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Message to our Readers

Welcome to the fourth volume of the *European Journal of Parapsychology* to be produced from Edinburgh. We are pleased to note the international nature of the journal, with authors from Scotland, Germany, Italy, Iceland, the USA, and Fiji. We welcome your interest in the *EJP* and would urge you to continue sending in papers, reviews, comments and letters for publication. The journal would not exist without *your* contributions.

Once again we are very proud to be able to provide for each article abstracts translated into six foreign languages. This would have been impossible without the generous help of the following individuals: Michel-Ange Amorim (French); Fátima Regina Machado & Wellington Zangari (Portuguese); Carlos Alvarado (Spanish); Hans Michels (Dutch); Gerd Hövelmann (German); and Massimo Biondi (Italian). We extend our sincere thanks to all these translators, who gave their time free of charge and at short notice. If others require

translation services, we would draw their attention to the Notice 'Overcoming the Language Barrier' on p.122, in which Carlos Alvarado, Gerd Hövelmann, and Hans Michels advertise their professional services as translators.

We would also like to thank all the anonymous referees whose careful considerations have improved the quality of the articles we publish. Finally, of course, we thank all the authors who have contributed their work and without whom there would be no journal.

The *European Journal of Parapsychology* is jointly produced by an Editorial Team: Caroline Watt does desktop publishing; Julie Milton is the copy editor; Deborah Delanoy and Robert Morris assist in the handling of articles; and the University of Edinburgh prints the journal. We hope that this journal will stimulate interest in parapsychology and will promote communication between parapsychologists.

Publishing and Funding Parapsychology in the Mainstream: A Mail Survey of Experiences and Strategies for Success

Julie Milton

Department of Psychology, University of Edinburgh

Abstract: The mail survey reported here was designed to obtain accurate information about the chances of parapsychological publications and grant proposals being accepted by non-parapsychological science journals and major funding bodies. All 242 members and associate members of the Parapsychological Association were sent a four-page questionnaire asking for details of the last 15 years of any such publishing and funding attempts and any experiences of being asked to referee parapsychology papers for non-parapsychology journals. They were also asked for their views on what factors might affect success rates, and what strategies parapsychologists might adopt to improve their chances.

A 30% overall response rate, representing a 44% response rate for actively publishing PA members, was achieved. The apparent acceptance rate of parapsychology papers in non-parapsychology journals was 73%, and of funding proposals by major, multidisciplinary funding institutions, 26%, although these figures are likely to be overinflated. Likelihood of success, particularly for grant applications, appeared to increase with the number of a researcher's publications. Research reviews had a particularly high apparent acceptance rate for publication of 100%. All those who had been invited by non-parapsychology journals to referee parapsychology papers believed they had been recruited via non-parapsychology contacts and information sources, none by editors monitoring the parapsychology journals. The factors affecting success rates, and strategies for improving success rates suggested by respondents are listed. The most frequently mentioned strategy was the development of increased contact with scientists in other fields.

Publication in a scientific journal is both the main way to inform other scientists about new ideas and discoveries, and a major route to achieving professional status and access to funding. However, many have doubts about the fairness and consis-

tency of the peer review process in science. This appears to be particularly the case in the biological and social sciences: the topic is taken so seriously in the medical profession that the American Medical

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part in the survey itself. An earlier version of this paper, 'Parapsychologists' access to mainstream science journals and funding: A mail survey of experiences and strategies for success', was presented at the 1993 Annual Convention of the Parapsychological Association held in Toronto, Canada.

Association have recently held two conventions devoted entirely to it.¹

Within psychology, bias in peer review has for some years been subject to empirical enquiry. One of the most well-known investigations was conducted by Peters and Ceci (1982), who took previously published research papers by authors at prestigious institutions, and resubmitted them for publication under the names of fictitious, and therefore unprestigious, authors and institutions. Eight of the nine papers were rejected, and the most likely interpretation of the evidence appears to be that reviewers are unduly influenced in their judgments of a paper by the prestige of its source. Similar research suggests the possibility of reviewer bias against papers written by authors with female names, on the part of both male and female reviewers (Goldberg, 1968; Paludi & Bauer, 1983).

Most relevant for parapsychology, however, is research that suggests that reviewers are inclined to think highly of the methodology of papers whose results confirm the reviewer's own beliefs, and poorly of those with disconfirming results. Mahoney (1977) invited reviewers to evaluate a paper on a controversial topic in behavioural psychology for a (non-existent) book on the subject. The papers had identical introduction and methodology sections, but differed in terms of whether the results confirmed or contradicted a reviewer's own position on the controversy. Reviewers rated 'confirming' papers as almost twice as high in quality of methodology and data presentation as 'disconfirming' papers; the usual recommendation for a 'confirming' paper was acceptance with moderate revision, and, for a 'disconfirming' paper, rejection or major revision.²

¹ The first International Congress on Peer Review in Biomedical Publication was held in 1989, the second in 1993: see, for example, the March 9, 1990 issue of the *Journal of the American Medical Association*, 263(10).

² It is interesting that Mahoney suggests, as a first step towards fairer peer review, that referees be asked to evaluate a paper on the basis of its introduction and methodology alone, without being shown the results or discussion. This option has, of course, been on offer to authors who

When a parapsychologist submits a paper to a general psychology or general science journal, it is likely that he or she will have to contend with a presumption against the very existence of the phenomena under study. Unlike other types of research, which are mostly reviewed entirely by specialists in the same field, parapsychological work submitted to mainstream journals appears often to be assessed by non-specialists, and non-specialist psychology reviewers appear likely to have an atypically low level of belief in paranormal phenomena. Surveys of US college professors (Wagner & Monnet, 1979) and of 'elite' members of the United States' most prestigious scientific association, the American Association for the Advancement of Science (McClenon, 1982), suggest that the scientific elite are more sceptical of the existence of psi phenomena than the scientific masses, and that psychologists are one of the most sceptical groups out of all of the scientific disciplines. Only 5% of the elite psychologists in McClenon's sample considered ESP an established fact or likely possibility, compared to roughly 56% of the social and natural science college professors in Wagner and Monnet's survey. Editors and reviewers tend to be drawn from this elite, and, if Mahoney's work is representative, the scepticism of the elite psychologists might lead them to reject a paper with results favourable to the existence of psi phenomena, when they would have accepted a paper with the same methodology but that obtained null results.

This is not necessarily an unjustified approach. Parapsychologists are familiar with the concept that 'extraordinary claims require extraordinary proof' (although see Palmer 1987), which scientists regularly apply to all types of claims they consider unlikely, not just parapsychological ones. However, whether parapsychologists view the rejection of any paper as 'fair' in this context will depend upon whether the reviewer offers a counter-explanation of

submit papers to the *European Journal of Parapsychology* since its inception (Johnson & Schouten, 1975).

any positive results that appears plausible. If this does not seem to be the case, the paper's author may suspect that the reviewer has a presumption against parapsychological phenomena so strong that it is impervious to empirical evidence.

That parapsychologists and reviewers may have different perspectives on this issue is suggested by the contrasting results of the groups surveyed by Allison (1973) and McClenon (1982). Many parapsychologists surveyed by Allison considered that scientists resist parapsychological work because the existence of psi phenomena would conflict with the world-view of those scientists, rather than because there is any serious problem with the quality of the research, whereas the elite members of the American Association for the Advancement of Science surveyed by McClenon did not attribute such reasons to themselves; they considered that an insufficient level of evidence for psi phenomena, and a lack of competence in the conduct of parapsychological work were the main barriers to science's acceptance of parapsychology.

It is not clear whether this difference in views arises purely out of the two groups requiring different standards of evidence, or whether there is, in some quarters, so absolute a position against the possibility that parapsychological phenomena exist that, even when a paper cannot be faulted in methodological terms, it will be rejected if it obtains positive results. This is difficult to determine without a Mahoney-style investigation with a methodology modified to allow a distinction between the two hypotheses.³ In the absence of such information, it is, however, worthwhile obtaining current information concerning parapsychologists' attitudes towards and experiences of the review process.

The present survey of Parapsychological Association (PA) members was designed to attempt to clarify parapsycholo-

gists' views of the current situation and to form a basis for them to improve their position, whatever it might be. Specifically, the survey aimed to examine how widespread any concern about mainstream reviewer bias still is among parapsychologists; to determine whether the low number of parapsychology papers and grants in the mainstream is due to high rejection rates or to demoralised parapsychologists not submitting their work in the first place; to ascertain how many parapsychologists are called upon to act as referees for parapsychological submissions to non-parapsychology journals, and on what basis the editors choose them; and to identify factors that appear to influence success rates, which researchers might be able to exploit to their advantage in the future.

Method

Questionnaire

A four-page questionnaire (see Appendix) was mailed, early in 1993, roughly five weeks before the stipulated reply deadline, to all 242 PA members and associate members whose current addresses were listed in the 1992 PA members' directory. A covering letter assured respondents of complete confidentiality for their answers, apart from their answers to free-response Questions 12 to 15 if they expressly gave permission for their names to be used. The questionnaire dealt with publishing and funding attempts during the last 15 years (since 1978).

Participants

Seventy-two replies (30% of all PA members) were received by the deadline. Forty-one (57%) were from the US, 17 (24%) from the UK, 12 (17%) from the rest of Europe (Belgium, France, Germany, Iceland, Italy, The Netherlands and Norway), and 1 (1%) each from Australia, Canada, China, and Israel.

Apart from the UK being over-represented (only about 10% of all PA members are from the UK), this distribution is

³ Any parapsychologist considering a such a project should bear in mind that the outrage of some participants at Mahoney's use of deception in his study led to charges of ethical misconduct and attempts to get him fired: see Peters & Ceci, 1982, p. 221.

roughly representative of PA membership as a whole. One hundred and six current PA members have published in a PA-affiliated journal or in *Research in Parapsychology* in the last 15 years, and at least 47 (44%) of them took part in this survey (seven respondents were anonymous).

Eighteen questionnaires returned after the deadline are included in the analysis of responses to free-response Questions 12 to 15 only.

Results

All analyses reported below were exploratory and unplanned.

Estimated and Actual Success Rates

Publishing: Most respondents to Question 1 felt that it was likely that proportionately fewer parapsychology than other papers are accepted by non-parapsychology journals (78%); 15% gave no answer, 4% guessed the acceptance rate to be the same, and 3% thought that parapsychology papers stood a better chance. The average acceptance rate for parapsychology papers submitted by PA members was estimated at 18% by those 45 respondents who answered Question 10. The actual average acceptance rate⁴ for those 21⁵ who indi-

cated in Question 3 that they had submitted parapsychology papers to non-parapsychology journals in the last 15 years, was 73%. (In this, and all other calculations of acceptance rates, the individual respondent was used as the unit of analysis.)

Funding: Similarly, most respondents to Question 4 thought that parapsychological research proposals have a proportionately lower chance of obtaining funding from major funding institutions that fund research in many disciplines, than other proposals (85%). Fourteen per cent gave no opinion, 1% thought that parapsychology proposals' chances were higher, and no one thought them equal. The 45 respondents who tried to estimate PA members' probable acceptance rate for such funding (Question 11) guessed an average of 8%. The actual rate of acceptance⁴, based on those 23 respondents who gave details in response to Question 6, was 26%.

Type of Research and Success Rates

Publishing: A detailed breakdown of responses to Question 3 is given in Table 1. The highest success rate was for research reviews (100%), followed by spontaneous case work (88%), theoretical/philosophical research (75%), and experimental papers (60%). One person whose survey data were consistent with the bibliography submitted historical papers, but he did not specify how many.

Funding: Responses to Question 6 are detailed in Table 2. The highest success rate was for proposals for research reviews (100%), followed by spontaneous case work (40%), experimental research (23%), theoretical/philosophical research (18%), and historical work (0%). Only the figures for theoretical/philosophical and experi-

⁴ If a respondent indicated that certain papers were purely 'anomalous' (not concerned with whether 'psi' effects really exist or what properties they might have) rather than strictly parapsychological, these data were discounted in order to produce a conservative measure.

⁵ The data of 11 respondents are excluded from analyses involving the number of parapsychology papers published in non-parapsychology journals, because the number of accepted papers that they claimed cannot be reconciled with a bibliography of such papers that I have recently compiled (Milton, in press, a, b). The discrepancies were too large in six cases (between five and twenty extra papers each) to be attributable to minor differences in definition of journal types or to the slightly different time period covered by the bibliography. These respondents may simply have misread the question and included papers in non-academic periodicals, or papers published well outside the correct timespan. In case other respondents had encountered similar problems,

another respondent who had claimed only one more paper than could be accounted for, and four respondents whose data could not be checked (three who were anonymous and one who is no longer a PA member) are similarly excluded from the relevant analyses.

Table 1

Acceptance rates of parapsychological papers for publication in non-parapsychological journals according to type of research⁴.

Type of research	Acceptance rate (%)	No. of respondents	Total papers submitted
All types	73	21	67*
Research review	100	6	8
Spontaneous case	88	4	11
Theoretical/philosophical	75	10	17
Experimental	60	10	22

*There are more papers of all types than of the categories combined because some papers did not fit any category.

Table 2

Acceptance rates of parapsychology grant proposals made to major funding institutions according to type of research.

Type of research	Acceptance rate (%)	No. of respondents	Total proposals submitted
All types	26	23	70*
Research review	100	2	2
Spontaneous case	40	2	7
Theoretical/philosophical	18	6	13
Experimental	23	18	35
Historical	0	2	2

*There are more proposals of all types than of the categories combined because some proposals did not fit any category.

mental work, however, are based on sufficient cases to be likely to be representative.

Researcher's Publishing Record and Success Rates

For respondents who had attempted to publish parapsychological research in non-parapsychology journals⁴, the more papers they had published in parapsychology journals, the more parapsychology papers they had published in non-parapsychology journals ($r_s = .593$, $N = 21$, $p < .005$, one-

tailed) and, to a lesser and non-significant extent, the more non-parapsychology papers in non-parapsychology journals ($r_s = .144$, $N = 20$)⁶. The number of publications of different types was related to respondents' acceptance rates (proportion as opposed to number of items accepted) as described below.

Publishing: The acceptance rate of parapsychological papers submitted to non-parapsychology journals⁴ was not

Table 3

Mean number of papers published by referees and non-referees^{4,6}

Type of paper	Non-Referees	Referees	Mann-Whitney $U (n_1, n_2)$	p (one-tailed)
Parapsychology papers in parapsychology journals	16.9	6.1	685.5 (56,16)	< .0007
Parapsychology papers in non-parapsychology journals	2.7	0.5	372.5 (51,10)	< .00012
Non-parapsychology papers in non-parapsychology journals	23.3	7.2	571.0 (55,15)	< .012

significantly related to the number of a respondent's parapsychology papers published in parapsychology journals ($r_s = .011$, $N = 20$)⁶ or to the number of non-parapsychology papers published in non-parapsychology journals ($r_s = -.048$, $N = 19$)⁶; however, it was correlated positively with the number of a respondent's parapsychology papers published in non-parapsychology journals ($r_s = .387$, $N = 20$, $p < .05$, one-tailed)⁶, a correlation that is likely to be at least partly artefactual.

Funding: The acceptance rate of parapsychological research proposals submitted to major funding institutions was positively related to the number of a respondent's parapsychology papers published in non-parapsychology journals⁴ ($r_s = .287$, $N = 17$, n.s.), and significantly so to the number of parapsychology papers published in parapsychology journals ($r_s = .378$, $N = 23$, $p < .05$, one-tailed), and the number of non-parapsychology papers published ($r_s = .399$, $N = 23$, $p < .05$, one-tailed).

These results suggest a generally positive, though not always strong, relationship between publication record and success rates.

Author's Country and Success Rates

It had been intended to compare success rates for publishing and funding for researchers working in different countries, but the numbers of respondents who sought publication or funding in each country were too low for such a comparison to be meaningful.

Parapsychologists as Mainstream Referees for Parapsychological Papers

Sixteen respondents (22% of the sample) had been asked by an editor of a non-parapsychological journal to act as a referee for at least one parapsychological paper. Those who were chosen had published many more papers than those who were not chosen, as summarised in Table 3.

Thirteen referees indicated, in response to Question 9, why they believed they had been chosen. None thought that it had been because the journal editor had seen their work in a parapsychology journal. Six thought they had been selected because the editor had seen their parapsychological work in a non-parapsychology journal; five, because they had previously refereed non-parapsychological work for that journal; four, because the editor had met them and knew of their interest in parapsychology; three, because a colleague had recommended them to the editor; one, because

⁶ Respondents who were not specific about the number of their publications in any given category were necessarily excluded from relevant analyses.

he was in charge of a special parapsychology section within the journal; one, because he was a friend of the editor who knew him well enough to trust his judgment; and one because he was on a shortlist of referees submitted by the author.

Reasons for Not Trying to Publish or Obtain Funding

Publishing: Twenty-four respondents who had written parapsychological papers but who had not attempted to publish any in non-parapsychology journals gave reasons for not having tried (Question 2). Fourteen thought non-parapsychology journals so hostile to parapsychology that it wasn't worth trying; eleven considered their papers too specialised to be of interest outside the field; five believed that parapsychologists should support the parapsychology journals as a priority; five were concerned that other parapsychologists would be less likely to see their work in mainstream journals; one did not want to advertise his interest in parapsychology too widely in case his career or reputation were damaged; one respondent had submitted work only to parapsychology journals because he thought them easier to get into; another, because she felt that the referees for the parapsychology journal would review her paper more knowledgeable than mainstream referees; one felt that, as a newcomer to the field, it was important to establish communication with parapsychologists first; and one respondent whose work had been funded by one of the psychological research societies that publish a journal felt honour-bound to offer the work there first⁷.

Funding: Of those 18 respondents who had wanted funding and said why they hadn't applied to major funding institu-

tions to support their parapsychology projects, 17 had not tried because they perceived the institutions as being too hostile to parapsychology to be worth the effort. Only one respondent had a different reason; he wished his research to remain independent.

Perceived Factors in and Strategies for Success

Questions 12 to 15 invited comments on what factors might affect the success of a mainstream publication or funding attempt for parapsychological research; on what strategies parapsychologists could use, individually or collectively⁸, to increase their chances of success; and on the topics covered in the questionnaire as a whole. Respondents' suggestions in the areas that emerged as the main themes are listed below. Researchers' names are given only where they gave their express permission.

(a) Personal contacts and networking

The most frequently-mentioned (by 30 respondents) factor believed likely to increase success was personal contact with scientists outside parapsychology, both to inform them about the field and to estab-

⁸ A number of proposals, based on the results of the survey and respondents' suggestions, were put to the August 14, 1993 meeting of the PA's Board of Directors. The Board agreed to have a directory compiled of specialists competent to review parapsychological submissions for use by non-parapsychology journals and major funding institutions; to encourage Program Chairs to circulate more widely the call for papers for, and announcements of PA conventions; and to encourage the setting up for the 1994 convention a panel at which outside experts would advise on ways to facilitate mainstream funding and publication of parapsychological work. In addition, an annual bibliography of PA members' recent publications will appear in future issues of *Research in Parapsychology*, for purposes that include enabling parapsychologists to monitor their publication success in non-parapsychology journals, and demonstrating to non-parapsychologists who monitor *RIP* the range of PA members' parapsychological and non-parapsychological work.

⁷ The (London) Society for Psychical Research has abandoned its former policy of requiring grant-holders to give first refusal of their research report to the SPR's own journal, because publication in any journal is seen as benefitting parapsychology as a whole (Gauld, personal communication 1993).

lish the personal credibility of parapsychologists on a face-to-face basis. Parapsychologists were recommended to:

- meet and forge links with other scientists, including attending the conventions of other disciplines and joining other professional organisations;
- initiate and engage in dialogues with others outside the field, including not letting unjustified criticism go by default; publication of papers on the sociology of disputes between parapsychologists and extreme critics might help;
- organise or take part in interdisciplinary meetings (some respondents gave examples of having done this successfully);
- hold workshops on parapsychology at mainstream conventions;
- invite other scientists, including editors and funders, to parapsychology conferences and meetings, as both listeners and speakers;
- present the problems as parapsychologists see them to editors and funders, and ask for their advice, perhaps in a special PA symposium, both for the direct benefit of having their opinion and because, having had some input into parapsychologists' efforts themselves, the decision-makers would tend to be favourably inclined towards any resulting efforts;
- lobby, via the PA, bodies such as the American Association for the Advancement of Science, American Psychological Association, British Psychological Society, etc.;
- attract more members to the PA from the outside, especially from other professional groups; the PA should advertise for members in academic journals, which would improve its currently poor name recognition even if it attracted few new members;
- extend the diffusion of parapsychology journals to the libraries of other fields;
- try to take up positions on mainstream committees, including editorial boards and funding committees that make decisions about funding parapsychological work;

- offer to be consultants and reviewers for parapsychological papers and proposals; the PA could offer to monitor any government-funded research to ensure that it is well-conducted and that no inappropriate conclusions are drawn from it.

(b) *Academic reputation*

A related factor, and one of almost equal concern (mentioned by 28 respondents), was that of the researcher's academic reputation and the prestige of the institution to which he or she belonged. Specifically, respondents felt that the researcher's reputation for his or her parapsychological work would have little impact, but the researcher's reputation for mainstream work would, particularly a good reputation in the field of a specialist journal or funding institution. Researchers who don't already have such a track record were recommended to:

- get one;
- collaborate with others who do have such a track record, including those who favour conventional explanations for parapsychological findings;
- cite in their papers or proposals high-quality parapsychological research conducted at prestigious universities;
- have larger parapsychological research groups support individual researchers seeking funding by assuring funders that necessary research facilities are in place and available.

(c) *Selection of and adaptation to journals and funding bodies*

The careful choice of where to send papers and proposals, and the tailoring of work to suit those specific institutions were issues for 16 respondents. Also, 20 respondents considered that the beliefs, open-mindedness, and even the psychological health of editors and referees were factors in whether a parapsychology paper or proposal was likely to be accepted. Non-specialist reviewers' lack of knowledge of parapsychological research methods was

also of concern. It was suggested that parapsychologists should:

- make sure that their research is relevant to the journal's or funding body's subject matter, including their theoretical concerns, terms of reference, and, especially, their current interests;
- consider the attitudes and interests of those people who make the decisions;
- announce successful publication attempts in the PA newsletter;
- use the appropriate style and language for the target institution (including the appropriate layout and format specified by each journal);
- select journals where any anti-parapsychology bias may not be so strong;
- go for the mid-level journals in which there is space to present an argument before working up to the more prestigious journals.

(d) Presentation style and tone

There were also some suggestions made concerning the style and tone likely to increase the acceptability of parapsychological research in general. Sixteen respondents thought that a sceptical stance, or one that supported an orthodox interpretation of anomalous findings would increase chances of success. Betty Markwick wrote:

In the long term, parapsychologists should work towards presenting parapsychology as a science directed to the resolution of anomalous experiences and happenings, free from any assumptions as to a psi interpretation. Individual parapsychologists may of course believe the psi hypothesis to be correct, but parapsychology itself needs to be presented as a neutral science, and to draw friendly sceptics into its ranks. Papers and research proposals issuing from such a broadened perspective would inevitably attract a more favourable response from non-parapsychological journals and funding institutions. Moreover, the new parapsychology should appeal to open-minded scientists in

other fields, and would enable parapsychology to gain a stronger foothold in academe.

Strident assertion of claims of paranormality were warned against. David Stevenson wrote:

...aggressive assertion of the actuality of "psi" may deter, suggestion that results have interesting implications which deserve further study may be more acceptable... Present findings and ideas in such a way that they will be thought about, rather than starting with assertions which you know are likely to be rejected before the paper has been read and digested... non-parapsychologists may want to be seen to be arriving at their own conclusions rather than being dragged into a new world-view by outsiders.

As well as taking care that papers were well-written, several respondents warned against using parapsychological jargon. Five respondents felt that the use of terms such as 'anomalous' rather than 'paranormal' were more likely to be acceptable, as well as perhaps more appropriate in terms of our current state of knowledge, although one respondent was against the use of such terms.

(e) Methodological quality

Eighteen respondents mentioned high methodological, conceptual or general quality of the research as a factor in success. Researchers were recommended to:

- emphasise the methodological rigour of their design;
- make the design and methodology very clear;
- set the research in a theoretical or philosophical framework wherever possible, and emphasise any links between the research and mainstream theories and concepts;
- get detailed criticism from knowledgeable parapsychologists before submitting the research in the mainstream;

- bear in mind that editors, referees and funders are generally unfamiliar with parapsychological research methods, so researchers should wherever possible anticipate and counter such criticism before it has a chance to be made.

(f) *Choice of research topic and methodology*

Apart from high methodological and intellectual quality, a number of other aspects of research were felt to be influential. Researchers were recommended to:

- choose parapsychological research topics with interesting interdisciplinary or general relevance (22 respondents), especially with relevance to disciplines of interest to the target journal or institution: the research might be a mix of parapsychological and non-parapsychological elements;
- choose research topics that are timely in terms of what is going on in mainstream science;
- choose topics of great interest to the general public;
- for funding proposals, make sure that the research is such that the funders will have got something useful even if there is no evidence of anomalous effects, and point that out to the funders;
- don't waste time on procedures that are known to be likely to give weak effect sizes; good potential results should help funding proposals;
- for funding proposals, ensure that the research will stand up to trustees' and public scrutiny and will make the funding institution look good;
- choose socially relevant topics;
- choose applied parapsychological research topics (13 respondents) such as paranormal healing, psychic detection of criminals, or topics that might have potential for application: applied research of social or military relevance, and applied research for making money, were proposed as being particularly likely to succeed;
- do anomalous psychology research, such as examining correlates of belief in the paranormal;

- concentrate on review articles and meta-analyses;

- only submit groups of thematically related studies and keep one-off experiments that haven't been replicated for the parapsychology journals;

- use exact rather than conceptual replications;

- don't use procedures that are conceptually difficult;

- use traditional statistical methods;

- do research that is limited in scope;

- include physical variables in the study, such as physiological measurements;

- stick to the most intuitively plausible forms of the paranormal, such as telepathy rather than precognition;

- concentrate on the less frightening aspects of the paranormal;

- avoid research with a high 'giggle' factor;

- base research on large databases;

- do research in groups rather than individually, especially in groups with non-parapsychologists, or at least do research in areas where many researchers are working.

(g) *General comments*

Although a few respondents were very despairing of parapsychology's chances in the mainstream, most who directly addressed the issue felt that persistence would pay. Dean Radin wrote:

It is difficult to repeatedly try for big name journals because of unfair rejection, etc. However, we have to continually push the prejudicial envelope because persistence (and high quality research) does eventually open doors.

The point was also made that initial rejection of a paper or proposal should not be taken as evidence of an anti-parapsychological bias; competition for publication and funding is fierce for all research, and mainstream referees often made good points that could improve the work before its resubmission, elsewhere if appropriate. Things were generally seen as improving,

particularly in the light of recent developments in the situation of ganzfeld research.

Discussion

The findings of the survey confirm that there are still many parapsychologists who feel that there is a strong presumption against parapsychology in the mainstream, beyond what might be justified by the principle of 'extraordinary claims require extraordinary proof'. The vast majority of respondents (78% for publishing, 85% for funding) felt that parapsychological research was more likely to be turned down than other types of research; for those who never tried to publish or fund their work in the mainstream, the most common reason for not trying was the expected hostility of their reception.

The data are more equivocal on whether these concerns about bias are justified. Acceptance rates were apparently quite high: 73% for papers, 26% for grant proposals. Respondents, however, underestimated these acceptance rates roughly by a factor of four. Are parapsychologists paranoid, or is the apparent acceptance rate, as measured in this study, highly inflated? Response bias is an obvious factor to consider here; researchers who have been unsuccessful might be embarrassed about it and not have taken part in the survey. However, at least 44% of actively publishing PA members did take part; even if we assume that the remainder had none of their papers or proposals accepted, that would still leave publication success rates at a very respectable 32%, and funding rates at 11%. A more difficult factor to gauge is that researchers may have an accurate idea (based on previous experience or, as a few respondents volunteered, informal 'sounding out' of institutions) of when rejection is certain and it is not worth trying at all; such strategies would have reduced the number of formal rejections measured here. It is also possible that careful selection of appropriate material and the most promising (perhaps less prestigious) journals and institutions meant that success rates are not representative. More

careful wording of Questions 3 and 6 might have ensured that repeated submissions were always included.

The position concerning the use of parapsychologist referees by non-parapsychological journals, on the other hand, appears much clearer. Those chosen to be referees had many more publications, both parapsychological and non-parapsychological, than non-referees. However, the comments of those chosen indicate that non-parapsychological journal editors recruited these parapsychologist referees either through personal contacts or through seeing that person's work outside the parapsychology literature. No referee thought that they had been chosen because the editor had seen their work in a parapsychology journal. The identities of the referees support the respondents' views. Some respondents, whom most parapsychologists would identify as being especially competent to spot methodological flaws, were not chosen as referees; some who were chosen have their main scientific interests in fields other than parapsychology, and have not tended to have been active in methodological debates. A proven track-record of concern with methodological issues in parapsychology does not appear to be a criterion that mainstream editors are using extensively. Personal contact, and an individual's visibility in the mainstream, appear to be more important.

Given that parapsychology has at least a toe-hold in the mainstream, but that there may be a strong presumption against the existence of psi phenomena and refereeing selection practices inspire less than confidence, what should parapsychologists do to improve the situation? Respondents have put forward many valuable suggestions, and there is no need to reiterate them here, but I think that there is a useful way for judging the relative merit of any particular suggestion.

Call up in your mind a research topic that you consider totally null and bogus, that is pursued by a small number of researchers personally unknown to you, using a methodology with which you are unfamiliar. Now imagine that you are the

editor of an important general science journal with a one in five acceptance rate, and that you have been sent a paper with positive findings on this research topic. You are now, if the views of some are correct, in the position of a mainstream journal editor who has received a parapsychology paper. What would make you accept that paper for publication? How would you choose someone to referee it?

From this perspective, we can perhaps see how tempting it is to take more seriously the work of someone well-known, or from a reputable institution, or whom we have met and who appeared personally credible. Respondents' suggestions that parapsychologists should increase their personal contacts with mainstream scientists, establish a reputation in the mainstream, employ a neutral, jargon-free tone, point out the methodological safeguards in their work, produce timely research with interdisciplinary relevance, and so on, make good sense from this point of view. Any activities that make parapsychologists appear to be a group involved in special pleading, or that involve applied research with questionable or unexplored ethical implications, look less good.

Some suggestions for success deserve further comment. In general, it seems best to begin publication and funding attempts with the type of research at present most acceptable to the mainstream, and then build upon that foundation. In terms of publications, the survey data support some respondents' opinions in suggesting that research reviews in particular have a relatively high chance of success, and may therefore be a useful route to establishing credentials. However, this finding should be treated with some caution; types of research with lower apparent success rates may simply be reflecting lower general acceptance rates of the types of journal to which they are usually submitted.

The need for methodological excellence for success was another important factor stressed by many respondents. However, mainstream researchers are generally unaware of parapsychological research findings and methods, which places the

burden on parapsychologists to demonstrate the soundness of their research every time that it is presented. For an experimental paper, for example, the author might point out that the research fulfils the latest relevant meta-analytic quality criteria, which in turn are based on decades of critical discussion of parapsychological methodology. The author might also anticipate and forestall criticism for false flaws that are regularly pointed out by non-specialists, such as the need for a 'non-ESP' control condition (Palmer, 1982). Such a strategy would help both to protect the author against a mistaken non-specialist reviewer, and to protect the journal against incorrect critical correspondence on the issue if the paper is published, as sometimes happens (e.g. Baron & Stampel, 1971). Parapsychologist reviewers could suggest that authors include such information, if they haven't already, and generally bear in mind the need to defend papers against erroneous criticism from other reviewers.

If a paper is nevertheless rejected because of a mistaken but unanticipated criticism from a non-specialist reviewer, the author can rewrite the paper, dealing explicitly with the misconception, in a way that will make it difficult for another reviewer to make the same error when the paper is resubmitted elsewhere. If non-specialist reviewers nevertheless persist in inaccurate criticism, then it would be possible to document the problem and draw attention to it, bearing in mind that parapsychology papers don't have a monopoly on being subjected to incompetent assessment. However, by no means all non-specialist referees' comments will be of this nature; many will contain useful suggestions for improving the paper. A paper that is rejected, but not fundamentally flawed, can be revised and resubmitted to another mainstream journal, working down the 'prestige ladder' if necessary. This is of course a common strategy among scientists in all disciplines; a number of respondents mentioned having been successful with it, and urged others similarly not to give up after just one rejection.

Although it is clearly desirable that parapsychologists as a group submit papers and proposals to the mainstream, a few poorly conducted submissions can harm the chances of good quality work being accepted. Anyone who is new to the field or who has not previously published their work in one of the leading parapsychology journals where specialist reviewing is likely to be at its best, owes it to themselves and their colleagues not to send work for mainstream consideration without first having their work very thoroughly checked out by more experienced researchers, preferably ones who have a special interest in methodological issues and who have experience of mainstream publication themselves. However, for those established researchers who are confident that their research represents some of the best that the field has to offer, it is important to submit papers and proposals to the mainstream in some numbers. If only a few researchers are venturing into the mainstream, it perpetuates the illusion that the field consists of a few lone eccentrics, easily dismissed. Gertrude Schmeidler, after a presentation of an earlier version of this paper, suggested that parapsychologists might from time to time concentrate their efforts on a particular mainstream institution by sending it several proposals or papers from different researchers, which would help decision-makers to get a more realistic context for the research. This is an interesting idea.

Only time will tell whether parapsychology's access to the mainstream will improve. It may help that parapsychology itself has, I believe, improved in recent years in terms of the attention paid to methodological safeguards and of the use of more revealing data analysis techniques (e.g. Hyman & Honorton, 1986). As one respondent wrote, 'I do think times are changing. I wouldn't use the results of the past to try to predict the future'.

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PUBLISHING AND FUNDING PARAPSYCHOLOGY

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Appendix

Publishing and Funding Questionnaire

1. How would you estimate the probable current publication rate of parapsychological papers submitted to non-parapsychological academic journals?
 - (a) the rate is about the same for parapsychology and non-parapsychology papers
 - (b) proportionately fewer parapsychology papers are accepted than others
 - (c) proportionately more parapsychology papers are accepted than others
2. If you have not attempted to publish any parapsychological work in non-parapsychological journals in the last 15 years (from 1978 onwards), please check the reason(s) listed below.
 - (a) I haven't written any parapsychology papers in this period
 - (b) my papers were too specialised to be of interest to non-parapsychologists
 - (c) these journals are so hostile to parapsychology that it wasn't worth trying
 - (d) parapsychologists should support the parapsychology journals and so I always sent my papers to them
 - (e) parapsychologists would be less likely to see my work if I published it in a non-parapsychological journal
 - (f) I did not want to advertise my interest in parapsychology too widely in case it damaged my career or reputation
 - (g) some other reason: please specify:
3. If you have attempted to publish any parapsychological papers in non-parapsychological journals in the last 15 years (from 1978 onwards), please indicate how many of each type were submitted and how many accepted:
 - (a) papers of all types in total
 - (b) experimental papers
 - (c) spontaneous case papers
 - (d) research review papers
 - (e) theoretical/philosophical papers
 - (f) historical papers
4. How would you estimate the probable current success rate of parapsychological research proposals submitted to major research funding institutions that fund research in many disciplines (government research councils, major charitable foundations, etc.)?
 - (a) proportionately more parapsychology proposals are accepted than others
 - (b) proportionately fewer parapsychology proposals are accepted than others
 - (c) the rate is about the same for parapsychology and non-parapsychology proposals
5. If you have not attempted to get funding for any parapsychological work from any major funding institutions in the last 15 years (from 1978 onwards), please check the reason(s) listed below.
 - (a) I haven't wanted funding for parapsychology in this period
 - (b) those institutions are so hostile to parapsychology that it wasn't worth trying
 - (c) some other reason: please specify:

6. If you have attempted to get funding for any parapsychological work from any major funding institutions in the last 15 years (from 1978 onwards), please indicate how many funding requests for each type of research were submitted and accepted:

- (a) research of all types in total
- (b) experimental research
- (c) spontaneous case research
- (d) research review work
- (e) theoretical/philosophical research
- (f) historical research

7. How many papers have you published in parapsychological journals in the last 15 years (from 1978 onwards)?

8. How many non-parapsychology papers have you published in non-parapsychology journals in the last 15 years (from 1978 onwards)?

9. How many parapsychological papers have you been asked to referee for a non-parapsychological journal?

If you have been asked, please check the reason(s) why you think that you were chosen.

- (a) I don't know
- (b) the editor had seen my parapsychological work in a parapsychology journal
- (c) the editor had seen my parapsychological work in a mainstream journal
- (d) the editor had met me and knew of my interest in parapsychology
- (e) a colleague recommended me to the editor
- (f) I had refereed non-parapsychological work for that journal previously
- (g) another reason: please specify:

10. What do you estimate is the overall acceptance rate for parapsychological papers submitted by PA members to non-parapsychological journals?

11. What do you estimate is the overall acceptance rate for research funding requests for parapsychological work submitted by PA members to major funding institutions?

12. What factors do you think might be important in determining whether a parapsychological paper gets published in a non-parapsychological journal, or whether a parapsychological research proposal to a major funding institution is successful?

13. What, if any, strategies do you think that individual parapsychologists could use to increase their chances of such publication or funding?

14. What, if anything, do you think that parapsychologists could do collectively to increase their chances of such publication or funding, including through the PA?

15. Do you have any other comments to make on the topics covered in this survey?

16. May I credit you by name in my report for your comments in reply to Questions 12, 13, 14 and 15?

17. Name (optional)

Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

Publier et financer la parapsychologie dans la ligne du courant dominant : Une enquête par courrier des expériences vécues et stratégies pour réussir.

Résumé: L'enquête par courrier rapportée ici a été mise au point afin de savoir quelles sont les chances exactes pour les publications parapsychologiques et propositions de bourses d'être acceptées par des journaux scientifiques et corps de financement majeurs non-parapsychologiques. On a envoyé à tous les 242 membres et membres associés de la Parapsychological Association un questionnaire de quatre pages demandant des détails sur les 15 dernières années de telles tentatives de publication et financement et toute expérience vécue d'avoir été sollicité pour être referee d'articles de parapsychologie pour le compte de journaux non-parapsychologiques. On leur a aussi demandé leurs vues sur quels facteurs pourraient influencer le taux de succès, et quelles stratégies les parapsychologues devraient adopter afin d'améliorer leurs chances.

On a atteint un taux de réponse global de 30%, représentant un taux de réponse de 44% pour les membres de PA publiant activement. Le taux d'acceptation apparent d'articles de parapsychologie dans des revues non-parapsychologiques a été de 73%, et de 26% pour les propositions de financement par des institutions de financement multidisciplinaires majeures, quoiqu'il soit probable que ces chiffres soient gonflés. La probabilité de réussite, en particulier pour des propositions de bourse, a semblé augmenter avec le nombre de publications du chercheur. Les comptes-rendus de la recherche ont un taux d'acceptation pour publication de 100% apparemment élevé. Tous ceux qui ont été invités par des journaux non-parapsychologiques à être referee d'articles de parapsychologie croient avoir été recrutés via des contacts et sources d'information hors parapsychologie, aucun par des éditeurs contrôlant les journaux de parapsychologie. On fait la liste des facteurs affectant les taux de succès, et les stratégies pour améliorer les taux de succès suggérés par les répondants. La stratégie la plus fréquemment mentionnée a été le développement de contacts plus nombreux avec les scientifiques d'autres domaines.

A Publicação e o Financiamento da Parapsicologia na Principal Corrente: Um Levantamento de Dados Realizado Pelo Correio Sobre Experiências e Estratégias para o Sucesso

Resumo: O levantamento de dados realizado via correio relatado aqui foi projetado para obter informações precisas sobre as oportunidades conquistadas tanto em relação a financiamentos quanto a publicações parapsicológicas aceitas pelos principais órgãos de financiamento e por periódicos científicos não-parapsicológicos. A todos os 242 membros plenos e membros associados da Parapsychological Association foi enviado um questionário de quatro páginas, com perguntas a respeito de detalhes dos últimos 15 anos sobre qualquer tentativa de publicação e financiamento e quaisquer experiências de terem sido convidados a julgar trabalhos sobre Parapsicologia para periódicos não-parapsicológicos. Perguntou-se também a opinião dos membros sobre quais fatores poderiam afetar o índice de conquistas e quais estratégias os parapsicólogos poderiam adotar para melhorarem suas possibilidades.

Obteve-se uma taxa geral de 30% de respostas, representando 44% da taxa de respostas de membros da PA que publicam ativamente. A evidente taxa de aceitação dos trabalhos sobre Parapsicologia em periódicos não-parapsicológicos foi de 73%, e de financiamentos pelas principais instituições de financiamento multidisciplinar, de 26%, apesar desses resultados serem frequentemente sobre-inflacionados. A probabilidade de sucesso, particularmente quanto aos pedidos de bolsas, pareceu estar aumentando com o número de publicações de pesquisadores. Revisões de pesquisas tiveram um aumento especialmente alto, 100%, verificado na taxa de aceitação para publicações. Todos aqueles que foram convidados por periódicos não-parapsicológicos para julgar trabalhos sobre Parapsicologia acreditaram ter sido recrutados via contatos e fontes de informações não-parapsicológicas, nenhum por editores que monitoram jornais parapsicológicos. Os fatores que afetam o sucesso das taxas e estratégias para melhorar os índices de conquistas sugeridos por aqueles que responderam ao questionário são listados. A estratégia mencionada com mais frequência foi o desenvolvimento a intensificação do contato com cientistas de outros campos.

**Publicando y Obteniendo Fondos en Parapsicología en Relación a el Establecimiento:
Una Encuesta Postal de Experiencias y Estrategias de Éxito**

Resúmen: La encuesta postal que reportamos aquí fue diseñada para obtener información precisa sobre las oportunidades de obtener aceptación de publicar artículos en revistas y obtener fondos de instituciones no-parapsicológicas. Enviamos cuestionarios de cuatro páginas a todos los 242 miembros y miembros asociados de la Parapsychological Association preguntando por detalles de los últimos 15 años de esfuerzos para publicar y obtener fondos y de experiencias de ser invitado a criticar artículos parapsicológicos para revistas no-parapsicológicas. También les preguntamos sus opiniones sobre los factores que pueden afectar la tasa de éxito en estas actividades, y cuales estrategias los parapsicólogos pueden adoptar para aumentar sus posibilidades.

Se obtuvo un 30% de respuestas, representando un 44% de los miembros de la PA que estan activos publicando. La aparente tasa de aceptación de artículos parapsicológicos en revistas no-parapsicológicas fue 73%. La tasa de becas de instituciones de gran escalas y multidisciplinarias fue 26%. Pero estas figuras posiblemente sobrestiman la situación. La posibilidad de éxito, especialmente en aplicaciones de becas, parecen aumentar con el numero de publicaciones de los investigadores. Artículos sobre revisiones de investigación obtuvieron una tasa de aceptación de 100%. Todos los que fueron invitados por revistas no-parapsicológicas a revisar artículos parapsicológicos creían que fueron reclutados a través de contactos y fuentes informativas no-parapsicológicas, pero no a través de editores que prestan atención a la literatura parapsicológica. Se presentan listas de las sugerencias de las personas que respondieron sobre los factores que afectan y que pueden aumentar las posibilidades de éxito. La estrategia mencionada más frecuentemente fue el aumento de contactos con científicos de otros campos de estudio.

Publikatie en subsidie in de parapsychologie: schriftelijke enquête over ervaringen en aanpak

Samenvatting: De hier behandelde schriftelijke enquête wilde nagaan hoe groot de kans is dat parapsychologische artikelen worden geaccepteerd door niet-parapsychologische wetenschappelijke tijdschriften en dat subsidies worden verstrekt door sponsors van buiten de parapsychologie. De 242 (full en associate) leden van de Parapsychological Association (PA) ontvingen een enquêteformulier van 4 pagina's. Dat bevatte gedetailleerde vragen over pogingen tot dergelijke publikaties en tot subsidieaanvragen over de afgelopen 15 jaar. Bovendien werd gevraagd hoe vaak men in die periode was benaderd voor het beoordelen van parapsychologische manuscripten voor niet-parapsychologische tijdschriften. Ook werd gevraagd welke factoren het succes van die activiteiten beïnvloeden en met welke strategieën de parapsycholoog zijn kans op succes zou kunnen vergroten.

Van de formulieren kwam 30% terug. Dat is 44% van de actief publicerende PA-leden. In 73% van de gevallen werden parapsychologische artikelen door niet-parapsychologische wetenschappelijk tijdschriften geaccepteerd en in 26% kreeg men subsidie van belangrijke multidisciplinaire instituten, maar beide scores lijken geflatteerd. De kans op succes, vooral bij subsidieaanvragen, leek te stijgen als een onderzoeker meer publikaties heeft afgeleverd. Onderzoeksoverzichten lijken zelfs 100% kans op publikatie te hebben. Iedereen die gevraagd was als beoordelaar op te treden geloofde dat zij door contactpersonen buiten de parapsychologie waren gekozen. Niemand dacht dat een redacteur van een parapsychologisch tijdschrift hen had gekozen. Het artikel geeft een overzicht van factoren en strategieën die de kans op publikatie of subsidie zouden kunnen vergroten. De meest genoemde aanpak was intensiever contact met wetenschappers in andere disciplines.

**Publikationsmöglichkeiten und Forschungsmittel für Parapsychologie in der
Normalwissenschaft: Eine Umfrage über Erfahrungen und Erfolgsstrategien**

Zusammenfassung: Die hier vorgestellte, schriftlich durchgeführte Umfrage sollte genaue Angaben über die Chancen für parapsychologische Publikationen in nicht-parapsychologischen Wissenschaftszeitschriften und für Anträge auf Forschungsmittel für Parapsychologie bei den

großen Förderungseinrichtungen ermitteln. Ein vierseitiger Fragebogen wurde an sämtliche 242 Vollmitglieder und assoziierten Mitglieder der Parapsychological Association versandt. Erhoben wurden für den Zeitraum der vergangenen 15 Jahre Angaben über alle versuchten Publikationen und Forschungsanträge der genannten Art, ferner über alle Anfragen von nicht-parapsychologischen Zeitschriften wegen Gutachtertätigkeiten zu eingesandten parapsychologischen Publikationsmanuskripten. Erfragt wurden außerdem Faktoren, die nach Auffassung der Befragten möglicherweise die jeweiligen Erfolgsquoten beeinflussen, sowie Strategien, die die Erfolgsaussichten von Parapsychologen verbessern könnten.

Der Fragebogen hatte eine Rücklaufquote von 30% bei einer Antwortrate von 44% von seiten aktiv publizierender Parapsychologen. Die sich ergebende Akzeptanzrate für parapsychologische Beiträge in nicht-parapsychologischen Zeitschriften war 73%, diejenige für Forschungsanträge bei den großen, multidisziplinären Förderungseinrichtungen 26%. Allerdings sind diese Quoten wahrscheinlich deutlich überhöht. Die Erfolgswahrscheinlichkeit insbesondere bei Anträgen auf Forschungsmittel schien mit der Anzahl der Publikationen des Antragstellers zu wachsen. Zur Veröffentlichung eingereichte Übersichten über Forschungsbereiche begegneten augenscheinlich einer besonders hohen Akzeptanzrate von 100%. Alle jene, die von nicht-parapsychologischen Fachzeitschriften um die Begutachtung von parapsychologischen Texten ersucht worden waren, glaubten, daß sie diese Anfragen nicht-parapsychologischen Verbindungen und Informationsquellen zu verdanken hatten, nicht etwa dem Umstand, daß die jeweiligen Herausgeber zu diesem Zweck parapsychologische Zeitschriften gesichtet hätten. Erfolgsquoten beeinflussende Faktoren sowie Strategien zur Verbesserung einschlägiger Erfolgsaussichten werden gemäß der Angaben in den Fragebogen aufgeführt. Die am häufigsten erwähnte Strategie war die Entwicklung vermehrter Kontakte mit Wissenschaftern in anderen Disziplinen.

Pubblicazioni e finanziamenti per la parapsicologia entro la cultura ufficiale. Un'indagine sulle esperienze e sulle strategie di successo

Sommario: L'inchiesta postale che viene qui riferita è stata progettata per ottenere informazioni attendibili sulle possibilità che i giornali scientifici e gli enti erogatori non parapsicologici accettino articoli di argomento parapsicologico e richieste di finanziamento per ricerche in quest'ambito. A tutti i 242 membri e associati della Parapsychological Association (PA) è stato inviato un questionario di quattro pagine che chiedeva informazioni dettagliate su simili tentativi di pubblicazione e di finanziamento compiuti negli ultimi quindici anni e sull'eventualità di essere stati contattati da riviste non parapsicologiche per compiere revisioni specialistiche di articoli parapsicologici. Il questionario chiedeva inoltre l'opinione personale degli interpellati sui fattori che potrebbero influire sul tasso di riuscita di questi tentativi e sulle strategie che i parapsicologi potrebbero adottare per aumentare le possibilità di successo.

Si è avuto un livello di risposta complessivo del 30%, che comprende un tasso del 44% relativo a quei membri della PA che pubblicano attivamente. La percentuale di accettazione dei lavori parapsicologici nelle riviste non-parapsicologiche risulta apparentemente del 73% e quella delle proposte di finanziamento, da parte dei principali enti che erogano fondi in una molteplicità di settori, del 26%: ma è possibile che tali dati siano sovrastimati. La probabilità di successo di un ricercatore, in particolare per quel che riguarda la richiesta di finanziamenti, sembra aumentare con il numero delle sue pubblicazioni. Gli articoli di rassegna sulla ricerca sembrano venir accettati per la pubblicazione a un tasso particolarmente alto, pari al 100%. Tutti coloro i quali sono stati invitati da riviste non-parapsicologiche a giudicare articoli di parapsicologia ritengono di essere stati raggiunti attraverso fonti e contatti personali non-parapsicologici, in quanto nessuno dei responsabili editoriali consultava le riviste parapsicologiche. Vengono elencati sia i fattori che a parere degli interpellati influenzano la riuscita delle richieste, sia le strategie che secondo loro migliorano le probabilità di successo. La strategia menzionata più spesso consiste nell'accrescere i contatti con scienziati di altri campi.

Remote Mental Influence of Human Electrodermal Activity: A Pilot Replication

Dean I. Radin
Robin K. Taylor
Department of Psychology
University of Edinburgh

William G. Braud
Institute of Transpersonal Psychology

Abstract: This experiment tested the hypothesis that mental intention can influence a remote person's autonomic nervous system, as measured by changes in electrodermal activity. The design was a replication of previously reported experimental work by the third author. A total of 16 pre-planned sessions were conducted with seven persons acting as mental influencers and ten acting as the remote targets of influence. Influencers were instructed to either calm or activate a remote person's electrodermal activity in randomly assigned 30 second epochs over a 32 minute session.

Overall there was less electrodermal activity in calm periods compared to activate periods, resulting in an effect size of $r = .27$ based on individual sessions as the unit of measurement, comparing favourably to a prior meta-analytic estimate of $r = .25$. A paired activate/calm analysis showed a significant tendency for electrodermal activity to be higher during activate periods and lower during calm periods ($p = .03$, one-tailed). In a post-hoc test, evidence was found that remote attention alone, independent of the assigned direction to calm or activate, tended to raise autonomic activity over baseline levels ($p = .001$).

Comparison of geomagnetic three-hourly Ap indices vs. the absolute magnitude of the results of individual sessions revealed a surprisingly strong correlation of $r = .727$ ($p = .001$).

Introduction

Over the past two decades, one of the authors (Braud) and his colleagues have conducted some 37 experiments testing the effects of mental influence on remote living systems ranging from bacteria to humans (e.g., Braud, 1993; Braud & Schlitz, 1989,

1991). Meta-analysis of these experiments reveals that the effect is replicable and relatively robust (effect size $r = .33$ based on 655 sessions). One of the target systems studied in these experiments was a remote person's autonomic nervous system, as measured by fluctuations in electrodermal activity (EDA). The EDA experiments also produced robust effects (meta-analytic effect size $r = .25$, based on 323 sessions).

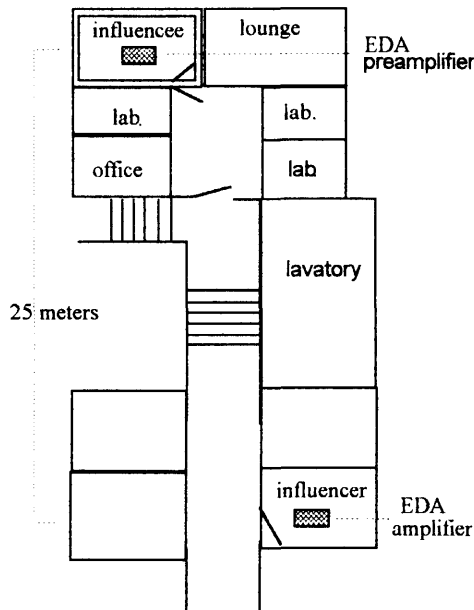
The present study was conceived as a close replication of the basic EDA experiment. To help ensure that the study was a fair and accurate replication, Braud

Note: The present address of D.I. Radin is University of Nevada, Las Vegas, Nevada, USA. The present address of R.K. Taylor is University of the South Pacific, Suva, Fiji.

Figure 1

Layout of the experimental rooms.

Electrodermal sensors were placed on the influencee's left palm; the sensor leads were connected to a preamplifier located in the influencee's room. A shielded cable ran from the preamplifier to an amplifier in the influencer's room, and the amplifier output was directed through an A/D converter into the computer that collected the data and controlled the experiment.



was invited to spend two weeks at the University of Edinburgh so we could learn the art and the science of conducting this type of experiment. In addition, Braud kindly brought along hardware to record electrodermal signals and computer software to help conduct the replication.¹ Witnessing Braud's interactions with the experimental participants proved to be invaluable in imparting important tacit knowledge that is often difficult to convey in formal experimental reports.

¹ We wish to thank Dennis DiBart, David Hurt, and Julian Isaacs for their valuable assistance with the hardware and software in this experiment.

Method

Participants

Staff members of the Koestler Chair of Parapsychology, friends of the staff, and three student volunteers from the University of Edinburgh participated as 'influencees' and 'influencers' in this study.

Laboratory facilities

The experiment was conducted in the laboratory facilities of the Koestler Chair of Parapsychology. Figure 1 shows the layout of these rooms. The 'influencee' and 'influencer' rooms for this experiment were separated by about 25 meters. The influencee's room was double-walled and acoustically insulated, attenuating airborne

sounds between the influencee and influencer rooms by a minimum of 60 db and a maximum of 100 db over the audio spectrum (50 Hz to 8000 Hz; MacKenzie, 1992).

Hardware and Software

EDA was measured by affixing two silver-silver chloride electrodes to the influencee's left palm, using a self-adhesive collar on each electrode. Conductive electrode paste² was used to improve electrical contact with the skin. The electrode leads were connected to a preamplifier. The preamplifier's output was connected by a long shielded cable to the influencer's room; this cable was connected to an amplifier; and finally the amplified output was connected to an IBM-AT computer through a 16-bit analog-to-digital (A/D) converter.³ This A/D device was set up to sample EDA values 16 times per second.

The EDA preamplifier/amplifier device ran on two 9-volt batteries and was provided by Dr Julian Isaacs. Through the use of hardware toggle switches, the device could be set to measure basal skin resistance level or galvanic skin reflex (Hassett, 1978), half-wave or full-wave rectification, and low, medium or high amplification. A software package controlled the data collection and pseudorandom assignment of activate and calm periods.¹

Procedure

The Experimenter (E) began by showing the Influencer (S, or 'sender' for short) and Influencee (R, for 'receiver') both experimental rooms. E explained that the experiment was designed to determine how well S might be able to influence R's autonomic nervous system from a distance.

R sat in a comfortable, reclining chair while E attached the EDA sensors. R was asked to remain as still as possible, but not to feel as though he or she had to remain paralyzed or immobile. E also explained to

R that the EDA measurement system was completely passive, ran on batteries, and was electrically isolated from the power mains, ensuring complete safety. E then asked R to watch a colourful display of random shapes on a computer screen⁴ and to listen to ambient music over headphones for about 35 minutes. E also explained that S, isolated in the remote room, would be asked at random times to attempt to mentally calm or activate R's electrodermal activity.

The task was presented as a joint effort involving both R and S, with both people asked to reach out to each other and 'meet halfway'. The random display was used to remind R to maintain unstructured thoughts, not striving to become either especially calm or aroused. R was asked not to try to guess when the calming or activating periods would occur, but simply to remain open to such influences. E placed the headphones on R, started up the visual display, turned off the lights, and closed the double doors of the influencee's room. E then started the audio tape and accompanied S to the influencer's room.

In the influencer's room, E immediately conducted a 90 second data collection session to measure basal skin resistance (BSR) and galvanic skin response (GSR) levels to ensure that the equipment was operating properly. If the hardware was working as expected, S was asked to sit in a comfortable reclining chair in front of a computer monitor that showed R's on-going GSR output. The monitor presented 30-second periods of GSR activity on the screen, presented as a strip-chart display. At the end of each 30-second period, the screen would clear and a new GSR sweep would begin.

In 6 of the 16 experimental sessions, one of the authors acted as both E and S; in the other 10 sessions E sat to the left of and slightly behind S during the experimental session. (Session details are presented in the results section.) During some sessions with both E and S present, E acted as 'coach', encouraging and in some cases

² *Sigma Creme*, Parker Laboratories, Orange, NJ, USA.

³ Dataq Instruments, Model DM-100, Akron, OH, USA.

⁴ Random visual displays were used from the computer 'screen saver' program called *After Dark*.

actively participating with S in the activate and calm intentions. In these sessions, E and S informally discussed the various mental strategies they were employing during the rest periods.

Calm and activate periods were interspersed by Rest periods, in which no mental intention was to be applied. The order of the periods, i.e. rest-calm-rest-activate or rest-activate-rest-calm, was pseudorandomly assigned at the beginning of the session by the data collection software. The identification of each period (a label saying *Rest*, *Calm*, or *Active*) was displayed on the screen for the duration of each 30-second period.

E suggested four strategies that S might use to try to influence R: (a) produce the desired change in himself or herself via physiological self-regulation, (b) imagine or visualize R in a situation that would produce the desired EDA change, (c) imagine that the GSR feedback on the screen matched the assigned instructions, or (d) use the GSR feedback for knowledge of results and try different mental strategies. To further enhance rapport between S and R, S was invited to wear headphones to listen to the same music being played to R. A number of Ss felt more comfortable without them. E then began the computer program to collect R's GSR.

When the data collection period was over, E and S returned to R's room and the session was over. After removing R's GSR sensors, all three people returned to S's room, and the results were statistically analyzed and discussed.

Pre-Planned and Exploratory Data Analyses

Each experimental session consisted of 64 30-second periods: 32 rest, 16 calm, and 16 activate periods. The entire experiment consisted of 16 such sessions.

Because one or both of the EDA sensors would occasionally lose contact with the skin, caused (we suspect) by a bubble in the electrode paste gel, it was necessary to establish a criterion to decide how and when a session should be aborted if the

data collection process were interrupted by a hardware problem. If such a problem did occur, it was immediately noticeable because the GSR output fell to zero and the resulting trace on the computer monitor was a flat line.

We decided that if at least one calm and one activate period had been successfully collected, the data for that session would be counted as valid. If a GSR flat line were observed for two complete sweeps of the computer screen (corresponding to 60 seconds of data collection), the session would be aborted. This meant that data for at least four 30-second periods had to be collected to count as a valid session since each calm and activate period was preceded by a rest period.

In this experiment, 4 of the 16 planned sessions developed hardware problems, but were deemed valid for analysis purposes because at least one calm/activate pair had been collected. In all four of these cases, data collection was incomplete due to problems in maintaining good electrode contact with the skin (see Table 1 later for details.)

Data Window

Several participants noted that it was psychologically difficult to shift mental gears and instantly 'turn on' their mental intention at the beginning of intention periods or to prevent themselves from relaxing their efforts before the period ended. Thus, to help reduce (a) 'linger effects' from preceding rest periods and (b) 'anticipation effects' from the end of intention periods, in all analyses presented here, rather than using data recorded in the full 30-second periods, we used data from a window consisting of the middle 20 seconds of each period.⁵ The analysis programs simply cut five seconds off from the beginning and ending of each 30-second data period.

⁵ The window size of 5 seconds was selected arbitrarily and before looking at the effect of different window sizes. Later we discuss the effect of using different window sizes.

Percentage score index

To provide a direct comparison with results from previously published studies (e.g., Braud & Schlitz, 1991), a conservative 'percentage score index' (PSI) statistic was used to summarize the results of an entire session. This statistic was calculated as follows:

$$PSI = \sum C / (\sum C + \sum A),$$

where $\sum C$ was the sum of GSR values over all calm periods, and $\sum A$ was the corresponding sum for activate periods. Under the null hypothesis, this figure would be 50%. Following Braud's previous method of analysis, a *t*-test was used to compare the 16 resulting PSI scores against chance expectation of 50%. The test was one-tailed as the hypothesis states that the total GSR activity in the 16 calm periods would be less than the total GSR activity in the 16 activate periods.

Matched-pair tests

While the PSI score is useful for summarizing an entire session, it obscures whether an overall result may be due to say, one wildly successful Activate period and near-zero activity in all of the other periods, versus a fairly consistent difference between calm and activate periods across an entire session. To examine the latter possibility, we used the Wilcoxon matched-pairs signed ranks test as an exploratory analysis where the matches were between calm and activate pairs in a given two minute testing period (Siegel & Castellan, 1988).

The per-session Wilcoxon score was in turn transformed into a Z score, and then to provide a single statistic for the overall experiment, all sessions where the planned 16 calm/activate pairs were collected were combined into a single Stouffer Z score. Sessions with fewer than 16 pairs were not used to create the Stouffer Z because Z scores based on the Wilcoxon test become

progressively less accurate as the number of matched pairs decreases.

In addition, because it was desirable to test all matched pairs without excluding any data, the number of matched pairs in all sessions was determined where calm GSR activity was less than the matched activate GSR activity. The null hypothesis was that the number of 'calm < activate' pairs would be equal to the number of 'activate < calm' pairs. The result was tested by exact binomial.

Geomagnetic effects

Finally, a preplanned analysis examined whether changes in the Earth's geomagnetic field might have been related to the remote mental influence effect, as suggested in previous studies (e.g., Braud & Dennis, 1989). The 3-hourly Ap index (Menvielle & Berthelier, 1991) corresponding to the date and time of each session was compared to the matched-pair Wilcoxon Z score resulting from each session. The 3-hour Ap index is a measure of fluctuations in the global geomagnetic field within a 3-hour window.

Results

A total of ten influencees, seven influencers, and four experimenters participated in the experiment. All participants were either laboratory staff, volunteer students from the University of Edinburgh, or visitors to the laboratory. Table 1 summarizes the participants' characteristics, various statistics, and the data associated with each session. As seen in this table, Braud assisted in all aspects of the experiment up to the sixth session. The last ten sessions were conducted by the first two authors after Braud had departed.

Example data

Figure 2 shows an example of data in this experiment. This was collected over four successive 30-second periods in the experimental session conducted on May 10 at 1:19 p.m.

REMOTE INFLUENCE OF ELECTRODERMAL ACTIVITY

Table 1
Details of experimental sessions.

Date	Time of day	Influencee	Influencer	Expt	PSI	Wilcoxon Z	C < A	N pairs	Ap
26 April	16:51	DR	WB	WB	.38	-1.00	1	1	2
27 April	12:04	staff 1	WB	DR	.58	0.93	8	16	2
27 April	17:55	DR	staff	WB	.50	-0.67	10	16	2
28 April	15:42	WB	DR	DR	.45	-0.21	8	16	2
29 April	16:33	staff 2	WB	DR	.37	-1.83	4	4	3
30 April	10:29	staff 1	WB	WB	.50	-0.26	8	16	2
3 May	16:24	RT	DR	DR	.72	1.40	6	16	3
3 May	17:24	graduate 1	RT	DR	.43	-1.45	11	16	3
4 May	16:21	staff 2	DR	DR	.55	0.45	1	2	2
5 May	11:14	staff 2	RT	DR	.42	-1.14	11	16	2
5 May	16:16	staff 2	DR	DR	.65	0.83	6	16	2
7 May	16:04	student 1	RT	DR	.39	-1.29	9	16	3
10 May	13:19	graduate 2	friend 1	DR	.43	-2.43	13	16	5
10 May	18:32	DR	friend 1	staff 1	.43	-0.83	9	16	3
11 May	10:40	student 2	student 3	DR	.38	-0.53	2	3	2
11 May	11:09	student 3	student 2	DR	.53	0.36	7	16	2

Note: DR, RT and WB are the authors. The other participants are noted as Koestler laboratory *staff*, Psychology Department *graduate* students, Edinburgh University undergraduate *student*, and laboratory *staff friend*. *PSI* is the percentage score index, *Wilcoxon* is the *Z* score resulting from the Wilcoxon test for matched activate/calm pairs; *C < A* indicates the number of matched pairs where the total activation in the calm period was less than in the activate period; *N pairs* is the total number of successfully recorded matched pairs in the session; and *Ap* is the 3-hourly Ap geomagnetic index for the session.

PSI Score

The average PSI score (plus and minus one standard deviation) for all 16 sessions was $PSI = .470 \pm .113$. A *t*-test comparing this value to the chance expected value of .50 resulted in $t = -1.071$ (15 df). The effect size, calculated as $r = \sqrt{\{t^2 / (t^2 + d.f.)\}}$, was $r = .27$, comparable to Braud and Schlitz's (1991) meta-analytic estimate of $r = .25$ for the same type of experiment, based upon 323 sessions.

We mentioned earlier that all analyses reported here are based upon truncating

the first and last 5 seconds in each period. It is instructive to see what effect the size of the data window has on effect size. Figure 3 shows how the PSI score effect size, calculated as above, changed as the data window size was varied from 0 (all 30 seconds of data used per period) to 7 (middle 16 seconds of data used). The graph suggests that the mental intention effect took a few seconds to build 'strength', or possibly that the influencer required a few seconds to clearly focus on the appropriate mental intention, or perhaps that a combination of both took place.

Figure 2

Example of data collected in four successive 30-second periods in session on May 10 at 1:19p.m. The ordinate is average CSR activity per second expressed in terms of volts recorded by the CSR instrument.

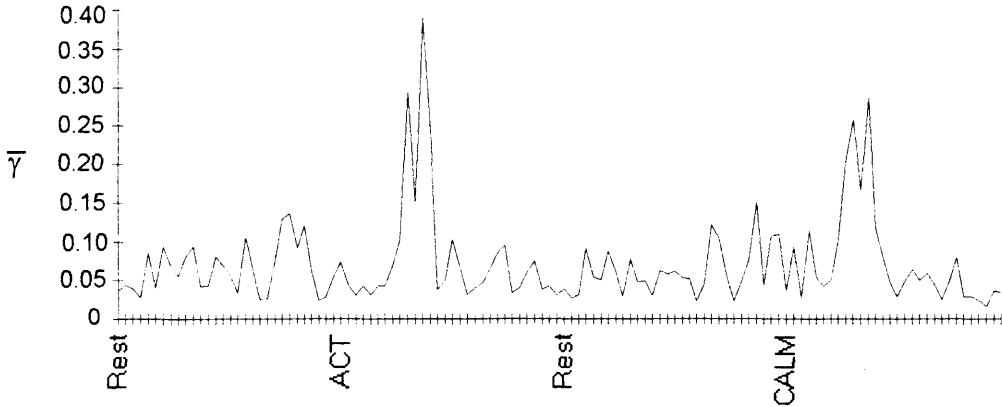


Figure 3

Effect size changes as different data window sizes are employed.

A window size of 0 means that all 30 seconds were used in the analysis. A window size of 5 means that the middle 20 seconds were used in the analysis.

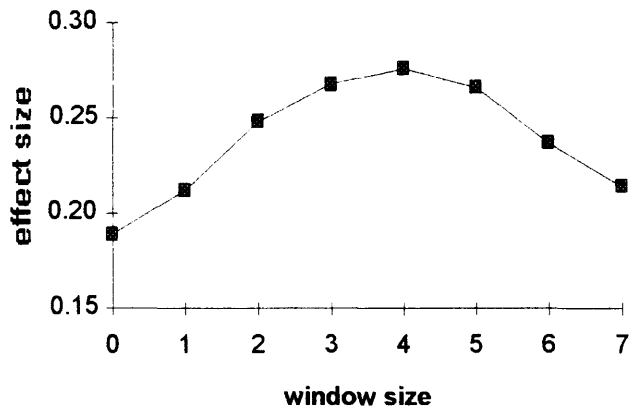
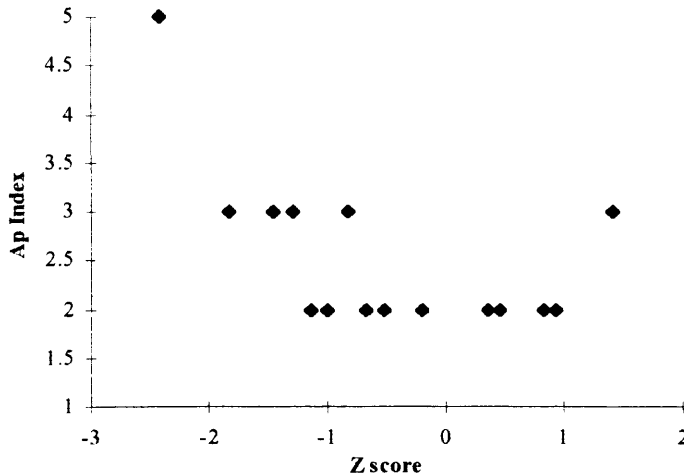


Figure 4

Planetary geomagnetic 3-hourly Ap index vs. the Z scores resulting from the Wilcoxon test for all 16 experimental sessions.



Matched Pair Analyses

The Stouffer Z score resulting from the 12 (full session) Wilcoxon Z scores was $Z_s = -1.373$, $p = .085$. The effect size for the Wilcoxon, calculated as $r = Z_s / \sqrt{N}$, where $N = 12$, resulted in $r = .396$. The Wilcoxon test provides slightly greater statistical power than the t -test of the PSI scores because it is based upon matched calm/activate pairs throughout each session rather than on a single summary statistic for an entire session.

The number of matched pairs in which calm GSR was less than activate GSR was 114 out of a total of 202 matched pairs, where 101 would be expected by chance. This results in an exact binomial $p = .029$, suggesting that the mental influence effect appeared uniformly throughout the experimental sessions rather than in one or two exceptionally activated or calm periods in each session.

Geomagnetic Effects

Figure 4 shows geomagnetic Ap index (3 hourly value corresponding to the time

of each experimental session) vs. the Wilcoxon Z score for each session. While the total number of points in this curve is small ($N = 16$), the apparent U-shaped curve suggests that psi effects in this experiment, independent of intended direction, may be related to geomagnetic fluctuations.⁶

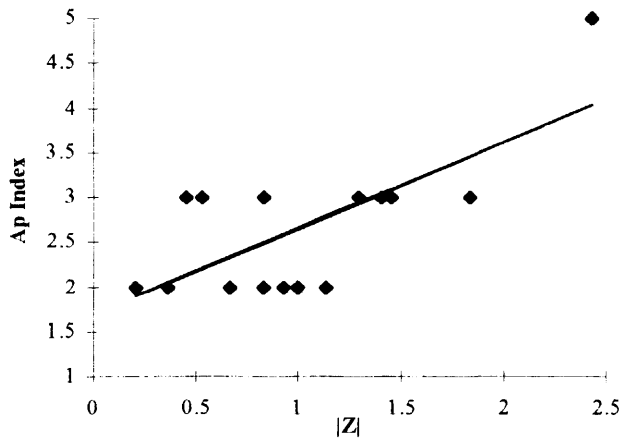
Note that the Wilcoxon Z scores are not measures of absolute autonomic activation, but of the *difference* between activation in matched calm and activate periods. Thus the relationship graphed below shows a possible relationship with remote mental intention and is not simply showing the nervous system responding to fluctuations in the geomagnetic field.

To more closely examine how geomagnetic fluctuations may be related to the mental intention effect, the absolute values of the Wilcoxon Z scores were plotted against the Ap indices. The result is shown in Figure 5. A linear regression on this data results in a correlation of $r = .727$

⁶ Two points in this curve coincide, making it appear as though there are only 15 points.

Figure 5

Planetary geomagnetic 3 hourly Ap index vs. the absolute value of the Z scores resulting from the Wilcoxon test for all 16 experimental sessions.



($p = .001$, 15 df).⁷ The strength of this correlation must be weighed against the small number of points in the curve; nevertheless the general trend suggests that fluctuations in the planetary geomagnetic field may indeed be a potent modulator of mental intention effects.

Testing for Condition Randomness and Activation Drift

The statistical measures used in this study rely on two important assumptions. First, it is assumed that the order of the calm/activate (CA) and activate/calm (AC) conditions is adequately random, and second, it is assumed that there is no systematic drift in the activation levels over a session. If the first and second assumptions are false, then the Wilcoxon, or more generally the matched-pair tests, can produce spuriously significant scores.

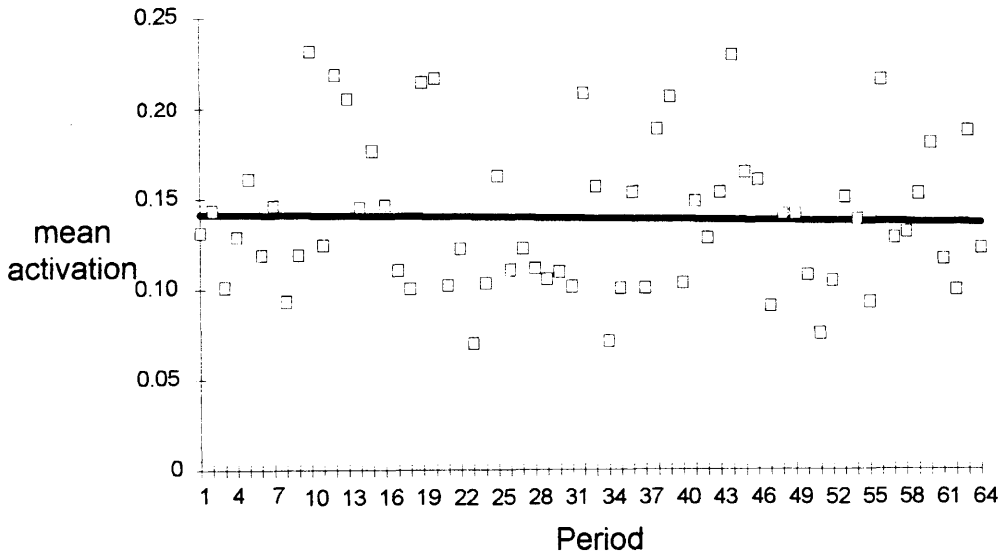
For example, say that GSR activity values tended to drift towards zero over the course of a session, reflecting participants' increasing basal skin resistance as they become progressively more relaxed while sitting for 32 minutes in a dark, quiet room in a comfortable chair. Further assume that there was an excess number of AC assigned conditions (i.e., higher followed by lower levels of activation) over the course of the experiment. If these two assumptions were true, then an excess number of matches between measured GSR levels and the assigned activate vs. calm conditions could be due to ordinary downward drifts in GSR levels rather than to supposed remote mental influences.

To test the randomness assumption, all 202 pairs of CA and AC conditions were examined to see how many of each order were used. This resulted in 110 CA conditions and 92 AC conditions, $\chi^2(1) = 1.604$, $p = .205$. Thus, condition orders were adequately random. However, we still do not know whether significant numbers of matches in GSR levels vs. assigned

⁷ This regression used all 16 Wilcoxon Z scores rather than just using Z scores from the 12 complete sessions. To demonstrate that the correlation using all data was not spurious, the same analysis was performed using only Z scores from the 12 complete sessions. This resulted in $r = .869$.

Figure 6

Mean activation levels for each of the 64 periods per experimental session, for all data combined. The best-fit linear regression results in a slope = $-.00008$, $r = -.037$, $p = .774$, revealing no consistent drift of activation levels over the course of the experimental sessions.



conditions might have been due to GSR drift.

To examine the 'drift hypothesis' more closely, the assigned conditions were split into CA vs. AC orders to see how many times under these two conditions activate GSR activity was greater than calm GSR activity (call this a 'success'). In the 110 times that the CA order was assigned, 58% of the times (i.e., 64 pairs) were successful, leading to an exact binomial $p = .035$. In the 92 times that the AC order was assigned, 54% of the times (50 pairs) were successful, leading to an exact binomial $p = .174$. These results counter a drift hypothesis because greater significance was achieved in the CA order (which would have required drifting from lower to higher activation levels).

Another way of looking for drifts in activation levels is to examine the mean activation levels of each of the 64 30-second periods for all collected data. Figure 6 shows that there was essentially no activation drift. Thus, the assumptions of the above statistical tests appear to be valid and

the results are not artifacts of inadequate randomness or GSR drift.

Discussion

Variety of Conditions

Although this experiment was conceived as a replication of the original studies, the first eight sessions were used to explore a wide variety of different displays for the influencees. We tried to create visual and auditory displays that would be not only sufficiently engaging to keep the influencees awake, but also not so arousing that their EDAs would be stressed. This proved to be more difficult to achieve than it may sound.

After experimenting with a wide variety of conditions, by the ninth session we had settled on a computer screen saver program for the visual display and a tape of pleasant synthesized music for the audio

display.⁸ In the last four sessions, we also positioned a weak bar magnet over the influencee's head to informally explore the effects of static magnetic fields on the experimental outcome. The rationales for using a magnet in these circumstances were that (a) geomagnetic fields seem to play a modulating role in these phenomena, and (b) there is a tradition in the Tibetan meditation literature that suggests that even static magnetic fields may cause changes in internal experience, and this has been confirmed by formal experiments (Green, Parks, Guyer, Fahrion & Coyne, 1991). So far, we have not attempted any formal comparison studies or other analyses related to the presence of a magnet.

Mental Intention Strategies

On occasion, we noticed that the influencer tried a mental strategy that seemed to cause a real-time EDA response in the influencee. Some of these time-correspondences were quite dramatic, giving the appearance that the influencer and influencee were in direct (albeit remote) contact with each other. At other times, no obvious differences were observed between calm and activate periods and statistical evaluation was required to see if the experimental sessions produced the desired outcome.

In the last few sessions, we decided to ask the influencees to name some situations that would calm or arouse them. We speculated that these situations could then be used by influencers for remote influence by 'projecting' these personalized situations 'into' the influencees. We subjectively feel this method worked to some degree, but discovered that people either do not readily volunteer what really arouses them, or they just don't think of situations on the spur of the moment that are exceptionally calming or arousing. Also, we noticed some confusion over what is *emotionally* calming or arousing vs. *physically* calming or arousing.

In future studies, it may be worthwhile to give the influencee a checklist of 20 or 30 typically calming and arousing situations, to spark their memory of what 'pushes' them emotionally, and to get a quick survey of high and low emotional situations that the influencer may wish to use as mental strategies. It may be that what calms or arouses an influencer is completely different from what calms or arouses the influencee. A conflict between the two people's assumptions is probably not conducive to good experimental results.

Effect of Attention

We noticed during some sessions that simply focusing one's attention on the remote influencee seemed to cause arousal, independent of whether the assigned mental intention was to calm or to activate. This suggests that even if one knows what thoughts or ideas will emotionally calm the influencee, placing too much focused *attention* on the person may arouse them instead.

Figure 7 shows a plot of the cumulative sum of activation in calm vs. rest periods and the cumulative sum of activation in activate vs. rest periods across all 16 sessions. The *y*-axis in Figure 7 is calculated (for calm, for example) as:

$$\delta = (\Sigma \text{calm} / \frac{\Sigma \text{rest}}{2}) - 1$$

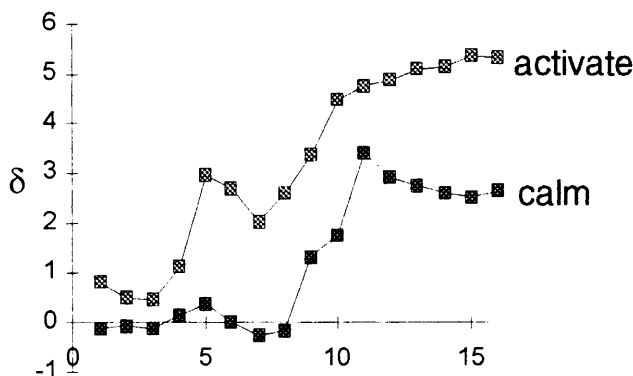
Under the null hypothesis, this value should approach zero because the total activation in calm periods should be equal to the total activation in (rest/2) periods. The fact that both the calm and activate graphs in Figure 7 increase suggests that remote attention alone arouses the autonomic sympathetic nervous system.

⁸ This ambient music tape was selected and assembled by William Braud.

Figure 7

Cumulative plot of the sum of activation in calm vs. rest and activate vs. rest periods.

This suggests that remote attention alone may be sufficient to arouse the autonomic nervous system. The y-axis is explained in the text.



This can be examined another way:⁹ within each group of four 30-second periods, we can calculate Wilcoxon matched-pair scores where one score is [$\Sigma \text{calm} + \Sigma \text{activate}$], called say, *Attention*, and the other is [$\Sigma \text{rest 1} + \Sigma \text{rest 2}$], called *Rest*. If the *Attention* condition tends to produce consistently higher activation levels than the *Rest* condition, then if we cumulate the Z scores resulting from the Wilcoxon tests, we should end up with a large Stouffer Z score.

In fact this analysis resulted in a Stouffer score of $Z = -3.03$, $p = .001$ ($N = 12$), suggesting that overall GSR activation during the calm/activate mental intention periods was higher than during the rest periods.

Influencees' Subjective Experiences

After each session we discussed with the influencee what sort of subjective experiences they may have felt. Most participants reported that at times they felt surges of activation or felt unexpectedly

sleepy. These feelings were reported as 'becoming aware of jittery feelings in the chest', 'an increased heart rate', and 'uncontrollable sleepiness'. One possibility is that sending arousing or calming thoughts may activate or calm many components of the nervous system, or it may be that many of the senders in this experiment were simply inexperienced at 'targeting' specific parts of the nervous system. This can be tested by monitoring several physiological signals simultaneously and seeing if EDA and heart rate, say, track each other and also match influencees' subjective experiences. Another approach would be to give influencees a button to press when they think they are being influenced, then comparing these subjective (and consciously aware) times against the influencer's assigned mental intention instructions.

There are, however, certain possible problems with such a dual responding method; for example, a button press could itself induce an artifactual autonomic reaction, and conscious guessing could lead to distortions of the 'unconscious' autonomic indicators.

⁹ We thank Prof. Robert Morris for suggesting this test.

Conclusion

A pilot replication of an experiment studying mental influence of a remote person's electrodermal activity resulted in an effect size close to that reported in a large body of previous studies. Given that a number of different conditions were explored during the experiment, the present results suggest that the effect is rather robust.

A matched-pairs test showed that differences in a remote person's autonomic activity significantly corresponded to the mental intention of the influencer. Additional evidence suggested that the mere act of directing attention to a remote person was sufficient to raise their remote electrodermal activity. A significant relationship between fluctuations in the planetary geomagnetic field and the mental intention effect was also observed.

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Dean I. Radin
University of Nevada
Las Vegas
Nevada, USA

Robin K. Taylor
University of the South Pacific
Suva, FIJI

William G. Braud
Institute of Transpersonal Psychology
744 San Antonio Road
Palo Alto, CA94303
USA

Influence mentale à distance de l'activité électrodermale: Une réplication pilote

Résumé: Cette expérience a testé l'hypothèse que l'intention mentale peut influencer le système nerveux autonome d'une personne éloignée, tel que mesuré par les changements dans l'activité électrodermale. Le plan expérimental était une réplication du travail expérimental déjà rapporté par le troisième auteur. Un total de 16 sessions pré-planifiées a été conduit avec sept personnes agissant comme influenceurs mentaux et dix agissant comme cibles de l'influence éloignées. On a demandé aux influenceurs soit de calmer soit d'augmenter l'activité électrodermale d'une personne éloignée pendant des durées de 30 secondes choisies au hasard à l'intérieur d'une session de 32 minutes.

Globalement il y a eu moins d'activité électrodermale dans les périodes "calmer" comparé aux périodes "activer", résultant en un effet de taille $r = .27$ basée sur les sessions individuelles comme unité de mesure, comparable à une estimation de méta-analyse antérieure de $r = .25$. Une analyse appariée activer/calmer a montré une tendance significative de l'activité électrodermale à être plus élevée durant les périodes "activer" et plus faible durant les périodes "calmer" ($p = .03$, unilatéral). Dans un test post-hoc, il est apparu que l'attention éloignée seule, indépendamment de la direction assignée de calmer ou d'activer, tendait à augmenter l'activité autonome au-dessus du niveau de base ($p = .001$).

La comparaison des indices de l'activité géomagnétique sur 3 heures avec la magnitude absolue des résultats des sessions individuelles a révélé une étonnamment forte corrélation de $r = .727$ ($p = .001$).

Influências mentais à distância sobre a atividade eletro-dérmica humana: uma replicação piloto

Resumo: Este experimento colocou à prova a hipótese de que a intenção mental pode influenciar o sistema nervoso autônomo de uma pessoa localizada à distância utilizando-se como medida as mudanças na atividade eletro-dérmica. O projeto foi uma replicação do trabalho experimental relatado anteriormente pelo terceiro autor. Realizou-se um total de dezesseis sessões planejadas anteriormente com sete pessoas que atuaram como agentes de influência mental e com dez que atuaram como objetivos de influência a distancia. Foi pedido aos agentes de influência mental que acalmassem ou ativassem a atividade eletro-dérmica de uma pessoa em um local distante em períodos de 30 segundos assinalados aleatoriamente ao longo de uma sessão de 32 minutos.

Em geral, encontrou-se menor atividade eletro-dérmica nos períodos de calma comparados aos períodos de ativação, os quais obtiveram uma magnitude do efeito de $r = .27$ tomando-se por base as sessões individuais como unidade de medida. Isso se compara favoravelmente com uma estimativa meta-analítica anterior de $r = .25$. Uma análise da combinação das condições de ativação e de calma mostrou uma tendência significativa para uma maior atividade eletro-dérmica durante os períodos de ativação e menor durante os períodos de calma ($p = .03$, mono-caudal). Em um teste *post hoc* encontrou-se evidência de que a atenção remota aumentou os níveis de atividade autônoma em relação às medidas de base independentemente das instruções de acalmar ou ativar o sujeito ($p = .001$).

Encontrou-se também uma forte correlação quando foram comparados os índices geomagnéticos A_p de três horas com a magnitude absoluta dos resultados das sessões individuais ($r = .727$, $p = .001$).

Influencias Mentales Remotas Sobre la Actividad Electrodermal Humana: Una Replicación Piloto

Resúmen: Este experimento puso a prueba la hipótesis de que la intención mental puede influenciar el sistema nervioso autónomo de una persona remota, según este es medido a través de cambios en la actividad electrodermal. El diseño fue una replicación de trabajo experimental

reportado anteriormente por el tercer autor. Se llevaron a cabo un total de 16 sesiones planeadas con anticipación con siete personas que actuaron como agentes de influencia mental y con diez que actuaron como los objetivos de influencia remota. Se le pidió a los agentes de influencia mental que calmaran o que activaran la actividad electrodermal de una persona en una localización remota en periodos de 30 segundos asignados aleatoriamente a través de una sesión de 32 minutos.

En general se encontró menos actividad electrodermal en los periodos de calma comparados con los periodos de activación, lo cual obtuvo una magnitud del efecto de $r = .27$ basada en sesiones individuales como la unidad de medida. Esto se compara favorablemente con una estimación meta-analítica anterior de $r = .25$. Un análisis de la combinación de las condiciones de activación y de calma mostró una tendencia significativa hacia una mayor actividad electrodermal durante los periodos de activación y menor durante los periodos de calma ($p = .03$, 1 cola). En un análisis post hoc se encontró evidencia de que la atención remota subió los niveles de actividad autonómica sobre las medidas de base independientemente de las instrucciones de calmar o activar al sujeto ($p = .001$).

También se encontró una fuerte correlación cuando se compararon los índices geomagnéticos Ap de tres horas con la magnitud absoluta de los resultados, de las sesiones individuales ($r = .727$, $p = .001$).

Op afstand mentaal beïnvloeden van huidactiviteit: een pilot replicatie

Samenvatting: Dit experiment toetst de hypothese dat mentale intenties het autonome zenuwstelsel van een persoon op afstand kunnen beïnvloeden, gemeten op basis van veranderingen in de huidweerstand. De opzet was zoals in eerder door de derde auteur gepubliceerd onderzoek. In totaal werden 16 vooraf geplande sessies uitgevoerd, waarin 7 proefpersonen hun mentale beïnvloeding moesten gebruiken en 10 anderen hun doelwit waren. De 7 beïnvloedende proefpersonen kregen de instructie de elders verblijvende persoon te kalmeren, dan wel te activeren. Zij moesten dit in een sessie van 32 minuten op willekeurig bepaalde momenten steeds gedurende 30 seconden doen.

De gecombineerde resultaten toonden minder huidactiviteit tijdens de kalmering dan tijdens de activering. De effectgrootte was $r = 0.27$ en was gebaseerd op afzonderlijke sessies als meeteenheid. Dit wijkt nauwelijks af van de op grond van een eerdere meta-analyse geschatte $r = 0.25$. Een gepaarde kalm/actief-analyse resulteerde in een significante tendens tot meer huidactiviteit bij activeringsperioden en minder bij kalmeringsperioden ($p = .03$). Een post-hoc onderzoek bewees dat mentale beïnvloeding op zich, onafhankelijk van de opdracht tot kalmeren of activeren, de autonome activiteit al tot boven de normale waarde verhoogde ($p = 0.001$).

De auteurs vonden een verrassend sterke correlatie van $r = 0.727$ ($p = 0.001$) tussen drie-uurs geomagnetische Ap-indices en de absolute score in afzonderlijke sessies.

Mentale Fernbeeinflussung des elektrischen Hautwiderstandes des Menschen: Eine erste Replikation

Zusammenfassung: Dieses Experiment untersuchte die Hypothese, daß mentale Absichten, wie sich an veränderten Meßwerten des elektrischen Hautwiderstandes ablesen läßt, das autonome Nervensystem räumlich entfernter Personen beeinflussen können. Die Versuchsanordnung replizierte jene aus früheren experimentellen Untersuchungen des dritten Autors. Eine im voraus festgelegte Anzahl von 16 Versuchsdurchgängen wurde durchgeführt, in denen sieben Probanden als gedankliche Einflußnehmer, zehn weitere als räumlich entfernte Einflußziele agierten. Die Einflußnehmer wurden angewiesen, in Zeiträumen von 30 Sekunden im Laufe eines Durchgangs von 32 Minuten, die nach einem Zufallsverfahren zugeteilt wurden, den elektrischen Hautwiderstand der entfernten Personen zu senken oder zu erhöhen.

Insgesamt war der elektrische Hauptwiderstand in den Absenkungsphasen geringer als in den Steigerungsphasen, was sich in einer Effektgröße von $r = .27$ ausdrückt, einem Wert, der auf individuelle Sessions als Meßzeiträume zurückgeht und der neben einer im Vorhinein auf Grund einer Meta-Analyse ermittelten Effektengrößen-Schätzung $r = .25$ recht gut dasteht. Eine vergleichende Analyse ergab eine signifikante Tendenz: In Steigerungs-Phasen wurde ein höherer elektrischer Hautwiderstand gemessen als in Senkungs-Phasen ($p = .03$, zweiseitig). In einem Post-hoc-Test, fanden sich Hinweise dafür, daß die gerichtete Aufmerksamkeit alleine, unabhängig von der Instruktionsrichtung, bereits eine Tendenz zeigt, autonome Aktivität über Baseline-Niveau hinaus zu steigern ($p = .001$).

Ein Vergleich dreistündlich ermittelter geomagnetischer Ap-Indizes mit den absoluten Größen aus Einzel-Sessions ergab eine überraschend starke Korrelation von $r = .727$ ($p = .001$).

Influenza mentale a distanza sull'attività elettrica cutanea. Una replicazione pilota

Sommario: Questo studio, basato sulle misurazioni dei cambiamenti di attività elettrodermica, ha esaminato l'ipotesi che si possa influenzare con il solo atto mentale il sistema nervoso autonomo di un'altra persona. La procedura consisteva nella replica di uno studio sperimentale già pubblicato dal terzo degli autori che firmano il presente articolo. In totale sono state svolte 16 sessioni preordinate, con 7 individui che hanno agito quali "influenzatori" mentali e 10 che hanno avuto il ruolo di bersagli a distanza. Agli influenzatori veniva assegnato il compito di deprimere o attivare l'attività elettrodermica di un individuo lontano, per periodi di 30 secondi fissati *random*, per una durata complessiva della sessione di 32 minuti.

In totale, considerando come unità di misura le singole sessioni, si è avuta un'attività elettrodermica minore nei periodi di depressione che nei periodi di attivazione, con un effetto finale $r=0,27$, che si rapporta favorevolmente a una precedente stima meta-analitica di $r=0,25$. Un'analisi delle coppie depressione/attivazione ha mostrato una tendenza significativa a un'attività elettrodermica superiore durante i periodi di attivazione, e a una attività inferiore durante i periodi di depressione ($p=0,03$, a una coda). In un'analisi *post-hoc* sono state trovate indicazioni che la semplice attenzione verso bersagli lontani, indipendentemente dal senso prefissato di depressione o di attivazione, tendeva ad innalzare l'attività del sistema nervoso autonomo oltre i livelli di base ($p=0,001$).

Confrontando gli indici Ap del campo geomagnetico, per segmenti di 3 ore, con l'ampiezza assoluta dei risultati delle singole sessioni si è trovata una correlazione sorprendentemente elevata di $r=0,727$ ($p=0,001$).

A Proposed Research Paradigm for the Study of Volitional Mentation

Loftur R. Gissurarson
Regional Office for the Affairs of the
Disabled, Reykjavik

Robert L. Morris
University of Edinburgh

Abstract: This paper reports four studies that were conducted at the University of Edinburgh in order to explore the possibility of operationalizing concepts related to volition by adopting an experimental design from parapsychology. The first study suggested that the time subjects spend on a volitional task remains unchanged from one occasion to another under identical conditions. The second study suggested that when confronted by a volitional task most subjects try out various mental strategies, most of which are imagery based, in their attempts to be successful on the task. The third and fourth studies suggested that different assigned volitional imagery strategies take different amounts of time to perform and such differences are affected by whether subjects are provided with feedback of how they are doing on the task. The possible implications of these findings for research into volition are discussed.

Introduction

Psychology in its early days concerned itself with concepts of mental life that were taken from philosophical traditions and from the assumptions inherent in everyday language. Often such concepts proved unadaptable to rigorous experimental approaches and were gradually discarded. In recent years, partly through the development of methods within cognitive psychology, some of these concepts, such as imagery, have been returning to the experimental scene. Volition, like imagery, has been a prominent concept both within the philosophy of mind and our ordinary language, and is regarded by lay people to be well within the domain of psychology. The main thesis of this paper is, however, that very little is known about volition in experimental psychology, and that parapsychological research protocols may provide an effective opportunity to do research on volitional mentation itself.

Volition, according to the *Collins Dictionary of the English Language*, is the 'act of exercising the will' (Hanks, 1985,

p.1624). Volitions have been postulated as special acts or operations 'in the mind' and are commonly thought to be important for initiating various aspects of human behaviour. Lay people readily talk about individual differences in volitional strength or willpower, with reference to task accomplishment, habit management and so on. Yet volition is rarely indexed in psychology texts and only mentioned informally, if at all.

Historically, the concepts of volition and will have fallen within the domain of philosophy (e.g. Nietzsche, 1923; Schopenhauer, 1859). Following the birth of experimental psychology in Germany around 1880, some of the first research attempts to tackle will-based processes were those by Ach (1905) and Michotte (1910). Although the early studies often employed innovative experimental designs, the methodology used was by and large introspective analysis, and was thus increasingly seen as unacceptable by proponents of the growing behaviourist movement (for a historical background on volition in psychology see especially

Aveling, 1931; Bourke, 1964; and Taschdjian, 1979).

Perhaps the most direct attempt in recent years in experimental psychology to examine volition in the traditional sense as a direct cause of behaviour comes from Howard and Conway (1986; see also Howard & Conway, 1987). They reported three studies. The first study involved having students, who liked to eat peanuts, either restrain themselves from eating peanuts or eat as many as they liked, according to instructions. The degree of volitional control was contrasted with an environmental factor (whether the peanuts were stored in sight or out of sight). The study found that the size of the effect on the dependent variable (weight of peanuts eaten) was much larger for the volitional factor (instructions) than for the nonvolitional environmental factor (sight). In follow-up experiments, Howard and Conway (1986, Studies 2 and 3) and Howard, Youngs & Siatczynski (1986, Studies 1 and 2, cited in Lazarick, Fishbein, Loiello & Howard, 1988) also found volitional control to be more powerful in accounting for subjects' behaviour than were various potential external causal influences. Subsequently, Lazarick *et al.* (1988) presented three studies in which they explored issues related to weight control, vocational exploration, and eating disorders. Their results indicated that people can willfully control (increase or decrease) their eating and exercise behaviour, and their search for career-related information. Furthermore, their findings suggested that binge eating can be modifiable in the short run by volitional efforts.

Hays (1987) pointed out that the effects observed in the earlier Howard and Conway studies could be explained on the basis of demand characteristics of the experiments. Ford (1987) pointed out that since the experimenter offered the subjects the various choices this only highlighted his choices but not the subjects'. Thus the apparent volitional activity of the subjects could have been the product of environmental causes after all. The same argument

applies to the early choice studies of Michotte (1910) and Barrett (1911) in which subjects had to choose between two alternative arithmetical operations, and between two pairs of liquids which they had to drink, respectively. The experimenter chose the task for the subjects. Ford, furthermore, pointed out that psychology already had efficient models of how people make and implement choices (e.g. Bandura, 1977, and many others). Finally he criticized the Howard and Conway studies for treating their attained effect sizes as though they really measured volition, since such a measure only served as a quantitative summary of the observed behaviour and yielded little or no information about any process of volition.

Towards a Working Definition of Volition

We do not pretend to have a theoretical framework of volition at this stage in our work. At the beginning of this paper we presented a definition of volition as the 'act of exercising the will'. English & English (1958) defined will as 'the capacity for deciding upon and initiating a course of action'. We want to argue that there may be two aspects of will, or, at least, two aspects of how we use the term. These can perhaps be described as 'will' as a noun, and 'will' as a verb. Will as a noun refers to the capacity of choosing a goal, whereas will as a verb refers to a consciously controlled mental effort to try to make a chosen goal happen. This dual aspect to volition can be seen even in our common usage of terms. A strong-willed person may be regarded as someone (1) who does quite a bit of deciding, choosing, resolving uncertainty, being active in determining what will be done, and/or (2) who follows up vigorously on those choices by striving to implement them.

The above distinction is in line with Spearman (1937). In surveying the early days of psychology, he felt that one of the biggest problems was in classifying those aspects of mentation that did not seem to fall readily under the label of cognition. He

divided mental processes into two main groups: cognition, which embraced topics similar to those found within present-day cognitive psychology; and orexis, referring to mentation involved with appetite, desire and interest. Orexis was further broken down into feeling (emotion) and volition, with volitional mentation centring on purpose and actions. Most importantly, Spearman then divided volition into resolution (mentation involving decision and choice), and conation, or mentation related to striving. Volition was seen as a two step process, with resolution or decision-making being followed by conation, the striving or trying involved in carrying the resolution into action.

Choice and decision-making have remained part of experimental psychology. Today a considerable research effort within cognitive psychology focuses on the factors that affect our judgement, decisions and actual choices (e.g. Arkes & Hammond, 1986; Bell, Raiffa & Tversky, 1988). Conation, however, has all but disappeared. Around the turn of the century, it was a dominant concept in the thinking of some British psychologists, such as Ward (1918) and Stout (1913). Ward felt that consciousness itself was the expression of a controlling central ego, and equated that ego with conation. Stout emphasized conation as dynamic mental activity, and held it to be central to understanding mental processes. Perhaps the best known theoretical system to emerge from this school of thought was that of MacDougall's purposive psychology, which emphasized the individual as a goal-seeker (e.g. MacDougall, 1923). Human experience and behaviour was seen as influenced by three innate predispositions: cognitive, affective, and conative. In his words:

Conative experience is the felt impulse to action; and it is felt, or is prominent in experience, in proportion to the strength of the working of the impulse. It takes the form of mere craving for some undefined goal, of definitely directed desire, of conflict or desires, of resolving, choosing, willing; and, when we are actively

occupied in working toward our goal, either by thinking or by bodily activity, the conative experience is complicated and obscured for introspection by the kinaesthetic sensory qualities set up by muscular strains. (ibid., p.320).

Thus for MacDougall conation seems to include resolving and choosing. Yet in distinguishing between conation and cognition, he states;

An impulse, once set to work toward a goal, continues in some degree after we cease to think of the goal or the means toward it. That is to say, conation outlasts the cognition which initiates it (ibid., p.282).

Conation, then, was clearly set apart from any cognitive problem solving activities that might lead up to choosing Action A over Action B. It referred to a very diverse set of mentations, very much linked to actual behaviour or performance, yet themselves still quite unobservable and thus not palatable to the experimental psychologist within the dominant behaviourist tradition.

The concept of conation died away, with MacDougall as one of its last major proponents, replaced by the more acceptable concept of motivation, with its emphasis on the internal (biological) and external stimuli responsible for observed behaviour. Sets of drives were postulated, ranging from simple biological ones to complex social ones (e.g. Geen, Beatty & Arkin, 1984; Mook, 1987). Theory and hypothesis testing were built around hypothetical interactions between external stimuli, internal drives, and overt responses. This allowed researchers to bypass the particulars of any mentation involved, focusing instead on more readily observed variables.

The above tentative distinction (choice vs. conation) may clarify some of the issues that have come up in discussion about the concept and study of will. It would seem, for instance, that although Howard & Conway (1986) set out to test volition as

'the act or the power of making a choice, or decision' (*ibid.*, p.1242), they use self-control as a warrant for inferring volitional control. They argued that eating peanuts could relate to weakness of will, or lack of self-control, and that the more peanuts the students consumed the less willpower they had. It could be argued that what Howard & Conway did, in fact, was not to examine the act of making a choice, but to explore the volitional mentations involved in trying to make something happen (or not happen) when temptations are either in sight or not in sight (but available), in other words, conation under feedback and nofeedback conditions.

In the remainder of this paper, we are concerned with those volitional mentations that occur during the implementation of choice; and we will refer to them as conative mentation, or conation. The fundamental hypothesis we set out to test regarding conation was that it is a measurable and distinct hypothetical faculty or component of the human psyche and behaviour. The counter-hypothesis is that conation does not exist as such, or that it does exist but as yet as an unmeasurable epiphenomenon (*e.g.* Ryle, 1949).

Parapsychology and Conation

Volition is an important concept for parapsychology. Although many of the anecdotes suggestive of psi involve spontaneous, unplanned incidents, almost all of the experimental evidence involves situations in which subjects intend to interact with a target of some sort specified by the investigator. In ESP tests, the subject deliberately generates mentation that is intended to resemble the target in some meaningful way. Some ESP tests are tests of guessing, or choosing; of making decisions in the absence of useful information about which choice in fact is correct. Such choosing epitomizes the kind of choice that is not merely the product of the kind of problem solving and deliberations studied so thoroughly by cognitive psychology. Rather, it would seem to be the kind of 'free' choosing more specifically tied to volition

as it has traditionally been construed. In PK tests the linkage is even more direct, because the subject is asked to influence the remote target purely by engaging in conative mentation, just by wishing, willing or wanting the dice or the noise-driven video game to behave in a prespecified manner.

The relevance of PK research for the study of conation can be seen more clearly by considering how modern PK research is conducted. A typical PK experiment can be said to consist of essentially three components:

- (1) The independent variable, which can be any volitional instructions given to the subject. Subjects may simply be asked to use PK ability to influence the PK target in a specific way, by 'willing', 'wishing' and using their 'willpower' or mind to generate the influence. More detailed instructions might involve asking subjects to use a specific conative strategy, such as goal-oriented imagery, to influence the PK target.

- (2) The dependent variable, which is the behaviour of the PK target. A PK target is generally a labile physical system having a random but measurable component. One often employed is the so-called random number generator (RNG). These are devices that use a source of noise, such as radioactive decay or electronic noise, to produce sequences of numbers drawn from a finite domain such as 0 to 9, or 1 to 4. Each successive number should provide no useful information as to the value of any other numbers; they should all be demonstrably mathematically independent. As an example, let us say that we have a RNG that produces the four numbers 1, 2, 3 and 4. Without any physical intervention by subjects and without any faults in the RNG, by chance the RNG should produce the number 1 equally as often as the numbers 2 or 3 or 4. In this example the PK instructions could be to try to increase the frequency of the number '3' via sheer willing or PK over the course of a prespecified number of occasions, or trials.

At the end of a run of, say 40 trials, a tally is automatically generated of the number of times each of the four options actually occurred. Mean chance expectation (MCE) for each should be 10. If the assigned target tends to occur consistently more often than MCE, the likelihood of such a score being greater than chance alone can be calculated by standard binomial expansion, or approximated by *z* scores. Such a score can be compared with the RNG's behaviour in the absence of attempts to influence it (control runs), or controls can be built into the experimental sessions, e.g. by counterbalancing target assignments.

(3) The precautions that have to be taken in order to eliminate any environmental factors that might affect or bias the PK target, including its determinants, and the possibility of subject fraud. We do not have space in the present article to consider in detail the various methodological problems and strategies for overcoming them, and we are not presently making any claims about the evidence for PK. For a discussion of the main issues, see Alcock (1990), Girden & Girden (1985), Radin & Nelson (1989) and Rush (1977)

We argue that a PK task can be especially suitable for research into volition, especially conation. First, because subjects can be instructed directly to use different kinds of conative strategies on the task, it would seem that a PK task could offer an opportunity to explore differences in relatively 'pure' conational characteristics, independent of psychomotor variables. Second, the probabilities of a 'hit' can be changed when using an RNG in the PK task, e.g. from $p = .5$, where success is frequent, to $p = .1$ or lower, such that success is rare. Thus different real-life volitional situations can be simulated, ranging from situations in which the subject is intending to be quite successful and can feel confident of frequent hits, on through to situations in which the subject is hoping for a relatively rare event to take place, with relatively low expectation of success for a given trial. Third, human volitions can either be

focused on doing or obtaining something, or on avoiding doing something. The PK task can simulate this situation insofar as subjects can either be asked to will hits to occur and attempt to score above chance (referred to as 'high-aim'), or to will hits not to occur and try to score below chance (referred to as 'low-aim'). Fourth, subjects can be given either conative instructions (e.g., 'intend or wish for the number 2 generated by an RNG to occur more often') or choice instructions (e.g., 'select the time when you think the number 2 is most likely to be generated by an RNG'). Fifth, individual differences in a variety of self-reported volitional strategies and related mentation characteristics can be assessed and used as variables. Sixth, as will be seen below, it is possible to measure various physiological and behavioural consequences of different assigned volitional strategies, which can serve as dependent variables. Obviously PK success rate is a dependent variable as well, but we are by no means confined to it.

Prior Research on Conative Strategies and PK

A few PK studies in parapsychology have been conducted to explore different conative strategies or styles. These studies have mainly investigated two strategies: the use of goal-oriented imagery, which involves willing a desired outcome of a PK trial to take place by visualizing strongly what a successful outcome will look like, once it has taken place; and process-oriented imagery, which involves visualizing some sort of process such as an energy beam or something mechanical interacting with the PK target so as to bring about the desired outcome. Each strategy is well known in the popular literature. There are eight studies on record that have explored the role of goal-oriented imagery in PK tasks (Gissurarson & Morris, 1990; Levi, 1979; Morris & Reilly, 1980; Morris, Nanko & Phillips, 1982; Nanko, 1981; Stanford, 1981; and Steilberg, 1975). Seven studies yielded PK scores in the expected direction for goal-oriented imagery, and the PK

scores were significantly above chance in three of the eight studies when immediate trial-by-trial feedback of performance was provided (Levi, 1979; Morris *et al.*, 1982; Nanko, 1981)¹. These studies have suggested that goal-oriented imagery may be important in the generation of extrachance PK scoring and that goal-oriented imagery does not seem to produce first-session effects (Morris *et al.*, 1982; Nanko, 1981).

Present Studies

When we started research into volition at the University of Edinburgh in 1987 we adopted our experimental design from parapsychology, rather than attempting to produce the mental state of willing by pitting attention against a habitual associative tendency as was the case in the work of Ach (1905) and later researchers. Most of the empirical work in psychology so far that describes itself as being directly about volition has primarily looked at gross behavioural consequences of assigned volitional activity. In turn these consequences have been used to draw inferences about possible processes of choice involved. However, there has been little coverage of how people go about volitional tasks, the conative styles or strategies involved, or of how we assess whether a person is actually following conative instructions. Our research was aimed at exploring means of measuring overt consequences of volitional mentation, in this case conative practice, and at exploring the effect of three conative styles or strategies on PK performance.

Although the PK score results are summarized in this paper, they are presented in more detail elsewhere (Gissurarson & Morris, 1990; 1991). Our main focus here is

on developing an objective indicator of differences in conative mentation, as described below, that is not dependent upon the PK, yet makes use of experimental techniques for assessing PK claims.

Study 1

Our initial study was aimed primarily at pretesting procedures to be used in later, more extensive studies. It also enabled us to look at the test-retest reliability of our measures.

Subjects

Ten subjects participated (4 female & 6 male, aged 24 to 45 years with a mean age of 30).

PK apparatus

A computer program was written in BASIC to provide a videogame-like PK test. Four green windows or boxes appeared in a row on the upper half of the computer screen against a black background. Embedded in the program was a pseudo-random number generator (pseudo-RNG) designed by Wichmann & Hill (1982; 1984; for information about the adequacy of its randomness see also Gissurarson & Morgan, 1988). Each of four possible numbers (1, 2, 3 or 4) produced by the RNG was assigned to one of the four windows on the screen.

At the beginning of a 40-trial run, the RNG automatically produced a random designation of one of the four windows which then became the target. A brown arrow appeared beneath the designated window showing that it was the target. The RNG selected a new target window for every block of 10 trials of the run. On each trial, subjects were to attempt to use their own mental activity to 'make the computer' select the designated target window each time they pressed the space-bar, thereby initiating the RNG once more. Each trial was automatically tallied as a hit or miss, depending on whether or not the number selected matched the assigned target num-

¹ A trial refers to, for instance, when a subject presses a button that triggers an RNG to produce one or (as is generally the case) several random numbers. Trial-by-trial feedback refers, roughly speaking, to information as to whether a trial was a hit or a miss, i.e., information as to whether the number produced by the RNG corresponded to the assigned number.

ber. A single trial was initiated by pressing down the space-bar, and then quickly releasing the pressure. If the subject felt that he or she was in an exceptionally favourable state, and could obtain many hits, he or she could hold the space-bar down continuously in order to have the computer select a few trials in a row (the number of trials depending on how long the space-bar was held down), while this 'feeling' lasted.

There were two levels of feedback available. In feedback mode, trial-by-trial feedback was provided as a beep sound immediately if a hit was made, and the screen lit up into a blue star with blue rays radiating out from behind. To provide cumulative feedback the number of hits was also continuously displayed at the bottom of the screen. In nonfeedback mode, no information was provided as to whether each trial was successful. For both modes, however, text appeared on the screen below the four boxes showing how many trials were left, and after a 40-trial run the final number of hits was displayed.

The time that passed between consecutive space-bar presses (trials) was automatically recorded in hundredths of a second. We called this time measurement 'Interval Duration' (ID) to distinguish it from reaction time (rt) measures. Whereas rt measures the time taken to accomplish a mental task, ID measures how long it takes for subjects to feel satisfied with their mental image and then feel it is the right moment to initiate a trial. As a standard procedure the time spent on the first trial of 40 was not included in the ID scores (unless otherwise stated, as in Study 3), because this time interval may reflect unfamiliarity with the test, time subjects spend on getting ready and so forth, in addition to the actual cognitive processes involved in PK efforts. The fixed time allocated to the appearance of the blue star for hits in the feedback mode was automatically excluded from all ID measurements.

We were interested in the ID scores as a potential measurement of ongoing volitional mentation processes having different internal properties and thus

taking different times to complete. The duration of imagery-related mentation appears to be related to the duration of analogous physical information gathering processes (such as visual scanning) when subjects perform spatial tasks or solve problems as measured by response/reaction time (e.g. Cooper & Shepard, 1973; Kosslyn, Ball & Reiser, 1978; Shepard & Metzler, 1971). If there were consistencies in ID score within subjects and between conditions, this would indicate that consistent mental processes were indeed going on, and would also provide evidence that subjects were in fact carrying out the volitional instructions they had been assigned. The present study is composed of two separate exploratory series involving the same subjects pretesting slightly different protocols, thus allowing us to examine the test-retest reliability of ID scores for a small number of subjects, which was our first concern in considering ID as a potential measure.

Experimental environment

The PK test was run on an IBM XT machine in a sound-attenuated room in the Psychology Department at the University of Edinburgh.

Procedure

There were two sessions. When a subject attended the first session the experimenter started by giving him or her a demonstration run of the computer test. Having discussed the PK task to the subject's satisfaction, the experimenter then restarted the computer program and pressed the space-bar (this was the starting point of ID for the first trial), whereupon the four boxes test display came on the screen. The experimenter then left the sound-attenuated room and closed the door. The initiation of a run was the same for the second session, following a brief chat and reminder of the procedure.

During each subject's first session (all of which were conducted in July, 1987), he or

she completed two runs of 30 trials each on the computer program with a break between; one in the feedback mode and another in the nonfeedback mode, and without being instructed to adopt any particular strategy. This was the only study that had 30 rather than 40 trials, but all other features were the same. The increase to 40 trials per run was done both to increase the amount of data available and to make statistical analysis more attractive (chance being 10 hits rather than 7.5). The order of feedback and nonfeedback modes was counterbalanced between subjects, such that half started with the feedback mode followed by the nonfeedback mode and the reverse for the other half. In December 1987, five months later, the same subjects completed another two runs of 40 trials each. Again, subjects were merely instructed to use their PK ability to influence the test, without any specific strategy recommended. Feedback and nonfeedback modes were counterbalanced, as before. The subjects in both sessions and in all later studies were unaware that the time they spent on the task was being recorded by the computer.

Results and discussion

Overall 1400 trials were completed but no significant PK results were found in the data. The PK scores did not correlate significantly with ID. However, ID in the first session correlated significantly with ID in the second session, with Spearman $r(9) = .72$ ($z = 2.16$, $p < .05$, two-tailed)². This robust relationship was mainly due to high consistency in the nonfeedback mode, where first and second session ID yielded a Spearman $r(9)$ of .82 ($z = 2.46$, $p < .02$, two-tailed). First and second session ID in the feedback mode produced a Spearman $r(9)$ of .41 ($z = 1.22$, $p = .22$, two-tailed). Further indication that ID did not change much

from the first session to the second, even with an increase in the number of trials, was given by comparison of the means of first and second session ID. For the feedback condition, the mean ID between trials in the second session was 5.16 seconds, whereas the mean ID between trials in the first session had been 5.45 seconds. For the nonfeedback condition, the mean ID between trials in the second session was 4.49 seconds, whereas mean ID between trials in the first session had been 4.65 seconds. Neither difference approached significance. The ID difference between feedback and nonfeedback modes was also nonsignificant according to the Wilcoxon Matched Pairs test both for Session 1 and 2.

These results would seem to suggest that under nearly identical conditions from the subjects' standpoint, and without any instructed strategy, ID does not seem to change much from one occasion to another and remains nearly exactly the same, even with a five month gap between tests and with a different number of trials. This effect was strongest in the nonfeedback mode, suggesting that perhaps the presence of feedback may serve somewhat as a source of noise. Furthermore, while acknowledging that ID was slightly higher in the feedback mode in both sessions, the difference was far from significant and it would seem that subjects spend about the same length of time on the PK task irrespective of whether they receive feedback of their performance.

Study 2

Study 2, which was conducted from September to November, 1987, served as a screening study to recruit subjects for Study 3, described below. It also enabled us to look further at ID scores in the feedback and nonfeedback conditions, and without assigned volitional strategies. Furthermore, it allowed us to collect descriptions of the natural volitional strategies that subjects would tend to use in our testing situation.

² When we plotted the distribution of ID measurements the result showed an asymmetrical, positively skewed distribution. Therefore we decided to use nonparametric tests when assessing relationships related to the ID scores.

Subjects

Forty volunteers were tested (20 male and 20 female, aged 17 to 75 years with a mean age of 37).

Apparatus

The same computer test, again with the Wichmann-Hill pseudo-RNG, and the same environment as before were used.

Procedure

Following a demonstration run of the computer test, the experimenter restarted the program for the subject and left the sound-attenuated room. Each subject did two runs in a single session: 40 trials in the feedback mode and 40 in the nonfeedback mode. Feedback and nonfeedback modes were counterbalanced across subjects as in Study 1. The subjects were instructed to use their mental activity to will hits to occur, but were not provided with any specific strategy. After the session, subjects were asked to write a description of any mental strategies they had found themselves using.

Results and discussion

A total of 3200 trials was completed, but no significant PK results were found. The PK scores did not correlate significantly with ID. The difference in ID between the feedback mode (mean = 6.06 seconds) and the nonfeedback mode (mean = 5.01 seconds) was not significant on a Wilcoxon test (which yielded $p = .89$, two-tailed).

Only four subjects reported not having used any strategy at all. Thirty-six subjects reported having used a strategy of some sort. The maximum number of strategies reportedly tried out during a single session was six. Overall there were 65 reported strategies, with a mean number of strategies of 1.63 tried out during a session. Those who reported using a strategy often changed from one strategy to another. Reported strategies ranged from relaxation, concentration, commanding the computer

to produce hits, talking in a friendly way to it, asking it to work, to watching light spots on the screen and mediumistic communication with 'controls'. Most frequently subjects reported developing one or more types of imagery strategy to assist them in their willing, such as creating mental pictures of the blue star, or imagining the energy from one's thoughts transferring from one's arm into the computer. Such imagery was accompanied by focusing of one's attention on the goal, which was the blue star, and on the successful achievement of that goal. Many of the subjects seemed to feel that the clearer and more precise their mental image, the more likely it was subsequently to occur in reality (see Appendix A for details).

Study 2 suggests that most subjects presented with a PK task and instructed to use their mind to will hits to occur (but without instructions to use a particular strategy), try out various strategies in their attempt to be successful on the task. Furthermore, the data would seem to suggest that when subjects are confronted with a task that requires them to engage in willing certain things to happen, they most frequently develop imagery-related strategies of some sort. This diversity of strategies is consistent with the observations of Jahn & Dunne (1987, pp.141-142), who reported considerable differences in the natural strategies that subjects employed for their PK tasks. Finally, although suggesting a slight trend towards being about one second longer in the feedback mode than in the nonfeedback mode, again the ID remained nearly the same between the two modes showing that subjects used about 5 to 6 seconds between trials, which is quite similar to Study 1.

Study 3

Study 3 was carried out from February to May, 1988. Its aim was to attempt to replicate the findings of those PK studies that had explored the effect of different conative strategies (goal-oriented and process-oriented) on PK performance. Furthermore, in order to extend this sort of PK research

we decided to explore the possibility of using the volitional strategies as a method of increasing PK scoring via repeated practice. With regard to ID, we wanted to see if there were consistent ID differences between assigned conative strategies and if those differences interacted with the level of feedback.

Subjects

Twenty-four unpaid volunteers participated (11 male and 13 female, aged 20 to 57 years with a mean age of 33), and consisted of those people who had scored at, or above mean chance expectation in Study 2 and had completed the Vividness of Visual Imagery Questionnaire (VVIQ; Marks, 1973).

Apparatus

The same computer test and environment as before were used.

Procedure

The subjects were allocated to three groups so that their average results from the VVIQ and their previous PK scores from the pretest occasion were about the same for all groups. A Kruskal-Wallis One-Way ANOVA by Ranks showed a nonsignificant difference between VVIQ and PK scores of the three groups: For the PK scores, $H(2, N = 24) = .081$ ($p = .95$); for VVIQ score, $H(2, N = 24) = .44$ ($p = .80$). Each group was provided with a specific conative imagery strategy. The strategies consisted of process-oriented imagery (PO), goal-oriented imagery (GO), and a new strategy termed end-oriented imagery (EO). PO involved visualizing some sort of energy building up inside the body and extending like a beam into the target window (on the screen), pulling out the blue star. GO involved willing the blue star to occur by visualizing strongly its appearance on the screen. EO involved willing the final number of hits for each run to be a certain number by visualizing strongly that particular number being

displayed at the end of each run. Subjects were kept blind as to the other imagery conditions.

Each subject attended an interview followed by six experimental sessions approximately one week apart. In the interview the subject was given his or her particular strategy instructions both orally and in written form on a sheet of paper as a reminder. While the experimenter explained how to use the strategy, the subject was also given a demonstration run of the computer test. In the experimental sessions, the subjects were simply reminded to establish their assigned imagery before doing the computer test. In each session subjects did two 40-trial runs on the PK test while alone in the sound-attenuated room. Half of the subjects did the first run in the feedback mode and the second run in the nonfeedback mode in their first session, whereas the other half had the reverse order of modes in their first session. For each subject the order of feedback and nonfeedback modes was counter-balanced between sessions. Subjects were asked to practice their particular strategy at home between sessions.

At the beginning of each session the experimenter set up the computer test. He took care not to start the test (by pressing the space-bar, which was the starting point of ID measurement for the first trial) until the subject was all set and ready to start practising his or her conative strategy. After starting the test the experimenter immediately left the sound-attenuated room and closed the door.

Results and discussion

There were 144 sessions, totalling 11,520 trials. PK scoring was nonsignificant for all three of the strategies. The study provided no direct support for any of the three imagery strategies as a method of increasing PK scoring. Hence the findings did not support what the earlier PK volition imagery studies had found.

It seems likely that subjects used their assigned strategies while doing the PK task

or at least tried to, based on the following observations:

(1) The mean ID after subjects had been instructed to use a particular strategy was 10.40 seconds between trials, whereas the mean ID for the same subjects during Study 2, when they had not been instructed to use any strategy on the pretest occasion, was 6.27 seconds. This difference was significant using the Wilcoxon test: $T(N = 24) = 58.0$ ($z = 2.63$, $p < .01$, two-tailed). In other words, when subjects are instructed to employ a specific conative strategy, the time they take on the task increases significantly.

(2) As a standard procedure we eliminated the first trial ID in Studies 1 and 2 because we suspected that it might be confounded by various variables in addition to the conative processes involved in PK efforts and because there was no specific event that could realistically serve to define the start of the period of effort. For Study 3, however, the experimenter was careful to wait until the subject had become settled in and ready to go, then pressed a key to start the run and immediately left the room. Therefore we did have a precise timing of the start of the first period of conative activity. When looking at the raw data of Study 3, we confirmed that subjects tended to spend considerable time before completing the first trial. Comparison of the time that subjects spent before the first trial (after the experimenter had left the room) and the mean time they spent on the remaining 39 trials suggests that they were engaged in some 'preparation' procedure before starting the trials, as if they were rehearsing their strategy and getting the assigned mental imagery ready in their minds. This may have been a result of the imagery instructions requiring subjects to go through their assigned strategy to start with. Perhaps once the imagery was established, it could be readily retrieved during the ensuing trials. We can probably rule out 'unfamiliarity' with the test out because all subjects had experienced this test earlier, during the screening session

and the interview, and then repeatedly throughout the six experimental sessions in this study. In fact, ID showed a slight increase from first to sixth session; perhaps due to a previous lack of PK success, participants might have felt they had to try harder or do more to be successful. The mean ID for the first trial was 46.04 seconds, whereas the mean ID for the following 39 between-trial intervals was 10.40 seconds. The difference was significant using the Wilcoxon test: $T(N = 144) = 1078.0$ ($z = 8.26$, $p < .0001$, two-tailed).

(3) If subjects engaged in their strategies, the differences in mentation could result in strategies taking different times to perform as measured by ID. The differences in overall ID (for 39 trials) between strategies were, however, not significant according to a Kruskal-Wallis test: $H(2, N = 144) = 2.830$ ($p < .25$), and neither were the differences in overall first trial ID between strategies: Kruskal-Wallis test: $H(2, N = 144) = 3.668$ ($p = .16$) (see Table 1). Within strategies, only the PO condition demonstrated a significant difference between feedback and nonfeedback ID (for 39 trials), with ID lasting longer in the nonfeedback condition than in the feedback one (Wilcoxon $T = 345$; $z = 2.49$, $p < .02$). No significant differences were found in the first trial ID between feedback and nonfeedback modes for the three groups.

In order to obtain a more complete picture of the total time involved in practising the strategies, we decided as well to add the first trial ID to the time spent on the remaining 39 trials (see Table 1). As it turned out, the difference in total ID (for 40 trials) for the three strategies was not significant according to a Kruskal-Wallis test: $H(2, N = 144) = 3.314$ ($p = .19$). The total ID in seconds for PO was 929.33, for GO, 936.59, and for EO, 843.44.

For all three strategies the nonfeedback condition consistently took longer than the feedback condition, reversing the weak trend found in earlier studies. This trend was significant for both PO ($T = 342$; $z =$

2.52, $p < .02$) and GO ($T = 385$; $z = 2.08$, $p < .05$), and almost significant for EO ($T = 412$; $z = 1.81$, $p < .07$). The GO data were unusually distributed. Although nonfeedback trials took longer than feedback trials in 35 out of 48 sessions ($z = 2.74$, $p < .01$), those few exceptions included some unusually long ID times during feedback, such that the overall mean for feedback, 479.33 seconds, was actually greater than the mean for nonfeedback times, 457.26 seconds.

In order to examine the test-retest reliability of the ID measure in this study, we looked at how ID score correlated between Sessions 1 and 6 in Study 3 (see Table 2). An intercorrelation between ID (for 39 trials) in Session 1 and Session 6 showed a moderate, significant correlation in the nonfeedback condition ($r_s = .50$, $p < .02$), but a lower, nonsignificant correlation in the feedback condition ($r_s = .30$, $p < .15$). The correlation between first trial ID in Session 1 and Session 6 also showed a moderate significant correlation in the nonfeedback condition ($r_s = .57$, $p < .01$) and a lower, nonsignificant one in the feedback condition ($r_s = .25$, $p < .25$). When feedback and nonfeedback trials are combined, all three measures produce a significant correlation between Session 1 and Session 6 ID scores. This would seem to confirm the finding of Study 1 in that ID seems reasonably reliable over time and that the nonfeedback condition contributed more to the stability of ID over time than the feedback condition. Furthermore, this would seem to provide further evidence that the first trial ID is worth considering as an indicator of differences in the practice of the strategies.

Study 4

Study 4 was conducted from October to December, 1988, in order to assess some possible procedural reasons why Study 3 did not yield any definite PK effects, as well as to attempt to replicate the ID findings. A larger sample size might have been required to allow the PK effects, if any, to reach significance, and psychological factors due to too many sessions per subject might have been unfavourable, if subjects had got bored with practising the same strategy, or had felt more pressure as the sessions went on. Providing subjects verbally with their mental strategy in an interview, and then reminding them of employing it at the start of the experimental sessions in Study 3 may have allowed various external and/or internal variables to confound the practice of the imagery strategies and hence the ID measure. In Study 4 an attempt was made to standardize the instructions and make them as identical as possible for all subjects and in all sessions in order to yield a more accurate and precise ID measure of conative imagery deployment.

Subjects

Fifty-two unpaid volunteers (18 male and 34 female, aged 16 to 69 years with a mean age of 30) were selected from a pool of subjects already given a screening session where they had completed the VVIQ and one feedback and one nonfeedback run on the computer test. Those selected had all scored close to chance or above in the screening session.

Table 1

Time (in seconds) spent on practising the three imagery strategies in Study 3.

Group	Time in feedback mode	Time in nonfeedback mode	Combined time	Wilcoxon test between feedback & nonfeedback modes ^a
ID means for 39 trials:				
PO	9.71	11.72	10.72	$T = 345.0$ ($z = 2.49$, $p < .02$)
GO	11.01	9.61	10.31	$T = 449.0$ ($z = 1.43$, $p < .15$)
EO	9.56	10.77	10.17	$T = 450.0$ ($z = 1.42$, $p < .15$)
ID means for the First trial:				
PO	41.58	51.96	46.77	$T = 513.0$ ($z = 0.77$, $p < .45$)
GO	49.91	82.29	66.10	$T = 429.5$ ($z = 1.63$, $p < .10$)
EO	22.34	28.17	25.26	$T = 427.0$ ($z = 1.65$, $p < .09$)
Total ID (sums) for 40 trials:				
PO	420.39	508.94	929.33	$T = 342.0$ ($z = 2.52$, $p < .02$)
GO	479.33	457.26	936.59	$T = 385.0$ ($z = 2.08$, $p < .05$)
EO	395.13	448.31	843.44	$T = 412.0$ ($z = 1.81$, $p < .07$)

^a The Wilcoxon tests are reported as two-tailed ($N = 48$).

Table 2

Spearman Rank Order correlation coefficients (r_s) between ID scores obtained in Session 1 and ID scores obtained in Session 6 in Study 3.

	Feedback condition	Nonfeedback condition	Combined conditions
Session 1 with Session 6 (39 trials)	$r_s(23) = .30$ $z = 1.45$ $p < .15$	$r_s(23) = .50$ $z = 2.41$ $p < .02$	$r_s(23) = .50$ $z = 2.39$ $p < .02$
Session 1 with Session 6 (1st trial)	$r_s(23) = .25$ $z = 1.18$ $p < .25$	$r_s(23) = .57$ $z = 2.74$ $p < .01$	$r_s(23) = .51$ $z = 2.44$ $p < .02$
Session 1 with Session 6 (40 trials)	$r_s(23) = .32$ $z = 1.53$ $p < .15$	$r_s(23) = .49$ $z = 2.34$ $p < .02$	$r_s(23) = .50$ $z = 2.41$ $p < .02$

Note: All p -values are two-tailed.

Apparatus

The same computer test as before was used to measure PK and ID, again run in a sound-attenuated room. This time, however, a live RNG source was embedded in the computer program (for details, see *User's Guide Random Bit Generator RBG 04CA-5*, 1988) along with the pseudo-RNG previously used, so that half of the trials were generated by the live RNG and half of the trials by the pseudo-RNG. The computer program was also changed so that subjects could not initiate many trials in a row by holding down the space-bar. A string of zero ID values might easily serve to distort the data, although there was no suggestion in the Study 3 data that there had been many such trials or that they had been unevenly distributed among the different conditions. Also, a subject unfamiliar with typing might simply hold the space-bar down for too long when intending to initiate one trial, and thus produce unintended trials. In the present version 0.5 seconds had to pass following a trial (space-bar press) before one could initiate the next trial. (This 0.5 seconds was not included in the ID measure.)

A tape-recorder was used in order to make the instructions identical for all subjects. Four separate audio-tape recordings were made. In an attempt to induce a relaxed yet alert and enthusiastic frame of mind, a four-minute 'pep-talk' of suggestions was given to each subject. Each tape-recording started with this pep-talk followed by the strategy instructions (PO, GO or EO). The fourth tape comprised only the pep-talk, and was played for a control group (CR).

Procedure

Subjects were allocated to four groups of 13 each so that their average results from the VVIQ and their previous PK scores from the pretest occasion were about the same for all four groups, according to a Kruskal-Wallis test. Subjects were kept blind as to the existence of other groups than their own. This time around there was

no interview, and all the necessary introduction and instructions regarding the study were provided at the start of the first session.

Each subject completed four sessions approximately one to two weeks apart, using their assigned strategy on the PK test. In the beginning of each of the four sessions, the experimenter set up the computer program and the subject put on headphones. The experimenter then pressed the space-bar, upon which the four boxes test display came on the screen. The experimenter left the sound-attenuated room, closed the door, and went to an adjacent room. The subject started by listening to the tape-recording alone in the sound-attenuated room, sitting in front of the computer console. The tape was monitored in the adjacent room by the experimenter. The subject was instructed to take off the headphones when a 'click' sounded at the end of the tape, thus signalling that it was time to start the run. Two 40-trial PK runs were completed with a break between. The tape was only played before the first run in each session. Half of the trials were done in the feedback mode and half in the nonfeedback mode, with order of feedback and nonfeedback counterbalanced both between subjects and between sessions for each subject. Subjects were asked to practice their particular strategy at home between sessions.

Results and discussion

There were 208 sessions totalling 16,640 trials but PK scoring was nonsignificant and as one would expect by chance for the three strategies. The subjects in Study 4 listened to the tape-recording, which was relevant to their particular group, after the timer had started running for the first trial. In order not to have ID reflect the length of the tape-recordings the first trial ID was excluded from all the following statistics.

The ID measurements showed that subjects had had significantly different ID measurements on their PK tasks depending upon what strategy they had been instructed to use, according to a Kruskal-Wallis

test: $H(2, N = 155) = 15.415$ ($p < .001$) for the three imagery groups (see Table 3)³.

If the CR group is included, $H(3, N = 207) = 16.313$ ($p < .002$) for the four conditions. Subjects who were instructed to engage in GO spent significantly more time on their task than subjects who had been instructed to engage in PO. Subjects who were instructed to engage in EO spent significantly less time on their task than any of the other groups, even less than CR subjects who had not been provided with any conational strategy. This is consistent with the trend in total ID found in Study 3 and

would seem to suggest that different conational strategies take different amounts of time to perform.

A further indication that mental processes are different for the three strategies is that for PO sessions, IDs were significantly longer in the nonfeedback condition than in the feedback condition; for GO sessions, IDs were now significantly longer in the feedback condition than in the nonfeedback condition (see Table 3). For EO sessions, IDs were nonsignificantly longer in the nonfeedback condition than in the feedback condition. Again this is consistent with the

Table 3

Time (in seconds) spent on practising the three imagery strategies and for the control group (CR) in Study 4.

Group	Time in Feedback mode	Time in Nonfeedback mode	Time means	Wilcoxon test between feedback & nonfeedback modes ^a
ID means for 39 trials:				
PO	8.49	10.14	9.32	$T = 461.0$ ($z = 2.08$, $p < .04$)
GO	14.32	10.98	12.65	$T = 302.0$ ($z = 3.52$, $p < .001$)
EO	5.79	6.15	5.97	$T = 570.0$ ($z = 0.87$, $p < .40$)
CR	7.85	7.45	7.65	$T = 624.0$ ($z = 0.59$, $p = .60$)

^a Wilcoxon tests are reported as two-tailed ($N = 52$ for all groups except EO where $N = 51$).

Table 4

Time (in seconds) after a hit and after a miss in the feedback condition in Study 4.

Group	ID after a hit	ID after a miss	Wilcoxon test between ID after a hit and after a miss ^a
PO	6.98	9.02	$T = 445.0$ ($z = 2.22$, $p < .03$)
GO	11.45	15.41	$T = 460.0$ ($z = 2.09$, $p < .04$)
EO	5.03	5.92	$T = 378.5$ ($z = 2.67$, $p < .01$)
CR	7.45	8.77	$T = 351.0$ ($z = 3.08$, $p < .005$)

^a All Wilcoxon tests are reported as two-tailed ($N=52$ for all groups except EO where $N=51$).

³ One outfile from an EO subject was overwritten by mistake, hence the lower N for ID measures of the EO group and, because listwise deletion of data was used, for all ID analyses that included the EO condition.

findings from Study 3 insofar as PO and, to an extent EO are concerned. For GO, this time the results are more clear-cut than in Study 3. The difference in overall means favouring feedback over nonfeedback is now also a consistent difference, being present in 38 of the 52 sessions ($z = 3.19$, $p < .005$, two-tailed), whereas the reverse was true for Study 3. Thus for the present study, the assigned difference in imagery strategy produced not only consistent differences in the overall ID measure, but also clear-cut differential responses to the presence and absence of feedback for PO and GO. The CR subjects, however, showed no comparable feedback effects.

Given the apparent interaction in Study 4 between assigned strategy and level of feedback, we decided to look directly at ID following trials that had been a hit (resulting in the blue star) and ID following trials that had been a miss within feedback runs, where any effect of the feedback would be most immediate and apparent (see Table 4). Surprisingly, at least to us, for all four conditions, subjects demonstrated shorter ID immediately following a hit, than immediately following misses. On the face of it, we would have expected people to slow down after hits thus reflecting a possible additional processing time of the visual input from the feedback. It would seem as if subjects feel more confident once they obtain what they want, whether that is important in itself as a goal (as in GO), important as a contribution to some total

that is the goal (as in EO), or important as the goal of some elaborate mental scenarios (as in PO).

We correlated ID scores from Session 1 with ID scores from Session 4 (see Table 5). For combined feedback and nonfeedback conditions, Spearman $r(50) = .70$ ($z = 4.95$, $p < .0001$, two-tailed). However, the tendency for nonfeedback ID to yield a higher correlation than feedback ID was not the case in the present study, and the feedback ID correlation between sessions was slightly higher than the nonfeedback one.

General Discussion

The present series of studies looked at the possible measurement of some components of volitional behavior through Interval Duration (ID), i.e., the time duration measured while people engage in 'volitional' mentation, specifically conative mentation or conation. Subjects were asked to engage in task-relevant conation under conditions in which there was no associated task-relevant overt behaviour. Thus any overt behaviour observed was likely to be linked fairly directly to the internal conative events themselves, because they would not be part of any task-related plan of behaviour.

What can we say about the validity of the ID indicator at this stage? Can differences in ID be taken as measuring differences in conative mentation? To assess this, we need to consider some specific questions.

Table 5

Spearman Rank Order correlation coefficients (r_s) between ID score obtained in Session 1 and ID score obtained in Session 4 in Study 4.

	Feedback condition	Nonfeedback condition	Combined conditions
Session 1 with Session 4 (39 trials)	$r_s(50) = .68$ $z = 4.83$ $p < .0001$	$r_s(50) = .64$ $z = 4.54$ $p < .0001$	$r_s(50) = .70$ $z = 4.95$ $p < .0001$

Note: All p -values are two-tailed.

First, are there individual differences in ID, and are ID measures sufficiently reliable that they can be used to correlate with other psychometric tests, including potential questionnaires aimed at assessing volitional behaviour? Although work now underway at our laboratory indicates that ID may indeed correlate with two such scalar instruments, one intended to measure self-reported determination and the other to measure volitional intensity (Gissurarson & Morris, 1992), we did not attempt to develop such questionnaires in the present studies. However, in Study 2 subjects were asked to describe any strategies they naturally tended to use. Almost all subjects reported using a strategy of some sort. As can be seen from Appendix A, these methods fall roughly into nine categories, seven relating more to conation (relaxation, communication, imagery, emotional and physical activation, summoning entities and feelings of resonance or harmony), the eighth relating more to choice (feeling or predicting when best to press the key) and the ninth category involving statements rather than a strategy.

Given that such differences exist and that subjects do not always use just one strategy, it would appear also that, when conditions are held constant, ID scores on one occasion are fairly strongly correlated with those on another. In Study 1, first session ID correlated significantly with second session ID, five months later ($r_s = .72, p < .05$); in Study 3, ID scores for the first training session were significantly correlated with ID scores from the final session, five weeks later ($r_s = .50, p < .02$); and in Study 4, ID scores for the first training session were significantly correlated with ID scores from the final session, three weeks later ($r_s = .70, p < 10^{-6}$).

A second question is, do these quantitative differences in ID reflect qualitative differences in mentation? We can get at this question in two ways. First, do differences in assigned volitional strategies seem to matter? When subjects are participating for the first time, and have no assigned volitional strategy, their IDs tend to be rela-

tively short (mean of about 5 to 6 seconds). Subjects given a specific imagery strategy to deploy in training sessions had IDs that were on average twice as long, indicating that they were taking extra time when requested to utilize a specific imagery-related conation. Some of that extra time may have been due to the subjects' awareness that they were participating in a 'training' study and thus should be making an additional effort to do well. The CR group in Study 4 produced mean IDs of 7.65 seconds, halfway between the single session IDs and the training session IDs. Further research is needed therefore to clarify just how much of the increase in ID during training is due to the general features of such sessions and how much is due to the specific deployment of the assigned imagery strategies. If we compare IDs across assigned imagery strategies, the picture changes from Study 3 to Study 4. For Study 3, there was no significant difference between the IDs for the three assigned imagery strategies. For Study 4, however, with taped instructions, no opportunity for rapidly generating trials by holding down the key, and a larger N , there were clear ID differences between the assigned strategies. In both studies, GO had the highest ID scores and the greatest variation in scores. Thus there are indications that differences in conative strategy can function as an independent variable to be manipulated and studied, with ID serving as a dependent variable.

Another approach to examining whether or not IDs can reflect qualitative differences in mentation is to examine the effect of stimulus variables such as access to feedback and content of feedback. During sessions in which no imagery strategy was assigned there was a nonsignificant tendency for IDs to be longer with feedback present. This was even true for the CR training sessions. However for those sessions in which a specific conative imagery strategy was assigned, the results are quite different. For Study 3, each of the three strategies showed a tendency for IDs to be longer in the absence of feedback. This was

independently significant for both PO and GO, and was quite significant overall ($z = 4.41, p < 10^{-5}$), where in fact 99 of the 144 sessions were in this direction. The results were complicated for GO by the presence of a few strong reversals of this trend within individual sessions, resulting in the mean feedback ID being slightly larger than the mean nonfeedback ID. In Study 4, ID was once again longer in the absence of feedback for PO and EO, significantly so, as before, for PO. For GO, however, the results were now significantly reversed, with ID significantly longer in the presence of feedback, thus resembling the overall average time taken to use GO in Study 3. These results would seem to suggest, then, that access to feedback is reflected in ID scores, in ways that can interact with assigned conative strategy.

Given that access to feedback makes a difference to ID score, we can also look at the content of feedback as well. As can be seen in Table 4, ID times were significantly shorter following positive feedback (feedback of a hit) for all three conative strategies as well as for the control condition. Thus subjects apparently were not spending extra time processing the complex hit feedback display. Once again, we have an indication that ID scores can reflect different mentation processes and that they can serve as dependent variables for studies in which features of the task or environment are manipulated and studied as independent variables.

One component of ID scores mentioned only briefly is the non-conative, choice component, that ID ends because the subject has chosen to press the key and initiate the next trial. This decision presumably reflects in part an assessment of the qualitative aspects of the ongoing conative mentation, perhaps a comparison of it to an additional internal representation of what the imagery 'should' be like, or what it was like on past successful occasions. We did not encourage our subjects to introspect on this aspect of their behaviour; and do not have much qualitative data of relevance.

Can we measure the choice components of ID separately from the conative compo-

nents? One strategy is to look at the variance of ID means, assuming higher variance indicates more choosing. If all ID scores were very much the same within a condition, this would seem to indicate that the decision to terminate the ID interval was being made in much the same way from time to time. Conversely, high variance would indicate that fresh choices were being made each time, that the decision-making process was more complex. For instance, were subjects using one assigned strategy less consistently in their overall mean ID scores than subjects using another? If so, the variance of their means should be significantly different when compared by *F*-ratio (e.g. Hays, 1963, p.351). For Study 3, GO had suggestively greater variance than PO [$F(7,7) = 3.84, p < .10$, two-tailed], with EO variance in between. For Study 4, GO had significantly greater variance than PO [$F(12,12) = 3.87, p < .05$, two-tailed], as well as EO [$F(12,12) = 5.97, p < .01$, two-tailed] and CR [$F(12,12) = 6.41, p < .01$, two-tailed]. Thus there was some evidence that subjects, when using GO, were more labile in their choosing behaviour than when using the other strategies or no specific assigned strategy. This method of analysis could be challenged because we do not yet know enough about the underlying distributions of ID scores under a variety of conditions; thus we are reluctant to draw firm conclusions from these data. We are suggesting, however, that when using ID scores to compare different assigned volitional strategies, comparison of mean IDs can provide information about differences in conative factors; comparison of variances may also be useful in providing information about differences in choice factors.

Final Remarks

In the present studies, we have not attempted to make a theoretical analysis of the ID measure. We prefer to proceed cautiously and await further manipulation and experimentation. To sum up our findings, if we differentiate between the possible

research on volition or will as on the one hand the capacity of choosing or deciding upon a goal, and as on the other the consciously controlled mental efforts to implement the choice and make the goal happen (or conation), the specific, empirical implications of our research are twofold: firstly, that it may be possible to measure or operationalize mentations to practise conational styles or mental strategies to some extent and not rely solely on subjects' verbal reports; and secondly, that different conational mentations or styles may take different amounts of time to perform. What this implies, in general terms, is that there may be a durational aspect to conation. For parapsychology, one methodological implication is that we may now have a partial solution to the problem of ascertaining whether or not particular mentation instructions in PK tasks have indeed been carried out.

We realize that by returning to the use of such concepts as volition and conation we may appear to be ignoring or setting aside the considerable benefits of information processing approaches within cognitive psychology. But such is not our intention. We use them because we find them convenient and heuristically useful in describing aspects of mentation that have been under-explored experimentally in recent years. Their usage may be quite temporary, with better terms available once we get a fuller picture of the range of mentation and associated behaviour involved. From an empirical perspective, our aim has been to introduce a new measure by which to explore such mentation and its characteristics in and of itself.

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Appendix A

The frequency of reported strategies used by subjects (Ss) in Study 2 in order to 'influence' the course of events on the PK computer program.

Reported strategies and a short description	Frequency
Adopting a relaxed approach:	
1) Physical relaxation: deep breathing, decreased bodily movement and general release of tension.	3
2) Mental relaxation: attempts to make the mind blank or calm, focused on emptiness.	3
	(6)
Communication of some sort:	
1) 'Communication' with computer: asking, commanding, or willing via speech or thought that the computer performs a certain task.	2
2) 'Communication' with oneself: positive, encouraging statements repeated to oneself in an effort to achieve results.	3
	(5)
Statements reported rather than a strategy:	
1) Statements of 'concentration': often Ss simply reported that they had concentrated hard on the computer test.	12
2) 'Will' statements: only one S reported simply having used his or her 'will' to influence the computer.	1
	(13)
Imagery methods:	
1) Negative imagery: Ss imagine the goal (scoring hits) as an act of destruction, or elimination.	2
2) Goal-oriented imagery: Ss imagine the goal (scoring hits) as an act of assistance or reconstruction.	10
3) Process-oriented imagery: some sort of process is visualized in order to bring about a successful outcome.	2
4) Other imagery: Mental pictures created of certain actions or scenarios, other than goal and process oriented.	4
	(18)
Space-bar pressing:	
1) Bar-pressing strategies: often Ss described certain ways of pressing the space-bar that became the strategy.	4
2) Guessing strategies: Ss attempted to feel or predict the right time to press the space-bar.	7
	(11)
Other methods:	
1) Positive emotions evoked: built up excitement, such as joy was released when pressing the space-bar.	2
2) Loss of awareness: reports of loss of awareness and/or feelings of harmony or melting (in spirit) with the computer.	6
3) Summoning of entities: one S reported summoning an external entity to his/her aid.	1
4) Physical activity: attempts to influence the course of events	

via flexing muscles in the body or staring fixedly at the screen.

3

Noatun 17
105 Reykjavik
ICELAND

Un paradigme de recherche proposé pour l'étude du contenu mental de la volition.

Résumé: Cet article rapporte quatre études qui ont été menées à l'Université d'Edimbourg afin d'explorer la possibilité d'aspects opérationnalisants liés à la volition, ceci en adoptant un plan expérimental pour la parapsychologie. La première étude a suggéré que le temps que les sujets passent dans la tâche de volition reste inchangé d'une occasion à l'autre sous des conditions identiques. La seconde étude a suggéré que, quand ils sont confrontés à une tâche de volition, la plupart des sujets essaient diverses stratégies mentales, dont la majeure partie est basée sur l'imagerie, dans leur tentative de réussir la tâche. La troisième et quatrième études ont suggéré que différentes stratégies attribuées d'imagerie de volition prennent des temps différents à s'effectuer et que de telles différences ont été affectées par le fait de fournir ou non un feedback aux sujets sur comment ils ont accompli la tâche. On discute des implications possibles de ces découvertes pour la recherche sur la volition.

Um paradigma de pesquisa Proposto para o Estudo da Atividade Mental Volitiva

Resumo: Este trabalho relata quatro estudos conduzidos na Universidade de Edimburgo a fim de explorar a possibilidade de operacionalizar aspectos relacionados à volição adotando-se um esquema experimental da Parapsicologia. O primeiro estudo sugeriu que o tempo que os sujeitos gastam em uma tarefa volitiva permanece imutável de uma ocasião para outra sob condições idênticas. O segundo estudo sugeriu que quando de frontados por uma tarefa volicional a maior parte dos sujeitos tentou soluções utilizando de várias estratégias mentais, a maioria das quais baseadas em imagens mentais, em suas tentativas de obter sucesso na tarefa. O terceiro e o quarto estudos sugeriram que diferentes estratégias de utilização de imagens mentais volicionais apontadas levam um tempo diferente para serem executadas e tais diferenças foram afetadas de acordo com o fornecimento ou não de um feed-back sobre como os sujeitos estavam se saindo nas tarefas. As possíveis implicações desses resultados para a pesquisa da volição são discutidas.

Propuesta para un Paradigma de Investigación para el Estudio de la Verbalización Volitiva

Resumen: Este artículo reporta cuatro estudios que se llevaron a cabo en la Universidad de Edimburgo para explorar la posibilidad de operacionalizar aspectos relacionados a la voluntad adoptando un diseño experimental parapsicológico. El primer estudio sugirió que el tiempo que los sujetos toman en tareas volitivas no cambia de una ocasión a otra bajo condiciones idénticas. El segundo estudio sugiere que cuando los sujetos se encuentran con una tarea volitiva estos usan varias estrategias mentales para obtener éxito en la tarea, la mayor parte de las cuales se basan en imágenes mentales. Los estudios terceros y cuarto sugirieron que las diferencias en estrategias volitivas de imágenes mentales se toman diferente tiempo en actuar y que tales diferencias son

afectadas por el hecho de dar información o no a los sujetos sobre los resultados de la prueba. Las posibles implicaciones de estos resultados para la investigación de la voluntad son discutidas.

Onderzoeksparadigma voor experimenten met mentale voorstelling van strategie

Samenvatting: Dit artikel behandelt vier in Edinburgh uitgevoerde onderzoeken naar de mogelijke operationalisatie van aspecten van wilskracht via een experimentele opzet uit de parapsychologie. Het eerste experiment lijkt aan te tonen dat de tijd die een proefpersoon spendeert aan een doelgerichte taak onder identieke condities gelijk blijft. In een tweede onderzoek bleek dat proefpersonen verschillende mentale strategieën, meestal visueel van aard, toepassen als zij een systeem hun wil moeten opleggen. De twee laatste onderzoeken toonden aan dat verschillend gerichte strategieën waarin een mentaal beeld wordt gehanteerd een verschillende tijd vergen. Die verschillen worden beïnvloed door het al dan niet geven van feedback aan de proefpersoon over zijn mate van succes. Het artikel behandelt ook implicaties voor verder onderzoek naar beïnvloeding via wilskracht.

Vorschlag eines Forschungsparadigmas für die Untersuchung von Willensstrategien

Zusammenfassung: Dieser Aufsatz berichtet über vier an der Universität Edinburgh durchgeführte Untersuchungen. Sie sollten anhand einer experimentellen Versuchsanordnung aus der Parapsychologie Möglichkeiten erkunden, mit Willensakten verbundene Merkmale zu operationalisieren. Die erste Untersuchung legte den Schluß nahe, daß die Zeit, die Versuchspersonen für eine willentliche Aufgabe aufwenden, in wiederholten Versuchen bei ansonsten unveränderten Bedingungen gleich bleibt. Die zweite Untersuchung ließ darauf schließen, daß die Mehrzahl der Versuchspersonen im Interesse der Erreichung ihres Zieles verschiedenartige mentale, zumeist vorstellungsgebundene Strategien ausprobieren, wenn sie Willensaufgaben zu lösen haben. Die Ergebnisse der dritten und vierten Untersuchung sprachen dafür, daß der Einsatz unterschiedlicher vorgegebener vorstellungsgebundener Willensstrategien unterschiedliche Zeitspannen in Anspruch nimmt, und daß diese Unterschiede davon beeinflußt wurden, ob die Versuchspersonen Feedback über ihrer Aufgabenbewältigung erhielten. Mögliche Konsequenzen dieser Befunde für die Erforschung von Willensstrategien werden diskutiert.

Una proposta di paradigma di ricerca per lo studio dell'elaborazione mentale volitiva

Sommario: L'articolo riferisce di quattro studi condotti all'Università di Edimburgo per esaminare, con una procedura sperimentale di tipo parapsicologico, la possibilità di processi operazionali correlati alla volizione. Il primo studio ha indicato che il tempo impiegato dai soggetti su un compito volitivo rimane immutato in occasioni successive, se le condizioni si mantengono identiche. Il secondo studio ha suggerito che alle prese con un compito volitivo, nel tentativo di aver successo, la maggior parte dei soggetti tenta strategie mentali diverse, per la maggior parte basate sull'*imagery*. Gli ultimi due studi hanno indicato che, se vengono assegnate, strategie volizionali diverse richiedono tempi diversi per essere eseguite e che le differenze dipendono dal fatto che i soggetti ricevano o meno un *feed-back* sulla loro riuscita nella prova. Vengono inoltre discusse le possibili conseguenze di questi dati sulla ricerca in merito alla volizione.

The Model of Pragmatic Information (MPI)

Walter von Lucadou
University of Utrecht

Abstract: This paper describes the Model of Pragmatic Information (MPI), which is based on the fundamental assumption of an isomorphism between the axiomatic structure of quantum phenomena and that of psi phenomena. The MPI is distinguished from other observational theories because it operates at the system-theoretical level, enabling it to consider psychological concepts without specifying the physical basis of psi phenomena. The reliability and autonomy of an organisationally closed system are postulated to interact with its environment by the exchange of pragmatic information. This exchange can be called a measurement. Only self-referential systems can become organisationally closed, requiring, in macroscopic systems, a degree of complexity and internal informational exchange. Independent measurements of organisationally closed systems exhibit characteristic correlations that mirror the symmetries and the conserved entities that generate the system. These correlations are non-local in nature and can be isolated from causal correlations. It is assumed that psi effects are manifestations of such non-local correlations. From this model, several limitations of psi effects can be predicted. Finally, the paper reports on several empirical findings that support the model of pragmatic information.

Since the Geneva conference on 'Quantum Physics and Parapsychology' in 1974, a new area of theoretical parapsychology has developed. This does not mean, however, that there were no earlier theoretical approaches that would not be worthwhile to consider as useful models for psi phenomena; but most of these hypotheses were proposed by individual scientists without causing a general discussion that led to the development of research programs. In the case of the so-called observational theories, however, several scientists have contributed different approaches that share a common starting point and that can be compared in relation to different experimental predictions.

An earlier version of this paper was presented at the 30th Annual Convention of the Parapsychological Association, held at Edinburgh, Scotland, August 5-8, 1987.

This basic starting point of the observational theories (see Millar, 1978) can be seen in an alleged isomorphism between the structure of quantum phenomena and that of psi phenomena. This isomorphism is mainly seen in the non-locality of the wave function in quantum physics and the seeming independence of space and time of psi phenomena. Another aspect of this isomorphism is the role of a measurement in quantum theory and the role of an observer in parapsychology. However, it is not the aim of this paper to give an overview on observational theories, but merely to discuss the state of the art of a special version that was proposed by the author and his colleague (Kornwachs & v. Lucadou, 1977, 1979; v. Lucadou & Kornwachs, 1975, 1977) some years ago to avoid certain problems that are inherent in the other, competing observational models.

A Systems-Theoretical Approach

The basic difference between the model of pragmatic information and the other observational theories (OTs) is that it does not start at the level of quantum theory but on a very general level of systems theory (v. Lucadou & Kornwachs, 1980). This means that the model does not say anything about the physical basis of psi phenomena, nor does it introduce the problem of reductionism (v. Lucadou & Kornwachs 1983a). Walker's model (Walker, 1975), for instance, is reductionistic in that it identifies consciousness with the wave function of quantum physics which can be conceived as a kind of physical entity. This is one of the basic reasons why Walker has great problems in incorporating detailed psychological features into his model. If he wants to do this, he has to solve the mind-brain problem first. He tries this by calling upon some neurophysiological considerations (Walker, 1979) but this is still far from the level of psychological descriptions. Schmidt's model (Schmidt, 1975) on the other hand is not reductionistic but purely phenomenological. However, so far it has not been detailed on the 'internal' properties of the 'psi source' that would be necessary to incorporate psychological features.

The advantage of system theory is that it can be applied to psychological problems as well as to physical problems simply because it does not say anything about the underlying basis of a phenomenon but only deals with the structure of our description language and the way we represent and map information derived from experiments on this descriptive language. The process of operationalisation and interpretation describes the interface between the 'real world' (whatever this may be) and our description of it.

The Basic Structure of Systems

The very basic assumption of the model of pragmatic information says that any

description of nature must have a structure that is isomorphic with the axiomatic structure of quantum theory. However, this does not necessarily imply that we can transpose without further assumptions the detailed structure of a special quantum physical system to another field as is done in the observational models. This is because the axiomatic structure of quantum theory (QT) (see Jammer, 1974) can clearly be separated from the detailed structure of the physics that is expressed in the properties of the observables and their corresponding operators. For different problems the corresponding operators might look quite different!

The model of Jahn & Dunne (1986) also makes explicit use of this isomorphism to present a new approach to a psychological theory. The main difficulty they have to solve, is to find plausible transformation rules to interpret the quantum theoretical structure of, for instance, atomic systems in terms of psychology. In principle our approach is very similar but starts at a more general level (see v. Lucadou, 1974).

There are several arguments for our basic assumption. The simplest would be that QT is the most successful basic description language of natural systems and hitherto no indications have been found that the axioms of QT have failed. Further, these axioms hold from microscopic to macroscopic and even cosmological dimensions and also to any sort of physical observables regardless of which special field (electromagnetism, elementary particles, solid state physics, etc.) is considered. Furthermore it can be shown that these axioms describe in a very general way how information concerning space and time (see v. Weizsäcker, 1985) can be obtained from any system in which the action of the 'measurement process' cannot be neglected.

An important consequence of the basic assumption is that structure (S) and function (F) of a system (from now on, for brevity, I will simply use the word 'system' instead of 'system description') have to be considered as complementary concepts in the quantum theoretical sense (for details

see v. Lucadou, 1989). Complementarity in this context means that we cannot measure S and F simultaneously in a natural system. Formally, we can write that the commutator

$$[S * F - F * S] \neq 0 \text{ or } S * F \neq F * S \quad (1)$$

which means that we will get different results in a measurement according to whether we measure the structure first or the function. To any biologist who wants to investigate the behaviour (function) or the anatomy (structure) of an animal, this statement sounds trivial. But as a consequence we have to formulate an uncertainty relation, namely:

$$dS * dF \geq i \quad (2)$$

which means that the product of the variance of the structure dS and of the function dF cannot be smaller than a certain value i . That implies that we cannot measure both S and F with infinite precision.

Now we have to ask what i means in the inequality (2). In the case of physics we can identify it with (Planck's constant), which has the dimensions of an action (ml^2t^{-1} , m = mass, l = length, t = time). If one introduces a generalised dimension of length l and mass m in system theory (see v. Lucadou, 1989) one can show that i also has the dimensions of an action. We will call i the minimum action of pragmatic information.¹

¹ $\text{Dim}(A*B) = G(ml^2t^{-1})$ means that the physical dimensions of the product of the observables A and B should be a mathematical function G of the dimensions of an action (m = mass, l = length, t = time). In the simplest case, the function G is an equality. For the categories of structure S and function (or behaviour) F it can be made plausible that they meet the above stated condition. Any function of input, output or state transition that describes F must be given as a function either in real time $f(t)$ or in terms of system steps $f(i)$. In both cases they contain information on changes per time interval such as information rates, counting rates, switching rates and so on. The dimension of a rate is (t^{-1}) . The category of a

Pragmatic Information

The concept of pragmatic information has been developed (see v. Weizsäcker, 1974; Kornwachs & v. Lucadou, 1982) to quantify the meaning of a given piece of information. It is assumed that the (potential) action that a meaningful piece of information exerts on a system can be used to make such a quantification. V. Weizsäcker proposed that pragmatic information (I) could be written as a product of two observables that he called 'Erstmaligkeit', E (novelty) and 'Bestätigung', B (confirmation):

$$I = E * B \quad (3)$$

This approach takes into account the fact that each piece of information must contain not only some pre-existing structure — for instance, one's native language — in order to be understood by the (receiving) system ('confirmation') but

structure is more complicated. By definition we assume that it does not contain the concept of time. This does not mean that structures may not depend on time. Certainly, structures may be spread out in space but it is not clear how to give a more precise description in general. A good starting point may be to look at a connectivity matrix where the structure of a system is described by its elements and their connections with each other. The connectivity matrix M_{ij} is fully determined by the three values M, i, j where M describes the nature of the connections and i and j are the two elements that are connected with each other. It is obvious that i and j have the same categorical status, which means that it is useful to assume that they have the same dimensions. M defines not only the nature of the connection but also the nature of the elements. Thus the categorical meaning of M is a property of the elements like the mass of a solid and therefore we could describe its dimension as a generalised mass. The categorical meaning of i and j contains the notion of a distance, otherwise it would not be necessary to 'connect' two different elements. Therefore it is useful to attribute to i and j independently the dimension of generalised length. As a result we get for the dimension of structure $\text{Dim}(S) = (ml^2)$. If we take the product of S and F we can indeed fulfil condition $\text{Dim}(A*B)$ above.

also something new in order to produce a change in the receiving system ('novelty'). For instance, a joke in a foreign language that you do not understand would not cause you to laugh (no 'confirmation'), and neither would an old joke, because it lacks novelty.

The model further assumes that there exists a minimum amount of pragmatic information (or action) i that has to be exchanged if an informational exchange (measurement) with another system takes place. This is simply another formulation of the inevitable interaction in a measurement.

Let us now assume that we transmit a piece of pragmatic information to a system (person) in two different languages, say, in an English and a German sentence. If the system is able to understand both sentences we can say that the system has got more confirmation (B) than if only one sentence had been (partly) understood. From the point of view of the receiving system, this means that it has a higher structural variance (dS) because it can potentially react to (or understand) different pre-existing structures (for instance, two languages). If the system is also able to incorporate much novelty (E) this would mean that it is able to process much pragmatic information. Of course much novelty can only be incorporated if the system is able potentially to react to it or, in other words, has a sufficient (potential) functional variance (dF). As a result we can connect structural variance (dS) with confirmation (B) and functional variance (dF) with novelty (E) of an incoming item of pragmatic information (I). Thus we can reformulate the inequality (2) in the following way:

$$E * B \geq i \quad (4)$$

If we consider the (potential) reaction of the system to the given pragmatic information, it may lead either to a change in structure, S , or behaviour (function), F . Potential changes in behaviour can be described as autonomy (A) of the system. If, on the other hand, the potential action of the system results in pure changes of the

structure this can be described as reliability (R) because the system has compensated for the pragmatic information without changing its function. In most cases, however, both S and F may change, but in any case the 'action' of the pragmatic information (I) has to be preserved. This leads to the equations:

$$I = R * A \quad (5)$$

$$I = n * i \geq i, n \in N \quad (6)$$

Equation 3 can be considered as the description from 'outside' and Equation 5 from 'inside' the system. If we put (3), (4), (5) and (6) together we get:

$$B * E = R * A = B' * E' = n * i \quad (7)$$

in which B' and E' belong to a different receiving system (see below).

This fundamental equation describes the action of a given piece of pragmatic information on a system. Because R and A , or B and E are corresponding pairs of complementary observables nothing can be said concerning a partitioning of an external piece of pragmatic information (I) from reliability (R) and autonomy (A) 'inside' of the system without further specifying the 'measurement' (see also Equations 16 and 17 below).

From Equation 7 one can conclude that any piece of pragmatic information (I) that interacts with a system also produces pragmatic information, but with a new partitioning of novelty E' , and confirmation B' , which can be interpreted as the 'reaction' of the (sub)system. Therefore, it is an important question to specify the subsystem; or, to be more precise, the boundary of the subsystem. Some formal criteria have been given in a previous article (Kornwachs & v. Lucadou, 1975) but here I will introduce another concept that is very useful for understanding the model in relation to experimental applications.

Organisational Closure

The new concept to describe the boundaries of natural systems was introduced by Maturana and Varela and is called 'organisational closure'. Varela (1981) states: 'An organisationally closed unity is defined as a composite unity by a network of interactions of components that (i) through their interactions recursively generate the network of interactions that produce them, and (ii) realise the network as a unity in the space in which the components exist by constituting and specifying the unity's boundaries as a cleavage from the background.' It is interesting to note that the concept of organisational closure makes no sense from inside of the (sub)system!²

A necessary condition of organisational closure is that the system is self-referential in nature. However, this does not necessarily mean that such systems need to be very complex. For instance, an atom can be regarded as such an organisationally closed system. In this case the 'interactions' are described by the specific form of the Hamiltonian H and the 'network'; the set of eigenfunctions is recursively generated by the Schrödinger equation. The constraints that produce 'unity's boundaries as a cleavage from the background' turn out to be conservation laws (of energy and angular momentum) or, more generally speaking, symmetries of the system. In general, Varela has shown that self-referential systems that are organisationally closed can be described by eigenequations such as

$$J|x\rangle = j^*|x\rangle \quad (8)$$

which have the same structure as the Schrödinger equation. However, macroscopic systems need a certain complexity to become self-referential (see Equation 10). There seems to be a (complexity) gap between microscopic and macroscopic self-referential systems!

² See also the exo-endo distinction of Primas (1992).

Neumann (1966) has shown that algorithms that are able to reproduce themselves in a computer system need a certain complexity or minimal program length to work. This is obvious because they need a kind of 'inner representation' (blueprint) of themselves. Self-referential systems by definition have such an 'inner representation'. The question of whether microsystems need no such minimum complexity is still unclear. Some authors assume that microsystems also have a kind of 'inner representation' (see for instance Berezin & Nakhmanson, 1990). According to the usual argument, self-reference in microphysics emerges from the fact that the measured object (micro-particle) is of the same order of magnitude as the 'measurement device' (also a micro-particle).

In general, pragmatic information can deliver a constraint that defines the boundaries of an organisationally closed system. The experimental setting of a PK experiment can serve as an example. If the subject does not receive any instructions or feedback, the subject and the (quantum physical) random source are separated, according to the null hypothesis. It does not matter whether we share Walker's view, which postulates an overlapping of non-local wave functions of the RNG with the consciousness of the subject or whether we adopt Bohm's view of a global implicate order (Bohm, 1980). In either case, there is no sufficient reason to consider RNG and subject as an organisationally closed system. However, as soon as we introduce an instruction and/or feedback the situation changes completely. The given instruction (pragmatic information) changes the system in such a way that now a link is introduced between the RNG events and the psychology of the subject. The random events have acquired some 'meaning' to the subject. Obviously it depends on the experimental conditions (namely the given pragmatic information) how strong the organisational closure can be made. For instance, feedback can be used to strengthen the organisational closure. The display itself need not

necessarily contain much pragmatic information for the subject. It is, however, necessary that the subject can make a distinction between a 'hit' and a 'miss'. According to the observational theories (OTs) no non-feedback experiments are possible because at the least the experimenter gets feedback when evaluating the experiment. The MPI takes this into account by the requirement that the organisational closure between experimenter and random process should be minimised (see Equation 14). From this point of view it is not useful to set up experiments in which the display contains only a little pragmatic information for the subject. (It is important to realise that, unlike the other OTs, from our system-theoretical point of view we need not make any detailed assumptions about a special interpretation of the physical wave-function or of quantum theory).

From this point of view we can distinguish between two different types of pragmatic information that are necessary to describe a psi experiment. The first type is the internal pragmatic information (I_{int}) that constitutes the organisational closure of the system. The second type is the external pragmatic information (I_{ext}) that the experimenter wants to get from the system to learn something about it. I_{int} can also be described as the pragmatic information that defines the 'preparation' of the system and I_{ext} as the pragmatic information that defines the 'measurement' of the system. Thus, for instance, in a PK study I_{int} might be the feedback from the RNG to the subject and I_{ext} is the Z score of the outcome or the experimental results.

The Divergence Problem

The assumption of space-time independence of PK in the OTs creates a lot of problems. It seems to be simultaneously the strength and the weakness of the model. On the one hand, it takes into account the most obvious features of experimental results. On the other hand, it produces the divergence problem. There is

even the danger that it makes the model irrefutable.

There have been several approaches to solving the divergence problem, which says that one cannot operationalise how future observers might influence the outcome of a PK experiment. Most of these models assume that the psi source of future observers would statistically cancel out among each other (see Schmidt, 1978; Walker, 1977).

Houtkooper (1983) was the only one who, independent from our approach, has tried to solve the problem on a more 'system theoretical' basis by describing the role of measurement and observer in a 'hierarchical model'. The basic idea of this model is that the 'psi source' of subsequent observers in a PK experiment is not only 'absorbed' by the pure (quantum mechanical) random event as in Schmidt's model but also by the previous observer. It is obvious that the word 'absorption' is one of these classical metaphors that must be used due to the lack of proper 'nonclassical' words because, as Houtkooper rightly emphasises, it does not imply the notion of transfer or transport. Moreover, it is not clear what is meant by a 'later' or 'subsequent' observer if psi *per se* really is time independent. In this respect, his solution to the divergence problem seems to create an inconsistency in the model. Thus, it would be necessary to find an independent variable that could function as an ordering parameter within this hierarchical model. It is a pity that Houtkooper did not borrow from our model of pragmatic information, because that would have provided him with a solution to the inconsistency of his model. However, it seems that he was very close to this solution. He writes that the 'preceding observer and the system (of observation)... is one level less complex... which may have some consequences for the absorbance if this parameter is dependent on the meaningfulness of the information in the feedback to the subject' (p. 64). This is exactly what the model of pragmatic information describes. The ordering parameter for such a hierarchical model

can be given quite naturally by pragmatic information involved at every observation.

To put it more precisely, the distinction between the pragmatic information of preparation (I_{int}) and measurement (I_{ext}) enables one not only to solve the measurement problem in quantum theory (see v. Lucadou, 1989) but also the divergence problem in OT as well. The ratio of I_{ext} to I_{int} is a measure of the presence of an experimenter effect. If (as an extreme case) the interaction of the experimenter I_{ext} were larger than I_{int} this would mean that the organisational closure of the system — RNG plus subject plus experimenter — was stronger than the organisational closure of the (sub)system — RNG plus subject - alone. In this case we would expect the experimenter effect to dominate. Thus, in a good experimental design the probability p of an experimenter effect should be small:

$$p = \frac{I_{\text{ext}}}{I_{\text{int}} + I_{\text{ext}}} \ll 1 \quad (9)$$

Hence, the experimenter, for instance, should not observe the feedback display during the experiment. On the other hand, this equation also gives a criterion for how to enhance possible psi effects in an experiment. The model simply says that we have to increase I_{int} .

What Is a Psi Effect?

The most important difference between a psi experiment and an experiment in other fields of science is that, due to the definition of psi, the 'normal causal' links between subject and target have to be ruled out, whereas in 'normal' science the structure of such 'normal' causal links is investigated. Of course, this definition is highly problematic (see v. Lucadou, 1984) but it seems that most researchers in the field are beginning to share a minimum consensus that psi effects could be positively defined as 'meaningful non-local correlations' between a living system and other systems separated from it. Thus, one could redefine parapsychology as the

investigation of non-local effects in living systems. The term 'non-local', however, is not just a new word for 'psi' but has a quite specific meaning in the context of the axiomatic structure of quantum theory.

It is important to notice that non-locality is no different in principle from all other physical interactions: but to isolate it for investigation, other interactions have to be ruled out. In physics, non-locality can be considered as a more or less well established fact, but most researchers doubt that it might play a role in living systems.

How Can Psi Effects Be Increased?

A main disadvantage of the OTs is that they do not give any practical hint on how to optimise the outcome of a psi experiment. In Schmidt's model, which is mainly goal oriented, nothing can be said about the size of the effects except that they are assumed to be small. At a first approximation it is assumed that the 'psi source' is constant but it may depend on psychological conditions. In this model the whole variance of the effect is explained in terms of (unknown) psychological conditions. In Walker's model there is at least some dependence on the physical process (the divergence of the quantum mechanical random process), which implies that the psi effect could be optimised by choosing such physical systems as a random source in a PK experiment that would allow a stronger effect (see also Mattuck & Walker, 1979). In Walker's model it might also be possible to manipulate the ratio C/W of the 'conscious data rate' (C) and the 'will data rate' (W) in the brain of the observer to optimise the PK effect. But in practice this would require the solution of the mind-brain problem; namely, how to connect psychological variables with neurophysiological ones.

In the model of pragmatic information, it is possible to give a clear and operationalisable criterion of how to optimise a possible psi effect. The advantage of the model is that it does not only give some necessary conditions for a psi effect (as do the psychological models

of Stanford (1974) and Batcheldor (1979), for example), but in principle also sufficient ones (however, see the following section). A further advantage is that both physical and psychological variables can be incorporated in principle.

We have learned from the section above on the divergence problem that, generally speaking, we have to optimise the organisational closure of the system or the internal pragmatic information I_{int} to get stronger non-local correlations or psi effects. However, what does this mean in detail? For brevity, we will only discuss this question in the context of a PK experiment, but the discussion can easily be transposed to any other psi experiment.

Of course, the model does not deny the role of motivational factors or personality characteristics (states and traits) of the subject in a PK experiment, but here we will only discuss such aspects as can easily be manipulated in an experiment.

Because feedback is the only (one-way) link between the quantum process and the subject, organisational closure can only be established by the pragmatic information that is delivered by this channel. In an experiment (v. Lucadou, 1986a, b, 1987) I did indeed show that a specific operationalisation of the pragmatic information of the display as a function of I_{int} showed the strongest non-local correlation to psychological variables. This means that the display information should be meaningful to the subject. However, this does not necessarily mean that the subjects have to understand the experimental setting; this may even be disadvantageous (as we shall see in the following section), but it should cause (mental) changes in the subject.

As we have already discussed in an earlier section ('Pragmatic Information'), pragmatic information is a measure for the changes it causes in a system. On the other hand, changes in a system 'produce' pragmatic information. Formally, we can express this as follows:

$$I = f(C, dC / dt) \quad (10)$$

The changes in the system are measured in terms of changes of complexity (dC/dt) in the system. Elsewhere (Kornwachs & v. Lucadou, 1975) we have described how in principle a measure of complexity can be given. At a first approximation we can neglect the fact that the pragmatic information (I) may also be a function of the complexity (C) itself. Thus we can write:

$$I = f'(dC / dt) \quad (11)$$

This simply means, for instance, that a surprising display would do better than a boring one. But only during the period of surprise, when the mental system undergoes a change, would a psi effect be expected from this model.³ Another possibility would be that the subject has to learn the 'meaning' of a display (Schouten, 1987). The model predicts that during the learning period the non-local correlation would be stronger than before and afterwards.

Obviously, this is in contrast to the usual observational theories where one would expect a larger psi effect after the learning period when the meaning of the display has been learned by the subject.

But the necessary changes in the system could also be induced willingly by the subjects themselves; for instance, by changing their internal attitude towards their task during the experiment. During such changes it should be easier to obtain a psi effect. In fact, many subjects report such experiences. Further, the decline effect or the release of effort effect would get a rather natural explanation, assuming that

³ The model predicts that the amount of temporal change in complexity determines the size of the psi-effect. This includes the prediction that surprising tasks should do better than boring ones. If a dynamic task should be a very boring one it would do less well than an exciting static one, but normally dynamic tasks are less boring than static ones. Those that involve heavy information-processing capacity might also be boring.

mental changes are very likely to occur at the beginning of a run or after the longer period of an unsuccessful trial when the subject 'gives up'.⁴

The model also sheds some light on the question of whether it may be possible to train psi. Since the effect is dynamic in nature, one would not expect the usual learning curve with a final saturation effect because saturation is antagonistic to dynamics. However, it might be possible for some persons to learn to keep their mind 'destabilized'. Qualitatively this fits with the reports of some mystics, who emphasise the dynamic structure of their mental states (often they say that the way is more important than the aim). During such meditation exercises allegedly paranormal events are often reported (see Thurston, 1956).

We may see from these examples that the dynamic structure of pragmatic information can indeed be used to optimise the organisational closure of the PK system; however, it also limits very much its size and extent of its practical applications.

Non-Locality and Influence as Conflicting Concepts

For the detection of a non-local correlation one has to compare two independent sets of data such as the psychological variables of the subject and the outcome of the RNG. If the usual physical influences (and artefacts) are ruled out and one observes a correlation between the independent (pre-test) measured psychological variables with physical variables (for instance, the hit rate) one can conclude that a PK effect or a non-local correlation is responsible for this correlation. Non-locality, however, implies that there cannot be a specific signal (tracer) on the random sequence alone,

which could be used to find the 'PK-effect' on the RNG data without comparing them with independent psychological or situational data. This means that the non-local correlation can only be found post hoc by comparison and cannot be used as a normal means of signal transfer. This especially holds true for the famous EPR correlation. Often, it has been argued that one could use the EPR correlation as a superluminal telegraph (see Costa de Beauregard, 1981). However this could not be done on the basis of physics alone because the distribution of the spin measurements on each side of the EPR system is totally isotropic. Only a coincidence measurement can reveal the non-local correlation. Therefore, Costa de Beauregard proposed using a PK 'influence' to bias the distribution of the spin measurement at one side of the system. As a consequence, in this case, there would indeed be a superluminal transfer of information to the other side of the system due to the physical non-local correlation. However, if it should turn out that PK *per se* is 'only' a non-local correlation and not an 'influence' (see v. Lucadou, 1984), as we have assumed in our model, this would imply that it would only 'prolong' the physical non-local correlation by a PK one, but not 'influence' the distribution of the spin measurement. In this case, Costa de Beauregard's superluminal telegraph would not work.

In the PK experiment already mentioned above (v. Lucadou, 1986a, b, 1987), special methods were used to find such a tracer; however, it turned out that PK correlations between psychological and physical variables could be found, but no signal transfer or tracer. This result supports the assumption that PK is indeed a non-local correlation (see v. Lucadou, 1984, 1986a, b, 1987). Similar results were obtained in a meta-analysis of a large database of PK experiments, where the data also supported the assumption that the RNG is not 'influenced' (see May, 1984; May, Hubbard & Humphrey, 1985). This led to the so-called 'intuitive data sorting' (IDS) model. This model, however, has

⁴ Whether mental changes due to a release of effort or due to learning would show a negative or positive sign regarding the change of complexity depends very much on the context of the experimental setting. Thus far the model does not specify this point in detail. It should be investigated experimentally.

some internal inconsistencies (Walker, 1987) which we cannot discuss here and it also does not fit with our data (see v. Lucadou, 1986b).

On the other hand, one could argue that the non-local correlation is only a part of the PK interaction and that the feedback loop is the other part that guarantees a normal signal transfer in such a way that PK can become a real influence. This is a basic assumption of Walker's model but is also inherent in the other OTs. At first glance, this argument seems to be convincing but I think that it contains a basic inconsistency with the underlying axiomatic structure of quantum theory. Formally, one can see this difference between Walker's model and our model in the way the concept of information is used in the respective models. Walker uses Shannon's concept of information which contains the notion of a signal transfer and connects it with the 'collapse' of the wave

function ($\psi_0 - \psi_1$):

$$W * t = \log p(\psi_0 - \psi_1) \quad (12)$$

In our model no 'collapse' of the wave function occurs because non-locality of the wave function has to be 'preserved' within the organisationally closed system; it generates the organisational closure. Consequently, in our model we use the concept of pragmatic information which is not a scalar function like Shannon's but an observable or operator:

$$I = P(\psi_0 - \psi_1) \quad (13)$$

This approach is consistent with our basic assumption described in an earlier section ('A Systems Theoretical Approach') that the underlying structure of any system is the axiomatic structure of quantum theory. There is no place for a 'collapse' of a wave function. In his critique of our model, Walker (1984) has not realised that this is a

fundamental difference (among others) between these two models.

It is obvious that the assumption that PK is by its very nature a non-local correlation, limits very much its operation because, whenever it is 'used' like a normal signal, energy or force, it must necessarily disappear. We have tried to make this concept plausible with a 'thought experiment' with pre-recorded targets (PRTs) (see v. Lucadou, 1983, 1992) where any pre-inspector would make the PK effect vanish if and only if he or she had received the full pragmatic information on the system $I_{\max}$ with

$$I_{\text{int}} + I_{\text{ext}} \leq I_{\max} \quad (14)$$

enabling him or her potentially to 'use' the pre-recorded sequences as a carrier of information. In the meantime such experiments have been carried out under several conditions that allow the application of our model. In any case, the predictions of our model were verified by these (independent!) experiments (see Schmidt, 1976, 1981, 1984a, 1985, 1986).

These experiments by Schmidt can be summarised according to our model (Equation 9) in the following way. A second observer of a sequence of PRTs could exert a PK effect on it if and only if he or she undergoes an organisational closure with the system that is stronger than the organisational closure of the first observer (or pre-inspector) and the first observer has not received $I_{\max}$ during his or her experimental session. This was the case in the 1976 and 1981 studies in which no pre-inspector was present or had insufficient pragmatic information to interact (potentially) with the system. In the 1984a, 1985 and 1986 experiments, due to trial-by-trial feedback, the first observer might have received an amount of pragmatic information, I_{int} , not much smaller than $I_{\max}$. Applying Equation 9 we would expect a probability of $p < .5$ of obtaining a second observer effect. Indeed it turned out that the second observer did not play any role.

The goldfish experiment (Schmidt, 1986) is of special interest because it turned out that the goldfish did not produce a significant PK effect and that this result could also not be 'changed' by the subsequent observer. Obviously, the goldfish received strong feedback information I_{int} due to the electrical shocks, but on the other hand, the unobserved non-local correlation did not lead to a significant deviation. However, because a later observer obviously had no 'further' effect on the system we can assume that the unobserved organisational closure was rather strong. This could be interpreted as indicating that the goldfish was not able (due to its small IQ or complexity) to 'know' which state of the system could have been favourable to it. Schmidt (1984b) interprets his results as indicating a 'partial collapse' of the wave function, a notion that I have already criticised above.

The same arguments can be applied to precognition experiments where the use of a checker may result in there being two competing levels of organisational closure: either the checker or the subject could be responsible for the extra-chance ESP hits. In an actual experiment it may become rather difficult to define clear-cut boundaries because they depend on the (implicit) operationalisation of the 'meaning' of the given (feedback or instruction) information. Details of this argument can be found in Weiner & Zingrone's experiment on the checker effect (1986): their results seem to be in general agreement with our model.

It is important to realise that the statement that PK will disappear whenever it is used like a 'normal' signal or force, only makes sense in a given experimental setting. If the pragmatic information concerning the setting would allow such a possibility, the PK effect would have to disappear, but it does not matter whether this possibility is actually used. This is similar to the situation where a particle detector destroys the interference pattern in a dual slit experiment regardless of whether the detector actually finds a particle or not. In our case, this means that

the 'potentiality' of the given pragmatic information limits the PK effect.

However, this does not completely exclude a somewhat purposeful 'use' of non-local correlations. If the subject could not only undergo an organisational closure with the separated system but simultaneously 'observe' the states of this total system from a 'second' hierarchical level of 'awareness' or 'system description', then the subject could select those (eigen)states that are favourable to him or her. This could also be described as an organisational closure on a second hierarchical level (see Equation 10) or as BOOLE (breaking out of loops everywhere) as Hofstadter (1982) called it. Of course, this is no normal signal transfer because only 'allowed' states can be 'selected' and it may be very difficult to select them without disturbances (see also the discussion in the section below on what is a psi effect). The goldfish in Schmidt's experiment obviously could not make such a selection. With the OTs this result cannot be understood because the goldfish did not collapse the wave function (there was no deviation from chance).

However, this is not the only severe limitation for a possible PK effect in an experiment.

The Limitations of a Psi Effect

In the preceding section I mainly referred to psi as a non-local correlation; however, normally a psi effect is considered as something else. Traditionally, it is defined as a (significant) deviation from chance expectation. This need not of course be in conflict with the idea that PK is basically a non-local correlation rather than a signal because under certain experimental or situational conditions, the PK correlation can 'express' itself as a significant deviation from chance or, to be more precise, as a 'selection' of a chance fluctuation that, as a selection, differs significantly from the remaining ensemble. Extra-chance fluctuations are normally regarded as 'psi-hitting' or, if negative, as 'psi-missing'. According to the MIP these

fluctuations correlate with independent psychological variables relating to the subject such as extraversion, the sheep-goat variable, and so on. It is important that these variables are measured before the PK experiment. To measure the correlations it is necessary to test more than one subject (as many as possible because the correlation coefficients are very small). So far our model is in agreement with the IDS model, but the 'selection' occurs not because of the subject but as a consequence of the hierarchically nested organisational closure.

If PK can be regarded as something 'exceptional', within our model this means novelty (E). This is especially so in spontaneous cases where psi effects normally occur unexpectedly. We can also say that the pragmatic information that describes the non-local correlation is mainly represented as novelty (E). Implicitly this means that there is not much confirmation (B) present. Since, due to Equation 4, pragmatic information is the product of novelty (E) and confirmation (B), the same amount of pragmatic information can be expressed either as much novelty and little confirmation or vice versa. However, if the total amount of pragmatic information is limited, it follows immediately that events containing much novelty, E (unexpected events), cannot occur very often, which would mean very much confirmation (B).

In an earlier section ('Pragmatic Information'), I said that the partitioning of pragmatic information into novelty and confirmation depends on the measurement we apply to get the information (I) from the system. Thus, the experimental conditions mainly determine whether we get mainly novelty or confirmation or both in equal measure from our organisationally closed system. Assuming we could perform two PK experiments where all conditions except the run length could be kept identical, (in practice this would of course be very difficult) and assuming further that the Z-score of our PK experiments was a good measure for the PK correlation then we could conclude from our model that the hit

rate, H, depends on the run length (n) in the following way:⁵

$$H(n) = c/\sqrt{n} \quad (15)$$

This means that the integrated amplitude of the 'extra-chance fluctuation' of this run must decline with run length. Here we have made the assumption that the Z-score is a meaningful measure for the psi effect which means that it is used as the criterion for the subject to indicate the success of fulfilling the experimental instruction. Other criteria would of course yield different functional dependencies in Equation 15.

This consideration would also naturally explain why the increase of the measurement resolution in PKMB experiments did not lead to an equivalent increase in the measurement signal (see Hasted, 1981).

This model has also been applied in a qualitative way to the description of RSPK cases (see v. Lucadou, 1982). In this case, we have to start with Equation 7 which describes the system both from 'inside' and 'outside', because it is no longer possible to define sharply the boundaries of organisational closure in the system according to the criterion of Equation 8.⁶ Of

⁵ Equation 15 is derived as follows. The criterion that is used to judge whether an experiment is 'significant' is the Z-score or the p-value. It is based on the statistical accumulation of the psi-effect. The MPI, however, assumes that the yield of pragmatic information of an experiment depends only on the experimental setting (such as psychological and situational variables) and not on the number of trials (insofar as it is not a relevant variable). Concerning the experimenter, the Z-score is usually a measure of pragmatic information. If everything else could be kept constant except the number of trials, the Z-score would be constant for these experiments. The hit rate $H(n)$ of a binary sequence (0,1) is defined as the number of hits minus the mean chance expectation, $e=np$ divided by the number of trials n : $H(n) = (h-e)/n$. The Z-score is $Z = (h-e)/\sqrt{npq}$. Since the model assumes that Z , p , and q are constant, we get Equation 15: $H(n) = \text{const.}/\sqrt{n}$.

⁶ RSPK cases are, at least at the beginning, completely different from experiments because the situation is not at all defined and set up by an

course, one could primarily consider the focus person and his or her environment (the 'focus system') as an organisational closure but the interactions with, for instance, the family or other 'observers' and finally with the whole social environment are so strong that the boundary of the organisational closure is always changing. In Equation 7 autonomy (A) and reliability (R) describe RSPK phenomena in the focus system. The recurrence of the phenomenon is a mark of the reliability (R) of the focus system. Any interaction of this system with other 'observers' must be described as an exchange of pragmatic information (I). However, not only does the focus system deliver pragmatic information, but also the observers 'produce' pragmatic information that manipulates the focus system, for instance by their expectations. For example, if these 'observers' 'expected' more confirmation (B) of the focus system for a specific 'surprising' phenomenon x , such that the product $E(x) \cdot B(x)$ exceeded the product $I = A \cdot R$ (according to Equation 7), the phenomenon might produce a displacement in such a way that something unexpected happens, for which $E(x') \cdot B(x') = A \cdot R$ holds. Again, we must emphasise that 'expectation' in this context means a pragmatic criterion; namely, a (potential) measurement of x and not only a mental attitude. According to Equation 8 we write symbolically:

$$E|x \rangle = e \cdot |x \rangle \quad (16)$$

or

$$B|x \rangle = b \cdot |x \rangle \quad (17)$$

experimenter. Spontaneous events occur that cannot be planned. At the beginning of an RSPK case no one knows what is relevant for the system — what belongs to the system and what does not. In later phases of RSPK cases the observers often begin to 'experiment'. These phases can be compared to experimental situations. For this reason the MPI predicts (see v. Lucadou, 1982) a decline phase for RSPK cases which can actually be observed (See Huesmann & Schriever, 1989).

respectively. The installing of a video recording system may serve as an example of something that might record a larger amount of pragmatic information $I = B \cdot E$ than the focus system could afford. From this example we can see that the model is able to give a natural explanation of the remarkable 'elusiveness' of RSPK phenomena. A closer consideration (see v. Lucadou, 1982) shows that the model reveals four distinct phases of the time development of RSPK cases that are called: 'surprise', 'displacement', 'decline' and 'inhibition'.

It is important to note that it would be a misunderstanding of the model if one concluded from the latter consideration that psi phenomena could not be recorded at all. The size of the effect that can be recorded depends on the absolute amount of the pragmatic information (I) that is 'produced' by the focus system. This also holds for other spontaneous phenomena. Normally, this absolute amount seems to be pretty small. We have called this empirical finding 'Timm's Rule' (see Timm, 1983), which says that whenever a 'psi event' is 'too reliable or too strong' it is highly probable that it is an artefact or fraud. For instance, Schouten (1982) reports in his analysis of spontaneous ESP cases that the amount of the 'correctly transferred ESP-information' that in the post hoc (!) analysis produces the ESP correlation is only about the magnitude of one bit! Furthermore, he found a kind of uncertainty relation that, roughly speaking, says that either a single fact (for instance the death of a person) is reported correctly but without further details, or correct details are reported but their meaning remains unclear to the ESP subject. This finding is indeed in qualitative agreement with the prediction of Equation 7.

From the previous discussion we can recognise that the model of pragmatic information imposes strong limitations on the practical application of psi effects in the usual science fiction sense (See Roney-Dougal, 1983). On the other hand, the model delivers a lot of detailed predictions for future experiments. It is logical, self-

consistent, and in overall agreement with other natural sciences. Moreover, it has a lot of applications in areas of 'normal' psychology such as perception, pattern recognition, learning and cognition (for details see Kornwachs & v. Lucadou, 1984, 1985). Therefore, I think that the criticisms of Houtkooper (1983) and Rush (1986) who maintain that the model has no predictive power have become obsolete.

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Hildastraße 64
D-79102 Freiburg i. Br.
GERMANY

Le Modèle de l'Information Pragmatique (MPI).

Résumé: L'article décrit le Modèle de l'Information Pragmatique (MPI) qui est basé sur l'hypothèse fondamentale d'un isomorphisme entre la structure axiomatique des phénomènes quantiques et celle des phénomènes psi. Le MPI est distinct des autres théories observationnelles parce qu'il opère au niveau de la théorie des systèmes, ce qui lui permet de prendre en considération des concepts psychologiques sans spécifier le substrat physique des phénomènes psi. On postule que la fiabilité et l'autonomie d'un système clos organisationnellement interagit avec son environnement par l'échange d'information pragmatique. On peut appeler cet échange une mesure. Seuls les systèmes auto-référentiels peuvent devenir clos organisationnellement, ce qui requiert, pour les systèmes macroscopiques un certain degré de complexité et d'échange d'information interne. Des mesures indépendantes sur des systèmes clos organisationnellement présentent des corrélations caractéristiques qui reflètent les symétries et entités conservées qui génèrent le système. Ces corrélations sont non-locales par nature et peuvent être isolées des corrélations causales. On suppose que les effets psi sont des manifestations de telles corrélations non-locales. D'après ce modèle, on peut prédire plusieurs limites aux effets psi. Enfin, l'article rapporte différentes découvertes empiriques qui soutiennent le modèle de l'information pragmatique.

O Modelo de Informações Pragmáticas

Resumo: O trabalho descreve o Modelo de Informações Pragmáticas (Model of Pragmatic Information or MPI) que está baseado no pressuposto fundamental de um isomorfismo entre a estrutura axiomática dos fenômenos quânticos e dos fenômenos psi. O MPI distingue-se de outras teorias observacionais porque opera a nível sistemático-teórico, possibilitando considerar conceitos psicológicos sem especificar o substrato físico dos fenômenos psi. A segurança e a autonomia de um sistema organizacionalmente fechado são postuladas para interagir com seu meio pela troca de informações pragmáticas. Essa troca pode ser chamada de mensuração. Apenas sistemas auto-referenciais podem se tornar organizacionalmente fechados, requerendo, em sistemas macroscópicos, um grau de complexidade e troca de informações interna. Mensurações independentes em sistemas organizacionalmente fechados exibem correlações características que

espelham as simetrias e as entidades conservadas que geram o sistema. Essas correlações são não-locais por natureza e podem ser isoladas de correlações causais. Admite-se que os efeitos psi são manifestações de tais correlações não-locais. A partir desse modelo, várias limitações dos efeitos psi podem ser previstas. Finalmente, o trabalho traz o relatório de diversas descobertas empíricas que sustentam o modelo de informações pragmáticas.

El Modelo de Información Pragmática (MIP)

Resúmen: Este artículo describe el Modelo de Información Pragmática (MIP) que se basa en la premisa fundamental de un isomorfismo entre la estructura axiomática de los fenómenos cuánticos y de los fenómenos psi. El MIP se distingue de otras teorías observacionales porque opera a nivel sistémico-teórico, lo cual le permite considerar conceptos psicológicos sin especificar la base física de los fenómenos psi. Se postula que la confiabilidad y la autonomía de un sistema organizacional cerrado interactúa con su ambiente mediante el intercambio de información pragmática. Este intercambio puede llamarse medición. Solo los sistemas auto-referenciales pueden volverse cerrados en términos de organización, lo cual requiere en los sistemas macroscópicos cierto grado de complejidad y de intercambio de información interna. Medidas independientes de sistemas organizacionales cerrados muestran correlaciones características las cuales reflejan las simetrías y otras variables que generan el sistema. Estas correlaciones son de naturaleza no-local y pueden ser separadas de correlaciones causales. Se asume que los efectos psi son manifestaciones de tales correlaciones no-locales. Este modelo puede predecir varias limitaciones de los efectos psi. Finalmente, este artículo reporta varios hallazgos empíricos que apoyan el modelo de información pragmática.

Het Model van de Pragmatische Informatie (MPI)

Samenvatting: Dit artikel behandelt met "Model of Pragmatic Information" (MPI), gebaseerd op de aanname van een overeenkomst tussen de beschrijvende structuur van quantumfysische en van psi-verschijnselen. In tegenstelling tot andere observationele theorieën is het MPI een systeemtheoretisch model, zodat het psychologische begrippen kan behandelen zonder het fysische aspect van psi-verschijnselen te definiëren. Het model veronderstelt dat een organisatorisch gesloten systeem stabiel en autonoom is en dat de interactie met de omgeving verloopt via het uitwisselen van pragmatische informatie. Die uitwisseling kan als meting worden beschouwd. Alleen zelfregulerende systemen kunnen een organisatorisch gesloten geheel worden. Binnen macrosystemen vereist dat een bepaalde mate van complexiteit en intern informatietransport. Onafhankelijke metingen aan een organisatorisch gesloten systeem leiden tot karakteristieke correlaties die een afspiegeling zijn van de symmetrie en de opgeslagen entiteiten waardoor dat systeem wordt gestuurd. Die correlaties zijn onafhankelijk van tijd en ruimte (non-local) en kunnen los worden gezien van oorzakelijke verbanden. Het MPI ziet psi-verschijnselen als uitingen van dergelijke niet-lokale correlaties. Het model voorspelt diverse beperkingen aan psi-effecten. Ten slotte rapporteert dit artikel empirische resultaten die met MPI ondersteunen.

Das Modell der Pragmatischen Information (MPI)

Zusammenfassung: Dieser Artikel beschreibt das Modell der Pragmatischen Information (MPI), das auf der Grundannahme einer Isomorphie (Ähnlichkeit) zwischen der axiomatischen Struktur von Quanten- und Psi-Phänomenen beruht. Das MPI unterscheidet sich von anderen Observational

Theories (beobachterorientierten Theorien), insofern es auf systemtheoretischer Ebene agiert, weshalb es psychologische Begriffe berücksichtigen kann, ohne das physikalische Substrat von Psi-Phänomenen benennen zu müssen. Es wird gefordert, daß Reliabilität und Autonomie eines organisatorisch geschlossenen Systems mit der Umgebung durch den Austausch pragmatischer Information wechselwirken. Diesen Austausch kann man als Messung bezeichnen. Nur selbstreferentielle Systeme können organisatorisch abgeschlossen werden, was in makroskopischen Systemen einen bestimmten Grad von Komplexität und internem Informationsaustausch erfordert. Unabhängige Messungen an organisatorisch geschlossenen Systemen ergeben charakteristische Korrelationen, die die Symmetrien und Erhaltungssätze widerspiegeln, die das System erzeugen. Diese Korrelationen sind nichtlokal und lassen sich von Kausalkorrelationen trennen. Es wird angenommen, daß Psi-Effekte Manifestationen solcher nichtlokaler Korrelationen sind. Aus diesem Modell lassen sich mehrere prinzipielle Begrenzungen von Psi-Effekten vorhersagen. Schließlich stellt der Beitrag einige empirische Befunde vor, die das Modell der Pragmatischen Information unterstützen.

Il modello dell'informazione pragmatica (MPI)

Sommario: Il lavoro descrive il Modello di Informazione Pragmatica (MPI), basato sull'assunto fondamentale di un isomorfismo tra la struttura assiomatica dei fenomeni quantistici e i fenomeni psi. L'MPI si distingue da altre teorie osservative in quanto opera al livello teorico di sistema, il che gli consente di considerare i concetti psicologici senza specificare il substrato fisico dei fenomeni psi. Viene postulato che l'affidabilità e l'autonomia di un sistema organizzativamente chiuso interagiscano con l'ambiente del sistema per scambio di informazione pragmatica. Questo scambio può venir chiamato misurazione. Solo i sistemi autoreferenziali possono diventare organizzativamente chiusi; quelli macroscopici possono richiedere un certo livello di complessità e di scambio informazionale interno. Misurazioni indipendenti sui sistemi organizzativamente chiusi esibiscono correlazioni di caratteristiche che riflettono le simmetrie e le entità conservate che generano il sistema. Queste correlazioni sono di natura non-locale e possono essere isolate dalle correlazioni causali. Si assume che gli effetti psi siano manifestazioni di queste correlazioni non locali. Da questo modello si possono predire molte limitazioni degli effetti psi. In ultimo l'articolo riferisce numerosi riscontri empirici che supportano il modello dell'informazione pragmatica.

Pseudopsychics and the Barnum Effect

Chris A. Roe
University of Edinburgh

Abstract: The present study considers whether statements used by pseudopsychics in ostensibly psychic readings are accepted as true by clients because they exploit the Barnum Effect. Material drawn from the pseudopsychic literature was mixed with classic Barnum statements and given to 44 subjects ostensibly as feedback on a projective measure completed earlier. Subjects rated the degree to which they felt the statements were accurate in describing them. Comparing the performance of the two statement sets indicated that the Barnum items were more strongly accepted ($W = 75, p < .0005$, two-tailed), but that acceptance of the two item types covaried across subjects ($r_s = .597, p < .0001$, one-tailed). It is suggested that this was unlikely to be due to differential susceptibility to demand characteristics. Attempts are made to account for these findings by proposing that a subset of outlying pseudopsychic statements which did not induce acceptance may have served to depress the performance of the set as a whole. The subset was characterised as being less general or favourable in form than is usually necessary to induce the Barnum Effect.

Introduction

Critics of parapsychology have often dismissed evidence drawn from psychic readings, however impressive, as being the result of the (conscious or unconscious) use of deceptive practices known collectively as 'cold reading' (e.g. Dutton, 1988; Randi, 1981; Tobacyk, Milford, Springer & Tobacyk, 1988). The cold reading technique has been defined rather loosely by Hyman (1977) as 'a procedure by which a "reader" is able to persuade a client whom he has never met before that he knows all about the client's personality and problems' (p. 20). In practice, the meaning of the term has tended to vary from case to case; from active pre-session information gathering (Couttie, 1988) to simple reliance on using statements that are true of most people (Dutton, 1988). Although this would seem to be a somewhat simplistic characterisation when applied to all instances of

apparent paranormal communication, it rightly draws attention to some of the ways in which clients can be deceived into believing that a claimant has uncanny knowledge about them.

The extent to which it is possible for claimants to use such deceptive practices is perhaps best illustrated by reference to the growing specialist literature that claims to provide instruction on how to set up as a pseudopsychic¹. Many of these publications include formulaic readings for the prospective reader to adapt for their own use (e.g. Cain, 1991; Webster, 1990), or provide at least some examples of statements that are general enough in character to be used with most, if not all, clients (e.g. Corinda, 1958/1984; Earle, 1990). Given enough information leakage from the client (using sources such as their clothing, physical features, and carriage through to non-verbal and even verbal communica-

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¹ A pseudopsychic can be defined here as a person who produces information or effects that are claimed to be the result of special abilities, but that are in fact generated through normal means.

tions from them) the process of cold reading can give rise to what appears to be accurate specific information (Couttie, 1988; Schwartz, 1978). However, a recent review of this literature (Roe, 1991) has suggested that even where a pseudopsychic is using a number of quite sophisticated cold reading techniques, a substantial proportion of the reading necessarily remains in the form of general statements.

The success of these statements has been attributed, by both skeptics (e.g. Hyman, 1977) and pseudopsychics (e.g. Earle, 1990) alike, to clients' susceptibility to the Barnum Effect, an effect defined by Dickson & Kelly (1985) as 'the psychological phenomenon whereby people accept general personality interpretations as accurate descriptions of their own unique personalities' (p. 367). Indeed, Earle (1990) has actually recommended that aspiring pseudopsychics use a crib sheet made up exclusively of Forer's (1949) original set of Barnum statements. However, this is very much the exception, and many recommended personality descriptions are of a form that shares what may only be superficial similarities with the Barnum personality sketch, as the pseudopsychic presentations respond to the different expectations of their intended audience. Thus while the comparison seems plausible enough, the claim that pseudopsychic statements are accepted because they utilise the Barnum effect must remain as yet unsubstantiated.

Research on the Barnum effect has been considerable; to date, there have been three substantial reviews of Barnum literature (Dickson & Kelly, 1985; Furnham & Schofield, 1987; Snyder, Shenkel & Lowery, 1977), which together refer to over seventy independent studies. Typically, these studies are presented to subjects as an attempt to further evaluate some assessment device (such as a Rorschach test) by considering how successfully it can describe respondents purely on the basis of their responses (such as their interpretation of Rorschach inkblots). Subjects complete the measure, and after some delay are provided with personality feedback ostensibly derived

from it. In fact, all subjects are provided with the same personality sketch made up of general statements. These statements are rated according to the degree to which the subject believes them to be true of himself or herself. Subjects tend to be very impressed with the accuracy of such feedback (see, e.g., Ulrich, Stachnik & Stainton, 1963), which can even lead them to have elevated faith in the assessment device or the diagnostician (Snyder, Larsen & Bloom, 1976).

Much of the literature has concerned itself with attempts to characterise the factors that may induce acceptance, and has concentrated on three broad areas, namely (i) characteristics of the subject, (ii) characteristics of the feedback items, and (iii) characteristics of the context in which (sham) assessment occurs and/or feedback is given. The findings of these studies have generally been disappointing, producing equivocal or at best weak effects (Furnham & Schofield, 1987): but of these effects, the most robust have been associated with aspects of the subject's personality that may be seen as reflecting some general concept of 'gullibility' (for an alternative characterisation, however, see Johnson, Cain, Falke, Hayman & Perillo, 1985; Layne, 1979; Standing & Keays, 1987). In particular, high acceptance of Barnum statements has been associated with subjects expressing a high need for approval (Mosher, 1965; Orpen & Jamotte, 1975; Snyder & Larson, 1972), and with external scores on measures of locus of control (Orpen & Jamotte, 1975; Snyder, 1974; Snyder & Shenkel, 1976). Should pseudopsychic statements be exploiting the same mechanism that induces Barnum acceptance, then they may be expected to show similar covariance with such personality factors.

However, the Barnum effect should not be seen simply as a matter of exposing gullibility on the part of the respondent. Rather, properties of the statements themselves, including most notably generality (Furnham & Varian, 1988), favourability (Snyder *et al.*, 1977) and triviality (Greene, 1978), suggest a more complex interpretation. Recent work (Roe, 1994) has argued

that such characteristics are effective not because the items are thus *necessarily* true for subjects, but rather because they encourage the respondent to actively elaborate on and interpret the given material in personally relevant ways. Thus the Barnum effect may involve both characteristics of the subject and properties inherent in the statements themselves.

The present study was designed to assess whether the statements recommended and used by pseudopsychics could replicate the performance of traditional Barnum statements when presented in the context of a conventional Barnum study. Replication here would consist of similarly high general acceptance, and similar covariance with individuals' scores on measures of need for approval and locus of control. Thus it was predicted;

H₁: Acceptance levels for the pseudopsychic items will differ from those for traditional Barnum statements².

H₂: Acceptance levels for pseudopsychic statements will covary with Barnum acceptance across individuals, such that high scorers on one will also score highly on the other³.

H_{3a}: As individuals' scores on a measure of locus of control tend towards the external, so their acceptance levels for Barnum items will increase.

H_{3b}: As individuals' scores on a measure of locus of control tend towards the external, so their acceptance levels for pseudopsychic items will increase.

² The wording of this hypothesis is such that it can be directly tested. Given the assumed similarity of the statement types, I would expect this prediction not to be supported here. I am grateful to one referee for drawing attention to the difficulty of drawing meaningful conclusions on the basis of accepting the null hypothesis. However, we do not have to address those problems here, since the null was, in fact, rejected.

³ Note also that if H₂ is supported, then H_{3a} and H_{3b} will not be independent; nor will be H_{4a} and H_{4b}.

H_{4a}: As individuals' scores on a measure of need for approval increase, so their acceptance levels for Barnum items will increase.

H_{4b}: As individuals' scores on a measure of need for approval increase, so their acceptance levels for pseudopsychic items will increase.

Method

Materials

Discrete items consisting of between one and four sentences were drawn from the pseudopsychic literature⁴. Repetitions of items from different sources, or items expressing similar themes, were omitted to give an initial pool of potential Barnum statements (henceforth referred to as pseudopsychic statements). Other changes were made to some items in order to increase their appropriateness to the given context (i.e., so that they would be plausible as feedback from a projective test). Changes were only made at the superficial level of, for example, replacing terminology associated with psychic reading (such as 'I see you as...' or 'I get the impression that you...') with wording more typical of psychometric feedback (such as 'You are...' or 'You have found...').

Despite these changes, some items were by their very nature unsuited to presentation via a 'traditional' Barnum protocol, since they made predictions about the client's future that would not be likely to be revealed through completion of a psychometric test, no matter how 'mysterious'. To overcome this, all pseudopsychic statements were assessed independently by five judges (members of the parapsychology unit at Edinburgh) who rated the likelihood that the information revealed could legitimately be gleaned, or readily inferred, from information contained in a projective measure. It was not important here to generate an actual measure of likelihood, but

⁴ Further details of the selection procedure are given in the appendix.

rather to reflect subjects' likely satisfaction with the information they would be given, and thus maintain their faith in the dummy protocol. Using a six-point scale, where 1 = 'certainly detectable' and 6 = 'certainly not detectable', only those items with a total rating of 15 or less (which equated to a mean rating of 3 or under) were retained to give a pool of 30 items for use in the study.

In order to ensure that subjects were given a manageable amount of feedback to evaluate, statements were divided randomly by the experimenter using random number tables (RAND Corporation, 1955) into two subgroups (A and B), each containing 15 items. To these were added a set of 15 Barnum statements drawn from items used by Forer (1949), Sundberg (1955), and Paterson (1955) so that each personality sketch consisted of 30 statements. A full list of items is given in the appendix.

The study made use of a sham context in which subjects were given the House-Tree-Person projective measure (Buck, 1949), chosen because the test's assessment method was seen as being suitably vague or 'mysterious', a factor that has previously been shown to facilitate acceptance of Barnum statements (Richards & Merrens, 1971; Snyder, 1974).

Subjects

Forty-four volunteers acted as subjects, of whom 9 were male and 35 female. Previous studies (e.g. Halperin, Snyder, Shenkel & Houston, 1976; Snyder & Shenkel, 1975) have indicated that sex differences have little influence on strength of the effect and so the biased sample was not considered problematic. Subjects' ages ranged from 18 to 37 with a mode and mean of 20 ($SD = 2.73$).

Procedure

Second year psychology undergraduates at the University of Edinburgh were asked to participate in 'a study to evaluate new assessment techniques for the House-Tree-Person (H-T-P) test'. Subjects were approached individually or in small groups

to enable a more relaxed but involved recruitment procedure, in which the dummy protocol was explained and questions answered. They were asked to decide whether to participate only after they were confident that they understood what would be expected of them.

Upon recruitment, subjects were given measures of locus of control (Rotter, 1966) and social desirability (Crowne & Marlowe, 1960) which were to be used ostensibly as sources of external validity for assessing the H-T-P feedback. These were completed at leisure and returned after approximately seven days when subjects met individually with the experimenter to undertake the H-T-P test. The test itself involved drawing a house, a tree and a person using any or all of a range of materials provided. It was emphasised that artistic ability was not an important factor, and that the test gave best results if the subject relaxed and tried to have fun while producing his or her drawings. Subjects were informed that the drawing(s) would be sent out to a team of people who had recently been trained to interpret this type of material in a more holistic and meaningful way than had previously been attempted. This was designed to prime them to be more open to the kind of psychic-reading-based information they would receive.

A further week later subjects returned to be given feedback. This was supplied in typewritten form as discrete, numbered statements on a sheet marked with each subject's own personal identification number. Statements were evaluated individually by each subject according to how well each statement applied to them, using a five-point scale from 1 ('almost entirely wrong') to 5 ('amazingly accurate')⁵.

Subjects were debriefed individually once they had completed the evaluation of

⁵ This rather non-standard rating scale (after Carrier, 1963) was adopted because it has a history of use in the Barnum literature and is still current (see e.g. Furnham & Varian, 1988). It would be counter-productive to introduce changes to the protocol just for change's sake, in particular as this would restrict the comparability of these data with previous findings.

feedback⁶. The debrief, which typically lasted between 30 and 60 minutes, concentrated on three issues: firstly, a justification of the necessity of an element of deception here in order to elicit valid responses from them; secondly, an assurance that their acceptance of the dummy protocol did not reflect badly on them since great care had gone into generating a plausible cover story; and thirdly, an opportunity was given for subjects to work through their own thoughts and feelings about the study and to ask questions about the design should any aspect still be unclear to them. It is comforting to note that no subject exhibited any negative reaction (such as annoyance or embarrassment) on being informed of the true nature of the study. Rather, the typical reaction was one of pleasant surprise, with many being intrigued by the relative complexity of the design.

Results

Mean ratings for pseudopsychic statements were similar for subsets A and B [$U = 101.0, p = .89$, two-tailed] suggesting that the two statement sets were comparable. Similarly subjects allocated subsets A and B did not differ in their degree of acceptance of conventional Barnum statements [$U = 261.5, p = .611$, two-tailed]. It was therefore felt appropriate to combine data for subsequent analyses.

(i) Do pseudopsychic statements produce similar ratings to those for the Barnum statements?

(a) Viewing the frequency distributions for the mean ratings of the two sets of statements (Figure 1) suggests that as a group, acceptance was higher for the Barnum statements than for either set of pseudopsychic statements. Comparing mean

acceptance rates for the two types of statement reveals that Barnum items were better accepted to a highly significant degree [$W = 75, N = 42, p < .0005$, two-tailed], requiring us to reject the null hypothesis in favour of H_1 . However, almost all of the pseudopsychic statements still achieved mean ratings that are post hoc significantly above the mid-value of 3 ('about half and half') [$W = 27, N = 30, p < .001$, one-tailed] and therefore tended to be accepted as true of the client.

(b) We can also consider whether acceptance of the pseudopsychic statement pool covaries with Barnum pool acceptance across individuals, which would be suggestive of both sets of stimuli adopting the same *modus operandi*, or exploiting similar intra-subjective variables (such as, though not necessarily, some general gullibility factor). Results from this study (Figure 2) suggests that they do. Correlating subjects' ratings for Barnum and pseudopsychic statements yields a highly significant result [$r_s = .597, N = 44, p < .0001$, one-tailed], indicating a strong tendency for high scorers on one measure to score highly on the other, and low scores on one to be associated with low scores on the other. Thus we may feel justified in accepting H_2 .

(ii) Does acceptance covary with the other personality data in similar ways?

Subjects' mean ratings both for pseudopsychic and Barnum items were correlated with scores on measures of locus of control and need for approval. These results are summarised in Table 1. No significant relationship is evident between acceptance ratings for pseudopsychic statements and these personality measures ($p = .953$ and $.516$ for need for approval and for locus of control respectively), and we therefore reject H_{3a} and H_{3b} . However, there is equally no relationship apparent between these variables and acceptance of classical Barnum statements either ($p = .886$ and $.572$ respectively). Given the weakness

⁶ It should be noted here that it is rare for accounts of Barnum studies to include details of how the deception was justified to subjects. The unfortunate implication is that little care has been taken to ensure subjects' subsequent psychological well-being.

Figure 1

Frequency distribution of subjects' acceptance ratings for Barnum statements and for Pseudopsychic statements

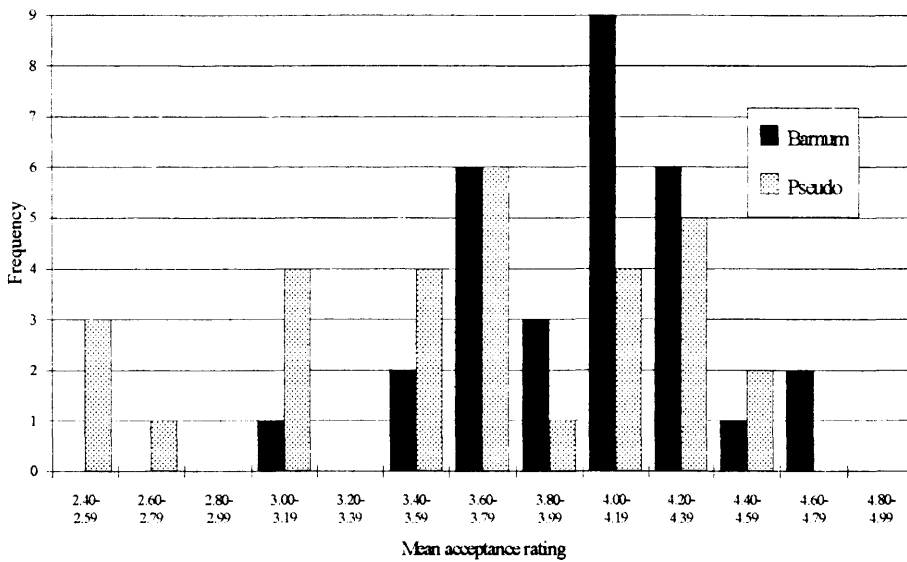


Figure 2

Scattergram of subjects' acceptance ratings for Barnum statements against acceptance for Pseudopsychic statements

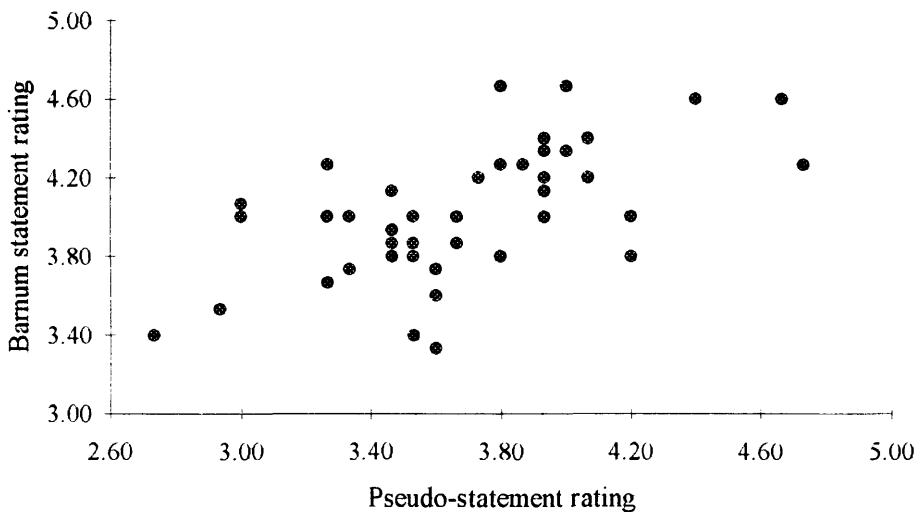


Table 1

Correlation coefficients describing the relationship between subjects' personality measures and acceptance data.

	Acceptance of Pseudopsychic statements	Acceptance of Barnum statements
Need for Approval	.009	-.022
Locus of Control	.100	-.086

of the correlations, which are likely to be due to chance, little store should be placed by the differences in the direction of the relationships between these variables and the two statement types.

(iii) Confounding variables

It is, of course, possible that acceptance represents an artifact of situational variables linked to demand characteristics, to which some subjects are more susceptible than others (although this would not directly explain the greater success of Barnum statements over pseudopsychic statements here). A post hoc measure was taken to evaluate this likelihood. Two independent judges, who were unaware of the purpose of the study, rated subjects' drawings in terms of the amount of effort that had been put into making them, independent of any natural drawing ability. Presumably, subjects who were more sensitive to situational pressures such as demand characteristics would be more likely to work longer or harder in producing their drawings. Alternatively, subjects who had spent longer on their drawings would feel greater pressure to accept feedback that was ostensibly based upon it (Festinger & Carlsmith, 1959; Linder, Cooper & Jones, 1967).

Although the judging criteria were loosely defined, there was a reasonable degree of inter-judge agreement ($r_s = .576$, $N = 44$, $p < .0001$, one-tailed). The mean effort rating so obtained for each drawing was compared with subjects' feedback ratings. Neither comparison approached significance (for the Barnum statements, $r_s = -.076$, $p = .618$; for the pseudopsychic state-

ments, $r_s = -.068$, $p = .654$), suggesting that situational factors of this sort at least were not influential in subject acceptance.

Another potential confound that can be considered here could be termed a 'scepticism variable' since it reflects subjects' scepticism in the assessment measure and in the proposed attempt to broaden the range of information given as feedback, as suggested by the sham context. This can be assessed by comparing judges' pre-study ratings of the likelihood that the information would be readily available, to a projective measure such as the House-Tree-Person test with subsequent acceptance ratings for those items. Statements rated as least likely to be available to a projective measure would presumably also tend to be regarded by subjects as the most speculative or tentative suggestions made by an assessor experimenting with the new-found freedom of interpretation. However, correlating mean judge ratings with mean acceptance ratings for each pseudopsychic statement⁷ gives a negative but non-significant result ($r_s = -.126$, n.s., one-tailed).

Discussion

Although the results do not provide wholehearted support for the notion that pseudopsychics are (wittingly or unwittingly) making use of the Barnum Effect in their selection of material, there is some room for optimism. In particular, the rea-

⁷ Note: we are restricted to considering the performance of pseudopsychic statements here, since no pre-test judgement of the appropriateness of classic Barnum statements was considered necessary.

sonably strong correlation between Barnum and pseudopsychic statement acceptance is perhaps a little surprising, given that the two sets of items exhibit significantly different acceptance levels.

How can the two findings be reconciled? A plausible explanation is that the pseudopsychic statements represent a weak subset of Barnum statements, whose performance is not quite as extreme as that of the latter, but which shadows their characteristic variation across individuals. It does seem unlikely that this covariance can be explained simply in terms of some individuals' increased susceptibility to demand characteristics, since those subjects who were targeted as especially susceptible did not exhibit greater levels of acceptance. Indeed, it should be noted from the distribution of mean acceptance ratings for pseudopsychic statements given in Figure 1 that it may be the inclusion of a few outlier statements that would not qualify as Barnum statements (indeed they fail the principal criterion of achieving a mean acceptance rating of 3.0 or better) that serve to depress the performance of the whole sets of items in relation to conventional Barnum statements.

The pseudopsychic literature may therefore provide a fruitful source of items to enable the pool of Barnum statements to be expanded. A number of researchers have previously attempted to introduce new items, but the selection process has often been very haphazard, with very little effort made to systematically validate them beyond some subjective measure of face validity. For example, Paterson's original profile was devised for use in luncheon club lectures, and no insight is given into how items were selected (see Forer, 1949), and Sundberg (1955) generated additional fake descriptions from 'judges' selections' without giving any detail as to the criteria used for selection. Yet these items are still preferred as stimulus materials (e.g. Johnson *et al.*, 1985). Pseudopsychic statements do at least lay claim to face validity, since they are recommended on the grounds that (it is claimed) they are generally accepted as true by clients.

The distribution of acceptance ratings does seem to support the consensus that important characteristics of Barnum statements are their generality and favourability (Furnham & Varian, 1988), since the pseudopsychic statements that did not fare so well give descriptions that are relatively specific⁸ (e.g. 'Children play an important role in your life') or relatively negative in orientation (e.g. 'Your life hasn't developed exactly the way you expected or would have liked. Many of your goals and plans have failed to materialise'). However, this is not a hard and fast rule, as other apparently negative statements (such as 'There are times when you felt your life is one long battle...') are nevertheless accepted. The determining factor appears to be a generality/vagueness attribute which these items possess, and which presumably allows the subject to interpret the statements in a less ego-threatening manner. It would be interesting to see whether subjects did show evidence of interpreting Barnum statements in this way. Research attempting to characterise the properties of Barnum statements has thus far met with only limited success (Furnham & Schofield, 1987), and further work certainly needs to be done in this area.

Some researchers have drawn attention to a self-other asymmetry, according to which subjects do not recognise that statements that they accept as true of themselves are equally likely to be true of others (Johnson *et al.*, 1985). While this is not yet considered a defining characteristic of Barnum statements, it would nevertheless be interesting to see if this property is also common to pseudopsychic statements. According to one account of the process by which Barnum statements are effective (Roe, 1994) this would necessarily be the case, and this represents a promising direction for future research. However, the present study was not concerned with characterising the properties of statements that

⁸ We might note here that Boerenkamp (1985) found that more than half of the statements produced by psychics in his study were regarded as having a greater than 1-in-2 chance of being true simply by chance.

cause them to be generally accepted, but rather was designed to determine whether pseudopsychic statements act in a similar manner to Barnum statements. Only once this general acceptance has been established does it become meaningful to investigate the characteristics that may induce it.

In retrospect, the item describing subjects as 'above average in intelligence' is problematic given the undergraduate population used here, for whom it is presumably a truism. We share the surprise of one referee in noting that the mean rating for this statement was 'only' 4.18, suggesting that unless our subjects were particularly concerned to express humility, they were not simply reflecting on the objective truth of statements, but were offering a subjective interpretation.

There could be *a priori* grounds for expecting pseudopsychic statements not to perform quite as well as classic Barnum statements, because many of the former were generated with a very different context in mind; however, this argument is not supported here. Judges' ratings of the appropriateness to the context of particular information were not able to predict subjects' subsequent acceptance levels to any significant degree. This notwithstanding, the psychic reading environment undoubtedly places alternative emphases on the type and form of information to be elicited, and the transformations that were necessary to generate appropriate pseudopsychic statements may not have been totally successful. The relative impact of the two sets of items may be markedly different if presented in a context that more accurately simulates the psychic reading environment. There is also a danger that the items that survived the selection process may no longer be representative of pseudopsychic statements generally, consisting instead of a particular subset with distinct (selected) characteristics. The selection process, however, was inevitable given the need to maintain the appearance of the supposed purpose of the study. If future studies were to present items in different contexts (e.g.

as feedback from a psychic reading) then they would not be so constrained.

Initially, it would also seem surprising that neither acceptance of Barnum nor of pseudopsychic statements was correlated to any degree with the personality measures of locus of control and need for approval, which represents a failure to replicate the findings of others (Mosher, 1965; Orpen & Jamotte, 1975; Snyder & Larson, 1972; Snyder, 1974). However, it should be noted that the effects previously reported have generally been quite small and inconsistent (Fichten & Sunerton, 1983), suggesting that even where such personality measures have some influence, theirs is not the primary motivation towards acceptance. Other work (Roe, 1994) has cast doubt upon the 'gullibility hypothesis' as the most appropriate means of accounting for the phenomenon. Alternatively, it could be argued that the generally high acceptance of statements across all subjects generates a ceiling effect that limits the amount of variance in scores within each factor, thereby artificially reducing any estimate of correlation between them. We await further work to resolve the relative influence of such factors upon Barnum acceptance.

In summary, the results of this study suggest that pseudopsychics are recommending the use of some statements that act in a similar way to Barnum statements, and that are capable of eliciting similar (if not quite so extreme) personal validation from recipients. This would tend to support the contention offered by some commentators sceptical of the veridicality of psi (e.g. Dutton, 1988; Randi, 1981) that apparently impressive psychic readings are, in part, a consequence of Barnum acceptance coupled with faulty recall. Such an interpretation is in keeping with the general finding that psychic readers tested under controlled conditions do not seem to be more accurate in their predictions than would be expected by chance (Boerenkamp, 1985, 1986; Schouten, 1993).

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Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

Appendix: Statements and mean acceptance ratings

The initial pool of pseudopsychic statements was made up from readings in pseudopsychic manuals by Cain (1991), Earle (1990), Hester & Hudson (1977), Hobrin (1990), Martin (1990) and Webster (1990). Recommended readings in these sources are given in one of two forms, either as a series of distinct items, with instructions for when to apply each, or as one or more 'formula readings' designed to provide a template to be adapted for specific readings. For cases of the former, all items were initially taken, whereas for the latter, the readings were divided at those points at which the topic changed (which in practice was a very straightforward procedure). Some items expressed very similar ideas or even reproduced elements from other, earlier sources. Such repetitions were eliminated to give an initial pseudopsychic statement pool of 74 items. On the basis of independent judges' assessments, the 30 that were deemed most appropriate for the context were retained. These are listed

below, along with the classic Barnum statements used in this study. For all items, the mean acceptance rating is included for information.

Pseudopsychic statement set

1. You like to keep an open mind. [4.25]
2. You appear to be concentrating on the things that have gone wrong in your life. [3.54]
3. You wonder if your career is going in the right direction. You are feeling restless and unfulfilled. [3.63]
4. Children appear to play an important role in your life. [2.71]
5. You are quite concerned about a member of your family, possibly a child. They have caused trouble for you and your family on many occasions. You have tried to help but have met with resistance. [2.42]
6. You are a versatile person. You are both creative and practical, but it seems that these creative abilities have barely been tapped. [4.13]
7. There are times when you feel your life is one long battle. You become overwhelmed with your responsibilities, and lack the focus you once had. If you concentrate on those jobs that are really important, you can live through these phases. [3.79]
8. Your life hasn't developed exactly the way you expected or would have liked. Many of your goals and plans have failed to materialise. [2.50]
9. Relationships have not always been as easy as you would have liked. You are a good friend once people get through, but there is a reserve present, and I sense that you would rather have one or two close friends than a room full of acquaintances. [4.13]
10. You don't mind solitude at times. In fact you really benefit from time on your own. It gives you a chance to work things out and put things into perspective. [4.58]
11. You do best when working for yourself, or in a situation where you are entirely left to get on with it. [3.79]
12. You seem to know yourself pretty well, and have few illusions about what you are capable of. At times, you do dream about all the things you would like to be and do, but you do know inside yourself which of these are possible and which are flights of fancy. [4.21]
13. You are not so open as you used to be, not as ready to share with just anyone everything about your inner self as you once were. I think you've seen how that can backfire sometimes. Some of these experiences are even now still too uncomfortable to sit around and remember. [3.71]
14. There is a strong urge in you to be in control of your own destiny. You want to make sure that things work out the way you want them to. You don't like being along on someone else's ride. You would rather have your options open, be able to choose as you wish, and not have to depend on other peoples' schedules. [4.38]
15. You are still affected by a recent argument with someone. [3.58]
16. You have a pleasant personality. [3.95]
17. You are above average in intelligence. [4.18]
18. You appreciate lovely things, and may even be a bit of a collector. [3.55]
19. You are basically a friendly person and have many acquaintances but few close friends. There is a long-lasting relationship with one person in particular. [3.77]
20. You need to try and relax more than you do. Your life seems to be running at such a hectic pace. Try to get things in the proper perspective. [3.50]
21. You tend to put off chores which must be done but do not particularly interest you. You find yourself rushing and frequently face frustration with all the little things which have to be done. You need to learn how to better apportion your time and energies. [3.68]
22. You have often dreamed of visiting strange and exotic lands. [4.18]

23. You tend to act before thinking things through. You often spend money on things you don't need, and feel sorry later. [3.14]
24. You sometimes feel as if you attract the wrong sort of person. Others have got you into trouble more than once. [2.50]
25. You give the impression of being a sensible person, with your feet firmly rooted to the ground. There are times when you can get a bit carried away, but basically you live in this world. [4.27]
26. You are good with people, and would be excellent at dealing with the public (if you don't already do this), but you do also need some time to yourself. [4.32]
27. There is a woman in your past who has had a strong influence on you. The way she lived her life, some of the things she said, affected the way you have come to view parts of your life as well. [3.18]
28. In your past there has been a brush with death, either for you personally, or someone close to you. [3.00]
29. You are the sort of person who doesn't always speak up when you think you should. You may take some bad treatment from someone and you let it go, unwilling to start a screaming battle over some small but stupid, annoying or unfair incident. But then you can be pushed too far and just 'explode' over something just as trivial, because you've been saving up all that feeling. You need to speak up sooner, stop yourself feeling moody or guilty because of the way others have behaved. Don't let them control you like that. [4.41]
30. You seem to be preoccupied with money matters, perhaps concerning a recent hitch in finances. [3.14]

Barnum statement set

1. You have a tendency to be critical of yourself. [4.45]
2. You like to be with people, especially to mix with those you know well. [4.61]
3. You pride yourself as an independent thinker and don't accept others' statements without satisfactory proof. [4.13]
4. You occasionally get depressed, but you couldn't be called moody. [3.96]
5. You tend to be fairly normal in your attitudes and behaviour. [3.86]
6. You are usually outgoing and friendly, although at times you can be wary and reserved. [4.22]
7. Sometimes you have difficulty in concentrating. [4.15]
8. You secretly wish you had a better developed and healthier body. [3.84]
9. While you have some minor personality weaknesses, you are generally able to compensate for them. [4.17]
10. You prefer a certain amount of change and variety, and become dissatisfied when hemmed in by restrictions and limitations. [4.05]
11. Your hopes and ambitions tend to be fairly realistic. [3.70]
12. You are occasionally bothered by minor physical ailments such as headaches, but they seldom get you down. [4.24]
13. Usually disciplined and self-controlled outside, you can sometimes be feeling worrisome and insecure inside. [3.71]
14. Your sexual adjustment has caused only minor problems to you. [3.49]
15. There are times when nothing seems to please you. [3.72]

Pseudosensitifs & Effet Barnum

Résumé: La présente étude examine si les déclarations formulées par les pseudosensitifs durant leurs lectures apparemment psychiques sont acceptées comme vraies par les clients du fait de l'Effet Barnum. On a mélangé du matériel verbal emprunté à la littérature pseudosensitive à des déclarations classiques Barnum que l'on a fourni à 44 sujets comme feedback d'une mesure projective effectuée plus tôt. Les sujets ont évalué à quel point les déclarations étaient exactes en tant que description d'eux-mêmes. La comparaison de la performance des deux types de déclarations a montré que les items Barnum étaient les plus fortement acceptés ($W = 75$, $p < .0005$, bilatéral), mais que l'acceptation des deux types d'item covariait parmi les sujets ($r_s = .597$, $p < .0001$, unilatéral). On suggère qu'il est improbable que cela soit dû à une susceptibilité différentielle à des caractéristiques de demande de la tâche. On tente de rendre compte de ces découvertes en proposant qu'un sous-ensemble particulier de déclarations pseudosensitives qui n'a pas induit d'acceptation a pu servir à diminuer la performance de l'ensemble comme un tout. On a caractérisé ce sous-ensemble comme étant moins général ou favorable dans sa forme que ce qui est habituellement nécessaire pour induire l'Effet Barnum.

Os pseudo-paranormais e o efeito Barnum

Resumo: Este estudo analisa se o que os pseudo-paranormais dizem em suas supostas sessões paranormais é aceito como certo por seus clientes devido ao fato de que os paranormais utilizam o Efeito Barnum. Juntou-se material retirado da literatura pseudo-paranormal a outros relativos a afirmações Barnum clássicas que foram administradas a 44 sujeitos como *feedback* dos resultados de um teste que eles haviam realizado anteriormente. Os sujeitos avaliaram em que grau eles sentiam que as afirmações eram precisas no que tange às suas descrições. Ao comparar ambos os grupos de afirmações verificou-se que as afirmações Barnum tiveram mais aceitação entre os sujeitos ($W = 75$, $p < .005$, bi-caudal), mas que a accitação destes dois grupos de afirmações co-variaram entre os sujeitos ($r_s = .597$, $p < .0001$, mono-caudal). Sugere-se que é pouco provável que isto possa ser explicado devido às diferenças de susceptibilidade às características demandadas. Trata-se de explicar estes resultados propondo que um sub-grupo das afirmações pseudo-paranormais que não foram aceitas podem ter causado a diminuição da efetividade desse grupo como um todo. Este sub-grupo caracterizou-se por ser menos geral ou favorável em relação ao que é geralmente necessário para provocar o Efeito Barnum.

Pseudopsíquicos y el Efecto Barnum

Resúmen: Este estudio considera si lo que los pseudopsíquicos dicen en sus supuestas sesiones psíquicas es aceptado como cierto por sus clientes debido a que los psíquicos usan el Efecto Barnum. Se juntó material tomado de la literatura pseudopsíquica con material sobre afirmaciones Barnum clásicas y se le administró a 44 sujetos como si se les estuviese informando sobre los resultados de una prueba proyectiva que ellos habían tomado anteriormente. Los sujetos evaluaron el grado en el cual ellos creían que las afirmaciones eran correctas en términos de describirlos a ellos (a los sujetos). Al comparar ambos grupos de afirmaciones se encontró que las afirmaciones Barnum resultaron tener más aceptación entre los sujetos ($W = 75$, $p < .0005$, 2 colas), pero que la aceptación de estos dos grupos de afirmaciones covariaron a través de los sujetos ($r_s = .597$, $p < .0001$, 1 cola). Se sugiere que es poco probable que esto pueda explicarse debido a diferencias de susceptibilidad a características demandadas en este contexto. Se trata de explicar estos resultados proponiendo que un subgrupo de las afirmaciones pseudopsíquicas que no fueron aceptadas pudieron haber causado la disminución de la efectividad de este grupo de afirmaciones. Este

subgrupo estaba caracterizado por ser menos general o favorable en forma de lo que es usualmente necesario para provocar el Efecto Barnum.

Pseudo-psychics en het Barnum-effect

Samenvatting: De auteur heeft onderzocht of uitspraken van pseudo-begaafden in zogenaamde "psychic readings" door de vragensteller als correct worden beschouwd omdat die nepbegaafde het Barnum-effect uitbuit. Uitspraken uit literatuur van pseudo-psychics werden gemengd met klassieke Barnum-uitspraken. Die combinatie werd vervolgens aangeboden aan 44 proefpersonen (ppn) onder het mom van feedback over een eerdere projectieve test. De ppn gaven op een schaal aan hoe goed de uitspraken hen beschreven. Vergelijking van beide sets uitspraken toonde aan dat de Barnum-uitspraken als de beste werden beschouwd ($W=75$, $ptweez.<0,0005$) en tegelijk dat er een covariantie was van de beoordeling van beide typen uitspraken over de ppn ($rS=0,597$, $peerz.<0,0001$). Het lijkt onwaarschijnlijk dat dit een gevolg is van een verschillende ontvankelijkheid voor het type vraagstelling. De auteur probeert het resultaat te verklaren met het idee dat een subset met opvallende uitspraken van een pseudo-psychic de beoordeling van de hele set in negatieve zin heeft beïnvloed, omdat die subset minder aanleiding gaf tot een beoordeling als correcte uitspraak. Uitspraken in die subset bevatten een minder algemene of minder positieve beschrijving dan meestal nodig is om het Barnum-effect op te wekken.

Pseudo-Medien & der Barnum-Effekt

Zusammenfassung: Die vorliegende Untersuchung befaßt sich mit der Frage, ob Aussagen, die von Pseudo-Medien in vorgeblich paranormalen Readings gemacht werden, von Klienten als wahr akzeptiert werden, weil sich diese Aussagen den Barnum-Effekt zunutze machen. Material aus der Literatur über Pseudo-Medien wurde mit klassischen Barnum-Aussagen vermischt und 44 Personen als angebliches Feedback über zuvor erhobene Ergebnisse projektiver Tests präsentiert. Die Versuchspersonen stuften die Aussagen nach dem Grad der Übereinstimmung mit ihrer jeweils eigenen Person ein. Ein Vergleich zwischen den beiden Aussage-Typen zeigt, daß die Barnum-Aussagen die am stärksten akzeptierten waren ($W = 75$, $p < .0005$, zweiseitig), aber daß die Akzeptanz der beiden Typen über die Gruppe der Versuchspersonen kovarierte ($rs = .597$, $p < .0001$, einseitig). Vermutlich hat dies wohl nicht an unterschiedlichen Empfänglichkeiten für Demand-Charakteristika gelegen. Es wird versucht, diese Ergebnisse durch die Mutmaßung plausibel zu machen, daß eine Untergruppe weit hergeholter pseudomedialer Aussagen, die keine Akzeptanz gefunden haben, vielleicht dazu gedient hat, die Leistungsfähigkeit der gesamten Aussage zurückzudrängen. Diese Untergruppe wurde als weniger allgemein gehalten oder günstig beschrieben als normalerweise erforderlich ist, um den Barnum-Effekt auszulösen.

Pseudosensitivi ed effetto-Barnum

Sommario: Lo studio ha verificato se le affermazioni formulate da pseudosensitivi in apparenti esperienze di sensitività vengono prese per vere dai clienti perché si produce un effetto-Barnum. Materiale tratto dalle produzioni di pseudosensitivi è stato mescolato a classiche affermazioni Barnum e dato a 44 soggetti facendolo passare per il risultato di un test proiettivo condotto in precedenza. I soggetti dovevano esprimere con un voto in che misura ritenevano le affermazioni accurate nel descriverli. Confrontando i punteggi ottenuti dai due tipi di materiale si è riscontrato che le affermazioni Barnum erano le più accettate ($W=75$, $p<0,0005$, a due code), ma che

l'accettazione dei due tipi di responsi covariava nei soggetti ($r_s=0,597$, $p<0,0001$, a una coda). Si ipotizza che non è probabile che ciò avvenisse per la diversa suscettibilità alle varie caratteristiche. Si tenta invece di spiegare simili dati proponendo che un sottogruppo di apparenti affermazioni di pseudosensitivi che non ha indotto accettazione possa aver abbassato il punteggio dell'intero materiale di questa classe. Il sottogruppo di affermazioni era caratterizzato dall'avere una forma meno generica o favorevole di quanto è generalmente necessario per indurre l'effetto Barnum.

Research Note: Exploring Defensiveness and Psychokinesis Performance

Caroline A. Watt
Department of Psychology,
University of Edinburgh

Loftur R. Gissurarson
Regional Office for the Affairs of
the Disabled, Reykjavik

Abstract: Reviews of the few experimental studies of PK and its relationship to attitudes, self-perceived 'luckiness', personality, imagery, and 'cognitive mode/style' have frequently been unable to present an unambiguous picture of individual differences in PK performance (e.g., Gissurarson, 1989, 1990-91, 1992a, 1992b; Gissurarson & Morris, 1991; Stanford, 1977). This apparent inconsistency may simply be due to the small number of relevant PK studies. For the present study, the performance of subjects who had participated in two separate series of experiments was analysed retrospectively. Prior to enrolling in an experimental series by Watt on developing a prototype indicator of perceptual defence and vigilance, the subjects had participated in an experimental series by Gissurarson on psychokinesis. This enabled us to examine a hitherto unexplored PK-defensiveness relationship. Participants' performance on the perceptual defence/vigilance task correlated with their prior performance on a computer PK task, with low PK performance being associated with defensiveness ($r_s = .23$, $N = 24$, n.s.). This finding, in need of systematic replication, suggests that a defensiveness relationship with PK performance may be worth exploring further. Broader implications for models of ESP and PK are discussed.

Introduction

Research on individual differences in ESP performance has found some consistent trends, including that of individuals who show signs of defensiveness scoring lower on ESP tasks than low-defensive individuals (e.g., Haraldsson & Houtkooper, 1992; Watt, 1992, 1993a). In contrast, consistent findings on individual differences in PK performance are sparse, and there has apparently been no *direct* study of a possible PK-defensiveness relationship. Von Lucadou's multivariate PK experiment (von Lucadou, 1987a, 1987b; von Lucadou,

Lay & Kunzman, 1987) found that non-anxious and non-neurotic participants were relatively successful at a PK task. These personality characteristics might be related to low defensiveness, so von Lucadou's experiment may provide some indirect support for a PK-defensiveness relationship. Do the different relationships that ESP and PK have with psychological variables suggest different models for ESP and PK? This paper presents the results of some explorations of the relationship between PK and defensiveness that may contribute to this debate.

The distinction traditionally made between ESP and PK may be based more upon operational than upon theoretical and empirical foundations. Parapsychologists ask participants to 'influence' the random number generator, or to 'gather impressions' of the remote video target. In the past

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two decades, however, an alternative idea has emerged, that PK and ESP may be unitary phenomena. In his Conformance Behaviour model, for instance, Stanford (1978) suggests that PK and ESP should be reconceptualised in terms of a conformance between an organism and its environment that is contingent upon the relationship between the items in the system rather than representing a causal connection between those items. Von Lucadou's Model of Pragmatic Information (e.g., von Lucadou 1989, 1995), whose foundations lie in the observational theories of quantum physics, conceptualises ESP and PK as 'non-local correlations', as patterns emerging from organisationally closed systems. An alternative theoretical formulation is found in May, Radin, Hubbard, Humphrey & Utts' (1986) Intuitive Data Sorting (IDS) model (recently re-named Decision Augmentation Theory, or 'DAT'), which postulates that PK is not physical interference with an RNG's output, but that the participant uses precognition to choose the right moment to operate the RNG, such that non-random sequences are chosen. If the same individual differences found in ESP performance exist with PK performance, this would provide some support for the notion that ESP and PK may indeed be unitary phenomena on some microscopic, statistical level. Hence, it would not be unreasonable to expect a defensiveness-PK relationship to parallel that found for ESP.

Unconscious processes of defensiveness — the tendency to avoid or distort perception of threatening, stressful, or emotional material — have stimulated much research over the years. Parapsychologists have explored how individuals may defend in similar ways against both sensory and extrasensory perceptions, and a number of studies have sought to compare individuals' defensiveness with their ESP performance. Defensiveness has been found to be related both to free-response and forced-choice ESP performance, with those who are relatively low in defensiveness typically scoring better at ESP tasks.

The most frequently-used indicator of defensiveness in parapsychology is the

Defence Mechanism Test (DMT; Kragh, 1955). This projective psychological test presents participants with brief exposures of pictorial stimuli that are intended to activate defence mechanisms in the viewer. Because of the brevity of exposure, the stimuli appear unclear yet viewers are asked to try to describe what they thought they saw at each exposure. The test assumes that the defence mechanisms that are activated by the threatening stimuli are revealed in the viewer's descriptions of the stimuli, especially in the distortions and inaccuracies that these descriptions can contain. A total of 16 studies, conducted in the USA, Holland, and Iceland, have correlated participants' DMT scores with their performance on forced-choice ESP tasks (e.g., Haraldsson, Houtkooper & Hoeltje, 1987; Johnson & Kanthamani, 1967; Johnson & Lübke, 1977). The results of the DMT-ESP studies are consistent with those found using other indicators of defensiveness: generally, the highest ESP scores were from the least defensive participants, while the highly defensive individuals had the lowest ESP scores. While not all DMT-ESP studies were independently significant, the overall DMT-ESP correlation was highly significant. Haraldsson & Houtkooper (1992) found that the combined DMT-ESP correlations from the Icelandic series gave a z of 2.611 ($p = .0045$, one-tailed, $N = 462$); adding in the data from the US and Dutch studies gave a z of 3.870, ($p = .00006$, one-tailed, $N = 582$). In all, 13 out of 16 DMT-ESP studies showed correlations between DMT and ESP scores in the predicted direction.

Given that the DMT is time-consuming to administer and score, and that considerable expertise is required to interpret DMT results, Watt (1993a, 1993b) conducted two experiments that examined perceptual defence (defined as delayed perception of subliminal emotional stimuli compared to neutral and control stimuli) and perceptual vigilance (defined as rapid perception of subliminal emotional stimuli compared to neutral and control stimuli) in relation to forced-choice ESP performance. The predicted relationship between perceptual

defence/vigilance and forced-choice ESP performance was confirmed, with defensive participants scoring relatively poorly at the ESP task, and vigilant participants scoring relatively well; this relationship was statistically significant for the two experiments combined ($t[65] = -2.132$, $p = .018$, one-tailed), and was independently significant in the second experiment ($t[41] = -2.077$, $p = .02$, one-tailed).

While developing the methodology for measuring perceptual defence/vigilance, a series of preliminary studies was carried out (Watt, 1993b). None of these studies was aimed at correlating perceptual defence/vigilance with psi performance; however, the participants in one of these studies had previously taken part in a series of experiments carried out by Gissurarson (1989, 1990-91), in which participants did a PK task. Thus it was possible to compare participants' perceptual defence/vigilance scores with their psi scores obtained in Gissurarson's experiments. Although the study was post hoc, Watt remained unaware of subjects' PK scores and Gissurarson remained unaware of their perceptual defence/vigilance scores until analysis was carried out; however, the analysis was not pre-planned at the time of Watt's and Gissurarson's experiments. Two to three years separated defensiveness testing from PK testing. On the one hand, this time delay reduces the likelihood that participants' recollections of their PK scores might somehow influence their performance on the measure of defensiveness. On the other hand, the validity of the resulting defensiveness-PK correlation would be reduced if either of these attributes was unstable over time.

Method

Procedure for measuring psychokinesis

The PK task was a computer test called 'Synthia' (see Gissurarson, 1989 and Gissurarson & Morris, 1990, for details). It consisted of a VDU display of four windows, one of which would be designated the 'target' window by the computer. The

participant was required to press the space bar on the keyboard and try to make the computer 'select' that window. When the space bar was pressed a random number was generated to select a window randomly. Thus, the participant's implicit aim was to bias the output of the random number generator (RNG) so as to select the designated target window. Some versions of Synthia used a pseudo-RNG (Wichmann & Hill, 1982), while other versions employed a live RNG (using electronic noise; for details see *User's Guide RBC 04CA-S*, 1988). Gissurarson's participants were not aware that different RNGs were being used. No significant difference in PK performance for the two kinds of RNG was found. If the target window was selected then a hit was scored. Half of the trials done with Synthia were in 'feedback mode' where a hit resulted in a display of a bright star intended to give congratulatory feedback to the participant on a trial-by-trial basis; and half were in 'non-feedback mode' where there was no trial-by-trial information on whether or not a hit had been scored, although at the end of the session the total number of hits was displayed on the screen. No significant difference in overall PK scoring for feedback versus non-feedback modes of the PK task was found. Participants completed some questionnaire material prior to being led to a sound-attenuated room where they completed two runs of 40 trials each in a session on the Synthia test with a break in between. There was a one-in-four probability of selecting the target window by chance, therefore MCE was 10 hits. The first version of Synthia consisted of 30 trials during a run, where MCE was 7.5 hits. Half the subjects started with the feedback mode first, the other half started with the non-feedback mode.

Procedure for measuring perceptual defence/vigilance

The prototype apparatus was developed by Watt to enable automatic administration and objective scoring of a measure of perceptual defence/vigilance. Partici-

pants gazed into a modified tachistoscope, at a dimly illuminated screen that formed the background field onto which a stimulus slide was projected with gradually increasing brightness. The participant was required to press a response button to indicate when they became aware of the *presence* of a stimulus slide. A computer controlled the timing and illumination of the slides, and automatically recorded the slide illumination level when the participant pressed the response button (thus keeping the experimenter and the participant blind as to the slide order and the participant's scoring). After 5 practice slides, 16 stimulus slides were shown four times each in a random order. The stimulus slides depicted four simple black and white line drawings that had previously been judged to be emotionally unpleasant (E), four similarly judged neutral drawings (N), and eight control slides that were matched to each of the E or N slides (EC and NC), and that consisted of a re-arranged version of the E or N slides, such that the control slides were the same brightness as their E or N partner, but conveyed no meaningful information¹. Participants were unaware that the slides contained any information, and, when asked, participants reported no awareness of slide contents. It was assumed, therefore, that the slide brightness level at which the participant indicated awareness of the overall *presence* of a stimulus slide represented an index of his or her awareness threshold for the subliminal information *content* of each slide. Therefore, variations in the slide illumination levels at which awareness was indicated might reflect variations in perceptual threshold for the subliminal information. An individual was judged to be perceptually defensive if he or she took longer to indicate awareness of the E slides, compared to the N, NC, and EC slides; like-

wise, perceptual vigilance was indicated by relatively quick responses to the E slides compared to the N, NC, and EC slides.

Procedure

There was a total of 29 participants in the subliminal perception study, 24 of whom were recruited from volunteers who had participated in PK studies by Gissurarson, and five of whom were also volunteers who had not done any PK studies. In the subliminal perception study no reference was made to prior PK performance because the purpose of that study was simply to refine methodological details for the administration of the prototype test of defensiveness/vigilance. Each participant was tested individually, and each had a different random slide order. They were instructed to regard the task as 'like an eye-test', where their visual sensitivity to each slide was being measured. The participants were to concentrate on responding consistently to each slide, so that in their judgement each slide reached the same level of brightness before they responded to it. Following the practice slides, the participant proceeded with the 64 experimental slides, separated into two runs of 32 slides with a break at the halfway point. When all slides had been responded to, each participant was asked whether they had seen anything on the background screen apart from the light rectangular shape that they had been expecting.

Analysis of data

Because previous experiments with the prototype indicator of perceptual defence/vigilance had shown only weak signs of defensiveness and vigilance, a criterion was set in advance for the inclusion of participants' data in any analyses: it was decided to terminate the present study when 24 individuals responded most quickly or most slowly to the E slides, relative to EC, N, & NC slides, in either the first or the second half of the session (in the expectation that these participants were more strongly defensive or vigilant than

¹ So, for example, if participants took longer to respond to E than to EC slides, this would suggest that it was the emotionally meaningful content of the E slides that was influencing participants' scores, rather than, say, the possibility that the E slides were generally darker than the others and so were less easily perceived.

those who showed no delayed or quick responding to the E slides). Only the data of these 24 'criterion' participants would be analysed. Following completion of all subliminal perception testing sessions, we looked back over Gissurarson's records to discover the participants' PK scores, and to correlate these scores with perceptual defence/vigilance.

Results

Twenty-nine participants were required in order to reach the criterion number of 24 (i.e., five participants showed no particularly quick or slow responding to the E slides). Of the criterion participants, 13 were female and 11 were male. All the criterion participants had taken part in some of Gissurarson's PK studies, so it was possible to compare their defensiveness/vigilance with their PK scores.

The mean brightness scores on the measure of subliminal perception were calculated for each participant, for the E, EC, N, and NC slides. If the overall mean brightness scores were highest for E slides compared to EC, N, and NC slides, that participant was described as perceptually defensive; vigilant participants were those whose mean brightness scores for E slides were lower than for EC, N, and NC slides. The 24 participants were categorised as follows: 12 were perceptually defensive (with a mean brightness score² of 1.028, SD = 0.015), five were vigilant (mean = 0.978, SD = 0.010), two were mildly defensive (i.e., second highest scoring for E slides, mean = 1.025, SD = 0.004) and five were mildly vigilant (i.e., third highest scoring

for E slides, mean = 0.993, SD = 0.001). When asked whether they had seen anything other than the light rectangular shape of each slide, most participants reported no other visual impressions. A handful had occasional vague visual impressions ('a blob', 'a blotch'), but when they were later shown the actual stimulus slides the participants did not associate what they had seen with the contents of the slides. Most participants were surprised to learn that there had been information on each slide. It can be assumed, then, that the slide contents remained subliminal, as intended.

When checking Gissurarson's records for PK scores, 20 participants had done 80 trials on Synthia, and four had done 60 trials. To make the scoring of all participants comparable, therefore, each participant's hit-rate was calculated (i.e., the ratio of hits to trials, where MCE = 25%). The mean PK hit-rate was 24.62% (SD = 5.00), slightly below the 25% expected by chance. Overall, there was some suggestion that defensive participants had lower PK scores than vigilant participants, with a mean hit rate of 22.48% (SD = 5.03) for the former and 24.26% (SD = 4.12) for the latter. Given this slight difference in scoring, and the small number of participants, we decided to correlate PK with defensiveness scores.

Spearman correlation coefficients were calculated between PK hit-rate and ranked mean brightness scores for E and EC slides respectively. Support for the PK-defensiveness hypothesis would show up in a significant correlation between PK scores and responses to the critical E slides (since these are assumed to produce defensiveness or vigilance), but one would not expect to find a significant correlation between PK scores and responses to the matched EC slides (since the latter do not portray any emotional or meaningful information). It was found that PK hit-rate correlated with perceptual defence/vigilance, with the correlation in the direction that might be expected based on previous research with ESP tasks: the highest PK scoring tended to come from the vigilant participants, while the lowest PK scoring tended to come from the defensive participants ($r_s = .23$). Given

² In order to explore the effects of changing the brightness properties of the stimulus slides in this experiment, participants were exposed to slides that were either light (i.e., dark pictures on a light grey background) or dark (i.e., light pictures on a dark grey background). Participants who saw only 'dark' stimulus slides therefore had higher overall brightness scores than those who saw only 'light' slides. In order to compare directly the scores of the two sets of slides, a ratio measure is given here. This is calculated by dividing the brightness scores of the E slides by the mean of brightness scores of EC, N, and NC slides.

that the present analysis is based on only 24 participants, this correlation is not statistically significant (it would need to be .34 for $p = .05$, one-tailed). The correlation between PK and responses to the EC slides is smaller than for the critical Emotional slides ($r_s = -.182$); therefore the overall pattern of scoring was as predicted, but not to a significant degree.

Conclusion

The defensiveness-ESP relationship has been conceptually replicated by Watt using a prototype indicator of perceptual defence/vigilance. During the development of the prototype apparatus, a study was conducted with participants who had previously taken part in a PK study. The experimenter who measured perceptual defence/vigilance remained blind as to participants' PK scores. Likewise, the experimenter who obtained the PK scores was unaware of participants' subliminal perception scores. There was a modest non-significant correlation in the predicted direction between defensiveness and PK scoring, in line with the findings of previous ESP-defensiveness research: the highest PK scores tended to be associated with perceptual vigilance; and the lowest, with perceptual defence.

The PK-defensiveness relationship suggested by this study needs further replication, given the small effect found, the small number of participants, and the prototype nature of the defensiveness measure. These preliminary findings are nevertheless of interest because, so far as we know, until now there has been no published comparison of defensiveness and PK scoring, despite the many studies that have compared defensiveness and ESP performance. If defensiveness turned out to be associated with PK performance in the same way as has been found for ESP performance, this would have implications for our models of the interaction of psychological and psi processes; perhaps more intensive study of individual differences in PK performance would similarly begin to reveal that PK performance is as affected by attitudes,

beliefs, motivations, and more persistent personality factors, as ESP has been shown to be. As we discussed in the introduction, traditionally PK and ESP have been treated as distinct from one another, especially in terms of how we frame the psi task for experimental participants. But if psychological processes influence both similarly then perhaps the distinction lies more in how we define the participant's task than in the processes underlying PK and ESP, at least on a microscopic level. Stanford's (1978) Conformance Behavior model, and von Lucadou's (1989) Model of Pragmatic Information suggest that we cease to think of ESP and PK in causal terms, and instead regard them as patterns of information that are meaningful to observers. If so, then one might indeed expect the observer's normal psychological processes to interact in similar ways with both ESP and PK phenomena.

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Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

Explorer la défense et la performance de psychokinésie

Résumé: Les comptes-rendus des quelques études sur la PK et les attitudes, la perception de sa propre 'chance', la personnalité, l'imagerie, et le "mode/style cognitif" ont fréquemment été incapables de présenter une description non-ambigüe des différences individuelles dans la performance PK (par ex., Gissurarson, 1989; 1990-91; 1992a; 1992b; Gissurarson & Morris, 1991;

Stanford, 1977). Cette incohérence apparente peut simplement être due au petit nombre d'études pertinentes de PK. Pour la présente étude, la performance des sujets qui ont participé à deux séries d'expériences séparées ont été analysées rétrospectivement. Avant d'être engagés dans une série expérimentale par Watt sur le développement d'un indicateur prototype de la défense perceptive et de la vigilance, les sujets avaient participé à une série expérimentale par Gissurarson sur la psychokinésie. Ceci nous a permis d'examiner une relation PK-défense inexplorée jusqu'ici. La performance des participants à une mesure de défense/vigilance perceptive a corrélié $r_s = .23$ (ns, $N = 24$) avec leur performance précédente à une tâche informatisée de PK. Cette découverte, en attente d'une réplique systématique, suggère que la relation de défense avec la performance PK peut valoir la peine d'être explorée plus avant. Des implications plus larges pour les modèles d'ESP et de PK sont discutées.

Explorando tendências de defesa e resultados de psicocinesia

Resumo: Revisões dos poucos estudos experimentais de PK e atitudes, auto-avaliações de 'sorte', personalidade, imagens mentais e 'estilo de funcionamento cognocitivo' não puderam apresentar uma perspectiva precisa das diferenças individuais nos resultados de PK (ex.: Gissurarson, 1989, 1990-91; 1992a, 1992b; Gissurarson & Morris, 1991; Stanford, 1977). Esta aparente inconsistência talvez ocorra devido simplesmente ao fato de haver poucos estudos relevantes de PK. Neste estudo, analisou-se retrospectivamente os resultados dos sujeitos que participaram em duas séries separadas de experimentos. Antes de participar de uma série experimental de Watt, sobre o desenvolvimento de um protótipo de indicador de defesas perceptuais e de vigilância, os sujeitos participaram em uma série experimental de Gissurarson de psicocinesia. Isto nos permitiu examinar uma relação que não havia sido explorada anteriormente quanta às defesas e PK. A performance dos participantes em relação à medida de defesa perceptual e de vigilância apresentou uma correlação de $r_s = .23$ (ns, $N = 24$) em relação aos resultados anteriores de um teste de PK por computador. Este resultado, que deve ser replicado sistematicamente, sugere que uma relação da defesa com a PK merece ser explorada no futuro. Também são discutidas implicações gerais de modelos de ESP e de PK.

Explorando Tendencias de Defensa y Resultados de Psicocinesia

Resúmen: Revisiones de los pocos estudios experimentales de PK y actitudes, auto evaluaciones de 'suerte', personalidad, imágenes mentales, y 'estilo de funcionamiento cognoscitivo' no han podido presentar una perspectiva precisa de las diferencias individuales en los resultados de PK (e.g., Gissurarson, 1989, 1990-91, 1992a, 1992b; Gissurarson & Morris, 1991; Stanford, 1977). Esta aparente inconsistencia quizás pueda deberse simplemente a que hay pocos estudios relevantes de PK. En este estudio se analizó retrospectivamente los resultados de sujetos que habían participado en dos series separadas de experimentos. Antes de participar en una serie experimental de Watt en la cual se trató de desarrollar un prototipo de un indicador de defensas perceptuales y de vigilancia, los sujetos habían participado en una serie experimental de Gissurarson de psicocinesia. Esto nos permitió examinar una relación que no había sido explorada anteriormente en relación a defensas y PK. Los resultados de los participantes en la medida de defensa perceptual y de vigilancia obtuvieron una correlación de $r_s = .23$ (ns, $N = 24$) con los resultados anteriores de una prueba de PK por computadora. Este hallazgo, el cual necesita replicarse, sugiere que una relación de la defensa con la PK merece explorarse en el futuro. También se discuten implicaciones generales de modelos de ESP y de PK.

Verband tussen defensieve houding en PK-resultaten

Samenvatting: Onderzoek naar de samenhang tussen PK-resultaten en een bepaalde attitude, denken dat je geluk hebt, persoonlijkheidskenmerken, verbeeldingskracht en cognitieve aanpak of stijl, heeft zelden geleid tot een duidelijk beeld van de verschillen in PK-scores tussen proefpersonen (b.v. Gissurarson, 1989, 1990-91, 1992a, 1992b; Gissurarson & Morris, 1991; Stanford, 1977). Die schijnbare inconsistentie kan natuurlijk gewoon een gevolg zijn van het kleine aantal PK-experimenten. Dit artikel behandelt een analyse die achteraf werd uitgevoerd op de scores die proefpersonen eerder in twee afzonderlijke experimenten hadden behaald. Voorafgaand aan hun deelname aan een experiment van Watt, over de ontwikkeling van een voorlopige indicator voor perceptieve afweer en waakzaamheid, hadden die proefpersonen een PK-experiment bij Gissurarson gedaan. Dit stelde ons in staat de niet eerder onderzochte samenhang tussen een defensieve houding en PK te meten. De correlatie tussen de scores in het onderzoek naar een defensieve/waakzame houding en de eerdere scores in de PK-taak was $r_s=0,23$ (ns, $N=24$). Dit resultaat, dat natuurlijk systematisch moet worden herhaald, suggereert dat verder onderzoek naar de verhouding tussen een defensieve houding en PK-scores nuttig is. De auteurs bespreken ook implicaties voor modellen over ESP en PK.

Untersuchung von Abwehrbereitschaft und PK-Leistung

Zusammenfassung: Übersichten über die wenigen vorliegenden experimentellen Untersuchungen zum Verhältnis von PK zu Einstellungsmustern, selbsterlebten 'Glücks'-Fällen, Persönlichkeitsvariablen, Vorstellungsbildern und kognitiven Verarbeitungsweisen sind häufig nicht in der Lage gewesen, ein unzweideutiges Bild individueller Unterschiede bei PK-Leistungen abzugeben (z.B. Gissurarson, 1989; 1990-91; 1992a; 1992b; Gissurarson & Morris, 1991; Stanford, 1977). Diese offenkundige Vieldeutigkeit mag einfach auf die geringe Zahl relevanter PK-Studien zurückzuführen sein. Für die gegenwärtige Untersuchung wurden retrospektiv die Leistungen von Versuchspersonen analysiert, die an zwei separaten Versuchsserien teilgenommen hatten. Bevor sie an einer Versuchsserie von Watt über die Entwicklung eines Indikator-Prototyps für Wahrnehmungsabwehr und Vigilanz teilnahmen, waren die Probanden Versuchspersonen in Gissurarsons PK-Experimenten gewesen. Dies setzte uns in die Lage, eine bisher unerforschte Beziehung zwischen PK und Abwehrbereitschaft zu untersuchen. Die Werte der Versuchspersonen für Wahrnehmungsabwehr/Vigilanz korrelierten mit $r_s = .23$ (ns, $N = 24$) mit ihren vorherigen Leistungen bei einer Computer-PK-Aufgabe. Dieser Befund, der einer systematischen Replikation bedarf, legt den Schluß nahe, daß das Verhältnis zwischen Abwehrbereitschaft und Psychokinese weitere Erforschung verdient. Weiterreichende Implikationen für ASW- und PK-Modelle werden diskutiert.

Studio su difensività e prestazioni psicocinetiche

Sommario: Le analisi cumulative dei pochi studi sperimentali riguardanti il rapporto tra psicocinesi (PK) e atteggiamenti, "fortuna" (secondo quanto viene percepito soggettivamente come tale), personalità, *imagery* e "carattere/stile cognitivo", non sono riuscite finora a spiegare in maniera coerente le differenze individuali nelle prestazioni psicocinetiche (vedere per es. Gissurarson, 1989; 1990-91; 1992a; 1992b; Gissurarson e Morris, 1991; Stanford, 1977). E' possibile, tuttavia, che una tale apparente inconsistenza dipenda dal limitato numero di studi esistenti sull'argomento. La presente indagine è consistita in una valutazione retrospettiva delle prestazioni di soggetti che avevano preso parte a due diversi tipi di lavoro. Prima di venire impegnati da Watt in una serie sperimentale che tendeva a ottenere un prototipo di indice di difesa percettiva e di vigilanza, i soggetti avevano partecipato a un esperimento sulla psicocinesi, condotto da Gissurarson. Questo

ha consentito di analizzare il rapporto, mai indagato in precedenza, tra PK e difensività. Le prestazioni dei partecipanti nel test di difesa/vigilanza percettiva hanno avuto una correlazione r_s con la precedente riuscita in un test PK al computer pari a 0,23 ($n=24$; non significativa). Questo riscontro, che deve ancora ricevere una replica sistematica, indica che il rapporto tra difensività e performance PK potrebbe meritare uno studio ulteriore. Vengono infine discusse alcune implicazioni di questi dati per i modelli dell'ESP e della PK.

Addendum to 'Parapsychological Publications in Non-Parapsychology Journals'

Bruno Severi

Institute of Clinical Electron Microscopy, University of Bologna, Italy

Abstract: A second list of 276 parapsychological articles in mainstream non-parapsychology scientific journals is here presented in addition to the one already published in *EJP* (1994). More than 1000 references have been collected in all. As before, this new list is taken from some important psychological catalogues and databases. It makes even more evident how the topic of paranormal belief is widely studied by a considerable number of scientists for its relevance in the socio-cultural context as well as in other areas. The author's opinion is that there are sufficient proofs suggesting that Science has to confront parapsychology's evidence to reach a deeper knowledge of Nature.

I present a new list of 276 references in addition to the one already published in my previous article 'Parapsychological publications in non-parapsychology journals' in *EJP* (Vol. 10, pp. 104-129, 1994). A good deal of these references are taken from some new sources I found at the Department of Psychology of the University of Bologna, i.e.: *Current Contents: Social and Behavioral Sciences*, *Psychological Abstracts*, and *PsycLit* (the CD-Rom data base edited by the American Psychological Association). Other sources from other university departments have been consulted as well.

I think that this inclusion of parapsychological material in these sources represents a further encouraging finding for parapsychology for two main reasons: (1) because it reveals an increasing interest in this topic thanks to the larger number of high standard academic catalogues that cover parapsychological papers appearing in selected scientific journals; and (2)

because some parapsychological journals are also indexed in some of the sources, for instance: *Journal of the Society for Psychical Research*, *Journal of the American Society for Psychical Research*, *Parapsychological Review*, *Journal of Parapsychology*, *Psi Research*, *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*, *Proceedings of the Society for Psychical Research*, *Research in Parapsychology*, etc.

This new list of references does not change significantly the statistical data and observations presented in my previous paper. Below I only add some new additional information:

(a) Summing the two lists, the total number of references I have found is now 1051;

(b) the scientific journals covering parapsychologic topics are 339 in all;

(c) the oldest article in either list is by the famous British physicist Michael Faraday, and is dated 1853. It appears in a journal unknown to me;

(d) from a deeper analysis of the enclosed list of references, a new interesting datum stands out: the enormous relevance that the 'anomaly' of paranormal belief, shown by a considerable part of humanity, deserves in the psychological, anthropologic, sociologic and other fields (more than 50 articles in this list).

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I have also a third, unpublished list of 47 parapsychological references contained in journals for which I have not yet collected sufficient information regarding their scientific standards.

Besides the considerations reported in my previous article, which now appear strengthened by these latest findings, with this effort of mine I hope I have collected sufficient material to show that parapsychology is not so neglected by the scientific community as is commonly thought. As well as the skeptics, I have met numerous researchers interested in and favourable to this topic at the University of Bologna and at some other Italian scientific institutions familiar to me. Most of them wished to know where they could learn more about parapsychology (books, journals, associations, etc.). On the other hand, others showed a more conflicting and a less comfortable view of parapsychology because of the mass-media: often Italian periodicals and television programmes present parapsychology in a misleading, if not in an erroneous, way. However, there are many academic journals of high reputation that accept parapsychology articles by parapsychologists as well as by authors whose main concerns lie elsewhere. This latter group of authors, which is composed of both critics and those well-disposed to parapsychology (or neutral), with their numerous articles and their different positions, testify that Science has to confront the paranormal to reach a more complete knowledge of Nature. I also believe that the most immovable critics of paranormal phenomena have to take into account the indications derived from this research before claiming their position and their truth in public.

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Addendum à "Les publications parapsychologiques dans des journaux non-parapsychologiques"

Résumé: On présente ici une seconde liste de 276 articles parapsychologiques parus dans des journaux scientifiques non-parapsychologiques en addition à celle publiée dans *EJP* (1994). Plus de 1000 références ont été collectées en tout. La majorité des références de cette nouvelle liste est issue

de certaines bases de données et catalogues importants de psychologie. Il devient de plus en plus évident que le thème de la croyance au paranormal est largement étudiée par un nombre considérable de scientifiques pour sa pertinence dans le contexte socioculturel aussi bien que dans d'autres domaines spéculatifs. L'opinion de l'auteur est qu'il y a suffisamment de preuves suggérant que la Science doit se confronter aussi à la parapsychologie afin d'atteindre une connaissance plus approfondie de la Nature.

Adendo às 'publicações parapsicológicas em revistas de temas não-parapsicológicos'

Resumo: A presenta-se uma segunda lista de 176 artigos parapsicológicos em revistas científicas de temas não-parapsicológicos em adição à que foi publicada no *EJP* (1994). Foram compiladas mais de 1000 referências no total. A maioria das referências desta nova lista foi retirada de alguns bancos de dados e catálogos psicológicos. É evidente que o tema da crença no paranormal está sendo amplamente estudado por um número considerável de cientistas devido à sua relevância no contexto sócio-cultural assim como em outras áreas de especulação. Na opinião do autor, há provas suficientes para sugerir que a ciência tem que levar em conta a parapsicologia para poder obter um conhecimento mais profundo da natureza.

Apéndice a 'Publicaciones Parapsicológicas en Revistas de Temas no-Parapsicológicos'

Resúmen: Se presenta una segunda lista de 276 artículos parapsicológicos en revistas científicas de temas no-parapsicológicos en adición a la que se publicó en el *EJP* (1994). Se han recopilado más de 1000 referencias en total. La mayoría de las referencias de esta nueva lista fueron tomadas de algunos catálogos psicológicos y catálogos de computadora importantes. Es evidente que el tema de creencia en lo paranormal esta siendo ampliamente estudiado por un número considerable de científicos debido a su relevancia en el contexto socio-cultural al igual que en otras áreas de especulación. En la opinión del autor hay prueba suficiente para sugerir que la ciencia tiene que considerar a la parapsicología para poder obtener un conocimiento más profundo de la naturaleza.

Addendum bij "Publikaties over parapsychologie in niet-parapsychologische tijdschriften"

Samenvatting: Dit artikel behandelt 276 parapsychologische artikelen indiverse niet-parapsychologische wetenschappelijke tijdschriften. Het is een aanvulling op een eerste overzicht in het *EJP* (1994). In totaal zijn ruim 1000 verwijzingen verzameld. Het grootste deel van deze tweede lijst stamt uit belangrijke catalogi en databanken voor psychologie. Het wordt steeds duidelijker dat geloof in het paranormale door een groot aantal wetenschappers wordt onderzocht vanwege de raakvlakken met desociaal-culturele en andere context. De auteur is van mening dat er voldoende bewijs is dat de wetenschap zich ook over de parapsychologie zal moeten uitspreken om tot een dieper inzicht in ons bestaan te komen.

Ein Nachtrag zu "Parapsychologische Publikationen in nichtparapsychologischen Zeitschriften"

Zusammenfassung: Ergänzend zu dem bereits 1994 im *EJP* veröffentlichten Verzeichnis von parapsychologischen Artikeln in normalwissenschaftlichen, nichtparapsychologischen Zeitschriften,

wird hier eine zweite Liste von 276 einschlägigen Publikationen vorgelegt. Insgesamt sind mehr als 1000 Referenzen zusammengekommen. Die meisten Titel dieser neuen Aufstellung sind einigen wichtigen psychologischen Verzeichnissen und Datenbasen entnommen. Es zeigt sich immer deutlicher, daß das Thema paranormaler Glaubensbereitschaften von einer beachtlichen Anzahl von Wissenschaftlern im Hinblick auf ihren sozio-kulturellen Kontext und andere ungesicherte Gebiete untersucht wird. Der Autor ist der Auffassung, es spreche hinlänglich viel dafür, daß sich die Wissenschaft auch mit der Parapsychologie befassen muß, um ein genaueres Verständnis der Natur zu erzielen.

Aggiunta a "Pubblicazioni parapsicologiche in riviste non parapsicologiche"

Sommario: Viene qui presentata una seconda lista di 276 articoli parapsicologici, apparsi in importanti riviste scientifiche non parapsicologiche, che va ad aggiungersi a quella già pubblicata sull'*E/P* (1994). Complessivamente si raggiunge così un totale di oltre 1000 citazioni. La maggior parte delle voci di questa nuova lista è stata ripresa da importanti cataloghi e archivi psicologici. Diventa sempre più evidente come l'argomento della credenza nel paranormale venga studiato approfonditamente da un numero considerevole di scienziati, a motivo della sua rilevanza socioculturale e per altri campi del pensiero. Secondo l'autore vi sono prove a sufficienza per affermare che la Scienza deve confrontarsi anche con la parapsicologia, se vuol conseguire una conoscenza più approfondita della Natura.

Book Review:
Emilio Servadio: Dall'Ipnosi alla Psicoanalisi
[*Emilio Servadio: From Hypnosis to Psychoanalysis*]

Carlos S. Alvarado
Department of Psychology, University of Edinburgh

A review of *Emilio Servadio: Dall'Ipnosi alla Psicoanalisi* [*Emilio Servadio: From Hypnosis to Psychoanalysis*], by Emilio Errera. Nardini Editore: Firenze, Italy, 1990. Pp. 154. Illustrated. L. 25.000. ISBN 88-404-4514-5.

When one thinks of senior leaders of European parapsychology many names come to mind. The late Hans Bender, from Germany, Robert Tocquet, from France, and George Zorab, from Holland are cases in point. In Italy psychoanalyst Emilio Servadio was probably one of the best known and respected representatives of a qualitative and psychodynamic approach to psychic phenomena. I recently reviewed aspects of Servadio's parapsychological career on the occasion of his 90th birthday and of the Life Achievement Award granted to him by the Parapsychological Association in August of 1994 (Alvarado, 1994). Sadly, Servadio died in January 1995.

Giovanni Errera's book reviewed here presents a wider overview of Servadio, one not limited to parapsychology. However, Errera has not written a biography of Servadio. Instead he asked Servadio a number of questions that he presents with the answers, grouped into many topics. Part One focuses on biography. We learn from Servadio's answers to the questions that he obtained in 1926 a degree in jurisprudence with a thesis on hypnosis in legal medicine. During the early 1930s he met and was trained in psychoanalysis by Edoardo Weiss, a pioneer of psychoanalysis in Italy. Much is said by Servadio about his participation in the development of psychoanalysis in Italy. In addition, there is information about Servadio's political, literary and social activities. This includes his move to India to escape Nazism. The second part of the book is about Servadio's

opinions on topics such as drugs and violence, among other issues.

In addition, the book contains several photographs of Servadio, from his childhood to his senior years. One of them shows Servadio at what seems to be one of the early conferences sponsored by the Parapsychology Foundation, sitting with Eileen Garrett, in the presence of Jule Eisenbud, Frances Bolton, and Rosalind Heywood. Many of these pictures say much about Servadio's personality and his varied and fascinating life.

A short section is devoted to psychical research. Servadio talks about his involvement with Italian parapsychology associations. He was one of four founding members of the Società Italiana di Metapsichica, the other members being Ferdinando Cazzamalli, Giovanni Schepis, and Luigi Romolo Sanguinetti. In addition, Servadio talks about academic parapsychology around the world and about other general topics related to the scientific study of psi phenomena.

Servadio answered the questions directly and in detail. But it is disappointing to see that Errera did not try to obtain details about many aspects of Servadio's work. Little is said about his work and ideas regarding psychoanalysis or parapsychology. This is a shame because Servadio had a fascinating career in both fields. For example, regarding parapsychology, the interviewer could have asked about his early writings on mediums Hélène Smith and Pasquale Erto, his writings on telepa-

thy in relation to the transference and counter-transference context of the psychoanalytic session, his discussions on the dynamics of ESP dreams, and his ideas regarding the importance of psi phenomena for the concept of a transpersonal dimension of human beings.

One hopes that a future biographer will present a carefully crafted biography that gives us details of Servadio's life in the context of his work in parapsychology, psychoanalysis, poetry, politics, and other areas. In the meantime, Errera's work will be useful for the general biographical information it presents, much of which is not available from other sources.

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Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

email: calvarad@afb1.ssc.edinburgh.ac.uk
Fax: 044-0131-650-3461

Book Review:
***Introduzione alla Parapsicologia: Studio del
Comportamento Paranormale Umano***
**[Introduction to Parapsychology: The Study of Human
Paranormal Behaviour]**
and
***Fenomeni Parapsicologici: Manifestazioni del
Comportamento Paranormale Umano***
**[Parapsychological Phenomena: Manifestations of Human
Paranormal Behaviour]**

Carlos S. Alvarado
Department of Psychology, University of Edinburgh

A review of *Introduzione alla Parapsicologia* (San Marco Libri, 1991. Pp. 281. L. 30.000) and *Fenomeni Parapsicologici* (Bologna: Nuova Libria Editrice, 1994. Pp. 282. L. 35.000), by Enrico Marabini.

Enrico Marabini, a well known Italian physician and parapsychologist, presents in these two books overviews of parapsychology's findings and his own concepts and classifications regarding psychic phenomena.

The purpose of the first book, *Introduzione alla Parapsicologia*, is to introduce parapsychology to the general public. After a section discussing basic notions of psychology, neurology and other topics, Marabini discusses definitions and classifications and then enters into methodologies used in the field. In his view, the importance of parapsychology is not that we are studying extraordinary or exceptional phenomena, but that we are studying human beings as producers of such phenomena. In this context, Marabini regrets that the emphasis on veridicality in the field 'has lost the view of man . . . ' (p.xiv).

In his concept of 'paranormal behaviour' Marabini does not limit the term to observable behaviour, but includes emotions, impulses, desires and intuitions, among other cognitive and affective human expressions. His view is a holistic one in

which psi is seen in the context of psychological, social, medical and other dimensions. Following the European tradition clearly seen in treatises such as that of René Sudre (1926) and in more recent writings, Marabini presents a classification including a great number of phenomena. Regarding mental phenomena, Marabini considers a pure category (telepathy, clairvoyance) and a mixed category. The mixed group includes phenomena such as travelling clairvoyance, divination techniques, xenoglossy and possession, among others. Marabini divides physical phenomena into telergy (e.g., mechanical, thermal, luminous effects) and teleplasty (materialization, transfiguration, objective apparitions). Of course, not all of these phenomena are accepted by everyone in the field, a topic that deserves more attention in the discussion of the classification.

Part of the classification scheme of Marabini includes an 'intrinsic' and an 'extrinsic' dimension. The first one refers to innate characteristics of the phenomena, such as their relation to vital processes and their independence of space and time. The

second includes external but potentially modifying variables of psi functioning such as sex, age, physiological state, culture, state of consciousness and other variables.

The rest of the book covers findings organized by methodological approaches drawn from examples from the old and recent international literature. Marabini discusses examples of qualitative and quantitative methods and of a combination of these approaches, the 'quali-quantitative method'. The quantitative section includes discussion of standard deviations, critical ratios and chi-squares. An omission in the discussion consists of examples of spontaneous cases evaluated statistically. Examples of this approach can be found in case collections and, particularly, in the survey literature.

While *Introduzione* was intended as a general introduction to parapsychology, *Fenomeni Parapsicologici* was intended as a more detailed coverage of the field following a conceptual approach some Italian parapsychologists call 'humanistic parapsychology'. In this view 'parapsychological research not only studies man as a bio-psycho system, but in terms of a bio-psycho-transpersonal reality' (p.v). In Marabini's view parapsychology (and disciplines such as medicine, psychology and anthropology) emphasize humans in their relation to the world. The point is to study a 'responsive reality' in relation to 'a moment in the life of a human being' (p.xvii).

To conceptualize psi in the context of these ideas, Marabini presents his concept of a 'paranormal phenomenological system'. This is a hypothetical cognitive system that interacts with different aspects of the psyche and the environment in the production of psi phenomena. It is the hidden process that brings psi to the surface level of the mind where it 'manifests at an experiential level as a subjective event of the cognitive type . . . or as an objective manifestation' (p.24). But these are only the end manifestations of an intervening process that cannot be observed directly. Marabini argues that there are many variables that interact with the processing of psi informa-

tion. Among others he lists age, sex, relationship of percipients and agents, ethnicity, culture, emotional state, personality variables and state of consciousness. His discussions of relevant examples focus on spontaneous cases, but experiments are not completely ignored.

At the end of the book, Marabini presents a chapter that focuses on the way psi emerges or manifests. The variety of illustrative cases and phenomenological descriptions remind one of the writings of Edmund Gurney (Gurney, Myers & Podmore, 1886), G.N.M. Tyrrell (1946), and Louisa Rhine (1953). Some of the most interesting forms of manifestation in spontaneous contexts considered by Marabini include cenesthetic sensations such as undefined senses of ill feeling (worries, presentiments) and emotions (sorrow, excitement, anxiety), and somatic, motor, or visceral reactions such as motor automatisms (the prompting or inhibition of action) and somatic or vegetative responses (visceral and dermal reactions). Marabini argues that the psi experience manifests through a 'preferential neuro-psycho-emotive-sensorial pathway with the appearance of a psychic dissociation' (p.245) of temporary character. This implies that each individual has predispositions to manifest psi in ways related to the use of some cognitive pathways over others, and that this process interacts with alterations of consciousness. Although not mentioned by Marabini, such ideas have common points with Harvey Irwin's (1979) information processing model of psi and is related to both spontaneous and experimental work dealing with imagery in psi processing (George & Krippner, 1984).

Both books are excellent in that they show an interesting holistic view of psi functioning, creative ideas, and a good ability to synthesise a great number of bibliographical sources. I welcome the international bibliography of the author, an aspect that helps Marabini present a good general view of parapsychology and argue his conceptual points. *Introduzione* fulfills well Marabini's purpose in presenting a general introduction to the field. *Fenomeni*

is also successful to the extent of presenting the author's ideas and the complexity of the form psi takes in its manifestation, especially in spontaneous contexts. Although Marabini's ideas are interesting and consistent with a long tradition of ideas of parapsychological information-processing, I would argue that they still need further empirical exploration and refinement, a point that I think the author will agree with. One hopes that these concepts continue to be explored empirically in spontaneous and experimental contexts both in Italy and elsewhere.

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Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

email: calvarad@afb1.ssc.edinburgh.ac.uk
Fax: 044-0131-650-3461

Book Review: *An Introduction to Parapsychology, 2nd ed.*

Nancy L. Zingrone
Department of Psychology, University of Edinburgh

A review of *An Introduction to Parapsychology, 2nd ed.*, by H.J. Irwin. McFarland: Jefferson, NC, 1994. ISBN 0-89950-808-1, LC 93-41761. \$27.50. 393 pp.

Recently, McFarland & Company published the second edition of Australian psychologist Dr. Harvey Irwin's eminently readable text, *An Introduction to Parapsychology*. Based on 15 years of experience teaching parapsychology at the University of New England in New South Wales, Irwin's text is clearly written and well-organised. Comprising 17 chapters, a bibliography, a name and subject index, and with 19 photographs of people and apparatus, *Introduction* acquaints the reader with some of the most important issues and findings in the field. The over-arching theoretical stance is a moderate one that occupies an intellectual space mid-way between committed sceptics and committed paranormalists. Irwin defines parapsychology as 'the scientific study of experiences which, if they are as they seem to be, are in principle outside the realm of human capabilities as presently conceived by conventional scientists' (p. 1). Irwin separates himself from 'those contemporary parapsychologists who actually regard parapsychology as the study of the paranormal'. For Irwin it is more reasonable to investigate the 'actual bases of these experiences, in determining the extent to which the phenomena are explicable within the framework of... mainstream science, and if appropriate, defining the respect in which the framework should be extended in order to accommodate empirical findings on the phenomena' (p. 2, my italics).

For Irwin, then, the paranormal hypothesis is only one of many possible hypotheses that might adequately explain

reports of seemingly paranormal experiences. Unlike many parapsychologists and critics who cover themselves in such a cloak of moderation for social and political reasons, Irwin's commitment to this fruitful stance is real: the tone and approach of his own body of research is evidence of this. By occupying this theoretical space, Irwin is able to build up a picture of parapsychology that, at its core, does not deviate markedly from the aims and methods of mainstream science. Irwin's parapsychology is not in search of the soul, but rather attempts to integrate a persistent and intriguing set of claims and beliefs with what is known about the human organism and its relationships to the natural world.

There is a naive positivism alive today in parapsychology — as alive as it still is in some branches of the physical sciences — that worships at the altar of 'objectivity' and 'parsimony' as if either existed in any absolute sense in the real world. It is a positivism that expresses the fervent belief that the experimental method can proceed as if the messiest of all confounding variables, the human being, were not involved as either experimenter or subject. It is a positivism that denigrates any naturalistic, clinical or otherwise non-experimental study of human experience as a kind of scientific atavism from a pre-modern era. For those of us who believe in a kind of 'reconstructed' positivism in which an understanding of the complexities of the human being as scientist and the human being as subject is coupled with old empirical values, a point of view in which

experiment and experience are held in balance, Irwin's emphasis on the importance of the experiential approach in parapsychology is a welcome corrective.

If *Introduction* has any failings, they lie in those few moments when the balance falls into disequilibrium. The first chapter is excellent. Irwin argues in it for an expanded definition of parapsychology that in practice pursues a research program not 'predicated upon authenticity' (p. 10). However, at the end of an otherwise excellent second chapter in which the discussion of mediumship phenomena is extended by a review of the career of the Australian medium, Charles Bailey, Irwin locates the work of the Society for Psychical Research in a 'much simpler' past from which present-day parapsychology has presumably evolved. (If the balance were still in equilibrium the lessons of past methodologies would not be abandoned merely because they belonged to the past.) Irwin says:

... the study of spiritualist mediums may offer a novel avenue of inquiry into the capacities of the subconscious mind... The less than convincing personations represented by some of Mrs. Piper's spirit controls similarly suggest the operation of distinct subconscious 'personalities', a notion that has given rise to some discussion of mediumistic performance in relation to the clinical concepts of dissociation and multiple personality disorder... But such advanced questions were far from the minds of the British group that set about the establishment of the SPR in 1882. Their perspective was much simpler, namely that the phenomena of the seance room did deserve some manner of intellectual scrutiny (p. 37).

The approach of the early SPR was anything but 'simple' as can be seen in Gauld's depiction of the motives and interests of Sidgwick, Myers and Gurney in *Founders of Psychical Research* (1968). In addition, in the *Proceedings* and other publications that followed, it is obvious that many deeper psychological questions were heavy on the minds of Gurney and Myers in particular,

and in Myers' case extended to an interest in multiple personality which was then called 'double consciousness'.

Similarly, after three excellent chapters (that describe the findings, value, and limitations of spontaneous case research; experimental work on ESP in historical, methodological and theoretical depth; and the thorny issues raised by precognition), the chapter on psychokinesis is less systematic. The balance is disturbed when certain key sets of data are ignored, among them the work of the Princeton Engineering Anomalies Research team, whose data and theory are of central importance to any discussion of experimental approaches to PK. Granted, the PEAR laboratory tends to publish the bulk of its data in privately printed technical reports. However, some good articles are available in our literature (in the journals of the Society for Scientific Exploration and the Society for Psychical Research) as well as in physics journals. In addition, the PEAR group is very generous with its technical reports, all of which are long and carefully-written. One hopes Irwin will take advantage of these reports when it comes time to bring out the third edition of *Introduction*.

Following the chapter on 'special topics' in PK in which Irwin discusses some of the wilder phenomena that are usually categorised as macro-PK, he does a very good job of comparing and contrasting a variety of difficult scientific theories designed to account for ESP and PK, among which he even-handedly includes sceptical theories. In the next seven chapters in which Irwin reviews the issues and findings on survival, reincarnation, poltergeists, apparitions, the near-death experience, the out-of-body experience and belief in the paranormal, Irwin is at his phenomenological best. His long experience with the issues that surround these areas of research are everywhere apparent, even though he is rather modest about the place of his own work in the delineation of the phenomenological approach to the study of seemingly paranormal experience. Especially in the chapter on belief, Irwin shows his ability to deal with research done from the sceptical point

of view as it should be dealt with, by taking the findings of such work as seriously as that done within the boundaries of parapsychology proper.

Introduction's last two chapters deal with a number of important concerns: findings from the clinical context; how to deal with hoaxers and 'seekers after reassurance'; how to react to those who may need therapeutic intervention; the potential applicability of psi abilities and findings from psi research; the ramification of parapsychology's position as a marginal science; and the potential impact on parapsychological method of post-modern critiques of traditional scientific practice and findings. The study questions at the end of the chapter called 'Matters of Relevance' are particularly useful as they force the reader to think through a series of issues that invariability arise when researchers come into contact with experiencers.

On my wish-list for the third edition are the inclusion of a glossary and more specific attention to methodological description, whether inserted at the appropriate points in the text, or bracketed by side bars, or provided in appendices. Although the findings are extensively covered in the text (those of the experiential side more than those of the experimental side, an imbalance that could also be corrected in a third edition), a student would be hard-pressed to design an experiment,

construct a questionnaire, or conduct a field study if *Introduction* was their only source of information. Although methodological expertise is usually conveyed in a companion volume or through a separate series of practical exercises, it is not beyond the scope of *Introduction* to present a clearer description of methodology. Even if only one of each type of investigative technique were described in full, the usefulness of Irwin's text as a first point of contact with parapsychology would be greatly enhanced.

In any case, taken as a whole, both author and publisher can be justifiably proud of this new and updated edition of *An Introduction to Parapsychology*. It is, without a doubt, the best text of its kind in print today.

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Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

Notice: Overcoming the Language Barrier

Carlos S. Alvarado, Gerd H. Hövelmann & Hans A.G. Michels

Summary: We (Carlos S. Alvarado, Gerd H. Hövelmann and Hans A.G. Michels) have decided to offer translation services to our colleagues in the field of parapsychology. We shall offer translations at prices considerably below those that are usually charged by official translation agencies. We can handle any kind of text (correspondence, abstracts, full-blown papers) and translate them from and into English, German, Dutch, French, Spanish, Portuguese and Brazilian Portuguese. Other languages (Italian and Japanese, in particular) could be added at a later stage. Please contact one of us for more information.

In the past, Carlos Alvarado (1989, 1991) has repeatedly stressed the need for a translation service in parapsychology as a means of improving communication within the field. He has pointed out that, for reasons of language, many papers of high quality have never been published in English and are therefore barely accessible to the majority of parapsychological researchers, whose first language is English. As Alvarado emphasized, parapsychological publications in languages other than English tend to be neglected, not because of a possible lack of scientific quality but purely for reasons of language. Hövelmann (1986) also noted that the language barrier stands in the way of international cooperation and the exchange of ideas.

As one possible way of overcoming this much-lamented language barrier, Alvarado (1991) suggested the 'development of an active translation policy' for the field. During several of the Euro-PA conferences, Hans Michels and Robert Morris had therefore discussed the possibilities and prospects for providing translation services at reasonable prices. In 1991, then, during the 39th Annual Conference of the Parapsychology Foundation, held in Dublin, Carlos Alvarado, Gerd Hövelmann and Hans Michels decided to join forces and to make preliminary arrangements for providing a

translation service to the parapsychological community.

In her Presidential Address to the 37th Annual Convention of the Parapsychological Association, held in Amsterdam, Deborah Delanoy (1994) again addressed the problems that language barriers persistently cause to the field of parapsychology. This inspired us to take up the old plans and elaborate on them. Immediately after Delanoy's address, we made initial arrangements for the establishment of a regular translation service. Both Delanoy and Morris welcomed this initiative and offered space in the *European Journal of Parapsychology* for the publication of this announcement.

For obvious reasons, translations of the required quality can only be done by persons who are both experienced translators and who have sufficient knowledge of the field of parapsychology. We feel that the three of us meet these requirements. We shall not establish an official firm or institution. Rather, we offer our services as private persons. We can translate any kind of document (correspondence, abstracts, full-blown papers) from and into the following languages: English, German, Dutch, French, Spanish, Portuguese and Brazilian Portuguese. Other languages (Italian and Japanese, in particular) could be added at a later stage.

As professional translators, Hövelmann and Michels have daily experience with the fees that are usually charged for professional translations. We know that translations at normal rates are far too expensive for many of our colleagues in the parapsychological community. Consequently, we will offer translation services at considerably reduced prices. Please contact one of us for more information.

Summaries

Wir (Carlos S. Alvarado, Gerd H. Hövelmann und Hans A.G. Michels) haben uns entschieden unsere Fähigkeiten als Übersetzer unseren Kollegen in der Parapsychologie zur Verfügung zu stellen. Wir werden Beträge in Rechnung stellen, die wesentlich unter dem bei offiziellen Büros üblichen Niveau liegen. Wir können mehrere Dokumententypen (Korrespondenzen, Zusammenfassungen, vollständige Artikel) aus den und in die nachfolgenden Sprachen übersetzen: Englisch, Deutsch, Niederländisch, Französisch, Spanisch, Portugiesisch und brasilianisch-Portugiesisch. Andere Sprachen (besonders Italienisch und Japanisch) könnten später hinzugefügt werden. Wenden Sie sich an eines der erwähnten Adressen, wenn Sie Näheres erfahren möchten.

Wij (Carlos S. Alvarado, Gerd H. Hövelmann en Hans A.G. Michels) hebben besloten onze diensten als vertaler aan te bieden aan collega-parapsychologen. Wij zullen tarieven hanteren die lager zijn dan gewoonlijk door officiële vertaalbureaus in rekening worden gebracht. We kunnen diverse soorten documenten (correspondentie, samenvattingen, volledige artikelen) vertalen vanuit en naar het Engels, Duits, Nederlands, Frans, Spaans, Portugees en Braziliaans-Portugees. Andere talen (m.n. Italiaans en Japans) worden misschien later toegevoegd. Neem met een van ons contact op voor meer informatie.

Nous - Carlos S. Alvarado, Gerd H. Hövelmann et Hans A.G. Michels - avons décidé de proposer nos services de traduction à nos collègues dans le domaine de la parapsychologie. Une offre à des prix avantageux, nettement au-dessous des tarifs officiels pratiqués par les bureaux de traduction. Nous savons réaliser tous les types de textes (correspondence, résumés, articles complets) et les traduire dans les langues suivantes et à partir de celles-ci: l'anglais, l'allemand, le français, l'espagnol, le portugais et le portugais brésilien. A un stade ultérieur, d'autres langues telles que l'italien et le japonais pourraient venir s'y ajouter. Pour plus d'informations, n'hésitez pas à nous contacter.

Nosotros (Carlos S. Alvarado, Gerd H. Hövelmann y Hans A.G. Michels) hemos decidido ofrecer servicios de traducción a nuestros colegas en parapsicología. Ofreceremos traducciones a precios menores de los que actualmente se requieren por agencias oficiales de traducción. Podemos manejar cualquier clase de textos (correspondencia, resúmenes, artículos) y los podemos traducir a y de inglés, alemán, holandés, francés, español, portugués, y portugués de Brasil. Otros idiomas (especialmente japonés e italiano) serán añadidos más tarde. Para obtener más información favor de ponerse en contacto con uno de nosotros.

Carlos S. Alvarado, Gerd H. Hövelmann e Hans A.G. Michels, decidiram formar um grupo apto a oferecer, aos seus colegas, serviços de tradução no campo da parapsicologia. O preço dos serviços de tradução deste grupo é consideravelmente inferior ao preço praticado pelas empresas de tradução oficiais. Aceitamos qualquer tipo de texto (correspondência, resumos e artigos científicos) e podemos traduzi-lo de e para Inglês, Alemão, Neerlandês, Francês, Espanhol, Português e Português do Brasil. Poderão ser adicionadas, posteriormente, outras línguas (como por exemplo o Italiano e Japonês). Para mais informações

é favor entrar em contacto com um dos componentes do grupo.

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Carlos S. Alvarado
Koestler Chair of Parapsychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK
Tel. +44 131 6503391
Fax. +44 131 6503461

Gerd H. Hövelmann
Hövelmann Communication
Carl-Strehl-Str. 16
D - 35039 Marburg/Lahn
GERMANY
Tel. +49 6421 26744
Fax +49 6421 13278

Hans A.G. Michels
Translators International
Schootsestraat 134
5616 RH Eindhoven
THE NETHERLANDS
Tel. +31 40 551010
Fax +31 40 571212

Notice:
19th International Conference of the
Society for Psychical Research
Notice and Call for Papers

The 19th International Conference of the Society for Psychical Research will be held at the Berkeley Hotel (formerly Cotford Hall Hotel) in Bournemouth between Friday 15th September and Sunday 17th September 1995. Accommodation and meals will be provided at special concessionary rates and there will be a reduced registration fee for Members and Associates of the SPR.

Any person may submit a paper for presentation at the Conference and papers may be on any aspect of parapsychology and psychical research. The usual time allocated for each paper, including discussion, will be 30 minutes. Speakers should aim to make papers readily understandable to a non-specialist audience and they should not simply read aloud an article intended for publication. There will be no simultaneous sessions. Since the Conference is a residential one, there will be ample opportunity for informal contact between participants. The programme committee would welcome suggestions for symposia or round-table discussions that could take place outside the time reserved for presentation of papers.

Summaries of proposed papers must reach the Programme Committee before **31st May**. These summaries should be **single spaced** and have a length of 500-1000 words (at most two pages). They should be neatly typed since, if accepted, they will be photocopied and distributed to the participants at the Conference. Authors whose contributions are accepted will be informed by the end of June.

Summaries should be sent to: **SPR Conference, Society for Psychical Research, 49 Marloes Road, Kensington, London W8 6LA, England** (telephone or FAX: 0171 937 8984). Overseas submissions should be sent air mail. The Conference programme and further details of the Conference arrangements will be enclosed in the July issue of the SPR Journal, or they can be obtained on provision of a stamped, addressed envelope to Eleanor O'Keefe at the SPR after mid-July.

Notice: APA Advertises for 'Members of Underrepresented Groups' to Review Journal Manuscripts

In her survey of publishing and funding parapsychology in the mainstream, Milton (this issue) found that only 22% of her respondents had been asked to referee a parapsychological paper for a mainstream journal. Parapsychologists should therefore not miss the opportunity to respond to an advertisement from the American Psychological Association for members of underrepresented groups to review journal manuscripts. According to an advertisement placed in *Contemporary Psychology*, 1995, vol. 40, no. 2, the APA Publications and Communications Board 'is particularly interested in encouraging members of underrepresented groups to participate more in this (reviewing) process'. The advertisement states that 'to be selected as a reviewer, you must have published articles in peer-reviewed journals', and that, 'to select the appropriate reviewers for each manuscript, the editor needs detailed information', including your vita. You should also say which APA journal you are interested in, and specifically state your area of expertise. Of course, you should also be prepared to invest the time and commitment required to thoroughly evaluate a manuscript. If you feel you are qualified, write to Leslie Cameron, Journals Office, American Psychological Association, 750 First Street, NE, Washington, DC 20002-4242, USA.

Publication Policy and Instructions to Authors

The aim of the *European Journal of Parapsychology (EJP)* is to stimulate and enhance activity in parapsychology, especially in Europe, by communicating research results and issues related to parapsychology. To this end, *EJP* publishes original papers on a variety of topics including empirical, theoretical, historical and sociological issues. Translations of papers originally published in a language other than English are also welcome.

The *EJP* actively encourages the submission and evaluation of papers before the experimental data have been collected. The study's rationale, number of subjects and trials, hypotheses and planned statistical analyses should all be included in such submissions. Priority will be given to the publication of such studies.

Manuscripts, written in English, should be submitted preferably on computer disk in ASCII format. Otherwise, authors should submit three copies, typed double-spaced throughout. The organization of the paper must be clearly indicated by appropriate headings and subheadings. Include names of authors, positions and other affiliations, and mailing addresses. Each table should be given a descriptive title. Number tables consecutively with Arabic numbers in order of their mention in the text. Figures should be drawn on white paper in black lines: because they are normally reduced in reproduction, allowance should be made for the corresponding reduction in the thickness of lines and the space between them. References in the text should be indicated by dates in parentheses following the authors' names and should correspond to an alphabetical list at the end of the paper. References to books should include year of publication. References to journal articles should include volume number and year of publication. When quoting from a book or article, indicate the page(s) on which the quote appears. For statistical analyses,

report not only the inferential statistics (e.g., *t* values) but also the descriptive statistics for the data evaluated (e.g., group means and standard deviations). Report the number of subjects, trials or experiments on which the analysis was based, and where possible report effect sizes (e.g. actual values of correlation coefficients), and give exact probability values. Such information can be vital for later cumulative reviews, including meta-analyses. The *EJP* adheres to British Psychological Society style rules; please see volume 10, p. 151 for details. Each manuscript must include an abstract of not more than 200 words.

The *EJP* is published in the first quarter of each year. **The annual deadline for submissions is 31 August.** Subscription fees include shipping costs, and subscriptions may be taken out for one year (£12) or two years (£24). *For members of the Parapsychological Association*, a reduced rate is available of £10/£20. Please make payment in Pounds Sterling. Payment in any other currency must include an additional £8 for the cost of currency exchange to Sterling. Cheques should be made payable to 'The Koestler Chair of Parapsychology'. All submissions and subscriptions to and enquiries about the Journal should be sent to *European Journal of Parapsychology*, Koestler Chair of Parapsychology, Department of Psychology, University of Edinburgh, 7 George Square, Edinburgh EH8 9JZ, Scotland, UK.

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