

**SCIENTIFIC MISCONDUCT BY THE U.S. NATIONAL ACADEMY OF SCIENCES IN
RECOMMENDING LNT FOR RISK ASSESSMENT**

Edward J. Calabrese, Ph.D.

Professor of Toxicology

Department of Public Health

Environmental Health Sciences

Morrill I, N344

University of Massachusetts

Amherst, MA 01003

Phone: 413-545-3164

Fax: 413-545-4692

E-mail: edwardc@schoolph.umass.edu

SCIENTIFIC MISCONDUCT BY THE U.S. NATIONAL ACADEMY OF SCIENCES IN RECOMMENDING LNT FOR RISK ASSESSMENT

ABSTRACT

The National Academy of Sciences Biological Effects of Atomic Radiation I (NAS BEAR I) Committee Genetics Panel (1956) recommended that regulatory agencies switch from a threshold to a linear model for radiation risk assessment of genomic endpoints. This recommendation was generalized to somatic cells for cancer risk assessment and later applied to chemical carcinogens. At the heart of the recommendation were independent estimates by panel geneticists of adverse reproductive effects in humans exposed to a given radiation dose. This paper reveals both an enormous variance and uncertainty in the independent estimates of genetic damage in reports submitted by six geneticists. These estimates were far greater than reported in *Science*, substantially misrepresenting expert disagreement. While the *Science* paper indicated that only six (of the 12) geneticists provided estimates, nine did. The three excluded estimates indicated markedly lower damage than did the six. The censoring of the three estimates and the mischaracterization of the uncertainty of the remaining six geneticists deliberately and substantially overstated the degree of confidence and agreement in the estimates of genetic harm. Based on internal correspondence, these actions were designed to ensure that linearity at low dose would be adopted for risk assessment. The omission and misrepresentation of data intended to represent the independent appraisal of panel experts is consistent with definitions of falsification and fabrication of the research record. The principal scientific document therefore supporting the adoption of the LNT was the product of scientific misconduct and that its adoption by regulatory agencies worldwide was based on a process involving falsification and fabrication.

Key Words: linearity, dose response, risk assessment, threshold, mutation, cancer

Introduction

Cancer risk assessment policy in the United States (US) originated from the US National Academy of Sciences (NAS) Biological Effects of Atomic Radiation (BEAR) I Committee, Genetics Panel, in 1956 when they recommended a shift from a threshold to a linear dose-response model for the effects of ionizing radiation on genomic mutations. This linearity recommendation was soon generalized to somatic cells by the National Committee on Radiation Protection and Measurement (NCRPM) and applied to cancer risk assessment (Whitemore 1986 – Page 608, Footnote 260). This perspective was widely adopted in numerous countries by various regulatory and public health agencies (e.g. International Committee for Radiation Protection-ICRP) and continues to the present (Calabrese 2009, 2011a, 2013)

Historical reassessment of the BEAR I Genetics Panel report and its scientific foundations has revealed a series of key scientific concerns, flaws, and misrepresentations that challenge the basis of the original linearity recommendation (Calabrese 2011b, 2013). These findings provide substantial evidence that the switch from threshold to linearity occurred due to ideologically-based deceptions of key papers on ionizing radiation induced mutation by leaders in the radiation genetic community, most notably Hermann J. Muller, Nobel Prize winner, and Curt Stern, University of California-Berkeley professor.

The current paper extends this historical re-evaluation into the activities of the BEAR I Genetics Panel with particular focus on the key publication of that panel in *Science*, entitled “Genetic Effects of Atomic Radiation”. The *Science* paper represents the “technical” publication (Anonymous 1956) of the panel whereas a “popular” version (NAS/NRC 1956) was written for the media and public and included summaries of each of the NAS BEAR I Committee Expert Panels. Each of the technical Panels published a separate article in the journal *Science* in 1956.

The present paper originated as a result of an unexpected observation that the *Science* paper of the Genetics Panel reported genetic risk estimates of only six of the 12 geneticist members of the Panel. Yet, an investigation of the personal correspondence and unpublished technical writings of the Genetics Panel uncovered detailed assessments from nine members of the Panel. This previously unrecognized discrepancy led to a further investigation resulting in the present paper.

Estimating radiation induced genetic risk

A key finalizing activity of the Genetics Panel was to provide an estimate of the total number of offspring (including embryonic/fetal deaths/stillbirths and those born but unable to reproduce) that would be adversely affected by mutations (i.e. often referred to as “genetic deaths” –adversely affected offspring that could not reproduce) assuming the entire adult reproducing population received a single dose of 10 roentgens (R) (0.1 Gy) to the gonads. The specific charge to the Genetics Panel was given by the chairman, Warren Weaver of the Rockefeller Foundation, the organization which funded the assessment. This finalizing topic was extensively discussed on Feb. 6, 1956 in the Panel meeting, occupying about 40 transcript pages (NAS 1956a). On Feb. 8, 1956 a memo was sent by Weaver to the Genetics Panel with the subject heading of “Reminder” (Weaver 1956a). He stated the following:

“At the Chicago meeting it was agreed that every geneticist on the panel was invited and indeed urged to undertake an estimation of the expressed damage due to detrimental mutations. The estimation was to apply to the total number of children (say 160 million ?) which will in the future be born to persons now alive in the U.S.; and to their children and so on. At least three estimates are desired: 1) expressed damage to the first set of 160 million children due to a dose of 10r to the persons now living; 2) expressed damage to the

F1 through F10 generations of children due to a single dose of 10r to the persons now living; 3) expressed damage to F1 through F10 due to a dose of 10r per generation.

It would be most helpful also to have comparable estimates for 2) and 3) above when the dose is 50r. Linearity applies to 1), so that it is trivially easy to adjust that estimate to any dose.

It was agreed that each would state the range which applies to the estimates entering the calculation, and the range applying to the final result; and would state his reasoning in enough detail so that the other members could intelligently weigh, criticise (criticize), and compare the various estimates.

May I urge you that you undertake this as promptly as possible, and that you duplicate your report and send it to every member of the panel?"

The basis for the charge conferred upon the geneticists by Weaver was a discussion of a written position statement of geneticist Panel member Tracy Sonneborn that was read into the transcription record on Feb. 5, 1956. In addition to a statement of genetic risk assessment principles, Sonneborn raised the question as what would be the risk communication message for the general public based on their Panel deliberations. This led Weaver to trace three possible options with the most desirable, being the most explicit. That is, he wanted the Panel to provide reproductive risk estimates to the general public that were specific and quantitative, estimates that would build upon the belief that all doses of radiation were harmful, cumulative, and irreversible.

On page 244 of the Feb. 6, 1956 transcribed proceedings of the Panel (NAS 1956a), Weaver stated his desire to obtain the independent views of the geneticists for the prediction of

radiation induced genetic damage for subsequent generations of humans, using the US population as the working example. This was viewed as a means to define the range of informed judgment and to ascertain how much the Panel geneticists were in agreement, a key factor needed to support policy recommendations of the Panel. On page 257 of that same transcript Sonneborn stated “**that the thing of most value in all this calculation would be to show how one can use different methods to make estimates, and see to what extent methods, if possible, variations in approach, lead to different answers. So that if they converge, or tend to converge, then we might have more willingness to put them forth.**” Weaver and Sonneborn were, of course, mindful of the fact that the geneticists brought differing technical abilities, research experiences and judgments to the Panel. Panel geneticists were experts with *Drosophila*, bacteria, paramecia, mice and human population genetics. Some of the Panel members were far more mathematically oriented than others. Each was expected to independently consider the problem and then document in writing their methods, assumptions and their estimated population-based radiation induced genetic harm. These estimates were to be collectively summarized and would define the range of expert agreement/disagreement using the different but complementary approaches of each contributing expert geneticist.

The Genetics Panel

There were 13 *bona fide* geneticists who were members of the Genetics Panel. These included: George W. Beadle – California Institute of Technology; James Cotterman – Baylor University; James F. Crow – University of Wisconsin; Milislav Demerec – Carnegie Institution of Washington; H. Bentley Glass – Johns Hopkins University; Berwind P. Kaufmann – Carnegie Institution of Washington; Clarence C. Little – Roscoe B. Jackson Memorial Laboratory; Hermann J. Muller – Indiana University; James V. Neel – University of Michigan; William L

Russell – Oak Ridge National Laboratory; Tracy Sonneborn – Indiana University; Alfred Sturtevant – California Institute of Technology; and Sewall Wright – University of Wisconsin. Other members of the committee were the chairman Warren Weaver, Rockefeller Foundation, a mathematician; C. Failla, Columbia University, a health physicist; Alexander Hollaender, a physical chemist and administrator of Oak Ridge National Labs; and Shields Warren, New England Deaconess Hospital, a physician, who was also a member of the NAS BEAR I Pathology Panel. After the meeting in Chicago (February 5/6, 1956), Cotterman resigned from the Panel due to academic work load issues, leaving 12 geneticists on the committee. Of the 12 geneticist members, estimates of nine members have been obtained via searching past committee communications and letter exchanges from all members of the committee. The three members not providing estimates were Little, Neel, and Sonneborn. While there is no record of Little's actions on this matter, Sonneborn and Neel refused the request of Weaver to provide estimates of genetic damage since they did not believe that they could be reliably made. In fact, Neel argued that the scientific basis for such estimates were so uncertain that providing them would be a violation of his obligation as a scientist to society, an unethical condition. On April 6, 1956 he specifically addressed his concern with Chairman Weaver (Neel 1956a):

“The geneticist has social responsibilities, but he also has responsibilities as a scientist. One is that in an area as critical as this one is, he must beware of letting his conjectures get too far in advance of his facts. It is to me an exceedingly tenable position, having stated the general genetic argument, to say flatly that we know so little about the quantitative aspects.” In fact, Neel was so adamantly opposed to the decision to develop and provide such genetic estimates of damage that he wrote Weaver on March 8, 1956 stating that he would **“go down with flags flying and guns booming to the last”** (Neel 1956b).

Organizing the estimates

The nine estimates were provided by the end of February, 1956. At a March 1, 1956 meeting of the Genetics Panel, a decision was made on how the estimates would be processed. In a March 2, 1956 brief summary (Weaver 1956a) (Muller manuscripts (mss), Lilly Library) of the March 1 meeting, Weaver indicated that **“Jim Crow was made the chairman of a sub-committee (he can commandeer help on this if he wishes) to go through all the damage estimates, compare them, and display assumptions, methods, input, and results in some sort of chart or graphic form.”** This description did not indicate that Crow was authorized to exclude individual contributions, nor to make judgments as to which estimates were the most/least credible. Crow’s role as sub-committee Chair was to organize and integrate the submitted estimates in a coherent manner so that the entire Panel could intelligibly, efficiently and objectively view the submitted estimates.

The Science journal article

In the Genetics Panel *Science* paper it was stated that **“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 200 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.”** Note that authors published an errata one month later indicating that the 200 million number of

children should have been 100 million (Errata-*Science*, Volume 124, page 170, 1956). The paper goes on to state that **“the most probable estimates as thus calculated by the six genticists (geneticists) do not differ widely. They bunch rather closely around the figure 5 million. 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.”

Inconsistencies emerge from the Science paper

A detailed evaluation of the Genetics Panel article in *Science* reveals a number of anomalies and concerns.

1. The invitation to provide estimates was made with some urgency to all geneticists on the panel. The reader is informed that only six provided estimates. The specific six geneticists were not identified in the *Science* paper. The *Science* paper did not provide any description of the biological models and methods used by the six geneticists to obtain their respective estimates. While only six geneticists were stated as having **“considered the ...problem”**, all 12 geneticists were urged to take on this task. We also know that the number of geneticists providing written assessments was nine. Based on a letter from James Crow to Warren Weaver on May 21, 1956 (Crow 1956a), he listed the names of six geneticists on the Panel who provided estimates. These geneticists included Beadle, Crow, Glass, Muller, Russell and Sturtevant. It is this group of six that the *Science* article values were based upon. However, detailed mutational estimates were provided in a

professional and timely manner consistent with the request of Weaver by three other Panel geneticists [Demerec (2/14/56 letter; 2/11/56 document; and stamped received 2/16/56) (Demerec 1956); Kaufmann (2/27/56 letter to Weaver and stamped received 2/29/56) (Kaufmann 1956); and Wright (2/22/56 letter to Weaver and stamped received on 2/24/56 (Wright 1956)]. Each of these three documents (Demerec, Kaufmann and Wright) were obtained from the Lilly Library, papers of Hermann J. Muller at the University of Indiana as well from the files of multiple other members of the Panel. It therefore seems that Weaver received the assessments and that they were distributed to Panel members, as expressly recommended in the Weaver memo (Weaver 1956b). Thus, it is likely that all or most Genetics Panel members saw the three assessments that were excluded from the reporting in the *Science* publication. The question needs to be raised as to why didn't the *Science* article incorporate the independent findings from the other three Panel geneticists since they were professionally addressed and completed in a timely manner?

2. The estimates of the six Panel geneticists were summarized by James Crow in three letters [March 12, 1956 (Crow 1956b); March 29, 1956 (Crow 1956c); and May 21, 1956 (Crow 1956a)] to Weaver. The March 12, 1956 letter provided a graph showing the minimum, maximum and best estimates for total genetic damage expected from all descendants of the first generation of parents exposed to 10r, the cumulative damage through ten generations for a single generation exposed to 10r or the damage at the 10th generation from 10r per generation up to that time. In this letter, Crow stated that he made a decision to exclude the estimates of Drs. Wright and Demerec while the non-cited

report of Kaufmann was not mentioned. However, the estimates of Wright were included in the March 12, 1956 graph. More specifically, in the final paragraph of this letter, Crow writes that he did not provide **“Dr. Wright’s methods which are greatly different, but clearly given in his letter.”** In the next sentence he then writes that **“I haven’t included Dr. Demerec’s estimate on the graph for it, too, is based on quite different assumptions that lead to a greatly different value than the others obtained”**. The only specific comment that Crow offered for the Demerec estimation was that it **“was based on bacterial mutation rates”**. As for the Wright estimates, Crow stated that he **“counted only mutations causing conspicuous effects in postnatal life”**. However, the Wright document states that his analysis included the **“conspicuous detrimental effect on viability or fecundity”**. Thus, the effects estimated by the Wright analysis would not have been restricted to only those occurring in postnatal life, thereby contradicting the statement of Crow. Furthermore, the postnatal effects estimated by Wright were not restricted to an age of the offspring to display the harmful effect. However, Crow accepted the analysis of William Russell (1956) which restricted the age of expressing damage to only three weeks, a limitation that Russell noted. The actions of Crow to exclude the Wright and Demerec estimates are at variance with the instruction that he present **“all the damage estimates, compare them, and display assumptions, methods, input, and results in some sort of chart or graphic form.”**

3. In the March 29, 1956 letter to Weaver, (Crow 1956c), Crow stated: **“The limits presented on our estimates of genetic damage are so wide that the reader will, I believe, not have any confidence in them at all.”** He then makes the statement that **“I**

recommend one of two things: omit the estimates entirely, or b) give a single best estimate of the number of mutations, or a narrow range of estimate, based on direct extrapolations from mouse and Drosophila.” Crow then inexplicably states that **“We then state that these are based on mouse data and let the reader add his own uncertainty factor.”** Basing estimates on Drosophila and mice and then telling the reader that the estimates are based only on mice is deceptive. Asking the reader to construct their own uncertainty factor with what is highly censored data lacking upper and lower bounds is disingenuous. This recommendation followed from his earlier comment in the March 12, 1956 letter (Crow 1956b) that **“...the groups differ widely in their confidence in the best estimate, as indicated by their grossly discrepant minimum and maximum estimates.”** In the May 21, 1956 letter, he again provided mutational estimates of the six geneticists, including their best estimates and ranges (Crow 1956a). He stated in the letter that he did not support publishing the table. This issue was eventually considered for a vote by the entire committee and the table was not included, only the summary statement as given above.

4. Table 1 reveals that the best (i.e., most probable) estimates of affected children after 10 generations ranged from 2 to 10 million, a five-fold range. The range of uncertainty varied considerably amongst the geneticists. Beadle and Glass, for example, gave a 2,000 fold uncertainty range while Muller’s estimate was 10-fold. In the *Science* paper, it was stated that the uncertainty estimates were within 10-fold of the best estimates (i.e., 100-fold range). However, in the case of Beadle, the bounded values range from 20-fold below to 100-fold above his best estimate. For Glass, his range was from 40-fold below

to 50-fold above his best estimate. In the case of Muller, his bounded values were 4-fold below and 2.5-fold above his best estimate. Similarly large variation in uncertainty estimates was also presented by Crow in his March 12, 1956 (Crow 1956b) letter to Weaver for the damage expected in the first offspring generation. In fact, the estimated number of affected individuals ranged from 5000- 20,000,000, a 4000 fold value. It is not possible to discern how the minimum and maximum values were derived in some cases. In the case of Beadle, for example, the values are provided without any explanation. As for Russell the best estimate was derived but lower and upper bound estimates are not included. It is not known how Crow derived such values. The case of Crow's own estimate may also be instructive. He stated on March 29, 1956c (Crow 1956c) **“I shall use as a minimum estimate a direct extrapolation from Drosophila and as a maximum some calculation from the sex-ratio data in the Japanese cities. An estimate from mouse data turns out to be just about half way between these, so I shall use it as the most probable estimate.”** With such non-sequitur biological reasoning guiding the genetic risk estimates as well as with estimates lacking any documentation, it is not surprising that the committee members did not want to share their procedures with others. Furthermore, there was an absence of criticism of the bizarre approach offered by Crow as well as the lack of documentation for upper and lower bound estimates by Beadle, Russell and others despite the high priority placed on such values. Even Sturtevant, whose lower and upper bound estimates were exactly \pm 10-fold, stated on February 20, 1956 to Weaver (Sturtevant 1956), **“After going through these calculations I come out with a feeling that they are rather futile. At almost every step it has been necessary to make a guess, often with little to go on, and with**

no real basis for setting limits within which the true value probably lies”. This statement raises the question that Sturtevant may even have been open to the possibility that the true estimate was not even within his derived lower and upper bounds. The statement of Sturtevant contradicts the earlier statement in the *Science* article which claimed that **“Each thus said, in effect, I feel reasonably confident that the true value is greater than my minimum estimate and less than my maximum.”** In fact, the term guess was also used by other reporting geneticists. For example, Russell referred to his extrapolation process also as a “guess” (Russell 1956).

5. According to Neel the closeness of the best estimates of the six geneticists selected by Crow was due to the fact that they used essentially the same assumptions for gene number, mutation rates, and other parameters (Neel 1956c). Neel thought that their scientific “agreement” was illusionary since there was little independence of thought. In fact, once each of the six had to think far more independently on the problem as with the case of estimating upper and lower uncertainty bounds their “apparent” agreement strikingly disappeared, supporting the Neel perspective. However, knowing that he had been overruled on genetic risk estimations, Neel asked to be dissociated with any aspect of the report that provided quantitative estimates of genetic damage or a recommended permissible dose (Jolly 2003 - page 359).

6. The 100-fold range reported in the *Science* article to characterize the uncertainty about the best estimate strikingly conflicted with and misrepresented the range of uncertainty of this group of geneticists which had a mean value of 745 (uncertainty range 10-2000 – see

Table 1) for the 10 generation estimate (median 180) and a mean of 756 (uncertainty range 100-2857) for the first generation (median 312.5) (data from March 12, 1956 Crow Letter to Weaver) (Crow 1956b).

Draft BEAR I Genetics Panel Report of March 19, 1956

In this draft report (NAS 1956b), Weaver stated that the **“estimates have been independently furnished by seven of the geneticists of this Panel. Each estimated the total damage (that is to say, the number of genetic deaths or extinctions) which would occur among our 100 million children. Each of the seven geneticists stated his result in terms of a range of values, giving what he considered to be a reasonable lowest figure and also a reasonable highest figure. Thus there were in all seven low estimates and seven high estimates. The lowest of the low estimates was 5,000 damaged individuals among our direct children. The highest of the high estimates was 20 million. This extreme range simply reflects our lamentable lack of information on human radiation genetics.”** This range of high to low would indicate a 4000-fold range and probably reflected damage estimated in the first offspring generation. This draft differed from the May 21, 1956 table of Crow which showed a maximum 2,000 fold range, reflecting damage after 10 generations. There is also no explanation why there were seven geneticist estimates stated in this draft but only six were mentioned in the *Science* paper.

Chairman Weaver also raised the question that **“The public may well ask, why were there only seven such estimates when this Panel includes a larger number of geneticists? The answer is that many highly competent geneticists would not wish to undertake a calculation of this sort, either because they are not specifically experienced in the more**

mathematical aspects of genetics, or because they doubt that such a calculation is very useful at the present state of knowledge.” The response of Weaver failed to indicate that nine of the 12 geneticists provided estimates. He referred to seven Genetics Panel members as providing estimates. At some point one of the seven reports was reduced to six without explanation.

There were substantial differences as to what the trans-generational mutation risk might be, even amongst the six geneticist values that were used. The extent of the variation and the acknowledgment of the “futility” in making the given estimates undercut the confidence in the process and in its conclusions. Furthermore, this high degree of uncertainty would have been considerably greater if these geneticists were not constrained by Weaver (in his memo directions to the geneticists) to employ a linearity dose response assumption. The variability characterization estimates in the *Science* article provide a distinctly different impression than the actual estimates of the six geneticists, reflecting data censoring, and leading to false conclusions concerning the degree of uncertainty among the participating geneticists and the extent to which the experts agreed/disagreed amongst themselves on this matter.

On June 5, 1956 Chairman Weaver (1956c) wrote to the Genetics Panel informing them of the results of the voting on an unresolved aspect of the final report. One of the key questions was whether to include the table of Crow which showed the variation in genetic damage estimates between and within subjects. This decision was accomplished by a vote of 15 members of the Panel. The results were 7 against including the table, 6 for publishing it, with two designated as “indifferent”. Thus, the recommendation of Crow prevailed and the table was not included. Based on incomplete obtained correspondence, it is known that Crow, Muller, Glass and Sonneborn voted against publishing the table.

The Missing Expert Geneticist Estimates

1) What information did the three missing geneticist estimates provide?

Kaufmann: His report was six pages, providing a detailed mathematical derivation of mutation frequency based on human mutation rates, providing all assumptions used in his calculations. Three pages were text, two pages were tables and one page contained two summary figures. He provided genetic damage estimates over 10 generations and expressed the number of affected children per 200 million. Adjusting for 100 million, he reported 195,000 affected children in generation #1. In his February 27, 1956 letter (Kaufmann 1956) to Weaver, Kaufmann stated that his **“calculations show that under the defined conditions the visible genetic damage resulting from chronic exposure to 10 r per generation is small in comparison with that of spontaneous origin.”**

Kaufman noted that his estimates, based on research of the geneticist Herman Moser, were quite complicated and he was not able to provide upper and lower confidence intervals. Note that Crow (1956b) included estimates of “Moser” which were subsequently dropped. These appear to be the estimates of Kaufmann.

Wright: His report provided a nine page detailed assessment and mathematical derivation of genetic damage over 10 generations (Wright 1956). He also provided two estimates of damage based on assumptions related to the proportion of dominant mutations. The estimates provided in the report range from 34,000 to 67,000 (i.e. ~50,000 average) children affected in generation #1 based on the assumptions used. Upper and lower bound estimates were provided.

Demerec: He provided a detailed, single spaced, three page assessment using *E. coli* as his model. He presented evidence for spontaneous mutation rates for 26 genes and X-ray induced mutation rates for the same genes. He also indicates that similar data existed for multiple chemical mutagens (Demerec 1956). The information that Demerec had developed on the *E. coli* model far exceeded that of any of the other models presented including *Drosophila*, mice and humans. His risk estimates were based on 160,000,000 as was originally proposed by Weaver. He estimated that there would be 14,200 affected children in generation #1 due to spontaneous mutation; in contrast, 8,320 additional children were estimated to be affected by the 10 r exposure to their parents. This value was adjusted to 5,200 for a 100 million population. Demerec stated that since he did not include “lethals” his estimate would need to be adjusted to some degree upward. He also stated that **“I do not wish to venture into speculations about the genetic damage that would be sustained by subsequent generations, for such speculations could only be based on assumptions not supported by experimental evidence.”**

- 2) The three best estimates were 195,000 (Kaufman), 50,000 (Wright), and 5,200 (Demerec) (83,000 mean) for the first generation. On balance these are lower (~70%) than for the other five geneticists (275,000 mean) (note that Beadle did not provide an estimate for generation #1). If all geneticist estimates for generation #1 were included, the generation #1 mean estimates would range from a low of ~ 5000 to a high of ~350,000, a ~70 fold difference, a value reflecting considerable variation amongst the geneticists.

Discussion

The present assessment indicates that the US NAS BEAR I Committee Genetics Panel *Science* paper included a series of significant misrepresentations relating to the central charge of the Panel, that is, predicting the public health risks of ionizing radiation from all sources, such as medical, fall-out and other means of exposure. The Panel knowingly reported that only six of the 12 geneticists provided estimates, when nine did. While reasons for this omission of data are speculative, two (i.e. Demerec and Wright) of the three excluded views offered notably lower estimates of risk than the six geneticists presented in the paper. If all geneticists' estimates were presented, it would have markedly altered the range of the best estimates; the range of the ~2-fold mean difference would increase to ~70-fold for generation #1 damage. If the experts were shown not to be in agreement, it would affect the credibility of their report and most likely weaken support for its policy recommendations.

The decision by Crow to exclude the estimates of Demerec and Wright overstepped his authority as given by Chairman Weaver. There was no documentation to support these actions. There is no indication of why the estimates of Kaufmann were omitted. Despite such actions of Crow to exclude these three, there is no evidence that such actions were disputed. This suggests that the affected members and the entire Panel accepted this decision. If this is the case, then it is even more problematic as the incorrect statement that only six geneticists provided estimates rather than the nine was known and agreed to by all. Whether this was due to internal pressure to conform to the goal of displaying highly consistent estimates is not known. Yet, the efforts of the three excluded geneticists were substantial, with no evidence that they withdrew their estimates.

The Panel also stated that the uncertainty range (i.e., upper to lower bound) was about 100-fold for the six geneticists used while the table of Crow had Beadle and Glass with ranges of

2,000 and a mean of 745, and uncertainty values that were even higher after the first generation. This misrepresentation, though different from omitted data, is consistent with the intention of creating the appearance of greater agreement amongst the geneticists than actually existed. Furthermore, the *Science* article failed to provide the reasons why three individuals (i.e., Little, Neel, and Sonneborn) did not provide estimates. Given the critical significance for Panel agreement with respect to the acceptance of policy recommendations, it was necessary to present the views of the entire group. In fact, the views of Sonneborn and Neel were that such estimates could not be reliably done, with Neel being particularly strident on this issue as quoted above. It is not clear what the opinions of Little were. Thus, if the broad spectrum of views and risk estimates had been summarized in the *Science* article, it would have undermined the conclusions and policy recommendations of the Panel even further. On this later matter, it is worthy of note that in his May 21, 1956 letter to the Panel (Crow 1956a), Crow states that **"Once again, I urge that we not include the table at all..."** He then displayed a table of the values of the six geneticists and stated that **"I include these values for Committee members' inspection because I believe the only thing worse than publishing the estimates at all is to publish the wrong values"**. This recommendation was adopted and the table showing the extremely wide differences between the experts was dropped and was replaced with the above noted misrepresentation.

While the estimates of the panel members were clearly efforts in risk assessment speculation, they contributed to the appearance of confident conclusions throughout the *Science* article. In retrospect it appears that the exclusion of the Demerec and Wright estimates occurred because the derived values were "different", that is, far lower than the other six. This was assumed to be due to the use of other methods, models or approaches. However, the charge of

the Panel was to present the range of independent estimates and to describe them along with their assumptions. If complementary methods and models could be shown to have considerable agreement, it would bring added confidence to the recommendations of the Panel as noted above by Sonneborn. However, if the expert estimates were widely or wildly divergent, then it was feared that it would undermine acceptance and use of their report. In many ways, the future course of action would be affected by the degree of concordance of their estimates. When the estimates were found to be extremely variable, Crow and the other members of the Panel refused to show this variability/uncertainty and in fact acted to exclude low estimates and mischaracterize the estimates retained.

When placed in perspective what does this story reveal? The geneticists of the Panel firmly believed their genetics mutation-credo of nearly always harmful, cumulative, irreversible, and linear at low dose. However, when challenged by Weaver to translate this credo into independent estimates of societal harm, using their own research methods and experience, the results were unanticipated. That is, the estimates revealed much quantitative variability and uncertainty between and within the experts. So great was the range of estimates that actions were taken to prevent this from being exposed, deliberately creating the false impression of agreement that was not warranted, as seen in the omission and mischaracterization of estimates. Comparing the message of the *Science* paper with the internal correspondence of the Panel members, reveals a striking dichotomy and dishonesty of the Panel. The falsification of the research record provided the vehicle for the acceptance of the Panel's goal, that is, the use of the LNT for risk assessment and ultimately its acceptance worldwide by regulatory and public health agencies and within the legal system. In a highly ironic twist to the present story, it should be noted that the US National Academy of Sciences building in Washington, D.C. has a statute of Albert Einstein,

which is accompanied by a series of his famous quotes. One of these captures of the essence of the presence paper and the intolerable actions of the Genetics Panel. “The right to search for truth implies also a duty; one must not conceal any part of what one has recognized to be true.” In a retrospective statement some 40 years later concerning the actions of the BEAR I Genetics Panel, Crow (1995) confessed that he and especially Muller exaggerated the dangers of low level radiation and should accept significant blame for an irrational emphasis on such matters by the public and regulatory agencies.

Acknowledgement

Research activities in the area of dose response have been funded by the United States Air Force and ExxonMobil Foundation over a number of years. However, such funding support has not been used for the present manuscript. Author declares no conflict of interest.

Table 1. Trans-generational estimations of the NAS BEAR I Genetics Panel – Subcommittee- The values represent "the total genetic damage expected among all descendants (through 10 generations) of a single (i.e., first) generation exposure to 10r". Source: Crow, 1956b - March 12, 1956 of Crow to Weaver; Letter of Crow provided the four columns to the right.

Min to Max Range (i.e., uncertainty range)	Minimum	Most Probable	Maximum	Author
2,000-fold	100,000	2,000,000	200,000,000	Beadle
2,000-fold	100,000 ^a	4,000,000	200,000,000	Glass
260-fold	250,000	5,000,000	72,000,000	Crow
100-fold	600,000	6,000,000	60,000,000	Sturtevant
100-fold	700,000	7,000,000	70,000,000	Russell
10-fold	2,500,000	10,000,000	25,000,000	Muller

^a On May 21, 1956 Glass wrote to Weaver indicating that his minimum estimate was in error and should be 200,000 rather than 100,000. He stated that the reason for the error was that he based his estimate on 1 r rather than 10 r in his calculations. He stated that his most probable and maximum values were correct. Coincidentally, Crow (1956d) wrote to Muller on that same day (May 21, 1956) with copies to Weaver and Glass indicating that Glass's minimum estimate needed to be adjusted to a 10 r exposure (rather than 1 r), which when normalized to a population of 10^8 , his value was 100,000, the same as Glass originally reported. No changes were needed for his most probable and maximum values. Thus, the memo of Crow did not agree with the recommended change in damage estimate of Glass. This technical point was not resolved since the Panel decided not to provide the Table in their final report/*Science* paper based on a vote by the Panel on May 29, 1956. Note that Glass received the copy of Crow's letter as it was in his files. Glass's May 21, 1956 (Glass 1956) letter explanation seems unlikely as his original genetic damage estimates included values for both 1 and 10 r, with the expected 10-fold damage difference.

References

Anonymous (1956) Genetic effects of atomic radiation. Summary Report of the Committee on Biological Effects of Atomic Radiation by the National Academy of Sciences, BEAR I Genetics Panel (W. Weaver, Chair). *Science* 123:1157-1164 (1956). [Erratum, *Science* 124:170].

Calabrese EJ (2009) The road to linearity: Why linearity at low doses became the basis for carcinogen risk assessment. *Arch Toxicol* 83:203-225.

Calabrese EJ (2011a) Toxicology rewrites its history and rethinks its future: Giving equal focus to both harmful and beneficial effects. *Environ Toxicol Chem* 30:2658-2673.

Calabrese EJ (2011b) Key studies used to support cancer risk assessment questioned. *Environ Mol Mut* 52:595-606.

Calabrese EJ (2013) How the US National Academy of Sciences misled the world community on cancer risk assessment: New findings challenge historical foundations of the linear dose response. *Arch Toxicol* 87:2063-2081.

Crow JF (1956a) Crow, Letter to Weaver, Lilly Library, (Muller mss), Manuscript Department, Indianan University, Bloomington IN, USA. May 21.

Crow JF (1956b) Letter to Weaver, Lilly Library, Manuscript Department, Indiana University, Bloomington IN, USA. March 12.

Crow JF (1956c) Letter to Weaver, Lilly Library (Sonneborn mss), Manuscript Department, Indiana University, Bloomington IN, USA. March 29.

Crow JF (1956d) Letter to Muller, American Philosophical Society (APS), Philadelphia PA, USA. May 21.

Crow JF (1995) Quarreling geneticists and a diplomat. *Genetics* 140:421-426.

Demerec M (1956) Letter to Weaver, Lilly Library, (Muller mss), Manuscript Department, Indiana University, Bloomington IN, USA. February 14 with February 11 document.

Glass HB (1956) Letter to Weaver, American Philosophical Society (APS), Philadelphia PA, USA. May 21.

Jolly JC (2003) Thresholds of uncertainty: Radiation and responsibility in the fallout Controversy, Ph.D. thesis. Oregon State University, USA.

Kaufmann BP (1956) Letter to Weaver, Lilly Library (Muller mss), Manuscript Department, Indiana University, Bloomington IN, USA. February 27.

National Academy of Sciences (NAS) (1956a) BEAR I Committee Transcript Proceedings Conference on Genetics, February 5-6, 1956, Chicago IL, USA.

National Academy of Sciences (NAS) (1956b) Unpublished Draft Report of the Genetics Panel of the National Academy of Sciences, Study of the Biological Effects of Atomic Radiation (BEAR I), American Philosophical Society (APS), Philadelphia PA, USA. March 19.

National Academy of Science (NAS)/National Research Council (NRC) (1956) The Biological Effects of Atomic Radiation. A report to the public. Washington DC, USA.

Neel JV (1956a) Letter to Chairman Weaver. American Philosophical Society (APS), Philadelphia PA, USA. University of Wisconsin. April 6, (Jolly, 2003-page 398).

Neel JV (1956b) Letter to Weaver. American Philosophical Society (APS), Philadelphia PA, SUA. University of Wisconsin. March 8, (Jolly, 2003-page 359).

Neel JV (1956c) Letter to Weaver, American Philosophical Society (APS), Philadelphia PA, USA. March 16 (Jolly, 2003-page 159).

Russell WL (1956) Report to Weaver, Lilly Library. Estimate of expressed damage in the offspring of individual exposed to 10r. Lilly Library, (Muller mss) Manuscript Department, Indiana University, Bloomington IN, USA. February 24.

Sturtevant A (1956) Letter to Weaver, American Philosophical Society (APS), Philadelphia PA, USA. February 20.

Weaver W (1956a) Memo to Genetics Panel, brief report, Lilly Library (Muller mss), Manuscript Department, Indiana University, Bloomington IN, USA. March 2.

Weaver W (1956b) Memo to Members of the Genetics Panel, Lilly Library, Manuscript Department, Indiana University, Bloomington IN, USA. February 8.

Weaver W (1956c) Memo to Genetics Committee, Lilly Library, Manuscript Department, Indiana University, Bloomington IN, USA. June 5.

Whittemore GF (1986) The National Committee on Radiation Protection, 1928-1960: From Professional Guidelines to Government Regulation. Ph.D. thesis, Department of the History of Science. Harvard University, Cambridge MA, USA.

Wright S (1956) Letter to Weaver, Lilly Library (Muller mss), Manuscript Department, Indiana University, Bloomington IN, USA. February 22.