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PSYCHOLOGY OF THE SCIENTIST: LXXVIII. RELEVANCE OF A SCIENTIST'S IDEOLOGY IN COMMUNAL RECOGNITION OF SCIENTIFIC MERIT[1]

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Summary.—A scientist's work product can usually be rationally appraised apart from ideology. "Bad science" includes formal and material fallacies and poor data; nonoptimal judgment calls and theorizing refuted by later knowledge do not count. In bestowing encomiums, a scientific organization with multiple goals may consider—with caution and wide tolerance—social impact where ideological applications abhorrent to most members or unethical conduct *qua* scientist become relevant. Members valuing only scientific merit may either effect policy change within the organization or join another. A scientist who extrapolates scientific theory to make social policy judgments should emphasize the limitations of technical expertise in these matters.

What relevance does a scientist's ideology and its relation to a scientist's work have to the appropriateness of a scientific award? When are flaws in an intellectual work product not minor and peripheral, but major and central, so cognitively erroneous, ethically evil, and socially dangerous that a scientist should not be awarded a prize for other achievements of high scholarly merit? When does ideology warrant a label of "bad science"? A recent episode[2] suggests that there is considerable lack of clarity about basic philosophical issues and general principles involved in such an assessment. I do not claim to have clear answers to all of the issues one must confront in thinking about such a matter, but an analytic parsing is a precondition for rational judgment, and this article is a summary of my efforts.

By the phrase "bad science" I take it we refer to procedural errors in one's scientific work leading to major errors of substance. Some kinds of errors are plain *mistakes* that are sufficiently clear-cut and objectively identifiable that

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[1] I am grateful to Leslie J. Yonce for assistance in preparing this article and to the reviewers, whose comments led to what I hope are substantial improvements in the exposition.

[2] The eminent psychologist, Raymond B. Cattell, was awarded the American Psychological Foundation's Gold Medal for Lifetime Achievement in Psychological Science, but heated controversy was generated when the presentation ceremony was cancelled by officials (with the agreement of Cattell and his family) and an *ad hoc* committee appointed to investigate ideological allegations made against Cattell. With Cattell's decease, the *ad hoc* committee was discharged and the case became moot. This episode is the precipitator for reflections on a topic of intrinsic philosophical interest and one that, in these days of political correctness, is likely to surface again in specific cases. *This article is not about the Cattell case but about a general question it exemplifies.*

questions of ideology do not arise. Examples are invalid mathematical derivations, formal and material fallacies in logic, calculational errors, inappropriate statistics, unreliable measuring instruments, misattribution, misquotation, distorted restatement, falsification of the intellectual history, failure to give credit, and biased literature review (see Good, 1959; Hamblin, 1970; Meehl, 1997a, 1992, pp. 353-354, references p. 352, fn. 10, p. 355; Pitkin, 1932; Reason, 1990; Sutherland, 1994; Thouless & Thouless, 1990). These references show that the common errors in reasoning are not typically those discussed in old-fashioned logic texts inspired by Aristotle and the medieval logicians, e.g., undistributed middle, illicit major, denying the antecedent, *argumentum ad hominem, argumentum ad verecundiam*, composition, *post hoc ergo propter hoc.* Although I have not done a content analysis, I am pretty sure that the most frequent fallacy in scientific reasoning is one that most logic textbooks do not even name, to wit, violating the Total Evidence Rule. In scientific research, the typical epistemic situation presents factual evidence apparently pointing in opposite directions, "good arguments on both sides." A common error is to treat the favorable evidence not merely as probative (tending to prove) but as dispositive (settling the question conclusively), or at least to assign the adverse evidence insufficient weight. The result is a matter of biased factual bookkeeping.[3] Another common error is ambiguity in the slippery term 'proof,' whose seven meanings (see Meehl, 1992, p. 356) make it a projective technique and a dangerous weapon in scientific controversy, often wielded unscrupulously.

An over-all assessment of an individual scientist as one who is guilty of bad science depends upon how often these fairly clear-cut errors are made, whether they are big or little in their substantive consequences, and—very importantly—on those occasions when such an error is pointed out, whether the scientist retracts it or persists. Scientific work products are like achievement test items in that, while it may be possible to arrive at a highly reliable intersubjective classification of a single piece of work (even a single sentence in an article) as being erroneous, to what extent this justifies labeling the *individual* as a "bad scientist" depends upon the frequency and gravity of the errors and the scientist's stubbornness in adhering to them despite clear proof of error by the scientific community.

A second class of scientific errors is composed of unwise judgment calls, usually discerned with hindsight. Here, we must be careful because, unlike the preceding class of plain mistakes, that a judgment call was not only nonoptimal but "unwise" is itself a metatheoretical judgment call that *we* are making. There is no sharp dividing line between the first class of errors ("fallacies") and this class, barring an unconventional stipulation that 'fallacy' designates only a

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[3] The textbook I read passionately and studied assiduously at age 16, Alburey Castell's *A College Logic* (1935), is one of the few that provides a name for this material fallacy, calling it "Neglected Aspect."

formal invalidity. Material fallacies often involve a component of unwise judgment, e.g., how false must an analogy be to constitute the fallacy of "False Analogy," how small a sample to constitute "Hasty Generalization," how different the application of a term to two particulars to be an "Equivocation"?; hence a few very bad judgment calls could equally well be put in the previous class ("fallacies"), whereas many complex, debatable, hindsight only, non-optimal judgment calls we (properly) do not label "fallacies" but only "mistakes." References relevant here are therefore also relevant to portions of the first class of errors (Dawes, 1988; Faust, 1984; Faust & Meehl, 1992; Hogarth, 1987; Mayo, 1996; Meehl, 1990a, 1990b, 1997a; Nisbett & Ross, 1980; Pious, 1993). The reason why a competent scientist may make a nonoptimal judgment call without committing clear errors (plain mistakes, bloopers, fallacies) is that *there is no algorithm for ampliative inference,* no formal procedure for making "the best possible inference" in inductive logic. An "ampliative inference" is one in which the conclusion has semantic content not contained in the premises. Most logicians think that there never can be such an algorithm, but whether or not they are correct in this prophecy, none presently exists. To my knowledge, there is only one empirical domain in which there are algorithms *attempting* to objectify ampliative inference, and that is inferential statistics, wherein we infer the value of a parameter from the statistics of a sample. But even there—considering procedures such as zeroing a partial derivative to obtain the "best" estimator, applying Bayes's theorem, or conducting a significance test—disagreements among the statisticians have persisted for more than two generations. Since there is no agreed-upon algorithm even for this special (mathematical, formalized) subdivision of ampliative inference, *a fortiori* we don't have one for the generic class of evaluating substantive theories, e.g., the probability of Freud's theory of dreams, or Meehl's theory of schizotaxia, given a heap of qualitatively diverse bits of empirical evidence (Meehl, 1997b).

The third kind of scientific error is not an error of procedure at all, either in the strong sense of the first class or the weaker sense of the second class, but arises from the human epistemic situation: we are always in the position of making inferences without having sampled all of the facts in the universe.[4] Thus, even an ideal scientist, who avoids all procedural errors as strictly defined and always makes what the scientific community with hindsight considers wise judgment calls, will nevertheless make substantive mistakes because, at any given time, the scientific community's empirical knowledge of the facts is incomplete. Scientists are sometimes in the position of people who decide to

[4] My discussion presupposes finding truth as the aim of science—that there exist objective facts independent of our knowing them, that some reported observations ("protocols") are correct and others not, that some theoretical propositions are true and others false, and, hence, that scientific theories differ as to verisimilitude (an *ontological,* not an epistemological metapredicate). Whether or not one can define 'bad science' without this presupposition I do not consider here; but I doubt it.

buy life insurance with no possible way of foreseeing that they will live to be 103, predeceased by spouse and children; they make a rational bet with the insurance company, but they will lose. The human epistemological predicament is what gives rise to Sir Karl Popper's famous dictum that the history of scientific progress is a record of corrected mistakes. The vast literature of epistemology and philosophy of science is largely about this type of error (chiefly how to detect and minimize it). A few works that I have found illuminating are Earman (1983), Feigl (1981), Glymour (1980), Haack (1993), Humphreys (1989), Lakatos (1970), Laudan (1977, 1984), Mayo (1996), Nagel (1961), Pap (1962), Popper (1962, 1983), Reichenbach (1938), Russell (1948), Salmon (1990), Trout (1998), and Watkins (1984).[5]

Note that all of the above errors (the first two procedural, the third substantive but procedurally unavoidable) can be identified without reference to an ideology held by the scientist who makes the mistakes. This is fortunate for any committee sitting in judgment on a scientist's merits because (a) there are no agreed-upon criteria for appraising the reprehensibility of an ideology (short of extreme examples unlikely to arise), and (b) if a clearly reprehensible ideology seems to be held by the individual, there is the further problematic inference that it was a major source of the scientist's egregious errors. There are, to be sure, a few notorious instances where this strong inference is warranted. The German Nobel laureate Philipp Lenard condemned the general theory of relativity, despite the evidence in its favor, partly on the grounds that it was "Jewish physics," and in 1936, he published a book on "German physics" whose preface contained harsh anti-Semitism. His student, Johannes Stark—another Nobel laureate—took the same position and was sentenced to four years' imprisonment by a de-Nazification court (although the sentence was commuted). Less malignant but still disappointing, the eminent French physicist

[5] Younger readers brainwashed by the positivist-bashers may wonder at my inclusion of a couple of (reformed) logical empiricists in the list. That no one today takes the overly optimistic triumphalist position of the famous manifesto (Carnap, Hahn, & Neurath, 1929) does not mean that the logical positivists and their critical allies had nothing valid and valuable to say. Some of their conceptual analyses and metatheoretical arguments are here to stay, and truths do not become untruths merely because they were uttered 60 years ago. The great movement begun in Berlin and Vienna developed as they hoped, in the way science itself develops, by correcting its mistakes. (The important reformations here came largely from within rather than from their opponents.) Over some 30 years I was privileged to observe my mentor and then colleague Herbert Feigl's evolution from (tolerant) logical positivism to logical empiricism to empirical realism, and I moved with him. These men were heavyweights, and they taught us much that was permanent and useful. My respectable scientific career is partly attributable to having learned their lessons. A psychologist with methodological worries who ignores the logical empiricists and other analytic philosophers in favor of lightweights' faddish pseudophilosophy is making a grave mistake.

Pierre Duhem (1914/1991), in what was otherwise a very high-quality book, aimed sarcasm at British physicists for their fondness for mechanical models: "We thought we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory" (p. 71). Such clear examples are hard to come by, and the majority of scientific errors of procedure or substance are either unmotivated (Faust, 1984; Nisbett & Ross, 1980) or, at most, have been influenced to a degree we cannot accurately quantify by subtler biases, e.g., dislike for one's PhD advisor, unpleasant experiences with an Adlerian marriage counselor, sentimental anthropomorphism toward white rats, having gotten a C in high school algebra. Ordinary scientific conservatism—a statistically rational policy because there are more deviant cranks than deviant geniuses—plays a major role in what history shows to have been "negative mistakes" (Barber, 1961).

I employ the word "ideology" broadly, not in the original (narrower) sense of Karl Marx and Friedrich Engels or of Karl Mannheim but to include ethical, religious, ethnic, political, class, and economic belief systems and lifestyle orientations. Ideologies are a fusion of primary value commitments and empirical conjectures that may or may not be supported by scientific or anecdotal evidence but which are far from conclusively proven. A person's "ideological complex"—like the complexes of psychopathology—usually involves bidirectional causal arrows between cognitive, affective, and conative components.[6] We can conclude for an accusation of "bad science" on the demerits of the scientific work product itself. That this clearly bad science was *motivated by* or *employed to support* an unsavory ideology then would constitute additional probanda, but these can be set aside when we are in the business of awarding scientific prizes. It would be odd for a committee to condemn a scientist for a track record of excellent work products, free of procedural errors, because we had suspected that this "good science" was ideologically motivated.

Finally, we have the situation of a scientist whose work product is praiseworthy and who does not hold a clearly reprehensible ideology but who *acts unethically as a scientist.* Examples would include writing an article based on a student's remarks in a seminar without asking the student to be co-author, regularly voting to fail Irish PhD candidates, ridiculing students of one gender or the other for classroom contributions, denigrating scientists with whom one disagrees, peculating research funds, or terminating therapy patients who decline to participate in an outcome study. The crucial point about these

[6] O1d Aristotle's model of the mind was not so bad, at its level of description.

examples is that they are violations of ethics, or the positive law of the state, *while one is functioning in the social role of scientist.* A routine practice of demanding sexual favors from research assistants is arguably relevant to whether a person should be considered a good *scientist,* given that his scientific work product is good, but having an adulterous affair with the next door neighbor, something outside one's role *qua* scientist, is irrelevant to awarding a prize for scientific attainments. A paradigm case is that of Nazi scientists immersing Jews and gypsies in ice water to ascertain time to die, fatal temperatures, and treatment efficacy. As controlled experiments, some of these studies were technically sound, but would we bestow a prize on such evil persons? We would not even consider doing so.[7]

Which of these errors in scientific work product or wrongdoings in scientific conduct should be taken into account in deciding whether to award a prize for otherwise distinguished scientific achievement? I would count scientific errors of the first kind, if they are sufficiently numerous and serious or stubbornly persisted in without answering clearly valid criticisms. But in such cases a person would hardly survive the first screening as candidate for a prize. On the other hand, no informed person thinks that committing *any* of these errors, however few and sincerely retracted, would be grounds for rejection. Of the seven towering intellects who created quantum mechanics (Planck, Einstein, Bohr, de Broglie, Heisenberg, Schrödinger, and Dirac), I believe every one of them committed mathematical errors. Einstein did it several times, including issuing erroneous statements to the press that he had finally solved the unified field problem. A criterion of procedural perfection that would condemn seven Nobel laureates in the best science we have is surely absurd, and especially so if we apply it to a rather primitive science like psychology. Errors of the second kind, nonoptimal judgment calls, and of the third kind, due to the inherent incompleteness of human scientific knowledge, are obviously inappropriate grounds for rejection.

As to an unacceptable ideology, if it results in bad science, we can decide the latter on the merits as I explained above. If it doesn't result in bad science or in unethical or illegal conduct, I don't know what to say. I consider this a deep, complex question in political theory and social ethics. My own predilection would be for having extremely wide (but not unlimited) tolerance. If a scientist advocated in lectures, articles, or books that all infants born to parents with IQs

[7] The PhD physiologists, bound by no medical oath, grossly violated basic ethical principles; the physicians did that and violated the Hippocratic oath they had taken, not to destroy human life but to preserve it. The 'good' in 'good *science*' is an adjective modifying the work product. The 'good' in 'good *scientist*' characterizes jointly the work product and the individual, at least on one defensible interpretation. An evaluator who employs the phrase 'good scientist' in this conjunctive manner should clearly say so. But a scientific purist who says that the phrase can *only* characterize the work product is semantically dogmatic.

below 90 should be exposed to the elements on a mountain side, I would not want that person to receive any awards or prizes. But note well: I use this extreme example to highlight the point that, although passing judgment on the social dangerousness or moral wickedness of somebody's ideology is at times a legitimate procedure, one must approach it with humility, intellectual scrupulousness, and full awareness of the inherent doubtfulness of most of our moral and political opinions. One fairly objective criterion might be required: when a social or biological scientist makes problematic and controversial inferences of an ideological sort, such a scientist has a strict obligation, in writing for a general readership, to make it clear that, while having some expertise *qua* scientist about the empirical matters relied on in the ideological argument, the scientist is here occupying a double role.

Are there some ideological stances that are so socially dangerous that they should not be published? This involves complicated issues regarding freedom of speech and press, to do justice to which would require much more space. Informed, scholarly, rational arguments do exist counter to the strong libertarian view (e.g., Sir James Fitzjames Stephen's [1873/1991] powerful critique of Mill's [1859] essay *On Liberty*), but this is not an appropriate forum for discussion of that deep question (Cass, 1987; Sunstein, 1993).[8] The "hard case" of Hitler's freedom of speech is badly chosen by antilibertarians, inasmuch as his effective speechifying pre-1933 was against the Versailles treaty and the world communist conspiracy, largely valid; and—free speech aside—after the beer hall putsch he could have been imprisoned for a long term or deported to his native Austria as an alien. The problem with Hitler was not free speech but the feckless pusillanimity of the Weimar government and the rightwing bias of its officials.

Almost all groups of persons who form organizations do so with more than one purpose in mind, the multiplicity of socially defined goals being sometimes explicitly stated, e.g., in the Preamble of the U.S. Constitution, sometimes clearly discernible in an organization's provisions and bylaws, and unfortunately, sometimes only in the form of hidden agenda. Furthermore, especially in the case of hidden agenda, the individual members may be unconscious of

[8] These writings are by law professors dealing with constitutional law, how the courts have interpreted the First Amendment's protection of free speech and press. Of course, a private scholarly organization's discretionary awarding of a prize is not a constitutional matter. These discussions (and copious references therein) include careful philosophical analysis of the *underlying rational grounds*—moral, political, psychological, economic—for protecting free expression of ideas. Psychologists who hold a naive and unexamined dogma that *everyone* must be free to say or print absolutely *anything* under *all* circumstances will be cured in a hurry by even slight dipping into this scholarly literature on a deep, complex, subtle, and highly debatable issue. Our relatively free society formally recognizes at least a dozen kinds of "unprotected speech" and enforces penalties (civil damages or criminal punishments) against the speakers. I know of no lawyers, political scientists, or moral philosophers who disagree.

certain of their own motives. The social forces that motivate forming organizations and the imperfections of the human mind are such that no guarantee exists that these multiple aims, overt or latent, will never contradict each other. The collision of aims need not be a *prima facie* contradiction; it can be one that surfaces only in the presence of certain social facts. In this respect, organizational aims are analogous to ethical postulate sets, in which two generic ethical principles may not appear *prima facie* contradictory as stated in the abstract, but moral agents are sometimes confronted with ethical dilemmas in concrete situations wherein application of two theorems, each validly derivable from the primitive ethical postulates, prescribe incompatible actions. In ethical theory, this has given rise to the concept of a "preference rule" (Ross, 1930). A satisfactory ethical system is probably impossible without shifting from the usual qualitative formulation of moral principles to a quantitative structure such as used by economists and some political scientists.

I exemplify by a trivial and unimportant example, *chosen as such* with the hope that readers will be capable of objectivity and not suspect me of grinding some ideological axe. Consider the Siamese Cat Club. One might think it has only a single aim, or, at any rate, multiple aims that are invariably coherent. I can discern from glancing at publications in the veterinarian's office at least four distinct aims:

1. To preserve and improve the Siamese variety's gene pool;
2. To enhance the health and comfort of the Siamese cat;
3. To enhance the pleasure of the cat owners;
4. To increase the prestige, popularity, and attention given to the breed.

Prima facie these are, while distinct and not mutually interderivable principles, not inconsistent. It is equally clear that under certain empirical fact conditions, they may collide. Thus, it has taken a long time to eliminate from the gene pool a mutation conducing to strabismus and another conducing to an almost invisible tail kink, each disqualifying a cat from being of show quality. Suppose a cat lover would like his crossed-eyed female to bear young, because he attributes to her a "maternal instinct" and because he thinks it would be educational for his children to watch a mother cat caring for her offspring. This owner is confronted with a conflict produced by the conjunction of goals (1), (2), (3), and the fact situation, i.e., the existence of an undesirable mutation.

These kinds of problems are not unusual in social groups organized for various purposes. On the contrary, they are the norm. Hardly anybody who joins a political party can subscribe wholeheartedly to every one of its numerous planks. Hardly anybody who joins a religious denomination sincerely believes all of its doctrines. In the case of a scientific society, it may happen that the stated aim of advancing and disseminating scientific knowledge conflicts with some other aim, stated or covert. (Note that even the advancement and dissemination of scientific knowledge are sometimes in conflict, because dollars used to subsidize research cannot be used to pay for publication.) Presumably, the bestowal of awards for meritorious scientific research is subsidiary to the over-arching primary aim of advancing and disseminating scientific knowledge. With respect to that aim, nothing about a scientist's ideology, either as motivating the research or as purportedly supported by the research, and nothing about the scientist's ethical behavior *qua* scientist is relevant. Nor is the possible social impact, good or bad, of the scientist's facts or theories relevant. That seems to be the position taken by many, perhaps most, of my most esteemed colleagues in psychology.

What should one do if a prize committee, while taking the advancement and dissemination of scientific knowledge as the primary organizational aim, nevertheless, in deciding whom to reward and present to young scientists as a role model, takes as negative evidence some extreme of ideological influence, ideological misapplication of scientific knowledge, or personal unethical conduct *qua* scientist? If I individually cannot accept any competing purpose as an aim of the organization and for that reason believe that these considerations should play *zero* role in awarding prizes, what solutions exist for me? I can attempt to effect change in the policy, meanwhile making strong representations against the action in the instant case; and if I fail at these, I can quit the organization and join or found one which has only the monolithic scientific aim. This is inconvenient and irksome for many persons, myself included.[9] But my dilemma is not a consequence of somebody's incompetence or wickedness, it is intrinsic to social organizations having multiple goals. It is worth pointing out that such dilemmas are an unavoidable consequence of living in a political democracy. I cannot be said to "govern myself" when I am in the 49% minority that opposes, say, a foolish or immoral war.

I do not see any clear way to "fault" a scholarly organization on the ground that it cherishes a mixture of aims. Surely nobody thinks that the Society of Black Psychologists, or the APA division interested in gay and lesbian issues, or the Society for the Study of Social Issues (SPSSI), or the American Catholic Psychological Association, have absolutely no interest in anything except ascertaining scientific facts and propounding empirically warranted scientific

[9] I asked, anonymously, 16 colleagues whom I have known long and intimately enough to be sure they are first-class intellects, major research producers, clear thinkers, and not fanatics ("left" or "right") whether they would join, or remain in, APA if it adopted, officially or in practice, a policy that some facts or theories well supported by facts are so socially dangerous that they ought not to be widely expressed. Eleven said "No," 2 said "Yes" (one of them "to effect policy change"), and 3 did not reply. This tally is about what I predicted, as I believe any rational informed psychologist would have done.

theories. If, however, the organization's stated goals and the official guidelines (not *rules*!) of awards committees say or imply nothing about ideology as a motivator or as an inferred application, then it is rational and appropriate to criticize it because those acting in authority are acting at least mistakenly, perhaps not in good faith, and in clear cases, acting beyond their authorized powers. The main things to be clear about here are the distinctions between forbidding an organization to have multiple goals, which is irrational; criticizing it for not acting in accordance with its stated goals; and deciding not to remain in an organization for either of these reasons. I know individuals who joined the Psychonomic Society, or the American Association of Applied and Preventive Psychology, or the American Psychological Society, meanwhile either remaining in the APA or resigning from it, for such reasons.

That APA has multiple aims is explicit in its constitution. The influence of ideology (obvious or latent) is discernible in some of its actions. I offer one recent and clear example of which I have direct personal experience. In 1997, Division 5 conferred awards on some two dozen elder psychologists, commending their lifetime achievements in contribution to scientific knowledge and applications of evaluation, measurement, and statistics. I was pleased to be one of the recipients and like to think I deserved the award; but I was also a bit embarrassed. There was one name conspicuously missing from the list, someone whose contributions, in both quality and quantity, certainly excelled mine, namely, Arthur Jensen. At least a third, and arguably the majority, of the recipients would have to say that about themselves in relation to Jensen. No informed rational mind can have the slightest doubt as to the explanation of this distressing social phenomenon: Arthur Jensen's facts are unpleasant to face, and his theoretical inferences from the facts are politically incorrect.

Multiple aims set by a social organization are analogous to the mixed motives and multiple social roles of individuals, and the latter may rationally influence the person's adherence to the former. Such locutions as "The scientist *qua* scientist...," *"Speaking as* a taxpayer...," "From a parental *standpoint*...," "With my *socialist hat on*..." presuppose this multiplicity of social roles, and there is no logical contradiction or semantic confusion in occupying and affirming them. It is psychologically possible and ethically licit to distinguish beliefs and actions that are consonant with one role from those that are not but which do "fit" another role. But making this distinction does not always suffice to conclude for or against a contemplated stance, action, or group membership. That I occupy several roles does not imply that I am several persons. It cannot—there is only one single person, I, who am a moral agent, forced to take decisions in various contexts that *specify* which roles are relevant. The trouble is that I do not have complete freedom in this respect. For example, in voting on a tax increase or speaking at a P.T.A. meeting, I cannot properly argue only "*As a taxpayer*. . ." thereby disconnecting my role as a parent, if I am a parent. Nor can I properly argue only "*As a parent*. . . ." Nor may I require others to adopt only one role. This sort of situation involves the moral philosophers' preference rules, telling a moral agent which of two *prima facie* duties (prescribing incompatible actions in a given situation) should prevail.

How apply these considerations to the bestowal of a research award? Qualitatively, the answer is obvious: If a scientific society has no stated competing aims, its sole purpose being the advancement and dissemination of scientific knowledge, then the scientist's ideology, acceptable or loathsome, is irrelevant. If I as an individual scientist joined the organization with that understanding (relying on the explicit language of its constitution), I have cause for complaint if an awards committee pays attention to ideology, pro or con. I should try to alter the policy or quit the organization. But we must be evenhanded about this, however scientifically "pure" our individual motives may be. Suppose the organization has stated multiple goals, such that a scientist's use of theoretical inferences to bolster a socially impactful ideological position conflicts with a major goal. The organization's officers or awards committees, if acting in good faith and consistently with the constitution, cannot be faulted on the ground that I, as individual member, would prefer to give the competing aim less weight than they did. It is a judgment call, where the absence of objective weights precludes fault-finding except in extreme cases. If I always opt to wear my "pure scientist" hat, I should join, or found, a scientific organization that explicitly specifies either that scientific truth is its only aim or that it always prevails over other "secondary" aims when there is conflict.

When an ideology produces defective science, the science can be evaluated on the merits; we need not even know about the scientist's ideology. When science—good or bad—is invalidly used to bolster an ideology or fused with it to yield a social misapplication, we need to know that in appraising the scientist's total social impact. But even here our *evaluation* of the ideology need not always enter because the misderivation or misapplication is a *cognitive* error and could fairly be called "bad science," whether or not we like the ideology.[10]

But a scientific purist might object, "No, by 'bad science' *I* mean *only* the facts and theories of the science itself, I don't care about pragmatic inferences

[10] In ethical, religious, and political matters, informed and sophisticated persons often disdain weak arguments offered by the less competent on their side. A truism among politicians is "I have more to fear from my friends than from my enemies." This applies to the Cattell controversy, judging from the massive e-mail exchanges one sees. (It also illustrates Mencken's dictum that people believe what they want to believe. Nobody is persuading anybody.)

from it." Those are the purists' semantics, to which they are entitled, but they cannot impose them on everyone else. How do we avoid this semantic impasse? (As Sir Karl Popper said, debating the meaning of words is one of the most useless of philosophical activities.) Very simply. Whether "science" is taken to include only facts and theories or their applications as well (which may be *cognitively* challenged) is not debated but is *stipulated* on the basis of the organization's goals. There is no "correct" or "incorrect" word meaning to dispute over; there is simply a question of shared or unshared purposes. When it comes to awarding prizes, always a judgment call on a matter of degree, should the group's abhorrence of an interfering or misapplied ideology be taken into *any* account? I do not know, but if at all, only at extremes, e.g., Nazis.

Objection: "Well, all right, you make a plausible case for an organization sometimes paying attention to ideology or conduct in bestowing awards. I am not so 'scientifically purist' as to deny that this is ever, *ever* conceivably appropriate, as with the Nazi doctors. But where do you draw the line? Once you start allowing this stuff in, what's the stopping point? You begin with clear, Quaker-consensus, shocking, abominable examples, but the principle could extend to less and less clear ones. Others, relying on such principles, will go on to reject unpopular genius contributors; and then to suppress facts or theories because a few people find them mildly offensive." Yes, indeed, there is that danger. It is a grave danger. No one who loves science, leads the life of the mind, prizes liberty and autonomy, should ever dismiss that concern or treat it lightly. Jefferson had it right, that eternal vigilance is the price of freedom. The Thought Police are always lurking. Most people who *say* they believe in freedom of expression do not actually when it comes down to specific ideas they dislike. I know all that, and I hope never to forget it.[11]

But it is not a logically necessary or sociologically inevitable development. The possibility of such insidious and dangerous extension is underestimated by fanatics,[12] while its intractability is overestimated by zealots of the ACLU type. The objection states a valid and important concern, but it is not dispositive (see Footnote 8, p. 1129, and associated text). In form it is known to lawyers, political scientists, and moral philosophers as the *slippery slope* argument, itself so slippery and widely abused that some logicians call it the "slippery slope *fallacy"* although it isn't always fallacious. The big questions are when it is valid, and, if valid, what *weight* it deserves in a given moral, legal, or political context. The involved considerations present terrible difficulties, such that

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[11] In the 1940s, I read of a Marxist psychologist who published a paper opposing all research on the Stanford-Binet because it was counter-revolutionary, favoring the exploiting class.

[12] I cannot improve briefly on Webster's definition of 'fanatic,' except to recommend reading a profoundly insightful book by a longshoreman devoid of academic credentials, Eric Hoffer's *The True Believer* (1951).

numerous scholarly articles and at least two whole books have been devoted to its analysis (Lamb, 1988; Walton, 1992). I confine myself to two points that are not disputed. First, not all slippery slopes are equally slippery. Second, just where on the slope the toeholds and pitons should be located, while somewhat conventional, is not completely arbitrary. Some placements (too soon, or too late) are indefensible. We must remember Edmund Burke's dictum that the existence of twilight leaves night and day tolerably distinguishable. A good working rule is "When in doubt, allow!", i.e., do not condemn a scientist because of ideology. I would comfortably withhold encomiums from a physicist (Lenard) who argued that relativity theory was false because a Jew invented it. Doing this does not require or permit me to do the same against Duhem for poking mild fun at British mechanism. Just as not all literally erroneous theories are equally in error ("all theories are lies"—and *that's* not exactly true either), not all ideological sins are mortal. The slippery slope objection is strongly probative, but it is not dispositive. When public utility lawyers argued from Marshall's "the power to tax is the power to destroy," envisaging a slippery slope from a state's 5% tax to a confiscatory tax of 50%, Justice Holmes rebutted it with, "Not while this Court sits." That's a good reply, unless the members of the Court are stupid, uninformed, irrational, unethical, pusillanimous, or popularity-seeking. Ditto for a scientific organization's awards committee.

But what if the "court" *is* terribly defective in one or more of these ways? Then injustices will probably occur. If we require infallible cognitions and flawless integrity, we can shut up shop, as human affairs cannot be conducted. As Madison said in the *Federalist Papers,* if men were all angels, constitutions would not be needed. If they were all devils, constitutions would be worthless.[13] Principles, policies, and rules are applied by *persons,* they are not self-enforcing. Suppose, for an extreme case, the members of a scientific society fear the slippery slope so strongly that the society's constitution or by-laws state explicitly (as a *rule,* not a mere guideline), "In awarding prizes for scientific achievement, no consideration shall be given to the scientist's ideology, either as influencer of the scientific research or as allegedly supported by the research." The tokens of this sentence consist of "mounds of ink" (Neurath) on a page. Mounds of ink cannot coerce anybody to do or refrain from doing anything. If the awards committee lacks competence or integrity, can the ink mounds force them? Of course not. Can anyone else ascertain for sure that they were, or were not, subtly influenced by ideology? No. Admittedly a flat, clear, strong rule may make it more difficult for a "bad" committee to start the slope

[13] Tyrant Stalin's "democratic" constitution of 1936 appeared the same year as the first of the fake Moscow showcase trials.

slide. But if they do, the society's members will be able to repudiate them only if *enough of them* see clearly and reject vigorously what is happening. The political scientist's *Quis custodiet ipsos custodes?* is an insoluble problem if no decision makers can be trusted to do their job. [Radical societal revision, rather than misapplication, of cognitive and ethical norms is discussed in the Addendum (pp. 1138–1143).]

Those of us who passionately cathect the life of the mind and consider its untrammeled exploration and free expression necessary to its high quality, instinctively fear the Thought Police and notice even small movements toward the ant society. That value system, if adhered to consistently, demands intellectual fairness. A scientist does not lose the normal right to speculate publicly about social problems, crime, economic dependency, income inequality, large-scale human relations, international affairs, war, taxation, trade unions, education, and eugenics. But it is obligatory for a scientist to make explicit to readers that these ethicopolitical conjectures and proposals are offered as a concerned citizen of the world and are not direct deliverances deducible within the science proper. It would be foolish to hold that everyone *except* a physicist may think or write about nuclear waste, or that everyone *except* a biologist may think or write about endangered species. But it is an obligation of scholarly writing to assure that readers do not receive *any* impression that the ideological ideas expressed carry the full expert authority that we usually attach to strictly intrascientific expert opinion. A scientist's book should state loudly and clearly, in its introduction or first chapter, that pragmatic recommendations about starvation in developing countries, or governmental influence on family size, or use of the taxing power to straighten out the Lorenz curve of income inequality, or the direction of expenditures on education, or economic policies concerning marginal workers are, by their very nature, evaluative judgment calls. They may be *suggested* by psychological and social theories (which are in turn only corroborated but not conclusively demonstrated by statistical methods such as factor analysis); but they are not themselves part of the theoretical science, nor are they direct, immediate technological consequences of theoretical science. Hence the scientist's technical expertise and the "authority" that nonexperts (reasonably and unavoidably) ascribe to it do not fully apply, and any such implication ought to be explicitly disclaimed.[14]

[14] I have an article in preparation on a "psychologist's Utopia," suggesting radical changes in our democratic polity. I attempt to integrate scientific knowledge from several domains: psychodynamics, trait theory, psychometrics (areas of my expertise); genetics, sociobiology, primate ethology (areas in which I claim marginal competence); political science, biography, history (relying on others' expertise). The first paragraph will make clear that I am *not* technically qualified in history, economics, or political science but am a psychologist-citizen who is reflecting on these deep and important questions.

When the profession bestows an award, it intends to give pleasure to the person, highlight the achievement, and identify a role model for young scientists to emulate. It is arguably appropriate to appraise the person's over-all impact, including all domains addressed by the person in a scientific capacity. I assert this with uncomfortable awareness that it will be tendentiously used by the Thought Police. I cannot help that.

RESUMÉ

1. Receiving a prize for one's scientific work is not a right but a privilege, bestowed, in discretion, by a scientific society to reward the individual, express group approval, and point to a role model.

2. A candidate's ideology may properly be taken into account as a negative factor only under three (empirically unusual) conditions:

a. The ideology influences the candidate to commit scientific errors which are major or numerous and unretracted despite being clearly shown.

b. The scientist misapplies scientific findings to support an ideology fallaciously or without clearly stating that the extrapolation is not strictly part of, or deducible from, the scientific content proper and that the ethical, religious, metaphysical, or societal theses advocated should not be weighted on the basis of the scientist's scientific attainments or scientific credibility.

c. The scientist, when functioning in a scientific capacity, commits clear and gross violations of law or (quasi-universally accepted) ethics.

3. In case (a), since the scientific errors are discernible on the merits, it is unnecessary to be able to appraise the ideology's quantitative influence; its role is inferentially causal but the resulting "bad science," by itself, speaks against a prize.

4. Mistakes in theorizing or theory appraisal arising from nonoptimal judgment calls (rather than clear, major, unretracted fallacies) do not speak against awarding an otherwise deserved prize except in the quantitative sense of "total track record," which of course is what an awards committee considers. Some of the greatest scientists have made poor judgment calls, and hardly anyone (Einstein included) could get a Nobel Prize if that sort of "mistake" were a bar.

5. Highly subjective judgment calls that ultimately turn out to have been objectively incorrect, given the subsequent development of new evidence and argument, are inherent in our epistemic predicament and are irrelevant to awarding prizes for otherwise distinguished scientific achievement.

6. Whether there are a few observational facts or well-tested theoretical truths that are so socially dangerous that one is ethically obligated to refrain from communicating them is a hard, deep ethicopolitical question. A scientific society, taking discovery and disseminating truth as its prime concern, should be extremely wary of such an idea. What if a scientific society collectively

decides that such extreme cases do, however rarely, arise? Such a society's awards committee would, on occasion, take the social impact of publications into account in evaluating a scientist's over-all record.

7. What should a member of such a society do, feeling strongly that (6) is highly objectionable and dangerous—as dangerous as expression of the suppressed truths? Obvious answer: Leave the organization and form, or join, one that rejects (6) as counter-scientific.

Personal Addendum

A reviewer complains of my bringing up Nazism, that we should "get away from Nazis" (as if frequent reiteration of that dreadful history has somehow made it irrelevant), and reminds me that they are not the only offenders—why do I not mention Communists? I assure such readers that I am entirely evenhanded in detesting tyrannies of both right and left. I chose the Nazi example because nobody questions its fact and its evil, whereas there are still a few muddle-headed academics who think Stalinism was not quite as bad—that the Communists merely "made some mistakes," "overdid things under pressure." I had hoped to examine the award/ideology issue without reference to my own social or political views, which are not germane and which I believe play a negligible role in my analysis. But several referees' comments suggest that this cannot be done, that readers are moved to make inferences about Meehl's ideology and, perhaps, to discount my arguments accordingly. So let me clarify it. I am relatively apolitical, have seldom voted since the Vietnam period, and cannot be labeled "liberal" or "conservative." The liberal-conservative "dimension," long known to be psychometrically multifactorial, has undergone steady semantic erosion for two generations, so that today it has little precision in characterizing a person's orientation, is counterproductive to rational problem-solving, and in appraising sociopsychological theory is positively obfuscating. I would strike it from our vocabulary.

My sympathies are somewhat libertarian (lower case *l*) and as a result my score (meaningless as it is) on liberal-conservative questionnaires usually falls in the middle. If we did not need police, firefighters, safety regulations, highways, sewage disposal, national defense, etc., I might consider myself a kind of anarchosyndicalist. I am not so naive as to be unaware that psychologists tend, statistically, to be "left" more than "right" (terms which, designating extremes, retain somewhat more semantic content than "liberal" and "conservative" and are closer to unifactorial), as even some official pronouncements of APA reveal.[15] In my youth I was a democratic socialist,

[15] Thirty some years ago, a meeting of SPSSI entertained a motion officially to "condemn" Professor Henry E. Garrett, a distinguished psychometrician, because he thought that the Black/White IQ difference was partly genetic in origin. I was APA President at the time and made a

doing research on political behavior; but I refrained from joining SPSSI because I had reason to believe that some of its founders and leaders were Communists, fellow travelers, or apologists. Only a small minority of psychologists were duped by Stalin's bloody tyranny, which murdered three or four times as many innocents as Hitler's and, as the secret Kremlin documents show, was also an international criminal terrorist conspiracy. (It is an interesting sociological question why there were no comparable social science defenders of the "right" dictatorships of Mussolini, Franco, Salazar, etc. I do not know how to explain that.)

If I have an ideology, its core is respect for the liberty, autonomy, dignity, and value of the unique human person. A correlate (not a deduction) is a deep and abiding fear of the power wielded by large organizations: corporations, labor unions, chambers of commerce, churches, the media, environmentalists, big science, professional societies, political parties, special issue advocates, book publishers, and *especially*—because it claims and commands an effective monopoly of violence—the State. I submit that these views, while deviant, have only a slight connection (except perhaps temperamental) with the issue considered in this article. But to the extent that professional organizations control scientific publication, influence legislation, accredit training programs, certify competence, mold public opinion, and publicly praise or condemn individuals, they are also to be feared, watched carefully, and, on occasion, vigorously criticized.

Another reviewer asks how I can know whether several decades hence cultural change will classify science as 'poorly reasoned' or 'socially malignant' differently from today. I can't, I am not Omniscient Jones. I am not even a clairvoyant (Reichenbach, 1938). As to being poorly reasoned, the foregoing analysis shows that 'bad science' should only designate clear, gross, impactful *mistakes.* It does not include judgment calls or rational inferences to theories subsequently refuted by evidence unavailable to the scientific community when made. I am confident that 100 or 500 years hence 'undistributed middle' will still be recognized as a fallacy, as will dividing by zero, ignoring the negative facts when appraising a theory, or testing for a general factor by Varimax rotation. Scientific progress, which sometimes includes abandonment of received theories and invention of new investigative procedures, almost never revises basic logical and mathematical rules. Aristotle's rules of the categorical syllogism have survived over two thousand years, despite the several profound and sometimes catastrophic cultural changes, e.g., fall of Rome, medieval Christianity, the Reformation, religious wars, Enlightenment, Darwinism, rise

short but strong speech against the resolution, which failed. I like to think that my intervention as a prestige figure helped to prevent this intellectual atrocity.

of democracy, nationalism, rise and decline of Marxism.[16] Aquinas's world picture, the doctrinal substance, was very unlike that of the 'Greek miracle' or our contemporary views. If one questioned the Holy Trinity in 1298, one was in serious trouble. But the Church's *logicians* didn't countenance an Illicit Major any more than Plato or Bertrand Russell did. As to my narrower, discipline-tied example of the Varimax rotation, psychometricians a century hence may have quit doing factor analysis, having discovered something better (which I doubt, after nearly three generations of it); but they will not have refuted the proofs that, *if* you choose to factor analyze with an eye to finding a big general factor, Varimax is mathematically a poor way to do it. Scientific change as regards *method* is usually improvement (sometimes leading to substitution) rather than refutation. The reader may object that *sometimes* an apparently valid principle of logic or mathematics is shown to be incorrect, much to our surprise. Sure, with probability $p < 10^{-3}$. That is too small to set up as a guiding meta-principle. Probability, as Bishop Butler said (echoed by atheist positivist Carnap) is the guide of life. If you fret about odds of 1: 999 on a danger, you cannot rationally get out of bed each morning.

As to 'socially malignant,' the reviewer provides the answer, saying that "all awards and communal recognition . . . are social statements." Agreed, unqualifiedly. And no social statements are infallible. Ethics, of course, is not on the same footing as logic or scientific methodology when it comes to marshaling consensus; the culture can change more with respect to ethics. However, the possibility that the world of 3000 C.E. may come to approve again of genocide, infanticide, judicial torture, or chattel slavery cannot prevent *us* from disapproving of these things and taking a scientist's advocacy of them into account in expressing social approval of that person *qua* scientist. (If Western culture does revert to such practices, it will have declined even more than I or Oswald Spengler expect.) I use such extreme examples because less ethically extreme examples do not, *today,* command consensus, and hence ought not to be considered.

I have repeatedly hedged consideration of ideology, specifying "with caution and wide tolerance" (p. 1123), "extreme examples unlikely to arise" (p.

[16] I am aware of contemporary symbolic logicians' criticism of Aristotle's (A→E) implication in the categorical syllogism, where a universal ("All men are mortal") was taken by Aristotle to imply the particular ("Some men are mortal"). In contemporary symbolic logic the form $(\square x)$ $(\phi x \rightarrow \psi x)$ has no existential content, as there need be no men for it to be true; whereas $(\exists x)$ $(\phi x \cdot \psi x)$ asserts their existence. This technical logician's nicety improves the formalism, but it obviously has no impact on science, business affairs, courts of law, or the household life. The revision merely shows that formal logic, like empirical science, advances. This emendation, while clarifying (and *perhaps* having metaphysical ramifications), did not junk the whole syllogistic structure; and it surely did not turn Illicit Major, Equivocation, *Dicto Simpliciter,* False Analogy, *Post Hoc,* and the many other formal and material fallacies into acceptable arguments.

1126), "precludes fault-finding except in extreme cases" (p. 1133), "if at all, only at extremes, e.g., Nazis" (p. 1134). Primary ethical postulates are not rigorously deducible or "self-evident" as Euclid thought his geometric axioms were; and major cultural changes, especially derivative norms ("theorems") under changed conditions, are a social fact.

The ontological and epistemological status of ethical propositions has been debated for two and a half millennia and remains unsettled today. Such little progress—despite clarification of issues—is one reason for doubting whether ethics consists of cognitive claims at all. Logic, mathematics, and empirical science not only change, they *advance*—they "settle things" once in a while. What's wrong with moral philosophy, that it doesn't get anywhere, that the Socratic dialogue is interminable? For example, is a primary ethical principle properly conceived as stated in the indicative or the imperative mood? If the former, to what objective fact does it "correspond"? How would one prove it? If the latter, who is the authoritative law giver? What credentials? These deep perennial puzzles are beyond the scope of this paper. My analysis and argumentation concerns prize-giving in relation to the scientific community's values (aims and ethics) and does not depend on the content of Meehl's values—although mine happen largely to accord with the community's.

Although my ethics are irrelevant, reviewers' queries about ideological change and whether I can predict it suggest that a brief statement of my meta-ethical position is appropriate, lest it should be misunderstood from the preceding. Like Bertrand Russell, I strongly prefer, and vaguely intuit, ethical realism—that some ethical norms are objectively valid, would hold whether or not most humans grasped them, and are independent of how well people conform to them. But, again like Lord Russell, I do not know how to prove this, or even how to explain exactly what it means. Having been in this cognitive limbo for over 60 years, I do not anticipate escaping from it. The extreme form of ethical anti-realism (emotivism, relativism, subjectivism) says that my moral condemnation of Hitler and Stalin for murdering millions is qualitatively of the same nature as my preference for vanilla ice cream over chocolate, or for Mozart over Alban Berg. I can neither accept this nor refute it. At times I am willing, reluctantly, to settle for "ethics as pure postulate" (Williams, 1933/1952), accepting the distinctive ethical relation term 'ought' as a theoretical primitive, and a half-dozen primitive ethical properties (a good list is in Ross, 1930). I am pretty sure that any ethical system purportedly based on a single principle (e.g., Plato, Kant, Mill, Moore) will be inadequate, and I see no good reason to think that when geometry, chemistry, genetics, and economics each require several postulates, the complicated domain of human ethical conduct can be axiomatized with only one. I take the term 'ought' as a primitive because all efforts to reduce it to psychological descriptors have failed, and no meta-proof exists that this uniquely ethical concept *can* be reduced to nonethical notions. That rational discussion, including the marshaling of psychology and social facts, can take place concerning ethics *without* settling the rock-bottom question of primary postulate objectivity is argued in Meehl (1981). I show there how it is possible to be "ethically mistaken" apart from whether one's postulates are considered to be *assertions* or *commitments.* The philosophical literature on ethical objectivity is vast and of varied quality, and I make no effort here to summarize it or guide the reader. But a superb recent analysis of the ethical realism issue by two first-class intellects is the written exchange between philosophers Gilbert Harman and Judith Jarvis Thomson (1996); I have also profited greatly from Mackie (1977).

Some believe that the ethical realism issue can be settled by the facts and theories of sociobiology (Wilson, 1998). I assert, without proof: Sociobiology can facilitate analytic and creative thinking about ethics. Conjoined with ethical theorems, it can help us make concrete moral choices. It can criticize an ethical system by showing its pragmatic incoherence, given the facts of genetics and society. It cannot, however, *derive* one's primary ethical postulates. This is because I accept what philosophers call "Hume's Guillotine," that a statement containing the distinctive moral term 'ought' (or 'obligatory') cannot be validly inferred from statements not containing that term.[17]

Some may infer from all this that *no* ethical stance or conduct, *however* detestable in the eyes of *however many* informed, thoughtful, humane psychologists, should play *any* role whatever in communal bestowing of encomiums. I cannot accept this, and am sure most psychologists do not.

But the question of possibly extreme shifting social consensus is moot and bootless anyway, arising from what logicians call the "K-K fallacy," that is, thinking that you can't know anything unless you *know* that you know, *for sure.* We are examining our contemporary scientific organizations' award practices in the best light we have. If the mere possibility of unforeseeable radical revision of social values, basic scientific method, or rules of logic precluded qualitative or quantitative appraisal of *all* scientific work, then the whole problem disappears—no such collective judgments could be properly made. Nobody should receive any prizes; and, in consistency, journal editors dare not evaluate manuscripts nor academic departments promote to full professor rank by distinguishing good from bad science. Indeed, this very article should not have been reviewed or evaluated for publication, since if contemporary criteria of good and bad science have no validity, *a fortiori* no one can validly evaluate theses on topics like this one! This kind of objection is always cognitively obscurantist and socially

[17] I am aware that a minority of philosophers reject Hume's Guillotine, but I cannot see how to avoid it.

paralyzing. The K-K error must be eschewed in ethics as in epistemology, otherwise we would have to close up society's shop and return to the caves.

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