When Right Results Are Wrong

Stanley Lieberson

We know there are myths in all professions, deceptions that practitioners foist on both the public at large and themselves. We know that actual practice deviates from the overt rules and the public posture that is taken. What people or organizations say they do is often far different from what they actually do. Why should we think that social science is any different? A great discrepancy exists between what we do and the claims we make about topics that are controversial or in the public limelight. I am convinced that the social sciences follow certain implicit rules of thinking that are totally inappropriate, illogical, and ultimately undermine our ability to advance knowledge about society. Many of my examples will be from the field of race and ethnic relations because that is the area I know best, but the problem is widespread and covers far more than that subject.

The problem exists for good reasons, and it is not easily eliminated; indeed some social scientists of good will and kind intent may find the implications of my thesis repulsive. But if we start with the notion that the social science disciplines are truth-seeking (ignoring for the moment philosophical and relativistic questions about the nature of "truth"), then my comments follow logically even if their implications are sometimes uncomfortable.

Research and Ambiguity

There are those who essentially reject all empirical information, or at least statistical data, because they claim it is an inkblot that one can interpret or massage to reach any desired conclusion, or because they hold that empirical analysis cannot really get at what is important. That is the extreme view and is probably held by only a small segment in our disciplines. (If someone rejects any and all empirical data, honesty requires that data which support someone's theory be rejected or ignored as readily as when the data clash with the same person's ideas.) Most of us do not fall into this extreme; we would not be troubled, for example, to learn that courts approve of statistical models in discrimination cases in order to compare the probability of a qualified minority being hired with that for the majority group.

On the other hand, most of us are unlikely to automatically accept empirical data without careful evaluation. First of all, one rarely finds a single data set that provides such a sharp resolution of a problem as to result in an unambiguous and incontrovertable conclusion about the correctness of a major theory or even a major factual question. It is uncommon in the social sciences—and probably in many of the natural sciences—to find a "critical" test or experiment that alone resolves a controversy. In addition, there can be many technical problems with our research, and it can be particularly difficult when we work with nonexperimental data, as I showed in *Making It Count* and as Christopher Achen describes in chapter 1 of *The Statistical Analysis of Quasi-Experiments*. Any decent undergraduate methods text is filled with technical issues that must be considered lest a research project go awry and generate meaningless output. In addition to these two problems, which are both increasingly recognized and understood, there are also a set of exogenous pressures that affect our study and the interpretation of the results. It is not at all unreasonable to view the results of social research as a dependent variable that is affected by the reality of what it is meant to study, and by other various technical matters that keep the study from being ideal, and also by the social context of the work itself. By the latter I mean a wide variety of social pressures on the researcher(s) such as: the dominant theories and paradigms in the field; the predisposition of the researcher; the dispositions of foundations and other funding sources; the dispositions of journal editors and reviewers; senior colleagues; the prestigious in the profession, and so forth. (Here and elsewhere in this article I will not deal with intentional cheating with the data in the sense of fudging about the actual numbers—as opposed to ignoring contradictory results or otherwise analyzing the data with the intent of obtaining a certain result.)

If the results of social research are to be viewed as a dependent variable that is affected by more than the reality it is meant to ascertain, it does not follow that the influence of extraneous forces is of equal magnitude for all research problems. Some problems are intrinsically hard to get at; for example, heredity-environment issues well known to us with respect to black-white differences in IQ tests. Others are relatively more manageable. Consider, for example, the serendipitous discovery by Stephen D.

Stitt (a professor of law at the University of Florida) and S. Kay Isaly (a paralegal with a Jacksonville law firm). While preparing the defense for a client in a Florida murder case in 1977, they learned that 94 percent of the 114 men on Florida's death row in 1977 had killed white victims, whereas 4 percent had killed blacks, with the remaining 2 percent being equal opportunity killers having both white and black victims. (This is described by Hans Zeisel in the 1981 *Harvard Law Review.*) There were strong grounds for concluding that race of victim affected a murderer's likelihood of receiving the death penalty. Such a result is consonant with a long-standing notion in race relations that white-dominated society values white life more highly. Hence violations against whites were punished more severely.

We also have several other careful analyses of related questions: in a 1981 *American Sociological Review* article, Michael L. Radelet found in Florida that, "Those accused of murdering whites are more likely to be sentenced to death than those accused of murdering blacks . . . due primarily to the higher probability for those accused of murdering whites to be indicted for first degree murder." Race of defendant did not strongly affect the indictment or the probability of a death penalty once race of victim is controlled. On the same topic, a thorough logit and log-linear analysis of the data reported by Radelet appears in the second edition of Alan Agresti and Barbara Finlay's *Statistical Methods for the Social Sciences* on the influence of racial characteristics on the odds of a death penalty being imposed after an individual is convicted of homicide. These results contrast with earlier periods for which there is evidence that black criminals were at substantially greater risk of receiving the death penalty. These studies are probably not the final statements on this topic, but the quality of the analyses is high; it should not be hard for criminologists to evaluate the results and pursue the relevant data, if unanswered issues still remain.

Most of us would have no difficulty appreciating this sort of research: race of offender either does or does not influence the sentencing of someone found guilty; likewise, race of victim either does or does not have some impact on the sentence; and, in turn, severity of sentencing either does or does not reflect an interaction between the victim's race and that of the killer. In similar fashion, we can appreciate and understand research dealing with complicated equal employment cases. As Joseph L. Gastwirth and Samuel W. Greenhouse show, in the 1987 *Journal of the American Statistical Association,* there are special statistical problems when the minority fraction of qualified applicants is unknown and/or when the number hired is small relative to the number of applicants which is, in turn, a small part of the large pool of qualified persons. We can see how such statistical issues must be studied when a court deals with charges of discrimination by the University of Texas at Dallas against women, or when it has to decide if hiring policies by the government of Fairfax County discriminates against blacks.

Under any circumstances, it is appropriate that the existing research results be challenged if more than one interpretation is plausible. Such a give-and-take is not always pleasant for those who are "taking" it, but it helps us seek the truth. It leads to more and better procedures, the examination of newer and different dimensions to the problem, theoretical reformulations, and so on. In evaluating the work, we should start with the assumption that the researcher is giving his or her most honest effort to interpret the results, but there are these additional forces operating that may distort that interpretation. No matter what we may think of the methods used, the quality of the data, indeed the intelligence of the author(s), the results are to be dealt with directly and in terms of the standards of scholarship in the social science discipline. If we ignore the study and/or its conclusions contradict one's own assertions, it cannot be on the grounds that we do not like the results.

Above and beyond the social and structural pressures—which are shared with many other disciplines in the hard sciences—the social sciences are hampered by an additional pressure that leads us to ignore some research and all too quickly and unthinkingly embrace other studies of less merit. In a nutshell, results that clash with normative views about contemporary social and political issues are treated differently than either those results that are in harmony with such views or deal with topics that have minimal relevance for contemporary policy debates. There are a set of illogical procedures and inappropriate ways of thinking about results that are implicitly defined as "unacceptable." What are they?

Ad Hominem Thinking

Let us start with South Africa's massive racial gaps in income. According to their 1986 *Official Yearbook,* for 1983 the average monthly earnings (in South African rand) in various industries was R1,200 for whites; R571 for Asians (mainly of Indian ancestry); R404 for "Cape Coloured"; and R300 for blacks—the latter figure being exactly one-fourth of the average for whites. (I assume the gap would be even greater if agriculture were included, as well as data for so-called independent African states.) What causes this difference? Is it discrimination of one sort or another? Not according to a study of 1970 wage differences reported in the 1985 *Official Yearbook.* Whites had earnings 5.2

times greater than blacks in 1970; of this huge gap, according to the statistical analysis presented, between 87 and 96 percent of the total black-white gap is due to nondiscriminatory factors. Controlling for racial differences in age (an approximation of experience), hours worked, education, and occupational status, it was found that about 70 percent of the black-white differential could be explained as not due to discrimination but to "factors relating to productivity." That is not all; they proceed to take into account the fact that the quality of black education is lower because of the standards existing for teachers, differences in the student-teacher ratio, and gaps in the per capita money spent on students. By the time they get through it turns out that 91.5 percent of the gap (±4.5) is not discriminatory.

It is inappropriate to criticize a work because of what might be done with the results.

How should we interpret this result? Let us assume that the data are not fudged, that is, that the gaps are not greater than the South African government reports, or educational differences are not smaller, and so on. The results cannot be rejected on the grounds that we know why the government is providing us with such data, we cannot say that we deny the results because South Africa is a racist country that is trying to rationalize or justify intense forms of discrimination.We may wonder about that, and it may alert us to question the data very closely, but we cannot reject the results on these grounds. Such a flat declarative statement can be made on simple logical grounds: namely, to reject any data set or intellectual argument because the presenter has certain motives or attitudes is to take an ad hominem position that has nothing to do with the validity of the argument. If a natural scientist claims that grapefruit causes cancer, one does not think about what makes the scientist look at grapefruit so negatively (even though there may be such reasons), nor does one say that the scientist is antigrapefruit as if that is the explanation for the scientist's results. It is perfectly possible that the scientist hates his or her father and mother and they own a highly mortgaged grapefruit farm. Or the scientist might have always found the taste of grapefruit to be repulsive. But it is not consequential because consumption of a given amount of grapefruit either does or does not affect the risk of cancer. That is all that matters, pure and simple. Ad hominem arguments, defined in *Webster's Ninth New Collegiate* as "marked by an attack on an opponent's character rather than by an answer to his contentions," are illogical and irrelevant. In the South African case, the analysis collapses because it is so bad; it takes black characteristics which are themselves products of discrimination, for example, the occupation held, or level of education, and sneaks these in as control variables.

Consider a tougher example of this misstep in our thinking. In 1974, Robert William Fogel and Stanley L. Engerman published a two-volume study in economic history, *Time on the Cross: The Economics of American Negro Slavery.* The entire second volume was devoted almost exclusively to evidence and methods, and the first volume—although very readable—included six tables and 46 figures. So their work was hardly speculative. Their reinterpretation of slavery generated considerable controversy. Among their conclusions are these: Slavery was highly profitable; the system was going strong at the time of Civil War; slave agriculture was efficient compared to the northern system of family farming; and the demand for slaves was increasing. There are two others that I want to specially emphasize: (1) "The material (not psychological) conditions of the lives of slaves compared favorably with those of free industrial workers. This is not to say that they were good by modern standards. It merely emphasizes the hard lot of all workers, free or slave, during the first half of the nineteenth century." (2) "Slaves were exploited in the sense that part of the income which they produced was expropriated by their owners. However, the rate of expropriation was much lower than has generally been presumed. Over the course of his lifetime, the typical slave field hand received about 90 percent of the income he produced."

Fogel and Engerman mention, in the book's epilogue, that they had presented papers in many places on their research dealing with the economics of slavery: "Sometimes, after the end of a discussion, one of our colleagues would come up to us and, with a nervous smile, ask, 'What are you guys trying to do? Sell slavery?'" Elsewhere they mention a conference on these matters in Philadelphia in 1967 which eventually lead to accusations of racism and questions of whether the authors were arguing that blacks were better off under slavery than freedom, that the abolition of slavery was a mistake.

Are Fogel and Engerman correct in their assertions? Consult an important critique by Paul A. David et al., *Reckoning with Slavery,* and the literature that has built up on these issues. But the authors' motives are not grounds on which to decide if their data are analyzed correctly or if the conclusions are correct. One should not be so confident that the motive of the social scientist is all that ob-

vious or suspect, given the conclusion reported. The scientist who finds a linkage between grapefruit and cancer may well have been a great lover of grapefruit who gradually got interested in studying the fruit. Likewise, there is no reason to think that Fogel and Engerman are anything but concerned social scientists (see, for example, the motives they give in the first volume for attacking the then traditional interpretation of slavery). Unless we believe that data are totally the product of the researcher's wishes, then we have to allow that the results a social scientist reports is indeed the best sense he or she can make of the data.

One may ask: Does this position on ad hominem arguments run counter to a basic thrust in the sociology of knowledge, which not only posits certain social conditions underlying knowledge, but also has as its mission the study of these conditions? The answer is that there is not really any direct contradiction. The key is to not confuse the mission of the sociology of knowledge with the substantive problem being studied. It is perfectly appropriate at any and all times to ask about the social underpinnings of knowledge, but that is not equivalent to asking whether the knowledge is valid—at least valid under the broad criteria of what the society is able to define as "true" in its current state of affairs. If there are certain commonly shared criteria for determining whether consumption of a give quantity of grapefruit increases the risk of cancer, then that question is not addressed by asking why the scientists are doing these studies. They are separate questions; and unless empirical work is ruled out entirely, each addresses distinctive and separate questions. As Howard S. Becker observed in *Social Problems* in 1961, "The sociology of knowledge cautions us to distinguish between the truth of a statement and an assessment of the circumstances under which that statement is made; though we trace an argument to its source in the interests of the person who made it, we have still not proved it false."

Ad hominem arguments permeate the thinking of social scientists, students in the classroom, to say nothing of the population at large. Here are two of many examples from my experience. In 1971, when I was on the faculty at the University of Washington in Seattle, I published a paper on the military-industrial complex in the *American Journal of Sociology.* As far as I could tell from the data that I gathered and analyzed, the notion was greatly overblown: a minority of industries stood to gain by increased military spending, but most gained more if the same amount of federal spending was devoted to nonmilitary spending. I was able to explain how increased military spending could still occur even if the majority of American industry did not stand to gain. After the publication of that paper, on more than one occasion I received joking inquiries as to whether Boeing—a company with large military contracts—had paid me to do the study. Second, some months ago I attended a brown-bag talk on values in prime-time commercial television. The speaker, in the course of his discussion, observed that television producers and writers were largely Jewish and they had significant influence on the values embedded in television output. Nobody in the audience responded. Judging from the speaker's name, I would guess that he was Jewish. I could not help but wonder if the comment would have been as readily accepted if the speaker had been either black or a non-Jewish white.

There is good reason for the extensive use of ad hominen arguments: the data are often not that clean and clear; there is rarely a definitive result; and hence there is the propensity to go to the extreme of assuming that the outcome is really a reflection of the investigator's disposition. This is the "Fallacy of the Consequent," where one argues from a consequent to its cause. For example, if someone dislikes hot and humid weather, he or she will not want to take a summer vacation in Florida. But if you do not take a summer vacation in Florida, it does not follow that you dislike heat and humidity. To put the argument in a different form—a statistical one—if the dependent variable has several common causes, one cannot infer with much confidence which cause is operating by simply observing the presence of the dependent variable.

A final example of how ad hominem thinking puts pressure on us can be drawn from William Julius Wilson who, in *The Truly Disadvantaged: The Inner City, The Underclass, and Public Policy,* appropriately describes the "liberal" perspective on the underclass as "timid." In writing about the deterioration of ghetto life through the years, Wilson observed:

> The only study that provided at least an abstract sense of how the problem had changed down through the years was the Moynihan report on the Negro family, which presented decennial census statistics on changing family structure by race.
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> However, the controversy surrounding the Moynihan report had the effect of curtailing serious research on minority problems in the inner city for over a decade, *as liberal scholars shied away from researching behavior construed as unflattering or stigmatizing to particular racial minorities* [emphasis added].

In 1985, C. Arden Miller published an article in *Scientific American* purporting to show that reductions in the funding of various programs "for children, mothers of young children and pregnant

women" during the Reagan administration had "contributed significantly to the change of trend in the infant mortality rate." The change in trend was based on the fact that the rate of decline had slowed down in one year, 1984, a drop of 2.7 percent in that year as contrasted with 4.6 percent per year in preceding years.

Such a conclusion is totally unwarranted. In an outstanding article, reprinted in the second edition of *Statistics: A Guide to the Unknown* by Judith M. Tanur et al., Donald T. Campbell examines the problems of measuring the effects of innovations by means of time series analyses. He is concerned primarily with the influence of various government policies on automobile-related incidents of death, accidents, and so on. He observes, "It is clear that the more unstable the line is before the policy change or treatment point, the bigger the difference has to be to impress us as a real effect." If we see that the dependent variable bounces around from year to year prior to the introduction of a new program, it is difficult to know whether a shift shortly after the innovation is a function of the policy or merely reflects changes that would have otherwise occurred.

The existence of a double standard, such that studies supporting our predispositions are examined less critically than are studies running counter to them, is by no means uniquely a problem in the social sciences or in that part of our work dealing with issues of policy and public controversy. (I believe it was Peter Rossi whom I first heard use the term *double standard* in this way.) But it is exacerbated in these areas because it is difficult to stand up to a double standard without again having one's motives questioned. If a study provides weak evidence to support the claim that group X is being discriminated against in some arena of life, it is hard to criticize the study without giving the impression that you do not think group X is being discriminated against even if all you want to say is that the data fail to show it in a convincing way (not that there is anything inherently improper with such a conclusion, anyway). This again involves the irrelevant ad hominem nature of such a conclusion. Moreover, the inference is not a logical one since it entails an incorrect conclusion about motives that is due to the Fallacy of the Consequent.

Another example of the Fallacy stems from my comments about Ronald Reagan. If I try to argue that the *Scientific American* article provided extremely shaky evidence about the effect of Reagan's policies on infant mortality in the United States, does this mean one should infer that I am "for" Reagan? Yes if we fall for the Fallacy of the Consequent. Someone who is pro-Reagan will defend him. From such a statement it does not follow: if someone defends him, therefore he or she is pro-Reagan. Actually I am not: I am pro good thinking; I am pro doing good work in the social sciences.

Undoables

There is a second reason for my mentioning this *Scientific American* article about Reagan and infant mortality. That pertains to the fact that our eagerness to address public policy issues can lead to totally unjustified studies and ways of thinking. We seek to do studies that are simply undoable for one reason or another. Sometimes the data just are not there to answer the question. This is not easy to grasp; Reagan's policy either did or did not affect infant mortality. But that does not mean that it is an answerable question with the information at hand (in this case, the rate for a single year). To use a gambling example: Suppose I have a hunch that the roulette wheel will come up red on the next spin and it does. Do I have some power to forecast the wheel? Such a question can be asked and there has to be an answer, but we are in no position to draw a conclusion after the one spin—or at least to make it with any confidence. This is not a cop-out; indeed it is a helpful way of thinking about program evaluations if questions are raised when there is not enough time and information to reach a meaningful conclusion.

I have discussed other undoables in *Making It Count,* so the point I emphasize here is: we should use our knowledge and skills to address public policy issues, but we do harm to ourselves and to society when we pretend to have reasonable grounds for a meaningful empirical conclusion when the data are inadequate. (If, on theoretical grounds, we have something to say, then that is appropriate as long as it so labeled.) An especially helpful contribution can also be made to society when we are able to show how it is not possible to answer the empirical question under the existing conditions or why it is the wrong empirical question. An example of the former is what I call the "premature question," namely one that occurs when we do not yet have the underlying knowledge necessary to reach an answer to the question that is posed.

Undesirable Consequences

If results have potential consequences that are viewed by someone as undesirable, then it does not follow that the results are wrong. Suppose we attempt to ascertain the truth of the following statement: if P, then Q. The statement about P leading to Q is not in error if we do not like Q; nor can we conclude that P does not exist simply because we object to the consequences of P being true. This is an obvious point, but such faulty reasoning is common when the issue has consequences for public policy. For example, William Julius Wilson's book, *The Declining Significance of Race: Blacks and*

Changing American Institutions, was criticized in part because it would be used as justification by the dominant white society for changing some policies toward blacks or might be misunderstood in such a light. It is appropriate to criticize a scholarly product because of errors in its results, procedures, theories, or logic; but it is inappropriate to criticize a work because of what might be done with the results. Doing so will produce a perverse form of logic: I do not like Q, therefore I reject the truth of P and/or the causal linkage between P and Q. This is different from simply opposing Q on openly stated value reasons even though the truth of the linkage between P and Q is accepted. But distorting the "if P, then Q" result simply in order to avoid Q runs counter to the ideals of any truth-seeking discipline. (For a more complex statement about the issues involved, at least as it is manifested in criminology, see a 1981 book review by Travis Hirschi in *Crime and Delinquency.*) Barrington Moore, Jr., cited by William Julius Wilson in the 1980 *Contemporary Sociology,* has stated the issue very well: "it is important to remember that in pointing out the political implications of an argument one has said nothing whatsoever about the validity of the argument. It is a common polemical device to discredit a line of argument by demonstrating that it leads to unwanted political conclusions."

The second issue pertains to our ability to present other perspectives to people who otherwise have no contact with them. Working in race and ethnic relations, one quickly becomes sensitive to this since we often do not fully understand the perspectives that members of another group have toward a social issue. Social science cannot say which perspective is correct, but we can play a valuable role in helping society realize that there are different perspectives on the same topic that would not otherwise be understood. One would think this position is hardly controvertible, but it is when the investigator helps us understand a perspective that the members of a discipline generally (or at least publicly) disapprove of, for example, the point of view of antiabortionists, bigots, and so forth. Although perfectly happy to understand the viewpoints of persons we are sympathetic toward, we often act as if someone presenting us with the viewpoints of others is necessarily someone who is sympathetic or inclined in that direction—as opposed to providing us with basic information.

In *Conversations with Isaac Bashevis Singer,* the Nobel Prize winning writer was asked whether he endorsed the views of the hero in one of his novels. After Singer denied such an endorsement, the interviewer pressed him further, and Singer responded:

> The tendency to identify a protagonist with a writer is a very silly one. Some people came to the conclusion that Dostoyevsky really was a murderer. Give him freedom and he would kill a Russian woman and her niece and so on. It's ridiculous. The fact that you know the way of thinking of a murderer does not mean that you are a murderer. It means that you can project yourself into his way of thinking. This is the very essence of talent. But this suspicion that the writer is always the hero of his book can do great damage to literature.

In the same way that Dostoyevsky is not a murderer because he wrote a novel about a murderer, social science should not be taken as endorsing a position simply because it helps us understand those who hold such positions. It is as important for us to learn about perspectives that run counter to the dominant ethos of the disciplines as it is to learn about perspectives that we tend to support. It should not be the author's duty to assault the subjects or counter their perspectives in ethnographic research. Moreover, even if the scholar were to be personally supportive of, say, right-to-life legislation, it should not be relevant to evaluating the product as scholarly information.

Distinctive Qualities

If we are serious about doing research that has a public policy orientation, it is necessary to consider some of the special problems that occur in this type of research, as opposed to academic work. First, many of us would be outraged if a drug company introduced a new medicine without first considering possible side effects caused by the drug. Not only do we want to ascertain whether the drug is useful in treating a given disorder; we also want to know if the side effects are worse than the cure, that is, if the treatment does more harm than good. At the least, a patient should be forewarned in order to determine if the drug presents a greater risk than he or she is willing to take. Knowledge about side-effects also helps us decide if the net effect of the new drug is an improvement over alternative therapies. This is so important that we have a government agency to insure that drug companies make the appropriate tests and report all of the relevant information.

In policy-oriented research—for the very same reasons—it is also necessary to consider side effects. This requires social research of a sort that is radically different from the customary focus on how one or more independent variables alters a given dependent variable. In policy research we have to be concerned not only with how certain causal variables might affect the dependent variable of policy interest, but also with the influence that changes in the independent variables (and the dependent varia-

ble) may have on other attributes in the society as well. Unless this is done in policy research, we are just as irresponsible as a drug company that neglects to study a new medication's side effects. Yet one rarely encounters discussions of the possible side effects of specific policy proposals. If we know enough from the analysis of a data set to determine the social consequences of a given policy, should we not also expect some understanding of the policy's influence on other societal phenomena? Likewise, if we have enough theoretical knowledge about how to alter a given societal condition, it is reasonable to expect information on side effects as well. Should the standards for social research be lower than the standards we have for drug companies?

Ad hominem arguments permeate the thinking of social scientists.

Second, in *Making It Count,* I suggested that social events have lives of their own; so that if the original cause of a given dependent variable were to return to an earlier state, it would not necessarily lead to a similar shift in the dependent variable. For example, if the variable(s) that caused the women's movement were to return to an earlier state, it is unlikely that the women's movement would disappear. If this proves to be the case—and I suggested ways of testing that proposition—then the usual research approach will not be a satisfactory way of addressing policy issues. Normally, we ask what independent variables affect the observed dependent variable; but this is of relevance only when those linkages are what could be called "reversible." In situations in which the association with X is totally or largely irreversible, then policies based on how X caused the current level of Y will hold little help for understanding how to change Y since shifts in X back to an earlier state will have little or no impact on Y. In the area of race and ethnic relations, for example, I am impressed with how critical this factor is. All too often it is assumed, in the most cavalier fashion, that to understand the causes of some of our most serious and pressing problems means simultaneously learning how to remove these problems. This could be true not merely if the analysis of the causes of current conditions is correct, but also if such causal links are totally or largely reversible—a separate question.

Social science can and must make important contributions to social issues and social policy. We know about the need to guard against interference from the larger society if our results appear unacceptable to influential segments who sometimes find the conclusions too radical, or who fear that our results will provide grounds for introducing changes in the social order, or who simply fear that the results challenge beliefs that are held sacred by some. We are alert to this issue and the importance of defending ourselves from such assaults. But we are apt to overlook another danger, namely when we ourselves insist that our social research generate certain results that are somehow defined as appropriate and politically correct. We have a set of illogical but punishing conclusions about those whose results do not meet certain predetermined notions. We also are a bit too prone to think that any question asked can be answered with the information that we are presently capable of obtaining. We are also too willing to allow socially and politically relevant subjects to be studied in the form in which the society states the questions, rather than by the way our knowledge tells us to approach the problem. On the other hand, because of this overly ambitious and overly politicized agenda when we deal with subjects pertaining to national controversy and concern—whether that be gender, abortion, poverty, power, education, the family, crime and delinquency, race and ethnic relations, political institutions, and the like—we do not expect enough from our scholars in the way of intellectual integrity such that they approach the data as best they can.

We tend to accept or not accept a result because of its possible political and/or social implications; but that is irrelevant to the job at hand. Or if it is to be defined as relevant to the job at hand, then what does that mean our discipline is? A lawyer or an advertising agency is paid to compile all of the possible evidence to support one position or another. That is understood and we would certainly fire an attorney who defended us by indicating there are twelve reasons for thinking we are inocent but nine reasons for thinking we are guilty. We cannot have our cake and eat it too; asking ourselves to present "good" results but suppress "bad" results, and act as if those reporting the latter have certain suspicious motivations. If we are truth-seekers, then the results are what they are, pure and simple, like it or not.□

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