SCIENCE AND POLITICS: DANGEROUS LIAISONS*

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SUMMARY. In contrast to the opinion of numerous authors (e.g. R. Rudner, P. Kitcher, L. R. Graham, M. Dummett, N. Chomsky, R. Lewontin, etc.) it is argued here that the formation of opinion in science should be greatly insulated from political considerations. Special attention is devoted to the view that methodological standards for evaluation of scientific theories ought to vary according to the envisaged political uses of these theories.

Key words: Science, politics, acceptance of theories, dangerous knowledge, self-censorship, objectivity

> Les querelles entre les sectes *scientifiques* tendent à laisser le terrain aux querelles entre sectes *politiques*. Et comme les critères politiques sont socialement inavouables lorsqu'il s'agit de question de nature scientifique, la "contradiction" est résolue par un effort pour revenir à la vieille distinction entre bonne et mauvaise science. Mais le bon et le mauvais se distinguent en dernier ressort à partir de critères politiques. –
>
> *Raymond Boudon*

The search for truth was once regarded as the categorical imperative for scientists. Today things are not so simple. Having learned from some dramatic instances that there is no guarantee that the new knowledge would serve the good of mankind, many scientists have become increasingly reluctant to pursue some lines of investigation, because of their possible adverse social and political consequences.

We are indeed used to the tones of alarm and urgency which characterize the public debate over the social implications of scientific research. To what extent are the fruits of science sometimes viewed as an immediate threat to society, even by philosophers of science, is perhaps best illustrated by the following words of Max Black: "Science is even more terrible in its potentiality for evil than the atomic bomb for which it is responsible. Over every scientific laboratory should be pasted the warning 'DANGER: SCIENTIST AT WORK'." (Black 1978, p. 62)

PLAYING IT SAFE

One strategy for reducing the danger of the socially harmful effects of science is the proposal to make the standards of acceptance of theories

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considerably higher in politically sensitive areas of research. In this way political considerations would not be allowed to dictate the choice of hypotheses since *sufficiently strong* evidence would still command the acceptance of any hypothesis, however politically unpalatable it may be. And room would also be made for the demand for caution, and for the social responsibility of scientists when they are dealing with politically hazardous topics.

This rather widely adopted view is reflected in the following statements:

> [T]he more potentially dangerous to society the results of research might be, the more rigorous one should insist that the methodology for that research must be. (Graham 1981, p. 311)
>
> [S]ince no scientific hypothesis is ever completely verified, in accepting a hypothesis the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of hypothesis. Obviously our decision regarding the evidence and respecting how strong is 'strong enough', is going to be a function of the importance, in the typically ethical sense, of making a mistake in accepting or rejecting a hypothesis.
> (Rudner 1953, p. 2)
>
> Everybody ought to agree that, *given sufficient evidence*, for some hypothesis about humans, we should accept that hypothesis whatever its political implications. But the question of what counts as sufficient evidence is not independent of the political consequences. If the costs of being wrong are sufficiently high, then it is reasonable and responsible to ask for more evidence than is demanded in situations where mistakes are relatively innocuous. (Kitcher 1985, p. 9)

There is no doubt that accepting a scientific theory may under certain circumstances have repercussions outside the area of pure research, and that it may lead to generally undesirable social and political consequences. The most often discussed cases alleged to be of this kind are the theory about the genetic origins of differences in intelligence and various socio-biological explanations of human behaviour. So, it was, for instance, in setting the stage for his criticism of human sociobiology that Kitcher actually introduced the idea that methodological standards for evaluation of theories should vary according to the envisaged political uses of these theories.

The crucial question, of course, is whether the acceptance of a theory should really be allowed to be influenced by estimates of its probable consequences. Here I shall argue that this should not be the case.

The standard statistical procedure (see, for example, Blalock 1972, pp. 155–167) for deciding whether to accept a given hypothesis H_1 or not is to introduce an alternative hypothesis H_0 (the "null-hypothesis") and then to estimate whether upon available evidence H_0 is to be rejected. Generally, rejecting H_0 is equivalent to accepting H_1. (Surely, not all instances of acceptance can be reduced to this neat scheme of deciding between a given hypothesis and the null-hypothesis. It seems to me, however, that by concentrating on these relatively simple situations it is possible more clearly to point out the differences between the rival views of acceptance.)

Two types of error are possible: one can reject the null-hypothesis when it is in fact true (type I error), and one can fail to reject it when it is in fact false (type II error). Let us for the moment consider the problem of how to avoid the type I error.

However strongly evidence may in particular cases support the rejection of H_0 (and accordingly favour the acceptance of H_1) the possibility will never be excluded that H_0 is nonetheless true. How strongly then ought H_1 to be supported in order that we take the risk and actually reject H_0? Or, to put it differently, what is the highest probability of making the type I error that should be compatible with our rejection of H_0? The conventionally accepted answer to this question are the probability values of 0.05, 0.01 and 0.001. (They are called the "significance levels" of a test.) The value 0.05, for instance, means that one should reject H_0 only if on given evidence the odds against mistakenly rejecting H_0 are greater than 19 to 1.

Although the adopted values of the three significance levels are obviously to a certain extent arbitrary, some choice of this kind simply has to be made if acceptance of a theory is to be connected with *sufficiently strong* evidence in its favour. Given (a) that scientists accept hypotheses and (b) that on any evidence the probability of a hypothesis being false is always greater than zero, there must be a region where the probability of making a mistake becomes so small as to warrant acceptance. (It is well known that claim (a) is rejected by Carnap, Popper, Jeffrey, and some other philosophers. I do not find their quite different grounds for opposing (a) convincing, but it seems to me that the consideration of their views would be out of place here for the following reason: if (a) is rejected the whole issue that I am at present addressing simply does not arise. Since I am here dealing with the question whether acceptance of hypotheses ought to be influenced by political and ethical considerations, it is for the sake of the discussion that I shall proceed on the assumption that scientists *do* accept hypotheses.)

According to Graham, Rudner and Kitcher, acceptance of scientific theories should not be contingent upon some *fixed* and sufficiently small probability of error. In their opinion, acceptance should be a function not only of the probability of error but also of its *seriousness*, in an ethical or political sense. The contrast between this view and the standard statistical approach is displayed in Figures 1a and 1b. For the ease of graphic depiction, probabilities mapped on y-axis are not given on a linear scale.

The lines in Figures 1a and 1b indicate *critical* probabilities for acceptance. They represent different views as to what is the highest probability of making a mistake in the face of which a hypothesis should still be accepted. "Admissible" probabilities are situated in the area under the curves.

In Figure 1a all lines are horizontal, which means that all the proposed threshold probabilities are constant: they do not respond to varying degrees of ethical or political seriousness of a possible mistake.

A quite opposite picture emerges from Figure 1b. The lines representing

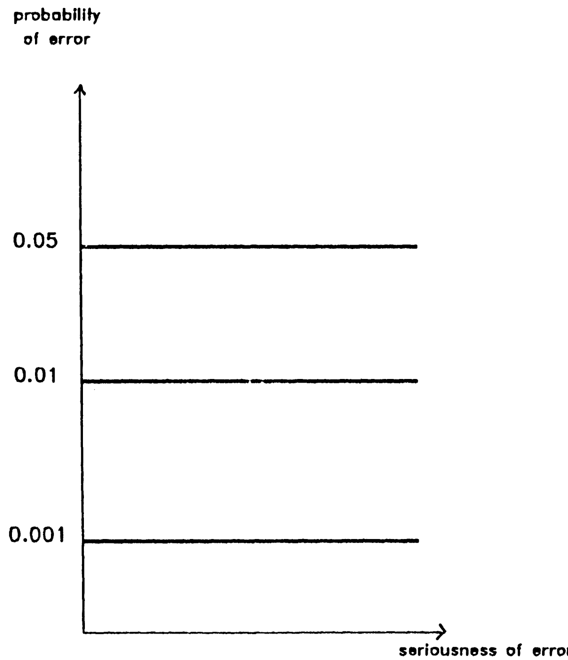
probability
of error
0.05
0.01
0.001
seriousness of error

Fig. 1a

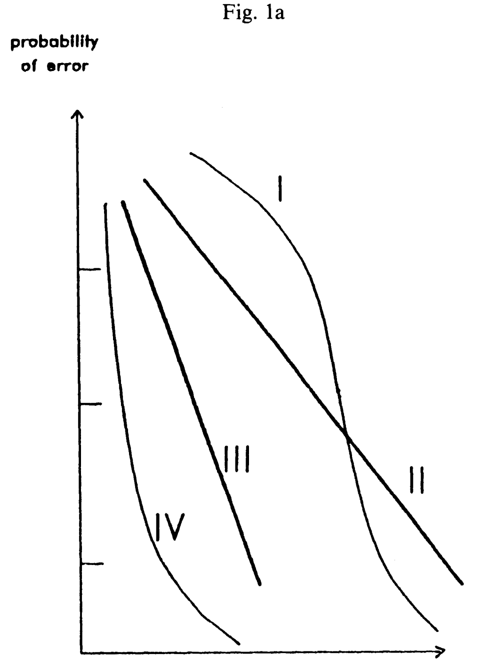
probability
of error
I
II
III
IV
seriousness of error

Fig. 1b

threshold probabilities for acceptance are here conspicuously sensitive to changes on the "social concern" axis x: acceptance should be guided *both* by probability *and* by seriousness of error.

In contrast to Figure 1a, where the three critical probability values were simply taken over from the standard statistical account of theory testing, the four lines drawn in Figure 1b are offered just for the purpose of illustration, and are not to be attributed to anyone in particular. The reason for this is that the advocates of "socially concerned science" are directing all their efforts to trying to establish *some* connection between acceptance and possible political dangers of this act, without ever showing any interest in the *precise form* of this functional relationship. Their standpoint is consequently made to a large degree indeterminate; this is best manifested by the fact that the crucial condition "The more dangerous mistake, the more difficult acceptance", jointly brought forward by Rudner, Kitcher and Graham, is perfectly well satisfied by all four strongly diverging lines in 1b.

The lack of clarity with respect to the precise shape of the acceptance function gives additional force to the well-known objection that allowing social concern to affect acceptance might impose such high standards for the investigation of socially sensitive issues that any empirical research in this area would be thereby effectively discouraged. To give just one example, after having quoted the opinion that hypotheses reinforcing social stereotypes should not be advanced unless it is clearly shown that the probability of their truth is high, L. J. Cronbach commented:

> One infers that the social scientist with a disturbing hypothesis should pursue it privately, keeping his dark suspicions secret until he has a solid case. Given the social nature of the scientific enterprise, this seems as inhospitable to heterodoxy as an outright embargo on a research topic (in Frankel 1976, p. 134).

Particularly vulnerable to this kind of criticism are the views (like the one represented by line IV in Figure 1b) which state that the maximal probability of error compatible with acceptance falls very steeply with the rising "social concern" value of a hypothesis.

In point of fact, once the relevance of social parameters in the cognitive sphere is admitted, the possibility cannot be excluded that the acceptance of a theory would in some situations be blocked by these "external" reasons, despite its being *known* to be true. There is, namely, no guarantee that the acceptance of *true* theories will not sometimes lead to socially and politically harmful consequences. It is a mere article of faith to believe in such a preestablished harmony of the true and the good, according to which ignorance is never better than knowledge, even from the political perspective.

Furthermore, although presumably in rare and extreme cases, it is by relying on this logic of social concern and by fearing the bad consequences of their cognitive act that scientists could find themselves deciding not to accept a given hypothesis despite its being true beyond any reasonable doubt, saying perhaps something to the effect: *Amica veritas, sed magis amica humanitas*. On such grounds even a "noble lie" could be defended and become a live option in science. Interestingly, most advocates of "socially concerned" science balk at this consequence of their standpoint, and are rarely willing to draw the inference to its logical end. One exception is Gunther S. Stent, who not only clearly perceived but also embraced this implication (Stent 1978, pp. 217–218).

A CURIOUS MIXTURE

Accepting a scientific theory means (roughly) taking it to be true. Although it may seem rather unusual to let this eminently *cognitive* act be influenced by *decision-theoretic* considerations, such a view is among many others explicitly defended by Kitcher:

> [My] conclusions do not rest on misty sentimentality or unrealistic standards of evidence. A familiar principle of *decision making* is that agents should act so as to *maximize expected utility*. The rationality of adopting, using, and recommending a scientific hypothesis thus depends not merely on the probability that the hypothesis is true, given the available evidence, but on the *costs and benefits* of adopting it (or failing to adopt it) if it is true and on the *costs and benefits* of adopting it (or failing to adopt it) if it is false. The abstract principle is familiar to us from many concrete cases. Drug manufacturers rightly insist on higher standards of evidence when there are potentially dangerous consequences from marketing a new product. (Kitcher 1985, p. 9).

It is, however, far from being self-evident that the cost-benefit analysis should play such a prominent role in the acceptance of hypotheses, as suggested by Kitcher. It is after all primarily meant to apply to the field of *practical* decisions and is by no means automatically transferable to the *theoretical* domain.

It is indeed remarkable how frequently, in addressing the question of when theories should be accepted, one slips imperceptibly into talk about what practical action should be undertaken. Kitcher supports his opinion concerning the adoption of scientific theories by giving the dilemma of a drug manufacturer as an allegedly relevant example. Rudner uses an almost identical illustration. Strangely enough, such running together of theoretical and practical questions has become so common even in *Fachliteratur* that R. E. Henkel, writing about the acceptance of theories, complains that "what comes through in most teaching of statistics is a curious mixture of the Fisher approach and the decision theory approach" (Henkel 1978, p. 38).

The reasons relevant for the *practical* decisions of the drug manufacturer type are actually of a fundamentally different kind from those that govern the acceptance and rejection of *theories*. Since the principal aim in the first case is *action*, the weighing of possible consequences has obviously to play a great part in the process of deliberation. Indeed, the desirability and probability of envisaged consequences of various options constitute paramount *reasons* in favour of, or against, them.

The theoretician's dilemma, however, does not fit easily into this pattern. The two situations are no doubt similar in that the acceptance of a theory, like the adoption of a course of action, may with a certain degree of probability be expected to give rise to some more or less socially desirable consequences. Moreover, after comparing rival theories in this respect, it could even transpire that the acceptance of one of them would be clearly and by far the most undesirable. But it is dubious whether this information should be a *reason* not to accept that theory.

Accepting or not accepting a theory is a cognitive act *par excellence*. Since the essential (although not fully realizable) aim here is to accept the true theories and not to accept (or, better, to reject) the false ones, it is only natural to assume that the acceptance should be influenced solely by considerations relevant to the truth of the theories in question. The expected social consequences of the acceptance of a theory are patently quite irrelevant to its truth-value: they bear no *evidential* relationship to it.

The proposal to resolve an epistemic dilemma by the cost-benefit analysis has already been advanced by Pascal in his famous "wager argument". Pascal, too, has weighed the desirability-cum-probability of belief and non-belief (in God) letting then a simple calculation determine the preferred state. It is difficult to understand why philosophers have almost unanimously condemned Pascal's argument as a paradigm of irrationality and as a kind of *sacrificium intellectus* (Cargile 1966; Hacking 1975, pp. 63–72; Mackie 1982, pp. 200–203; Elster 1984, pp. 47–54), while at the same time they rarely see anything nearly so objectionable in the widespread view of science according to which, in a very similar way, theoretical reason should, *in its own province*, be subjugated to practical objectives.

Accepting a theory is essentially tied to searching for truth. Of course, it may turn out that the thing pursued can be only approximate truth (as fallibilists insist) or only empirical adequacy (as anti-realists say), but some kind of truth is nevertheless aimed at in the process. There is hence something very odd in allowing the intrinsically cognitive and truth-directed behaviour to be determined by non-cognitive and truth-irrelevant causes. For instance, if someone opposes the acceptance of a theory at least partially because he believes that the acceptance would have undesirable social or political effects this cannot constitute his *cognitive* reason against it. But the acceptance being essentially a cognitive act and a social fact only accidentally (through its consequences), it seems especially inappropriate to allow the issue here to be influenced in any way by non-cognitive reasons.

There is, in fact, an ineradicable tension between the pursuit of truth (which gives science its essentially epistemic character) and permitting the social usefulness of different cognitive options to affect a final outcome. If in the process of the fixation of the belief one steps back from the cognitive perspective every now and then in order to check that he does not land in some socially threatening position, is it not glaringly obvious that his so "socially corrected" views are epistemically compromised?

All this, of course, should not be taken to mean that scientists have no social responsibility. It only means that their responsibility cannot consist, as it is sometimes suggested, in the duty constantly to keep an eye on possible social consequences of their work and to adjust their beliefs accordingly. It is simply incoherent to put forward the search for truth as the general aim of scientific activity and then to require as well that in particular cases the scientists partially shape their views in conformity with truth-irrelevant considerations.

SOCIALLY CONCERNED SCIENCE FROM 'OUGHT' TO 'IS'

Fortunately, we are in position to see how it works in practice when scientists act in accordance with the Rudner-Kitcher-Graham advice – that different methodological standards should be used when assessing theories which differ from one another politically. This most interesting information comes from a psychological investigation carried out recently by S. J. Ceci, D. Peters and J. Plotkin (Ceci, Peters & Plotkin 1985).

The object of their study were the "human subjects committees" in the United States, bodies whose primary duty is to evaluate whether in scientific research proposals (submitted to them for inspection) the treatment of human subjects is acceptable, i.e., whether there is something ethically objectionable in the proposed research procedure. In deciding on whether to approve a project or not these committees are allowed to cite methodological objections, but they are explicitly warned that their ruling should in no case be influenced by socio-political implications they may be ascribing to some proposed research.

In the study reported, three types of research projects have been sent for assessment to the randomly chosen human subjects committees at the universities all over the United States. The proclaimed aim of all these projects was to test a hypothesis about possible discrimination in the hiring procedure. Three different kinds of discrimination were involved: (1) discrimination against women and racial minorities, (2) discrimination against white males (reverse discrimination), and (3) discrimination against obese and short individuals. The design of the proposed research was the same in the three cases, so that they differed only by their subject matter, but not methodologically. Furthermore, each of these three types of research projects was being presented to the committees in one of two forms: (a) with some ethically objectionable features in the proposed treatment of human subjects (*e.g.* deceiving the subjects), or (b) without such features.[1]

It turned out that the research projects about (3) were much more often approved by the committees than those about (1) and (2) although the only difference between them was that (1) and (2) included politically sensitive parameters (race and gender), while (3) did not. The results are presented in the following table:

TABLE I

	(1)		(2)		(3)	
	Discrimination		Reverse Discrim.		Height/weight	
Decision	(a)	(b)	(a)	(b)	(a)	(b)
Approved	18	9	11	6	22	19
Nonapproved	18	10	19	10	14	1
Total	36	19	30	16	36	20

Despite the projects having shared all properties relevant for their evaluation, while the proportion of approved to nonapproved proposals was 27:28 for (1), and 17:29 for (2), it was 41:15 for (3). Bringing together (1) and (2) it transpires that only 37% projects of this group were approved, in clear contrast to the group (3) where the rate of approval was no less than 73%!

An even more striking fact emerged from this study. As it is easily recognizable from our table, in the process of judging the socially sensitive projects (the groups (1) and (2)) it played no great role whether they belonged to class (a) or class (b) – that is, whether they included the ethically objectionable treatment of human subjects or not. In ethically "problematic" category (a) 29/66 of the projects (or 44%) were approved, hardly a significant difference from the ethically "improblematic" category (b) where the proportion was 15/35 (or 43%).

This is surely surprising. One should have expected that, in reaching their decisions, the human subjects committees would have been influenced by those features of the research projects they were supposed to monitor in the first place. Indeed, this expectation was fulfilled in group (3), the politically non-sensitive cases of investigating a possible height-weight "discrimination". The projects with ethically objectionable treatment of human subjects were here more often rejected than the others: the approval rate of the former was 22/36 (or 61%) while among the latter it was 19/20 (or 95%).

How to explain, then, the curious fact that with respect to the politically sensitive projects (1) and (2) the committees entirely failed in their selection procedure to treat differently the cases (a) and (b) that obviously differed in a relevant way. The answer is fairly simple. Faced with the research proposals on politically sensitive topics the committees tended to justify their negative decision by pointing to the ethically objectionable treatment of human subjects, *when such grounds were available*. When there was no place for this kind of criticism, however, their opinion was, oddly enough, not more favourable: now they cited methodological objections as reasons for their opposition. So, the research proposals which could not be faulted ethically were simply submitted to a more rigorous methodological scrutiny, and it is precisely the new flaws extracted in this way that account for the curious fact that the rate of project approval was not higher at all in the group (b) consisting of ethically "clean" cases. The following table shows different frequencies of raising methodological objections in rejecting proposals from different groups.

TABLE II

	(1)		(2)		(3)	
	Discrimination		Reverse Discrim.		Height/weight	
Decision	(a)	(b)	(a)	(b)	(a)	(b)
Meth. obj.	20–25%	70%	20–22%	60%	14%	0%

Although all the proposals were deliberately made so as to be methodologically indistinguishable, the politically sensitive proposals were in *methodological* respect judged strikingly unlike, depending on whether they belonged to group (a) or group (b). The fact that methodological objections were here to such a large extent more frequent in (b) than in (a), *i.e.* 60–70% versus 20–25%, suggests that the committees fell back on methodological criticism whenever they had no more recourse to other (ethical) grounds for opposition. This is actually what the authors of the investigation themselves inferred:

Thus, the conclusion that can be drawn from the narratives for the proposals that did not contain ethically problematic procedures is that [human subjects committees] found the sensitive proposals to be socially objectionable, especially the reverse discrimination ones, *and invoked whatever reasons were most convenient to justify their decision to deny approval* (Ceci, Peters & Plotkin 1985, p. 1000).

The scientists who were members of the committees acted fully in accordance with Rudner-Kitcher-Graham maxim "The more sensitive the issue, the stricter methodological standards".

We usually expect that science should supply an objective and impartial information, no matter how closely a certain topic may be connected with burning social and political issues. This expectation is naturally expressed through the demand that dealing with the *scientific* aspects of these two-sided questions be largely insulated from political influences. Moreover, as A. Kantrowitz put it: "In order to maintain democratic control of mixed decisions, it is essential that great care be taken to avoid the invasion of objectivity by strongly held moral or political views." (Kantrowitz 1975, p. 507). The idea that questions in which both politics and science are involved can be rationally solved only if the two are clearly separated beforehand is also advocated in Mazur 1981.

If the principle that everything ought to be debated on its own terms (political issues with political arguments, methodological issues with methodological arguments) is abandoned, and if methodological canons are thus politically relativized, this will invite unnecessary confusion and bring about endless discussions at cross-purposes between the advocates of rival standpoints.

Indeed, such a state of affairs is typical for politicized scientific controversies like the one, say, concerning the genetic determination of behavioural traits. Moreover, in that controversy we can observe something very similar to the extreme inflexibility of attitude demonstrated in the human subjects committees. Take the hypothesis that genes play an important part in explaining individual differences in intelligence. This theory has continuously and firmly gained empirical support, and the case for it is incomparably stronger today than it was, say, 40 years ago. It is remarkable, however, that there are critics who oppose it with equal force and contempt as they oppose some antiquated versions of it. The best illustration is Steven J. Gould's book *The Mismeasure of Man* where he dismisses in the same tone of voice both contemporary "hereditarian" theories and notoriously untenable 19th century speculations in this vein[2]. In his historical account, the view shared today by numerous scientists that the intelligence variance is under strong genetic influence is presented as merely the most recent form of an old pseudo-scientific idea, endorsed by the opponents of human equality. This uniform and indiscriminate rejection of such mutually diverse theories suggests that, as in the above example, it was the perceived *political* "content" of these theories that fixed the attitude toward them in advance, and the reasons were only subsequently squeezed out from the reservoir of methodological objections.

Heeding the Rudner-Graham-Kitcher advice and, consequently, contaminating the methodological appraisals in science by political considerations will naturally result in the decline of objectivity and in the pressure not to bend to "socially dangerous" theories. Such a tendency to force a scientific consensus externally and artificially is well described in the following first-person report:

> There is simply no doubt about it: There is a double standard among journal editors, referees, book review editors, textbook writers, and reviewers of research proposals when it comes to criticizing and evaluating articles that appear to support what the readers may interpret as either "hereditarian" or "environmentalist" conclusions... I approve the thorough critical scrutiny to which "hereditarian" articles are subjected but deplore the fact that many "environmentalist" articles receive much more lax reviews. There is unquestionably much more editorial bias favoring "environmentalist" findings and interpretations. For example, I was recently told by a journal editor that one of my articles .. had to be sent to seven reviewers in order to obtain *two* reviews of the article itself; the rest were merely diatribes against "Jensenism"; the editor apologized that they were too insulting to pass on to me... Many young Ph.D.'s just starting their academic careers and in need of favorable reviews, publications, and research grants are understandably discouraged by this climate from embarking on research programs that might result in "hereditarian" findings. (Jensen 1981, p. 490)

VIRTUES COGNITIVE, VICES POLITICAL

Making the acceptance of some theories more difficult, on the grounds that it would probably have undesirable social or political consequences, goes together with making the *non-acceptance* of some other theories more difficult for the same reason. For, once the relevance of consequences of this type is admitted into the cognitive sphere it is hard to see why should they be allowed to pull only in one direction (away from acceptance) and not in the other (towards acceptance). We can well imagine situations in which, with a high degree of probability, socio-politically damaging consequences would ensue if scientists *failed* to accept a certain hypothesis. (For instance, if scientists were reluctant to accept a hypothesis H_1 because this, by far the most plausible hypothesis, was regarded as still not being quite sufficiently supported to warrant acceptance, this hesitation itself could encourage the advocates of a socially threatening alternative H_2, thus bringing about "unnecessary" political harm.)

Were scientists indeed to be doubly sensitive to social considerations which would in one sort of case make acceptance of theories easier and in the other more difficult, the public credibility of science would be seriously disrupted. In any particular situation we would then just never be in the position to know whether a scientist's publicly expressed view is due to his evaluation of relevant evidence or (at least in part) to his letting the cost-benefit calculation suggest the socially most beneficial belief.

Another problem is that in order to apply the cost-benefit analysis one has to have a fairly accurate knowledge of all possible consequences of different options and of their respective probabilities. This condition is very rarely satisfied even in comparatively simple scientific dilemmas. Let us here only mention the fact that the applications which may result from the pursuit of a given research program are essentially unpredictable. The conceptual development of a hypothesis and the working out of all its various implications could throw an entirely new light on it and indeed even completely reverse the initial judgment about its socially threatening character. One striking illustration of how an over-hasty socially motivated intervention in science can, through a lack of wider perspective, cause more harm than good is the recombinant DNA debate. The research and experimentation in this area were deliberately restricted because of short-term and largely imaginary dangers, whereas it was only later realized that what was also blocked thereby were very important attempts to isolate the genes that cause cancer (Watson 1986, p. 21).

The usually available information about possible social repercussions of scientific work is in general too vague and too unreliable to serve as a basis for any decision-theoretic inference. The cognitive web of scientific theories is so intricate and in such a continuous fluctuation that it is purely presumptuous to aspire to have an overview of this whole tangle and to harness it to human needs.

On the other hand even if, *per impossibile*, the consequences of various scientific opinions could be predicted and mutually compared, it is by no means obvious that the scientists themselves would possess the necessary competence for that task. For, the scientific knowledge, however wide and encompassing, would plainly not be sufficient; in order to estimate correctly the *social* consequences, the very detailed knowledge of all kinds of concrete social and political circumstances would be required as well. Is it realistic to expect of scientists that, beside all the time-consuming work in their professional hours, they also be in command of the vast quantity of that "external" information?

Quite curiously, this implication that, over and above their research, scientists have the obligation to become social and political analysts, which would be regarded by many as a *reductio ad absurdum* of the whole idea of socially concerned science, is sometimes defended quite explicitly:

Needless to say, the requirement for rational acceptance decisions that scientists attempt to determine the consequences of their decision alternatives introduces new problems for them. It appears to require of physicists, for example, to do environmental or political research in the course of their work. Nonetheless, regardless of these difficulties, scientists cannot be relieved of the responsibility of trying to foresee ethical consequences and (when foreseeable) of making acceptance decisions with regard to them. (Gaa 1977, p. 535)

The implausibility of this view is enormous. To begin with, today when all scientists are under great strain to keep in step with the constantly rising ocean of literature and with the ever growing need for interdisciplinary work, it might indeed turn out that the newly demanded expertise "in environmental and political research" could be acquired only at the price of lowering the internal standards of the discipline in question. Secondly, and more importantly, since the decision arrived at in this way would obviously be of *political* nature it could be objected that the scientists would thereby overstep the area of their legitimate concern, and that they would in fact thus usurp the role which properly belongs to society as a whole. And in addition, there is no reason to expect the political opinions of scientists to reflect accurately the distribution of different political attitudes in the society at large. On the contrary, as some investigations revealed (*e.g.*, Ladd & Lipset 1975), the scientists tend to occupy the left end of political spectrum significantly more often than the average citizen. To bequeath political decisions to the scientists might in such circumstances actually mean permitting undue influence of a particular political standpoint.

The blurring of borders between science and politics gives rise to implications that are likely to find few adherents, when stated explicitly. It is not only that the conflicts between rival scientific theories will cease to be resolved on the exclusive ground of their respective cognitive merits and relevant evidence, and that the externally introduced information about their expected impact on society will now tend to prejudice the issue. Worse still, by thus "socializing" and "politicizing" the scientific activity an uninviting dilemma will be forced upon us: either to attribute to the scientists special authority in politics or to claim that the whole society has the right to decide "democratically" which scientific theories are to be accepted. Science and politics are odd bedfellows in that the two spheres are regulated by profoundly different systems of norms, and that each of them has its own distinctive mode of arriving at decisions. It should therefore come as no surprise that, when they are nonetheless compelled to mix, one ends up with these hybrid and ill-conceived ideas like the elitism of experts in politics or the public referendum in science.

THE ARGUMENT FROM EXTREME CASES

The claim that the scientists' cognitive decisions should be affected by the social background in which they take place is very often supported by giving some extreme situations as examples. The strongest and the most frequently cited case is a dilemma faced by real or hypothetical scientists in the Third Reich. Arguing in this way, Ned Block and Gerald Dworkin state that they

> take it as evident that one should condemn the actions of, say, those German scientists who pursued research into atomic phenomena with the aid and encouragement of the Nazi regime, and who knew that the probable consequences of their research, if succesful, would be the construction of weapons of immense destructive capacity.

They conclude that 'the right course of action for the scientists would have been to abandon such research' (Block & Dworkin 1976, p. 507). In a similar vein, Chomsky (Chomsky 1976, pp. 294–295), Dummett (Dummett 1981, pp. 296–298) and Graham (Graham 1981, pp. 254–255 & pp. 410–411) say that a scientist, living in Hitler's Germany, would have a moral obligation not to pursue research and not to publish results that could be grist for the mill of Nazi ideologists.

Although this reasoning is basically sound, it is important not to overestimate its force. First, notwithstanding all these quite exceptional and extreme political circumstances this argument apparently cannot establish the strong conclusion that it is *acceptance* that should be determined by non-cognitive factors. Take Graham's example (not so far-fetched as an example independently suggested by Chomsky and Dummett) of a German scientist who, living under Nazi regime, discovered Tay-Sachs disease, a genetically caused mental disorder that is more frequent among Jews than in the general population. In view of a very probable serious misuse of this opinion under these conditions, he would be morally obliged (as Graham himself says) to withhold *publication* of this discovery, but surely not (I would add) to withhold *assent* to what is strongly supported by all relevant evidence. However dreadful the consequences of the public *expression* of an opinion may possibly be, for a rational person they can obviously carry no weight at all while, *in foro interno*, he weighs reasons for and against the belief in question.

Secondly, conceding (as I just did) that in the extreme cases of the above type one should refuse publicly to disclose his opinion on some socially sensitive issues by no means entails that such a behaviour would be equally defensible under normal circumstances. This dubious line of reasoning according to which a point proved for extreme cases holds true outside this scope as well is indeed among important arguments put forward in favour of "socially concerned" science.

What is wrong, though, with making this inferential step? In order to answer that question, let us first of all see what actually justifies the scientist's self-censorship in Graham's example. The political conditions of the case are clearly such that the scientist could know with certainty that the announcement of his discovery would be widely misused by the powerful Nazi propaganda machine in proving the racial inferiority of the Jews. Morever, what is worst and what should have been also obvious to him is that he could not hope, under these conditions, that he or anyone else would have a chance to set the matter straight and to correct the officially upheld misinterpretations. When ideological *Gleichschaltung* uproots the autonomy of science itself, and when only those scientific ideas are permitted to reach the wider public which, after being distorted beyond recognition, can serve the aims of a totalitarian government, it would indeed be irresponsible and inhumane to refuse to pay any attention to the possible harm ensuing from publicizing a scientific discovery.

In liberal democracies, however, the scientist finds himself in a markedly different situation. To be sure, he can still occasionally with good reason fear the exploitation of some scientific ideas for the political purposes he disapproves of; but, notably, he has now at his disposal less extreme means to fight such misuses than avoiding to talk about perilous topics altogether and exhorting others to do the same. Whether addressing his peers or the general public he can, whenever he deems it necessary, interject an explicit warning not to draw from the research in question certain erroneous political implications. True, his admonitions will not always be heeded, and pernicious misinterpretations will sometimes prevail in defiance of all attempts to prevent them; however, this hardly constitutes adequate justification for the scientist to demand the suppression of a public discussion on the 'dangerous' topic.

The failure to rectify the prejudiced understanding of a scientific hypothesis may not be due to its being by itself inextricably conjoined with political abuse; this may rather be a result of not having used the most effective explanatory strategies in clearing up the major confusions to which public opinion is prone. In that case, a piece of knowledge is blamed although it is solely the medium in which it is presented that should have been altered.

When Richard Lewontin, for instance, asserts that "[a]ny investigation into the genetic control of human behaviors is bound to produce a pseudo-science that will inevitably be misused" (quoted in Wade 1978, p. 330) it is hard to see with what kind of argument such a sweeping claim could be vindicated. To realize how preposterous this apodictic prediction really is, it is enough just to think of what a vast amount of information about society, public opinion and the future development of science would be needed in order to condemn one whole area of research (behavioural genetics) as necessarily and forever being only the source of misinformation and political manipulation.

With eyes fixed on socially threatening features of science one tends to be overprohibitive with respect to research on "sensitive" topics, and to forget that some good consequences, although not always fully visible, are usually expected to be forthcoming with the acquisition of new knowledge. These somewhat indeterminate but nevertheless real advantages awaiting us in the future ought surely also to be taken into account when deciding whether to put embargo on a certain subject.

Someone will perhaps object here that knowing more may bring with it new unanticipated evils as well, and that for lack of information concerning the still unexplored areas we simply have no right to be optimistic (or pessimistic, either). What is wrong with this objection is that it views the effects of the growth of knowledge as something that just happens to us by chance and that it is altogether outside the control of our will. The truth is, of course, that the consequences resulting from new cognitions are not befalling us in such a random manner. On the contrary, scientific discoveries are usually followed by an organized and coordinated effort to put them to the best possible social use. This in fact constitutes the reason for presuming that blocking a line of investigation might deprive the society of important but presently not foreseeable advantages.

This presumption ceases to be justified in the extreme case, discussed above. It would, namely, be silly to doubt that in the Third Reich setting the new scientific knowledge would entirely share the fate of the old one in being similarly abused and permitted to reach the public only in a crude, ideologically distorted form. The dysfunctioning of science is here an essential and irremovable characteristic of the situation, whereas under less extreme circumstances it is highly improbable that some pieces of knowledge are predestined to have only detrimental social and political effects. Even with respect to knowledge that were particularly susceptible to misinterpretations and political misuse, one is hard put to produce a single example where it could be demonstrated that, despite all safeguards that the democratic institutions normally offer and despite all attempts to enlighten public opinion on the matters in question, the threat could be adequately met in no other way than by restricting the freedom of research and by keeping closed "the doors that are too dangerous to open".

I would like, however, to go further and to argue that, whatever the final outcome of such a calculation of social consequences, there is something deeply wrong with the whole idea of making the decison to disclose scientific information dependent on its expected impact on society. From granting (as we did) that in the extraordinary conditions of the completely disrupted autonomy of science and its political instrumentalization by a totalitarian government it could be a scientist's duty not to report a predictably misusable discovery it by no means follows that, when living in a democratic environment, he has right to decide, according to his political judgment, on what is publishable and what is not.

The free circulation of ideas and their constant public availability to anyone interested in them are not only indispensable for the proper working of science; they have great *social* value as well. A new contribution to the corpus of scientific knowledge is intrinsically valuable, irrespective of its possible social utilization, and scientists have a *prima facie* obligation not to prevent anyone from being acquainted with it. Moreover, were this openness even to be expected eventually to result in some negative social or political consequences, nothing insures in advance that in the conflict of the social and cognitive values it is always the cognitive side that will have to give in. That is, if the acquisition of knowledge is valuable in itself, it should not be impossible to imagine a situation where the public possession of a piece of knowledge, although leading to some undesirable socio-political effects, would clearly and universally be preferred to saving the *status quo* by the scientific conspiracy of silence.

How, then, and on what principles should the intrinsic value of a knowledge claim be measured against its possibly harmful consequences for society? To the best of my knowledge, an answer to that question is at present completely missing. Worse still, the problem itself is most frequently not recognized at all. The dilemma of what to do when it is impossible simultaneously to achieve both of the highly approved goals (to get knowledge and to foster various social and political ends) is usually resolved *ad hoc* – by relying on common sense and without utilizing any explicit criteria for weighing the respective imports of these different scales of values.

Interestingly, when a group of scientists was asked in a poll whether a scientist "should (1) withhold a discovery from the world when convinced it would be productive of more evil than good, or (2) never withhold a discovery, leaving it to the moral sense of mankind to decide its ultimate use', about 80% of them replied that they would never withhold a discovery, whatever the consequences (Barber 1952, p. 210). I suggest that this result should not be interpreted as showing that these scientists were callous and insensitive to social issues; rather, answering the way they did, they perhaps only displayed a sound and laudable scepticism in that they just could not bring themselves to take seriously the idea of ever having good reasons rigidly to attach to a scientific discovery the unavoidably bad consequences.

POLITICAL IMPUTATION AS A WEAPON IN SCIENCE

In his story *The Voice of the Dolphins* Leo Szilard remarks that when a scientist says something, his colleagues ought only to ask themselves whether what he says is true, but when a politician says something his colleagues should first of all raise the question: "Why does he say this?". Such a division of labour, it seems, has had its day. A scientist dealing with socially sensitive topics is today no more surprised (or at least he should not be) if his colleagues open the discussion with the "political" question.

The sociobiology controversy is a good example. As Arthur L. Caplan observed, "[c]ritics of biological research into the etiology of behaviour often begin with valuational objections, but, often end up invoking methodological reasons why such research is unsound" (Caplan 1980, p. 98). Caplan himself offers no explanation for this "evolutionary trend". Others have, however, suggested that such an illogical order of moves in scientific criticism simply showed that opponents had inferred the (theoretical) untenability of sociobiology from its truth allegedly being (socially or politically) undesirable. This mistake in reasoning Bernard Davis has aptly called "moralistic fallacy" (Davis 1986, pp. 34–36). In contrast to naturalistic fallacy which, according to Moore, consists (roughly) in arguing from "is" to "ought", moralistic fallacy is committed by arguing from "ought not" to "is not".

In fairness to the critics of sociobiology and "hereditarianism" who are indeed known to be quick in imputing to their opponents the political attitudes which they deplore, it must be said that, at least in their better moments, they are not guilty of this really elementary logical mistake. Actually, they often take great pains to make it clear that the political imputation is just a second, separate step in their argument, the first and the crucial one always being the refutation of their opponents' views. The strategy thus invariably begins with "error search", and it is only its success that can open the path for the second stage (the political unmasking). The more serious scientific errors that can be unearthed in, say, the work of sociobiologists and "hereditarians", the more warranted the ascription of political attitudes to them as the explanation of these very errors.

I shall argue here that such an explanatory legitimization of political imputations has more difficulties than it initially appears, and that, besides, the encouragement of this type of discussion will tend to create an obstacle to the free exchange of ideas in science.

First of all, there is an obvious question: what kind of scientific "error" is assignable to *political* bias? For, even concerning socially and politically sensitive issues there is always place for a *bona fide* mistake. However strongly a scientist were assured of the truth of his views, it would be plainly absurd if he pronounced the contrary belief to be "externally", *i.e.*, politically mediated on the sole ground that *he* considers it wrong. To substantiate a claim like this, more is needed than a sheer subjective conviction that the other side is in error.

Occasionally, though, the mere fact that the belief opposite from one's own perseveres is considered sufficient basis for hypothesizing the illegitimate influence of political attitudes. Let me illustrate this with just one extreme example. The well known critic of the heritability of intelligence, psychologist Leon J. Kamin is fully aware that most of his colleagues disagree with his uncompromising environmentalism. He even admits that his conclusion "is so much at odds with prevailing wisdom that it is necessary to ask, how can so many psychologists believe the opposite?" His answer: "[t]he I.Q. test in America, and the way in which we think about it, has been fostered by men committed to a particular social view" (Kamin 1974, p. 1).

More usually, however, a stronger basis is sought to support the political imputation. The standard procedure is to point out some *blatant* mistake in the opponent's reasoning and then to argue that his political bias is the most probable explanation of such a blunder. So, it is only a scientist's going very seriously astray that will justify invoking "external" causes.

The trouble with this is that in the ongoing scientific debate it may prove very difficult to ascertain objectively which mistakes are "normal" (that is, to be expected in view of human fallibility), and which ones are so gross as to be unintelligible without digging up some impairing psycho-political influence. Especially in politically heated controversies there will be a tendency for overkill: people will be strongly disposed to exaggerate the methodological weaknesses of the theories they oppose, and to dismiss them as products of inexcusably sloppy thinking.

A case in point is an alleged elementary fallacy very often attributed to Arthur Jensen. His critics (R. Lewontin, S. J. Gould, D. Layzer, R. C. Richardson and many others) have repeatedly argued that Jensen was guilty of cardinal *non sequitur* when he inferred (as he did) the high heritability of inter-group differences of intelligence from the high heritability of within-group differences. Robert Richardson was most explicit in taking this "fallacy" as sufficient ground for launching a very serious political imputation:

> How might we explain this blindness on Jensen's part? It is exactly here that the point that his doctrine is a racist doctrine – as it manifestly is – enters in. The latest racism explains the persistence of the view despite its untenability on scientific grounds. (Richardson 1984, p. 407[3])

Is it really true that Jensen's inference rests on nothing but incredible oversight and logical blindness on his part? When we turn to his 1969 article from the *Harvard Educational Review* which initiated the whole discussion, we find out in fact that he had a positive and quite specific ground for making a transition from within-group heritability to inter-group heritability. The ground was the following statement which Jensen described as being "practically axiomatic" among geneticists:

> Any groups which have been geographically or socially isolated from one another for many generations are practically certain to differ in their gene pools, and consequently are likely to show differences in any phenotypic characteristic having high heritability. (Jensen 1969, p. 80)

Moreover, in later exchange over this point he has disputed his opponents' claim that there is no relation whatever between the two heritabilities by citing a formula (derived by geneticist J. L. Lush), according to which the between-group heritability is a monotonously increasing function of within-groups heritability (Jensen 1973, p. 146; Jensen 1976, p. 104).

Needless to say, this is not the end of the controversy. One could (and did) object to Jensen's so reconstructed argument too; the important result, however, obtained from this closer look at the debate is that his standpoint is certainly not so manifestly irrational as to cry out for ideological imputation. But it should also be said here that if Jensen's reasoning were indeed glaringly fallacious, even then the ascription of racism to him would by no means follw without further ado. Other possible psychological explanations would have to be taken into consideration as well (*e.g.*, unwillingness to change an already expressed opinion, presence of "blind spots", inadequate knowledge of genetics, etc.). It is in particular the *unconscious* political bias (which is the most frequently hypothesized form) that will typically be quite difficult to demonstrate because of its subtle way of causal operation.

In a sense, it is ironical that exactly those scientists who like to criticize "hereditarians" for not being aware of the arresting complexity of the task of explaining human behaviour are themselves disposed to engage in such crude and inept speculations about the psychology of their colleagues. No one should really be surprised when this overeagerness to find politics everywhere leads sometimes to bizarre results. For instance, in a book written very much in this spirit of political debunking S. Rose, R. Lewontin and L. J. Kamin concluded that Francis Crick's formulation of the "central dogma" of molecular biology (that information always goes from DNA to protein and never from protein to DNA) restates "the essential ideological concern of this mechanist tradition", while at the same time they exalted as a paradigm of sientific understanding the view about "the essential dialectical unity of the biological and the social" advocated by Mao Tse-tung in his "On Practice", a text that was in fact originally (in 1937) delivered as a lecture to an anti-Japanese military-political academy in Yenan (Rose, Lewontin & Kamin 1984, p. 60 & p. 76).

The tendency to regard the scientists' professed views as always suspect of hidden political bias and not to hesitate openly to voice these doubts on the scantiest evidence has yet another far-reaching consequence. The mere expression of some ideas will in this way be made difficult through the build-up of a great external pressure: those who happen to tread some "dangerous ground" and who may anticipate that their opinions will with virtual certainty be unmasked by someone or other as the pure vehicle of a certain political interest (racism, conservatism, the New Right, etc.) will think twice before going public. Robert A. Gordon nicely described this phenomenon:

It is troubling to realize that a wider, and perhaps ultimately stronger, scientific argument can easily be the more vulnerable argument politically, and that aspects of it might sometimes be suppressed voluntarily in favour of a narrow statement, perhaps with the author's intention of presenting those aspects elsewhere, in a more cloistered setting, before a smaller audience, at some future time - which can mean never. (Gordon 1987, p. 87)

A remarkable empirical confirmation of the political factors occasionally preventing the prevailing scientific opinion to be publicly recognized as such was recently brought to light by Mark Snyderman and Stanley Rothman (Snyderman & Rothman 1986, 1987). They made a survey of scientific opinion on the usefulness of intelligence tests in general and on the heritability of intelligence in particular. The sample consisted of the scientists who where most competent to judge these issues, but the class was made wide enough, so as not to include only those who might have had a vested interest in defending tests. The two authors were themselves fairly surprised when they discovered not only that the use of intelligence tests was approved of by the majority of these experts, but that, moreover, most of them largely shared the publicly anathematized views of Jensen and Herrnstein on the heritability of intelligence.

So it turned out that the standpoint which was continually and loudly condemned as pseudo-science inspired by reactionary politics was in fact the dominant scientific opinion. The vocal minority appropriated to itself the role of representing "the official view of science", while the voice of numerous dissenters was effectively stifled: some of them just did not see, under the circumstances, any chance of success in undermining this false picture, while others (perhaps more frequently) were simply too intimidated to try.

By way of illustration, two more examples may suffice here. The first is an autobiographical remark of Sandra Scarr:

As my friends well know, I was prepared to emigrate if the bloodgrouping study had shown a substantial relationship between African ancestry and low intellectual skills. I had decided that I could not endure what Jensen had experienced at the hands of colleagues. (Scarr 1981, p. 525)

The second example comes from Jensen himself:

One professor, when asked if he would write a letter-to-the-editor of a scientific journal and include some highly cogent points he made in private correspondence about the issues raised in my *Harvard Educational Review* article, declined apologetically but frankly, saying, "I have to admit to fears, both of what would happen to me professionally if I became identified with you, and plain gut fear of being beaten up, arson, and the like. These things, if they are not here, are coming". (Jensen 1972, p. 47)

All this clearly demonstrates that the campaigning against what one sees as science with a wrong political message may in fact itself lead to the political deformation of science of a much more serious kind.

NOTES

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[1] I am here simplifying a bit by omitting some distinctions not particularly relevant in the present context.

[2] For example: "What craniometry was for the nineteenth century, intelligence testing has become for the twentieth...". "Jensen has combined two of the oldest prejudices of Western thought...", (Gould 1981, p. 25 & p. 318). But see also Gould 1987.

[3] Although less frequently, "hereditarians" are sometimes prone to return in kind and to launch a similar imputation: "The conclusion is now so strong that we must suspect those who continue to espouse theories of individual differences in personality which centre on family environment and cultural influences, of motives other than scientific" (Martin and Jardine 1986, p. 41).

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