INTERVIEW: DR. EDWARD CALABRESE

# How a 'Big Lie' Launched The LNT Myth and The Great Fear of Radiation

*Dr. Edward Calabrese is Professor in the Environmental Health Sciences Division at the University of Massachusetts at Amherst. As a toxicology specialist, he has written scores of articles about the nonlinearity of dose-response, including the benefits of low-dose radiation (called hormesis). He is founder and chairman of the advisory committee of BELLE, the Biological Effects of Low Level Exposure, a group founded in 1990, which includes scientists from several disciplines and aims to encourage assessment of the biological effects of low-level exposures to chemical agents and radioactivity.*

Laurence Hecht

*Dr. Calabrese recently made the startling discovery that the linear no-threshold or LNT hypothesis, which governs radiation and chemical protection policy today, was founded on a deliberate lie to further a political agenda. According to LNT, there is no safe dose of radiation; the known deleterious effects of very high dose levels, under LNT, can be extrapolated linearly down to a zero dose.*

*As Dr. Calabrese elaborates in the interview, the contrary evidence was deliberately suppressed by Nobel Laureate Herman Muller, who won the 1946 Nobel Prize in medicine for his discovery that X-rays induce genetic mutations. Muller stated flatly in his Nobel speech that there was "no escape from the conclusion that there is no threshold," although he knew at the time that there was reliable contrary evidence.*

*Society is still paying for this "big lie" in billions of dollars spent to meet unnecessarily strict regulations, in generations of people taught to be irrationally scared of any radiation, and in millions of lives lost as the cost of not going nuclear.*

*Dr. Calabrese was interviewed on Sept. 26, 2011 by Managing Editor Marjorie Mazel Hecht.*

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*Hermann Muller (1890-1967) receiving his Nobel Prize from the King of Sweden in 1946, for his discovery that "mutations can be induced by X-rays." In his Dec. 12, 1946 Nobel speech, Muller stated that there is "no escape from the conclusion that there is no threshold" for radiation effects, although he knew this to be untrue, based on the research results of a respected colleague.*

**21st Century: You have long argued that the science does not support the establishment dogma of LNT, the linear no-threshold view of radiation, which proclaims any radiation dose down to zero to be bad. How did you come across the duplicity of Nobel Laureate Hermann Muller, who lied about his results to justify the LNT theory?**

**Calabrese:** It happened somewhat unexpectedly. I was preparing a manuscript on the history of the dose-response relationship, and I had reached what I felt was a final stage where I could show it to someone. I sent it to about 12 people whom I felt could be somewhat friendly, but critical, reviewers, before I would send the manuscript out for publication consideration. I received various comments; one of these reviewers indicated that I needed to do a better job on evaluating Hermann Muller from the time of his Nobel Prize in 1946 through probably the next 10 to 15 years and his impact on the acceptance of the linear dose response.

Agreeing with that criticism, I spent several months following up on this suggestion. During this process, I developed several new insights and that's what actually brought me to this point.

What I learned was that one of the critical studies that the low dose linearity radiation work was based on was a 1948 publication from the University of Rochester, by the eminent geneticist Dr. Curt Stern, and his co-researcher Dr. Warren Spencer. During that same year, there was another publication by Stern and Dr. Ernst Caspari. The data of these papers were collected during the 1943/1944 and 1945/1946 time periods, respectively. Hermann Muller, then a professor at Amherst College, was a paid consultant on these projects. The manuscripts could not be submitted for publication until they were given a U.S. government clearance, sometime in 1947, after the end of World War II.

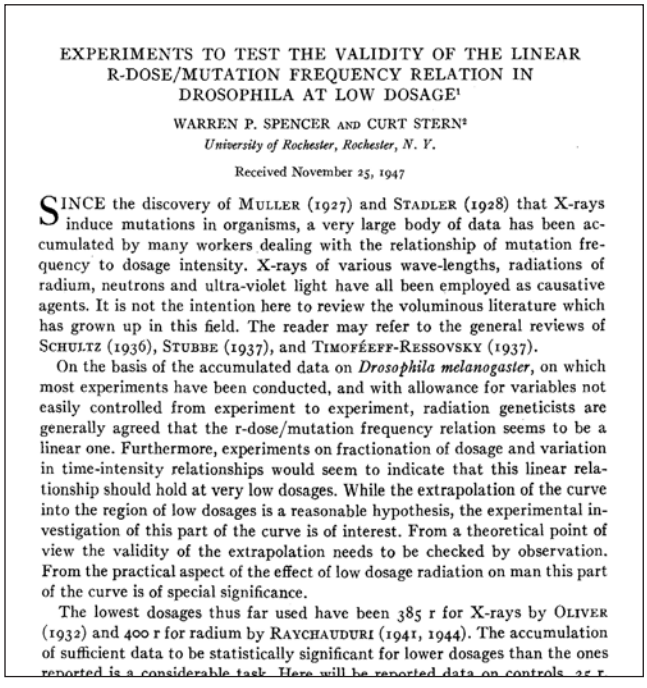
The earlier research of Spencer and Stern, a study of an acute exposure to ionizing radiation, supported the linear dose response, whereas the Caspari and Stern research, which involved chronic exposures, showed no support for the linear model; it supported a threshold interpretation.

This finding of Caspari was unexpected and created a problem for Stern, who was hoping to support a linear perspective. The Caspari findings were of considerable importance since it was the strongest study that had been done on low-dose ionizing radiation and mutation in *Drosophila*. The dose rate employed was far lower than any previous study of ionizing radiation.

The study also included key improvements in various experimental methods, execution, and data analysis over the Spencer and Stern study. Thus, in a number of important ways, the findings were more reliable than the Spencer and Stern paper and more relevant to public health concerns, as it was dealing with exposures in a low-dose zone. In fact, the dose rate of the Caspari study was only about 1/15,000 of the Spencer acute study.

The research of Caspari was concluded in August 1946. One month later Muller was notified that he was going to receive the Nobel Prize in Biology and Medicine. I was aware of the fact that in his Nobel Prize Lecture on December 12, 1946, Muller strongly rejected even the possibility of the threshold dose response model for radiation, passionately arguing for the adoption of the linear at low dose model. So the following question arose in my mind: Did Muller actually know of this major finding by Caspari prior to his Nobel Prize Lecture?

If he did, then why would he have made the statement that the one could no longer even consider it as a possibility? So I tried to track down an answer to this question. I had read a couple of Ph.D. dissertations about Muller from this era before, so I re-



EXPERIMENTS TO TEST THE VALIDITY OF THE LINEAR R-DOSE/MUTATION FREQUENCY RELATION IN DROSOPHILA AT LOW DOSAGE[1]

WARREN P. SPENCER AND CURT STERN[2]

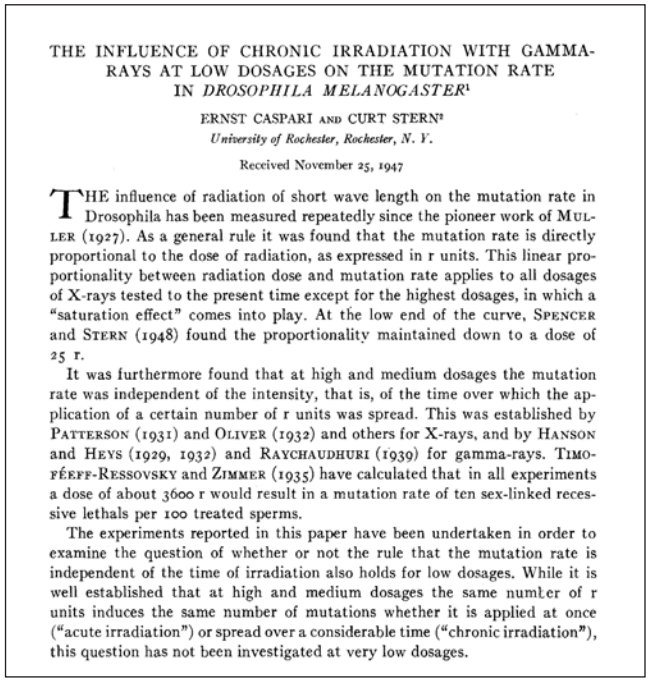
*University of Rochester, Rochester, N. Y.*

Received November 25, 1947

SINCE the discovery of MULLER (1927) and STADLER (1928) that X-rays induce mutations in organisms, a very large body of data has been accumulated by many workers dealing with the relationship of mutation frequency to dosage intensity. X-rays of various wave-lengths, radiations of radium, neutrons and ultra-violet light have all been employed as causative agents. It is not the intention here to review the voluminous literature which has grown up in this field. The reader may refer to the general reviews of SCHULTZ (1936), STUBBE (1937), and TIMOFÉEFF-RESSOVSKY (1937).

On the basis of the accumulated data on *Drosophila melanogaster*, on which most experiments have been conducted, and with allowance for variables not easily controlled from experiment to experiment, radiation geneticists are generally agreed that the r-dose/mutation frequency relation seems to be a linear one. Furthermore, experiments on fractionation of dosage and variation in time-intensity relationships would seem to indicate that this linear relationship should hold at very low dosages. While the extrapolation of the curve into the region of low dosages is a reasonable hypothesis, the experimental investigation of this part of the curve is of interest. From a theoretical point of view the validity of the extrapolation needs to be checked by observation. From the practical aspect of the effect of low dosage radiation on man this part of the curve is of special significance.

The lowest dosages thus far used have been 385 r for X-rays by OLIVER (1932) and 400 r for radium by RAYCHAUDURI (1941, 1944). The accumulation of sufficient data to be statistically significant for lower dosages than the ones reported is a considerable task. Here will be reported data on controls 25 r.



THE INFLUENCE OF CHRONIC IRRADIATION WITH GAMMA-RAYS AT LOW DOSAGES ON THE MUTATION RATE IN *DROSOPHILA MELANOGASTER*[1]

ERNST CASPARI AND CURT STERN[2]

*University of Rochester, Rochester, N. Y.*

Received November 25, 1947

THE influence of radiation of short wave length on the mutation rate in Drosophila has been measured repeatedly since the pioneer work of MULLER (1927). As a general rule it was found that the mutation rate is directly proportional to the dose of radiation, as expressed in r units. This linear proportionality between radiation dose and mutation rate applies to all dosages of X-rays tested to the present time except for the highest dosages, in which a "saturation effect" comes into play. At the low end of the curve, SPENCER and STERN (1948) found the proportionality maintained down to a dose of 25 r.

It was furthermore found that at high and medium dosages the mutation rate was independent of the intensity, that is, of the time over which the application of a certain number of r units was spread. This was established by PATTERSON (1931) and OLIVER (1932) and others for X-rays, and by HANSON and HEYS (1929, 1932) and RAYCHAUDHURI (1939) for gamma-rays. TIMOFÉEFF-RESSOVSKY and ZIMMER (1935) have calculated that in all experiments a dose of about 3600 r would result in a mutation rate of ten sex-linked recessive lethals per 100 treated sperms.

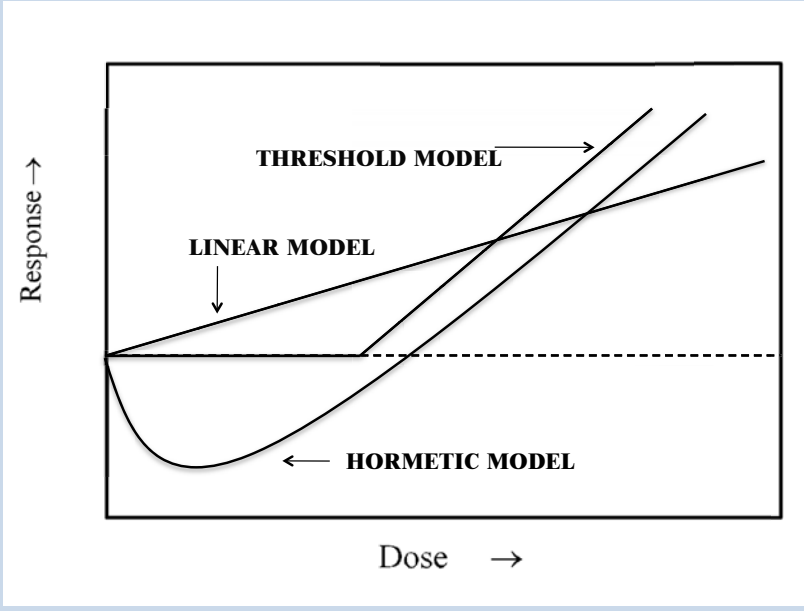
The experiments reported in this paper have been undertaken in order to examine the question of whether or not the rule that the mutation rate is independent of the time of irradiation also holds for low dosages. While it is well established that at high and medium dosages the same number of r units induces the same number of mutations whether it is applied at once ("acute irradiation") or spread over a considerable time ("chronic irradiation"), this question has not been investigated at very low dosages.

*The Spencer and Stern article and the Caspari and Stern articles, which both appeared in the journal* Genetics. *Calabrese documents from Muller's correspondence that Muller knew of Caspari's dose-response results and their significance before his Nobel speech.*

**THE HORMESIS 'J' CURVE**

Both radiation and chemicals demonstrate a threshold dose response, the 'J' curve shown here, where the effects are beneficial (called hormesis) up to a threshold, and high doses are harmful. The response curve is the same for radiation and other chemical and biological agents. However, against the empirical evidence, the threshold dose response model was replaced by the linear no-threshold model, which extrapolates linearly the harmful effects from the known damage of high doses all the way down to zero.

The shift from a threshold to the dominant linear model resulted from a campaign initiated by geneticist Hermann Muller, who, in his 1946 Nobel Prize speech stated flatly that there was no evidence for a threshold effect, although he knew this to be untrue.




Source: Dr. Edward Calabrese

read key portions but could not find an answer to my question.

So I tracked down the researcher who was the most relevant; he went through his files and could not find an answer. So this forced me to obtain the correspondence, the unpublished communication between Curt Stern and Muller and between Caspari and Stern and Caspari and anyone else who was connected to them. I tried to obtain any conceivably relevant written communication. In the case of Muller, I made sure that I obtained his communications with Stern and Stern's with him from different sources.

Then one day when I was returning from one part of campus to my office around 6 o'clock, I found this big stash of letters and other communications sent by the American Philosophical Association. Too excited to eat, I read through hundreds of pages of material. At some point, I came across a series of letters in the key 1946 time period. In going through those, I found that there was a letter from Stern to Muller which said that they had finished the Caspari study, asking Muller if he would be willing to review the manuscript.

During the research, Muller had made a fair number of trips from Amherst to Rochester to meet with Stern; in fact, Muller even provided the strain of flies that Spencer and Caspari used in their experiments. So he had a reasonably close relationship with Stern and the group. He knew everybody and how things worked.

Upon the receipt of Stern's letter, Muller wrote back indicating that he would critique the findings. The manuscript was finally sent to Muller on November 6, 1946. For me the smoking gun occurred in a Nov. 12, 1946 letter from Muller back to Stern. In this letter he indicates that he received the manuscript, scanned through the entire document, saw its significance, knew that the findings were refuting the low-dose linearity concept, that the study was done by Caspari, whom he viewed as a very competent person, so he couldn't challenge the findings.

Muller indicated that the study needed to be replicated, because the findings were so diametrically opposed to their linearity perspective. He concluded that he would get his de-

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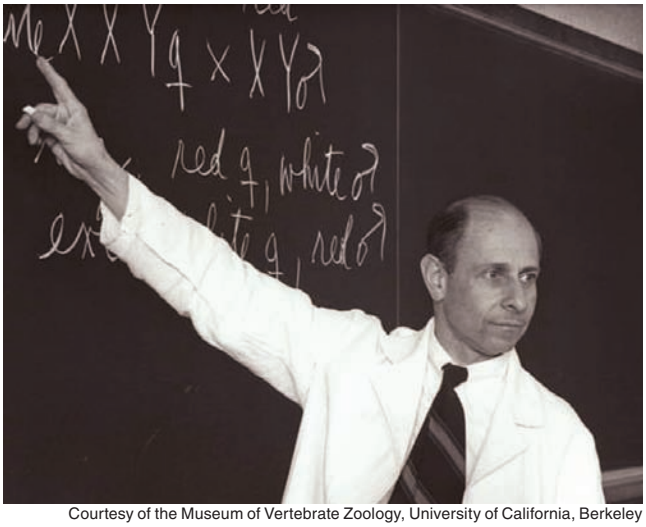
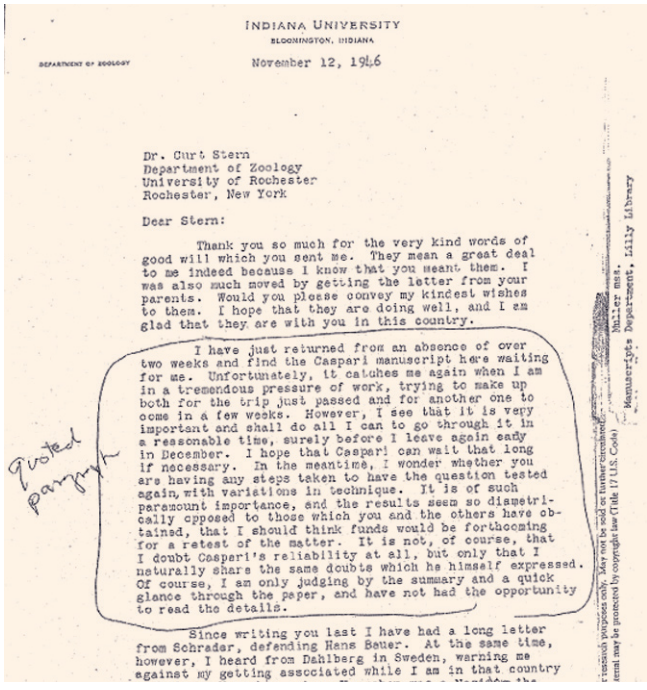
*Hermann Muller and two staff members in the "fly room" at Indiana University. Muller began teaching at Indiana University in 1945.*



Courtesy of the Museum of Vertebrate Zoology, University of California, Berkeley

*Curt Stern in March 1951 in a photo by Oliver P. Pearson. As the editor-in-chief of the journal* Genetics*, Stern marginalized the significance of the Caspari results when they were published, thus saving Muller's reputation.*



INDIANA UNIVERSITY
BLOOMINGTON, INDIANA
DEPARTMENT OF ZOOLOGY

November 12, 1946

Dr. Curt Stern
Department of Zoology
University of Rochester
Rochester, New York

Dear Stern:

Thank you so much for the very kind words of good will which you sent me. They mean a great deal to me indeed because I know that you meant them. I was also much moved by getting the letter from your parents. Would you please convey my kindest wishes to them. I hope that they are doing well, and I am glad that they are with you in this country.

I have just returned from an absence of over two weeks and find the Caspari manuscript here waiting for me. Unfortunately, it catches me again when I am in a tremendous pressure of work, trying to make up both for the trip just passed and for another one to come in a few weeks. However, I see that it is very important and shall do all I can to go through it in a reasonable time, surely before I leave again early in December. I hope that Caspari can wait that long if necessary. In the meantime, I wonder whether you are having any steps taken to have the question tested again, with variations in technique. It is of such paramount importance, and the results seem so diametrically opposed to those which you and the others have obtained, that I should think funds would be forthcoming for a retest of the matter. It is not, of course, that I doubt Caspari's reliability at all, but only that I naturally share the same doubts which he himself expressed. Of course, I am only judging by the summary and a quick glance through the paper, and have not had the opportunity to read the details.

Since writing you last I have had a long letter from Schrader, defending Hans Bauer. At the same time, however, I heard from Dahlberg in Sweden, warning me against my getting associated while I am in that country

Quoted paragraph

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tailed comments back before he took off on his next trip, which was his boat trip from New York to Stockholm.

Muller didn't send the detailed review in until January, after he came back. But in that November 12 letter, all the essential points were established: that he knew it was a competent study, detailed, significant, that it challenged his basic theme substantially, and he knew it. He also knew that he could not dismiss it. It would have to be scientifically confronted.

So now that I knew that Muller knew of the Caspari study prior to his Nobel Prize Lecture, I next wondered how he could have given this most significant of lectures—one truly on the world's stage—in Stockholm, and actually said that there is no longer any possibility to adhere to a threshold model. He had seen the data, he knew the investigators, he was their paid consultant. He could have—and should have said—as I indicated in my article, "I think that this is an area where more research needs to be done," but he had an agenda that wasn't scientific.

The strangest thing to me is that he knew this study was going to be published. Surely he knew the other shoe was going to drop—so to speak? At some point in the not-so-distant future, he would have to confront the fact that he knew there was this other study, that it was relevant, and that it challenged and actually rebutted what he said in his Nobel Prize Lecture.

If this study ever made the light of day, then it would profoundly affect his credibility. So the question is, how would Muller, and perhaps Stern, deal with this? That became even more intriguing to me. I needed to try to figure this one out as well. How would he get out of this potentially profoundly damaging situation? He knows that ultimately the study would be published.

And that leads to the next crazy and unpredictable course of action. When Caspari and Stern ultimately publish their work, they devoted the entire discussion to arguing that their data should not be accepted until it can be learned why their data differed from that found in the Spencer and Stern paper.

*Photos courtesy of Hermann Hartwig, in "Seventy-Five Years of Developmental Genetics: Ernst Caspari's Early Experiments on Insect Eye Pigmentation, Performed in an Academic Environment of Political Suppression," by Ulrich Grossbach,* Genetics*, April 2009*

*Geneticist Ernst Caspari (1909-1984) is second from left in this 1934 photo of the Alfred Kühn laboratory staff at Göttingen University, where he began his career. Inset is Caspari around 1933. Although a Protestant, Caspari's family heritage was Jewish, and he, along with dozens of other Jews, was dismissed by the Nazis. Caspari fled to Istanbul to continue his work, and in 1938, he came to the United States. The Göttingen Center for Molecular Biosciences has a building named after Caspari.*

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*Muller with a fruit fly model, teaching a class at Indiana University.*

Now the Spencer and Stern paper had at least two dozen major differences with the Caspari and Stern paper, as my article reports. One used male flies, and the other used females; they used fundamentally different diets; one administered X-rays, and the other gamma rays; different rearing temperatures were used—there were many other differences. And now, 60 years later, no one has ever attempted to explain these differences. Today, you couldn't get away with comparing the two experiments, because there are too many differences between them.

But Stern and Caspari set up a straw man, a foolish premise.

**21st Century: Did they raise the straw man because Muller intervened, to make sure that they dampened any enthusiasm about their actual findings?**

**Calabrese:** What Muller actually said was: I can support the publication of this because there are so many caveats in the discussion, that essentially nobody can use the data anyway.

And, to top it all off: You would think that writing a paper in this way, that you could never actually get it through peer-review. How could you submit a paper, with your data, and then disavow the use of your data—unless you were submitting it to the journal for which you were the editor-in-chief?

**21st Century: Which they did.**

**Calabrese:** Yes, they submitted it to Curt Stern's journal, *Genetics* where he was the editor-in-chief. And they submitted the paper on Nov. 25, 1947, and it's a very long paper, as is the Spencer and Stern paper. And they submitted them both on the same day. Both papers were published essentially about one month later, in January 1948, which meant to me that they actually were not sent out for peer-review; they weren't corrected or changed—nothing, given snail mail, given everything. I've seen the papers that were submitted, and I've seen the papers that were published, and there really isn't any difference between them.

So, I'm 99 percent sure that the papers weren't submitted for peer-review. Basically, Curt Stern controlled the reality of these papers. He published them the way he wanted to, and had all the caveats that he and Muller desired. And so that achieved a couple of key goals for Muller and Stern. It allowed Caspari to get the publication of all the work that they did, which they owed to the government that was paying for the research.

But even more important to them, they marginalized the Caspari findings that supported the threshold and basically gave Muller protection, by concluding that you couldn't even use/accept the Caspari work. Thus, Muller's Nobel Prize Lecture assertion—that you could no longer accept the threshold model—could not be effectively challenged. Stern was saving Muller's reputation, all for a common ideological agenda centered on the dose response.

Stern did try to follow up on the Muller suggestion, which was to try to replicate the work of Caspari. However, at that point Caspari and Spencer were leaving Rochester; Spencer returned to his faculty position at the College of Wooster in Ohio, and Caspari to a faculty position at Wesleyan University in Connecticut. So Curt Stern turned to a new graduate student, Delta Uphoff, who took over the role of trying to replicate the Caspari study.

Stern gave her three major experiments . . . but each ended in confusion. In reality, she was new to the research game, just coming from an undergraduate situation. In the first attempt to replicate at least part of Caspari's findings, Uphoff reported control group mutation rates that were aberrantly low, being about 40 percent lower than expected from the literature and their group's experience.

Initially, Stern tried to use Uphoff's findings to discredit the work of Caspari, by saying that his control group was too high, by chance or whatever reason, and that was the reason that Caspari did not see any treatment-related effects.

Caspari, however, fought back. He went into the literature in great depth, contacted Muller, got a lot of unpublished findings from Muller, and ultimately assembled a very large amount of data that demonstrated that his control group values were consistent with the vast body of published and unpublished literature on that model and control group responses.

So Stern had to back down. Stern then made Uphoff the "fall guy," blaming the low control values on her possible bias . . . a comment that was actually included in the manuscript submitted to the Atomic Energy Commission. In their own language the aberrantly low control values made this experiment "uninterpretable." The second experiment fared no better, as Uphoff's data again displayed an aberrantly low control group value. With two key experiments unusable, things were not looking too good.

**21st Century: How would her bias make the control group have such a low response?**

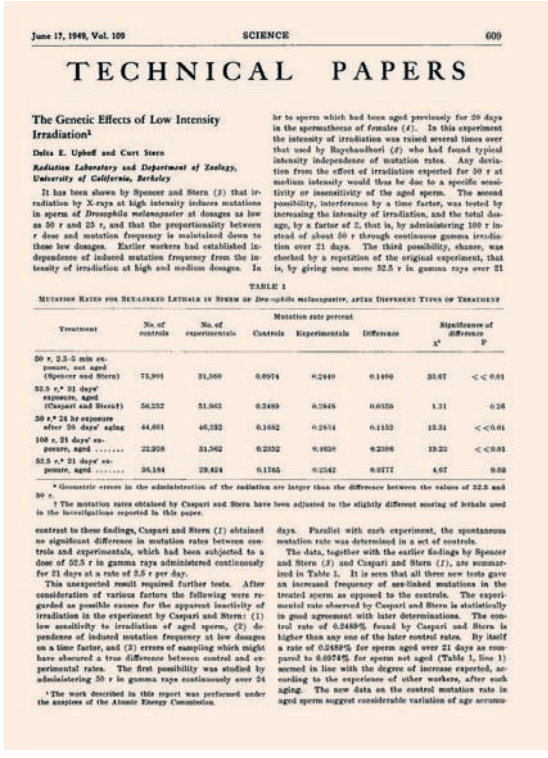
**Calabrese:** As you count the recessive mutations shown under a binocular microscope, there can be a certain amount of uncertainty at times, in terms of whether something would be considered a mutant form or not. As it turns out, there was also a potential for bias. They also didn't have double-blind read-

ings, so they knew what the control group was, so there's a potential for bias there.

**21st Century: So it's human decision about whether it's one thing or another.**

**Calabrese:** It could also have been inexperience—it's her first research experience. I went back and found a paper in 1928 or '29 or so, by Muller, who was attempting to get information on background mutation rates in *Drosophila*, and he was working with somewhat inexperienced people in the lab in Texas; he became frustrated and quit the experiment because they were having such a difficult time properly doing this. It takes an awful lot of effort to do it. He attributed it to inexperience, and I was able to cite that in my more detailed paper.

Mostly it's probably inexperience. There may be some bias, but nobody really knows. Whatever it was, in the write-up that Stern sent to the Atomic Energy Commission in 1947, they had all the data, and they also had the disavowing of their results, saying that their results were unreliable, and uninterpretable.



June 17, 1949, Vol. 109 SCIENCE 609

TECHNICAL PAPERS

**The Genetic Effects of Low Intensity Irradiation[1]**

Delta E. Uphoff and Curt Stern

*Radiation Laboratory and Department of Zoology, University of California, Berkeley*

It has been shown by Spencer and Stern (*3*) that irradiation by X-rays at high intensity induces mutations in sperm of *Drosophila melanogaster* at dosages as low as 50 r and 25 r, and that the proportionality between r dose and mutation frequency is maintained down to these low dosages. Earlier workers had established independence of induced mutation frequency from the intensity of irradiation at high and medium dosages. In contrast to these findings, Caspari and Stern (*1*) obtained no significant difference in mutation rates between controls and experimentals, which had been subjected to a dose of 52.5 r in gamma rays administered continuously for 21 days at a rate of 2.5 r per day.

This unexpected result required further tests. After consideration of various factors the following were regarded as possible causes for the apparent inactivity of irradiation in the experiment by Caspari and Stern: (1) low sensitivity to irradiation of aged sperm, (2) dependence of induced mutation frequency at low dosages on a time factor, and (3) errors of sampling which might have obscured a true difference between control and experimental rates. The first possibility was studied by administering 50 r in gamma rays continuously over 24 hr to sperm which had been aged previously for 20 days in the spermatheca of females (*4*). In this experiment the intensity of irradiation was raised several times over that used by Raychaudhuri (*2*) who had found typical intensity independence of mutation rates. Any deviation from the effect of irradiation expected for 50 r at medium intensity would thus be due to a specific sensitivity or insensitivity of the aged sperm. The second possibility, interference by a time factor, was tested by increasing the intensity of irradiation, and the total dosage, by a factor of 2, that is, by administering 100 r instead of about 50 r through continuous gamma irradiation over 21 days. The third possibility, chance, was checked by a repetition of the original experiment, that is, by giving once more 52.5 r in gamma rays over 21 days. Parallel with each experiment, the spontaneous mutation rate was determined in a set of controls.

The data, together with the earlier findings by Spencer and Stern (*3*) and Caspari and Stern (*1*), are summarized in Table 1. It is seen that all three new tests gave an increased frequency of sex-linked mutations in the treated sperm as opposed to the controls. The experimental rate observed by Caspari and Stern is statistically in good agreement with later determinations. The control rate of 0.2489% found by Caspari and Stern is higher than any one of the later control rates. By itself a rate of 0.2489% for sperm aged over 21 days as compared to 0.0074% for sperm not aged (Table 1, line 1) seemed in line with the degree of increase expected, according to the experience of other workers, after such aging. The new data on the control mutation rate in aged sperm suggest considerable variation of age accumu-

TABLE 1

MUTATION RATES FOR SEX-LINKED LETHALS IN SPERM OF *Drosophila melanogaster*, AFTER DIFFERENT TYPES OF TREATMENT

| Treatment | No. of controls | No. of experimentals | Mutation rate percent: Controls | Experimentals | Difference | Significance of difference: $\chi^2$ | P |
|---|---|---|---|---|---|---|---|
| 50 r, 2.3-5 min exposure, not aged (Spencer and Stern) | 73,991 | 21,900 | 0.0074 | 0.2440 | 0.1466 | 33.67 | $<<0.01$ |
| 52.5 r,* 21 days' exposure, aged (Caspari and Stern†) | 54,252 | 51,862 | 0.2489 | 0.2848 | 0.0359 | 1.31 | 0.26 |
| 50 r,* 24 hr exposure after 20 days' aging | 44,601 | 46,332 | 0.1682 | 0.2814 | 0.1153 | 13.31 | $<<0.01$ |
| 100 r, 21 days' exposure, aged ..... | 22,208 | 21,762 | 0.2252 | 0.4528 | 0.2206 | 19.22 | $<<0.01$ |
| 52.5 r,* 21 days' exposure, aged ..... | 36,184 | 29,424 | 0.1765 | 0.2542 | 0.0777 | 4.67 | 0.03 |

* Geometric errors in the administration of the radiation are larger than the difference between the values of 52.5 and 50 r.

† The mutation rates obtained by Caspari and Stern have been adjusted to the slightly different scoring of lethals used in the investigations reported in this paper.

[1] The work described in this report was performed under the auspices of the Atomic Energy Commission.

*The Uphoff and Stern technical note, which appeared in* Science *magazine June 17, 1949. In this note, which is only one-page and two paragraphs long, Stern used a meta-analysis to make the Caspari results on dose/response "disappear." Details were promised, but never appeared, and subsequent researchers cite this article, and ignore the original Caspari work.*

They then tried a third and final experiment. Stern had now moved from Rochester to the University of California at Berkeley. And Delta Uphoff followed him out there a few months later. This time, the control group was in the range that it should have been. However, the treatment response was very high in terms of a mutation rate. The response was about threefold higher than expected if it were in a linear relationship.

It's unlikely that their results were reliable and it made me think that this was an aberrantly high value, comparable to their aberrantly low value for the controls. So, in either case, it was very disconcerting, to say the least.

For reasons that are hard to explain, Stern—and this is a really key point in the story—decides to integrate all five studies together, the three Delta experiments and the Spencer and Caspari studies. He wraps them up all together in his own version of a meta-analysis, publishing a one-page paper, a technical note, in *Science* in which he presents a table and some introductory and conclusionary remarks.

Even more bizarre, he reverts to the two-year earlier position he had, that the original Caspari paper was due to an aberrant control, and that the Delta Uphoff controls of the first two experiments, that were aberrantly low, were now called normal. Stern basically reversed his position on these matters, never sharing with the *Science* readership his previous disavowals. It was only by such indefensible actions that was he able to make a case to support a low-dose linearity.

**21st Century: So, he makes the Caspari study go away.**

**Calabrese:** That's what he did. A key for me is the last sentence in that paper. Stern did not present any of their methodology, and other supportive material in the *Science* paper—only summary findings. However, he (and Uphoff) promised that they would publish the details in a subsequent paper. Thus, the bottom line is that he used his connections to get a note in *Science* but then never delivered on the promise to provide the necessary experimental details that reviewers and others needed to see.

In the aftermath of this episode, various investigators who published papers began to discredit the Caspari study, saying that it had aberrantly high control values and uncertain findings, and they began to marginalize the Caspari paper, which was the strongest study. They began to cite the *Science*/Uphoff and Stern paper which had a one-page summary and the weaker and less relevant effort by Spencer.

**21st Century: And no data—**

**Calabrese:** And no data, and the scientific community, especially the radiation geneticists never demanded of Stern and Uphoff to actually present/publish their findings along with their detailed methods and supplementary data. In the end, the Spencer and Stern and the Stern and Uphoff papers became the two key studies for the Biological Effects of Atomic Radiation (BEAR 1) committee, when it recommended the change from a threshold to a linear model. It's unbelievable. In effect, Stern was successful in distorting the scientific reality. Muller was only too happy to lead the charge.

**21st Century: What's the date on that?**

**Calabrese:** The Committee met from November of 1955 to April of 1956, so they issued their report in the Spring of 1956.

**21st Century: It seems like he orchestrated the entire 10-year campaign.**

**Calabrese:** In any case, the facts are there. Muller and Stern manipulated the field and the course of risk assessment history. There is some historiography that I've put together on it. I think it holds together.

**21st Century: I think you're absolutely right. Here you have a Nobel Laureate who lied and who established a policy which has contributed to killing people—to put it in its starkest**

**terms—has cost the public billions of dollars, and has created fear. So why not tell the story?**

**Calabrese:** Given the significance of the issue, it should be a front-page story in the *New York Times*.

**21st Century: Except that the *New York Times* has been on the other side. That's really the problem.... For the general readership, the technical discussion you've presented on the fruit fly experiments might still be a bit difficult to get a handle on.**

**Calabrese:** Yes, it's a hard story to tell.

**21st Century: I think that to go from fruit flies to human protection and make a policy based on a lie is crazy.**

**Calabrese:** That makes it even more bizarre.

**21st Century: Yes, because you're talking about a handful of experiments, a big lie, and a policy that is costing people billions of dollars and is really at the basis of creating all this fear of radiation that we see with Fukushima.**

**Calabrese:** In 1957, the future Nobel prize-winning geneticist E.B. Lewis, right after that BEAR 1 committee meeting and report, published a crucial paper in *Science,* where he generalized this linear relationship from a reproductive endpoint to somatic cells, to cancer. He relied very heavily in the Stern and Uphoff *Science* paper and the Spencer and Stern paper, which I was critical of as well.

Almost as soon as that paper was published, the National Committee for Radiation Protection, the NCRP, generalized the linearity concept to cancer, and then many other national advisory committees did copycat acceptances, and linearity became a done deal. The tide turned. It was a paradigm shift within a very short time period.

About 20 years later, the U.S. Safe Drinking Water Committee used the BEAR 1 report—with very little further consideration—and transferred the linearity concept to chemicals. The U.S. adopted low-dose linearity for all chemical carcinogens. And it was really like an environmental ideological coup affecting all the classrooms, all the media, all regulations, the risk communication message—almost overnight.

**21st Century: It's an enormous brainwashing, really.**

**Calabrese:** Absolutely amazing. It's a story to be told and a history to be rewritten.

**21st Century: Well, you've launched the re-writing. What I'd like you to talk about now, is the political motivation on the part of Muller in hiding his results. Because when I looked up just very briefly Muller's biography, I saw that he was a protégé of Julian Huxley, who was an infamous Malthusian eugenicist. After World War II, Huxley said that Hitler gave eugenics a bad name, but we needed to convince the population now to "make the unthinkable thinkable," and then he launched the environmentalist movement. He founded the World Wildlife Fund, and as the head of UNESCO, he pursued population reduction policies.**

**So he chose Muller to come to the new Rice Institute in Texas in 1915, and Muller wrote a eugenics book. I don't know if you've read it.**

**Calabrese:** I haven't read the book.


"Burn Down Blog," Rice University

*In this 1916 publication, Julian Huxley is top row, second from left and Hermann Muller is second from right, bottom row. Huxley, a eugenicist-environmentalist who became the first head of UNESCO, recruited Muller to teach at Rice in 1914-1915.*

**21st Century: It's hard to get—Used copies are $200 to $400,so I asked for it via Inter-library Loan. But if Muller is like Huxley, a population control eugenicist, how do you think that works into this? Is that what you were thinking about when you questioned his political motivation?**

**Calabrese:** No. Actually it wasn't. I was looking at it differently. I saw this group of geneticists that he was the leader of. I viewed them as a cohesive "Band of Geneticist Brothers."

**21st Century: Band of genocidal brothers....**

**Calabrese:** They all had the same ideology, they believed, in my view, that they were the only ones who could understand the new biology and save the world, and save the human genome. They believed that they were confronting the medical community that had adopted a threshold model. The geneticists tried to gain influence on all the major health advisory committees, and get geneticists on all those committees. They were always outvoted on a series of committees, but then they got the majority to get appointed to the first BEAR committee of the National Academy of Sciences. And that's what they had to do to win the so-called "big one."

Muller had tried to estimate cosmic-radiation-induced mutation rates back in 1930, and he did this using a *linear* model. And his predictions were off by 1,300-fold! So he couldn't go further on it, but he never abandoned his flirtation with it. That should have told him that he was wrong, but it didn't.

What Muller and his band of radiation geneticists did was to scare everybody, from the press to politicians to the general public, and in a way it became a wildfire, and ultimately it

*Bertrand Russell presiding over a press conference at to launch the Russell-Einstein manifesto in 1955. Hermann Muller signed this, and was recruited by Russell into the Pugwash and the Ban the Bomb movement, attending the first Pugwash meeting in 1957.*

spread to all chemicals and then regulation, and ultimately a mindset that has affected the entire world.

And the interesting thing is that after the atomic bomb was dropped, one thing that was *not* observed in Japan was a significant increase in birth defects. And that is amazingly ironic.

**21st Century: I have two other topics that I'd like to raise. One is that Muller was involved closely with Bertrand Russell's "Ban the Bomb" movement and Pugwash. Russell was an extreme Malthusian. So there you have another connection to a very upfront anti-population philosophy. And the question is really, how much did Muller share their views?**

**Calabrese:** I am not sure, as I have not focussed on this aspect of his life.

**21st Century: The same brief biography I read said that his 1935 eugenics book was translated into Russian, and Stalin didn't like it, for whatever reason, and that's why he had to leave Russia.**

**Calabrese:** Muller had a very strong socialist philosophy that permeated his life, and probably affected a lot of his public life and viewpoints.

**21st Century: Well, Huxley and Russell both had that same kind of "left" profile—they were fascists really, with a "socialist" cover.**

**The second thing that came to my mind is that the whole global warming package follows the same trajectory. And you get the same kind of people. I wrote an article a couple of years ago on how the global warming hoax got its start. Margaret Mead, who was head of the AAAS (the American Association for the Advancement of Science), and who fits the Bertrand Russell/Julian Huxley philosophical profile ideologically, pulled together a meeting of atmospheric scientists, and they did the same kind of thing. They established that you needed this kind of scare story, in order to get people to cut back on consumption, so we could further depopulation. And the people who were at that 1975 meeting were Stephen Schneider, all of the bigwigs of global warming. . . .**

**I don't know what kind of a reaction that you are getting now from the scientific community to your exposés of Muller, but it's very difficult to break through the created myth.**

**Calabrese:** It is probably too early to know.

**21st Century: But it will be hard to get around what you found in the archives. Somebody preserved that evidence.**

**Calabrese:** I'm very fortunate to have the archives. It was amazing to see in the draft paper that they had used the word "threshold," "tolerance threshold," and that in the published version, they put in an acknowledgement to Muller and took out the threshold phrase.

**21st Century: It is very similar to what happened with the global warming hoax, and the effects of both are extremely costly and not helping the population. . . .**

**Calabrese:** I think that the story has to get out.

**21st Century: Truth gets buried, truth just falls by the wayside.**

**Calabrese:** That's right and my sense here is that I'd love to have other freelance writers pick up on this, write their own stories. UMass sent out a press release. . . .

**21st Century: The press release was very good. We'll get the story out. We are not the *New York Times,* but we will tell the truth! And in this case, that's what you need. You need to get your smoking gun out there.**

### For Further Reading


Edward J. Calabrese, 2008. "Hormesis: Why It is Important to Toxicology and Toxicologists," *Environmental Toxicology and Chemistry,* Vol. 27, No. 7, pp. 1451-1474.

Edward J. Calabrese, 2011. "Key Studies Used to Support Cancer Risk Assessment Questioned," *Environmental and Molecular Mutagenesis, 2011.*

Edward J. Calabrese, 2011. "Toxicology Rewrites Its History and Rethinks Its Future Giving Equal Focus to Both Harmful and Beneficial Effects, *Environmental Toxicology and Chemistry*, 2011.