “... *et durera ceste cy jusqu’à l’an sept mil cens nonante deux que l’on cuidra estre une rénovation de siècle ...*” This quote is followed by Fontbrune’s translation into modern French, which is here translated into English: “And this [the monarchy] will last till the year 1792, which will be believed to be a renovation of the age.”

This seems an impressive hit, for the French monarchy was in fact abolished in 1792. However, the “translation” is utterly misleading, and this cannot possibly be the result of

an honest mistake. By placing “monarchy” in brackets Fontbrune unambiguously states that, in the sentence preceding the short quote, Nostradamus was referring to the French monarchy. However, the full text of the Letter to King Henry is not included in the book and the reader who does not have access to other sources cannot look up the literal context.

Here is what Nostradamus really wrote in 1558: “This year [a year identified only by astrological configurations] will see the beginning of a worse persecution of the Christian Church than ever took place in Africa, and this will continue till the year 1792, which will be taken for a renovation of the age.”

Nostradamus had *not* been referring to the French monarchy, but to the end of a period of religious persecution. No known historical event can be matched to this prediction, as was remarked as early as May 1792 by an anonymous contributor to the *Berlinische Monatsschrift*.

Also, Fontbrune conveniently ignores what follows the “1792” quote. There, in one of his few specific predictions, Nostradamus states that “sometime” after 1792 Venice will rise to the status of a super power comparable to ancient Rome. The prophet was as wrong as he possibly could have been. In 1796, after a long decline, the Venetian Republic was abolished by Napoleon Bonaparte, never again to play an important role in world events.

Both in the introductory chapter and in the Epilogue Fontbrune names Century VIII:12 as one of the most impressive examples of accurate prediction to be found in all of Nostradamus’ verses.

He will appear near Buffalora
the highly born and prominent one who entered Milan
the abbot of Foix with those of Saint Morre
will commit roguery, dressed up as villagers.

According to Fontbrune this quatrain applies, with startling precision, to Napoleon III, who in 1859 entered Milan, which is not very far from the village of Buffalora. Soon after, the French emperor cheated on his Italian friends by engaging in peace talks with the Austrians.

Now the naive reader will object that, whatever else Napoleon III may have been, he most certainly was not the abbot of Foix whom Nostradamus casts as the villain of the piece.

This problem, however, is taken care of by Fontbrune’s rigorous method. He first explains that “*Morre*” is somewhat like “*mos*,” which is Latin for “law” or “rule.”

Alliances being concerned with laws and rules, “*Sainct Morre*” refers to the “Holy Alliance” to which Austria had been a party in 1815. Next, Fontbrune reminds us that “abbot” is derived from the Syriac “*abba*” which means “father.” From this it is only a short step to “owner.” As it happens, there once lived a Count of Foix, Gaston III, who was nicknamed “Phoebus.”

This is the clue Nostradamus had cleverly hidden in his quatrain; for, believe it or not, Napoleon III owned a horse also named Phoebus! So the “abbot of Foix with those of Sainct Morre” means: “the owner of Phoebus” and the Austrians. *Quod erat demonstrandum!*

Editorial Postscript on a Surprise Find

In 2008, one of the editors (H.M.) visited Hoebens’ brother Lodewijk to peruse a collection of photographs, cartoons and similar memorabilia in the family archives. A truly unexpected extra item found during that afternoon was an English manuscript of someone by the name of “Erik A.H. Machete.” We had never seen that manuscript before, neither in the Hoebens Files nor in any published version. We decided to append that brilliantly satirical manuscript to Hoebens’ review of De Fontbrune’s book on Nostradamus, because Machete’s amazing revelations shed a totally new light not only on Nostradamus, but also on the notorious “Mars Effect Affair” that is independently discussed in several chapters throughout this book. Machete put his findings to paper at some point between 1982 and 1984, but we do not have any indication as to the precise date. One thing is for sure, however: Erik A.H. Machete is no other than Piet Hein Hoebens. (Eds.)

DID NOSTRADAMUS PREDICT THE sTARBABY CONTROVERSY?

By ERIK A.H. MACHETE

The 12th quatrain of the VIII Centurie has always appeared as one of the more puzzling of Nostradamus’ predictions. Attempts to match these verses to the rise of Mussolini, the fall of Mussolini, the Italian campaign of Napoleon III, the attempt to kill the pope, the career of Al Capone and the Second Coming have been *partially* convincing at most.

Only recently has it been possible, using rigorous scientific methodology, to discover the actual message the Great Prophet has been trying to convey through this quatrain.

For a proper understanding, it will be necessary to acquaint oneself with the original French text, because it contains a number of puns, allusions and anagrams that get lost in any translation.

“Apparoistra auprès de Buffaolore
l’hault et procere entré dedans Milan
l’Abbé de Foix avec ceux de Saint Morre
feront la forbe habillez en vilain.”

A literal translation would run as follows:

“Near Buffaolore will appear
the high and prominent one entered into Milan
the abbot of Foix and those of Saint Morre
will commit mischief, dressed up as villains.”

No important historical event can be said to correspond to these verses. However, it has always been assumed, uncritically, that Nostradamus was only concerned with predicting *political* events. It is too often forgotten that Nostradamus was not a politician, but a scientist, and so would naturally be interested in the future of science, especially that science he himself excelled in: astrology.

In this paper, it will be clearly demonstrated that, in VIII:12, Nostradamus predicted, with amazing accuracy, a critical occurrence in the history of astrology. I am referring to the recent attempt by CSICOP, an international group of extreme skeptics who want to discredit the scientific revolution associated with the Age of Aquarius, to suppress important evidence in favor of the so-called Mars Effect. For the details, the reader is referred to two important articles by Dennis Rawlins,⁴ “sTARBABY” and “Remus Extremus.” Suffice it to say that the distinguished parapsychologist Dr. R.A. McConnell has labeled this affair “the biggest scandal in the history of rationalism.” Nostradamus foresaw this major scandal and described it, in astonishing detail, in the present quatrain. *He even named the three major perpetrators and two of their accomplices!* Once the cleverly hidden clues are discovered, the startling truth becomes apparent. *This cannot be a matter of coincidence!*

4 See Rawlins (1981, 1981-1982). (Eds.)

The first line is obvious: “Buffaolore” is only a slightly distorted version of “Buffalo.” The headquarters of CSICOP are located in Buffalo, N.Y.!

The second line is more enigmatic. Here, Nostradamus has employed some literary devices borrowed from classical Latin literature. “L’hault” means, literally, “the high one” or “the long one.” It is not clear, to whom the prophet is referring, except when we realize that Nostradamus has employed the ancient device of the “ironical inversion.” Ironical inversion is saying the exact opposite of what is intended. (A familiar example: saying “nice weather” when it is raining cats and dogs.) So “the long one” must be read as “the short one.” Who is this “short one”, then? Nostradamus’ clever pun becomes clear when we realize that the German word for “short” is “kurz.” The name of the chairman of CSI-COP is Kurtz! The word “procere” means “prominent.” Kurtz is a University professor. Professors are prominent people. The word “Milan” is a transparent anagram of “malin,” which means “the evil one.” Readers of “sTARBABY” and “Remus Extremus” will know that Prof. Kurtz is an evil person. So, properly interpreted, the second line reads: “The prominent Kurtz, engaged in evil.”

The first words of the third line hardly present a problem. “L’Abbé” is an obvious anagram of “Abell.” Prof. George O. Abell was one of Kurtz’ two chief collaborators in the Mars Effect imbroglio! The surprising explanation of the rest of this line will be given below.

The fourth line clearly indicates that mischief is done. This is what “feront la forbe” means. But why does Nostradamus insist that the perpetrators were “dressed up as villains”? This puzzle can only be solved if we closely study the French text. This contains yet another clever anagram. Only two letters have to change place for “lez en vilain” to read “Zelen vilain.” The name of Kurtz’ other collaborator is professor Marvin Zelen! Assuming that the “b” is a spurious interpolation (printers were notoriously sloppy in the 16th century) the remaining letters form the word “hail.”

So the fourth line should be read as: “will make mischief and hail the villain Zelen.”

Now we should return to the enigmatic mention of “de Foix avec ceux de Saint Morre” in the third line. Here, Nostradamus shows himself the unsurpassed master of cryptogrammatical acrobatics. Fontbrune has already demonstrated that “de Foix” stands for “Phoebus” (Gaston III, count of Foix, was nicknamed Phoebus). Alas, Fontbrune has erred in assuming that Nostradamus had been referring to Napoleon III, whose horse was also nicknamed “Phoebus.” Given the “sTARBABY” context, another interpretation is called for. There is little doubt that the prophet is referring to the treacherous role a Dutch skeptic has played in the affair. Phoebus should be read as P. Hoebus. Now the

Dutch CSICOP member Piet Hoebens (pronounced the same as Hoebus) initially was – or at least pretended to be – critical of Kurtz & Co, but in a later stage became an accomplice. In a letter dated 1 May 1982 Abell wrote to him: “Many, many thanks for your thoughtful help!”⁵

Perhaps the biggest surprise of the quatrain lies in the words “de Saint Morre.” This is another anagram, by means of which Nostradamus indicates that he foresaw even the role of those “sTARBABY” conspirators who have attempted to deny any personal involvement. It is as if the prophet tells us: “I see what you are thinking and doing, even if you conceal yourself – like your name is concealed in this quatrain.” The solution of the anagram is: “Remo, c’est Randi” or, in translation: “Randi is for Remus.” The profound meaning of this statement becomes clear when we know that Dennis Rawlins gave Kurtz the nickname “Remus” and that James Randi, member of the Executive Council of CSICOP, has played a mysterious but no doubt important backstage role in the affair. More persons were involved in the scandal. Why did Nostradamus mention only Hoebens and Randi (in addition to the three principal offenders)? The prophet knew what he was doing. With his amazing prophetic gift he foresaw that, shortly after the scandal was revealed, Hoebens and Randi would conspire, falsely to discredit not only the Mars Effect, but also the Greatest Prophet of All Times – Nostradamus.

5 This is in fact confirmed by Abell’s letter of that date in the Hoebens Files. (Eds.)

CHAPTER 4-15

Editorial Introduction

Since Hoebens was a professional journalist, well known and widely read in his home country, it seems only natural that he keenly observed the ways his journalistic colleagues represented and commented on "The Paranormal." In most cases, he felt compelled to draw unflattering conclusions. Hoebens therefore was thrilled when he learned that a book on *Superstition and the Press* was to be published, compiled and written by Curtis D. MacDougall (1903-1985), a legendary figure in U.S. journalism education. Yet, Hoebens was not overly impressed by what eventually he found. Nevertheless he reviewed the book, shortly after it had appeared, for the *Skeptical Inquirer*, 8, 1984, 371-373. (Eds.)

A GUIDE TO PRESS COVERAGE OF THE PARANORMAL

Superstition and the Press. By Curtis D. MacDougall. Buffalo, NY: Prometheus Books, 1983, 615 pp.

Superstition and the Press is a valuable book. It is a highly recommendable book. It is, alas, also a book that defies all attempts to review it properly. In his concise "Some Recent Books" column (*Skeptical Inquirer*, Winter 1983/84), Kendrick Frazier has summed up the merits and shortcomings of MacDougall's approach to his subject: "Everything you want to know about what the major daily newspapers have said about the paranormal in recent years ... MacDougall's treatment is extensive rather than intensive ... His style is matter of fact: there is a minimum of commentary and interpretation ..."

According to the blurb, "In *Superstition and the Press* America's most distinguished journalism professor and veteran newspaperman provides a devastating critique of the treatment by the press of claims of supernatural phenomena." I am afraid this is hardly a felicitous statement. MacDougall has not provided a critique, but an inventory. Structurally, *Superstition and the Press* resembles the *Book of Lists* and the *People's Almanac*: a compilation of fascinating facts, not a systematic analysis of an issue.

With remarkable industry, MacDougall has for the past three decades clipped and filed from several U.S. newspapers every item he could find that related to the "paranormal" in the broadest sense of the word. There are chapters on such diverse topics as fundamentalism, sea serpents, fortune telling, ghosts, ESP, and gurus. "I believe I come close to a 100 percent sample for the past three decades of what appeared on my subject

in Chicago, New York, Washington and Los Angeles newspapers,” MacDougall proudly states in the Introduction.

As a source book, *Superstition and the Press* is an indispensable aid to the active skeptic. Thanks to MacDougall’s exquisitely dry wit it is also a pleasure to read. The title seems ill chosen, as it carelessly lumps together crackpot doctrines, religious superstition, and folk beliefs with controversial but respectable research programs like psychical research and cryptozoology. A spot-check revealed that the index – an important ingredient in a book of this type – is inadequate. Most of the names mentioned in the text are missing.

I noticed several errors of fact, none of them particularly serious. For example, MacDougall lists Frederick Myers (born in Keswick, England; died 1894) and Edmund Gurney (born in Horsham, England; died 1888) among the 20th-century Americans interested in the phenomena.¹ The statement that the journal *Free Inquiry* “takes on Jerry Falwell’s Moral Majority and other right-wing organizations and publications” to the uninitiated readers suggests that *Free Inquiry* is a left wing periodical. Paul Kurtz, CSICOP chairman, editor of *Free Inquiry* and publisher of Prometheus Books, will be unhappy with this compliment, especially as opponents have systematically tried to link CSICOP skepticism to leftists conspiracies.

The usefulness of the book would have increased if MacDougall had consistently drawn the reader’s attention to reliable sources of information on specific claims he feels have been inadequately treated by the media. He does so only sporadically, which may confuse the reader.

According to the blurb, “The author’s conclusion is that newspapers, with rare exceptions, treat claims of supernatural experiences and paranormal phenomena without questioning their validity.” I doubt that his verdict is fully justified. To judge from MacDougall’s own myriad quotes, the newspapers’ record in this field is actually better than I had expected. Sure, many journalists have been taken in by the most transparent of pseudo-miracles, but many others have done their best to remain skeptical and responsible. *Superstition and the Press* presents many encouraging examples, especially in the sections on paranormal healing. Nevertheless, I must agree that a person who had to rely solely on (U.S.) newspapers for his information on ESP and similar phenomena would be insufficiently enlightened. But can we blame the press for this state of affairs? I am not sure. Journalism is not scientific inquiry. It is not the journalist’s business exhaustively

1 The problem is not merely that Myers and Gurney both were 19th-century British scholars instead of 20th-century Americans. Rather, Myers’ first name (correct: Frederic) and the year of his death (1901, not 1894) are also wrong. (Eds.)

to investigate the events he is covering. He simply lacks the time, and, often, the necessary expertise for examination in depth. His task is to locate the best available sources of information and to use these as carefully as he can. The journalist unavoidably has to rely on “authorities” – on people he has reason to believe know more about the subject than he does.

In dealing with the “paranormal,” the newspaperman is faced with a tricky problem: There are no universally accepted criteria by which one can identify a “reliable source.” Criteria that in any other context would appear solid can be perfectly useless in the borderlands of science. Readers of the journal know why we are well advised to take the interesting claims by Drs. Puthoff and Targ with a pinch of skeptical salt. However, can we reasonably demand that a newspaper reporter intuitively distrusts statements made by two scientists with excellent credentials in mainline science and employed by a prestigious scientific institute? Perhaps we can demand that he try to balance his report by offering “equal space” to the skeptical opposition. However, the journalist would soon discover that the skeptics are far from unanimous in their skepticism.

What if, for example, our hypothetical newspaperman were to see a copy of *Skeptical Inquirer*, vol. 5, no. 1, where Ray Hyman, a member of the Executive Council of CSICOP, writes: “I find it dismaying that most of the criticism of current parapsychological research is uninformed and mis-represents what is actually taking place”? Wouldn’t he have second thoughts about offering equal space to critics who seem to spend so much of their time being uninformed and misrepresenting their opponents’ positions? Nobody will demand that a reporter, before reporting the claim that the earth is spherical, rush to the office of the Flat Earth Society in order to take down its comments. To judge from the public statements of some of the most vociferous antagonists, the proponents and the skeptics are each other’s Flat Earth Society. This is very confusing to the journalist, who cannot possibly decide which expert is right unless he first becomes some sort of expert himself.

It is a pity that MacDougall does not discuss such issues at length. *Superstition and the Press* is an admirable achievement, but the author might wish to consider writing a companion volume where the remarkable raw material presented in the first one is subjected to a systematic analysis.²

2 For extensive documentation, evaluation and discussion of representations of „The Paranormal“ in leading German magazines and newspapers (such as *Der Spiegel* and *BILD*), see the recent studies by Mayer (2003, 2004, 2008). In contrast to MacDougall’s useful compilation, these are not mere inventories, but detailed analytical studies of press treatment with a wealth of interesting findings. (Eds.)

CHAPTER 4-16

Editorial Introduction

Hoebens, in 1984, wrote a review of the (then) latest book by French psychologist and statistician Michel Gauquelin. Since Gauquelin's "neo-astrological" claims had been the subject of much controversy in the 1970s and early 1980s both German and English readers could be expected to be interested in an unprejudiced assessment of the book. Hoebens therefore wrote the review (in English) and both Eberhard Bauer (who also took care of the German translation) and Marcello Truzzi agreed to publish it in their respective German and American journals. In both cases, the reviews appeared posthumously in, respectively, the Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie (vol. 28, 1986, pp. 161-163) and the Zetetic Scholar (No. 12/13, 1987, pp. 160-162). (Eds.)

BOOK REVIEW: THE TRUTH ABOUT ASTROLOGY

The Truth About Astrology (U.K. title) or *Birthtimes: A Scientific Investigation of the Secrets of Astrology* (U.S. title). By Michel Gauquelin. Oxford: Blackwell, 1983; New York: Hill and Wang, 1983, 204+ix pp. hardbound (paperback, 1984).

The range of topics discussed in Michel Gauquelin's latest book is somewhat narrower than the British title suggests. *The Truth About Astrology* does not offer a comprehensive evaluation of astrological claims. Apart from the chapter "The Horoscope Falls Down" (debunking traditional astrology), the book deals almost exclusively with Gauquelin's own work in "neo-astrology."

Readers familiar with Gauquelin's earlier publications will find comparatively little new information in *The Truth*. The book recommends itself as an excellent summary of "neo-astrology" and is particularly interesting for what it reveals about its author's intellectual personality.

In the concluding chapter, Gauquelin confesses: "Though I am so full of my subject, so determined to defend it, so proud of my discoveries, I am still tormented by two feuding demons. The first is the fear of having been mistaken in asserting that astral influence is real; the second is the agonizing thought of all I have been unable to discover or explain."

Gauquelin describes his own relationship with astrology in terms of “passion.” His passion, however, is a complex affair. At the same time he is repulsed by astrology’s inanities and attracted by its quaint and mysterious charms.

A case in point is his attitude towards the astrological tradition as an “explanatory model.” Methodologically a conservative, he assumes – most of the time – that the “planetary effects” he believes he has discovered will eventually be explained in terms of non-occult physics. However, occasionally he wonders whether “perhaps I am making a mistake in trying to rid the planetary effect of all ‘absurdity’” and “to substitute a rational and convincing argument for the astrologers’ explanations” (p. 159).

Obviously, Gauquelin finds it hard to make up his mind. Here I cannot but sympathize with him. Much as I am impressed with the quality of the evidence, I must confess to being unable to make any sense of these planetary effects. All attempts to explain them (i.e., to suggest a way they might cohere with the rest of nature) strike me as in varying degrees implausible.

Although I agree with Professor Abell that more independent replications will be needed before all doubts about the rectitude of Gauquelin’s data have been dissolved, I notice that Gauquelin’s claims have proven resistant to debunking attempts to such a degree that, in this exceptional case, I would personally place my bets on the proponent rather than upon the critics. However, the question “What does it all mean?” remains an open one.

A major part of *The Truth* is concerned with the search for a rational explanation of the strange findings, i.e., an explanation that assumes the reliability of the data and tries to account for these without appealing to occult forces or influences. Gauquelin’s favorite guess is that the effects may be correlated with changes in geomagnetic activity caused by the Moon, Mars, Venus, Jupiter and Saturn. The foetus is presumed to be able to detect minute variations in the magnetic field and to choose to leave the maternal womb when the planet most congenial to its “genetic temperament” is rising or culminating on the horizon.

One of the major problems with this proto-theory is that it does not adequately explain why the actual distance between the Earth and the “midwife planets” does not seem to make any difference for the strength of the effects. Neither does it explain why the effects are noticed only when the planets find themselves in two of the twelve celestial sectors.

Gauquelin is very well aware of the “distance” obstacle (p. 152) but almost seems to have forgotten it when, in the very next section, he discusses the magnetic field hypothesis. To complicate matters even further, he suggests on p. 158 that the

enormous distances might account for the apparent non-existence of any Uranus, Neptune or Pluto effects.

It is not clear to me to what extent Gauquelin's "naturalistic" research programme may be expected to lead to a solution of the puzzle. To the philosophers and sociologists of science, it will be interesting to watch what Gauquelin will do if and when he comes to the conclusion that his search has failed. Will he acquiesce in the conclusion that he has discovered one more Fortean phenomenon, an inexplicable oddity in nature? Or will he finally surrender to the siren song of astrological occultism?

Traditional astrology does not explain anything, but it *did* to a certain extent anticipate Gauquelin's positive findings (although it is flatly contradicted by Gauquelin's even more numerous negative findings), and at least it provides a terminology eminently suited for concealing our ignorance. Even more important, it intuitively appeals to those who, for one reason or another, have become disenchanted with the world view of mainline science.

Michel Gauquelin is both a skeptical inquirer and a proponent of a Claim of the Paranormal. I very much wonder how he will eventually solve the identity problem resulting from this strange situation. *The Truth About Astrology* gives no unambiguous answer. Gauquelin is still too confused about his own discoveries.

Readers of *Zetetic Scholar* will be particularly interested in Gauquelin's account of his confrontation with the scientific establishments in France, Belgium and the U.S. Perhaps it is a pity that the relevant chapter, "Science and Proof," was written at a moment when the chances of an honorable peace to end the Mars-Effect war with the Committee for the Scientific Investigation of Claims of the Paranormal seemed close to zero. The remarkable "Reappraisal" by Professors Abell, Kurtz and Zelen in *The Skeptical Inquirer*¹ (which Gauquelin acknowledges in a note added at proof stage) has since resuscitated hopes of a happy ending. I understand that Michel Gauquelin's current view of CSICOP is far milder than the casual reader of *The Truth* would probably expect.

A few critical remarks: I found the penultimate chapter, "Neo-astrology' Under Attack," not entirely convincing. Gauquelin laments modern obstetric policies in the West (with Holland, I am happy to say, as the favorable exception) because "the mechanization of childbirth" may affect "the link binding us to the cosmos and the evolution of our species" (p. 175). It may be true that modern obstetric techniques destroy the neo-astrological effects, but to add in this context the warning that "to violate the laws of nature may have serious unknown consequences for the future of our descendents" is a

1 See Abell *et al.* (1983). (Eds.)

little insipid. Theologians have used this argument against every technological innovation from the invention of the wheel to the introduction of contraceptive devices.

I do not understand why Gauquelin takes for granted (p. 178) that the observation of planetary effects at birth would be a scientific revolution of Copernican magnitude. Until some sort of “explanation” is found for the neo-astrological anomalies, it will remain futile to speculate about their implications for science. An explanation in terms of “disturbances in the magnetic field” (Gauquelin’s favorite), remarkable as it might be, would probably leave the temple of orthodox science pretty much intact.

Apart from these quibbles, I found *The Truth About Astrology* an excellent book by one of the most interesting and engaging figures in modern anomalistics.

CHAPTER 5

Instead of a Postscript



CHAPTER 5

INSTEAD OF A POSTSCRIPT:

Singing Between the Stools – Remembering Piet Hein Hoebens

by EBERHARD BAUER¹

In this short contribution, I will try to explain two things to the readers of this anthology of Piet Hein Hoebens' writings: (a) how it happened that Piet Hein's (PH's) first substantial articles on the problems of scientific parapsychology were originally published in German translations, and (b) how it was that, after a relatively short period of time in the early 1980s, PH had become a highly visible and serious skeptical "counterpart" to Professor Hans Bender (1907-1991), at that time the most prominent representative of academic parapsychology in Germany. For these purposes I must provide some personal background information from a "Zeitzeuge" perspective – that is, from the perspective of a contemporary witness who was actively involved in the relevant events all those years ago.

I met PH for the first time on April 24, 1980, at the offices of the Society for Psychical Research (SPR) in London after I had attended, with my friend and colleague Walter von Lucadou, the Fourth International SPR Conference in Brighton (Bauer & Lucadou, 1980b). According to my handwritten notes that summarized that first meeting, "PHH knows our *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*; re-investigates cases of Tenhaeff; critical attitude toward Prokop." My first letter to PH was dated May 2, 1980, with his response dated May 12; his first visit to Freiburg took place on June 6 of the same year – taken together, all these events marked the beginning of an extensive personal exchange that lasted from 1980 to PH's death in the fall of 1984. It comprised the exchange of numerous letters, long phone conversations, sometimes on a weekly basis, and mutual visits to Freiburg and Amsterdam, all documented and preserved in a thick

1 Dipl.-Psych. Eberhard Bauer studied psychology, history and philosophy at the universities of Tübingen and Freiburg, Germany. As this contribution shows, he has been associated, in various roles, both with the IGPP and its founder, Prof. Hans Bender, and with the University of Freiburg. Since 1970, Bauer has been the editor of the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*. He now also is a member of the Corporate and Management Boards of the IGPP, a member of the IGPP Council and the director of both its Information and Counseling Department and its Department for Cultural and Historical Studies. (Eds.)

folder in my professional archives. In addition, there were (and still are) hundreds of copied manuscript pages, draft versions and assorted material.

At that time, in the spring of 1980, I was 36 years old, a psychologist and a member of two sister institutions in Freiburg, Germany, that both dealt with parapsychological research and counseling. These were (1) the privately-funded “Institut für Grenzgebiete der Psychologie und Psychohygiene e.V.” (IGPP) [Institute for Border Areas of Psychology and Mental Health], founded in 1950 by Prof. Hans Bender, now one of the most important institutions of its kind in the world (Bauer, 2000); and (2) the “Lehrstuhl für Psychologie und Grenzgebiete der Psychologie” [Chair for Psychology and Border Areas of Psychology] at the Psychology Department of Freiburg University, first occupied, from 1954 to 1975, by Hans Bender himself who then was followed by his disciple, Prof. Johannes Mischo (1930-2001) (Bauer, 2000-2001).²

In addition to my academic teaching and research responsibilities I was acting as the managing editor of the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, which had been founded by Bender in 1957 and which, at that time, served as the official mouthpiece of scientific parapsychology in Germany. In fact, already back in 1970, when I was still a psychology student, Professor Hans Bender, who had been one of my academic teachers in psychology, had asked me to take over that responsibility thus showing considerable confidence or trust in my still unproven editorial skills. In 1980 the *Zeitschrift* had been taken over by a new publisher, Aurum Verlag in Freiburg (Bauer, 1989). The editorial board consisted of Hans Bender, Johannes Mischo, Walter von Lucadou (then a collaborator at Mischo’s Chair), Ulrich Timm (a psychologist with a long-standing IGPP research connection who had been the managing editor of the *Zeitschrift* until 1968), and myself. The members of the editorial board used to meet at the IGPP on a more or less regular basis to review submitted papers and to plan and discuss the contents of future issues of the *Zeitschrift*.

Always on the look-out for new and promising authors for the *Zeitschrift*, I had encouraged PH, in a letter of May 19, 1980, to submit an article describing the results of his research into W. H. C. Tenhaeff’s cases and especially the so-called chair tests with Gerard Croiset – a research topic and experimental paradigm that was intimately connected with the early history of the IGPP (Bauer, 2000; Timm, 1994). My letter to PH continued, then still formally addressing him as “Sie”: “I have a lot of sympathy for your attitude of ‘zetetic doubt’, and to me Marcello Truzzi’s attempt to create an objective forum for the intellectual exchange of ideas [*Gedankenaustausch*] between ‘believers’ and

² After Mischo’s retirement in 1998, that chair was turned into a chair for “Pedagogical Psychology.”

‘disbelievers’ is the only correct way – even at the risk of having to sit between all stools” [translated from the German original].

My invitation resulted in the publication of PH’s essay review “Vom Lob der Genauigkeit in der Parapsychologie” [In Praise of Meticulousness in Parapsychology]³ in our *Zeitschrift*, critically reviewing two books by Tenhaeff that had been published shortly before, while also referring to Bender’s early work with Croiset. This publication opened Pandora’s Box, as it were, because PH’s article instigated a protracted controversy in the pages of the *Zeitschrift* about the merits (or lack thereof) of an early chair experiment with Croiset (the so-called “Pirmasens Chair Test”) that Bender had performed, in collaboration with his friend and colleague Wilhelm Tenhaeff, as long back as June of 1953. Even though Bender was not amused by PH’s critique (to put it mildly) – the consequence being that I fell from Bender’s grace for quite a long time –, he personally was sufficiently convinced of the evidential strength of the case to invite PH to visit the IGPP and to consult the archival documentation of the Pirmasens experiment (see Bender’s re-appraisal of this 1953 case: Bender, 1981b). I believe Bender also felt, understandably to some extent, a certain “obligation of honor” to defend the memory of his old friend and colleague Tenhaeff who had died on July 9, 1981, only a few months after PH’s critical essay review had appeared in the *Zeitschrift*. In his obituary Bender (1981c) remembers in warm and personal words his early connections with his Dutch colleague, but also cautiously mentions a different side to Tenhaeff’s character – his rigidity and irreconcilability regarding those he perceived as “enemies”: “Even qualified contemporaries and successors within the parapsychological scene in the Netherlands – he had not cared for a ‘crown prince’ of his own – had a hard time with him” (*ibid.*: p. 236; translation from German).

The controversy around the evidential value of the Pirmasens experiment was eventually carried through and fought out on more than fifty pages in the *Zeitschrift*: see the papers by Bender (1984a), Timm (1984b) and Hoebens’ “Abschied von Pirmasens” [Farewell to Pirmasens].⁴ In an editorial specifically written for that issue of the *Zeitschrift* (Bauer & Lucadou, 1984), Walter von Lucadou and I were able to refer to the considerable publicity and awareness that debate between PH and Professor Bender already had achieved in parapsychological circles as well as among the general public: It had formed a central element in PH’s presentation of the skeptical “Hume Game”⁵ at the combined Centenary-Jubilee Conference of the SPR and the PA in Cambridge in August of 1982

3 Reprinted, in English, as chapter 2-04 in this book. (Eds.)

4 Reprinted, in English, as chapter 3-11 in this book. (Eds.)

5 Reprinted as chapter 2-02 in this book. (Eds.)

(see the in-depth conference report by Bauer, Hövelmann & Lucadou, 1982), and it also had featured prominently in another published exchange between PH and Bender in the September 1983 issue of the popular *Psychologie heute* magazine (see Bender, 1983b, and Hoebens' "Treacherous Facts"⁶).

And more frictions were to come: Surely I must have strained Professor Bender's forbearance when I invited PH to also contribute an article in defense of "The Legitimacy of Unbelief"⁷ to a special issue of the *Zeitschrift* – that title now also forms the title of this very anthology. In fact, that special issue of the *Zeitschrift* had been dedicated to Bender's 75th birthday (Bauer & Lucadou, 1982), and in the following year a somewhat expanded version reappeared as a *Festschrift* for Hans Bender, entitled *Spektrum der Parapsychologie* (Bauer & Lucadou, 1983), which of course again included PH's critical article. I will leave it to future historians of German parapsychology (including the IGPP's intricate history) to describe in detail how precisely Professor Bender reacted to this new thorn in his flesh. At a certain point, however, he was at least able to acknowledge, depending on the audience, that PH's article was "brilliantly written" (Bender, 1983b, p. 67) and that the sophistication of PH's argumentation would not compare with the clumsiness and polemics that characterized the usual psi controversy in Germany as it was represented by militant "skeptics" such as Professor Otto Prokop and Dr. Wolf Wimmer (e.g., see Prokop & Wimmer, 1976; Wimmer, 1979, 1980⁸). Some reviewers of the Bender-*Festschrift* did not fail to reflect specifically on the inclusion of PH's article as a remarkable sign of how a "sympathizing disbeliever" should be dealt with (Mees, 1984, p. 161).

Finally, I can refer readers to a number of other public occasions, where PH adopted an active role as an intellectual "catalyst" in the German parapsychological scene: In cooperation with Gerd Hövelmann, PH had helped, in the Summer of 1983, to organize a meeting with Professor Irmgard Oepen in Marburg, in which Walter von Lucadou and I also participated. The intention was to explore the possibility of a rational "dialogue" between parapsychologists and their outside critics, and it resulted in the so-called "Marburg Manifesto" published by PH in the *Skeptical Inquirer*⁹ (see also Bauer & Lucadou, 1983-1984, on the status of the "Manifesto", and the complete story in Bauer, Hövelmann, & Lucadou, 2013: 100-109). When, in December of 1983 at Freiburg, Walter von Lucadou and I had organized the First Workshop of the "Wissenschaftliche

6 Reprinted, in English, as chapter 3-12 in this book. (Eds.)

7 Reprinted, in English, as the opening chapter (2-01) in this book. (Eds.)

8 For detailed rebuttals of the Wimmer encyclicals, see Bauer & Lucadou (1980a) and Mischo, Bauer & Lucadou (1980).

9 Reprinted as chapter 2-13 in this book. (Eds.)

Gesellschaft zur Förderung der Parapsychologie” (WGFP), a scientific SPR-like society founded in 1981 in cooperation with Mischo’s university chair, we had invited PH to give a talk on the CSICOP conference on “Science, Skepticism, and the Paranormal” in the presence of Professor Bender (Bauer, 1983).

The last time I met PH in person was on May 24, 1984, at the Eidgenössische Technische Hochschule (ETH) Zurich where we both participated – on an invitation by Paul Feyerabend – together with Professor Hans Bender in a controversial panel discussion on the scientific status of parapsychology, which was later published in a proceedings volume (Feyerabend & Thomas, 1985) on *Grenzprobleme der Wissenschaft* (for the relevant contributions, see Bauer, 1985, Bender, 1985, and Hoebens’ “Wrong Question” essay¹⁰). Walter von Lucadou, who also was present as an observer, has described the very special atmosphere prevailing at that meeting (Lucadou, 1984). My own contribution to that panel was entitled: “Gesang zwischen den Stühlen – oder wie fühlt man sich als ‘Parapsychologe’? [Song between the stools – or: How does it feel to be a “parapsychologist”?] As the present essay has shown, this was much more than a purely rhetorical question at that time.

10 Reprinted, in English, as chapter 2-03 in this book. (Eds.)

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BIBLIOGRAPHIES

Introduction

For reasons of clarity and ease of orientation, the bibliographic references for this volume are presented in three separate sections.

The *first section* is devoted to Piet Hein Hoebens' own writings. Within little more than seven years, he has written an impressive number of (often quite substantial) articles on parapsychology and various related areas in what now often is referred to as "anomalistics." We have included a complete list of all his more strictly scientific articles that were published, some of them posthumously, as journal papers or book chapters. These are supplemented by several dozen examples of his journalistic writings on the same topics. The latter, it must be understood, is only a representative selection of a far greater number of relevant journalistic texts. Of the 176 entries listed below, 44, i.e. those preceded by an asterisk (*), will be found in their original English versions or in English translations in the pages of this book.

The *second section*, "General References," lists all the other books and articles that are referred to in the various chapters of this book, including Eberhard Bauer's personal reminiscences. It also covers the literature used in the preface as well as that which the editors added in their chapter introductions or as supplementary information in various editorial footnotes throughout this book. In some of his writings Hoebens provided incomplete references. The editors have identified them all, added the details missing in the original publications and checked references and quotes for accuracy.

The *final section* provides a (presumably complete) list of the obituaries for Hoebens and of the articles or books that were dedicated to his memory.

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1978

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Illustrations

All illustrations in this book (pages 23, 165, 297, 409, 417), except for the frontispiece photograph, were painted or drawn by Piet Hein Hoebens. They all are © Hoebens Family (Liesbeth, Lodewijk and Henriëtte Hoebens).



NAME INDEX

A

Abell, George 324, 363, 365–367,
398–399, 406–407, 434
Achttert, Lutz 110
Adamsky, George 347
Adelung, J.C. 391, 434
Agpaoa, Tony 143, 385
Akers, Charles 73, 115, 141, 159, 162,
322, 325, 434
Albert, Gretl 156–157, 452
Alcock, James E. 12, 14, 20, 103–104,
114, 318, 326, 434
Allan, A.J. 206–207, 434
Allan, Alban N. 112
Allison, Dorothy 121–122, 434
Anson, Jay 313, 434
Archer, Fred 168, 174, 434
Asimov, Isaac 383

B

Baerwald, Richard 391, 434
Bainbridge, William 315, 434
Bakfark, Bálint 284
Ballard, Jim 318
Banerjee, H.N. 354–355, 357
Barker, David Read 357, 448
Bartlett, Laile E. 192, 195, 197, 434
Batcheldor, Kenneth J. 153, 434
Bateman, F. 156, 452
Bauer, Eberhard 11–12, 16, 19, 25, 55, 61,
96, 108, 111, 131–132, 135, 142, 145,

194, 261, 322, 383, 405, 411–415, 419,
434–435, 446, 456
Beauregard, Laurent 35
Beck, J.W. 9, 20, 435
Bellamy, H.S. 304, 434
Beethoven, Ludwig van 283–285, 288
Behrendt, H.B. 319
Beloff, John 5, 14, 19–20, 34–36, 39–40,
78, 80, 82, 108–109, 115, 139, 142, 145,
157, 177, 230, 317, 322–324, 332, 339,
384, 435, 450, 456
Bem, Daryl 328–329
Bender, Hans 25–26, 34, 37, 48–52,
55, 58, 63–65, 83, 108–114, 144, 155,
158, 179, 182, 184, 204, 231, 234, 244,
247, 261–289, 291–296, 350, 380–381,
411–414, 435–436
Berendsen, Anne 197
Bergengrün, Werner 293
Berglas, David 79, 87, 151
Berlitz, Charles 314–315, 323, 384, 436
Bernstein, Morey 356, 436
Bierman, Dick J. 323, 325, 333–341, 343, 436
Blackmore, Susan J. 12, 39, 73, 96, 115,
138, 144, 162, 318, 322, 437
Blake, William 94
Blavatsky, Helena 144, 348
Blitz, Antonio 374
Blumrich, J.F. 301, 303, 437
Boerenkamp, Henk 116, 437
Bok, Bart 78, 437

NAME INDEX

- Bootsman, S. 229, 261–289, 437
Boss, W. 111, 446
Bouwman, Mr. 168
Brandon, Ruth 141, 437
Breederveld, Heyme 82, 86, 159–163,
437, 456
Brink, Filippus 12, 20, 119, 175, 181,
216–218, 230, 437
Broad, Charles Dunbar 71, 149, 156, 437
Broad, William 153, 437
Broughton, Richard S. 12, 89–92, 161,
319, 437
Brown, Rosemary 105
Browning, Norma Lee 168–171, 174
Buget, P.F. 391, 437
Bunge, Mario 327, 438
Busschbach, J.G. van 245–246, 438
- C**
Cassirer, Manfred 73
Chari, C.T.K. 12, 353–362, 438
Cheetham, Erika 392, 438
Child, Irwin E. 318
Clark, Jerome 324, 385, 438
Clark, Thelma 364, 446
Christopher, Milbourne 79, 87
Churchill, Winston 272–273
Cohen, Daniel 271, 438
Collins, Harry 27–28, 31–32, 139, 438
Conan-Doyle, Arthur 104
Copernicus, Nicolaus 205
Cornell, Tony 319
Cox, Ed W. 107, 319
Crespi, Father 313
Croiset, Gerard 26, 33, 46–53, 57, 62–65,
67, 70, 78–80, 84, 86, 89, 105, 111–112,
122, 128–129, 132, 140–141, 155, 167,
177–190–193, 201–213, 215–224,
225–229, 231–241, 243–248, 250–253,
255, 257–258, 261–289, 294, 337, 348,
350, 413
Crussard, Charles 312
Curry, Patrick 363, 365, 438
Czechowsky, Ursula 110
- D**
Daalder, S. 255, 438
Däniken, Erich von 136, 299–308,
312–313, 315, 323, 438–439
Dali, Salvador 95
Darwin, Charles 9
Dessoir, Max 136–137, 381
Diaconis, Persi 324, 338, 439
Dingwall, Eric John 12, 19, 138, 144, 319,
324, 439
Ditfurth, Hoimar von 307
Dixon, Jeane 109
Dongen, H. van 347, 439
Drost, August 123
Ducasse, Curt 356, 439
Duerr, Hans Peter 19, 147, 439
Durm, M.W. 116, 452
Dykshoorn, Marinus B. 122, 167, 178,
191–202, 350, 439
- E**
Ebon, Martin 49, 264, 439
Eekhof [Police Commander] 218–219,
226–227, 246, 250–253
Eeman, Werner 321, 439, 456
Einstein, Albert 80, 205, 226
Eisenbud, Jule 64–67, 112, 229, 231–241,
248, 257, 269, 336–338, 439

NAME INDEX

Ejvegaard, Rolf 96
Ellis, David 144, 439
Ernst, Heiko 19, 291
Ertel, Suitbert 12, 20, 363, 368, 439–440
Evans, Christopher 78, 86, 111, 440
Eysenck, Hans Jürgen 112–113, 350, 365, 440

F

Faraday, Michael 78
Feininger, T. 9, 20
Feyerabend, Paul K. 19, 53, 55–56,
147–148, 415, 440
Flammarion, Camille 293, 440
Flew, Anthony 136–137, 139, 149, 440
Fontbrune, Jean Charles 389–396, 440
Forthuny, Pascal 229
Frazier, Kendrick 12, 14, 19, 87, 103,
131–134, 203, 215, 317, 331, 347, 367
Freud, Sigmund 205, 226
Fried, Lee 113, 295–296
Fritschler, Kurt 269, 440

G

Gadd, L. 114, 296, 440
Galilei, Galileo 36, 70, 226
Gardner, Martin 12, 34, 36, 85, 111, 137,
315, 384, 440
Garrett, Eileen 184
Garton, R. 147, 441
Gauger, Wilhelm 147, 441
Gauquelin, Michel 12, 323–324, 363–368,
385, 391, 393, 405–408, 441
Geisler-Werry, Werner 111, 380–381
Geller, Uri 70, 72, 75, 79, 84–85, 99, 105,
107, 110, 140, 192, 312, 319, 322, 332,
379

Gerding, J.L.F. (Hans) 116, 347–348, 439,
441
Girard, Jean-Pierre 312
Goethe, Johann Wolfgang von 300
Goldney, Kathy M. 85, 147, 155–156, 452
Goldsmith, Donald 315, 441
Gondry, Dirk 321, 441, 456
González-Quevedo, Oscar 385, 441
Goozencz, Hert 169, 171
Gorzitze, A.B. 147, 448
Greenberg, D. 192, 441
Greven, E. 194–202, 245
Griaule, Madame 301
Gruber, Elmar R. 39, 113, 295, 317, 436,
441
Gubisch, Wilhelm 184, 231, 381, 441
Gulat-Wellenburg, W. von 110, 141, 441
Gzara, Mirjam 132, 441

H

Hammerton, Max 332, 442
Hansel, C.E. Mark 12, 27, 30–33, 78,
85–86, 106, 110, 137, 153–154, 156, 185,
218, 230, 258, 325, 327, 335–336, 379,
442
Hansen, George P. 319
Haraldsson, Erlendur 96, 107
Harley, Trevor 95–96
Haskell, Peter 156–157
Hasted, John 387–388
Hebard, Arthur F. 387–388
Heilijgers, Cor 126, 128, 181, 442
Hellwig, Albert 119, 123–125, 167, 442
Hettinger, J. 265
Heyerdahl, Thor 307
Hibbard, W.S. 116, 442

NAME INDEX

Hileman, John 362, 442
Hitler, Adolf 391–392
Hodgson, William 144, 442
Hoebens, Henriëtte 18
Hoebens, Lodewijk 6, 18, 389
Hoebens-Hoedemaeker, Liesbeth 13, 18,
97, 288
Hörbiger, Hanns 384
Hövelmann, Gerd H. 9–21, 39, 55, 57, 73,
82, 87, 97–98, 108, 115–116, 131–132,
135, 141, 145, 147, 162, 198–199, 201,
291, 306, 318, 321–322, 324–325, 353–
363, 372–374, 377, 414, 435, 442–443,
456–457
Hogendoorn, W. 269, 272, 443
Hollander, Don 185–186
“Holmes, Sherlock” 117
Holzer, Hans 323
Home, Daniel Dunglas 40–41, 43, 78, 140
Honko, Lauri 147, 443
Honorton, Charles 39, 100, 110, 131, 318
Horowitz, Paul 388
Houdini, Harry 79, 151, 380
Hubbard, L. Ron 384
Hume, David 40–42, 44–45, 47–48, 53,
413, 443
Hurkos, P[i]eter 33, 45, 89, 121, 125,
167–175, 178, 191–193, 201, 348, 350,
443
Hyman, Ray 11–12, 30–31, 35, 39, 43, 46,
78, 87, 107, 139, 141, 159, 228, 318, 324,
331, 336, 349, 443, 446
Hynek, J. Allen 323

I

Inglis, Brian 318–319, 392, 444
Institoris, Henricus 83, 452

J

Jacobs, Jeff C. 13–14, 17–20, 81, 322, 444,
456
Jager, Cornelis de 20
Jerome, Lawrence 78
Johnson, Martin U. 5, 12, 20, 57, 73–74,
77–80, 90, 96–101, 105–108, 114, 137,
139, 141–142, 145, 322, 324, 332, 345,
379, 444
Jones, Jim 74
Jongsma, W. 209
Jüdt, Ingbert 299
Jung, Carl Gustav 17

K

Kammann, Richard 12, 141, 152, 268,
285, 363, 365, 368, 386, 444–445
Kant, Immanuel 348
Karr, Barry 19, 317
Kelly, Ivan 14, 20, 324, 363, 444
Kennedy, John F. 109, 391
Kennedy, Robert 391
Khomeini, Ruholla [Ayatollah] 76
Kitschenberg [Kriminal-Oberrat] 199
Klass, J. Philip 315, 366–367, 444
Klinckowstroem, Count Carl of 110, 136,
138, 141, 144, 162, 293, 367, 381, 391,
393, 441, 444
Kloosterman, Ingrid 347, 444
Knowles, Judy 312
Kobler, John 173, 444
Kornwachs, Klaus 08, 145
Krall, Karl 371–373, 444
Kramer, Gert G. 112, 434
Kramer, Wim 14, 18–19, 202, 224, 245, 444
Krippner, Stanley 12, 14, 20, 57, 131, 317,
325

NAME INDEX

Kübler-Ross, Elisabeth 104
Kuhn, Thomas S. 147
Kunneman, Harry 69
Kurtz, Paul 11–12, 14, 18–19, 74, 78, 87,
115–117, 328, 365–367, 385, 398, 407,
434, 444
Kusche, Larry 314, 444

L

Lachapelle, Sophie 378, 445
Lakatos, Imre 25, 27, 32, 80, 163, 445
Leefflang, H.A. 116, 445
Leonie, E. 391, 393, 445
LeVert, L. 391, 445
Levy, Jay 72, 147, 160–161,
Lippard, James 12, 363, 368
Louwerens, Nicky [Nicoletta] G. 223–224
Lucadou, Walter von 19, 25, 108,
131–132, 135, 142, 145, 322, 411–415,
435, 45–446
Lucas, W. 116
Luijtelaar, Maurice van 224
Lunan, Duncan 300, 306–307, 445
Lutz Family 313
Lyons, Arthur 116, 445, 456

M

M., Rony 319
MacDougall, Curtis D. 401–403, 445
Mackenzie, William 373, 443
Maday, Stefan von 372, 445
Maeterlinck, Maurice 372, 445
Maharishi Yogi 311
Marks, David 141, 152, 268, 285, 386,
445
Machete, Erik A.H. 396–399

Magritte, René 94–95
Markwick, Betty 33, 115, 141, 157, 445
Martens, Ronny 19, 321, 324, 441, 456
Mayer, Gerhard 19, 445–446
McConnell, Robert A. 364–365, 446
Medhurst, Roger G. 156, 446
M[eier], Silvio 111, 380–381
Mees, Ulrich 414, 446
Melton, J. Gordon 324, 385, 438
Metzger, Heinz 183–184, 446
Michels, Hans [J.A.G.] 9–21, 81, 322,
396, 446, 456–457
Michelson, A.A. 80
Milde, W. 9, 20, 446
Mildenberger, Florian 28, 446
Millar, Brian 14, 20, 69, 82, 97, 116, 162,
229–230, 236, 322, 339, 441, 456
Minnebo, Dirk 69
Mischo, Johannes 111, 113, 412, 436, 446
Mishlove, Jeffrey 331, 446
Mörling, Ulf 99, 107
Moore, Robin 16, 19, 255
Móricz, J. 305, 446
Morley, Edward W. 80
Morris, Robert L. 5, 12, 14, 20, 313, 322,
325–326, 447
Müller, Lutz 31, 111, 141, 377–381, 447
Mulacz, Wilhelm Peter 61–62, 68, 447
Mulder, B. [Ps.] 17
Mussolino, Benito 391

N

Navia, Luis E. 300
Neu, J.G. 116, 447
Neuhäusler, Anton 184, 447
Nolen, William A. 143, 447

NAME INDEX

Noordegraaf, J. 255, 438
North, Steven 388
Nostradamus (Michel de Notre Dame)
389–399

Nouwen, Pieter 14, 18
Nouwen, Tine 18

O

Oepen, Irmgard 19, 132, 135, 143, 414,
447
Olinger, Mrs. 65–66, 234–239
Osten, Wilhelm von 371
Osty, Eugène 229, 447
Ottervanger, P.B. 193, 216, 223
Ovendon, M. W. 301

P

Palladino, Eusapia 107
Palmer, John 12, 40, 42, 69, 74, 96, 319,
447
Panati, Charles 111, 447
Pannekoek, Leo 345
Paré, René 19, 322, 456
Parker, Adrian 96, 318, 448
Parsons, Denys 12, 19, 87
Pasteur, Louis 36
Patterson, Francine 369
Pearce, Hubert 85, 110, 140, 159
Pelz, Carl 51, 64, 216, 230, 264–265,
267–268, 272, 276, 448
Pfungst, Oskar 371, 448
Philips, Gene M. 300
Piper, Leonore 204
Pinch, Trevor J. 27–28, 31–32, 139, 438
Pinno, Dieter 110
Podmore, Frank 141, 144, 380, 448

Poirier, F. E. 147, 448
Pollack, Jack Harrison 63, 119, 179–186,
190, 204–206, 208–209, 215–217, 226,
246, 448
Popper, Karl Raimund 34, 100–101, 150
Pos, H. J. 255
Praag, Henri van 69, 77–80, 82, 105, 144,
179, 326, 344
Pratt, J. Gaither 32, 85, 157, 159, 374, 448
Pressman, Zev 387–388
Price, George R. 156, 448
Prokop, Otto 17, 28, 57, 83, 135, 137,
149, 381, 387, 411, 414, 449
Puharich, Andrija 332, 449
Puthoff, Harald 86, 386

R

Randi, James 11–12, 14, 19–20, 33, 40,
46, 78–79, 84–85, 99, 103–104, 122, 128,
137, 141, 151, 311–312, 318–319, 323,
325, 336–337, 339, 383–388, 399, 449
Randow, Thomas von 39, 317, 449
Rao, K. Ramakrishna 30, 41–42, 161,
318, 357, 437, 449
Rawlins, Dennis 324, 363–368, 385, 397,
449
Rebcke, Louis 193
Reagan, Ronald 348
Reekers, Arno 19
Reiser, M. 119–120, 449–450
Resch, Andreas 179,
Resch, Tina 344
Rhine, Joseph Banks 107, 110, 159–163,
319, 325, 374, 384, 450
Rhine, Louisa E. 450
Richards, R. J. 9, 21
Richet, Charles 190

NAME INDEX

Richter, Jonas 299
Ridpath, Ian 300–301
Rockwell, R. 322, 450
Rockwell, T. 322, 450
Rockwell, W. T. 322, 450
Rogo, D. Scott 115, 161, 388, 450
Roll, William G. 21, 344–345, 450
Rosenbusch, Hans 110, 141, 441
Rosenthal, Robert 318, 370, 451
Roosmalen, T. van 178, 213, 215–217,
230, 450
Roy, W. de 62, 450
Runge, Rüdiger 291
Rushton, William A. H. 76
Ryzl, Milan 208, 450

S

S., Annemarie [Rosenheim] 112
Sagan, Carl 306
Sai Baba 105, 107
Sanford, Terry 113, 296
Sargent, Carl 93–96, 112–113, 147, 319,
350
Sassoon, George 300
Schäfer, Herbert 16, 83, 113, 381
Schatz, Oskar 66
Schechter, Ephraim 131
Schetsche, Michael 19, 451
Schiff, Herbert 112, 434
Schmeidler, Gertrude Rachel 192
Schmidt, Helmut 36, 45, 100, 106, 326,
332, 384, 451
Schmidt, Stefan 19, 343
Schneider, Rudi 109
Schneider, Willy 109
Scholz, Heiner 113

Schouten, Sybo A. 12, 20, 90, 96, 98,
115–116, 210, 322, 451
Schriever, Friederike 325, 443
Schwartz, Bethold Eric 205, 226
Scott, Christopher 12, 33–34, 39, 50, 87,
156–157, 231, 266, 318, 451
Sebeok, Thomas A. 19, 368–370, 374–
375, 451, 454
Serios, Ted 72, 79, 84, 105, 111, 337–338
Servadio, Emilio 293, 451
Seyss-Inquart, Arthur 202
Shackleton, Basil 44, 110, 155–156, 158
Sherman, Harold 104
Shuff, Derek 225, 244
Sitchin, Zecharia 300
Smith, Gudmund W. 96
Soal, Samuel G. 32–33, 44, 72, 85, 110,
141, 147, 155–158, 160, 452
Solfvin, Jerry 345
Sommer, Theo 39, 452
Spigt, P. 256–257, 452
Sprenger, Jacobus 83, 452
Staub, Werner 231, 452
Stelter, Alfred 143, 153, 452
Stemman, Roy 178–179, 243–244, 452
Stevenson, Ian 110, 157, 353–362, 452
Stokes, Doug 104, 115, 318, 322, 452
Storey, Ronald 304–305, 312, 344, 450,
452
Stuldreher, C.J.F. 170–171
Swann, Ingo 104
Sweat, J.A. 116, 452
Swedenborg, Emanuel 174

T

Tabori, Paul 119, 121–125, 191–192, 197,
199–200, 452

NAME INDEX

- Targ, Russell 86, 386, 452
Tart, Charles T. 384
Temple, Robert 301–302, 452
Tenhaeff, Willem H.C. 17, 30, 33, 46–49, 61–69, 72, 77–78, 80, 82, 84, 86, 97, 105, 112, 119, 122, 144, 155, 177–190, 203–205, 208, 212–213, 215–224, 228, 231–241, 247–248, 250, 252–253, 255–259, 261–265, 270–271, 279–281, 285, 289, 322, 334–335, 344, 411–413, 452–453
Terentius Maurus 9
Thalbourne, Michael A. 325, 453
Thomas, Christian 55
Timm, Ulrich 25, 29, 231, 261, 266, 291, 412–413, 453–454
Tobias, A. 338, 454
Tobias, E. 387
Toulmin, Stephan 327, 454
Truzzi, Marcello 5, 10, 12, 14, 17–20, 33, 39, 46, 77–78, 82, 87, 115–129, 167, 318, 324–325, 364–365, 367, 378, 385–386, 405, 412, 445, 454, 456
Tuck, Mr. 66–67, 234, 236–238
- U**
Utts, Jessica 318
- V**
Velikovskiy, Immanuel 85, 136, 315, 323, 384
Venturi, Rita 42
Verbrugh, H. 19
Verster, J. 9, 21
Vierboom, Piet 173–174
Voltaire, François-Marie Arouet 70, 74
Vries, T. P. de 201
- W**
Wade, Nicholas 153, 437
Wälti, Bernhard 111, 446
Walther, Gerda 378, 455
West, Donald J. 39, 109, 142, 455
Wheeler, John Archibald 337
White, Rhea A. 82, 131, 450
Wiklund, Nils 96, 318, 448
Wilson, Clifford A. 305
Wilson, Colin 117, 119, 455
Wilson, Kathy 96
Willem the Silent 196–197
Wimmer, Wolf 17, 28, 36, 83–84, 108, 131, 135, 149, 162, 339, 381, 387, 414, 449, 455
Woerden, Baron Speijart von 173–174
Wolf, Gustav 373, 455
Wolman, Benjamin B. 115, 157, 455
Woodruff, Joseph 85
Worrall, W. S. 350, 455
Worrying, R. W. 116, 442
Woudenberg, G. D. H. van 210–211, 230, 455
- Z**
Zander, Helmut 356, 455
Zelen, Marvin 365–367, 407, 434, 455
Ziegler, H. E. 372
Zorab, George A. M. 63, 178, 202, 216, 220, 231, 247
Zutz, Sabine 110