RECRUITING FOR RESEARCH

GERTRUDE SCHMEIDLER

In parapsychology, research is the name of the game. This was explicit when the area was called "psychical research" by those who first set it apart as a field of study; and it is implicit even now, after J. B. Rhine has renamed it. The "ology" ending of our modern term "parapsychology" stands for science, and science is defined by its methods of investigation.

But aside from semantics, I will argue that we need to center our educational as well as our other activities around research—or at least to aim our activities toward furthering it—partly because of moral reasons and partly because of the simple, eminently practical reason that we do not yet know enough to be effective in doing more. You may differ with me on this point, and if you do I hope you'll try to convince me otherwise in the discussion after this paper. But now, while I have the floor, let me state and defend the position.

The strength of my own conviction on this point was brought home to me just last May, when someone from a well known publishing house asked me to write a book for them on "How to Develop Your ESP." My immediate response was tactless, I suppose. I said, "No! It wouldn't be ethical." But thinking about it afterwards, it seemed to me that the conviction was sound, even though the phrasing was on the crude side. The arguments go along these lines.

In the first place, we have no infallible technique for ESP (or PK) development. Training techniques that have worked well for one investigator have not, so far, shown consistent success when they are tried by others. We have no cookbook formula that can be guaranteed to improve anybody's ESP performance if its directions are followed. A "How to—" book would promise more than it could fulfill; the title would be a kind of false advertising.

In the second place, I put to you a different hazard in overselling, familiar but still worth stating. Probably all of you have often heard someone talk about what he was sure were ESP experiences although

you, listening critically, felt uncertain that they were. The various incidents were perhaps attributable to normal inference, or else to disregarding negative instances and therefore overweighting the positive ones, or even to the person's assuming that hallucinations or delusions had a basis in reality. Any onesided emphasis in "How to Develop—" or even in educating about the fact that ESP and PK can occur might unhappily encourage overdependence upon wishful thinking, or even neurosis or psychosis. Perhaps an ethical and needed book would be one called something like, "How to Recognize When You Are NOT Using ESP."

And in the third place, more remote but troublesome, are ethical issues of invasion of privacy, or of undue power. At the present stage of our knowledge, if ESP were a readily trained ability accessible to anyone no matter what his motives were, it might be misused to do harm instead of good. And the same principle applies with even greater force to PK. Anyone concerned with application or with general education should, I think, give as much attention to the question of how to shield from the undesirable use of telepathy or PK by others as to the question of how to facilitate the use of either.

This leads back to my initial point, the need for further research. The basic problems of parapsychology were set before us generations ago by the early members of the London Society for Psychical Research. The work of the nearly hundred years since the SPR was founded has told us that the problems are worth study and can yield answers; but we have only the roughest approximation of what even our firmest answers are. By the scientific standards which we are surely all glad to accept, we should not be content until we can make accurate, quantitative predictions about what will occur under given sets of conditions. But I think there are none of us (and feel sure there are few of us) who can state confidently and correctly, in numerical terms, what the results of any investigation will show.

We have the problems, and have had them for a long time. How do we work toward the solutions? From a broad perspective there are many possible routes toward contributing usefully. The most direct, of course, is to do the research: to try to find the answers ourselves. Another is to encourage members of the scientific community to be alerted to and interested in these problems, in the hope that others will turn to them and do the work. Another is to build the broad base of academic support which will let scientists who are already interested feel that they can work on parapsychological problems without jeopardizing their own careers—or even feel that it will further their careers to work on such problems. Another is to build an even broader base in the business and governmental organizations that help to fund

the scientific community. And another is to work toward the broadest base of all: general public acceptance of the legitimacy and importance of the issues.

We might think of all this as a pyramid, where the broad public acceptance provides the foundation and the other layers rest progressively upon it. All are useful, but my own paper will be addressed to only two levels of this total structure: the parts near the apex, closest toward providing the research cadre we so need, where I have had a fair amount of experience. It will deal with the encouragement of research in the academic or scientific community, and especially with the possibility of encouraging parapsychological research among advanced undergraduates or graduate students. Its topic is the attempt to recruit and train scientists to work in parapsychology.

You may have noticed that I told you about having had a fair amount of experience with this. The sad truth is that my experience is rather substantial but its success rate has been low. Let me start with an analysis which seems to show that in one subset of my efforts the success rate has been zero or near zero, and that whatever successes there were cluster in another subset.

The formula for training scientists to work in parapsychology surely must begin like the recipe for rabbit stew: first catch your scientist. The obvious way to do this is to go where the scientists are: to research organizations and colleges and universities. Then, in the ideal if not the real world, you alert the scientists to the research issues; you show the relevance of these issues to the problems that already interest them; and you thus direct them to integrate those issues into their own ongoing projects.

This is what I've tried again and again, but—at least when I try it—it doesn't work. Scientific groups to which I lecture seem attentive; they ask good, research-oriented questions; they show interest by staying after the session is over to keep on with the discussion. They give every evidence of being stimulated and concerned—short of the only evidence which is important: doing something constructive about it on their own, after the lecture is over. And this has been true both for specialized groups actively involved in ongoing research, such as engineers and Ph.D.s at NASA or at Bell Laboratories, and for doctoral students at places like Harvard and Columbia, and also for adult groups with a sizable proportion of trained professionals, like Mensa.

Why so? My guess is that by the time research people have grown set in their profession, most of them find security in their own field of expertise. They know how long it takes to become an expert; and from

the point of view of safeguarding and advancing their own careers, they know they have to keep on running to stay in the same place. If they shift to a different area, they recognize that they will have to devote a very large number of hours to familiarizing themselves with its prior findings and with its new techniques. But if they spend so much time on the new area, there will not be hours enough to keep on their old one; and this means a hiatus in publications and research output—a hiatus that the effective worker has learned to avoid. (I omit here a discussion of any fear that becoming a specialist in parapsychology means working themselves into a corner where job opportunities and research grants are scarce, because this applies across the board to young people who have not established themselves, as well as to the older ones. It is the special difficulty of recruiting experienced scientists that I am discussing now.)

The preceding argument may be only rationalization: an attempt to justify to myself my repeated failures at recruitment with the trained scientists who would make the best recruits of all. But if it were only a personal failure, I would expect to hear of others who are better lecturers having a great deal more success—and there have not been many reports along these lines. Two exceptions need to be noted, but neither (I think) affects the general argument. The less interesting one is that if research money is available, members of the lecture audience who need money will apply for the position, just as they might answer a classified advertisement. But this seems like recruiting a technician, not an independent scientist. The other exception, a pleasant but rare long shot, comes when someone who has developed his own interests will utilize the occasion of the lecture to make them known and then begin the work toward which he had been heading before the lecture began. Here what happens is less recruitment than internally controlled self-starting, and the lecture merely provides a convenient opportunity for what would have occurred without it.

I therefore put to you my second proposition, one which I would be very pleased to have you refute in the discussion that is to follow this paper. My proposition is that, with the two exceptions stated above (job applicants and self-starters), working scientists who already have their research degrees are too well set into their own specialities to be good prospects for recruitment into another area such as parapsychology. They may feel the allure of working on its problems, but they are well able to resist it.

Where, then, is it worth directing our educational efforts so as to recruit the research workers we need? Not among adults who are set into their own careers. Should it be among young people who have not yet

decided on a vocation? Perhaps; but few young people have the temperament for scientific training and its peculiar disciplines. My guess is that attempts to recruit at the high school level or below will spark short-burning fires that soon die out. I therefore suggest to you that the best possibility lies in a middle ground between these extremes: with young people who have already exposed themselves to scientific disciplines and who find them congenial, but who have not committed themselves as yet to the long course of training which will make them expert in some other highly specialized area.

And where do we find this most favorable group? My suggestion is that it is among the advanced undergraduates who are science majors, or among the first or perhaps the second year graduate students in science at a university. They will have already gone through the preliminary hazing that science programs demand: the required techniques courses in laboratory methods and in statistics or other mathematics—courses notorious at most colleges for having stringent requirements but being low in interesting content. Students who have survived these courses without changing their majors are outstandingly our best candidates for education in parapsychology. It is to them, I think, that we should direct all possible educational efforts.

The reason I consider this group so promising comes only secondarily from the logical arguments I have been trying to present. The primary reason is experience. And here, again, it seems appropriate to tell you the kind of experiences that have predominantly led to failures on my part before recommending that you as well as I should emphasize the other kind that has been more successful.

Over the years a very large number of people have written to me or spoken to me about wanting to do parapsychological research. My natural response has been to encourage them and try to help. We first specify together the particular questions which make them most enthusiastic, state the hypothesis they will examine, and then work out together the details of method, recording, finding appropriate subjects, and so on. And I assure them that after they have their data, I will help with—or perform, if they choose—the data analysis. They seem eager and active: good research recruits.

These preliminaries take many letters—long letters!—if the people are some distance away, or many interviews if they are in the neighborhood. A lot of their time and mine goes into it, but from my point of view most of the efforts are wasted. Typically, if the persons had no prior research background, the data-collecting stops with a pilot study, which satisfies them but not me. There have been a few occasions when the person will go on to complete useful work, and

these exceptions have been encouraging enough so that even now I cannot help responding enthusiastically to enthusiasm and spending hours trying to think through problems with the scientifically untrained. But dispassionately, looking back over the years, it seems that time spend this way is not well spent. If our primary goal in advancing parapsychology should still be the research that adds to our information about it, then it seems more useful to direct our efforts to those trained in research than toward even the most enthusiastic of the untrained.

Now let me speak to the efforts that have been more productive. Occasionally undergraduates have come to me for an honors project, or some similar plan which could fit into a unit of the college curriculum, in parapsychological research. They would normally have had basic experimental and statistical training before they felt themselves ready for it. I've taken them on, of course; and fairly often the research that they did merited publication. Some stopped there; others stayed with the field for a short time after commencement; but in either case the work seemed useful. For an area as short of manpower as ours, even a one-shot published research report or a few productive years are likely to be helpful.

First or second year graduate students fit into the same general category, and recently there have been many more of these. With such students, little effort is wasted. As we work out the particular problem they want to cope with, and the appropriate way of going about it, these trained people recognize quickly how much hard work it will entail. With this recognition, they soon reach either the decision to drop it or else the decision to continue. They know where they are. And of course anyone who has served as apprentice in a parapsychology laboratory, and gone through the laborious process of data collection and data analysis, has a background comparable to what is gained from statistics and laboratory courses. He knows where he is, too.

From the point of view of research outcome, then, working with trained people is rewarding. Often it results in good research; and when it does not, little time has ordinarily been lost. But there is another further reward, which might in the long run be even more meaningful. These trained people are likely to go forward to professional work, if not in parapsychology then in their own scientific fields. They often end up in university teaching positions. They are thus, after a parapsychology project, our emissaries to the academic world. They will have had enough background in our area to vote favorably for the introduction of a parapsychology course if the question comes up in a faculty meeting, or probably to bone up for and

then teach such a course when it is offered. Training future professionals is, probably, our best long term investment.

More recently a different way of reaching the same population has become possible for me. Instead of merely letting it be known that I was willing to sponsor research in parapsychology, I have twice been able to offer a parapsychology course, and it looks as if next year I will teach two more, one undergraduate and one doctoral. The doctoral course will of course be research oriented and therefore needs no discussion here. But it may be worthwhile to tell you about the undergraduate ones, because they have produced what seem promising results: three active members of a working team in parapsychology. Though two courses are obviously a small sample from which it is unsafe to generalize, we might share the unsafe hope that what worked well twice will work again if it is tried, at City College or elsewhere.

What seems to have been the most important factor in their working well is something that was put into effect before enrollment: the decision to limit matriculation to qualified students. The particular qualifications that the registration desk demanded were, as by now you of course expect, that psychology majors should have statistics and experimental laboratory courses as prerequisite or corequisite, and that students not majoring in psychology should have the equivalent in their own fields. It seems to me that there are two useful functions of this requirement. One is that it brings together a group that knows the ground rules. The other is the converse: it warns away the students whose interest is only in personal discussion. They are shown a red flag, as it were, telling them that the road ahead is a bumpy one where they have to go at low speed.

One further part of the course seems to have been useful, and since it is perhaps unusual, I will tell you about it in some detail. That was the option, offered early, between writing the usual kind of library term paper or doing an experimental project instead. The choice (rather than requiring an experiment) seems to me to be necessary so that students will not feel they are being drafted as unpaid assistants in what, after all, is more my interest than theirs. And making the research project equivalent to a term paper means that a substantial block of time will be devoted to it, far more than if data-collecting were only a part of the regular course assignments.

For a laboratory project, it's long been my feeling that students should be advised to do a replication of published research. Both in the experimental psychology courses which I have taught our graduate students for many years and here, I urge them to choose for replication either of two types of research. One type is the kind that leads to a

conclusion which seems so natural and "right" to them that they are willing to accept it as a basis for their further thinking. Redoing it will show if it is as dependable as the published report seems; whether they should continue to accept it or should look elsewhere for stability. The other type is the kind that seems to them incredible and bizarre, contrary to their preconceptions. If the reported results work out for them too, they may need to change their preconceptions; and if the results fail to replicate, they may be able to publish a refutation which will keep others from falling into the error of accepting the original author's contention.

For students who feel insecure about research, advice to replicate, I find, alleviates anxiety. It gives them guidelines to follow; it makes them secure enough to take that crucial first step into the laboratory. But the best part of such advice is that bright students don't take it. What happens again and again is that someone will start with an article which he or she intends to copy exactly, but then says, "What would happen if. . ?" and proposes a change in the method. Ideally, then, they would split up their subjects and collect half the data with the old procedure and half with the new. More often, because there is not time to do the whole job, they will (with my encouragement) try out their own idea, and find themselves doing original research when they had not realized they could. Attempting to follow often turns into leading instead. And of course advising rather than requiring replication permits sufficient freedom for a student who prefers to investigate some radically novel idea, if that idea seems practical; it leaves options open.

There are two other advantages in it, I think: one technical and one fundamental. The technical one is that attempting to follow someone else's published method results in more careful reading than usual—reading which all too often shows that important points were not adequately described. This not only makes for healthy skepticism about the generality of the conclusions but also for the equally healthy resolve that when the reader writes up his own research for publication, he will do a more complete description of factors like subject selection, experimenter attitude, subtleties of instructional wording, and others which could determine the outcome of the research.

The more fundamental point deals with research values. Naive students, and unfortunately some professionals, feel that the research most worth doing is something that will lead to a sensational breakthrough, something radically original. And of course this would indeed be valuable, if it stands up to later replication. Too often, as we

all know, the sensational claims go up like a rocket but come down like a stick. Much more important, as Gardner Murphy kept telling me years ago, is any finding—even a small, pedestrian one—that will stand up to replication by oneself and then others. This seems to me a basic attitude to inculcate, for a research worker in any field. Unless his values are true, he may become a liability instead of an asset. Emphasis on replication therefore serves the further purpose of showing the potential research worker subtly and indirectly, as well as telling him directly, that there would be no point in publishing work unless the results are stable enough that others who try for them will find them, too. Overstating one's findings, or failure to specify the particular conditions which led to successes, will not go uncorrected for long. It is the results which stand up in other laboratories for which even the beginning research worker should aim.

This is where I planned to end my paper, but as I reread it, it seemed to me that some of you might want—since this is an educational symposium—to hear more detail about the formal courses in parapsychology which our conservative university has recently put into the books. Impetus for them came from two sources. The more important one was student demand: the groundswell of interest that we see almost everywhere. The other came from having available a member of the college community (myself) who was able to propose a course which seemed appropriate for academic learning, which emphasized the critical appraisal of published research, and in general spoke the approved academic jargon. My estimate is that we do well if we work from within rather than from the outside; that we can best take advantage of student demand if conventionally qualified instructors are available. Training in parapsychology alone may be counter-productive; it may make the college departments which consider including a parapsychology course refuse to hire someone who seems alien to them. I recommend rather a sort of gradualism: that someone with an orthodox degree in physics or philosophy or psychology or some other standard discipline be, in addition, trained in parapsychology.

On this basis, if a course is approved within a college department rather than being relegated to the limbo of adult education or a nondepartmental offering, library and laboratory facilities offer no problem. Any college library will ordinarily have a budget item for new courses, and be able to buy most or all of the journals and books needed, or perhaps at worst require a very small supplemental grant. The college laboratory facilities will similarly be open; with them as a base, only small requests will be needed for specialized equipment. The

library and laboratory changes will be smooth, instead of seeming to be a foreign body grafted on a dubious or rejecting host.

The single need which I have felt most pressing is for a suitable textbook. Texts in better established specialties are usually so good that student standards are high: they demand careful and full coverage, a conservative but interesting writing style, and so on. We have nothing that is both appropriate and up to date as a text, though we abound in excellent supplementary readings. A text that is modelled after the good introductory ones in the usual academic areas would not only be a help to both instructors and students but would also, I think, if scanned by university professors, add to parapsychology's prestige.

Let me summarize. I have been trying to put three more or less controversial theses to you. The first is that at present, educational efforts in parapsychology should center, directly or indirectly, on furthering research rather than on application or popularization. The second is that the best way of encouraging research is to recruit among advanced undergraduates or else students in the first years of graduate work in a science. The third is that the primary goal to be put forth to the new recruits is that their work should be so careful and so clearly described that it will stand up to replication. This last is a demand for dullness, in a sense; but I think it would be easy to defend the contention that even the most quick and brilliant person must sometimes change his pace and work out his ideas in a pedestrian, careful, plodding way if he is to make scientific advances.

DISCUSSION

ONETTO: I would like to ask you a question about the three people you say were working with you. Did they really come from undergraduate courses or levels, or from other sources?

SCHMEIDLER: There are about seven people who meet regularly with me every week in a noncredit seminar, and of that number, three came from undergraduate courses. It's a serious discussion group, research-oriented.

ONETTO: In other words, you have a higher percentage than you usually get from a group of students from the regular courses.

SCHMEIDLER: You're comparing it to my regular courses?

ONETTO: Yes. The percentage is much higher—practically forty per cent—from seven, you say three.

SCHMEIDLER: It's a special kind of group—doctoral student, M.A.s and two undergraduates, one from a different university.

ONETTO: The point I want to sustain is that perhaps it's not so bad as you say; that sometimes professional people also have graduate courses in parapsychology, say, clinical psychologists or a physician or an engineer. They don't need parapsychology. So if they come to your courses, I would expect that you have a higher percentage. You will find people who are really interested, because they don't need it.

SCHMEIDLER: Yes, that's true. But I think that if, instead of an undergraduate course, I had been giving a graduate or a post-graduate course, then more than three out of forty would have come.

ONETTO: Three out of forty. . .

SCHMEIDLER: Well, enrollment in those two courses was officially limited to twenty. It seems to me that the proportion of students who turned into actual workers is rather low. It would be interesting, I guess, to get some statistical comparison of how many students and how many courses started at each level and at what level did more published research projects come.

TART: Gertrude, I agree with you about recruiting of the early graduate student level, but maybe I'm more conservative than you. I tell them to read up, get their interest going, but for God's sake, don't do their dissertation on parapsychology if they want to get a job! Do you have that problem?

SCHMEIDLER: Well, what I keep telling them is we need to tie things in, and you can be working on an orthodox psychology problem and parapsychology at the same time.

TART: Especially if it's something that will give you tools that you can later use in a parapsychology study.

SCHMEIDLER: Yes, the tools are the same for psychology and parapsychology. I'm kind of worried about the young man who is doing a doctoral dissertation with me now in straight parapsychology, without investigating any psychological problem at the same time. One reason that I went ahead with it was that he wouldn't have it any other way, so I didn't have a choice. But the reason that it seemed all right to me was that he had already been supporting himself by psychological work for years, so he knew how to function as a psychologist. He knew what reality was. I don't think it's quite as bad in the job market now as it was a few years ago, to be labeled as interested in parapsychology; but it still isn't good.

ROGO: I'm wondering, really, how eager we should be to recruit students for research. Having conducted some seminars and having taught in seminars such as Bob is doing in the University of California at Santa Barbara, I think that students coming into our field are much too eager to do research. They don't have that same level of eagerness to sit down and really learn the background history and literature of the field before they do that research. I'm wondering if it really serves the students to encourage them right off the bat to get into experimental research before they really have sat down and gotten a good grasp of what other people have done, what the whole history and literature on experimental parapsychology is, and what the history and literature of non-experimental parapsychology is before they try to embark on something that really might be over their heads completely.

SCHMEIDLER: That's a very difficult point, and I think not just in parapsychology. It's true in psychology too, that if you get a really conscientious person who wants to work on some special area and decides to read everything in the *Psychological Abstracts* that have been published in that area, he spends so much time reading that he never gets his thesis done; and of course, if he goes in blind, all he does is replicate in a weak way what somebody else has already done well and what he's ignorant of. So I think what you did was correct. My tendency is to overstate one side of a difficult issue. . .

ROGO: I'm actually thinking more in terms of the undergraduate students than students at the graduate level who do have thesis requirements.

SCHMEIDLER: Well, maybe a compromise there—that is, when a person says that he is interested in this particular project, you say, "Read these two articles, or those three. They're short. They're in your journals; they're on your topic. Then see if you want to do something along those lines. . ." and then maybe let him do some selective reading besides.

KRIPPNER: I'd like to comment on Dr. Schmeidler's advocacy of the replication of past research studies. Frankly, I feel that this is one of the great problems in our field; that people are so eager to attack the thousands of hypothetical research experiments that they don't try to replicate what has already happened, to check on the conditions, collect more data, start to look into the variables that can be manipulated, and build up a much more solid body of evidence. It might not be as glamorous or exciting to repeat somebody else's experiment, but I think in the long run for the field it will be more helpful.

We have an observer sitting in back of me—Henry Dakin—who has a laboratory here in San Francisco where a number of researchers are doing some interesting work. It is to his credit that he has emphasized repeating work that's been done, especially replications of the early Grad experiments. Once they find out if Dr. Grad's work on seeds and plants can be replicated, they might go on from there. There are all sorts of interesting research studies lying around in the journals which nobody has done since they were reported by the original experimenter.

SCHMEIDLER: I wonder if Mr. Dakin would like to say whether his students, too, find that when they start working on a replication they keep being allured by wanting to introduce this change of procedure or that change of procedure, so they compromise between original research and replication research and do both at once.

DAKIN: We haven't finished analyzing the data on that.

KRIPPNER: What's the process that your assistant has gone through with the seeds exposed to water treated by a "psychic healer?" Has he been sticking very closely to the Grad work or has he attempted to veer off on his own?

DAKIN: I think he's trying to follow it as closely as he can.

SCHMEIDLER: That's virtuous. That's the way it should be.

STANFORD: I don't want to disagree with what you say about the value of the approach that you've advocated in training persons to do research. I see no problem with that. I do want to point out that some other kinds of approaches are being tried in some places and I think are having some usefulness. Now there was a program for undergraduate psychological research instituted at St. John's University some time before I came there, which seems to have resulted in an undergraduate program of laboratory experimental courses where the students seem to have a higher level of morale than in any other undergraduate program that I've ever personally encountered. Now the way that this is done is that even in undergraduate labs, let alone graduate labs, *every* experiment that a student undertakes, even in a group lab, is never an exact replication of anything that is done. The students seem to derive from this the feeling that there is a possibility for them to contribute in some way to our knowledge. I have had the experience, and many other teachers have also had the experience of students being utterly bored with doing standard textbook type experiments over and over again. One of the results of that boredom is

"I really don't want to do that," and some students lapse into cheating; some expect that if this was gotten before, it's got to be had again—we've got to produce those results.

Now I'm not pushing this paradigm as an ideal one either, but I am saying that in training and education I think we ought to benefit from learning theory and recognize that if what a person does, if he's doing it right and if it's rewarding to him, this is a valuable educative experience. What you're proposing, Dr. Schmeidler, could be rewarding to certain students. My own feeling about this is that when I work with students, I find I try to feel out the inclinations of the individual student. There are some students who seem to be inclined toward doing exact replication or a slight modification; there are others who want to go way off into outer space, and usually with a little talk you can tone that down, or you can get them to tone that down a bit, but I think that in trying to get an individual trained for research, one needs to consider the inclinations of the individual. We know that there are some scientists whose forte really is in doing the small step-by-step research and others, the bolder, leaping type of thing, and if we can train our students with regard to their individual inclinations yet with plenty of discipline, I think that we may turn out a quality product.

SCHMEIDLER: I would be interested to know how you introduce the replications into your students' plans. Do you let them work out their own variations, or do you impose the variations on them?

STANFORD: Well, it would depend to some extent on the circumstance of the individual student, but one of the things that I would certainly always try to do is to make sure that they first of all really understand what was done in the original experiment, and that is really a problem, because I've seen people try to replicate things where they didn't understand it. They made a horrible mess of it.

JOHNSON: I would like to add that very often it's impossible to replicate because the way the articles are written and published there isn't enough information given, and I think that has an educational impact.

SCHMEIDLER: I agree, fully.

MORRIS: I'd like to follow up on just that last point because I think one of the most valuable things in Dr. Schmeidler's talk was the whole business of having people read a journal article in detail, to try to understand what was done in order to repeat it. It's been almost a uniform experience that articles are not well written, such that we don't get the wealth of procedural details that seem to be necessary in

parapsychology, especially with all the variables involved. One strategy I use to get this point across to students is to require Hansel as a text for our research methods course, since many of the criticisms that he comes up with simply involve the lack of experimental procedure description and precision. He's always saying, "Well, now thus and so might have happened. The author doesn't let us know," and that leads students to question the details of any study. I also think that this problem is not going to be completely solved until and unless we find better ways of writing up our procedures and finding techniques for compressing the space that it takes, because we could blather on for hours describing what we wore and what our attitude was at the moment, etc. We do need to solve a problem here.

WHITE: Maybe one possible solution to that problem would be to write up a very thorough description of what you did, and even though it can't be published in a report, you could put a footnote, as sometimes people do, stating that anybody interested in further information could write for the complete protocol. Or we could even have the Parapsychological Association or some other organization deliberately keep this sort of thing on file so the individual wouldn't have to be involved with paper work.

SCHMEIDLER: That sounds like a very good idea.

RAO: How does it compare with the published psychological experiments? My own feeling is that the procedural details that are given in the reporting of parapsychological experiments are far more extensive in comparison to a number of psychological studies that are published. I think the failure of repeatability in obtaining the same results is very much in the nature of the subject matter we are investigating rather than the lack of appreciation of the test procedures involved. Even if there is the desired appreciation, I think, I can safely bet on failure of repeatability rather than successful repetition. I think it is in the very nature of the subject matter with which we are dealing rather than in the lack of communicating the proper procedural details. The way we handle our subjects is extremely crucial and this cannot be easily communicated in words.

SCHMEIDLER: My impression is that psychology and parapsychology are just about parallel there. In psychology experiments, too, you often find people from one university getting one result with the procedure, and others who claim to be using the same procedure getting different results, until the two visit each other and find out the things that each has omitted in the writeup; and then when they try varying those

previously unstated factors, their results begin to converge. As far as the box score of replications in psychology is concerned, there are many well-established areas where the percentage of replicated results is just about comparable to what we get in a lot of parapsychological areas. And this teaching method that I've been suggesting for parapsychology is the one that I've been using in psychology and that students seem to like. It starts them toward dissertation research. So I agree with the tenor of your remarks. When your question is "How different is parapsychology?" I'd say, "Not very."

NOVILLO: Some researchers in the field of parapsychology want to replicate the investigations performed in other research centers, but obtain different results. I am talking about replication in my country. I think the results are different because the researchers don't pay attention to different conditions when they are testing the subjects.

Good subjects are children at elementary school level, but it is necessary to know beforehand the psychological situation of the students in their family and social life and their relationship with their teacher. If the researcher wants to interpret correctly the results of his tests, he must know the psychological environment of the subjects from talking with great prudence to the director and the psychologist of the school in order to get information about teacher and students as well. On the other hand, if the students dislike the teacher, the researcher or the tests, then the results would be very different. It would be a totally different research work and not a replication of the former experiment because of the changed conditions.

SCHMEIDLER: So you're suggesting that a lot of us are working in problems of social parapsychology without knowing it and that this ought to be stated too.

NOVILLO: I don't want to say that all researchers don't consider this social parapsychology, if you want to call it that. What I am trying to explain is that we need to know very precisely the psychological environment where the research work will be done.

I have got good results when there was good relationship between students and teacher, and where the teacher explains and encourages the research work; and other times negative or chance results where the teacher does not properly handle the students.

SCHMEIDLER: It sounds like a brilliant way of handling research, but you might make yourself so unpopular with all the teachers who lose the contest that you'd have to leave that town quickly when you got your experiment done.

TART: I understand the importance of replication as we've talked about it, but I think to some extent we're a little bit too taken with it. Replication is very important in a science when you're in a paradigmatic stage and your results are cumulative in a very real sense. To take the devil's advocate position, let me make the extreme statement that the level of results we generally get in parapsychology represent such a low absolute level of the phenomena, that why bother to repeat them? Maybe we'd be better off just trying this, that, and the other thing in the hope that we'll stumble on something by luck or some unknown unconscious psi process that will give a much higher level of results, and that's what we need. So, yes, replication, but maybe we're better off stumbling around to some extent at this point.

SCHMEIDLER: Well, as to that, I do think I have a response. And that is, not everybody is brilliant; that people can make useful contributions along a whole gamut of intelligence; that offering replication or even suggesting it as one worthy possibility, alleviates anxiety and brings in the worthy but more pedestrian thinkers. I'm suggesting it as a weak possibility which offers an option to the bright innovators. If they say, "Well, replication is okay but I don't want to," then they can do their own thing. I do try all the way through to advise but not to require.

TART: It's really a mixture of both things and depends very much on the personality of the experimenter. Some people like to do careful replications and build steadily, and others hop around.

BELOFF: Could I ask you simply, what is your policy with regard to publication of any findings that come out of these replications, whether positive or negative?

SCHMEIDLER: I've been thinking about that lately, especially since Bob McConnell has been circulating his questions about suppressing results, and it seems to me that I just have no policy. Students vary so much. Some of them give me the impression of doing extremely careful work. When the careful students are done with an experiment—whether the results are positive or negative—I urge them to write a report and submit it. But other students don't impress me as being so rigorous and with them, no matter what the results are, I'm likely to say "Very interesting," and let it go at that. That's really not a policy at all, is it? That is, I treat some students as colleagues, but others just as beginners.

BELOFF: Well, I think it's a very excellent policy. In other words, it's merit that decides whether you push them to publication, not whether it's positive or negative.

SCHMEIDLER: Oh, sure.