John Beatty

Masking Disagreement among Experts

ABSTRACT

There are many reasons why scientific experts may mask disagreement and endorse a position publicly as "jointly accepted." In this paper I consider the inner workings of a group of scientists charged with deciding not only a technically difficult issue, but also a matter of social and political importance: the maximum acceptable dose of radiation. I focus on how, in this real world situation, concerns with credibility, authority, and expertise shaped the process by which this group negotiated the competing virtues of reaching consensus versus reporting accurately the nature and degree of disagreement among them.

I am concerned here with the epistemological asymmetry between scientific experts and the lay public, and the ways in which expert reporting contributes to that asymmetry. I will pay particular attention to the ways in which simplification and especially "joint acceptance" result in the withholding of information from the public—especially information about the degree of disagreement among experts. To illustrate and motivate my points, I will discuss an example from the history of science: the deliberations, in the cold war era, of a group of scientists charged with assessing the genetic hazards of radiation exposure and determining what should count as a permissible dose.

1. MASKING DISAGREEMENT: IN THE ABSTRACT

The asymmetry between scientific experts and the lay public is not just a matter of scientists *knowing more about the world*. Scientists also *know more about the state of knowledge itself*. For example, they know better who among them thinks what, and why.

One might think that the point of an expert committee report is to reduce these asymmetries. But there are features of expert committee reports that reinforce the asymmetries instead. Take for instance *simplification*. When scientific experts report to us, they do not simply tell us what they know; rather, they tell us simply. And much is withheld in the process. For example, they often leave out qualifications to their theories about the world, and also who among them insists most strongly on those qualifications.

Moreover, scientific experts often implicitly or explicitly *agree to withhold information*—for example when they "*jointly accept*" the position that they report. The notion of joint acceptance has been applied to all sorts of group communications;

https://doi.org/10.3366/epi.2006.3.1-2.52 Published online by Cambridge University Press

here I extend the notion to expert committee reports. According to Margaret Gilbert (paraphrasing slightly),

> A group jointly accepts *p* if and only if the individual members have openly agreed to let *p* stand as the position of the group. (Gilbert 1987, 194)

Joint acceptance of a position is consistent with considerable disagreement among group members, and with considerable abstention as well. I may agree to let *p* stand as my *group's* position, even though I *myself* reject it and accept *q* instead. Think of your own academic department—for example, the decisions you make with regard to hiring. You personally do not approve of hiring Jones, and several members have abstained for whatever reason, but you all dutifully report to the dean that Jones was the department's choice. Similarly, a member of an expert committee may, himself or herself, reject *p*, but nevertheless sign a report recommending it.

Consensus is critical in the case of joint acceptance, but it is consensus at a different level: not agreement concerning *p per se* (again, concerning *p per se* there may be considerable disagreement), but rather agreement to let *p* stand as the position of the group. It is, moreover, an open agreement that carries with it an obligation not to contradict *p* publicly as the position of the group. Of course, no group member would be obligated to lie and state that he or she personally accepts *p* even when that is not the case. But any dissent must be expressed carefully:

> It is understood that when a set of persons jointly accepts that *p*, then each of the individuals involved is personally obliged to act appropriately. Such action consists, roughly, in not publicly denying that *p* or saying or doing anything which presupposes its denial. More positively, one is publicly to affirm *p* and to say things that presuppose that *p* when it is appropriate to do so. There is the escape clause . . . that if one feels bound to speak against the group view, though one is not ready to challenge its status as the group view, one must preface one's remarks making it clear that one is speaking *in propria persona*. (Gilbert 1987, 194-195)

Given the meaning of joint acceptance, and the obligations incurred in an act of joint acceptance, this form of agreement can clearly lead to the withholding of information about the state of knowledge, and especially the extent of disagreement within a group. By jointly accepting a particular position, and further simplifying it, experts can significantly reinforce the epistemological asymmetry between them and their audience.

One important factor that contributes to a more candid communication of the state of knowledge, including the extent of disagreement, within an expert committee is that such groups are generally *representative* of expertise in a particular area rather than *constituting the sum* of that expertise. The signatories speak not only for the committee, but also for the profession from which they are drawn. The committee members may agree among themselves to let a particular position stand, but how can they be sure that their profession as a whole would let it stand? On what grounds can they expect peers outside the committee not to challenge the report? To avoid serious contestation from outside experts, then, members of an expert committee must represent the state of knowledge in their field in a way that is recognizable by their extended peer community.

But putting aside the opinions of experts outside a chosen committee, why would a

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minority of scientists *within* an expert committee agree to let their rivals' position stand, and in the process have their own alternative overlooked? Leaving a particular point of view out of a select committee report could have the effect of reducing the likelihood that others would pursue it in the future, and that funding agencies would consider its pursuit worthy of support.

These sorts of considerations would seem to count strongly in favor of candidly communicating the extent of disagreement within an expert panel or community. What could possibly count in favor of simplifying or jointly accepting a position, and thereby masking the extent of disagreement? One might offer paternalistic reasons. For example, an expert committee might decide to let a particular position stand in spite of considerable disagreement within the group, on the grounds that it is (supposedly) good for the public that they speak with one voice, just as it is (supposedly) good for children if their parents put aside differences in their views of child-rearing and issue univocal advice (e.g., Gilbert 1996, 353). Experts outside the committee might concur with this reasoning.

Another reason why scientific experts might simplify the state of knowledge with regard to a particular issue, or why they might let a particular position stand as the group's in spite of disagreement among themselves, is to protect their expert status. As long as they openly contest each other's knowledge with regard to an issue of public concern, they may raise questions in the minds of the lay public as to whether they know what needs to be known, and even whether they have the competence to figure it out. By withholding information about the degree of disagreement among them, a group of scientists might preserve its perceived status as the group to consult and defer to—i.e., the experts—with regard to a particular set of issues. The literature in "science and technology studies" includes the analysis of numerous cases in which scientists play active roles in acquiring expert status—e.g., by managing the public perception of disagreements among them—rather than just passively having expertise conferred on them along with whatever certificates and degrees they may have earned (an excellent study that has substantive connections to the one presented here is Balogh 1991).

An alternative way in which putative experts might maintain their relevance in the face of persistent disagreement is to appeal to their track record on issues of public concern. They may not be able to reach consensus now, but their record might suggest that, with a little more time, and perhaps more funding, they can deliver the goods in the near future. If, however, there is no track record to appeal to, or if the track record is unappealing, then downplaying their current disagreements might be crucial to gaining the public's confidence. This could be self-serving, in that the public's confidence might also pay off in terms of financial support. But it may not be entirely self-serving. The group of scientists in question might believe that they really are the public's best advisors in the long run, and that the only way to convince the public is by downplaying their differences in the short run (again, the paternalistic line). Presumably experts outside the committee in question could reap some of the same benefits from this sort of solidarity. Proponents of a minority position might not receive as much support as those in the majority (for reasons discussed above), but they would be no worse off than if their profession as a whole lacked expert status.

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So there are reasons against, and reasons for, withholding information about the state of disagreement among putative experts. One might well expect that the proceedings and reports of expert panels would reflect some compromises between candid reporting of the state of knowledge and the masking of disagreement.

Recent philosophical literature has perhaps underestimated the reasons for masking disagreement among scientific experts, albeit in very different ways. For example, some epistemologists have been (rightly) concerned to show how the lay public can rationally choose between dissenting experts (e.g., Goldman 2001)—except, of course, when the public is unaware of the extent of disagreement, or unaware of exactly who thinks what. Other epistemologists have been (rightly) concerned to articulate the productivity of differences in viewpoint and opinion (e.g., Longino 1990; and many of the authors of this volume)—notwithstanding the significant incentives and effective means for minimizing those differences.

I will turn to an example from the history of science—concerning differences among experts with regard to the genetic hazards of radiation exposure—to illustrate the above points. This is a case in which putative experts were trying to assert their social relevance in the face of a questionable track record, which included publicly aired disagreements about the issues at stake. How could they convince the public to have confidence in them? A highly publicized meeting of the minds offered the opportunity to take control of the situation. We are fortunate to have a transcript of their deliberations, in addition to the report that they most definitely "let stand."

2. MASKING DISAGREEMENT: A CONCRETE CASE

In his 1956 article, "Genetics in the Atomic Age," Curt Stern celebrated a new era in the social relevance of genetics. He acknowledged that the field had developed a reputation for offering dubious, even unseemly advice, referring in particular to the recent history of eugenics. (As an aside, there is no clear way to say for sure whether, in the wake of the Holocaust, the public had really lost its trust in genetics as source of sound advice. But geneticists like Stern certainly worried that that was the case.) Nevertheless, he continued, there was good reason for the public to turn to genetics for advice in the atomic age.

The prototype of the new atomic-age geneticist, according to Stern, was Hermann Muller, a Nobel Prize winner for his discovery of radiation-induced mutation. The timing of Muller's award, in 1946, seemed itself to underscore the relevance of genetics to the atomic age, following as it did so closely on the heels of the bomb, amid considerable speculation about genetic damage to descendants of the survivors in Hiroshima and Nagasaki. Muller had capitalized on his new-found eminence to drive home to the public the genetic hazards of radiation from bomb testing and possible nuclear war. The survivors at Hiroshima and Nagasaki served him well in this regard: as Muller said of them, "if they could foresee the results [for their descendants] 1,000 years from now . . . , they might consider themselves more fortunate if the bomb had killed them" (*New York Times*, 1 Nov. 1946, 21).

Ostensibly, Muller's chief detractor was the Atomic Energy Commission (AEC;

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the successor to the Manhattan Project), which had the combined (and somewhat conflicting) responsibilities of promoting military and peaceful uses of atomic energy, on the one hand, and on the other hand guarding the public and atomic workers from the dangers of radiation exposure. Muller was a nuisance to the AEC on account of his fearful predictions of generation-after-generation of radiation-induced abnormalities. Once the agency surreptitiously attempted to silence him by having him removed from the program of a major international conference. But its role was soon exposed (Kopp 1979; Carlson 1981, 352-367).

AEC officials sometimes claimed that the biological (including genetic) effects of radiation exposure from bomb testing and other sources were negligible (e.g., United States Atomic Energy Commission 1954; see also Kopp 1979). But with respect to the genetic effects in particular, they mostly chose to rebut Muller by emphasizing the lack of consensus among geneticists concerning the hazards of radiation. Muller and his similarly minded peers liked to portray the disagreement as if it were just between the geneticists, on the one hand, and AEC officials and other non-experts on the other hand. But in fact there was considerable disagreement among the geneticists themselves. I will briefly mention just a few of the contentious issues.

For example, evaluations of the impact of increased levels of radiation depended on general assessments of the deleteriousness of mutation, and estimates of the extent of existing genetic variation, both of which were very controversial subjects.

Muller himself believed that species are so finely adapted to their environments that any random mutational changes could only be deleterious: natural selection has accumulated the optimal genes for most every trait, so that the slight amount of genetic variation that remains consists almost entirely of recent mutations that have yet to be eliminated. Humans, he argued, are an exception in this regard, due to conditions of civilization like medicine and various forms of social welfare that have relaxed natural selection and have resulted in the accumulation of mutations that would otherwise be quite debilitating. Adding to that "genetic load," from his point of view, could only make matters worse (Muller 1950).

According to other prominent geneticists, like Theodosius Dobzhansky and Sewall Wright, populations and species have large stores of genetic variation, which hardly put them at risk, but rather help to ensure their evolutionary adaptability to changing environments. That variation is maintained, they argued, by a number of different mechanisms, including selection of heterozygotes (that have two different genes for a trait) over homozygotes (that have two copies of the same gene for a trait), the idea being that the former are metabolically more versatile than the latter. So in other words, genetic variation is good for populations and species, and also for their individual members (Wright 1931, 148 ff.; 1955; Dobzhansky 1937, 126-127; 1955).

Neither Dobzhansky nor Wright ever argued that more variation from radiation exposure would be even better. But they did argue that it was difficult to generalize about how detrimental (or beneficial) any new mutation might be. The reason is that, whether a new mutation is deleterious or not depends on the other genes that it works in combination with. But if we are all quite different genetically, then the impact of a particular mutation will differ from person to person. There is no way to generalize, and

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hence no way to know for sure what the impact of increased levels of mutation would be.

The AEC was well aware of these disagreements, funding, as it did, many of the most influential geneticists of the period (including Muller). AEC spokespersons could thus report "a rather wide range of admissible opinion on this subject" (United States Atomic Energy Commission, 1955; see also Strauss 1955, 36).

News coverage of professional meetings, like the First International Congress of Human Genetics, in Copenhagen in 1956, also revealed seeming schisms within the genetics community. Thus, on August 1 of that year, the *New York Times* ran a brief report on the congress with the headline, "Geneticists Find No Atomic Harm" (17), only to follow up on August 6 with another report from the congress with the headline, "Geneticists Caution Against Radiation" (5).

Commentators suggested that these ongoing arguments might reflect something gone wrong with the science. Daniel Lang of the *New Yorker* wrote on behalf of "many laymen who have come to expect scientists to be starkly objective in their approach to technical problems, and whose schooling pretty much encouraged the belief that there is always one right answer to any question concerning science . . . " To such people, "the current disagreement among the authorities is both exasperating and baffling, if not actually frightening" (Lang [1955] 1959, 382; keep this wording in mind).

It is difficult to reconstruct how much the public really knew about the controversy among the geneticists. But as we shall see, the geneticists were certainly concerned that the perception of disagreement among them was costing them their expertise and social relevance. A major opportunity to make a unified stand—publicly—came with the naming of a distinguished panel, sponsored by the U.S. National Academies of Science (NAS), whose task was to report on the genetic hazards of radiation exposure. The panel included many of the most accomplished geneticists of the period (there were sixteen members, thirteen of whom were geneticists). Upon its release, the widely anticipated report was published in its entirety in three full pages of the first section of the *New York Times* (13 June 1956, 18-20). It included acknowledgement of "some differences of opinion among geneticists," but also the assurance that there was "*no disagreement as to fundamental conclusions*" (emphasis in original).

Some of the best histories of the atomic age, and of the genetic issues in particular, take for granted widespread agreement among the geneticists about the hazards of radiation, and attribute most if not all the dissent to the AEC (e.g., Divine 1978). To that extent, the NAS panel, and other committees and initiatives, succeeded in containing the controversy. As I will show, the NAS panel achieved this end by means of simplification and the joint acceptance of their report.

Reaching that level of agreement—to let the report stand—was not easy. Much credit goes to the chairperson, who was not a geneticist. There had been so much disagreement among the geneticists that there was not one among them that the rest could accept as a neutral convener. So the NAS went outside the genetics community and chose for the job Warren Weaver, who had considerable experience managing research projects and initiatives for the Rockefeller Foundation.

In his opening statements, Weaver emphasized over and again the ways in which the

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social relevance of genetics was at stake. Their gathering, he suggested, was

> ...a very special opportunity. If the scientists seize this dramatic occasion and take proper advantage of it, I think this could have a large and beneficial effect on the whole relation of science to the public. (NAS 1956a, 10)

The difficulty was, he acknowledged, that there were considerable differences of opinion among the panel members, as the public seemed to be aware. This was understandable enough, Weaver reasoned, given the complexity of the issues and the insufficiency of the evidence. Under the circumstances, one could hardly offer unqualified advice:

> [T]here simply is not enough knowledge in this field to permit accurate, dependable, and logically formulated answers to logically formulated questions. This is one of the main reasons, of course, why the geneticists have themselves debated these issues before the public in the way in which they have. (*ibid*., 11)

One possibility, Weaver suggested, was just to be frank about the state of knowledge. Unfortunately, he continued, the public believes that scientists (presumably by virtue of their objectivity and methodology) are uniquely qualified to reach consensus, and when they fail to do so, something must be amiss. But surely the public could be made to appreciate the risks of coming to a premature consensus:

> The public has undoubtedly noticed [the disagreements], and has probably been confused and troubled by it. Science is supposed to be definite—open or shut. Things are supposed to be so or not so. . . . [But] the public should recognize that the attitudes and statements of geneticists about this problem of radiation damage have resulted not from eccentricity or irresponsibility, but on the exact contrary, from deep concern and from attempts to exercise due caution in a situation that is in essence complicated and confused. (*ibid*., 24)

But then, as if to acknowledge the absurdity of this strategy of openness, Weaver turned to a distinction that he wanted the panel to consider—between what he called "communicative accuracy" and "scientific accuracy." The committee should, he urged, aim for a communicatively accurate report, which is to say a report that increases the understanding of the audience, without necessarily being scientifically accurate.

> Now it can have communicative accuracy when...it does not have scientific accuracy at all. And very frequently when scientists read something, if they are awfully good and meticulous gentlemen, with a high sense of intellectual responsibility, they will read a sentence and they will say, "But, fellow, you cannot say that to people. That just isn't so. You must not say that to people. That requires 13 qualifications," etc., etc. .... Well, this is the distinction between scientific accuracy and communicative accuracy. (*ibid*., 24-27)

In a similar reversal about just how much information should be communicated to the public, Weaver at first suggested that the panel take votes on each issue and report the results:

> ... which would go like this: C9, I4, U3.
>
> Now what this means is that of the geneticist members of this panel and some of us who disqualify ourselves, there were 9 who will say, "Well, by and large, gentlemen, I consider this

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> remark to be correct. Don't sue me on details, but I mean by and large I consider it correct." Three that would say, "By and large I consider it incorrect," and there would be three more who would say "U," and that stands for "Unable," and that means either "I don't think it was a good question," or "I think the question needs an awful lot more clarification and qualification before anybody can try to usefully answer it," or "I don't think we know enough to attempt to answer it," or something of that sort, you see. (*ibid*., 42-43)

But then Weaver made it clear that he was elaborating such a strategy of openness only in order to show how inappropriate it would be. It would completely undermine the geneticists' authority:

> Well now, this is I think first of all perhaps a little too much mechanism. But I don't think we should hesitate to report to the public certain perfectly honest and understandable degrees of disagreement and what they are based on. I don't think that is the wrong thing to do. We have to be awfully careful. We must not do this in a way that runs two very serious dangers. First of all, I don't think it does any particular good. It just scares the pants off the public. I don't think that is useful. Secondly, we have to be very careful that we don't do science a disservice in this procedure by having a lot of people say, "Well, by George, just what I thought. They don't know anything about it themselves." We must avoid these dilemmas. (*ibid*., 43)

Several of the geneticists immediately responded that the differences between them were not that great. Muller suggested that the main differences were between geneticists and non-geneticists (*ibid., 44)*. Nonetheless, the issue arose as to whether one or more of them, or others outside the committee would issue a "minority report" at odds with the position of the committee. There was no enthusiasm for this way of handling disagreement. On the one hand it was urged that a minority report would be unnecessary as long as the panel reported only the areas of consensus. But the more likely means of avoiding a minority report would be compromise. As Alexander Hollaender argued,

> I kind of doubt actually whether we can get all geneticists to agree with even the statements of this group [i.e., even the statements that the committee members might agree on]. I suspect when it comes out there will be some minority reports coming out, but I think it desirable for us to make every effort to avoid a minority report from this committee; that is, I think we should try to compromise with each other's points of view as much a possible, so that one will come out as a solid committee decision of this group. Maybe that won't be possible but I think it will be highly desirable to work for. (*ibid*., 52-53)

An important concern of the geneticists was that if they could not come up with a univocal statement and recommendation, then someone else or some other group would step into the role. This was not merely a matter of the geneticists losing their authority with regard to these issues, but also reflected their concern that someone or some group *less qualified* would make *worse* decisions. Some decision or other was bound to be made, so that "the situation is forced on us because otherwise we will have no influence on the matter at all" (*ibid*., 42). They might not be able to reach consensus at present, but by making a recommendation now, they could at least stake a claim to authority for genetics, so that some future group of geneticists could authoritatively revise the recommendation if necessary:

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> ...as the result of accepting the responsibility for this we shall be regarded as the authority and if we find we are wrong it will be easier for this committee, or a succeeding committee of geneticists, to change the recommendation. (*ibid*., 52)

In this way, the geneticists could "take some control over the situation" (*ibid*., 52) ahead of "somebody else."

> I mean it has been said before if we don't do it somebody else is going to do it, but largely it seems to me it boils down to the question of whether we do it or whether we let somebody else do it. (*ibid*., 80)

Of course, that "somebody else" might be the AEC, which had already made much of the geneticists' disagreements, and might well use this as an excuse to assume the mantle of authority and make recommendations about dosage levels, etc. As Weaver urged, the AEC could well argue that,

> You fellows have no business to recommend a change in this figure, the permissible dose or whatever you want to call it, you have no business to recommend a change in this because you don't have any really sound basis for recommending the change. You don't know what it ought to be. You say you don't know what it ought to be and, therefore, you ought to leave it alone. (*ibid*., 93)

So the meeting began with many gestures toward unity against outsiders and in the eyes of the public. But as soon as the geneticists began to address the substantive issues, their differences emerged front and center. One issue that Weaver had hoped to get agreement on was the recommendation of a maximum permissible dose of radiation. This discussion started off contentiously. At one point, James Crow offered a way out of the impasse by suggesting that the panel members try to reach consensus on a permissible dose without trying to reach consensus on the manner of arriving at it (*ibid*., 95). But this did not move those members of the panel, like Wright, who considered the issue indeterminate. Wright responded, predictably, that such a recommendation would have to be based on some understanding of the general deleteriousness of mutations. But what could one really say in that regard?

> ... when we speak of all mutations being injurious, I think there is a big qualification there and probably if we leave out the lethals and semi-lethals and the very conspicuous ones, if we can talk about small mutations, the great bulk of them probably are always injurious in some combinations and beneficial in other combinations.

Moreover,

> The last 100 generations of conditions of life in man, of course, have changed enormously, and there is a possibility that a little more plasticity in germ plasm in man may actually be an advantage in evolution. That is, probably all of us are full of what were highly deleterious mutations ten thousand years ago but are desirable now, along the lines of "Blessed are the meek for they shall inherit the earth." There are certain types of character that would have been very undesirable ten thousand years ago that have a distinct social advantage now.
>
> So we are undergoing a period of change, and the question is balancing an increased plasticity of the germ plasm under these conditions of geologically extremely rapid need for

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> readaptation against the fact that probably most of the mutations—well, perhaps 100 per cent of the mutations have a net effect at the present moment that is deleterious, beneficial in 40 percent of the combinations in which they enter individuals and injurious in the other 60 per cent and the net effect is injurious. That is what we mean by injurious mutation. You can't properly speak of a mutation as being either beneficial or injurious. (*ibid*., 99-100)

After some discussion of Wright's points, Alfred Sturtevant asked him about his claim that forty percent of all small mutations were beneficial in the gene combinations in which they arose: "I have one question, Dr. Wright. Where did you get this figure of 40 per cent? If I had been asked to estimate it I would have said 4 (*ibid*., 101-102)." To which Wright replied, "I pulled it out of the air" (*ibid*., 102). Wright's point being that one could at best guess.

Nor was Wright the only member of the committee who was skeptical about the possibility of determining the general deleteriousness of mutation. James Neel was personally unwilling to recommend any particular permissible dose on these very grounds:

> ... I, for one, am unprepared to sign any report which gives a permissible dose. I do not doubt that radiation at all levels produces mutations. I believe it is highly probable, but not proven, that under the considerations of western civilization the net effect of increased mutation might prove undesirable.
>
> I might interpolate that you can argue that man is passing very rapidly into quite a different set of selective factors than he existed under two thousand years ago, and this is the circumstance where you might need an increased store of genetic variability. (*ibid*., 78)

Ultimately a *subgroup* of the geneticists agreed to meet in the afternoon—in lieu of a continuation of the general panel discussion—to discuss the question of a maximum permissible dose. The next morning they reported back the figure of 10 roentgens.

Discussion and debate about this figure went on for some time. The main reason offered for the figure was that 10 *r* would raise by one quarter the so-called "spontaneous" (naturally occurring) mutation rate. 20 *r* would raise the rate of mutation by one half, which the subgroup deemed too great. 5 *r* would have raised it by only one-eighth, but would, the subgroup felt, be unenforceable (*ibid*., 126-130).

Sensing that this Goldilocks strategy was not the best way to defend the 10 *r* recommendation to the public, Weaver proposed another assignment. He asked the panel members to try on their own to calculate what would be the genetic effects of administering 10 *r* to every reproductive age American—what would be the genetic effects on the 100 million offspring of the next generation of Americans (*ibid*., 238 ff). Go home and think about it, Weaver implored, "cinch up your belts" and give it a try (*ibid*., 255).

After this time, the panel members communicated only by correspondence and telephone, mainly by circulating their calculations and their proposed amendments to Weaver's multiple drafts of the report. In rewriting the report three or four times, Weaver took the suggestions of the panel members into account as best he could while trying to maintain consistency (though consistency was sometimes sacrificed). Prior to his final version, he circulated a ballot for voting on the final changes. Minority disagreements of three or less were disregarded. Closer votes were handled by one or another form of

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compromise (Weaver to Genetics Committee, 29 May and 2 June 1956, Beadle Papers, Caltech Archives, Accession 70.2; all of the correspondence cited in this paper is from the same archival collection, and so I will henceforth cite only the accession numbers: "70.x").

The final draft acknowledged that the report presented a simplified account of the issues that "would require various qualifications and a lot more detail to attain full technical precision," but should still "be recognized by, and it is hoped will not disturb, the more technical reader" (presumably other geneticists; NAS 1956b, 3).

The report also acknowledged what the public already seemed to know: that there were disagreements among the experts with regard to the issues at stake. But, it claimed, these differences had mostly to do with details, not "fundamental conclusions," and should not undermine confidence in the science, nor raise any questions about the "social importance" of the science:

> Does this mean that geneticists have, at the moment, nothing useful to say on this grave subject? Fortunately, this is not the case. We do know something, though not nearly enough to give definite answers to a great many important questions. There is a considerable margin of uncertainty about much of this, and as a result, there are naturally some differences of opinion among geneticists themselves as to exact numerical values, *although no disagreement as to fundamental conclusions* [emphasis in the original text].
>
> Many people, moreover, suppose science to be definite—open or shut. Things are supposed to be so or not so. And therefore some persons may, quite mistakenly, conclude that geneticists are unscientific because they do not completely agree on all details.
>
> In relatively simple fields, where both theory and experiment have progressed far, a comforting kind of precision does often obtain. But it is characteristic of the present state of human radiation genetics that one must carefully and painstakingly note a lot of qualifications, of special and sometimes very technical conditions, of cautious reservations. The public should recognize that the attitudes and statements of geneticists about this problem of radiation damage have resulted from deep concern and from attempts to exercise due caution in a situation that is in essence complicated and is of such great social importance. (*ibid.,* 6)

The report did not mention the procedures of voting and the amount of compromising that had been employed in its writing. One of the compromises involved the way in which Weaver (and the majority) wanted to express the detrimental effect of new mutations. Wright insisted on substantial revisions to this section, and threatened to resign if the changes were not made. His colleagues outside the panel knew his views on this issue; what would they think if he signed his name to a document that so misrepresented his views (Wright to Weaver, 18 April 1956, 5, 70.2; see also Wright to Weaver, 22 March 1956, 70.1; and Wright to Weaver, 22 May 1956, 70.2)? The changes Wright proposed, which involved a lot of qualifications to the notion of general mutational detriment, were grudgingly allowed (e.g., Muller to Weaver, 31 May 1956, 70.2).

This was also a case in which compromise came at the cost of some consistency. Consider the following two passages, which appear on the very same page. According to the first passage, mutations are "in the vast majority of cases" detrimental, whereas according to the second passage one can only say that the more easily "detectable" mutations are generally detrimental. Moreover, according to the second passage, the latter group of

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mutations—which are individually less detectable but which interact with other genes to produce the normal range of variation within a species—constitute "a large fraction" of all mutations. And of these mutations, the best one can say is that they are "sometimes deleterious and sometimes not."

> ...mutant genes, in the vast majority of cases, and in all the species so far studied, lead to some kind of harmful effect. In extreme cases the harmful effect is death itself, or loss of the ability to produce offspring, or some other serious abnormality. What in a way is of even greater ultimate importance, since they affect so many more persons, are those cases that involve much smaller handicaps, which might tend to shorten life, reduce number of children, or be otherwise detrimental.
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> ....
>
> [I]t is likely that a large fraction of the genes that determine normal variability are of this rather ambiguous type that are sometimes deleterious, sometimes not. Mutations within this sort would not necessarily be harmful. Such mutations presumably occur, but geneticists do not know what fraction of all mutations are of this type, for they are not ordinarily detectable. (*ibid.,* 12)

Immediately following the latter passage, the focus of the report narrowed to the dangers associated with the "relatively detectable mutations," without any suggestion as to the proportion of mutations that fall within that particular category (*ibid.,* 12, 15).

Especially interesting is the fate of Weaver's proposal that the geneticists try to calculate the genetic damage done to the 100 million American children born to parents exposed to the recommended maximum permissible dose of 10 *r*. Recall that all of the geneticists were asked to participate. Six of the committee members actually submitted figures. Several distinguished geneticists, including Wright, Neel and Milislav Demerec, explicitly refused to contribute a number to the final report, on the grounds that such a calculation relied on unknown quantities, and/or because calculations of only the number of deleterious and slightly deleterious mutations was not a good way to represent the overall genetic impact of radiation. The geneticists who did submit numbers, and their "minimum," "most probable" and "maximum" estimates, are included in the following table (Crow to Weaver, 21 May 1956, 70.6):

	minimum	*most probable*	*maximum*
George Beadle	100,000	2,000,000	200,000,000
Bentley Glass	100,000	4,000,000	200,000,000
James Crow	250,000	5,000,000	72,000,000
Alfred Sturtevant	600,000	6,000,000	60,000,000
William Russell	700,000	7,000,000	70,000,000
Hermann Muller	2,500,000	10,000,000	25,000,000

There is also a record of a single figure (350,000; presumably a most probable value) arrived at by Wright, but no calculation to go with it (see the hand-drawn graph that accompanies Crow to Weaver, 21 May 1956). Again, in the end, Wright refused to be party to this particular activity.

https://doi.org/10.3366/epi.2006.3.1-2.52 Published online by Cambridge University Press

Reflecting on the wide limits surrounding the figures that came in, Crow predicted that the public would have no confidence in the numbers. He proposed that the group either omit the numbers entirely, or else settle on a single best estimate, or some narrow range of estimates (Crow to Weaver, 29 March 1956, 70.1). The group voted seven to six, with two abstentions, not to include the table (Weaver to Genetics Committee, 29 May and 2 June 1956, 70.2).

According to the published version of the report,

> Six of the geneticists of this committee considered the following problem: suppose the whole population of the United States received one dose of 10 roentgens of radiation to the gonads. What is the estimate of the total number of mutants which would be induced by this radiation dose and passed on to the next total generation of about one hundred million children? Each geneticist calculated what he considered to be the most probable estimate, and then bracketed this by his minimum and maximum estimates. Each thus said, in effect: "I feel reasonably confident that the true value is greater than my minimum estimate and less than my maximum. My best judgment, as stated in a single figure, is what I have labeled the most probable estimate."
>
> The most probable estimates as thus calculated by the six geneticists do not differ widely. They bunch rather closely around the figure of 5,000,000. Four of the six estimates are very close to that figure, and the other two differ only by a factor of 2.
>
> These six geneticists concluded, moreover, that the uncertainty in their estimation of the most probable value was about a factor of 10. That is to say, their minimum estimates were about 1/10, and their maximum estimates about 10 times the most probable estimate. (*ibid.,* 26-27)

While it is true that "six of the geneticists of this committee considered the following problem," it is truer that all thirteen geneticists were asked to participate and seven declined. As for the calculations that were submitted, they seem to have been mostly independent (Glass's was "corrected"). The differences in minimum, maximum and most probable values are actually not great. The most probable values are quite similar (leaving out Wright's figure). Nonetheless, there was the feeling that the differences should not be communicated so candidly, especially the range of uncertainty, which is narrower than the table suggests.

3. CONCLUSION

In the end, all the geneticists signed the report, and in so doing very publicly agreed to let it stand as the committee's position, in spite of considerable disagreement about key issues. A similar understanding, with similar results, seems to have been reached by another, overlapping panel of geneticists convened by the World Health Organization to consider similar issues. As Muller, who also served on the WHO committee, confided to Beadle,

> It is important for our group to realize that there was a deep split in the WHO group, somewhat similar to that in our group, a split which is hardly to be discerned in their report, any more than in our own report (if as much). (Muller to Beadle, 27 August 1956, 70.3)

https://doi.org/10.3366/epi.2006.3.1-2.52 Published online by Cambridge University Press

To a certain extent, the agreement to let the report stand in spite of the many disagreements was motivated by the geneticists' concern to establish their social relevance and authority in the face of public concern (or perceptions of public concern) about past performances ranging from the earlier eugenics movement to the more recent airing of disagreements about the genetic effects of radiation exposure. But the decision was not merely self-serving. It was also motivated by a genuine concern not to allow a less knowledgeable group to take advantage of the geneticists' disagreements and impose far less justifiable standards. As Muller had urged, sarcastically, at one point in the deliberations,

> Is there any use of being geneticists when we guess at this? Can we get anywhere nearer by being geneticists than by not being? I mean, if none of this data is of any use to us why can't the man in the street guess just as well? (NAS 1956a, 141)

Even if simplification and joint acceptance are in fact common features of expert committee reports, one might still find unfortunate the effect that they have in reinforcing the epistemological gap between experts and the lay public. However, there may be some epistemological virtues associated with the aim of joint acceptance. I will be brief here; these suggestions are for following up elsewhere.

Consider the alternatives to a jointly accepted report. Aiming for a merely "aggregative" report of yeas, nays, and abstentions—the sort that Weaver rhetorically proposed, only to dismiss—might make disagreement among experts manifest, but might not encourage deliberation among them (in the same way that aggregative conceptions of democracy do not necessarily encourage deliberation among citizens; e.g., Gutmann and Thompson 2004). The particular level of consensus aimed for in a jointly accepted report—namely, agreement to let a report stand—may encourage more productive, back-and-forth consideration of the issues. The committee report at issue here certainly benefited from the deliberations leading up to it, despite the extent to which it masked the disagreements that produced it.

Rather than aiming for a merely aggregative report, or a jointly accepted report, a group might settle for no less than a one-hundred percent consensus report (if there could possibly be such a thing; consider the difficulties in achieving and measuring consensus discussed in Gilbert and Mulkay 1984). This would certainly encourage deliberation, but probably only up to a point, beyond which it would lead to quitting. Aiming for a jointly accepted report might encourage the most extensive deliberation, whatever other disadvantages it might have.

ACKNOWLEDGEMENTS

I am grateful for feedback from Naomi Oreskes, James Collins, the STS Collective at the University of British Columbia, and participants at the Third Annual Episteme Conference in Toronto, especially Alison Wylie, Miriam Solomon, and Deborah Tollefsen.

https://doi.org/10.3366/epi.2006.3.1-2.52 Published online by Cambridge University Press

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https://doi.org/10.3366/epi.2006.3.1-2.52 Published online by Cambridge University Press

John Beatty works on foundational issues in biology, and issues concerning the relationship between biology and the state. He taught for many years in the Program in History of Science and Technology, and the Department of Ecology, Evolution and Behavior, at the University of Minnesota, and moved in 2003 to the Department of Philosophy at the University of British Columbia.

https://doi.org/10.3366/epi.2006.3.1-2.52 Published online by Cambridge University Press