

CONTRA GEORGE HANSEN'S FLAWED CRITIQUE OF THE WORK WITH B.D.

BY EDWARD F. KELLY

George Hansen has alluded on several previous occasions (e.g., 1988, 1990) to supposed weaknesses in our work with B.D. More recently (1991, 1992), however, this has become for the first time a full-scale attack, one which Hansen apparently believes to be highly successful and which he therefore uses as a stepping-stone toward a larger thesis concerning the proper role of magic and magicians in psi research.

I will confine my response largely to Hansen's discussion of the formal publications describing our research with B.D. In brief, although I have qualified sympathy for the larger thesis, I have virtually none for his treatment of our experimental work or for his abuse of others for their confidence in it.

Let me begin by giving a correct account of the overall flow of the work. There were two periods of intense activity. The first occurred in February and March of 1972 during B.D.'s first visit to Durham and was chiefly directed toward (a) determining B.D.'s capacity for psi performance under controlled conditions, and (b) identifying potentially productive directions for systematic research. The main experiments—the single-card clairvoyance and shuffles experiments—took place between October 1972 and May 1973 during B.D.'s leave of absence from Yale Law School. This later research was supported by the Hodgson Fund, following B.D.'s appearance at Harvard at the session described in Persi Diaconis's 1978 *Science* paper, a subject to which I will return.

The work carried out during the first period (Kelly & Kanthamani, 1972) consisted not of *experiments* in the normal sense but of *tests* conducted with a wide variety of standard devices and procedures then in use at the FRNM. Foremost among these early efforts was the work with Helmut Schmidt's four-button machine. Hansen

This paper is a revised version of remarks originally prepared for oral presentation at the 1991 PA Convention by invitation of the Program Chair, but withdrawn when Hansen did not appear to deliver his own paper.

I wish to express my gratitude to Irvin L. Child for his help and constructive criticism throughout this project and to John Beloff, Gertrude Schmeidler, and John Palmer for reading earlier drafts of this paper and providing their comments.

dismisses all these results in a single short paragraph, primarily by reference to the Radin and Nelson (1987) meta-analysis of REG work, which he says "assigned these studies some of the lowest possible quality ratings." This seems to me an entirely inappropriate use of Radin and Nelson's rating scheme, which is concerned primarily with assurances of REG quality needed to support reliable measurement of small deviations from mean chance expectation in large numbers of trials (the normal situation even in experiments with subjects preselected for ability in the pertinent task). By contrast, consider B.D.'s first session in the FRNM library, with J. B. Rhine and Helmut Schmidt observing along with Kanthamani, myself, and other members of the FRNM staff. During the course of this get-acquainted meeting, in which he mainly chatted with us about himself and his views of psi, B.D. intermittently made responses on the four-button machine. Over the course of the approximately hour-long session he accumulated a total of 180 hits in 508 trials, for a scoring rate of 35.4%. (These figures, incidentally, were corroborated by the independent duplicate counters sealed inside the device.) The probability for this one outcome is under 10^{-6} . Had its occurrence depended on chance alone, the meeting might well have gone on for something on the order of a million hours (since the expected waiting time for an event of probability p is $1/p$). Even stronger performances were observed less formally, and the visit ended with a formal series of eight sessions, recorded both manually and on paper tape, in which B.D. progressively raised his scoring rate from 27% to almost 31% ($CR > 6$, $p < 10^{-9}$).

These very unusual scores, moreover, cannot plausibly be attributed to hypothetical failures of the test device. The physical device in question is the quantum-mechanical REG originally developed and tested at Boeing by Helmut Schmidt and brought by Schmidt to the FRNM for use in his continuing research. In addition to the various mechanical and electronic safeguards built into it, the device had been subjected to an unusually thorough regime of randomness testing specifically directed to the forms of potential failure that could be anticipated in light of its electronic design. Schmidt's original reports (1969, 1970) should be consulted for details, but control tests involving 4.7×10^6 numbers were generated on 100 different days over an 18-month period, interspersed with on-going experimental sessions and using a generation rate close to B.D.'s own routine response rate of about one per second. The results were analyzed both overall and in blocks of various sizes, including blocks approximating the size of B.D.'s tape-recorded sessions. Additional

control tests were carried out using the tape-recorded guess sequences of successful subjects to trigger the REG at a variety of input rates. In none of these tests was there any sign of significant departure from ideal randomness. Additional, more routine randomness checks conducted both before and after B.D.'s visit lend no support whatsoever to any casual speculation that local or global failures could have occurred on a scale sufficient to explain the extreme scores produced by B.D. I should also point out that large numbers of other persons, including myself, were trying hard with the same device, and in the same time frame, with very little success.

In short, I categorically reject Hansen's offhand dismissal of the REG results with B.D. Furthermore, Helmut Schmidt himself, who was present throughout B.D.'s first visit, is equally uncompromising in the view that this REG work by itself conclusively established B.D.'s capacity for controlled psi performance (personal communication, August 1, 1991).

I have dwelled on the REG work at length both for its own sake and because it formed for us a crucial part of the context in which the subsequent card experiments evolved (Kanthamani & Kelly, 1974a, 1974b, 1975). For us the goal of these later experiments was not to demonstrate that B.D. "had psi," but to learn something significant about its modes of operation. In that context, we deliberately sought to establish experimental conditions that would enable us to elicit strong performance from B.D. even if those conditions were less than ideal, *provided*, however, that they did not compromise in any fundamental way the integrity of the experiments, even on the assumption that the subject would be inclined and able to cheat if given the opportunity. This is the spirit in which the card experiments were designed and conducted. The only point at which we bent these rules even further was in the later series of the shuffle experiments, which certainly do not stand on their own in terms of the quality of conditions. However, in the published reports we ourselves clearly identified these weaker series, explained why we permitted them in light of B.D.'s previous performances, and segregated their results in analysis and discussion.

Hansen construes not only these "special" modifications but every other aspect of protocol originated by B.D. as presumptive evidence that B.D. obtained thereby an opportunity to cheat, and that he did in fact cheat. It is crucial to recognize, however, (a) that the alleged "flaws" are very abstract in character, and (b) that Hansen provides no direct, positive evidence that they actually occurred in a form that enabled cheating to occur. In fact, there are strong

counterarguments against all of Hansen's allegations, and especially against those involving the single-card clairvoyance series, which we too regard as the most significant part of the later work. Let me therefore comment briefly on each of the four major "flaws" Hansen claims to have identified.

1. *Commotion.* Hansen quotes a statement we made in our first paper regarding the need for sometimes heated arguments to convince B.D. to work in an experimental setting (Kelly & Kanthamani, 1972, p. 88) and uses it repeatedly to paint a picture of experimental sessions routinely characterized by commotion and distractions that provided B.D. with cover for his "moves." Such accusations are grossly inaccurate, however: Even during the initial visit, the argumentation—entirely between myself and B.D., by the way—took place *outside* the context of the formal testing; for example, during long walks around Duke's campus, trips between the lab and my house (where B.D. was staying), and late-night discussions of what we as experimenters were trying to do, and why. By the time of the later card experiments, B.D. had become much more tolerant of experimental requirements (although he still grumbled about them, and clearly found the work stressful), and these sessions were routinely professional and quiet.

2. *Confederates.* The primary potential exception to these statements concerns a subset of sessions in which additional persons were present. Hansen speculates that this could have permitted confederates in the room to glimpse the target cards and signal them to B.D. This suggestion also is without merit. Even had confederates been present, I very strongly doubt that they would have been able to "glimpse" anything (as discussed in more detail below), and there are very few candidates in sight. According to our records only two sessions introduced true "visitors" in the sense required by Hansen's criticism. One of these involved as its single visitor a law-school classmate of B.D., who certainly must be regarded as a potential confederate, and the scoring for this session was good although far from the best in the series. The other involved a group of students from Tennessee, and although conditions were optimal in this session for B.D. to take advantage of commotion and confederates, it actually produced some of the worst scores of the entire experiment (0 exact, 1 number, and 3 suit hits in 15 trials).

3. *Recording errors.* Repeating a suggestion previously made by Akers (1986), Hansen speculates that the recording procedure we adopted might have permitted B.D. to glimpse the target before he made his response, thus providing another example of how aspects

of protocol suggested by the subject enabled him to cheat. It should be pointed out first that this suggestion flatly contradicts the clear statement, repeatedly made in the published reports, that the experimenter *first* recorded the call and *then* displayed and recorded the target. This was easily accomplished because of the unusually slow rate at which trials were generated (typically on the order of one per minute). That an experienced experimenter such as Kanthamani would permit numerous breaches of this essential feature of protocol is simply not credible in the absence of positive evidence. In this regard, Akers's comments are not "especially weighty" (as Hansen contends) because (a) he did not witness the main experiments, but only a brief series carried out subsequent to my departure from the FRNM, and (b) even in that context he did not actually observe the hypothesized breach of protocol. Furthermore, this "flaw" in principle does not apply to the batches of no-feedback trials interspersed through the series, for which the recording of the targets was carried out separately after the recording of the calls. Nevertheless, the scoring for these (179) trials was actually substantially *higher* than it was for the normal feedback (289) trials of the same runs (4.36 vs. 2.34 exact hits and 9.0 vs. 7.02 number hits per 52 trials, with mean chance expectation being 1 and 4, respectively).

4. *Visual leakage.* Hansen's most serious allegation, in my estimation, is that B.D. might have used or arranged reflective surfaces within the experimental room to obtain occasional glimpses of the target cards as they were being selected and presented by the experimenter. Hansen intimates that we did not consider such possibilities, and he interprets our finding of visual-like confusion patterns in the clairvoyance data (Kelly, Kanthamani, Child, & Young, 1975) as evidence that visual leakage did in fact occur. This finding, however, is not an incidental result but the *main* result of the single-card clairvoyance study, the principal goal of which was to compare the systematic errors B.D. made under visual versus ESP conditions. This central comparison is obviously devoid of value unless the conditions of the ESP trials precluded visual access to the targets. To this end, we adopted a set of conditions specifically developed for this purpose by Irvin Child during his own earlier (and only marginally successful) work with B.D. To recapitulate briefly, successive targets were drawn from a pool of 10 decks of ordinary playing cards. The cards were thoroughly shuffled and arranged sideways, with their backs toward the experimenter, in a cardboard box kept in the bottom drawer of a large solid-backed office desk. The experimenter "randomly" selected a card and, with its face down, par-

allel to the floor, inserted it into an oversized black folder. I emphasize that the protocol required this target selection and masking to take place entirely within the bottom drawer, below the height of its walls, and was intended to assure that neither the subject nor the experimenter could have visual access to the target. The folder was then held up and displayed to B.D. with the back of the card facing him inside it.

I assert categorically, on the basis of our examination of this possibility prior to initiating the experiments, that under these conditions there existed *no* ready-made optical paths enabling B.D. or anyone else in the room to glimpse a target by way of windows, doorknobs, spectacles, or other reflective surfaces routinely present.¹ I also find it extremely implausible that B.D. could successfully have introduced, and used on numerous occasions without detection, an additional optical path or paths of his own construction. Such a path would necessarily have been complex and would have involved an outer wall or the floor of the drawer holding the cards. As experimenters and observers, we were in and around that drawer virtually every day, and at unpredictable times, and we never detected any trace of tampering with either the drawer, the desk, or the cards themselves. I should also mention here that we had told B.D. on a number of occasions (particularly during his first visit) that if he were ever caught cheating we would immediately terminate the experiments and renounce all previous work with him. In sum, although this alleged "flaw" is more open-ended than the others and we cannot claim to have dispatched it as conclusively, I believe that any fair-minded observer familiar with the experimental procedures and the physical setting would conclude with us that the visual-access hypothesis is not tenable, particularly in the generic form that Hansen advances unaccompanied by any specific proposal as to how such access could have been obtained. Indeed, to my mind the leakage hypothesis that we ourselves originally suggested and rejected—that is, leakage arising from breaches of the card-handling protocol (Kelly et al., 1975)—is the *least* implausible of the various non-psi hypotheses offered to date in explanation of the ESP confusion patterns we observed.

I will say little about the shuffles data beyond what I have already said. The failure of the visual-like confusion pattern to appear in these data, which Hansen interprets simply as evidence that the

¹ See also the more detailed description of the physical setting by H. Kanthamani presented in an accompanying article.

cheating in this series took a different form, has a good alternative interpretation: It was a different task, and B.D.'s achievement of high scores with small numbers of shuffles provides a statistical argument for construing the psi effect in this series as a PK effect rather than an ESP effect (see Kanthamani & Kelly, 1975). Furthermore, if the visual-like pattern *had* appeared, I am sure Hansen would have been equally quick to interpret it as evidence that B.D. cheated by glimpsing cards through the holes in the box or by momentarily exposing their edges. I would also like to point out in this connection that Hansen conspicuously neglects to mention two runs from this series that were specifically immune to the general form of cheating he suggests (inasmuch as B.D. did not touch the cards after his shuffling was completed). Both of these runs yielded even higher scores than the series at large, with 5 and 7 exact hits representing independent Poisson probabilities of .003 and .00007, respectively.

This completes the main outline of my responses to the substance of Hansen's critique; but before concluding, I also want to comment on what I perceive as an underlying double standard in terms of Hansen's willingness to accept without apparent question, in support of his own views, the unqualified defamatory statements regarding B.D. that have been issued by magicians such as Randi, Gardner, and especially Persi Diaconis. I find it particularly galling in this regard that Hansen chastises me for not inviting Diaconis to participate in the formal studies. Let me immediately set the record straight on that: I had never heard of Persi Diaconis until he published his paper in *Science* in 1978, six years after the session at Harvard and five years after completion of the card experiments. Had he approached me at the Harvard session and offered his services, I would have accepted on the spot. However, he apparently felt no obligation even to introduce himself, let alone to inform me regarding his suspicions.

The background of Diaconis's involvement in the Harvard session may also be of interest, albeit less certain historically. I had initially invited his statistical mentor, Fred Mosteller, to attend the meeting, but to my surprise Mosteller heatedly refused, on grounds that in his opinion Edgar J. Coover had already conclusively demonstrated in the 1920s that ESP research is all snare and delusion! I suspect that Mosteller then dispatched Diaconis to that session as his agent, and with deliberate secrecy, for the sole and specific purpose of "exposing" Bill Delmore. I further suspect that Mosteller, a former president of the AAAS, was instrumental both in *Science's*

publication of Diaconis's 1978 paper and in their refusal to follow the editorial practices stated on their own masthead in regard to the detailed reply that I submitted *as a paper* the following week (Kelly, 1979).²

Ironically, Diaconis himself characterized our published papers as describing experimental conditions "beyond reproach," but dismissed their results on grounds that the *actual* conditions must have differed radically (in some unspecified way) from those we described. The basis for this astonishing suggestion rests on a deliberate and untruthful characterization of the Harvard session, which Diaconis knew was completely informal, as not just one but a whole series of "experiments" in the normal sense. To my knowledge Diaconis has never directly addressed the details of our published experimental reports; indeed, I have some doubt, personally, that he had even *read* them at the time he wrote his *Science* paper, which in my opinion falls far beneath the customary standards of that journal (Kelly, 1979). Nevertheless, people such as James Randi and Martin Gardner—and no doubt numerous others by now—refer to his *Science* paper with unqualified approval. Can anyone seriously imagine these people to be open-minded students of the work with B.D.? In effect, they will use every means available, including unconstrained appeals to the powers of magicians, to dismiss any experimental outcomes they do not like. Involvement of magicians in work with special subjects can perhaps offer *some* protection against this sort of thing, and to that extent is desirable; but it is simply naïve to imagine that it can provide immunity. Instead, it simply raises new issues about the relative credibility of the various magicians who might line up on either side.

To conclude, I submit that we did in fact "reasonably exclude" cheating in our work with B.D. and that Hansen's critique has little or no real substance. In fairness, however, I must also acknowledge that some of Hansen's misperceptions were certainly encouraged, or at least not specifically *discouraged*, by what appears to me in retrospect to be a definite failure on our part to report sufficient detail on a number of significant points. I can and do thank him, therefore, for enabling us to rectify these deficiencies while we are still in condition to do so. I also think there is an underlying issue here, the open discussion of which could represent another positive outcome of this exchange. We deliberately chose in our write-ups of the

² The only response I ever received from *Science* was a phone call several months later from the "Letters" editor, who wanted to discuss which one paragraph they should select from my nine-page rebuttal (Kelly, 1979).

work with B.D. to avoid dwelling in a paranoid way on our defenses against possible attempts at cheating on the part of the subject. In hindsight I am inclined to think we went somewhat too far in that direction, to the ultimate detriment, perhaps, of both B.D. and ourselves. However, I would still argue strongly for a middle ground between our approach and that exemplified by Honorton's recent report of his work with Malcolm Bessent (Honorton, 1987), in which the central purpose and outcome of the experiment practically disappear beneath the welter of precautionary details. There is certainly room for expression of personal preference in these matters, but it might also be appropriate for the Parapsychological Association to try to establish guidelines for the conduct and reporting of future research involving special subjects. Collaboration with professional magicians could be useful, I think, provided that the participating individuals are not only technically qualified, but also emotionally and intellectually capable of entertaining the possibility that genuine psi phenomena do occur.

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*c/o Institute for Parapsychology
P. O. Box 6847, College Station
Durham, NC 27708*

A RESPONSE TO GEORGE HANSEN'S CRITIQUE: SOME SUPPLEMENTARY NOTES ON THE RESEARCH WITH B.D.

BY H. KANTHAMANI

In the preceding article in this number of the *Journal*, E. F. Kelly (1992) specifically addresses Hansen's alleged "flaws" in our work with B.D. Therefore, I will not discuss them in detail here. My own response is intended to furnish some supplementary information on the B.D. research in support of Kelly's rebuttal. For convenience, I am presenting this in two parts: Part I provides additional information on the research itself, and Part II addresses Hansen's allegation that we have not responded to his criticisms of our work. A summary and conclusions section is included at the end, which lists all the criticisms along with our responses.

Part I

Additional Information on the Research with B.D.

I would like to provide additional information on two aspects of the research with B.D.: first, I want to present further details relating to the procedures used in the experiments; and second, to recreate the physical setting that existed in the experimental room. These details were perhaps not spelled out sufficiently in our original publication; and it may be helpful to have them now for a better understanding of the conditions surrounding our work with B.D. I am restricting myself to the single-card clairvoyance experiments here, for, as Kelly (1992) has indicated, it is these data that form the most significant part of our experimental results.

I wish to acknowledge with gratitude the help and support provided by Irvin L. Child throughout the various stages of this work; without this it would have been impossible to provide all the details of the B.D. research. My thanks also go to John Palmer for reading an earlier draft of this paper and providing his comments, and to Richard S. Broughton for preparing Figure 1.

Single-Card Clairvoyance (SCC) Procedure

The SCC method was first introduced by Irvin Child in his work with B.D. prior to our FRNM research. He developed a set of procedures by which one could present a single target at a time to the subject, so that he/she could concentrate on it before making a response. Another feature of this method relates to the type of feedback provided. The experimenter, immediately after recording the response, proceeds to reveal the target so that it will facilitate subject's "checking" his internal cues for gaining insights into successful strategies for hitting. Thus, each trial forms a distinctive unit by itself and, as such, presents a unique challenge to the subject to focus his/her best efforts at every try. Child labeled this procedure the "SCC method," which is basically a modification of the old BT technique used by Rhine and others in the early Duke work (Rhine, Pratt, Stuart, & Smith, 1966).

An opportunity arose during 1972-1973, as noted by J. B. Rhine (1972), for a unique collaboration. B.D. obtained a one-year leave of absence from the Yale Law School, and Child used his sabbatical to spend six months at the FRNM; a grant from the Hodgson Fund (of Harvard University) supported B.D. for his work in parapsychology; and at the FRNM, E. F. Kelly, who was then on the staff, arranged for the collaboration to occur. In preparation for this, Child had worked with B.D. while both were at Yale, exploring different methods of testing. During this period, Child recollects (personal communication, April 29, 1991) that he initially used small, opaque, manila envelopes, each with a playing card inside, and that he presented them one at a time in a predesignated order. This forms the beginnings of the SCC method. However, Child noticed that taking out the target from an envelope to provide feedback after each trial took considerable time, which B.D. found somewhat frustrating. B.D. asserted, both to Child and to us later, that it was important for him to have quick feedback after making a response so that he could verify the actual target against the array of visual images he experienced at each try. To accommodate this idiosyncratic need, Child later introduced the folders, which, while still enclosing the target completely, aided faster feedback in revealing the target. These folders were prepared from opaque black construction paper. Child carried out some preliminary trials using the folders, which proved satisfactory both to him and to B.D. When he later came to the FRNM, Child continued to use the folders in his work with B.D. At this time Child also introduced the idea of cre-

ating a target pool by mixing 10 decks of ordinary playing cards ($N = 520$), from which successive targets were drawn "randomly" by the experimenter. (For further details, refer to Kanthamani & Kelly, 1974a; and Kelly, Kanthamani, Child, & Young, 1975). It may be noted that, prior to working with B.D., Child had used the SCC method in a long series of experiments (52 runs of 52 trials each) with L.H. (another special subject who was then at the FRNM).

I was away in India on a long vacation when Child initiated his SCC series with B.D. at the FRNM in the fall of 1972. By the time I returned and was invited to join the experimental team, Child had already accumulated a large database totaling 65 runs of 52 trials each. It is noteworthy that the results of this batch were not on a par with B.D.'s performances during his first visit (Kelly & Kanthamani, 1972). At best they can be considered as borderline in statistical significance ($p < .01$, only for the suit hits, although there was a strong trend toward improved scoring in the last seven runs). The significant drop in B.D.'s performance, compared to what we had witnessed a few months earlier on a variety of tests, motivated us to start a new series with a change in the experimenters, rather than to discontinue the whole line of research with SCC. We felt such a change might serve as a "novelty effect," well recognized in parapsychology literature. I had been an observer in Child's experiments with B.D., and when I felt sufficiently comfortable with the method he was following, I assumed the experimenter's (tester's) role in the new series. This led to the development of the Kanthamani-Kelly series, which was carried out exactly following the procedure used by Child (Kanthamani & Kelly, 1974b).

There were four series in all. The length of each was determined in advance, although our goal was to accumulate as much data as possible without sacrificing the subject's or experimenter's motivation. The first two series were planned as a pilot-confirmatory unit, each with 13 runs. The second two series had 10 runs each, which included an additional feature that formed a part of another study (Kanthamani & Kelly, 1974a). The first series had no extra observers, and it was witnessed by me alone. The results were highly encouraging, with an excess of number hits ($CR = 2.89$), but there was missing on suits ($CR = 2.49$). We felt B.D. was getting back on track and that his scoring level would soon stabilize. At this stage we invited other interested members of the FRNM staff to observe the sessions occasionally. Obviously, their responsibility included scrutinizing the proceedings and to report if they found any improper procedure. True outside "visitors" in the technical sense were pres-

ent only in two sessions, as noted in Kelly's (1992) article. The outcome of the second series showed a steep incline in B.D.'s performance, at which point the more challenging task of "confidence calling" was introduced in the remaining two series (Kanthamani & Kelly, 1974a). The scoring rate held up very well all the way through, although B.D. found the confidence-calling very stressful. This formed the natural end of the series; by then we had collected a total of 46 runs of 52 trials each, in four series.

It is this set of data, namely, the Kanthamani-Kelly series, that George Hansen finds problems with, and not the Child series, although both were carried out according to identical procedures. In fact, Child had transferred all his testing materials to us for our use, and we had created a similar testing set-up by having an identical desk and seating arrangement for the experimenter and the subject, as well as storing the targets similarly inside the bottom drawer of the desk. There is nothing we can think of about the experimental procedure that was different and would have compromised our competence as careful investigators or allowed the subject any opportunity not present in the earlier series to engage in fraud.

Furthermore, to complete the report on the Child series, there were eight additional runs¹ carried out subsequently to those mentioned above, for a total of 73 runs of 52 trials. The Fisher chi square (refer to Kanthamani & Kelly, 1974b, for details of this method) for all 73 runs is significant ($\chi^2 = 21.2$ with 8 *df*; $.005 < p < .01$), with the effect concentrated in an excess of suit hits ($CR = 2.55$, $p < .01$). There was also a sharp rise in the scoring level in these last eight runs, which were carried out *following* the first Kanthamani-Kelly series, with over twice as many exact hits as his earlier average (2.125 per run, including runs with 4 and 5 exact hits, overall $CR = 3.2$). As far as we were concerned, this trend indicated that B.D. had overcome his initial barrier, whatever its nature, and had adapted himself to the experimental routine. We may recollect here another situation when B.D. had shown a similar capacity for overcoming his initial barrier and adapting to the demands of the experimental set-up. During his earlier visit, we had collected a large batch of data on the Schmidt four-button machine. After many informal sessions, when the automatic recording on the

¹ The total number of runs as reported here is one less than the published version (Kelly et al., 1975). The extra one refers to a run where B.D. used a different strategy in making his responses than the rest of the SCC data. The discrepancy, however, was corrected in a later publication (Kelly, 1982), which reported the total number of runs in the Child series as 73.

punch tape was connected, B.D. first resisted the idea but then gradually built up his initial performance of 27% to almost 31% at the end of the series, for a total of 28.7% ($CR > 6$) on more than 5,000 trials (Kelly & Kanthamani, 1972).

A few observations about the prevailing socio-personal environment may also be worth recording here. The degree of rapport and camaraderie that existed between the "subject" and the "experimenters," as well as among the "experimenters" and other members of the FRNM team, was remarkable and unmatched by any other time in my long experience of nearly 30 years in parapsychology. The motivational level was incredibly high among us, who happened to be in many respects in unique situations in our lives and careers. There was very little stress from other sources, and we were able to devote our energies fully to the on-going experimental work. We also spent a lot of time together socializing away from the lab. However, I should add, all of us were professional psychologists, well versed in treading the fine line between personal cordiality and professional responsibility.

Physical Setting of the Experimental Room

Both the SCC and shuffle experiments were conducted in my office, then located on the second floor of the Institute's building. Irvin Child's office-cum-testing room was also on the same floor along the same hallway. As mentioned above, the desk arrangement remained similar for both the Child series and the Kanthamani-Kelly series. Even the test materials, including the folders, remained the same. All the SCC experiments and about half of the shuffle experiments were conducted in one room, and the remaining shuffle series were completed in an adjacent room, which became my office later on. For the present, I will restrict my description to the first office, which formed the sole site for our SCC experiments.

The floor diagram as shown in Figure 1 describes the layout of the room. It is a fairly small room, measuring approximately 9 ft. \times 10 ft. 9 in. \times 9 ft. 3 in. Entrance to the room is through a wooden door of normal size (3 ft. \times 6 ft. 6 in.). It has a metal door-knob slightly less than 2 in. in diameter. There are two large windows side-by-side (each 69 in. \times 36½ in.) on the east side, which remained shut all the time. Normal venetian blinds covered the windows, and were pulled up about half-way for lighting purposes. In addition to this natural light, there was a single fluorescent ceiling lamp. There is a closet in the northeast corner of the room. The

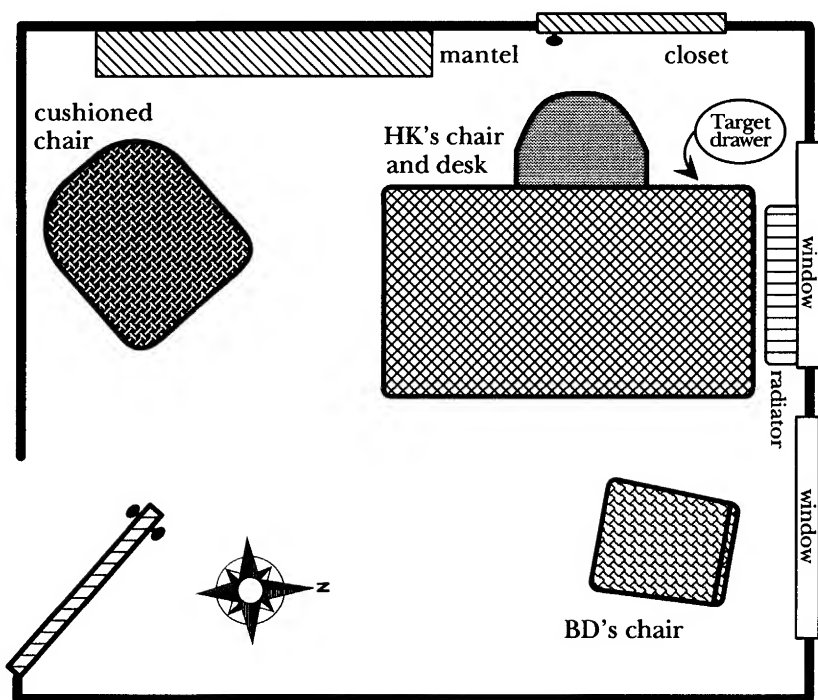


Figure 1. Floor diagram of the experimental room.

closet door is wooden, approximately 6 ft. 6 in. \times 2 ft. 6 in., with a metal doorknob fixed slightly below half-way down on the left-hand side. The knob, similar to the entrance doorknob, is slightly less than 2 in. in diameter, and the plate on which it is affixed in the center is approximately 7½ in. \times 3¼ in. The west side of the north wall is dominated by a small sealed fireplace with a wooden mantel over it (54 in. \times 7¼ in.), which served as a bookshelf. The fireplace had no metal fixtures or fireplace tools.

As to the furniture, there was a large wooden office desk and a couple of chairs at the northeast corner of the room, which constituted the testing area. The desk was in front of the closet, with the experimenter's back to the closet door, while the subject sat on the opposite side, usually facing the main door. The room's radiator, situated between the desk and the window, is about 24¾ in. in height and protrudes about 6½ in. from the wall. The desk had a solid wooden back extending almost to the floor (except for a gap of 6 in. from the bottom, which corresponded to the height of the legs

of the desk). There were two sets of drawers on either side of the front of the desk, and a central drawer (25 in. \times 3 $\frac{1}{4}$ in.). On the left-hand side, there were three drawers, the top two being identical (13 $\frac{1}{2}$ in. \times 5 $\frac{1}{4}$ in.) and the bottom one slightly larger (13 $\frac{1}{4}$ in. \times 6 $\frac{1}{4}$ in.). On the right side, the top drawer was the same as the left side, but the second was much larger (13 $\frac{1}{4}$ in. \times 12 $\frac{1}{4}$ in.). The writing arm on either side of the desk, when pulled out, measured 18 $\frac{1}{2}$ in. \times 13 $\frac{1}{2}$ in. in size. The chairs used by the experimenter and the subject were ordinary wooden office chairs, with straight or round backs. Usually the experimenter used the round-edged one (30 in. \times 21 in. \times 21 in.), and B.D. used the straight-backed one (35 in. \times 20 in. \times 17 in.). Because the solid back of the desk was facing the subject, B.D. usually sat facing the west wall with his long legs stretched parallel to the table. Whenever an assistant experimenter was present, a similar third chair was brought in. In addition to these chairs, there was one cushioned larger chair (36 in. \times 29 in. \times 26 in.) near the fireplace, which was sometimes moved around to accommodate an observer. Alternatively, the visitor(s) sometimes stood near the room entrance door.

There were no paintings, pictures, mirrors, or other reflective wall hangings. Also, there were no decorative pieces either on the desk or on the mantelpiece. No bookcases or even a telephone were present, and at that time I did not wear eyeglasses. It was a simple and essentially bare office room.

Visual Leakage and Fraud Hypotheses

From the preceding description it can be noted that there are two primary ready-made sources of potential "reflecting surfaces" in the experimental room, namely, the two windows and the two doorknobs and a doorknob plate. However, I must point out that to take the obvious precautions against such sources of error is a routine part of setting up any new psi experiment. Surely it is not credible that we would have failed to take them into account. However, to address this issue more directly, let me refer to the seating arrangement, which precluded all such possibilities. As can be seen from Figure 1, the testing area was at the end of the room away from the entrance door, and that doorknob provides no geometrically plausible access to the target site. Similarly, the closet doorknob (along with its plate) was behind me and to my right, whereas the targets were stored to my left in the bottom drawer of the desk. During testing, I leaned over and to the left and enclosed each target in its

folder *inside* the drawer before bringing it out for display to the subject. Additionally, the writing arm of the desk was pulled out, which provided further security. Likewise, the windows provided no visual access because they did not face the drawer in which the targets were stored, that drawer being well below the window pane and protected by the desk and the radiator.

Many times during the course of our research, both Kelly and I, as well as other members of the team, participated as subjects in informal trials sitting on the same chair used by B.D. No one ever claimed having discovered any form of sensory leakage in the procedure or in the setting. Kelly deliberately and systematically attempted, but failed, to find any angle conceivably available to B.D. that would provide visual access into the drawer. As far as we are concerned, any hypothesis involving *inadvertent* visual leakage is therefore completely without merit.

The other possibility, of course, is that without our becoming aware of it B.D. might have surreptitiously introduced certain gadgets to the experimental room that provided visual access to the targets. Several factors render this highly unlikely, however: First, had he placed any such objects in the room on a quasi-permanent basis they would almost certainly have been discovered. I constantly used this room, not only for B.D.'s experiments, but also for all my other activities. There were other "special subjects" I worked with at the Institute during that time. None of them, nor any other colleague who used B.D.'s chair when visiting me, ever reported noticing any foreign objects in the room. Second, if B.D. had brought reflectors into the room with him for use during the sessions we would likely have noticed that too. As far as his clothing is concerned, he never was overclothed, and in fact typically wore short-sleeved shirts and pants or shorts. It is also important to note: (a) that B.D.'s above-chance scoring was widely distributed through the four series of the SCC experiments, and (b) that he could not have succeeded in cheating simply by concealing reflecting surfaces on his own person, because altering his vantage-point in that way could not in itself provide visual access to the target drawer. That he could have succeeded on so many occasions to outfit himself and/or the room with the required devices, without ever being detected, is to us extremely implausible.

Although 20 years have lapsed since the B.D. experiments, there have been few structural changes in the FRNM building. The only noticeable change is the current floor-carpeting, which was not present then. Many of the original furniture pieces are still around and

in daily use. Therefore it would not be difficult to recreate to close approximation the original setting in order to investigate further the possibility of visual leakage, should anyone wish to do so.

Part II
Comments on the Allegation of Lack of Response

One of Hansen's allegations is that Kelly and I have not responded to his criticisms of the work with B.D. This is simply not true. Hansen was at the FRNM for more than three years (1981–1984), at a time when the Diaconis-Kelly debate was still current (Kelly, 1979). Never had he shown any concerns about our work with B.D. during that time, nor later when he used to visit the lab occasionally.

The first sign of Hansen's changing views was a letter I received from him dated November 29, 1987, along with a draft of his *JASPR* paper, "*Deception by Subjects in Psi Research*," in which he levelled various allegations relating to our work with B.D. nested among various other issues. His criticisms were briefly stated but essentially the same as they appeared in his later articles (Hansen, 1988, 1990, 1991, 1992). The tone of the letter was friendly and fair, however. He wrote: "I would be most interested in your comments. Although I've made considerable effort to accurately report your work, I may have erred. If so, please let me know. It seems more likely to me that we'll have disagreements over interpretations. I would prefer to resolve these (as much as possible) before submitting the paper for publication. I am quite open to making considerable changes and even fully reversing my opinion if it can be shown that my statements are unreasonable." Hansen further proposed to discuss his concerns face-to-face during a forthcoming visit to the FRNM.

All this sounded like a good and constructive beginning for a professional exchange. However, the ensuing meeting belied our hope, when it quickly became obvious that we were not in fact dealing with an open-minded critic. Kelly and I strived to answer Hansen's questions, to show him the room where the experiments were conducted, and so forth, but he essentially ignored our input and adamantly maintained his original positions. The session ended rather abruptly, leaving some unpleasantness and disappointment on our part.

Then came another letter from Hansen dated December 14, 1987, thanking us for the failed meeting. Along with this he en-

closed a 2½-page summary of his criticisms, which, while stating his position more candidly, only increased the number of allegations against our work. He further proposed that we could have a "public debate" at a conference organized by the ARE (Association for Research and Enlightenment) at Norfolk, Virginia, scheduled for the following February. We felt this would be futile, as Hansen now seemed to have formed an unshakeable opinion based on preconceived conclusions regarding our work with B.D. Accordingly, in my reply dated December 21, 1987, I wrote stating that we were not interested in such a debate, nor in any telephone discussions, but we would attempt to respond in writing to a full public statement of his views. I also suggested to Hansen that he should seek Professor Child's comments in this matter.

Next, there arrived a letter from Hansen dated January 20, 1988, along with a copy of the revised *JASPR* manuscript. The revision did not include any change in his criticism of our work. It may be noted that these two versions of his paper on deception are too complex for anyone to respond to easily, as they attacked many areas of research (and researchers) simultaneously. (In fact, at our meeting we ourselves had suggested to Hansen that he write a separate paper on B.D. so that we could respond cogently to specific and detailed criticisms, rather than trying to defend ourselves against remarks delivered in the context of a shot-gun blast aimed at the entire field.) Because of the pressure of other commitments, and because Hansen's criticisms at this stage still seemed to us so unsubstantial, we elected not to respond at that time.

The next significant event occurred at the P.A. convention at Montreal in 1988. I noted in the program a paper by Hansen entitled "*Risks of Deception by Subjects.*" I was completely in the dark as to whether this paper contained any aspects of Hansen's criticisms relating to the B.D. work, for neither he nor the Program Committee had informed us about it beforehand. Quickly skimming through Hansen's paper as printed in the *Proceedings*, however, I noticed only a passing reference to our work and thus saw no need to concern myself further. Consequently, I was shocked and appalled by the broadside launched against us during his oral presentation. Demanding first that all audio and video recordings in the auditorium be turned off (I have no idea why!), he abandoned his published text and presented instead an overhead projection relating solely to the experiments with B.D. His criticisms of our work, accompanied by complaints that we did not respond, and so forth, formed the focus of his entire presentation. In effect what should

have been a scientific session was turned into a "sneak attack." During the question-and-answer period, I tried to defend myself, explaining that we had indeed responded to him and referring to the meeting at the FRNM a few months earlier. I also objected that it was quite unethical to drag me into such a discussion without prior notice, and I reminded Hansen that our commitment was only to a written response in an appropriate forum and not to a public debate. It was also unfortunate that Kelly was not present to join in the defense.

This incident further ruptured the already strained relations and channels of communication. A few weeks later a private letter of apology did come from Hansen dated September 8, 1988, expressing his regrets for what had happened at the convention and complimenting us for our other contributions to the field. About the same time, he had written to John Palmer, whom in Montreal he had also accused, along with a number of other prominent parapsychologists, of endorsing our research. Hansen was seeking Palmer's further reaction to a list of comments he had sent relating to B.D.'s work. I collaborated with Palmer in his response to Hansen (October 11, 1988).

After this, there was no further communication for a long time. Hansen went on to publish in the *JASPR* his long "Deception" paper (Hansen, 1990) in essentially the form we had already seen regarding the work with B.D. Although his discussion of B.D. contained several inaccuracies and little substance, we still felt it did not provide the appropriate forum for a detailed reply. Parenthetically, let me add, the *JASPR* did not inform us in advance or invite any accompanying rebuttal comments.

After some time, we received another paper from Hansen (a draft of the present *JP* paper) along with a note (September 18, 1990) addressed to myself and John Palmer, seeking our comments. I acknowledged receipt (November 9, 1990); but we wanted to wait until Hansen actually submitted the paper to prepare our response. Eventually he did, and the editors of the *Journal of Parapsychology* invited us to respond. We immediately agreed to do so, once the final text of Hansen's paper became available. However, there was more to this. Hansen wrote to us that he was submitting the same paper for the 1991 PA Convention, and suggested that we should send our response also. We didn't relish the idea of hurriedly meeting the convention deadline, once again possibly getting dragged into a "public debate," especially since neither of us could be physically present (Kelly had recently assumed a new position at UNC-

Chapel Hill and I was already committed to a long-planned family vacation in India). However, when the program chair also invited a response from us, to be presented following Hansen's paper, we did prepare and send a response to be read by one of our FRNM colleagues. As it turned out, our response was not read because Hansen himself did not appear at the convention to present his own paper. Ironically, though, his paper had already been printed in the *PA Proceedings* (Hansen, 1991), whereas our rebuttal was not, since it was only an invited response. So once again the critic got the advantage.

The present submission by Hansen to the *Journal of Parapsychology* has provided the forum we sought, and we thank the editors, who made it possible for our rebuttal to appear together with the criticisms. I wish we had resolved most of the controversy by more open-minded personal and professional discussions. Since that did not take place, and since to the contrary we found such attempts to be counterproductive, *Journal* publication remained as the only alternative. It appears to me that, at some time, the field needs to address the scientific ethics of such controversies.

Summary and Conclusions

I believe that between Kelly's rebuttal and my response we have answered all the criticisms raised by Hansen in relation to our work with B.D. This task would have been easier if Hansen had presented his criticisms in a more piecemeal fashion. However, if we have left any points unanswered, it is only because we consider them trivial. Although I also have disagreements with him on many other aspects of his paper, I will not go into them here. The superficial way in which he has dispensed with our work presumably reflects the type of treatment other parapsychologists may receive. Restricting myself to our work with B.D., I offer the following comments as my summary statements:

1. Commotion and distraction was not an issue in our testing sessions. They were characteristically professional; in fact, we maintained a pleasant atmosphere throughout. Hansen misrepresents us here by repeatedly stating that the heated arguments (between the experimenter/s and the subject) might well have provided brief distractions that would allow a trickster to "make a move." This was simply not true. We pointed out this mistake to him, as well as many other aspects of his criticisms, in our first meeting (refer to Part II

of this paper for further details); but he chose not to correct any of them in his later versions of the paper.

2. Hansen also misrepresents the visitors/observers issue. In most cases they were other staff members, hardly likely to be in collusion with B.D. As noted in Kelly's article, the most plausible possibility involving confederates occurred on a single occasion when a friend of B.D. from law school was the visitor. A group of college students from out of state were present in one other session of the SCC experiment. In the shuffle experiment, there were no observers in the first three series (which, incidentally, Palmer [1985] considered to be a *weakness!*). A visiting journalist who referred to himself as a critic was present in two sessions of the remaining shuffle series.

3. Regarding the visual leakage hypotheses, let me quickly point out that the published reports of the SCC experiments have clearly stated that the target preparation, which included (a) selecting a target, and (b) enclosing it inside the folder, took place "out of subject's view," and that the experimenter herself had no glimpse of it. Hansen chooses to misconstrue this important aspect of the procedure and creates a complex scenario involving reflectors. I wish we had given more details at that time, which we hope we have accomplished in the present report. The important point to remember is that the whole process of preparing the target took place inside the bottom drawer of the desk, within its walls. It has also been documented here that the desk arrangement in the testing room, the desk's full-size solid back, and the fact that its writing arm was pulled out, all provided security against any form of visual leakage. There was no window at my back, or any other form of reflecting surface, except for the closet doorknob and its plate, which could not have served any leakage function because they were located on the opposite side of the drawer containing the targets.

Whether B.D. had succeeded in creating a complicated optical path of his own through certain special gadgets without ever getting caught in a period of six to seven months of testing, is to us highly improbable. Anyone who wishes to continue this argument should first show us how such a path could be created, given the details of the physical setting of the room. Maybe Hansen can recreate the setting and examine for himself how, and with what sorts of gadgets available 20 years ago, one could fraudulently create an optical path and keep it disguised for such a long time.

4. Hansen, when he talks about subject-based controls, misrepresents us by lumping together all the procedural aspects as having been instituted at B.D.'s demands. It is simply not true. We were sympathetic to the subject's needs, but still had the integrity of the experiments under our control. The SCC procedure, for example, was not dictated by the subject, but was carefully developed by Child and tested out on other subjects before being used with B.D. Only toward the latter part of the shuffle series did we allow B.D. greater freedom. We ourselves have clearly identified these weaker portions of the experiments and treated their data separately. Hansen misrepresents every aspect of our experiments as providing scope for B.D. to cheat.

5. Why weren't the tightest possible methods used? This is a fair question, which we have discussed in some detail in our published reports. To reiterate: our goal was not just to prove that B.D. "had psi," but to understand its *modus operandi*. Therefore, we provided special conditions to maximize the psi manifestation without sacrificing the basic controls necessary for parapsychological experiments. After establishing B.D.'s psi ability in our first article, our next attempts were directed toward understanding its unusually strong manifestations in this exceptional subject.

Exactly for this purpose, we launched a number of areas of research. In addition to the comparison of ESP and visual processes (Kelly et al., 1975), we undertook elaborate studies on personality and cognitive aspects (Kelly et al., 1973), as well as comparisons of other high-scoring individuals with B.D. in an attempt to understand the "psi burst" phenomenon (Kelly, 1982). Also, Kennedy used some of B.D.'s data in his studies on consistent missing and information processing mechanisms in ESP (Kennedy, 1979, 1980). Thus, our lines of research were programmatic, aimed at studying some of the mechanisms, at least with one individual, which we think succeeded to a modest degree. The experiments may not be technically perfect (but is there such a thing as a "perfect experiment?"), but neither were they flawed in any way that undermines our confidence in their main results.

6. Hansen tends to misrepresent Aker's position (1986) by stating that "Aker's comments are especially noteworthy because he conducted informal trials with B.D. . . ." As far as I know, Akers did not *conduct* any trials with B.D. by himself, although he witnessed part of an exploratory series (not the main SCC experiments) carried out long after our major projects had been completed and after

Kelly had left the FRNM. I was minimally involved in the actual testing sessions, as I had taken the new role of randomizing the targets.

Some additional details about this series may be worth noting here. It was conducted mainly to see how B.D. would perform when the two crucial aspects of the SCC method, namely, the folders and the manual sampling of targets, were changed (Kanthamani & Rao, 1974). Standard opaque black envelopes were used to conceal the targets, which were randomly selected either from standard random number tables or from RNG-based computer-generated random numbers. The results were encouraging in the initial runs, after which Akers volunteered as an observer and record keeper. A total of 526 trials were completed in all, which showed an excess of number hits ($CR = 2.04$). However, when the data were looked at separately according to the two types of target selection, an interesting pattern emerged. The trials for which targets were chosen from the RN tables produced above-chance scoring ($N = 369$, $p < .005$ by Fisher's method), whereas a negative deviation was obtained on the RNG-based trials ($N = 157$, n.s.). The same trend remained even when the analyses were restricted to the data witnessed by Akers ($N = 257$). Although the order of presentation was not controlled, B.D. was completely unaware of the nature of the randomization and that there were two types. The preference in favor of the RN-table targets over the RNG-based targets tends to support B.D.'s conscious dislike for mechanical methods, which he frequently expressed all through his work. It was mainly because of this that Child developed the manual quasi-random sampling technique used in the SCC method.

7. Some minor points:

a. In describing the visual task, Hansen misrepresents the procedure. He says: "[The experimenters tested B.D.] using tachistoscopically represented images of *playing cards*. B.D.'s task was to try to name the *card presented*" (my italics). (See Hansen's article in this number of the *Journal*, p. 316). We did not use cards; we used slides.

b. Hansen says: "The recording of targets and calls was not done on a blind basis" (see p. 314). This is only partially correct; calls were blindly recorded.

c. Hansen attributes the absence of confusion structure in the shuffles data to his suspicion that the SCC data must have been fraudulent. Then he says: "Perhaps he [B.D.] was able to surrepti-

tiously slide a corner of a card out from underneath the box and *steal a glance at it*" (my italics). (See p. 317). The question is, if B.D. had "glanced" at the targets, shouldn't such data show confusion structure similar to the SCC data?

d. Hansen not only treats our RNG work (Kelly, 1982) rather superficially, he neglects to mention Child's data ($N = 1800$ trials) on the Schmidt four-choice machine, which had a significant scoring rate (27.8%, $z = 2.74$, $p < .01$) and also a nice terminal salience.

In conclusion, let me point out that most of the criticisms raised by Hansen in relation to our work with B.D. are really not even new. John Palmer (1985) had earlier discussed many of the same issues in his report to the U.S. Army Research Institute. For example, Palmer considered the possibility that B.D. might have had a pocket mirror in his lap, through which he could have gained target information on certain trials. He also noted that the similarity in confusion structure between ESP trials and visual trials could be construed as supporting such a hypothesis. However, he rejected both, on the basis of an interview with me, when he learned that the desk used for the testing had a solid wooden back, extending almost to the floor, which precluded such sensory leakage. (One should also recall here that the target preparation took place inside the desk drawer, which further protected against any visual leakage.) Similarly, Palmer considered so-called "dermo-optic perception" as a possibility in the shuffles data, but he argued against it on the basis of the two extraordinary runs in which B.D. had no contact with the cards after his initial shuffling. Palmer also explicitly rejected inadequate randomization and recording errors as problems in the SCC data. Further, he takes issue with Diaconis by pointing out that Diaconis's objections are not applicable in the formal experiments and that he has not proposed any other plausible hypothesis to explain the experimental data.

In sum, we believe, that (a) in general, no one has yet produced a fraud hypothesis any more plausible than the ones we considered and rejected in our original reports; and (b) in particular, the criticisms raised by Hansen are neither novel nor substantial.

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Institute for Parapsychology
P. O. Box 6847, College Station
Durham, NC 27708

THE RESEARCH WITH B.D.: A REPLY TO GEORGE HANSEN

BY JOHN BELOFF

I have never met Bill Delmore and have never attended any tests in which he took part. The confidence I expressed in his case was based mainly on my high regard for those who tested him. If today I would be less confident, this would be due, not so much to any weaknesses in the experiments to which Hansen rightly draws our attention, as to the fact that B.D. himself has remained silent. It would, after all, be hard to conceive of any more dastardly act of treachery and duplicity than that of which he stands accused. An innocent man would seek to clear his name. I shall also want to see what response his investigators offer to Hansen's criticisms.

Hansen believes that B.D. deceived his investigators, exploiting, in the process, their ignorance of card tricks. Let us consider, then, the "single-card clairvoyance tests" that he discusses. I would naturally assume that the experimenter would place the target card in its folder *behind* the desk at which he/she was sitting and using that desk as a screen, would have "slipped it into the folder, all this out of the subject's view." (See Kanthamani & Kelly, 1974.) I would likewise assume that any observers present would stand on the *opposite* side of the desk from the experimenter. Am I mistaken in these assumptions? If not, then I see no possibility of B.D.'s making use either of a casual reflection of the target card from a polished surface or from a confederate.

In addition to his prodigious success on these card tests, B.D. also achieved a high score on the Schmidt machine. So far as I know, there is no way in which one could fake such a score on this device without first dismantling it. Hansen complains that the machine had not been tested for randomness. Is he seriously asking us to believe that B.D. was just the lucky beneficiary of a faulty machine? Kelly and Kanthamani (1972) tell us: "Under good conditions at the Institute, with Helmut Schmidt and J. B. Rhine observing, he produced a complete run of 508 trials with 180 hits for a *CR* of 5.4, $p < 10^{-7}$."

Hansen has singled me out, along with Gertrude Schmeidler, as an example of those whose ignorance of conjuring has made them

vulnerable to deception. I am honored to find myself in such distinguished company, but I am sure Gertrude does not need me to defend her. I will therefore confine myself to just three instances where my own judgment is impugned.

1. *Glenn Falkenstein*. Presumably something I must have read in that Australian newsletter must have given me the impression that Falkenstein (whose name I had never heard before) might be worth investigating. I then promptly forgot that I had ever written that brief letter. Later I learned that Falkenstein is, in fact, a well-known conjuror. So what? That Hansen should bother to pick on such a trifle shows how desperate he must be to discredit his opponents.

2. *Margery*. I am not in the business of "promoting" anyone's mediumship. My concern only is to get at the truth. All the world knows that there was much that was suspicious about the Margery mediumship. However, Tietze has not said the last word and, with the publication of Marian Nester's new book about Margery, the case will be due for a reevaluation. Meanwhile, I challenge Hansen to say whether he thinks: (a) that the wooden rings which Margery is credited with linking paranormally on many occasions never, in fact, existed, so that this whole episode is a myth perpetrated by a number of professional men who conspired together to fabricate the documentary and photographic evidence or (b) that his knowledge of conjuring enables him to say *how* she faked these objects?

3. "*Tim*." Hansen's remarks suggest that one is damned if one fails to consult a conjuror and damned if one does! In fact, Randi gave us very good advice on the protocol that we should use with "*Tim*," who, as a result, never once succeeded in bending metal in our laboratory. Eventually he was caught out by being left alone with a concealed camera.

I would never want to deny that a knowledge of conjuring is an asset to a parapsychologist, and I regret that I was unable to give my students such expertise. I would suggest, however, that it is not enough to be sophisticated about conjuring techniques. If Hansen wants us to reject such outstanding cases in the literature as those of Serios or of B.D., he must offer us a more convincing counter-explanation.

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

RESPONSE TO HANSEN: BACKGROUND, CORRECTIONS, AND AMPLIFICATIONS

BY GERTRUDE R. SCHMEIDLER

Hansen graciously says kind things about me, and I thank him for them. He then accuses me of a series of errors.

It is odd to reread his well-written paragraphs. Two years ago, after he mailed me a copy of the same accusations, I wrote him a long letter to set the record straight. He seems to have disregarded the letter; his wording now is almost identical with what it was two years ago. Let me try again to set the record straight, this time beginning with that worst, last item on his list.

He writes—he *still* writes!—that I “gave no citation” in a 1984 review article about PK. This would be very bad if it were true, but it seems true only because it is taken out of context. Here’s the context.

Krippner’s series, *Advances in Parapsychological Research*, includes several reviews of PK research that I wrote. My later reviews refer to earlier ones, then describe recent work and discuss some theoretical issues; they naturally do not repeat the earlier reference lists. Hansen quotes, from the 1984 review, part of a paragraph under the subhead “Background.” The background for his excerpt came (as I wrote him two years ago) from the 1982 review, where the details and reference are on p. 119. There may be other sins that I unwittingly committed, but I am innocent of that one.

Let us take the Serios pictures next because, if they are valid, they have such important theoretical implications. Hansen and I differ in our opinions on them; I will preface a discussion of Hansen’s specific point with some reasons for my opinions.

The pictures seem to show that Serios could affect photographs by PK, sometimes also using ESP to identify a target. Eisenbud describes precautions against Serios’s trickery. In some cases those precautions would not have sufficed if Serios were a skilled magician. Hansen therefore rejects the whole body of Serios material.

This seems to me to go too far. Consider, for one thing, that even when he was sober, Serios was not a skilled magician. For him, the precautions were probably often (perhaps always) adequate; and if they were adequate even once, Eisenbud has found James’s white

crow. Add to this probability something else I find impressive; that as Serios drank more and more beer in the course of an evening and therefore must have become less competent physically, the pictures of his psychic photography became better. Add also something else that impressed me: TV clips that Eisenbud showed at a PA convention. A TV crew whom Eisenbud (a psychiatrist experienced in diagnosis) described as skeptical and somewhat hostile pointed their camera at Serios's face. At first the face was clear, but successive frames showed a faint mist in front of it, then more mist and still more, until the face was obscured. That did not look like a Serios trick, though perhaps Hansen might suggest that the studio tampered with the record. (Eisenbud shows other pictures from the session [in the *Journal of the American Society for Psychical Research*, 1970, **64**, 261–276]. Apparently he considered the episode so run-of-the-mill among Serios phenomena as not to be worth journal publication.)

Now to the specifics: When I chaired the Program Committee for the 1990 PA convention, there was a poster (not a paper, as I pointed out to Hansen two years ago) about Serios that I approved for convention presentation. It proposed a unique analysis of the Serios photographs. Its argument was that sometimes, when a picture approximated a target but was inaccurate, the picture would show veridical details of the target's background. This had not previously been studied. Selected examples supported the argument. Hansen finds it surprising that I voted for the presentation. The new argument and new evidence seemed (and still seem) to me to be worth a hearing.

Hansen mentions disapprovingly a paper accepted for the same convention. The ESP subject was Olof Jonsson, whose earlier behavior Hansen thought suspicious. Hansen does not mention that in this study, the targets were in America. Jonsson was in Europe; he made his responses by intercontinental telephone. Trickery here would require connivance by someone conducting the research; and Hansen does not suggest that any investigator's behavior was (to use his own word) "suspicious."

Hansen also disapproves my having voted to accept for PA presentation a paper by Cox. Here we have a legitimate difference of opinion about a general issue: the purpose of the annual PA convention. Some think of it as a showpiece for parapsychology, where only the best of current work should be presented. Others, including me, think of it as an annual opportunity for those of us seriously interested in parapsychology to present our ideas and get feedback

on them; to hear and benefit from our peers' criticisms. This means that even though, of course, I reject research that is clearly defective, my standards for acceptance are lower than the standards of those who want the convention to be a showpiece.

For the mind-boggling incident with Delmore, I still do not see how that particular occasion could have involved a card trick. When I described the details to Hansen and asked how a trickster might have done it, he did not respond, and his silence left me wondering whether he knows or has an answer and chooses not to tell me, or whether he does not have an answer. But though this is of personal interest to me, it does not matter. It does not bear on the important question of the validity of the formal Delmore research.

*Department of Psychology
City College of the City University of New York
New York, NY 10031*

COMMENT ON THE HANSEN CRITIQUE

BY IAN STEVENSON

Hansen makes some good points, and I do not believe anyone will fault him for advising parapsychologists to consult more with magicians. I think Hansen's readers would listen to his central message more appreciatively if he were less given to confusing allegations of cheating with proof of cheating. For example, he applies the word *trickster* to anyone, such as Ted Serios, who has been *suspected* of using tricks. I think we should use the word *trickster* only for persons actually *caught* in trickery (or who have confessed to it).

Hansen would disallow investigators to study (and journal editors to publish reports about) any subject identified by him as a trickster. Surely such a policy would diminish opportunities for learning. If further experiments with persons alleged to have cheated prove that they do cheat, the investigators will gain by learning more about magic; and if, on the other hand, improved experiments with such subjects show that they could not have obtained their results by cheating, the investigators will obtain better evidence of the paranormal.

Division of Personality Studies
Box 152—Health Sciences Center
University of Virginia
Charlottesville, VA 22908