Mea Culpas and Lamentations: Sir Francis, Sir Isaac, and "The Slow Progress of Soft Psychology"

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INTRODUCTION

The first international conference ever entirely devoted to the study of interpersonal relationships has presented us with an extraordinarily rich array of food for thought—so rich, in fact, that some of us, like the man in the Alka-Seltzer commercial who groans "I ate the whole thing!" may already be appreciating the limitations of the intellectual digestive system. Thus, in anticipation of successfully providing more than even gargantuan appetites can comfortably accommodate, conference organizers often arrange to conclude their procession of offerings with an antidote of sorts. Customarily, this takes the form of changing the focus from the present to the future by enjoining the final speaker to step back from the banquet to address the twin questions "Well, now where are we?" and, perhaps more importantly, "What lies ahead?"

In addition to providing perspective, it is eminently useful to have the answers to these questions. Those who know where they are can navigate more swiftly and safely than those who do not. These questions are also especially appropriate for those of us who have been present at the birth of the science of relationships, because cartographers of science have yet to formally position us on their maps. If asked about us, in fact, they would probably place us somewhere in those unknown and mysterious regions ancient map makers designated with pictures of serpents and dragons and the legend "Here Be Monsters!" Here be monsters, indeed. Since they most certainly inhabit the territory we aspire to occupy, I take them as the theme of my concluding comments.
A SCIENCE OF RELATIONSHIPS: "WHERE ARE WE?"
AND "WHAT LIES AHEAD?"

Where is this territory? In a general sense, this is an easy question to answer. The emerging science of relationships lies on the fringes of several of the social and behavioral sciences, including (and especially) psychology and sociology. Within my own discipline, for example, a strong interest in relationships is emerging from social, developmental, and clinical psychology, or, from what are often regarded as the "softest" areas of psychology's terrain. The nascent science of relationships, then, is clearly located on the margins of the softer areas of what are often themselves considered to be the "soft sciences." That is where we undeniably are.

The answer to the second question "What lies ahead?" is virtually defined by the answer to the first. That is, given our geographical location, we have no reason to expect that the problems that have plagued the soft sciences as a whole, and their softer portions especially acutely, will not carry over to plague the study of human relationships. These old familiar problems, in fact, are likely to assume frightening proportions as we proceed in the relationship area.

Before amplifying this forecast of toil and trouble, it should perhaps be quickly acknowledged that, whatever lies ahead, the relationship domain is eminently worthy of capture. I simply take it as a given that we are all keenly aware of the necessity of an interdisciplinary science of relationships and of the enormous contribution such a science could make to virtually all of the social, behavioral, and health sciences, and thus ultimately to human welfare. It is not, then, our future glories that need discussion, but rather some of the troubles that lie ahead. These, as I have indicated, are virtually dictated by our position in a kind of scientific "no-man's land."

Specifically, our location on the fringes of the soft sciences means that at our back (and not infrequently on our back, as I want to discuss) is the traditional conception of scientific activity and, most importantly, the traditional standards against which the performance of any scientific enterprise is assessed. In front of us, indeed surrounding us on the other three sides, lies our restless and impatient public constituency—because, of course, to be located on the margins of the soft sciences is also to be positioned deep into public territory. Each of us knows all too well the problems, distractions, and vulnerabilities this entails, but these problems for a science of relationships are exacerbated by the fact that the public's appetite for the answers to a thousand cosmic questions about relationships can only be described as voracious, impatient, and unremitting.

My thesis, then, is simply that it is virtually inevitable that the infant science of relationships will get caught in heavy crossfire between the descendents of Sir Francis Bacon and the man on the street who happens to be suffering from his second divorce and wants us not only to tell him why but what he can do to avoid such disasters in the future. I wish to elaborate the nature of this crossfire,

because I suspect that many of the ambushes we in the soft sciences have experienced in the past would have been less successful had we had a clearer understanding of where hostilities were likely to come from and why. At the very least, if our detractors had not so often had the advantage of surprise, the number of self-inflicted wounds their attacks have occasioned might have been reduced.

Among these wounds I count the periodic "dark nights of the soul" that have characterized almost all of the disciplines from which a science of relationships is emerging. Within my own field of experimental social psychology, lamentations about the state of the discipline began to surface at least 15 years ago (dating these from the publication of Ring's [1967] paper about the "frivolity" of social psychology). If the ensuing so-called "crisis of confidence" in social psychology now seems to have settled down to the level of a low grade fever, it remains, nonetheless, a persistent and nagging one, as is illustrated by an article that recently appeared in one of the new European Monographs in Social Psychology series. In the first paragraph of the paper (Potter, 1982), there appeared a footnote; turning back to the end pages, I found the author's solemn caveat: ""Throughout the paper I assume that social psychology is a science" [p. 47]!

Sociologists who make social structural assumptions about the bases for social behavior have also had their doubts in recent times (e.g., Burgess & Bushell, 1969; Stryker, 1977). Ironically (considering the source of the doubts that plague the psychological social psychologists these days), some of them view, as Blank (1982) observes: "... experimentation and an individual orientation [as] essential palliatives to their own type's rigid reliance on extrapersonal, social structural explanations based in correlational methods of data analysis" [p. 226].

Clinical psychology and counseling psychology have not fared any better, which brings me to the confession that I poached both my title and the springboard for my comments from my colleague Paul Meehl and his 1978 Journal of Clinical and Counseling Psychology paper entitled "Theoretical Risks & Tabular Asterisks: Sir Karl, Sir Ronald, and the Slow Progress of Soft Psychology." Meehl's thesis (1978) is a familiar one. It is that theory and research in the soft sciences in general, and in the soft areas of psychology in particular, lack the cumulative character of scientific knowledge. He states:

I consider it unnecessary to persuade you that most so-called "theories" in the soft areas of psychology (clinical, counseling, social, personality, community, and school psychology) are scientifically unimpressive and technologically worthless. . . . In the developed sciences, theories tend either to become widely accepted and built into the larger edifice of well-tested human knowledge or else they suffer destruction in the face of recalcitrant facts and are abandoned. . . . But in fields like personology and social psychology, this seems not to happen. There is a period of enthusiasm about a new theory, a period of attempted application to several fact domains, a period of disillusionment as the negative data come in, a growing bafflement about inconsistent and unexplicable empirical results, multiple resort to ad hoc excuses, and then finally people just sort of lose interest in the thing and pursue other endeavors [pp. 806-807].
Meehl goes on to say:

I do not think that there is any dispute among psychologists familiar with the history of the other sciences. It is simply a sad fact that in soft psychology theories rise and decline, come and go, more as a function of baffled boredom than anything else; and the enterprise shows a disturbing absence of that cumulative character that is so impressive in disciplines like astronomy, molecular biology, and genetics [p. 807].

The major point of Meehl’s paper is that our failure to subject our theories to grave danger of refutation (hence the reference to Sir Karl Popper) stems importantly from our reliance on null hypothesis tests of statistical significance (hence the reference to Sir Ronald Fisher). It is not, however, this argument that I wish to pursue; rather, I wish to focus on his theme that there is something desperately wrong with soft psychology.

That theme, of course, we hear in a hundred different songs sung both within science, as Meehl’s paper illustrates, as well as without. To illustrate the latter, let me give an example of the public version of the concern that the study of human behavior lacks a cumulative character and the conclusion that there is, therefore, something wrong with us. In The New York Times of April 30, 1982, there appeared an editorial based on an article in Psychology Today in which several psychologists were asked to identify “the most significant work in psychology over the last decade and a half.” To quote the editorial: “The results are astonishing: it would seem that there has been none.” The editor based his conclusion on the fact that one psychologist cited teaching apes how to talk as the most significant advance, while another cited the failure of apes to learn how to talk as the most significant, yet another chose something else, and some persons asked couldn’t think of anything. The editorial concludes:

The subheadline for the editorial was: “If This is Consensus, Psychology Can’t Be Much of a Science.”

THE DIFFICULTY OF "SCIENTIZING" THE SOFT SCIENCES . . . AND THE USUAL CONCLUSION

Leaving our public for a moment (avidly reading their Psychology Today), let us return to Meehl’s (1978) paper, for he goes on to observe that, lest anyone think he is unjustly beating up on psychology, he recognizes that “human psychology is hard to scientize” [p. 807]. That word “scientize” doesn’t appear in my dictionary, but when Meehl asserts that human psychology is hard to scientize, I take it that he does not mean that it is hard to make systematic and interpersonally verifiable observations of human behavior, or even that it is all that hard to make probabilistic predictions about the likelihood of certain of those behaviors occurring in the future. Rather, I have little doubt that Meehl intended the word “scientize” to refer to science with a capital “S,” implicitly preceded by the adjective “hard” with a capital “H”; or, in other words, to refer to that conception of science that is drummed into the head of every school child, that we ourselves absorbed as students, and that our graduate students in the social and behavioral sciences continue to absorb today.

We are speaking, of course, of the legacy of Sir Francis Bacon, Sir Isaac Newton, and René Descartes (who, had he been English instead of French, undoubtedly would have been knighted and thus could have been included in my title where he very much deserves to belong). The word “scientize,” then, clearly refers to that model of science first provided by classical physics and its legendary triumphs, and to the Baconian—Carissian—Newtonian view of the world it embodies. Thus, by the assertion that “human psychology is hard to scientize,” and by his assumptions about the “slow progress of soft psychology,” I take Meehl to mean that despite almost a century of concerted endeavor, we in psychology, along with our brethren in the other soft sciences, have yet to come up with anything faintly resembling classical physics—or astronomy or molecular biology—or, in other words, we have yet to come up with anything that satisfies most people’s notions of what a science ought to look like.

Science with a Capital “S”

We are all familiar with this conception of science that most everyone, within science and without, pretty much agrees we don’t measure up to. The Cartesian—Newtonian model of science is outlined in a hundred philosophy of science texts. It also has been recently and engagingly discussed by the physicist Fritjof Capra (1982) (who also, and not incidentally to ourselves, chronicles the trauma that subatomic physicists have experienced in surmounting that view in treating material phenomena that defy its assumptions). Relying on Capra’s characterization, I should like to quickly recall some of its features to mind, since it is on this stage that a science of relationships must perform and it is by these standards that our performance inevitably will be judged.

From Bacon, of course, we were given the empirical method of science, or the inductive procedure of making experiments, drawing general conclusions from those experiments, and testing those conclusions through further experimentation. In addition to our passion for scientific experimentation, Capra (1982) observes that we got something else from Sir Francis:
The "Baconian spirit" profoundly changed the nature and purpose of the scientific quest. From the time of the ancients and the ancient Greek and Roman philosophers, the goal of science has been to discover the nature of the universe and the human condition. In contrast, the scientific method of the 17th century, founded on the work of Francis Bacon, emphasized the importance of observation and experimentation. This approach to science was characterized by a focus on the empirical, the measurable, and the observable, rather than on the speculative or the hypothetical.

From Bacon's time, science has evolved into a complex and diverse field, encompassing a wide range of disciplines and approaches. The scientific method has been refined and adapted to new challenges and new knowledge. However, the core principles of observation, experimentation, and hypothesis testing remain at the heart of scientific inquiry.

The "Baconian spirit" has continued to influence the way we think about science and its role in society. It has shaped the way we approach problems, from the development of new technologies to the understanding of complex systems. The scientific method has proven to be a powerful tool for solving problems and advancing knowledge, and it remains a cornerstone of modern science.
There are still other intrinsic difficulties with our subject matter. There is the problem of "feedback loops" that may be so complex that they simply cannot be decomposed, and then as all of us who have thought about relationships know, there is the problem of "random walk." With respect to this last, Meehl comments that "There is a widespread and understandable tendency to assume that the class of less-probable outcomes, given constancy of other classes of causally efficacious variables, should, in principle, be explicable by detecting a class of systematic input differences." But Meehl notes that there is an alternative possibility:

At several points that are individually minor but collectively critical, it is almost a "chance" affair whether the patient does A or not A, whether [for example] his girlfriend says she will or will not go out with him on a certain evening, or whether he happens to hit it off with the ophthalmologist that he consults about some particular visual disturbances that are making him anxious about becoming blind, and the like. If one twin becomes psychotic at the end of such a random walk, it is possible that he was suffering from what was only, so to speak, "bad luck"—not a concept that appears in any standard list of biological and social nuisance variables! [p. 811]

Even presuming that the relationship expert is able to identify the random walk that a couple has taken to disaster, one can imagine the reaction that would greet the therapist's scientific opinion that they had simply, and irrevocably, suffered from "bad luck."

Meehl goes on to list several other "intrinsic difficulties," but the one I wish to highlight, because it is the one that I believe to be at the heart of many of our other troubles, is that which Meehl calls "the sheer number of variables" problem. As he discusses, the number of variables we must deal with is large from several different viewpoints. It is large and heterogeneous on the causal side, both in terms of immediate precipitating factors and in terms of historical causal influences, and it is large on the effect side as well. Meehl goes on to say:

It should be noted that this matter of sheer number of variables would not be so important (except as a contributor to residual 'random variation' in various kinds of outcomes) if they were each small contributors and independent, like the sources of error in the scattering of shots at a target in classical theory of errors. But in psychology this is not typically the situation. Rather, the variables, although large in number, are each nuisance variables that carry a significant amount of weight, interact with each other, and contribute to ideographic development via the divergent causality mode [p. 812].

The fact that those who think about relationship phenomena are all too familiar with the "sheer number of variables" problem has been well documented. People who focus on relationships understand that they are dealing with a very large and causally complex system. For example, in our own conceptualization of relationships (Kelley, Berscheid, Christensen, Harvey, Huston, Levinger, McClintock, Peplau, & Peterson, 1983), we classify the relevant causal factors into, first, those primarily associated with the individual; second, those primarily associated with the partner; third, those associated with the interaction of these two sets of variables with one another; fourth, variables associated with the social environment in which the relationship is embedded; fifth, variables associated with the physical environment in which the relationship is located; and, finally, of course, those represented by the interactions that take place between all of these types of variables.

This is a monster of a system, both in sheer size and in potential causal complexity, and it is, perhaps, an occasion for self-congratulation that relationship theorists and researchers have shown no unwillingness to face the fact that this is, indeed, the causal domain of relationship phenomena. But recognizing the overwhelming intrinsic difficulties of our subject matter and conquering them are something else, especially when the criteria for determining what constitutes a successful conquest are set by the traditional standards of science.

IS THERE SCIENTIFIC LIFE AFTER REDUCTIONISM?

The essential problem that confronts us, then, is that the methods and standards of our scientific activity have been set with subject matter that seems to be different from our own in at least three critical ways: First, the systems that surrounded the phenomena with which the classical physicists were concerned often had the property that the interactions between the various subparts of the system were weak or nonexistent; second, the relationship describing the behavior of these sub-parts was often linear (see von Bertalanffy, 1980, p. 19); and, third, the system itself was often much smaller. Our knowledge that it is highly unlikely that the system in which relationship phenomena are embedded possesses these properties immediately robs us of the intrinsic intellectual comfort of the traditional reductionistic approach whereby the larger system is broken into smaller and more manageable subsets of variables whose relationships with each other are capable of being grasped by the human mind. That is, we are aware that the phenomena in which we are interested are surrounded by a large number of interdependent variables, which collectively and by the fact of their interdependence, constitute a system, and so we are also painfully aware that the reductionistic analytical method smashes that system into a thousand different fragments. And so, we know that reducing what is essentially an "open" system (and "open" if only by virtue of the sheer number of variables involved) down to a much smaller and manageable system may violently assault its integrity and, thus, compromise into uselessness anything we subsequently think we have discovered about it.
But our discomfort with the reductionistic method goes even deeper. At the
time we are reducing the system down to manageable size for study, we are
usually aware that the rules by which we are reducing it are pretty arbitrary, at
least in terms of the properties of the system as a whole. Usually, our decision
rules are not even stated. Although I know of no such formal set of rules, it
seems conceivable that some reasonable, if loose, guidelines could be con-
structed. For example, the values of some variables in the system may stay so
universally constant that they may be, for all practical purposes, ignored. Other
variables may interact with certain others in the system only weakly, producing
little diversity or magnitude of effect, and so possibly they too may be safely
excluded. Such arguments for exclusion are, however, rarely mustered and it's
easy to understand why: a great deal of knowledge is required to reduce a system
down intelligently for careful scrutiny of its subparts. Furthermore, even if both
we and our grandmother identify a variable as extremely central to the system, it
may be that for ethical or technological reasons (and usually both), we cannot
systematically examine its role. In any event, and for a wide variety of reasons,
we in the social sciences often just pluck out of the larger system a couple of
"do-able" variables that strike our fancy and hope for the best.

Unfortunately, the inevitable penalty for excluding from one's system vari-
able that everyone, including the man in the street, knows (intuitively if not
experimentally) belong in the system (on the basis that they importantly affect
the phenomena in question) is to have one's findings regarded as irrelevant, imprac-
tical, uninteresting, academic, and so forth. If one doubts that this is the penalty
for positing an arbitrarily constricted system for the phenomenon of interest, one
needs only to glance at the criticisms that have been chronically lodged against
much theory and research in social psychology in recent years. The lion's share
of that criticism goes something like this: "You may have, in your laboratory
and with your experimentation, shown that variable X influences phenomenon
Y, but this tells us little or nothing of interest about phenomenon Y as it appears
in nature because, first, you have wholly ignored the role of the social and
physical context in which you manipulated X (usually a laboratory experiment in
an academic setting) and the interaction of these contextual variables with X to
produce the Y effects; in other words, you have made the patently false assump-
tion that you may safely ignore the context in which a behavior is embedded in
your causal analysis of it. Furthermore (and especially if the context was a
laboratory experiment) X may have been manipulated in the presence of var-
iables that appear nowhere else in nature—and, in fact, X itself may only rarely
appear in nature and so, of all the variables that are believed to importantly affect
Y, X may be the least important of these." And so on the argument goes. The
essence of the criticism, then, is that: "The system in which phenomenon Y is
actually embedded in nature is most probably much larger, and possibly quite
different, than the system defined by your theory or your experiment, and,
therefore, your findings are of questionable interest; quite possibly, they are
irrelevant."

Regrettably, often the only response to that attack can be a well-memorized
recital of the reductionistic catechism that one can profitably study parts of the
system in isolation from the remainder, even though the remainder is suspected
to importantly affect the smaller subportion under scrutiny. But our problem in
the soft sciences, which is surely a major factor in our crises of confidence, is
that we have become too sophisticated to wholeheartedly believe the reduc-
tionistic dogma any longer.

Relationship theorists and researchers, especially, seem to be reluctant to
make the comfortable reductionistic assumptions. In fact, much of the interest
in relationships that is emerging is the result of dissatisfaction with these assump-
tions that continue to underlie most theory and research in the sciences, both hard
and soft. An interest in relationships has grown from the recognition that most
human behavior takes place in a social context, often within the context of
relatively enduring relationships with other people, and that human behavior
simply cannot be well understood without reference to that context. And so, for
example, relationship researchers already seem to be taking such problems as
"emergent novelty" seriously, or, as Feigl puts it (1958, p. 415), "The impos-
sibility of the derivation of the laws of the complexes ('wholes') from the laws
that are sufficient to predict and explain the behavior of their constituents in
relative isolation." The problem of "equifinality" within a large system is also
increasingly recognized, or the fact that there may be many causal paths to the
same end state and so delineating one of these paths does not exhaust the causal
possibilities.

IS THE "SYSTEMS" APPROACH OUR SALVATION?

Thus, in attempting to avoid a reductionistic approach to the problem of rela-
tionships, it appears that some theorists and investigators are beginning to em-
brace what has been termed the "systems" approach. At least theoretically if not
in practice, this approach tries to preserve the integrity of a highly interdepen-
dent system of variables for purposes of understanding it. This, in the social and
behavioral sciences, virtually requires observation of the system in vivo and in situ,
since by virtue of its size, as well as ethical and technological problems, it
cannot be duplicated in the laboratory.

It is not surprising that relationship researchers should look eagerly toward the
systems approach, for it is very much in the Zeitgeist. It is, in fact, increasingly
hailed as the salvation of the social and behavioral sciences as the best candidate
we have for that new "paradigm" Kuhn (1962) was talking about, a paradigm
that, as our faith in reductionism grows weaker, many of us feel we desperately
need. Some even regard the systems approach as the royal road to a new conception of all science. Within these growing ranks are physicists (e.g., Capra, 1982), biologists (e.g., von Bertalanffy, 1980) and others within the hard sciences, as well as those in the soft, who believe we must find an alternative to be Cartesian–Newtonian model (e.g., see Bevan, 1982a; Gergen, 1982). Perhaps most relevant to those of us interested in relationships is the burgeoning field of marital and family therapy from which much impetus for a science of relationships has been generated, and which itself has largely taken a systems position (see Berscheid & Peplau, 1983).

Unfortunately, the potential benefits of the systems approach strike me as discouragingly modest. For example, a reading of von Bertalanffy (1980) left me with only two: First, there is the possibility that different systems share the same principles, so that what is learned about one system may be transferable to another. That, indeed, would be useful; if it were to be the case, for example, that all systems evolve towards complexity (or, to the contrary, that all systems evolve toward homogeneity), that would be valuable information. In advance, however, we can speculate that this is somewhat unlikely and, at the least, very difficult to ascertain.

The second benefit held out to us by the systems approach is what von Bertalanffy (1980) calls “explanation in principle” for large and complex systems. He gives us an example:

Theoretical economics is a highly developed system, presenting elaborate models for the processes in question. However, professors of economics, as a rule, are not millionaires. In other words, they can explain economic phenomena well “in principle” but they are not able to predict fluctuations in the stock market with respect to certain shares or dates. Explanation in principle, however, is better than none at all. If and when we are able to insert the necessary parameters, system-theoretical explanation “in principle” becomes a theory, similar in structure to those of physics [p. 36].

It is here, of course, that one becomes overcome with waves of uneasiness. “Explanation in principle” sounds dangerously like ad hoc explanation; it does not at all sound much like what Bacon and Descartes had in mind. Or, let me put this another way: In 1973 when Bill McGuire published his influential article, “The Yin and Yang of Progress in Social Psychology,” he offered some opinions about what he called the “Sources of the New Social Psychology.” He predicted that a “radically different” paradigm would emerge and proceeded to sketch “The Ultimate Shape of the New Paradigm.” For example:

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 [p. 450].

Although McGuire cautioned that he felt somewhat “uncomfortable” detailing what this radically different paradigm would look like, its outlines were clearly “systemic.” A few pages later in the same article, however, and in the context of encouraging ourselves and our students to think in terms of more complex models, he made two acute predictions. The first of these was that we would all probably “shy away from the mental strain of keeping in mind so many variables, so completely inter-related” [p. 452]. The second was that:

... 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 [p. 452].

In other words, in a large and complex system there are so many wild cards that it is possible for anyone with any imagination to come up with a royal flush of causal explanation—or an “explanation in principle”—every time. Predicting in advance what will happen so as to be able to control it, even assuming we have the ethical right and the technological means to do so, is, then, an entirely different matter with such a system. It is, perhaps, an impossibility.

This may be what Harold Rausch (1981) had in mind in his comments in his Contemporary Psychology review of Robyn Pennman’s book on Communication Processes and Relationships (1980), which takes a systems approach. After noting the popularity of the systems perspective within the discipline of marital and family therapy, Rausch also comments:

When it comes to research (as we usually think of research) on interaction, however, systems theory has had far less effect. How are investigators to conduct “hard-nosed,” scientific research when they are asked to reject what are assumed to be fundamental premises of science? Systems theory rejects the search for cause-effect relations; it is concerned rather with variety and constraints and with equivalences and multi-finality. Rejected are notions of independent and dependent variables; emphasis is on interdependence and organization. Rather than seeking basic elements and static entities, its focus is on contextual hierarchical structures and fluctuating processes. Instead of trying to discover laws of behavior, it attempts to understand human rules of action. Systems “theory” is far less a theory than an epistemological position—a different paradigm, to use the currently fashionable term. As an epistemology, it has major methodological implications, and these implications are disruptive to our usual notions of research design and procedure [pp. 752–753].

To that I add my own suspicion that as an epistemology, the systems perspective not only has “major methodological implications,” implications for the manner in which knowledge is obtained, but it also has implications for the “limits” (see Feigl, 1958) of knowledge, at least within our domain.
In any event, the systems approach seems to me to be more problematical than it appears at first glance. This may help explain why, despite the glowing words and enthusiasm, the visible evidence of such an approach in concrete research is a good deal less than one might expect. Its adoption presents, first of all, some very practical problems. With respect to descriptive analysis, for example, the investigator who truly takes the systems approach can easily drown in his or her data. Measurement of all the variables suspected to be of import over the time dimension required for later causal analysis can enervate and demoralize even the most ambitious investigator. And, with respect to causal analysis itself, neither our minds nor our statistics are equipped to deal with such a welter, particularly if the relationships between the variables are not relatively simple and linear. And then, of course, there is the deeper problem of lowering our epistemological aspirations to something like “explanation in principle” and what the consequences of that are likely to be. Leo Tolstoy provides us with an “explanation in principle” of marital infidelity in Anna Karenina; we ourselves aspired to something different.

In sum, it is not at all clear that there is scientific life after reductionism. The systems approach appears more to describe our problems than to provide a certain means of salvation from them.

THE FUNDAMENTAL HEADACHE A SCIENCE OF RELATIONSHIPS PRESENTS

I have tried to sketch the fundamental dilemma that will surely plague the development of a science of relationships, and it is the same hoary dilemma that has haunted the disciplines from which this science is emerging: We are caught forever between the proverbial rock and a hard place, between our knowledge that the phenomena we are trying to understand lie deeply embedded in a large and causally complex system and that, therefore, the convenient reductionistic faith that we can smash that system into a million fragments in pursuit of an understanding of the system is, for us, very difficult to maintain; and, yet, we often seem to have no choice other than to take a piece-meal approach for, first, we simply cannot cope very well with anything else and, second, we seem to be in danger of losing our legitimacy as a scientific enterprise if we cut our aspirations down to anything we can ever hope to meet.

If this is the problem, what is the answer? Having come this far, I regret to say that I have no answer, not even the glimmerings of one. I believe this dilemma to be fundamental in the true sense of the word. It admits to no easy remedy; perhaps no remedy at all. If one truly confronts what Meehl calls the terrible “intrinsic difficulties” of our subject matter, and if one also strongly suspects that the Cartesian–Newtonian epistemology is, in some important degree, un-

suited to it, then that means that to work in the relationship domain is to forever bear the tensions and ambiguities and discomforts inherent in this dilemma.

SOME SUGGESTED “ASPIRINS” WE MIGHT TAKE EN ROUTE TO A SCIENCE OF RELATIONSHIPS

Nevertheless, I think there are some things we could do that could go a long way to ease our discomfort as we proceed, and I should like to quickly list them:

First, I think we might recognize, and lead others to recognize, those intrinsic difficulties in our subject matter more than we have. These difficulties are no mere academic footnote to our scientific enterprise; they are the screaming headlines. That we have not kept them in the forefront, I attribute to at least two factors. The first is our suspicion that if we looked these bogies square in the face, we would die of fright or otherwise lose our confidence that we in the social and behavioral sciences can ever accomplish anything useful. Our past record, which is nowhere near as dismal as some seem to think, should reassure us on that score (e.g., Kiesler, 1982; Tornatzky & Solomon, 1982). For social psychologists, in particular, the successful application of basic theory and research to problems in medicine and law, as well as business, ought to be heartening.

The second major factor responsible for our not keeping the intrinsic difficulties of our subject matter in mind is surely the pervasiveness of reductionistic dogma, which minimizes and denies these difficulties. It does so not only through its emphasis on the analytic method but also through another of its facets, which is that all phenomena can ultimately be reduced to physical phenomena which, in turn, implies that the epistemology suited to the latter is equally well suited to the former.

In any event and for whatever reasons, it is clear that we tend to forget that our job is infinitely more difficult than that which confronted the classical physicists, and that to characterize our progress as “slow” is to wholly overlook the differences between our subject matter and theirs. The truth of the matter (from the perspective of God and Omniscient Jones) may well be that our progress has been incredibly rapid. But when have we heard anyone say ‘that’? Despite the fact that ‘... none of us yet knows what the proper time scale for significant progress in psychology may turn out to be’ (1982, p. 1313), as Bevan put it in his 1982 Presidential address to the American Psychological Association, few of us give ourselves the benefit of the doubt. However, from the fact that not even the most advanced of the soft sciences bears much of a resemblance to classical physics, it seems to me to be as reasonable to assume that we will never look like classical physics as it is to assume that such a science is our ultimate destination and that, therefore, our progress toward it has been slow.
Second, just how much a science of relationships ultimately will differ from the model of science we now accept depends on just how mean Mother Nature has been to us. That she has not been as generous as we hoped is clear. Nevertheless, it may be that certain portions of the system with which we are concerned are less interdependent than others, and so can be broken off in reductionistic fashion with good results. Identifying just exactly what subparts function relatively independently of the others is undoubtedly a matter of luck and art, as always. In any event, abandoning the reductionistic faith does not necessarily mean abandoning the analytic method; it does mean, however, that it cannot be usefully employed indiscriminately and with abandon—that some justification is necessary, and that justification ultimately resides, not in philosophical treatises and personal preferences as some current debates seem to imply, but in the relationship of that portion of the system under scrutiny to the remainder of the system (e.g., see Baddely, 1981; Jenkins, 1974, for an illustration of the impact of this problem on cognitive psychology). And that is, essentially, an empirical question to which the current concern with “ecological validity” and “multimethod” approaches in many areas of psychology is, of course, addressed.

Third, and as this suggests, while I doubt that the so-called “systems” approach is our salvation, I do think that there are real benefits to be had from keeping the systems perspective in the forefront of our thinking. Only by taking the larger perspective can we hope to identify the variables importantly implicated in the system and their relationships with one another. Or, to put it another way, people who take a systems approach are unlikely to spend decades of their research time and effort minutely scrutinizing the elephant’s toenail in the blind faith that it ultimately will tell them something important about the nature of elephants.

For example, when one adopts a systems view of a phenomenon, certain problems emerge as interesting and important and other problems are rendered insignificant. To illustrate from my own area of interest, emotion, it will be recalled that William James’ “sequence problem” dominated emotion theory and research for many years. The problem was to ascertain the temporal causal order in which the various elements of emotional experience occurred. Did the perception of the “exciting” stimulus event occur first, then the internal physiological events, and then the subjective experience of emotion, or was the order different? The sequence problem assumed that there is an immutable linear causal order, and, also, that finding out what it was would tell us something interesting about emotion. Contemporary emotion theorists, however, have concluded that all of these occur within a continuous feedback loop such that each is both stimulus and response to the others, and that untangling the loop, or laying out the precise order of these events in a specific instance, probably isn’t going to tell us anything of much importance about human emotion (e.g., see Candland, 1977). As this suggests, another benefit of the systems view is the likelihood that questions of process will receive more attention, and questions of static structure will be regarded as less interesting, than they traditionally have been.

There are still other benefits to be had from a systems perspective, and many of these have been nicely laid out in the writings of Kurt Lewin (see Deutsch, 1954). The fact that Lewin is also commonly regarded as the “founder” of experimental social psychology may help explain why experimental social psychologists could win the prize for having the most crises of confidence in social science within the shortest period of time. That is, Lewin’s “systems” perspective, which forever sensitized us to the context in which behavior is embedded, was injected early into our bloodstream along with the Cartesian–Newtonian epistemology to which it is, in many ways, hostile. It is not surprising, then, that these two elements sometimes attack each other as foreign bodies, with the result that American social psychologists periodically come down with a fever. The irony, of course, is that Lewin’s systems perspective can be traced to the work of Faraday and Maxwell in physics and their “field theory,” which itself ultimately forced physicists to do their own painful soul searching and to drop many of their own Cartesian–Newtonian assumptions. I think it instructive, however, that the physicists blamed God and the subject matter He gave them for their troubles. We have blamed ourselves and have concluded that there is something wrong with us—that if we were only smart enough, or had the right tools, or if only we could create that new paradigm Kuhn was talking about, we could proudly join the ranks of “real” scientists; but, until then, elaborate statements of mea culpa, both to our brethren in the hard sciences and to our public constituency, are in order.

Fourth, I think we might be more aware than we have of how we have been encouraged to take an apologetic posture by our interested spectators. With respect to the hard sciences, we all recognize our tendency to look over our shoulder at “big brother” to make sure that we are doing what is expected of us, but, as George Mandler (1982) recently observed, we should also be aware that this influence is a two-way street—that big brother has been looking over his shoulder at “little brother” and “little sister” to make sure that we stay within the decreed realm of what is proper and permissible. (And I resist my impulse here to suggest some of the ways this influence is wielded through the National Academy [see The APA Monitor, June, 1982] and also through federal research funding.)

With respect to our public constituency, their demands for apology are even clearer. But what often has not been recognized is that despite the intrinsic differences in subject matter that have made it difficult for us to even approach the Cartesian–Newtonian standards, the standards that the public measures us by are even higher than these. They are, in fact, an absurd caricature of the standards to which the material sciences are held.

Consider, for example, the daily fare of the clinical or the counseling psychologist. The man on the street wants to know whether, if he goes ahead and marries
elevate actuarial prediction to something more than a technique used by book-keepers in insurance companies. Especially, we might accept with grace the fact that our theories never have been (any more than are most scientific theories) designed to permit individual point predictions in naturalistic situations.

And, finally and most importantly, we might simply accept that the science of relationships we are attempting to develop can no more expect to meet the Cartesian–Newtonian criteria of successful performance than have psychology, sociology, economics, or other of the social and behavioral sciences. There is little doubt, however, that we will be measured against these standards. And when, and inevitably, we come up short, we can conclude that there is something wrong with us; we can issue official mea culpas and lamentations to all external spectators; internally, we can point accusing fingers at each other while simultaneously rushing off to seek the Holy Grail of resolution and absolution in the form of a "new paradigm"; and we can, as always, plead our youth, even though we are getting a little long in the tooth for that. But there is another alternative: We can stop to consider not only the difficulties of the task we undertake, but its importance to the human condition. And if we do that, I doubt we will feel the need to apologize to anyone.

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