COMPUTER METHODOLOGY: TOTAL CONTROL WITH A HUMAN FACE

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At the first general meeting of the Society for Psychical Research on July 17th, 1882, Professor Henry Sidgwick delivered an address in which he discussed the necessity to accumulate evidence to combat scientific resistance to psychical research. He remarked, "We have done all that we can when the critic has nothing left to allege except that the investigator is in the trick" (Sidgwick, 1882).

This reference to a talk given almost 100 years ago may seem an odd way to begin a paper concerned with modern high technology computers, but it has direct relevance to my involvement with them. Since what I propose to do in this paper is to share with you the ways in which I personally have used computer methodology to run better experiments, I thought it might be illustrative if I started from the very beginning.

I came across Professor Sidgwick's remarks at a formative period in my parapsychology career, that period which many of us pass through when we feel that we have sorted out parapsychology's problems and it is just a matter of getting the experiments done so everyone else will see. Naturally, I was very concerned to do the experiments as flawlessly as possible and in this effort I was fortunate in having the very able guidance of Dr. John Beloff, as well as the benefit of a colleague with a gift for criticizing methodology. It was during my early period of unbounded confidence of success that I actually wondered how my work, if it did show some startling results, would be received. There were, of course, the very sobering examples of the critical reception given the work of Dr. Rhine and his colleagues. It was then that I decided that I should feel flattered if a critic accused me of fraud. It would mean that, following Professor Sidgwick's exhortation I had done my job so well that I had left the diehard critic no alternative.

The connection between this attitude and computers came about when I designed an experiment which required the presentation of hundreds of precisely timed tones and the recording of hundreds of millisecond response times with upwards of 50 subjects. The only sensible

way of carrying out that experiment was to automate it. Fortunately, by this time I had made the acquaintance of an early model laboratory computer and it has been somewhat of a love-hate relationship with these machines ever since.

Over the years of running parapsychological experiments with the assistance of a computer, I have incorporated a number of techniques which have been very useful in my research and I always thought they might be of interest to my colleagues. When I started this work, however, and up until a few years ago, I thought any discussions on these matters would be limited to those few other parapsychologists who had access to the large and very expensive laboratory computers. As we all know, there has in the past few years been another technological leap in this area and the techniques formerly of interest to only a select few researchers should now be of interest to all parapsychologists. Most of the principal parapsychology centers now have micro computers capable of running experiments of the sort I will be discussing and even if a particular researcher does not expect to use one himself certainly his students or colleagues will. The technology is ubiquitous and I am sure that those computer techniques which are found useful to our research will become as familiar as "Basic Technique" or "D.T." were to our predecessors.

Automation

The first and most obvious use for a computer is that of automating parts or all of an experiment. Computers have the useful advantage over us mere mortals that they do not make mistakes. Perhaps it might be better to say that they do exactly what they are told to do and they do it perfectly, time after time. They do not become bored or get tired. They are not subject to biases and they do not hold opinions of their own. In short, they are ideal experimental assistants.

In the early days of computers a number of experiments incorporated the computerized checking of calls and targets. Among the best known of the early experiments are the massive precognition experiment conducted by Dr. Rhine in conjunction with the Canadian Broadcasting Corporation and Dr. Schmeidler's precognition experiment which manipulated knowledge of results. Most of the early experiments were not ideal in that they required, at some stage, that the data be transcribed manually from a human readable form to a mechanically readable form.

The next step was to automate both the generation of the targets and the recording of the guesses. Though not properly a computer, the machine generally associated with this stage is the VERITAC, perhaps

because it is a prominent feature of certain critical books on parapsychology. The warm reception given the VERITAC machine by the critics indicated that they thought highly of studies which automated the recording of targets and guesses. Not surprisingly, when Helmut Schmidt began reporting significant results in the late 60's with a machine just as automated as the VERITAC, critics decided the automated experiment was not of itself sufficient to establish ESP or PK.

A major step forward took place in the early 1970's when laboratory computers became generally available. These were general purpose computers with a variety of connections to the outside world. Through these the computer could control experiments and collect the data in real time. In parapsychology it was the Foundation for Research on the Nature of Man which led the field in automation with the well known, but ill-fated, animal research.

It was at this point that automation acquired what I call its "Human Face." Early automation was directed toward insuring reliability of the data and, indeed, this remains its primary function. With the advent of the laboratory computer, automation could also serve to remove much, if not most, of the work involved in administering the experiment as well. I recall after reporting one of my first automated experiments in Edinburgh, some of my more humanistically inclined colleagues made rather disparaging remarks about the cold, dehumanizing nature of automated experiments. To correct their misconceptions I set two hypothetical scenes. Scene One: An experimenter and an assistant or two busily concerned with recording, synchronizing, changing conditions, only distractedly paying attention to the subject, muttering oaths when recordings were missed or presentations got out of sync. Scene Two: A single experimenter, relaxed and unhurried, enters a few details at a terminal, spends some time getting the subject prepared, then sits back and presses a button to start the experiment. He is free to chat with the subject if appropriate and when it is over, if called for, he has the results at his fingertips to discuss with the subject. No hustle, no bustle, plenty of time for the subject and perfectly reliable data. Which of these, I would ask my colleagues, is the more "humane" experiment and which is the computerized one?

So the advantages of computerizations may seem pretty obvious by now. The point which has not quite hit home yet is that experiments as secure as the famous VERITAC and as humane as you wish to make them are available to all experimenters with any research budget at all. No longer are they the province of a few big laboratories. The hardware is available; the programming languages are easy to master. All that is required is some consciousness-raising. Therefore, I shall be urging you to "think computer"!

To help you along, I would like to present two examples of computerized experiments I have carried out which will illustrate automation in practice. The first of these is the experiment referred to earlier (Broughton, 1977a), which was to investigate possible brain hemisphere differences using reaction time to tones presented to the subject bilaterally via earphones. The condition which was manipulated was whether or not the agent, in another room down the hall, received an advance warning tone by which he might psychically cue the subject. The experiment required the presentation of a tone to the subject which sometimes would be preceded by 250 milliseconds by a tone to the agent (experimental condition) and sometimes followed by a tone to the agent (control condition). The agent responded to his tone using the preferred hand and the response was monitored. The subject responded by blocks of 20 trials using the right or left hand and these were timed to the millisecond. Also, I wanted a record of the number of times that the subject responded before his tone in each of the four conditions (anticipation) as well as the automatic rerunning of any trial in which the response took longer than one second (mistrial). Obviously, without a computer this would have been a fairly unwieldy experiment and one subject to all manner of recording errors and unintentional biases.

I am quite sure that this experiment could now be conducted using an Apple II or TRS 80 sitting on an office desk. At the time I used a room-sized computer called a LINC-8 and had to fit the entire assembly language program and all the data into the machine's total complement of 4K words of memory. The important thing was that this machine had input lines which could sense the press of a button, output lines which could control the administration of a stimulus and a clock which could be checked, all under program control. These are all features which are standard or, at worst, inexpensive options on the present day microcomputers.

With a modest amount of planning and a fair bit of programming I was able to reduce the complex tone experiment to the following situation: I greeted the subject and agent; explained the experiment, showed them the rooms and then prepared each for the experiment. I entered the relevant data into a terminal and then gave the participants a few samples of the task. When everything was ready, I simply pushed a button on a control box and sat back with the relaxed confidence that the administering of the conditions would be correct and the data would be flawless. At the end of the experiment, the data were transferred to punched paper tape which was, in turn, fed into a statistical analysis program.

For me, this experiment was a real eye-opener. Granted there was a good deal of preparation involved, but when it came to the actual run-

ning of the subjects, I was more relaxed and easy-going than I had been for any of my previous experiments. With all the busy-work out of my hands, I was able to concentrate my attention on the subject and agent. By the time that series was over, I was convinced that, for those experiments which lend themselves to automation, it was the only way for my research.

The second experiment followed the first by about a year and I would just like to discuss some of its features because it represents automation taken to one of its extremes. I was by that time involved with the observational theories and I wanted to conduct an experiment in which I manipulated the subject's expectancies after the ostensible psi task was completed, at about the time the subject saw his or her results. It also seemed important then to keep the experimenter blind to the expectancies and scores of the subject as well as the raw data themselves. Unlike the previous experiment, which would have been difficult without a computer, this one would probably never have been conceived of without a computer.

This second experiment (Broughton, 1977b) consisted of having the subject do two 32-trial runs of 4-choice forced guessing at four lights arranged in a slight arc on a response box. They were not told that this was an ESP test, but that it was a study of how artifacts can enter parapsychological experiments and they were given an appropriate cover story which suggested subliminal auditory cues were to be used. The two runs were, in fact, identical, but at the end of the experiment the subject was given a computer report of his or her performance. This report informed the subject that one of the two runs should be high and the other low and it labeled the runs accordingly. Which run would be labeled "high" and which would be labeled "low" was based upon an RNG decision made several minutes after the subject had completed the tasks. The report, unseen by the experimenter, was handed to the subject to be taken away.

I was also interested in whether the subject would direct his or her efforts at the earlier guessing task or only at the numbers printed on the report, so I had an additional condition wherein half of the subjects received the scores, not of their guessing, but of concurrent "pseudo-guessing" which the computer carried out by matching two RNG outputs. Whether subjects got to see their real scores or the pseudo-scores was also determined by a random decision after the subject finished the task. Finally, based on my reading of Schmidt's model, I, as experimenter, wanted to restrict my feedback of the experiment to only the results of several pre-planned statistical tests.

Like the previous experiment the actual running of subjects was very

easy. The atmosphere was relaxed and my role consisted of little more than pressing a few buttons and then giving the subject his report at the conclusion. In this case, the real power of automation hit home when, after running 40 subjects, I sat down at the computer console and prepared myself to receive "instant results" of an experiment which had been months in preparation and weeks in running. All I had to do was to issue the command and all the statistical tests I had planned for these data would come out in seconds.

This experiment illustrates not only automation taken to a degree not often found in an experiment, but it also demonstrated and, in fact, was one of the first experiments to do so, the complete and precise control of feedback in an experiment. This second feature of computerized experiments is one that is absolutely essential in testing the various models which come under the heading of Observational Theories (Millar, 1978). We shall return to this topic later.

Simultaneous Control Condition

Having successfully made the leap to automated experiments which freed me of administration and data collection responsibilities and permitted a more relaxed and unhurried atmosphere in which to deal with my subjects, I then began to use the power of the computer to add more rigorous controls for possible artifacts. To some extent this was an outgrowth of the previous experiment with the "pseudo-guessing" condition.

Because a computer executes its instructions very quickly, in a typical experiment it spends most of its time waiting for something to happen, a response from the subject, a preset interval, etc. In some of my early experiments I made use of this idle time simply to run off RNG test numbers which I later checked by hand to insure that the RNG was up to par during the experiment. In developing my expectancy experiment, however, I realized that one could go further than this. It was entirely possible to run complete matched control conditions virtually simultaneously with the experimental conditions. In other words, I could run a parallel control experiment at the same time the subject was exerting his or her influence in the real experiment.

To give a hypothetical example, suppose we have an experiment in which the subject is trying to affect by PK a visual display which is governed by an RNG. To incorporate a simultaneous control condition we would simply arrange the program so that for every RNG number which governed the display, we would also get one which the subject knows nothing about and serves only as a check on the RNG's moment-

by-moment functioning. These data would be collected, stored and processed in exactly the same manner as the experimental data. It is important to duplicate conditions closely so that we would also be taking precautions against programming problems which could bias the data as badly as any faulty RNG. At the end of the experiment we would have two similar sets of data: one would have been exposed to the subjects' efforts and one would only be a test of the RNG and the controlling program. If we were then lucky enough to have some rather striking findings in the experimental results, we would also have the means by which to disarm any critical challenges to the adequacy of the RNG or the program which ran the experiment. We would have an entire duplicate set of control data collected during the actual experiment which could be subjected to the same tests as our experimental data.

The incorporation of simultaneous control conditions in computerized experiments takes very little added programming effort and for the majority of experiments it would have little or no effect on how the task appears to the subject. I have provided only one example of a SCC but, with few exceptions, the ways in which SCC's could be incorporated into experiments are limited only by the experimenter's imagination. I would particularly urge the many newly computerized parapsychologists to consider incorporating SCC's into their experiments.

Split Analysis Techniques

The third technique which I wish to discuss is not unique to computers, but it is one which computers can handle particularly well. It is not a new technique either, having been a feature of some of our better known experiments, such as the Fisk and West clock card experiment or the Feather and Brier checker experiment. It is simply the blind splitting of experimental data into two or more parts for separate analysis.

In the last few years these techniques have assumed a rather important position in our methodology. As we are all aware, the view of psi which for the past 30 years or so has dominated our interpretation of experimental data, that is, the view which assumes psi is widely distributed among the population, has received a strong challenge from one theoretical camp which reads the data differently. The latter group holds that the psi we see in our experiments comes only from a few "Psi Stars" and in many cases these happen to be experimenters themselves rather than those designated subjects. One of the weapons in what I view as a healthy and creative competition between these interpretations is the split analysis technique.

The rationale behind this technique is straightforward. The unseen

data from one homogeneous pool of subjects in a given experiment are split into two parts and analyzed separately, for example, by different individuals at the same time or perhaps by the same individual at different times. If there is a psi effect and it is coming from the subjects, then both parts of the data should exhibit the effect. If, on the other hand, there is a psi effect in one part of the data and not in the other or if there are different effects in the different parts, then it would be very difficult to attribute the psi to the subject pool. In that case, parsimony would suggest that the analyzer was the principal culprit. The split analysis technique itself is neutral in the theoretical controversy. It can serve the Psi Star advocate who wishes to demonstrate that the efforts of the unselected subjects have little effect on the pattern of results as well as the democratic psi proponent who boldly uses a split analysis technique to confirm that effects do indeed come from unselected subjects.

When Brian Millar and I first began to develop computerized split analysis techniques at the University of Edinburgh, it was largely a desire to confirm effects that we hoped to find in our subjects which guided our work. The experiment which prompted the first computerized use of a split analysis was a complex attempt to test covertly the psi of 16 sub-experimenters who were each going to test 16 subjects with identical PK tasks (Broughton, Millar, Beloff and Wilson, 1978). This was done using pre-recorded targets and all the data were held in the computer at Edinburgh.

The problem Millar and I faced was that we had no idea of what to expect from this experiment. We were unable to plan in advance any particularly interesting tests and we did want to keep our options open to examine anything that looked promising. We also knew that we did not want to have to repeat this very elaborate experiment to confirm some unusual effect. The solution which we hit upon was to let the computer split the data into two parts. Since we would be totally blind to the results, we could designate one part of the data as the pilot study and analyze these data for anything that seemed reasonable. When that was finished, we could then make certain predictions and subject the second part of the data to a rigorous confirmatory analysis.

This technique has since become known as the "Edinburgh Split" and a more detailed discussion of it can be found in *Research in Parapsychology 1979* (Broughton and Millar, 1980). As I have mentioned, it is not necessary that a computer be used for a pilot/confirmation split of a single experiment, but a computer makes it very easy and completely foolproof to carry out. If one is already using a computer to run experiments, the Edinburgh Split can be incorporated with very little effort at all.

The various recent papers discussing psi based experimenter effect

and the current interpretations of the Observational Theories have made many parapsychologists unsure of where the psi in their experiments is coming from. The Edinburgh Split is one way of trying to confirm that the subjects play the greater role in producing the psi, but other researchers have been using split analysis techniques in an attempt to trap experimenter or analyzer psi effects.

Leading this effort are the Amsterdam laboratories of Dick Bierman and Joop Houtkooper who conduct their research squarely within the framework of the observational models of psi. In a number of experiments (Bierman, 1977; Weiner and Bierman, 1978), these investigators have conducted conventional computerized experiments with subjects, but then had the computer blindly split the data into parts which would be analyzed by different persons. In other experiments (e.g., Bierman and Houtkooper, 1981) these investigators have attempted to manipulate even the future *potential* observers of psi effects by having the computer make blind decisions to destroy whole sections of the data.

It is not possible to go into all the details of this research. The main point to realize is that it represents a type of experiment which makes full use of the computer's ability to administer very complex experiments, collect the data and blindly distribute it for selective analysis according to the requirements of the design. Because attempts to work within the framework of the observational models require precise regulation of the feedback to the many participants in an experiment, it is quite likely that this entire line of research would not be possible without the aid of the computer.

Like simultaneous control conditions, split analysis techniques can take a variety of forms, limited only by the requirements of the hypotheses under examination. In the case of analyzer and observer splits these techniques represent practically the only effective way of coming to grips with some of the vague predictions of the observational models.

I have discussed three means by which the use of computers can help the experimental parapsychologist to conduct good experiments. There are, of course, many other ways, but these are the three methodological improvements which I have found most useful in my quest for the experiment which will make the critics yell "Fraud!" I have conducted only one experiment so far which embodied all three techniques and fortunately for my reputation it produced no results which would provoke critics to reach for their slanderous guns.

The experiment to which I am referring is called, "An Experiment with the Head of Jut" (Broughton, 1979) and I shall briefly review its features to illustrate the use of all the techniques which I have been discussing. It was basically a game-like PK test called in Dutch "Kop van

Jut," because that is the name for the fairground test of strength which my PK game was supposed to mimic. It consisted of a column of lamps which would light from the bottom upwards in a motion similar to that described by the weight on a wire in the real "Kop van Jut." Every time the button was hit a series of 50 binary trials was initiated, each hit adding another lit lamp to the column. Over 32 hits caused a bell at the top to ring.

I wanted to look at several things in this experiment beyond the basic above chance scoring which I had hoped this game would elicit. First, I wanted to see if there would be a difference between targets generated in real time and pre-recorded targets generated before the button was pressed. Secondly, I wanted to see if there was a difference within the runs between the early targets, where the lamps came on rapidly and were not really a focus of attention, and the later targets where the lamps slowed to a stop and were usually the most engaging. Thirdly, I wanted to see if there was a difference between playing this game alone and playing it with friends. Fourthly, I wanted to see if there was any relationship between a subject's self-perception of luckiness and performance in the experiment. On top of all this I wanted to look at most of these conditions not only in terms of scoring, but also in terms of variance differences.

The experiment itself was fully automated, apart from the luckiness questionnaire. After greeting the subjects, having them complete the questionnaire and introducing the experiment, all I had to do was to enter the subject's name into the computer. The rest proceeded automatically including allocation of real time and pre-recorded conditions.

Naturally, I had also incorporated a simultaneous control condition to circumvent any suggestions of temporary RNG bias. This simply consisted of getting two RNG targets for each trial. The first governed the display to the subject while the second was stored as a control. The matched control data were also available to serve as a control when checking correlations between perceived luckiness and actual scoring.

With the variety of ways in which I wished to examine the data, it was imperative that the Edinburgh Split be used to provide pilot and confirmatory batches of data. In this instance I split the data from each subject alternating in an ABBA or BAAB fashion and for each subject had six runs in each of the two conditions.

Unfortunately, for all my efforts I was able to come up with nothing of real substance in the pilot data. There were some significances in certain subgroups which looked hopeful, but they could have been the result of over-analysis. One subject did, however, produce some very odd variance effects which caught my eye. As is generally known, none

of these effects stood up to the confirmatory analysis with the second part of the data.

To be sure, It was disappointing to have to conclude that there was no evidence of PK in that experiment. A great deal of work had gone into the project and I had high hopes that the game would elicit PK from my subjects. Nonetheless, I was pleased with the way the three techniques had done their respective jobs. The experiment was easy to run and I was free to help subjects enjoy participating, as most of them did. Data were collected and analyzed flawlessly and the matched control part was useful in providing a comparison with the experimental data. The Edinburgh Split prevented me from further cluttering up the literature with some rather awkward findings and a most curious "possible special subject."

Needless to say, that experiment did not have a big impact on my parapsychological colleagues. It has not, however, gone unnoticed among our critics and is probably the only parapsychological experiment to be very favorably discussed in the *Skeptical Inquirer*. I must admit that I felt a certain sense of accomplishment when I read in that journal, "I find Broughton's determination not to be fooled by his own experiments entirely admirable. It should insure him of sympathetic attention from the skeptics if he ever comes up with positive findings" (Hobens, 1979–80).

As I embark on new research programs at the Institute for Parapsychology, I earnestly hope that I shall be able to give the critics something to take seriously. I also hope that some of my suggestions will help my colleagues do experiments which attract the same critical interest.

Before I conclude this paper there are a few more general topics which should be mentioned.

First of all, we cannot let mere computerization substitute for careful planning of experiments. A badly conceived experiment will not be improved by automation. Faultlessly collected data will be of little use if they turn out to be inappropriate for testing the original hypothesis. Automation relieves the experimenter of none of the responsibilities for the proper design of the experiment. What it gives him is the assurance that the design will be followed perfectly each time.

Secondly, the ease of using the programming languages associated with the popular microcomputers must not be allowed to lull the experimenter into a false sense of security that the program is really doing what he thinks it is. The experimenter must understand precisely what each and every part of his program does, just as he would have to understand a psychometric instrument or any piece of apparatus that a non-automated experiment might have required. Also, in this context

I cannot emphasize enough the need to test experimental programs exhaustively before embarking on serious data collection. In my work I would typically run two or three dummy experiments just to insure that everything was working properly.

The third general topic concerns the question of fraud. Assuming the subjects do not have the opportunity to get at the computer in ways which are not intended, one can make experiments virtually fraud-proof as far as the subjects are concerned. Subjects would have to be able to alter the relevant programs in undetectable ways or be able to get at the stored data (which could well be encoded) and such operations would require a considerable amount of access to the computer in question. By the same token, fraud by the experimenter with unlimited access to the computer can be made almost indetectable, even to other computer specialists. Computers will not, therefore, relieve us of the need to develop interlaboratory reliability, but they will help us in narrowing our search for the methodologies by which to achieve this.

I hope that I have not left you with the impression that the computer is "the answer" for all of parapsychology. It certainly is not. The computer and the techniques which I have discussed are only tools to be used by us in trying to understand psi. I have found them to be particularly useful and I hope some of my colleagues will find them so too.

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DISCUSSION

RUDOLPH: For the last forty years, computer people have been trying to make computers as reliable as they could by making them as deterministic as possible and yet here, in this field, we are suddenly beginning to insert random elements into computers and make them nondeterministic. The information theorist Colin Cherry back in the 50's said that if we want a computer to act like a biological organism, it will have to be nondeterministic. Looking at the complexity versus efficiency problem for computers, it's pretty clear to me that within forty or fifty years nondeterministic computers will have replaced deterministic computers in many applications. If you look at the evolution of biological organisms, you will find that they did not evolve into deterministic creatures and it's pretty obvious why. As far as I know, the development of nondeterministic computers is happening nowhere else.

I think it's exciting also to think of a computer as a psi detector; it's got some terrific potentials. Around the turn of the century, it would have been great to have been able to go into the seance room with adequate controls to do really good work. It couldn't be done then because the controls were so intrusive. Now it's quite possible to do that non-intrusively with modern computer technology and, since I think the problems in this field are primarily psychological, not technological, I think the existence of a nonintrusive probe is a very exciting possibility.

BROUGHTON: Naturally, I can only agree with you whole heartedly. I am vaguely familiar with the nondeterministic ideas and they are exceedingly exciting. One's speculations tend to run wild in terms of the experiments one can design with a nondeterministic computer because it might be the closest electronic analog to a human brain that we could come up with. But, at the moment, it's a bit in the future, but I think a very exciting future.

EDGE: Our computer expert at Rollins, in writing (in our alumni magazine), about the use of the Apple computers on campus called mine the most exotic simply because I had a random number generator. This anecdote goes to show that it's so unusual to have this sort of apparatus nowadays that somehow it's viewed as exotic. The question I wanted to ask, however, was about the simultaneous control condition. This would seem to be a problem in some circumstances. For instance, from what we know, it looks as if there has been some success in what's called the release of effort condition, and it seems as if what you're talking about or what you could be talking about is a perfect situation for a release of effort; that is, what you have is a PK effort and then right after that you have—when there would be a release of effort—the control going

on. I'm wondering whether or not you have some comments about that problem and, if that's the case, wouldn't you expect that the simultaneous control would not, in fact, be a control; that is, it would not come out to be random or if it did come out to be random, wouldn't the implication be that psi is indeed goal directed? That is, if we have data saying that the release of effort condition is a psi conducive condition, or one in which psi has happened, if it doesn't occur, then that would give some implication that one can discriminate that and it becomes psi conducive, I mean, goal directed.

BROUGHTON: The first thing I should make clear is that the simultaneous control condition is really a check on the computer and the randomness. It is not primarily a control condition in the sense that we would use it in psychology. That is, as the condition against which we are going to be comparing the experimental condition. For example, with the release of effort, there is no reason that the control condition, the SCC, has to sample immediately after the effort; it could pick it up before the effort. In other words, it is just to show that at about the time the subject's target is being pulled, another one pulled at virtually the same time did not show biasing. So it could be pulled before, after, or it could alternate, depending on how your program is set up. There are many ways of building it into an experiment so that it would not be subject to something like the release of effort effect. At the same time, one could program in, as I did in a number of experiments, release of effort tests, in which the RNG kept running when the subject finished, to see if we could catch something. In fact, we never did, but it was very easy to do in that situation. The main point is that in using the SCC, essentially what we are going to do is declare it to be a check of the random event generator. We are not going to allow ourselves to say if the SCC does become biased, "Oh, this is some kind of psi effect on our control," even though the experimental data were dead flat chance. We are declaring this to be our check condition, our control condition, and we simply will not accept effects in it. If we do find effects in the SCC we would have to interpret it the way we agreed; that is, there is something wrong with the program or something wrong with the RNG at the time. It is an experimental decision. Some people might not want to make it, but I prefer to do it that way. It gives me ammunition against the situation some people using RNG's have found themselves in: facing charges that there is a bias in the machine when one hooks up all the gadgetry to it, but not when one checks it by itself. I have a set of data which I am declaring to be my test of the RNG and if it turns out to be anything other than that, I'll throw away the experiment.

HONORTON: I have two comments. One is to elaborate on the point

that you made at the end, which is a very important one: that the computer is not a substitute for good experimental design. On the other hand, in a good computer psi experiment, the computer program is essentially the experimental design. And this is an aspect of the use of computers that I find particularly exciting because it is now possible to specify in much greater detail than ever before the details of the experimental design, particularly in game-type experiments, where most of the psychological or motivational aspects of the experiment are built into the program. But the main thing I wanted to raise has to do not with computers, but with an issue that you spent a fair amount of time discussing, and one that I think is very appropriate to a conference on methodology and psi, and that has to do with this question of experimenter's psi effects. Now, I realize that I have to accept some responsibility for introducing this problem in my presidential address at the Parapsychological Association convention some years ago, but I think we've gone too far with it, I think it has had a paralyzing effect on research. It is a question that naturally arises because we have no defined physical parameters for psi at this stage, therefore we cannot conclusively attribute the source of psi to any particular individual. But someone like your former colleague at Utrecht, Brian Millar, in flattering myself and several others that we are the only people on the planet who have this mysterious psychic ability and are contemporary mediums, is not talking about experimenter psi. He's talking about investigator psi. Because in most of the experiments that I've been involved with, for example, that have given significant results, I have not been the experimenter. I have been involved in the design and analysis of the experiment. Now, this raises—for me, at least—a question that if I can remotely influence subjects who are being run by other experimenters in my laboratory, and since we have no space-time constraints on psi, as far as we know, then why can't I also be influencing Brian Millar's subjects? That doesn't seem to work very well. Well, if I were, I would be doing it in my own self interest, which would not be producing the kind of results that Brian is recording. The point of all this is that this is a fruitless topic for research, in my opinion. If we look at what has been presented as experimental support for psi experimenter effects, with the same degree of hard-nosed rigor as we've looked at some other areas of research, it doesn't stand up very well; there isn't very much clear-cut experimental support for it and even with the Edinburgh Split, or any other sophisticated technique you can come up with, it's such a ubiquitous hypothesis there is really no way to falsify it. I suggest that this is not a fruitful area of research, that we should not be spending our valuable time trying to find out how to attribute the source of psi. It may very well be, as

Gardner Murphy, among others, said many years ago, that we're dealing with a function that is not fundamentally a property of individuals. Whether it is or not, I don't think we can do anything with that question and I would suggest that unless we come up with some physical boundary condition that would allow the experimenter to get on a plane or get on the space shuttle or even die, and not be a potential source of influence, that we consider this to be a nonproductive question.

BROUGHTON: Naturally, in this case, I would have to respectfully disagree. I do not think it is either nonproductive or nonresearchable. I will fully admit that it is exceptionally difficult to research it because, as I have said in one of my own papers, we have defined psi, and interpreted everything that we know about psi, to indicate that it has no boundaries. Perhaps this is a mistake. Perhaps it is not really that all-pervasive. I think techniques like analyzer splits in particular, even if in their present form they cannot conclusively isolate a source of psi, can go some way toward helping us look in different directions for sources of psi. It could be, as Gardner Murphy has said, that psi is some field effect, a product of groups of individuals, of situations, but it could also be the product of just individuals. Rather than declaring it unproductive or hopeless I think there is room for looking into this area and, particularly with the use of computers, looking into it productively for those who wish to pursue the question. I am not saying this is a need for all parapsychologists, but it certainly does fuel a number of fairly interesting theoretical lines being developed, particularly with the Amsterdam group.

ROSEN: My comment fits in with what Chuck Honorton was saying and with Richard Broughton's response to it. In support of Chuck, there is no denying that consideration of experimenter effects leads us into an infinite regress where additional psi sources can always be postulated beyond any we may attempt to isolate. But what shall we do about this? To ignore the problem because we can't address it fruitfully within our current experimental paradigm, will not make the problem go away. Perhaps a fundamental change in approach is called for, one in which the field characteristics of psi research are neither resisted (by continuing the vain attempt to isolate the psi source) nor ignored, but accepted.

BROUGHTON: I do not think I have too much to add. I might just mention that in this area, for example, while experimenter effect is thought by some to be insoluble, Joop Houtkooper has been looking at ways in which subsequent observers may have differing influences—his absorbance model. At this point, even though it is a very young problem, it is producing some interesting hypotheses, so we have to grant it at least that.

OSIS: I am an old-timer who came into the field of parapsychology at the time when electrical calculators first appeared. They were not electronic then, but mechanical with wheels running and groaning. A calculator then was the thing and we hoped that we would be much, much better off with calculators and they would increase the research output. Actually they didn't. What about the computers—do they actually make a difference in research output? I am all for the computers and all for what was discussed here, but let me give you one suggestion: Put your heart into the content of the research project not in technicalities or tools, but in what you really want to find out about psi processes, and the experiments will work. But I see one thing which computer-aided research can do that was not possible for old-timers. J. B. Rhine and Gardner Murphy always emphasized elusiveness and a sudden flare of psi; just for a few seconds it's there—and then it isn't. Gardner Murphy once compared the spontaneity of psi with Hamlet's ghost—it appears when it wants to and then it vanishes and you can't get him back again at will. So far I know only Grey Walter had suggested capturing psi in short snatches of a few seconds duration using physiological indicators, but he never made this method stick in parapsychology. I think with the new computerized methods it might be possible to segregate out these short duration flares of psi as astronomers do research on sun spots and sun flares. We could now research these spontaneous "psi flares," catch them, use them and make our predictions and theories about them and stick to them. Maybe the most remarkable thing in parapsychology is that we stubbornly refuse to accept the nature of psi processes which are elusive, of a short duration; interspersed in the flow of other things in our mentation as Rhine and Murphy suggested. We'll still insist that the long experiment as a whole has to show the psi effect or it's not valid. And we insist on so many repetitions that it becomes even more ridiculous. Are there new possibilities where you could use computer tools to bring these short sun flares of psi up to better scientific grasp?

BROUGHTON: Well, we all have particular prejudices. We like our methodology; we think it could offer this or that. I, for example, like particularly some of the work that Chuck Honorton is doing with game situations. We are making it very much like testing in one's living room with video games and things of that sort. I think these techniques might help, but we all know that we have traveled this road before. If I might just give a little instance, when I practiced this paper a few days ago at the Institute for Parapsychology, Dr. Louisa Rhine was there. She listened very thoughtfully and when it was over she said, "Well, it sort of makes me think that we were the pioneers who struggled across the country, slogging away in a horse and carriage, and now you fellows fly

over the same country in a jet plane." I thought that was a very apt analogy for what we are doing now. We have the same controls. We are just doing it easier and faster. We face the same problems and we are still fighting the same battle.

MCCARTHY: I have two comments, but I'll try to keep them both pretty brief. The first is concerned with the remark that Chuck Honorton made and I guess my remark is kind of tongue-in-cheek. Suppose Brian Millar is correct and that some people—such as Chuck Honorton—are sufficient psi stars that just by being investigators rather than experimenters, and designing the right kinds of experiments they can produce favorable results. Well, put this together with Chuck's first remark, that in a computer-controlled experiment the program itself *is* the design of the experiment to an unprecedented degree. If Brian was right, then I guess we should see some pretty good results from the kinds of computer-controlled experiments that Chuck is likely to produce that are run by other people. My second comment is I'm glad that you raised some points regarding the potential pitfalls of doing computerized research. The points were made very clearly in the paper and because of time, you didn't get into great details when you presented it orally. The point I'm referring to explicitly is the need for carefully checking that your computer program really does exactly what you think it does. You referred at one point in your paper to a possibility that you felt would be realized in the future—that these computer techniques would become as familiar to our successors as the DT technique and other card guessing methodologies were in the era of card guessing psi tasks. Well, Chuck has pointed out several times that in developing computer-controlled experiments we have to be very careful to avoid some of the pitfalls that were encountered in that earlier era of psi research; for example, we don't want to produce the analog of decks of ESP cards, where you can see the symbols through the backs of the cards.

BROUGHTON: Yes. I entirely agree with you. One has to know exactly what the computer is doing. Typically, I run two or three dummy experiments for test purposes. Ironically, I realized that while the computer saves me the trouble of analyzing data in the actual experiment, because I typically ran two or three dummy experiments in which I had to analyze the data by hand to check the statistical programs, in the end I was doing a lot more hand calculation than I would have done if I had run the experiment manually. At least I have the assurance that when I do run the experiment there will be no glitches and that is comforting.

RAO: I want to respond to Chuck Honorton's comment on the problem of experimenter effects. I do not think it is a pseudo problem. It is no more unfalsifiable than several of our sacred assumptions in psi research. I consider the problem of experimenter psi effect important

because it seems to question an assumption we have so far regarded as almost self-evident.

Most of psi research wittingly or unwittingly makes the assumption that in a psi experiment the source of the effect is the subject. That is the reason why we study the subject's attitudes and his/her personality. If we follow, for example, Rex Stanford's suggestions and experiment with word association kind of tests, we are implicitly making the assumption that it is the associations of the subject and not those of the experimenter that are important and relevant. Inasmuch as the hypothesis of experimenter effect suggests that, in some cases, the source of a psi effect is not the subject, but the experimenter, it questions the classical assumption of the subject being the source. This apparent contradiction needs to be resolved to make sense of the relative roles of the different variables that we study in parapsychology.

SCHECHTER: I'd like to return to the simultaneous control condition for a moment. We have several current theories, such as the observational theories and Dr. Stanford's conformance theory, which emphasize the role of random processes as psi detectors. Dr. Rudolph's comment about the electronic processes in a computer acting as a psi detector fits here as well. The more I think about it, the less comfortable I am with defining the simultaneous control condition as a true check of the normal operation of the RNG. I'd like to hear your thoughts.

BROUGHTON: Primarily, the SCC has a specific function and that is, as I have said, to disarm an attack on a particular experiment in which we are not looking for a generalized effect on the RNG. It is also for a specific audience, too, and that is someone who will criticize the experiment, which could be very elaborate, process-oriented research, on the basis that the random generator was faulty during the time subjects were there, or that the program was in some way incorrect and produced spurious results. What you would be saying is, "No, I have a complete set of data here which is, hopefully, dead flat chance which I conducted under identical conditions at exactly the same time." It is toward that sort of audience that the SCC is directed. We are really declaring that this is an area in which we are not looking for psi. If we find it, then we have got to start thinking about another experiment—an experiment to test that aspect. For the purposes here, we are using it to deflect the criticisms of a small audience that could be very disruptive to some very good process-oriented research. It need not be very elaborate. With a simple little incorporation of an SCC you have the means by which to deflect those criticisms. That is the main purpose I really would like it to serve.