Journal of Personality and Social Psychology
1973, Vol. 26, No. 3, 446–456

THE YIN AND YANG OF PROGRESS IN SOCIAL PSYCHOLOGY: SEVEN KOAN [1]

WILLIAM J. McGUIRE [2]

Yale University

We describe the current dissatisfactions with the paradigm that has recently guided experimental social psychology—testing of theory-derived hypotheses by means of laboratory manipulational experiments. The emerging variant of doing field experiments does not meet the criticisms. It is argued that an adequate new paradigm will be a more radical departure involving, on the creative side, deriving hypotheses from a systems theory of social and cognitive structures that takes into account multiple and bidirectional causality among social variables. On the critical side, its hypotheses testing will be done in multivariate correlational designs with naturally fluctuating variables. Some steps toward this new paradigm are described in the form of seven koan.

The Paradigm Recently Guiding Experimental Social Psychology

When the XIXth Congress met 3 years ago in London, and certainly a half-dozen years back at the Moscow Congress, social psychology appeared to be in a golden age. It was a prestigious and productive area in which droves of bright young people, a sufficiency of middle-aged colonels, and a few grand old men were pursuing their research with a confidence and energy that is found in those who know where they are going. Any moments of doubt we experienced involved anxiety as to whether we were doing our thing well, rather than uncertainty as to whether it needed to be done at all.

The image of these golden boys (and a few, but all too few, golden girls) of social psychology, glowing with confidence and chutzpah only 6 years back at the Moscow Congress, blissfully unaware of the strident attacks which were soon to strike confusion into the field, brings to mind a beautiful haiku of Buson that goes

Tsurigane-ni
Tomarite nemuru
Kochō kana

which I hasten to translate as follows

On a temple bell
Settled, asleep,
A butterfly.

We social psychology researchers know all too well that the peaceful temple bell on which we were then displaying ourselves has now rudely rung. During the past half-dozen years, the vibrations which could be vaguely sensed at the time of the Moscow meeting have gathered force. Now the temple bell has tolled and tolled again, rudely disturbing the stream of experimental social psychological research and shaking the confidence of many of us who work in the area.

The first half of this paper is devoted to describing the three successive waves of this current history. First, I shall describe the experimental social psychology paradigm that has recently guided our prolific research. Second, I shall discuss why this recent paradigm is being attacked and what, superficially at least, appears to be emerging in its place. Third, I shall say why I feel the seemingly emerging new paradigm is as inadequate as the one we would replace. Then, in the second half of this paper I shall offer, in the form of seven koan, my prescriptions for a new paradigm, more radically different from the recent one, but more in tune with the times and the march of history than is the variant that is supposedly emerging.

[1] This paper is based on an address given at the Nineteenth Congress of the International Union of Scientific Psychology at Tokyo in August 1972.

[2] Requests for reprints should be sent to the author, Department of Psychology, Yale University, 333 Cedar Street, New Haven, Connecticut 06510.

The Old Paradigm

What was the experimental social psychology paradigm which until recently had been unquestioningly accepted by the great majority of us but which now is being so vigorously attacked? Like any adequate paradigm it had two aspects, a creative and a critical component (McGuire, 1969, pp. 22–25). By the creative aspect, I mean the part of our scientific thinking that involves hypothesis generation, and by the critical aspect, I mean the hypothesis-testing part of our work.

The creative aspect of the recent paradigm inclined us to derive our hypotheses from current theoretical formulations. Typically, these theoretical formulations were borrowed from other areas of psychology (such as the study of psychopathology or of learning and memory), though without the level of refinement and quantification which those theories had reached in their fields of origin.

The critical, hypothesis-testing aspect of the recent paradigm called for manipulational experiments carried out in the laboratory. The experimental social psychologist attempted to simulate in the laboratory the gist of the situation to which he hoped to generalize, and he measured the dependent variable after deliberately manipulating the independent variable while trying to hold constant all other factors likely to affect the social behavior under study. In brief, the recent paradigm called for selecting our hypotheses for their relevance to broad theoretical formulations and testing them by laboratory manipulational experiments. McGuire (1965) presented an emphatic assertion of this recent paradigm in its heyday.

Assaults on the Old Paradigm

During the past several years both the creative and the critical aspects of this experimental social psychology paradigm have come under increasing attack. The creative aspect of formulating hypotheses for their relevance to theory has been denounced as a mandarin activity out of phase with the needs of our time. It has been argued that hypotheses should be formulated for their relevance to social problems rather than for their relevance to theoretical issues. Such urgings come from people inside and outside social psychology, reflecting both the increasing social concern of researchers themselves and the demands of an articulate public for greater payoff from expensive scientific research. While many of us still insist with Lewin that "There is nothing so practical as a good theory," the extent to which the pendulum has swung from the theoretically relevant toward the socially relevant pole is shown in the recent upsurge of publications on socially important topics of ad hoc interest, such as bystander intervention, the use of local space, the mass media and violence, the determinants of love, responses to victimization, nonverbal communication, etc.

At least as strong and successful an assault has been launched on the critical aspect of the recent paradigm, namely, the notion that hypotheses should be tested by manipulational laboratory experiments. It has been urged that laboratory experiments are full of artifacts (such as experimenter bias, demand character, evaluation apprehension, etc.) which make their results very hard to interpret. Ethical questions also have been raised against the laboratory social experiments on the grounds that they expose the participants to an unacceptable amount of deception, coercion, and stress.

In place of the laboratory manipulational experiment, there has been a definite trend toward experiments conducted in field settings and toward correlational analysis of data from naturalistic situations. A variety of recent methodological advances (which we shall list under Koan 5) has made alternative hypothesis-testing procedures more attractive.

The attacks on the old paradigm of theory-derived hypotheses tested in laboratory manipulational experiments have certainly shaken confidence in that approach. At the same time, there is some suggestion of an emerging new paradigm which has as its creative aspect the derivation of new hypotheses for their ad hoc interest and social relevance. And in its critical aspect, this new paradigm involves testing these hypotheses by field experiments and, where necessary, by the correlational analysis of naturalistic data. McGuire (1967, 1969) described in more detail the worries about the recent paradigm and the nature of the purportedly emerging one. Higbee and Wells (1972) and Fried, Gumpper, and Allen (1973) suggested that reports by McGuire, by Sears and Abeles (1969), etc., of the demise of the recent paradigm may be exaggerated, but perhaps they have underestimated the time that must intervene before a change of vogue by the leaders shows up in mass analysis of the methods used in published research.

More Basic Questions Regarding Both the Recent and Emerging Paradigms

My own position on the relative merits of the recent paradigm and this supposedly emerging new paradigm is a complex and developing one which I have detailed in print (McGuire, 1965, 1967, 1969) so the reader will be spared here a recital of my Byzantine opinions on this issue. Instead, I am raising the more fundamental issue of whether or not both the recent and the seemingly emerging paradigms which I have just described fail to come to grips with the deeper questions which lie behind our present unease. It seems to me that any truly new paradigm that ultimately arises from the present unrest is going to be more radically different from the recent one than is the supposedly emerging paradigm I have just depicted. It will represent a more fundamental departure on both the creative and the critical sides.

Inadequacies on the Creative Side

The switch from theory relevance to social relevance as the criterion in the creative, hypothesis-generating aspect of our work seems to me to constitute only a superficial cosmetic change that masks rather than corrects the basic problem. Socially relevant hypotheses, no less than theoretically relevant hypotheses, tend to be based on a simple linear process model, a sequential chain of cause and effect which is inadequate to simulate the true complexities of the individual's cognitive system or of the social system which we are typically trying to describe. Such simple a-affects-b hypotheses fail to catch the complexities of parallel processing, bidirectional causality, and reverberating feedback that characterize both cognitive and social organizations. The simple sequential model had its uses, but these have been largely exploited in past progress, and we must now deal with the complexities of systems in order to continue the progress on a new level.

The real inadequacy of the theory-derived hypotheses of the recent paradigm is not, as those now advocating socially relevant hypotheses insist, that it focused on the wrong variables (those that were theory rather than problem relevant). Rather, the basic shortcoming of the theory-relevant and the socially relevant hypotheses alike is that they fail to come to grips with the complexities with which the variables are organized in the individual and social systems.

Inadequacies of the Critical Aspect of the Recent Paradigm

The critical, hypothesis-testing aspect of the purportedly emerging paradigm also has the defect of being but a minor variant of the recent experimental social psychology paradigm rather than the fundamental departure which is called for. Let me first describe some of the deep epistemological uneasiness some of us have been expressing about the manipulational laboratory experiment that was the hypothesis-testing procedure of the recent paradigm. The crux of this objection is that we social psychologists have tended to use the manipulational laboratory experiment not to test our hypotheses but to demonstrate their obvious truth. We tend to start off with an hypothesis that is so clearly true (given the implicit and explicit assumptions) and which we have no intention of rejecting however the experiment comes out. Such a stance is quite appropriate, since the hypothesis by its meaningfulness and plausibility to reasonable people is tautologically true in the assumed context. As Blake said, "Everything possible to be believ'd is an image of truth."

The area of interpersonal attraction will serve to illustrate my point. The researcher might start off with a *really* obvious proposition from bubba-psychology, such as "The more someone perceives another person as having attitudes similar to his own, the more he tends to like that other person." Or a somewhat more flashy researcher, a little hungrier for novelty, might hypothesize the opposite. That is, he could look for certain circumstances in which the generally true, obvious hypothesis would obviously be reversed. He might hypothesize exceptional circumstances where attitudinal similarity would be anxiety arousing and a source of hostility; for example, if one loves one's wife, then one might actually dislike some other man to the extent that one perceives that other as also loving one's wife. Or another exceptional reversal might be that some people may think so poorly of themselves that they think less well of another person to the extent that the other person is like themselves. If the negative relationship is not found, we are likely to conclude that the person did not have a sufficiently low self-image, not that the hypothesis is wrong. Both the original obvious hypothesis and the obvious reversed hypothesis are reasonable and valid in the sense that if all our premises obtained, then our conclusion would pretty much have to follow.

Experiments on such hypotheses naturally turn out to be more like demonstrations than tests. If the experiment does not come out "right," then the researcher does not say that the hypothesis is wrong but rather that something was wrong with the experiment, and he corrects and revises it, perhaps by using more appropriate subjects, by strengthening the independent variable manipulation, by blocking off extraneous response possibilities, or by setting up a more appropriate context, etc. Sometimes he may have such continuous bad luck that he finally gives up the demonstration because the phenomenon proves to be so elusive as to be beyond his ability to demonstrate. The more persistent of us typically manage at last to get control of the experimental situation so that we can reliably demonstrate the hypothesized relationship. But note that what the experiment tests is not whether the hypothesis is true but rather whether the experimenter is a sufficiently ingenious stage manager to produce in the laboratory conditions which demonstrate that an obviously true hypothesis is correct. In our graduate programs in social psychology, we try to train people who are good enough stage managers so that they can create in the laboratory simulations of realities in which the obvious correctness of our hypothesis can be demonstrated.

It is this kind of epistemological worry about manipulational laboratory experiments that a half-dozen years back caused a number of observers (e.g., McGuire, 1967) to urge social psychology to search for interrelations among naturally varying factors in the world outside the laboratory. Out of these urgings has come the critical aspect of the apparently emerging paradigm which I have described above, calling for research in the field rather than in the laboratory.

Inadequacies of the Critical Aspects of the Purportedly Emerging New Field-Experiment Paradigm

Recently, I have come to recognize that this flight from the laboratory manipulational experiment to the field study, which I myself helped to instigate, is a tactical evasion which fails to meet the basic problem. We would grant that in the field we put the question to nature in a world we never made, where the context factors cannot be so confounded by our stage management proclivities as they were in the laboratory. But in this natural world research, the basic problem remains that we are not really testing our hypotheses. Rather, just as in the laboratory experiment we were testing our stage-managing abilities, in the field study we are testing our ability as "finders," if I may use a term from real estate and merchandising. When our field test of the hypothesis does not come out correctly, we are probably going to assume not that the hypothesis is wrong but that we unwisely chose an inappropriate natural setting in which to test it, and so we shall try again to test it in some other setting in which the conditions are more relevant to the hypothesis. Increasing our own and our graduate students' critical skill will involve making us not better hypothesis testers or better stage managers but rather better finders of situations in which our hypotheses can be demonstrated as tautologically true. Though I shall not pursue the point here, other objections to the laboratory experiment, including ethical and methodological considerations, that have been used (McGuire, 1969) to argue for more field research could similarly be turned against experiments conducted in the natural environment.

What I am arguing here is that changing from a theory-relevant to a socially relevant criterion for variable selection does not constitute a real answer to the basic problem with the creative aspect of our recent social psychology paradigm. And again, the switch from laboratory to field manipulation does not meet the basic objection to the critical aspect of the old paradigm. Neither the recent paradigm nor the supposedly emerging one really supplies the answer to our present needs. The discontent is a quite healthy one, and we should indeed be dissatisfied with the recent paradigm of testing theory-derived hypotheses by means of laboratory manipulational experiments. But our healthy discontent should carry us to a more fundamentally new outlook than is provided by this supposedly emerging variant paradigm of testing socially relevant hypotheses by experiments in natural settings.

SOURCES OF THE NEW SOCIAL PSYCHOLOGY

The Ultimate Shape of the New Paradigm

What I have written in the previous section suggests my general vision of what the more radically different new paradigm for social psychology will look like. On the creative side, it will involve theoretical models of the cognitive and social systems in their true multivariate complexity, involving a great deal of parallel processing, bidirectional relationships, and feedback circuits. Since such complex theoretical formulations will be far more in accord with actual individual and social reality than our present a-affects-b linear models, it follows that theory-derived hypotheses will be similar to hypotheses selected for their relevance to social issues. Correspondingly, the critical aspect of this new paradigm involves hypothesis testing by multivariate time series designs that recognize the obsolescence of our current simplistic a-affects-b sequential designs with their distinctions between dependent and independent variables.

But I feel somewhat uncomfortable here in trying to describe in detail what the next, radically different paradigm will look like. It will be hammered out by theoretically and empirically skilled researchers in a hundred eyeball-to-eyeball confrontations of thought with data, all the while obscured by a thousand mediocre and irrelevant studies which will constitute the background noise in which the true signal will be detected only gradually. Trying to predict precisely what new pardigm will emerge is almost as foolish as trying to control it.

But there is a subsidiary task with which I feel more comfortable and to which I shall devote the rest of this paper. I have come to feel that some specific tactical changes should be made in our creative and critical work in social psychology so as to enhance the momentum and the ultimate sweep of this wave of the future, whatever form it may take. I shall here recommend a few of these needed innovations and correctives, presenting them as koans and commentaries thereon, to mask my own uncertainties.

Koan 1: The Sound of One Hand Clapping . . . and the Wrong Hand

One drastic change that is called for in our teaching of research methodology is that we should emphasize the creative, hypothesis-formation stage relative to the critical, hypothesis-testing stage of research. It is my guess that at least 90% of the time in our current courses on methodology is devoted to presenting ways of testing hypotheses and that little time is spent on the prior and more important process of how one creates these hypotheses in the first place. Both the creation and testing of hypotheses are important parts of the scientific method, but the creative phase is the more important of the two. If our hypotheses are trivial, it is hardly worth amassing a great methodological arsenal to test them; to paraphrase Maslow, what is not worth doing, is not worth doing well. Surely, we all recognize that the creation of hypotheses is an essential part of the scientific process. The neglect of the creative phase in our methodology courses probably comes neither from a failure to recognize its importance nor a belief that it is trivially simple. Rather, the neglect is probably due to the suspicion that so complex a creative process as hypothesis formation is something that cannot be taught.

I admit that creative hypothesis formation cannot be reduced to teachable rules, and that there are individual differences among us in ultimate capacity for creative hypothesis generation. Still, it seems to me that we have to give increased time in our own thinking and teaching about methodology to the hypothesis-generating phase of research, even at the expense of reducing the time spent discussing hypothesis testing. In my own methodology courses, I make a point of stressing the importance of the hypothesis-generating phase of our work by describing and illustrating at least a dozen or so different approaches to hypothesis formation which have been used in psychological research, some of which I can briefly describe here, including case study, paradoxical incident, analogy, hypothetico-deductive method, functional analysis, rules of thumb, conflicting results, accounting for exceptions, and straightening out complex relationships.

For example, there is the intensive case study, such as Piaget's of his children's cognitive development or Freud's mulling over and over of the Dora or the Wolf Man case or his own dreams or memory difficulties. Often the case is hardly an exceptional one—for example, Dora strikes me as a rather mild and uninteresting case of hysteria—so that it almost seems as if any case studied intensively might serve as a Rorschach card to provoke interesting hypotheses. Perhaps an even surer method of arriving at an interesting hypothesis is to try to account for a paradoxical incident. For example, in a study of rumors circulating in Bihar, India, after a devastating earthquake, Prasad found that the rumors tended to predict further catastrophes. It seemed paradoxical that the victims of the disaster did not seek some gratification in fantasy, when reality was so harsh, by generating rumors that would be gratifying rather than further disturbing. I believe that attempting to explain this paradox played a more than trivial role in Festinger's formulation of dissonance theory and Schachter's development of a cognitive theory of emotion.

A third creative method for generating hypothesis is the use of analogy, as in my own work on deriving hypotheses about techniques for inducing resistance to persuasion, where I formulated hypotheses by analogy with the biological process of inoculating the person in advance with a weakened form of the threatening material, an idea suggested in earlier work by Janis and Lumsdaine. A fourth creative procedure is the hypothetico-deductive method, where one puts together a number of commonsensical principles and derives from their conjunction some interesting predictions, as in the Hull and Hovland mathematico-deductive theory of rote learning, or the work by Simon and his colleagues on logical reasoning. The possibility of computer simulation has made this hypothesis-generating procedure increasingly possible and popular.

A fifth way of deriving hypotheses might be called the functional or adaptive approach, as when Hull generated the principles on which we would have to operate if we were to be able to learn from experience to repeat successful actions, and yet eventually be able to learn an alternative shorter path to a goal even though we have already mastered a longer path which does successfully lead us to that goal. A sixth approach involves analyzing the practitioner's rule of thumb. Here when one observes that practitioners or craftsmen generally follow some procedural rule of thumb, we assume that it probably works, and one tries to think of theoretical implications of its effectiveness. One does not have to be a Maoist to admit that the basic researcher can learn something by talking to a practitioner. For example, one's programmed simulation of chess playing is improved by accepting the good player's heuristic of keeping control of the center of the board. Or one's attitude change theorization can be helped by noting the politician's and advertiser's rule that when dealing with public opinion, it is better to ignore your opposition than to refute it. These examples also serve to remind us that the practitioner's rule of thumb is as suggestive by its failures as by its successes.

A seventh technique for provoking new hypotheses is trying to account for conflicting results. For example, in learning and attitude change situations, there are opposite laws of primacy and of recency, each of which sometimes seems valid; or in information integration, sometimes an additive or sometimes an averaging model seems more appropriate. The work by Anderson trying to reconcile these seeming conflicts shows how provocative a technique this can be in generating new theories. An eighth creative method is accounting for exceptions to general findings, as when Hovland tried to account for delayed action effect in opinion change. That is, while usually the persuasive effect of communications dissipates with time, Hovland found that occasionally the impact actually intensifies over time, which provoked him to formulate a variety of interesting hypotheses about delayed action effects. A ninth creative technique for hypothesis formation involves reducing observed complex relationships to simpler component relationships. For example, the somewhat untidy line that illustrates the functional relationship between visual acuity and light intensity can be reduced to a prettier set of rectilinear functions by hypothesizing separate rod and cone processes, a logarithmic transformation, a Blondel-Rey-type threshold phenomenon to account for deviations at very low intensities, etc.

But our purpose here is not to design a methodology course, so it would be inappropriate to prolong this list. Let me say once again, to summarize our first koan, that we have listened too long to the sound of one hand clapping, and the less interesting hand at that, in confining our methodology discussion almost exclusively to hypothesis testing. It is now time to clap more loudly using the other hand as well by stressing the importance of hypothesis generation as part of psychological methodology.

Koan 2: In This Nettle Chaos, We Discern This Pattern, Truth

I stress here the basic point that our cognitive systems and social systems are complex and that the currently conventional simple linear process models have outlived their heuristic usefulness as descriptions of these complex systems. In our actual cognitive and social systems, effects are the outcome of multiple causes which are often in complex interactions; moreover, it is the rule rather than the exception that the effects act back on the causal variables. Hence, students of cognitive and social processes must be encouraged to think big, or rather to think complexly, with conceptual models that involve parallel processing, nets of causally interrelated factors, feedback loops, bidirectional causation, etc.

If we and our students are to begin thinking in terms of these more complex models, then explicit encouragement is necessary since the published literature on social and cognitive processes is dominated by the simple linear models, and our students must be warned against imprinting on them. But our encouragement, while necessary, will not be sufficient to provoke our students into the more complex theorizing. We shall all shy away from the mental strain of keeping in mind so many variables, so completely interrelated. Moreover, such complex theories allow so many degrees of freedom as to threaten the dictum that in order to be scientifically interesting, a theory must be testable, that is, disprovable. These complex theories, with their free-floating parameters, seem to be adjustable to any outcome.

Hence, we have to give our students skill and confidence and be role models to encourage them to use complex formulations. To this end we have to give greater play to techniques like computer simulation, parameter estimation, multivariate time series designs, path analysis, etc. (as discussed further in Koan 5 below), in our graduate training programs.

Koan 3: Observe. But Observe People Not Data

In our father's house there are many rooms. In the total structure of the intelligentsia, there is a place for the philosopher of mind and the social philosopher, as well as for the scientific psychologist. But the scientific psychologist can offer something beside and beyond these armchair thinkers in that we not only generate delusional systems, but we go further and test our delusional systems against objective data as well as for their subjective plausibility. Between the philosopher of mind and the scientific psychologist, there is the difference of putting the question to nature. Even when our theory seems plausible and so ingenious that it deserves to be true, we are conditioned to follow the Cromwellian dictum (better than did the Lord Protector himself) to consider in the bowels of Christ that we may be wrong.

But I feel that in our determination to maintain this difference we have gone too far. In our holy determination to confront reality and put our theory to the test of nature, we have plunged through reality, like Alice through the mirror, into a never-never land in which we contemplate not life but data. All too often the scientific psychologist is observing not mind or behavior but summed data and computer printout. He is thus a self-incarcerated prisoner in a platonic cave, where he has placed himself with his back to the outside world, watching its shadows on the walls. There may be a time to watch shadows but not to the exclusion of the real thing.

Perhaps Piaget should be held up as a role model here, as an inspiring example of how a creative mind can be guided in theorizing by direct confrontation with empirical reality. Piaget's close observation of how the developing human mind grapples with carefully devised problems was much more conducive to his interesting theorizing than would have been either the armchair philosopher's test of subjective plausibility or the scientific entrepreneur's massive project in which assistants bring him computer printout, inches thick.

The young student typically enters graduate study wanting to do just what we are proposing, that is, to engage in a direct confrontation with reality. All too often, it is our graduate programs which distract him with shadows. Either by falling into the hands of the humanists, he is diverted into subjectivism and twice-removed scholarly studies of what other subjectivists have said; or, if he falls under the influence of scientific psychologists, he becomes preoccupied with twice-removed sanitized data in the form of computer printout. I am urging that we restructure our graduate programs somewhat to keep the novice's eye on the real rather than distracting and obscuring his view behind a wall of data.

Koan 4: To See the Future in the Present, Find the Present in the Past

One idea whose time has come in social psychology is the accumulation of social data archives. Leaders of both the social science and the political establishments have recognized that we need a quality-of-life index (based perhaps on trace data, social records, self-reports obtained through survey research, etc.). Such social archives will also include data on factors which might affect subjective happiness, and analyses will be done to tease out the complex interrelations among these important variables. The need for such archives is adequately recognized; the interest and advocacy may even have outrun the talent, energy, and funds needed to assemble them.

In this growing interest in social data archives, one essential feature has been neglected, namely, the importance of obtaining time series data on the variables. While it will be useful to have contemporaneous data on a wide variety of social, economic, and psychological variables, the full exploitation of these data becomes possible only when we have recorded them at several successive points in time. Likewise, while a nationwide survey of subjective feelings and attitudes is quite useful for its demographic breakdowns at one point in time, the value of such a social survey becomes magnified many times when we have it repeated at successive points in history. It is only when we have the time series provided by a reconstructed or preplanned longitudinal study that we can apply the powerful methodology of time series analyses which allow us to reduce the complexity of the data and identify causality.

Hence, my fourth koan emphasizes the usefulness of collecting and using social data archives but adds that we should collect data on these variables not only at a single contemporaneous point in time, but also that we should set up a time series by reconstructing measures of the variables from the recent and distant past and prospectively by repeated surveys into the future.

Koan 5: The New Methodology Where Correlation Can Indicate Causation

If we agree that the simple linear sequence model has outlived its usefulness for guiding our theorizing about cognitive and social systems, then we must also grant that the laboratory manipulational experiment should not be the standard method for testing psychological hypotheses. But most graduate programs and most of the published studies (Higbee & Wells, 1972) focus disproportionately on descriptive and inferential statistics appropriate mainly to the linear models from the recent paradigm. The methods taught and used are characterized by obsolescent procedures, such as rigorous distinction between dependent and independent variables, two-variable or few-variable designs, an assumption of continuous variables, the setting of equal numbers and equal intervals, etc.

It seems to me that we should revise the methodology curriculum of our graduate programs and our research practice so as to make us better able to cope with the dirty data of the real world, where the intervals cannot be preset equally, where the subjects cannot be assigned randomly and in the same number, and where continuous measures and normal distributions typically cannot be obtained. In previous writings in recent years, I have called attention to advances in these directions which I mention here (McGuire, 1967, 1969), and Campbell (1969) has been in the forefront in devising, assembling, and using such procedures.

Our graduate programs should call the student's attention to new sources of social data, such as archives conveniently storing information from public opinion surveys, and to nonreactive measures of the unobtrusive trace type discussed by Webb and his colleagues.

Our students should also be acquainted with the newer analytic methods that make more possible the reduction of the complex natural field to a manageable number of underlying variables whose interrelations can be determined. To this end, we and our students must have the opportunity to master new techniques for scaling qualitative data, new methods of multivariate analysis, such as those devised by Shepard and others, and the use of time series causal analyses like the cross-lag panel design. More training is also needed in computer simulation and techniques of parameter estimation.

Mastery of these techniques will not be easy. Because we older researchers have already mastered difficult techniques which have served us well, we naturally look upon this retooling task with something less than enthusiasm. We have worked hard and endured much; how much more can be asked of us? But however we answer that question regarding our obligation to master these techniques ourselves, we owe it to our students to make the newer techniques available to those who wish it, rather than requiring all students to preoccupy themselves with the old techniques which have served us so well in reaching the point from which our students must now proceed.

Koan 6: The Riches of Poverty

The industrial countries, where the great bulk of psychological research is conducted, have in the past couple of years suffered economic growing pains which, if they have not quite reduced the amount of funds available for scientific research, at least have reduced the rate at which these funds have been growing. In the United States, at least, the last couple of years have been ones of worry about leveling scientific budgets. It is my feeling that the worry exceeds the actuality. In the United States' situation, psychology has in fact suffered very little as compared with our sister sciences. As an irrepressible optimist I am of the opinion that not only will this privileged position of psychology continue but also that the budgetary retrenchment in the other fields of science is only a temporary one and that, in the long run, the social investment in scientific research will resume a healthy, if not exuberant, rate of growth. I recognize that this optimism on my part will do little to cheer scientists whose own research programs have been hard hit by the financial cuts. To my prediction that in the long run social investment in science will grow again after this temporary recession, they might point out (like Keynes) that in the long run we shall all be dead.

I persist in my Dr. Pangloss optimism that things are going to turn out well and even engage in gallows humor by saying that what psychological research has needed is a good depression. I do feel that during the recent period of affluence when we in the United States could obtain government funds for psychological research simply by asking, we did develop some fat, some bad habits, and some distorted priorities which should now be corrected. While we could have made these corrections without enforced poverty, at least we can make a virtue of necessity by using this time of budgetary retrenchment to cut out some of the waste and distraction so that we shall emerge from this period of retrenchment stronger than we entered it.

The days of easy research money sometimes induced frenzies of expensive and exhausting activity. We hired many people to help us, often having to dip into less creative populations, and to keep them employed the easiest thing to do was to have them continue doing pretty much what we had already done, resulting in a stereotyping of research and a repetitious output. It tended to result in the collection of more data of the same type and subjecting it to the same kinds of analyses as in the past. It also motivated us to churn out one little study after another, to the neglect of the more solitary and reflective intellectual activity of integrating all the isolated findings into more meaningful big pictures.

Affluence has also produced the complex research project which has removed us from reality into the realm of data as I discussed in Koan 3. The affluent senior researcher often carried out his work through graduate assistants and research associates, who, in turn, often have the actual observations done by parapsychological technicians or hourly help, and the data they collect go to cardpunchers who feed them into computers, whose output goes back to the research associate, who might call the more meaningful outcome to the attention of the senior researcher, who is too busy meeting the payrolls to control the form of the printout or look diligently through it when it arrives. A cutback in research funds might in some cases divert these assistants into more productive and satisfying work while freeing the creative senior researcher from wasting his efforts on meeting the payroll rather than observing the phenomena.

I am urging here, then, that if the budgetary cutbacks continue instead of running ever faster on the Big-Science treadmill, we make the best of the bad bargain by changing our research organization, our mode of working, and our priorities. I would suggest that rather than fighting for a bigger slice of the diminishing financial pie, we redirect our efforts somewhat. We should rediscover the gratification of personally observing the phenomena ourselves and experiencing the relief of not having to administer our research empire. Also, I think we should spend a greater portion of our time trying to interpret and integrate the empirical relationships that have been turned up by the recent deluge of studies, rather than simply adding new, undigested relationships to the existing pile.

Koan 7: The Opposite of a Great Truth is Also True

What I have been prescribing above is not a simple, coherent list. A number of my urgings would pull the field in opposite directions. For example, Koan 1 urges that our methodology courses place more emphasis on the creative hypothesis-forming aspect of research even at the cost of less attention to the critical, hypothesis-testing aspect, but then in Koan 5 I urged that we, or at least our students, master a whole new pattern of hypothesis-testing procedures. Again, Koan 3 urges that we observe concrete phenomena rather than abstract data, but Koan 4 favors assembling social data archives that would reduce concrete historical events to abstract numbers. My prescriptions admittedly ride off in opposite directions, but let us remember that "consistency is the hobgoblin of little minds."

That my attempt to discuss ways in which our current psychological research enterprise could be improved has led me in opposite directions does not terribly disconcert me. I remember that Bohr has written, "There are trivial truths and great truths. The opposite of a trivial truth is plainly false. The opposite of a great truth is also true." The same paradox has appealed to thinkers of East and

West alike since Sikh sacred writings advise that if any two passages in that scripture contradict one another, then both are true. The urging at the same time of seemingly opposed courses is not necessarily false. It should be recognized that I have been giving mini-directives which are only a few parts of the total system which our psychological research and research training should involve. Indeed, I have specified only a few components of such a total research program. Any adequate synthesis of a total program must be expected to contain theses and antitheses.

I have asserted that social psychology is currently passing through a period of more than usual uneasiness, an uneasiness which is felt even more by researchers inside the field than by outside observers. I have tried to analyze and describe the sources of this uneasiness as it is felt at various levels of depth. I have also described a few of the undercurrents which I believe will, or at any rate should, be part of the wave of the future which will eventuate in a new paradigm which will lead us to further successes, after it replaces the recent paradigm which has served us well but shows signs of obsolescence.

A time of troubles like the present one is a worrisome period in which to work, but it is also an exciting period. It is a time of contention when everything is questioned, when it sometimes seems that "the best lack all conviction, while the worst are full of passionate intensity." It may seem that this is the day of the assassin, but remember that "it is he devours death, mocks mutability, has heart to make an end, keeps nature new." These are the times when the "rough beast, its hour come round at last, slouches toward Bethlehem to be born." Ours is a dangerous period, when the stakes have been raised, when nothing seems certain but everything seems possible.

I began this talk by describing the proud and placid social psychology of a half-dozen years back, just before the bell tolled, as suggesting Buson's beautiful sleeping butterfly. I close by drawing upon his disciple, the angry young man Shiki, for a related but dynamically different image of the new social psychology which is struggling to be born. Shiki wrote a variant on Buson's haiku as follows:

Tsurigane-ni
Tomarite hikaru
Hotaru kana.

Or,

On a temple bell
Waiting, glittering,
A firefly.

REFERENCES

CAMPBELL, D. T. Reforms as experiments. *American Psychologist,* 1969, **24**, 409–429.

FRIED, S. B., GUMPPER, D. C., & ALLEN, J. C. Ten years of social psychology: Is there a growing commitment to field research? *American Psychologist,* 1973, **28**, 155–156.

HIGBEE, K. L., & WELLS, M. G. Some research trends in social psychology during the 1960s. *American Psychologist,* 1972, **27**, 963–966.

MCGUIRE, W. J. Learning theory and social psychology. In O. Klineberg & R. Christie (Eds.), *Perspectives in social psychology.* New York: Holt, Rinehart & Winston, 1965.

MCGUIRE, W. J. Some impending reorientations in social psychology. *Journal of Experimental Social Psychology,* 1967, **3**, 124–139.

MCGUIRE, W. J. Theory-oriented research in natural settings: The best of both worlds for social psychology. In M. Sherif & C. Sherif (Eds.), *Interdisciplinary relationships in the social sciences.* Chicago: Aldine, 1969.

SEARS, D. O., & ABELES, R. P. Attitudes and opinions. *Annual Review of Psychology,* 1969, **20**, 253–288.

(Received December 1, 1972)