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Social Psychology and Science: Some Lessons From Solomon Asch

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This article presents a methodological critique of the predominant research paradigms in modern social psychology, particularly social cognition, taking the approach of Solomon Asch as a more appropriate model. The critique has 2 parts. First, the dominant model of science in the field is appropriate only for a well-developed science, in which basic, real-world phenomena have been identified, important invariances in these phenomena have been documented, and appropriate model systems that capture the essence of these phenomena have been developed. These requirements are not met for most of the phenomena under study in social psychology. Second, the model of science in use is a caricature of the actual scientific process in well-developed sciences such as biology. Such research is often not model or even hypothesis driven, but rather relies on "informed curiosity" to motivate research. Descriptive studies are considered important and make up a substantial part of the literature, and there is less exclusive reliance on experiment. The two parts of the critique are documented by analysis of articles in appropriate psychology and biology journals. The author acknowledges that important and high quality work is currently being done in social psychology, but believes that the field has maladaptively narrowed the range of the phenomena and methodological approaches that it deems acceptable or optimal.

> Psychology appears to progress by removing the obstacles it has placed in its path.
> —attributed to William Stern

> In their anxiety to be scientific, students of psychology have often imitated the latest forms of sciences with a long history, while ignoring the steps these sciences took when they were young. They have, for example, striven to emulate the quantitative exactness of natural sciences without asking whether their own subject matter is always ripe for such treatment, failing to realize that one does not advance time by moving the hands of the clock. Because physicists cannot speak with stars or electric currents, psychologists have often been hesitant to speak to their human participants.
> —Solomon Asch (1952/1987, pp. xiv–xv)

> The temptation arises to allow techniques called scientific to govern thinking and to dictate the range of interest.
> —(Asch, 1952/1987, p. xv)

> Before we inquire into origins and functional relations, it is necessary to know the thing we are trying to explain.
> —(Asch, 1952/1987, p. 65)

At the middle of the 20th century, Solomon Asch, one of the great figures in the history of social psychology, set a standard for a context- and culture-sensitive scientific social psychology. His cultured and balanced analyses in his book, *Social Psychology* (1952/1987), and a few brilliant "experiments" are superb models for the art and science of social psychology. Asch's experimental ingenuity has been admirably developed, but his orientation to real-world phenomena and sensitivity to context have been largely ignored.

> If there must be principles of scientific method, then surely the first to claim our attention is that one should describe phenomena faithfully and allow them to guide the choice of problems and procedures. If social psychology is to make a contribution to human knowledge, if it is to do more than add footnotes to ideas developed in other fields, it must look freely at its phenomena and examine its foundations. (Asch, 1952/1987, p. xv)

> Most social acts have to be understood in their setting, and lose meaning if isolated. No error in thinking about

I thank Solomon Asch for setting the standard and for discussions of an early draft of this article, Richard Shweder for indicating to me a productive new direction for the field, and Clark McCauley for comments on the article.

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> social facts is more serious than the failure to see their place and function. (Asch, 1952/1987, p. 61)

> We cannot be true to a fragment of man if we are not true, in at least a rudimentary way, to man himself. (Asch, 1959, p. 368)

Asch himself was disturbed by the strong movement to gain rigor at the expense of context sensitivity and phenomenon orientation. He comments on his disappointment in an article in 1959: "But social psychology grows up in the shadow of general psychology" (p. 366). "It will be my contention that this dependence has been responsible for the neglect of some central questions and for a limited horizon" (p. 367). "One would not often suspect that we were talking of an organism capable of keeping or betraying faith with others, in whose history religious beliefs have played quite a part, who can cry out for justice" (p. 367).

In the new preface to the republished 1987 version of his classic 1952 book, Asch expresses some misgivings about the enterprise he played a major role in creating: "Clearly I was swimming, often without realizing it, against the current" (p. x). "Why do I sense, together with the current expansion, a shrinking of vision, an expansion of surface rather than depth, a failure of imagination?" (p. x). "Why is not social psychology more exciting, more human in the most usual sense of that term? To sum up, is this discipline perhaps on the wrong track?" (p. x).

As a psychologist with a background in biology who has recently come to the study of human social life, I have been struck by the prescience of Asch's social psychology and the methodological and phenomenological narrowness of the core of much modern social psychology. I had the great fortune to know Solomon Asch as my colleague at the University of Pennsylvania for the last 8 years before his retirement. Our connection continued postretirement, and about 8 years ago I engaged him in the enterprise of writing a methodological critique of modern social psychology. A year or so later, I completed the first draft of this article, which Asch read and generally approved. However, he felt that he was too weak to actively participate in this endeavor and suggested that I continue alone. Unfortunately, Solomon Asch's health continued to decline over the next year or so, resulting in his death in February of 1996. I dedicate this article to him.

I believe that social psychology, modeling itself in the mid-20th century primarily on the natural sciences and on sensory psychology, has concentrated on the advancement of a formal, precise, and experimental science. However, unlike the successful work in the natural sciences and sensory psychology, the work in social psychology has not been preceded by an extensive examination and collection of relevant phenomena and the description of universal or contingent invariances. In the more advanced sciences that social psychology would like to emulate, there is much more emphasis on phenomena and "description" than there is in social psychology, and there is less reliance on experiment. Such sciences, particularly the life sciences, also pay less attention to models and hypotheses and more attention to evidence as opposed to proof or "definitive" studies. Especially in studies of whole human beings in social situations in which contextual effects are numerous and the organism is complex, the collection of findings that unambiguously support hypotheses is extraordinarily difficult. One can reasonably look only for evidence in single research ventures, not proof. Indeed, the best hope we may have (as worked so successfully in the validation of the theory of evolution by natural selection and in most historical and archeological studies) is to accumulate flawed (ambiguous) evidence in large amounts and from many different sources and approaches. This is probably the only practical route to understanding *Homo sapiens* in a social context. With respect to the difficulties in producing definitive results in the social sciences, Funder (1996) formulated Funder's third law: "Something beats nothing, two times out of three" (p. 33). Cronbach (1986), in examining the research enterprise in the social sciences, came to conclusions that are related to those I express here.

I claim that as a result of a misinterpretation of the approach of the basic natural sciences and a focus on design, experiment, and certainty over relevance, reality, and durability, much of the current field of modern social psychology has an unnecessarily narrow focus that, among other things, (a) pays little attention to powerful cultural influences (though this has been changing in the last decade), (b) discourages the discovery of new phenomena and creativity (Wegner, 1992), (c) discourages the description of basic regularities in the social world, and (d) presents a rather narrow model of what is acceptable science to graduate students in the area.

I do not claim that the problems I point out are unique to social psychology. (Indeed, some of the same criticisms might be leveled against aspects of modern cognitive science, which itself serves now as a model for social psychology.) However, I believe that, at least among the disciplines in psychology, they are most salient in social psychology, particularly in social cognition. Further, I do not claim that the field is not progressing. To the contrary, I see social psychology as moving rapidly and cleverly along a path it has defined for itself, but a path that I feel is not optimal if the aim of social psychology is the scientific study of human social cognition and behavior. Almost the entire field is devoted to studying a modest subset of the domains of social life with a limited range of salient methodolo-

gies. Graduate student readers of the *Journal of Personality and Social Psychology* are being socialized to an overly narrow set of criteria for research.

In this article, I first document what I claim to be the rather narrow range of research approaches to social phenomena that characterize much of modern social psychology. I then briefly outline what I take to be the time-tested mode of progress in the natural sciences. I illustrate this mode with what are generally regarded as the two greatest advances of modern times in the life sciences: the theory of evolution and the molecular biology of the gene. Then, by analysis of journal articles in different areas of biology, I demonstrate the range of methods and approaches currently in use. I follow this with a brief discussion of some of the limitations of experimentation and the value of other research techniques. Next, I very briefly discuss why, in my view, a major part of social psychology has become prematurely formal and experimental. Then I present an illustrative parody of the model of research that currently dominates social cognition. Finally, I make some suggestions for the future.

Methods and Approaches in Modern Social Psychology

The domain of social psychology is, presumably, social behavior, social cognition, and affect in a social context, with a focus on individual humans as opposed to groups or institutions (to distinguish it from sociology). The aim is to gain understanding of the relevant events and processes. There are many possible methods, including examination of historical materials or literature, observation, participant observation, laboratory experiment, natural experiment, questionnaire/survey, and interview.

To determine the extent to which the field samples the possibilities, I examined Volume 66 (1994) of the premier journal in the field, the *Journal of Personality and Social Psychology.* Each article in the two social psychology sections (attitudes and social cognition, and interpersonal relations and group processes) was characterized according to a number of dichotomies. The results are presented in Table 1.

As is clear from the table, for attitudes and social cognition, the great majority of research involves undergraduate students from North America. This confirms earlier observations by Sears (1986), who noted a major shift in this direction between 1940 and 1980. Although gender is usually specified, no other important participant characteristics are usually specified. Religion and time of study (year or season) are never specified, and race and social class are specified in only one study. Even the location of the study is often not specified, although it can be inferred, in most cases, from the home institution of the authors. It is as if the experiments in question transcend time, location, culture, race, religion, and social class.

The methodological narrowness of the research is more clearly demonstrated in the attitudes and social cognition section than in the interpersonal and group processes section (Table 1). For attitudes and social cognition, 96% of studies use analyses of variance (ANOVAs) and hence ANOVA designs. The ANOVA design is very powerful, and it encourages the study of interactions. However, it also constrains the type of study one may do. It discourages the use of continuous independent variables and promotes a particular style of thinking about problems and experiments. None of the attitudes and cognition articles in Volume 66 use either observational or interview techniques, 92% are in the multiple study/experiment format, and a clear majority (69%) are introduced as tests of specific models

Table 1. *Features of Articles in* Journal of Personality and Social Psychology, *Volume 66, 1994*

	Attitudes & Social Cognition		Interpersonal Relations & Group Processes	
Feature	**Number**	**%**	**Number**	**%**
Number of Papers	26		18	
North American Undergraduate Participants	21	81	11	61
Sex Specified	18	69	14	78[a]
Social Class Specified	1	4	0	0
Race Specified	1	4	2	11
Time (Year or Season) Specified	0	0	0	0
Religion Specified	0	0	0	0
Explicit Hypothesis or Model	18	69	5	28
ANOVA	25	96	17	94
Observations or Interviews	0	0	3	17
Inferential Statistics	26	100	18	100
Multiple Experiments	24	92	10	56

Note: ANOVA = analysis of variance.

[a]I am embarrassed to note that one of the studies in interpersonal relations that did not specify participant sex was authored by me.

or hypotheses. None of these features characterize the majority of articles in the biology journals reviewed following. In addition, although not tallied in Table 1, almost all of the studies could be classified broadly as "experiments," and almost all are either situated in a laboratory or are questionnaires (see also Sears, 1986).

Methodology in More Advanced Natural Sciences

Natural science enterprises often start with a domain of interest and curiosity, usually some real-world phenomenon. A first step is often verification that the phenomenon actually occurs. This may often be followed by an attempt to explore the generality of the phenomenon. A more disciplined description or exploration of the phenomenon often then ensues, with an attempt to discover laws or invariances. Such ventures are often not theory motivated, but rather are motivated by an attempt to be precise about the world, with the idea in mind that future theories will have to have something to explain. Boyle's gas law and the Mendelian laws of genetics are such examples; their motivation was not directly either model testing or model building, but precise descriptions of regularities in the world.

Fortunately, within social psychology there has recently been what I believe is a paradigmatic example of the establishment and articulation of an important phenomenon, or, if one prefers, a "social fact." I refer to the brief and marvelous book, *Culture of Honor,* by Richard Nisbett and Dov Cohen (1996). This work combines the admirable experimental ingenuity that is a hallmark of modern social psychology with evidence from a wide variety of other sources (i.e., crime statistics, voting records, survey results, natural-world experiments, literature, etc.), and generates a web of convergent evidence from many different sources. Although there are criticisms of each line of research, when combined, they result in a truly convincing case for a substantially higher level of violence in the service of honor, and tolerance for the same, among White American men of Southern United States origin in comparison to White men from the Northeastern or Midwestern United States.

It is at this point that hypotheses typically enter the enterprise, as well as models. At the same time, one looks for model systems, whether in the laboratory or real world, that capture the essential phenomenon and will allow variation of critical variables. A rich interplay between theory–model and empirical results ensues, representing the type of research that predominates currently in social psychology.

It is characteristic of an advanced science to have many (but not all) of its studies located at the later stages in this process (i.e., natural or laboratory experiments, more and more refined and formal theorizing). However, these activities only make sense if the earlier stages have provided an appropriate direction for the later research. I claim that in modern social psychology, an understandable urge to become a more advanced science has led to a slighting of the critical early stage work. Though some may take pride in the advanced state of the field as measured by increasingly sophisticated statistical techniques, greater experimental sophistication, frequent invocation of models, and the narrower level of appeal to nonspecialists consequent on the former, one can also be disturbed about these same developments. Why? Because they may be symptomatic of the form of advanced science, but not its substance. We are reminded of Asch's (1952/1987) statement, quoted earlier: "One does not advance time by moving the hands of the clock" (pp. xiv–xv). Premature advanced science stifles creativity, closes the eyes of the field to important new phenomena, is prone to generate long lines of research that ultimately have little to do with the basic target of the field (i.e., the social world), and generally pulls people prematurely away from the real world, where it all starts.

Most critically, if model systems, often in the laboratory, do not capture the essentials of important social processes, then the detailed examination of them may not be very rewarding. The strength of the immensely successful biological model systems, such as Mendel's peas, fruit flies, and bacteria of the species *E. coli* in the study of genetics or Galapagos finches or the squid giant axon, is that they instantiate the invariances that are central to the discipline. Their advantages from the point of view of experimental manipulation only accrue to the field insofar as they capture basic properties.

As scientists, we often tell students entering our disciplines that one just does not go out and make observations or explore a relationship. To make sense of the world, just as with our own perceptual systems, we must bring something to it, have hypotheses, or ideas about the structure of the world. Otherwise we will be overwhelmed by William James's (1890) buzzing confusion. We are right, of course, but it is a caricature of this truth to assume that one must have a model or even a hypothesis to find anything out. Most of the best early science (and much advanced science) is properly motivated by what I call *informed curiosity.* A mind familiar with the phenomena in question and prior thinking in the relevant discipline has a sense that there will be important dividends in exploring certain phenomena: The characteristics of the species on the Galapagos Islands suggest changes in traditional thinking, and the omnipresence of DNA in reproducing entities in cells and organisms of all types suggests its importance in the genetic process. I believe that informed curiosity plays a curiously small role in motivating research in social psychology, even less than it does in more ad-

vanced science. In a later section of this article, by examination of current biology journals, I indicate by counts of journal articles that models or even hypotheses are frequently not presented as the motivation for empirical studies.

In mid-20th century American experimental psychology, we can see in the behaviorism movement, as applied to learning, an example of the adoption of model systems and core concepts that preceded careful grounding in the facts of learning in the world. Ethology, which should have provided the foundation for a study of learning, was assimilated into the field only after decades of work on refined paradigms focusing on a subset of the types of plasticity found in animals in the natural world. These paradigms were taken to be a model for all plasticity, independent of species and domain. The findings about learning from this tradition are basic and important, but would have greater breadth, scope, and relevance if the research had considered learning in context from the outset.

In my view, much of modern economics suffers from the same problem of extensive abstraction and formalization before a substantial grounding in the phenomena. It is quite reasonable and appropriate that some areas within a relatively new science such as social psychology move ahead quickly into more advanced stages, but it is maladaptive to make these the almost exclusive endeavors for a field with such a rich set of phenomena to discover and explain.

This is a particular concern because, as people trained in modern experimental psychology, social psychologists have learned a great deal about measurement, scientific rigor, statistical analysis, and experimental design. These are powerfully important skills for any developing science. They can be thought of as a complement to the gathering and description of phenomena that is accomplished by some of the practitioners in the social sciences most relevant to psychology, such as anthropology and sociology. These social scientists seem to have their attention on some of the major social phenomena that should be at the center of social psychology: norms, rituals, institutions, the family, marriage, identification, nationalism, food as a social instrument, criminality, social class, taboos, leisure activities, and religion. Psychologists have much to offer in bringing the approaches that have been successful in exploring less social phenomena to bear, and they are much better suited to do this than other social scientists. However, they must respect the important real phenomena and be guided by some of the early science work done in these other disciplines.

Psychologists have the special burden of explaining mental as well as behavioral phenomena, but they also have the special advantage of self-reflection and language in themselves and their human participants. On the one hand, this source of information is extremely valuable and can perhaps be used to more advantage than it currently is; note the rare use of interview techniques in the corpus of modern social psychology I reviewed earlier.

> We conclude that phenomenal facts are a source of problems and insight in psychology and that theory, to be valid, must be consistent with them. However, the study of phenomenal facts is only one of the methods of psychology. It must accompany the method of experimentation and analysis, which is necessary to the discovery of causal relations. It must be part of the analysis of functional relations, which is the method of natural science. Psychology must employ this method as other sciences do, but in addition it possesses an invaluable ally in the possibility of studying the facts of consciousness. (Asch, 1952/1987, pp. 69–70)

On the other hand, as the emphasis on implicit cognitive processes in recent work suggests, people are also often unaware of what is going on in their minds, as we learn from the perspectives of Freud and experimental social psychologists (Nisbett & Wilson, 1977). Under certain circumstances, people are poor at predicting their own preferences and behavior (Kahneman, Wakker, & Sarin, 1997). Furthermore, as Fiske (1997) pointed out, much of what people do is essentially "mindless"; one might call it ritual or practices. Such activities are typically explained in self-report as "that's just the way we do things, the way we always have." Fiske (1997) noted that the most effective way to understand such prevalent human activities is by participant observation: the very way they are taught in the process of socialization.

In fairness to modern social psychology, it is true that the investigators are participant observers (at least of their own culture, religion, and social class), and so their life experience and acquaintance with their own thinking provides some of the base that is absent when we study animals. There is no doubt that some of the focal areas of interest in social psychology, such as stereotypes, attributional style, and romantic attachment, have been informed by our knowledge of ourselves and our social world, as have some of the well-studied phenomena, such as bystander apathy and the foot-in-the-door effect.

Modern Social Psychology, 19th Century Evolutionary Biology, and Mid-20th Century Molecular Biology: A Comparison

I compare the scientific process in modern social psychology with the process as it occurred with respect to what may arguably be regarded as the two most im-

portant advances of all time in the life sciences: the development of the theory of evolution in the 19 century and the discovery of the molecular basis of genetic transmission in the mid-20th century. I will refer to these two momentous events with the shorthand terms of *Darwin* and *Watson–Crick.* Both Darwin and Watson–Crick represent magnificent syntheses and theories of incalculable import. Nothing that has happened so far in the history of the behavioral sciences compares in synthetic power or scope. I claim that much of the science that led up to these two great discoveries is of the type that would be rejected by many modern social psychologists and journal editors as crude, prescientific, poorly controlled, susceptible to alternate explanation, and, most critically, not "model or hypothesis driven."

The marvelous accomplishment of Darwin was as inductive as one can imagine. Familiar with current evolutionary and geological theory, young Charles Darwin set sail on the Beagle as ship's naturalist. Darwin had no theoretical axe to grind, it was just the excitement of exploring new worlds (Darwin, 1845/1962; Mayr, 1991). Darwin was a superb naturalist, someone whose eyes were open for events and relationships that were either comprehensible to his 1835 mind or did not fit into his current categories. The phenomena he experienced led to musings and searches for parallel phenomena, and they became the foundation for a truly great theory. One can tell from *The Voyage of the Beagle* (1845/1962) that Darwin was a superb and informed observer, a person who had much experience in looking at the world of nature. His informed curiosity was a fine guide to what was important.

On the one hand, the theory of evolution is as basic, general, and certain as anything in the life and behavioral sciences. On the other hand, the evidence for this theory can be described as a truly massive amount of real-world observations (and very few experiments), all of which are individually subject to other interpretations. In short, it is a very large amount of convergent evidence, each piece of which is pretty questionable. (We are reminded again of Funder's, 1996, third law: "Something beats nothing, two times out of three"; p. 33). It is not clear that any of the pieces of convergent evidence for the theory of evolution would have ever passed the criteria for publication in the *Journal of Personality and Social Psychology.*

The empirical basis for the theory of evolution was (and, to a large extent, is) a large mass of questionable evidence. It is the mass of evidence that is persuasive, along with the lack of any alternative, because artifactual accounts of the mass of evidence would require arbitrary assumptions of thousands of processes rather than one synthetic idea.

In 1953, Watson and Crick made the great theoretical link between the molecular biology of the gene and the principles of inheritance. What was the material that formed the basis of the synthesis? It was empirical data on the structure of DNA, coming most critically from two sources: x-ray diffraction studies aimed at elucidating the conformation of DNA and studies on the frequency of the different nucleotide bases in DNA. Four of the six references in Watson and Crick's monumental 1953 article in *Nature* dealt with these two domains of empirical research. The motive for all four of these studies was, basically, as follows: "It looks like DNA is really important and a likely vehicle for genetic transmission. Let's find out more about it. What is its shape and what is it made of?" This work was not model motivated, although it was surely oriented toward the eventual construction of a model of the genetic process at the molecular level. The two studies on x-ray diffraction (Astbury, 1947; Wilkins & Randall, 1953) were attempts to develop a model of the structure of DNA, but the justification for this was that DNA was very important, so we should know its structure.

The explicit justification by the groups who discovered the matching incidence of guanine and cytosine, adenine and thymidine (the two sets of paired bases in DNA) was, essentially, "This is a very important molecule. Let's study it."

The article by Zamenhof, Brawerman, and Chargaff (1952) is part of a continuing line of research by Chargaff and his associates, which they justified by attempts in their laboratory to "gain an insight into the differences in composition, and therefore presumably, in nucleotide sequence, distinguishing the desoxypentose nucleic acids (DNAs) derived from different species" (p. 402). They noted the wider range of physiology and morphology in microbial organisms as opposed to "higher" organisms, and therefore saw DNA invariances that emerge from such diversity to be particularly important with respect to the constancy of DNA structure across species. They noted the importance of DNA because it was known that certain microbial DNAs are involved in bacterial transformation. Zamenhof et al. used three microorganisms and found the critical ratios across the three as follows: adenine-thymidine ratios around 1.0 (1.03–1.07) and guanine-cytosine around 0.85–0.93, whereas adenine:guanine varied from 0.68–1.75, and thymine:cytosine varied from 0.58–1.54. This study was clearly motivated by a search for the structure of DNA and invariances in it. It was not model driven.

The second cited article on the matched pairs of nucleotide bases (Wyatt, 1952) looked at DNA from 11 insect viruses. Previous work suggested a regular pattern of composition in which the ratio of several bases appeared relatively constant. Wyatt explained: "In the hope of establishing the generality of these observations and clarifying their significance it is worthwhile to continue accumulating data on the composition of

DNA from various sources" (p. 201). He concluded that adenine:thymine and guanine:cytosine ratios were constant and close to unity, whereas (adenine + thymine):(guanine + cytosine) ratios varied from 0.71 to 1.87. The Wyatt article is not model based, was interested in the generality of an empirical relation, and played a critical role in the theory of the molecular biology of the gene, which postulates that each member of these critical pairs dictates the presence of the opposite member on the accompanying strand of DNA.

Modes of Approach in the Biological Sciences: Examination of Contemporary Journal Articles

To get some objective data on the actual practices of biological scientists, I performed an analysis of articles in a few biology journals. I selected the year 1994 to match the analysis mentioned earlier of the *Journal of Personality and Social Psychology,* and I selected three premier biology journals to encompass a range of approaches: *Cell, Development,* and the *American Journal of Physiology.* In light of the dominance of molecular approaches in modern biology in recent times, I also examined the 1950 *American Journal of Physiology.* In all cases, I examined each full empirical article, starting with the first journal of 1994 or 1950 (January). If the journal issue contained fewer than 20 articles, I continued to the second journal issue of the year, stopping when, after completing a journal issue, at least 20 articles had been surveyed. The results are summarized in Table 2.

In all four journals

1. The range of species under study is wide (7–10). If we generously consider the North American undergraduate as a distinct species, then we can conclude that the range of participants (species) is more concentrated in social psychology.
2. Only a small minority of studies (18%) present an explicit model or hypothesis as motivation for the research, compared to a majority in social psychology.
3. It is hard to decide on what defines an experiment, but using a criterion of comparison of a manipulation with a control procedure, only a little more than half (54%) of the studies in the biology journals qualify (and note that all of the fields surveyed are very appropriate for experimentation, unlike evolutionary biology or epidemiology).
4. ANOVA is rarely employed (9%), and statistical inference (marked by presentation of p levels and explicit use of inferential statistics) of any sort is used in only a minority of articles (27%).
5. Functional relations, expressed as value of some dependent variable as a function of at least three levels

Table 2. *Features and Justifications in Major Biology Journals*

	Cell, 1994, Vol. 76, Issue 1 & 2		*American Journal of Psychology*, 1950, Vol. 160, Issue 1		*American Journal of Psychology*, 1994, Vol. 20, Issue 1, G15–165[a]		*Development*, 1994, Vol. 120, Issue 1	
Features	**Number**	**%**	**Number**	**%**	**Number**	**%**	**Number**	**%**
Number of Articles	21		27		20		21	
Number of Species	8		7		7		10	
Model or Hypothesis	4	19	5	18	4	20	3	14
ANOVA	0	0	0	0	7	35	1	5
Inferential Statistics	0	0	4	15	17	85	3	14
Functional Relations[b]	2	10	17	63	12	60	4	19
Justifications[c]								
Explicit Model/Hypothesis	4	19	5	18	4	20	3	14
Confirm, Found in *X*, Now *Y*	0	0	6	22	1	5	1	5
Characterize a Receptor/Structure, Localize	1	5	0	0	4	20	3	14
Describe Events From Time 1 to Time 2	0	0	2	7	4	20	9	43
Relation of *X* to *Y*, How *X* Affects *Y*	13	62	7	26	8	40	5	24
Method, Animal Model	2	10	3	11	0	0	0	0
Compare Effectiveness	0	0	4	15	0	0	0	0
Miscellaneous	1	5[d]	0	0	0	0	0	0

Note: ANOVA = analysis of variance.
[a]For the *American Journal of Physiology,* the more recent volumes divide articles by subfield, whereas the older journals (e.g., the 1950 volume examined) include all areas of physiology together. For 1994, the data were collected from the section on the gastrointestinal system (G). [b]Functional relation indicates records of dependent variable for at least three different levels of independent variable. [c]Each article was classified in accordance with one type of justification. [d]Justification is simply that "little is known about."

of an independent variable, are rather common in the physiology journals (62%) but uncommon in molecular biology.

6. Many of the studies in molecular biology journals are what psychologists would characterize as descriptive: The elaboration of structures, or what happens between time one and time two, are typically illustrated by photographs.

7. The single most common justification for a study (typically appearing at the end of the introduction) is the uncovering of the relation between *X* and *Y*. This is typically stated in just that way, rather than as a hypothesis about a particular effect, or direction of effect, of *X* on *Y*.

8. It is very common to justify a study along one of the following lines (all of the following are quotations from the articles reviewed in the table): "little is known about," "it would be of interest to examine the capacity of…," "This study was undertaken in order to determine the effects of ligation…"(*X* on *Y*), "An understanding in the changes in cellular associations of primordial germ cells, therefore, may shed light on the mechanism by which these cells are directed to the genital ridges."

In general (see Table 2), informed curiosity seems like quite an acceptable justification for a study. Many are open ended (in terms of hypothesis) explorations of relationships or sequences of events, some are confirmations, and a number are extensions of a finding in one species or system to another species or system. That is, a premier journal considers replication in another species or system important (note the critical importance of this in the DNA studies reviewed earlier). In psychology, replication on a different group of participants (different age, campus, culture, region, religion, or social class) might be publishable, but generally not in a premier journal.

This analysis demonstrates a sharp difference between methodology in current or 1950 biology journals and in modern social psychology. Although there is no doubt that biology is a more advanced science, it does not follow, of course, that the methodology in social psychology is "inferior." It is likely that in some areas, such as statistical sophistication, the selection of appropriate control conditions, experimental design, or elimination of artifactual accounts, psychological studies are more advanced. It is also possible that the particular problems presented by the study of human beings in social situations, such as multiple causation and complex interactions, require a different methodology. Nonetheless, this review indicates a mismatch and puts the burden of proof on psychology to produce the special arguments that would justify departures from the historically successful sequence of stages of scientific investigation.

The Illusion of Definitiveness in Experiments

There is no question that the experiment is the most powerful tool available to the sciences. This fact has not escaped psychologists; indeed, a good portion of modern academic psychology in the 20th century has gone under the name *experimental psychology.* However, there is a big jump from "objective" or "data-driven" to experimental. Correlational studies or careful, systematic observations and other forms of data collection make up a good part of the other natural sciences and a substantial part of some areas of psychology, such as personality. Experimentation has a particular power in isolating causes. However, especially when carried out in a laboratory and when dealing with persons as complete entities, the findings are particularly subject to limitations in generality. Any social laboratory experiment involves making a large number of rather arbitrary choices, including instructions to participants, the particular human and environmental situation, the selection of the manipulation and control, parameters of time and sequence, and the particular instantiation of the issue at stake. These necessary decisions entail two risks: (a) they allow for the possibility that the results will not bear on real social situations and (b) they may generalize to only a very narrow range of apparently similar experimental situations.

Controversies abound in areas in which experimental data are plentiful (e.g., in the effects of watching violent television programs). It is no accident that meta-analyses are employed to evaluate sets of experimental studies in both medical science (especially treatment evaluations) and psychology.

Finally, we know that great progress can be made in a scientific enterprise with limited possibilities for experimentation. The current advanced status of the fields of astronomy, geology, evolutionary biology, and epidemiology testifies to this fact.

Why Has Social Psychology Developed Into a Prematurely "Advanced" Science?

I believe that what has happened in social psychology is part of a general process that occurs at the margin of desirable groups. To be thought of as a *natural scientist* is a situation to be admired and sought. Social psychology (or psychology in general) is located at the fuzzy boundary between the natural sciences and the "less desirable" (as seen by many practitioners), less advanced, social sciences. Indeed, psychology is considered in the social sciences at some universities and colleges and in the natural sciences at others. Groups at

the margin of desirable groups may attempt to adopt the prototypical properties of the more desirable group, and hence exaggerate, as it were, their membership in the desirable group. This has been noted and accounted for by social psychologists with such terms as *status marginality, status envy,* or *status anxiety* (reviewed in Brown, 1965). The effect is particularly clear in people with a marginal status at the bottom of a social class who adopt the dialect and other speech habits of the class to which they aspire in a manner that exaggerates the properties in the aspired-to class, a phenomenon called "hypercorrection" (Labov, 1966). In general, there may be no one more upper class in outward properties than middle-class people who see themselves at the lower margin of the upper class and who wish to be members of the upper class. Psychologists in general, and social psychologists in particular, go out of their way to demonstrate the trappings of natural science, such as experiment and formal models.

The success of psychophysics as a highly advanced area of psychology has encouraged this process. However, note that much of the earlier formal success of psychophysics was descriptive rather than explanatory and is akin to Boyle's law or Mendelian genetics rather than to accounts of process.

Within psychology, sensation and perception, and cognitive science, have served as the principle models for social psychology. There are risks in emulating these successful areas. It is probably true that if you understand one eyeball, you will understand them all, but it is not at all true that if you understand one person, you will understand them all. In particular, people's lives, behavior, and mental events are strongly influenced and shaped by the culture they are members of, by the structure of their society and their place in it, and by the "domain" of activity (e.g., food related, religion). Culture, social structure and class, and domain may have minimal effects on sensory processes, but they have an enormous effect on social processes; the beginnings made in cultural psychology have already shown that self- and other-person properties are in a substantial part determined by culture and domain of activity (Fiske, 1991; Fiske, Kitayama, Markus, & Nisbett, 1999; Markus & Kitayama, 1991; Shweder, 1991a, 1991b; Triandis, 1995).

There is another factor to be considered in the particular history of social psychology. The evolutionary tradition was based in large part on the work of respected naturalists. Darwin could be so classified. There is no parallel tradition that is respected by social psychologists. The reluctant integration of ethology into psychology in the mid-20th century was highly beneficial in the study of animal learning and behavior. In the human domain, parts of sociology and cultural anthropology represent this ethological approach, but these disciplines have had very little influence on social psychology. An exception, perhaps, is Erving Goffman, whose observations (never with numbers or tables) are frequently reported in social psychology texts and have led to ideas, such as impression management, that have been developed by social psychology.

Studying Football

Before concluding, I present a whimsical scenario (modified and expanded from Rozin, 1981) illustrating the problems of prematurely entering the advanced stages of science. It is admittedly a caricature of what I claim has actually happened, and it is more directed to the grant award process than the journals.

> The Martian Institute or Foundation for Furthering Science (MIFFS) Earth Sport Section (MIFFSESS) was convening for its 10th year. Up to this time, the Research Program had been entirely devoted to a thorough study of one simple earth sport that the earthlings call *tennis.* Progress was rapid, and many laboratories were engaged in the enterprise. Thus, it was quite a shock when a few scientists at the 9th MIFFSESS meeting suggested that MIFFSESS support research on the uninvestigated sport of football, at some cost to the tennis program. The tennis researchers pointed out, with some justice, that they had made great progress and now understood the scoring, physics, and other aspects of the sport. Yet there were still many problems to be tackled in the microanalysis of the game. There was, for example, the well-known "yellow ball problem." A yellow ball was used on only *some* occasions, and no one could predict this distinct occurrence. Pigment analyses of the yellow ball were just beginning. "Why," asked the tennis workers, "commit money to the murky enterprise of football when such good problems remain with tennis?" Nonetheless, in Year 9 a small amount of money was budgeted for the following year for the investigation of football. And now, the Committee had to evaluate the proposals.
>
> The Committee was faced immediately with some fundamental disagreements among the applicants. Some claimed that the essential elements of the sport were six creatures with black and white striped costumes. They were the only participants who appeared to be on the field at all times. Others focused on the more than 50 creatures, some sitting, some running, each with his own number. Some claimed that the ellipsoidal object noted in some observations should be the focus of study, but others pointed out that this object was rarely visible and probably did-

n't matter. In the end, the Committee agreed that the numbered creatures might be the best bet for study; because they had numbers, they could form the basis of precise quantification. For unnumbered participants, one might have to make up arbitrary numbers.

One proposal suggested correlating two measurable variables: the number of the players, an incontrovertible datum, and the percent fat, of known biological importance. Other proposals suggested electrical rather than biochemical analyses. One group proposed use of the standard electroencephalogram (EEG) technique. Each player would be wired up, and the total set of generated potentials for all of the players would be measured with a computer. The investigators worried about tripping on wires, but came up with the clever idea of using a blimp over the stadium, from which all the wires could be suspended. Yet more clever proposals did away with the individual EEG and proposed a macroapproach, a total integrated reading, taken from the ellipsoidal extremes of the stadium itself. Another set of experimenters proposed to set up an animal model of football.

A group of economists proposed a model for football on the assumptions that (a) each player was totally independent of any other, (b) all actions in the game were symmetrical, (c) there was no change over time in the activities of any player or team, (d) all players were seeking the same goods, and (e) all players operated under the same constraints.

One group encouraged the search for invariances, and impressed the committee with the preliminary finding that the summed numbers of the players remained roughly constant through the game, although players came in and out. In the search for order, they pointed out that one should begin with what was apparently the most structured aspect of the game: the grouping of players in a circle, in fixed order, every minute or so. This was followed by another ordered formation, and then by an apparently disordered set of movements, probably the players "letting off steam." Plots of position in the circle against player's number seemed reliable, and a good point of departure. It was proposed that these observations be followed by detailed analysis of foot and hand positions of the players, in the circle and after, to build up the elements of the game.

There was one proposal that was easy to reject. It stood out as the one that failed to follow the basic scientific requirement of objectivity and quantification and was not model driven. The authors (from the fringe of science, at best) proposed to simply observe the general flow of the game and to supplement and guide these observations with interviews of the players in an attempt to find out what the game was about. They proposed to ask players such open-ended questions as: "What is the purpose of the game?" "Is the ball important?" "Why do the players move toward one end of the field for a while, and then to the other?" The Committee unanimously rejected this proposal, supported in this decision by unanimously negative reviews from tennis researchers. The grounds for rejection, contained in what might be called the quintessential pink slip, were many:

1. The study relied in large part on verbal reports, which were of questionable scientific status. Why, for example, should one believe a player's claim that he moved to the right to misdirect other players or that the rarely visible ball was the center of activity?

2. Worse, the reports were retrospective. Players were not asked while they were playing, but after a game, reducing even further the reliability of the results.

3. There was no control. At a minimum, it would be necessary to question a group of control people who were not familiar with football.

4. The authors were unaware of the importance of social desirability. To be sure that the informants were not making up stories, the Crowne–Marlowe test should be administered.

5. At best, the research proposed involved only a single study.

6. The study might not produce interpretable data.

7. The investigators had no model for football; they proposed simply to explore it. It was well known that observation not linked to model or theory would be useless and unable to discriminate essential processes from trivial events.

8. The authors did not make clear what were the dependent and independent variables.

9. The authors failed to rely on, or even utilize, the only reliable route to understanding: experiment.

10. The authors failed to describe how they would statistically analyze their data (if they could get any). In particular, it was not obvious how the authors could perform an ANOVA on their data. One reviewer suggested that they could use football and tennis as the categorical independent variable, but even this helpful reviewer couldn't think of a dependent variable.

11. One reviewer thought of a clever alternative account of any data the authors might gather. The reviewer noted evidence for a thea-

tre tradition on Earth, in which what were essentially imitations of real life were portrayed. Perhaps, the reviewer proposed, all the authors would be describing was such a theatrical portrayal, with considerable distortions, no doubt, of the actual reality.

12. If the authors were "successful" on their own terms, they would have accomplished merely a pushing off of the fundamental problems. To say the purpose of the game and the moves of the players were represented mentally in the heads of the players was not progress, because we would then have to study the representation in the heads.

There was another proposal asking for funds to explore books in libraries on Earth in the hope that some information on the game would be unearthed. Because work on Earth libraries was in its early stages, the proposers would need a year or more of support to try to find the material. The proposal was rejected. Although most Committee members agreed that it might uncover valuable information, it could not be funded because it did not involve the discovery of *new* facts through research. After all, the proposed findings were *already* in books, somewhere.

And so it was that a decade of studies of the arrangement of players in the football huddle was begun, along with an analysis of the biochemical and electrical events underlying this circular event.

As indicated at the beginning of this parody, it is a caricature, but I believe there is a kernel of truth in it. I offer one important limitation of the application of the parody to the current operations of social psychology (less so to the operation of the grant awarding process, in general). Unlike Martians, social psychologists are participant observers in what they study, so they bring some insight into the situations they are studying (pointed out by Daniel Gilbert, a nonanonymous reviewer of this article).

Conclusion

This article is a plea for balance, for a greater consideration for identification and description of phenomena and invariances as opposed to modeling, hypothesis testing, experiments, and sophisticated statistical approaches. The claim is not that the current approach is wrong or unproductive. Rather, the claim is that we have relied too much on the predominant current approach, given our stage of development as a science. Much of human psychology, like most of economics, has been so attracted to the trappings of science that it has invested insufficiently in the fundamental early stages of science. Careful observation, informed curiosity, recognition of the importance of context and the limits of abstract and laboratory-based models, and, in general, more emulation of the life sciences would be desirable—not to replace what we have, but to stand beside it. The outcomes of experiments may be clear, but their meaning and significance for the target phenomenon are often questionable.

Psychologists are extremely good at analyzing problems, making causal models, and experimentally teasing apart alternatives. If they are pointed in a particular direction, they find valuable and clever ways to advance. The skills of psychologists in this domain surpass those of their fellow students of the human social world in anthropology, political science, and sociology, and one might argue that these other social sciences could profit by incorporating these approaches into their scholarship. However, psychologists should learn from anthropologists, political scientists, and sociologists to keep their eyes on the "big social phenomena," and to situate what they study in the flow of social life.

The problem for social psychology is that there should be more concern about the directions in which the field is pointed. In evaluating research for grant support or publication, we should recognize that the requirements for scientific rigor and unambiguity are relative to the stage of advance of the investigation of the issue in question. A first article (or grant) on a subject should not be evaluated by the same criteria as the 100th article designed to provide evidence for a well-articulated issue on which there has been much previous research. The criterion should be: "How much does it increase our understanding?" This can be done by settling or contributing to a well-defined issue; opening a new area; calling attention to an anomaly; bringing to bear already published material relevant to, but not known by practitioners; integrating across paradigms; or introducing new models and theories. Advance is the critical issue, advance perhaps in relation to amount of journal space (or funding) required.

Perhaps social psychology is ready for the abstract, experimental treatments that it has, for the most part, adopted. This may be so in some areas, some of those well represented in the *Journal of Personality and Social Psychology.* But can we call a field ready, overall, for an almost exclusively formal, experimental approach to the social life of humans when

1. The data are based in large part on a sample of educated Americans ages 17 to 20 years, primarily from major universities, in the midst of a special and unique period of life transition between childhood and adulthood (Sears, 1986)?

2. The data are based almost entirely on findings from samples of less than 10% to 15% of the populations on the face of the earth (i.e., North America, Western Europe, and other parts of the English-speaking world)?

3. The phenomena and paradigms are for the most part not situated in the structure of social life? There is good evidence from ethology and anthropology that animals and people function differently in different domains of life. This point is documented by, among others, Rozin (1976) for learning and cognition and Fiske (1991) for various domains of social life. It is reflected by the importance of the idea of modularity (Fodor, 1983) in modern cognitive science.

4. The range of phenomena studied, as judged by listings in the indexes of major textbooks, omits the major domains of social activity of humans? A survey of the indexes of five leading social psychology textbooks from the early 1990s reveals that the median text has no reference to food, religion, ritual, leisure, sports, music, drama (theatre), money, or work. There is no doubt that food, work, and leisure are the three most time consuming waking activities of human beings, and all are deeply social. In the world at large, more consumer spending is devoted to food than to any other category of activities (e.g., shelter, clothing, leisure; Samuelson, 1990). In the Western-developed world, comprising a definite minority of humanity, leisure is the major category. Are we ready for the entire field to move to the formal, experimental stage when we have yet to determine whether the 800 million Hindus in India or 125 million Japanese have the same sense of self as we do, the same social motives, or the same attributional biases that have been found to be typical of American college students (Markus & Kitayama, 1991; Shweder, 1991a, 1991b)? Just as biologists have learned about life by studying different species and different environments, we would do well to open our eyes more widely before we dig too deep a hole at one place in the broad and varied terrain of human social life.

Psychology, as a discipline, has been divided into subspecialties based primarily on process. The chapters in introductory texts and the second level courses parse the world into entities such as sensation, perception, learning, motivation, memory, and thinking. This is surely a legitimate parsing, supported intellectually by the idea that these basic processes remain more or less constant across domain. Yet in sensation and perception, and now in cognition, there is more and more emphasis on the special properties (modularity) of mental function in particular domains. The special properties of language are perhaps most salient.

An alternative way of parsing the field would be by domain of activity: procuring nutrients, sexual activities, sleep and wakefulness, self protection, managing social relations, and social knowledge. Note that in many respects physics (e.g., mechanics, optics, electricity), zoology (e.g., along the lines suggested for domains in psychology earlier) and physiology (e.g., nervous system, gastrointestinal system, respiratory system) parse their fields by domain. To be sure, there are disadvantages of domain parsing, but it does have the advantage of situating the phenomenon under study in a functional and meaningful part of life. Within such an orientation, in social psychology, there would be great attention to leisure activities, work, ethnopolitical conflict, close interpersonal relationships, and food or eating as some of the social domains. On the other hand, a process orientation encourages controlled laboratory experimentation and no doubt promotes the discovery of general principles. We should think carefully, as a particular problem is approached, about whether the process or domain orientation will be more fruitful.

There are virtues and shortcomings in what I take to be the paradigmatic form of modern social psychology. It is impressive in sophisticated statistical reasoning, clever experimental designs, and hypothesis testing. However, it is inattentive to the domains of social life; although there has been a recent rise of interest in cultural variables, there is still resistance (unlike in cognitive science) to the idea that people's social worlds, thinking, and behavior are different in different social domains. There is resistance, even for some in "cultural psychology," to the importance of careful observation, insider knowledge, and the value of ethnographies.

Ironically, both the strengths and shortcomings of the field are amply illustrated in Solomon Asch's work. Asch was the author of classic experiments, and his impressions and conformity work illustrate the power of experiment and use of a rather abstract, refined experimental situation. These studies were among the precursors of the modern experimental paradigms. However, in general, Asch had his eyes on the big picture, and as a person well educated in history, literature, and the other social sciences, and as a Gestalt psychologist, he was very much inclined to put his work into a rich context.

As Asch (1959) pointed out, in psychology there has been over-reliance on the belief that "better a minute truth than a grand half-truth" (p. 367). "Psychology must center on great and permanent problems, and psychologists should avoid the undignified posture of those whom in another connection Santayana has described as redoubling their effort when they have forgotten their aim" (Asch, 1952/1987, p. 31).

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