"A Response to George Hansen’s Critique: Some Supplementary Notes on the Research with B.D." by H. Kanthamani
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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.

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 creating 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 |is less than~.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 present 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 runs(1) 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 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 \(|p > .06\)) 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. x 10 ft. 9 in. x 9 ft. 3 in. Entrance to the room is through a wooden door of normal size (3 ft. x 6 ft. 6 in.). It has a metal doorknob slightly less than 2 in. in diameter. There are two large windows side-by-side (each 69 in. x 36 1/2 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 closet door is wooden, approximately 6 ft. 6 in. x 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 7 1/2 in. x 3 1/4 in.

The west side of the north wall is dominated by a small sealed fireplace with a wooden mantel over it (54 in. x 7 1/4 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 24 3/4 in. in height and protrudes about 6 1/2 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. x 3 1/4 in.). On the left-hand side, there were three drawers, the top two being identical (13 1/2 in. x 5 1/4 in.) and the bottom one slightly larger (13 1/4 in. x 6 1/4 in.). On the right
side, the top drawer was the same as the left side, but the second was much larger (13 1/4 in. x 12 1/4 in.). The writing arm on either side of the desk, when pulled out, measured 18 1/2 in. x 13 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. x 21 in. x 21 in.), and B.D. used the straight-backed one (35 in. x 20 in. x 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. x 29 in. x 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.

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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 enclosed a 2 1/2-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.

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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:

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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.

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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, Aker 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 Aker 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 |is less than~ .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 Aker (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). We did not use cards; we used slides.

b. Hansen says: "The recording of targets and calls was not done on a blind basis". 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 surreptitiously slide a corner of a card out from underneath the box and steal a glance at it" (my italics). 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.
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.

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