Response

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Dr. Feldmann has written a concise and lucid account of some of the main themes articulated in my book, The Unheeded Cry: Animal Consciousness, Animal Pain, and Science, and I am grateful to him for his generous remarks concerning the quality of the book. This response, then, to employ a metaphor suitable to the occasion and the company, is not meant to bite the hand that feeds me. Rather, I would like to reply to some of Dr. Feldmann's critical comments in the hope of expanding upon some points which were perhaps not made sufficiently clear in my text.

To begin with, I want to comment on the alleged contradiction Dr. Feldmann has noted regarding my view of scientists and ethics. According to Feldmann, I both deny that scientists concern themselves with ethical issues, only at best placating society to assure continued funding, and yet also affirm that scientists are now considering research animal issues morally. Contrary to Dr. Feldmann's comment, the two remarks are not incompatible. The situation, I believe, is this: Scientists are indeed still being trained in a philosophy of science which disavows the relevance of ethics to science, and many leading scientists still echo that philosophy. For example, in Mader's popular basic biology text, Biology, 1990 edition, she asserts unequivocally that "science, by its very nature is impartial.... Science does not make ethical or moral decisions. If we wish to make value judgments, we must go to other fields of study." This is echoed in another popular text, Keeton and Gould's Biological Science (1986) wherein the authors affirm that "another limitation of science is that it cannot make value judgments.... [S]cience cannot make moral judgments." And in February of 1989, NIH director James Wyngaarden was quoted as saying that "research should not be hampered by moral considerations" (Michigan State News, February 27, 1989). Thus is scientific ideology alive and well.

If this is the sort of philosophy taught to nascent scientists, one can understand their failure to engage moral questions. This can be undone in only two ways: First, one can change how scientists are educated and show them that value questions are indeed part and parcel of science, something I have been doing for 15 years. This is very effective but not widely done. Alternatively, one can legislate attention to moral issues, in the hope that, in adhering to the law, scientists will change their gestalt. This is indeed part of the thinking behind mandating local review of animal research projects — those of us who drafted recent federal legislation on laboratory animals felt that mandating discussion in such committees would help break ideological bonds. But even for those who do not end up thinking differently, respect for law forces concern with what the law requires, for example, control of animal pain and suffering.

Thus, one can see that the "contradiction" cited by Feldmann is nothing of the sort. Scientists are still to a large extent in the grip of the claim that science is value-free, yet both educational and regulatory vectors have begun to undermine the hold of that ideology and have forced changes in behavior which lead scientists to conform with emerging public morality regarding animals. Inevitably, being forced to deliberate about research and the control of pain and suffering will lead some scientists to break their ideological bonds; others, on the other hand, will adhere to the letter of the law while finding its spirit incomprehensible or, at best, sentimental nonsense. Either way, things improve for animals, and implicit ethical judgments, hitherto ignored or disavowed, become more explicit.

Furthermore, as society in general becomes more concerned about the issues raised by animal use, scientists must increasingly answer to queries from the general public regarding the moral justification for such uses. Again, articulation of such justification in public forums such as news broadcasts, talk shows and magazines forces greater appreciation and greater sophistication regarding traditional ethical assumptions. As I point out in The Unheeded Cry, one recent textbook in psychology shows a photograph of a laboratory rat with a caption

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asserting that "for moral reasons, animals are used in psychological research" — as if the invasive use of animals does not raise a moral question! If the authors were compelled to defend that statement, they would probably be led to significantly amend it.

One of the most serious criticisms raised by Feldmann and others regarding my book is the charge of "science bashing" — the notion that if one is critical of some current practices in science, one is "anti-science." This is currently a popular strategy among those threatened by criticism of animal research — dismissing the critics as anti-intellectual Luddites out to restore the Dark Ages, unappreciative of the advances made by modern science. I'm surprised that Dr. Feldmann allows himself to be drawn into this mode.

To be supportive of something does not require that one ignore its defects. I am, for example, enamored of old Harley-Davidson motorcycles; nonetheless, I am aware that there are major problems associated with their engines. So, too, was the Harley-Davidson Company, and they have essentially solved the problems. Many members of the scientific establishment are extremely concerned about the statistically small number of cases of data falsification which Dr. Feldmann cavalierly dismisses. They are aware that such cases bespeak deep problems which are threatening to the very foundations and fabric of science. As many other people besides myself have pointed out, including Wade and Broad in their influential Betrayers of the Truth, such cases bespeak major "publish or perish" pressures associated with science as a *career* , rather than something pursued for its own sake or for social benefit. If feeding one's family and paying the bills is predicated on publication, there will naturally be far more pressure to publish, and researchers will be more tempted to cut corners. It is for this reason that some scientists, such as Marsha Angell of the New England Journal of Medicine, have suggested that publication be judged by quality not quantity.

In addition, as I point out in the book and Arthur Neufeld has also pointed out in a New Scientist article entitled "How far do you trust your colleagues?" (January 15, 1987), there is no money nor time for replication of research results. Often the relevant equipment is only available in the laboratory publishing the results, and as Neufeld says, "reproducibility is not one of the criteria used by referees when they accept an article for publication." Once again, then, we find another vector which makes it simpler and more tempting to cut corners in research, namely little chance of being caught by the traditional internal reproducibility check.

All of this is further complicated by the fact that in many large laboratories, the actual "scut work" of running an experiment is left to junior scientists, graduate students, and technicians. The principal investigator's name goes on the final paper, yet he or she may have had little to do with the generation of the data. Many senior careers have been wrecked by failures at a lower level of which the investigator had no knowledge, yet for which he or she was held administratively responsible. Sloppiness at the most basic levels, let us recall, was instrumental in forcing the passage of the Good Laboratory Practices Act in the 1970's, something a prominent scientist friend of mine has called "the shame of the scientific community" because it legally mandates what toxicological laboratories should have been doing anyway as presuppositional to their activities.

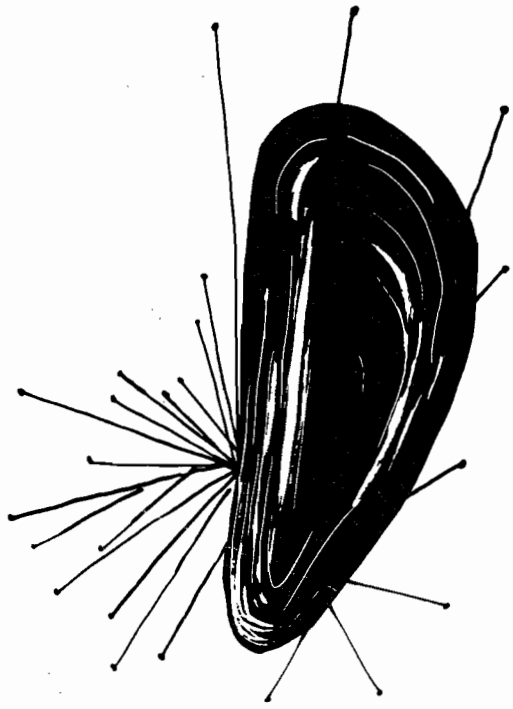
Numerous studies have indicated that most scientists know of data fudging or other intentional bias in reporting of research results. In a survey conducted and published by The New Scientist in November of 1976, 92% of respondents indicated that they had direct or indirect experience of intentional bias; 66% of respondents indicated that they had experience of more than one case. I urge Dr. Feldmann to candidly query his own colleagues in research as I have done. I am certain he will find similar results. In addition, the New Scientist survey indicated that in 80% of the cases nothing happened to the data falsifiers — in only 10% of the cases was the end result dismissal. One question not asked by this survey was how many scientists would report cases of suspected data falsification — not surprisingly, other research has shown that the majority would not. Our society has great ambivalence regarding "squealers" and "whistle blowers" — recall the message in Ibsen's An Enemy of the People. Recall the childhood bias against "tattle-tales" and the perjorative connotation of "ratting" on someone.

I am not suggesting that all or most or many scientists are guilty of fraud. I am simply echoing a point made by Dan Greenburg in another article in the New Scientist in November 1987 when he pointed out that there is little quality control in science and that "detection [of fraud] in most cases is ... accidental." Greenburg remarks that "for high octane gall in proclaiming its ethical purity, the scientific community

has long been the runaway winner on the institutional landscape. Miscreants in our ranks are rare, it insists, but when the integrity of science is betrayed, defrocking of the culprits is assured by sensitive internal checks." Greenburg goes on to affirm that this is not the case.

Finally, Dr. Feldmann should recall the context in which I raised the issue of fraud and bias in science. It was not to malign the research community, which is probably no more nor less honest than the rest of us, but rather to undercut the smug assurance which discredits anecdotal or observational accounts of animal behavior in contrast to controlled experiments. My point was to show that laboratory research is open to the same sort of extreme skepticism that time-tested anecdotal evidence of animal behavior is. The proper stance, in my view, is to examine both types of evidence with a critical eye, as Romanes did, not to dismiss either as intrinsically inferior or flawed, and not assume that either is perfect.*

* **Editors' Note:** It is our policy to allow the reviewer a brief final word. Dr. Feldmann's reply is brief indeed: "Professor Rollin ably explicates his intentions. I am reassured. I respectfully refer readers to the book itself for context and tone."



THE PASSING OF GIANTS

(for the African elephant, and especially for the more than half a million killed between 1980 and 1987)

They were the gods of thunder.
Survivors from the icy dawn.
Trembling the earth with their footfall.
An entourage of swirling dust clouds,
moving through the jungle,
with trumpets heralding
the approach of majesty.
Now dealers and carvers haggle the price
for dead pieces of greatness.
The sigh of the last elephant fades
to the music of dusty piano keys.
How pretentious we must be
to topple giants.

Kathleen Malley