

Blind Hostility: A Response to Russell and Nicoll (43958B)

PETER SINGER¹

Monash University, Centre for Human Bioethics, Clayton, Victoria, Australia

Introduction: An Allegation Withdrawn

A revealing episode occurred as I was drafting my response to Russell and Nicoll. Their paper had been sent to me by the editor of *PSEBM* on March 1, 1994, with an invitation to respond. On reading the paper I immediately found several false and misleading claims, which were naturally of concern to me. But there was one that was especially outrageous. In *Animal Liberation* I quoted Elizabeth Whelan, executive director of the American Council on Science and Health, as saying that "our regulators are beginning to take note of the scientific literature rejecting the infallibility of the laboratory animal test" (1, p58). Russell and Nicoll said that I appeared to have "fabricated" this quotation. They made the allegation prominently, not only in the text of their article, but also specifically emphasized it in their abstract. In support of their claim, they wrote:

First, we doubt that a knowledgeable person such as Dr. Whelan would refer to any laboratory procedure or test (animal or otherwise) as being "infallible." Second, the source of this quote is a [s]yndicated article appearing in *West County Times* (California), January 17, 1988" (4, Note number 62, p276.) This is a local newspaper with a limited distribution. If an authoritative figure such as Dr. Whelan had been quoted in a syndicated article as having made such a statement, it surely would have appeared in major newspapers that would be easier to check. By coincidence, the *West County Times* is published in our area of residence and copies are kept on microfilm in a local library. Thus it was as easy matter for us (but not for someone living outside the immediate area) to check the authenticity of the

quote. There was no "syndicated article" with the statements attributed to Dr. Whelan in the cited issue of that newspaper, nor in others around that date. Accordingly, we wrote to Dr. Whelan to inquire whether she knew what the source of that quote might be. In her letter of reply, she stated: "Peter Singer's 'quotes' are totally out of context and completely misrepresent the position of me as an individual and the American Council on Science and Health as an organization." (Russell S, Nicoll C, unpublished manuscript, p22, which corresponds to material on p122 of their printed Comments)

Had Russell and Nicoll been able to make all this stick, it would have been the most damaging of the claims they made against me and Chapter 2 of *Animal Liberation*. But on May 9, 1994—when my response to Russell and Nicoll was already well advanced, and indeed would have long been complete, had not other commitments forced me to seek an extension to the editor's original April 15 deadline—I received another letter from the editor of *PSEBM*. It contained, without explanation, a revised abstract and one revised page of the text of Russell and Nicoll's critique. On checking the revisions, it became apparent that Russell and Nicoll had found the quotation from Elizabeth Whelan, and withdrawn the allegation that I had fabricated it. But even that was not the end of the story. I completed my reply, and sent it off. Some nine months later—by which time I was wondering what had happened to the article—I received yet another revised version of the Russell and Nicoll article, with a request from the editor of *PSEBM* to take a further set of revisions into account! What Russell and Nicoll now say about use of the quotation from Dr. Whelan is far weaker than what they said in their original version (although it is still false, as I shall show in a moment).

This episode shows something about Russell and Nicoll's methods. If they are unable to find a quotation I use, and they are doubtful that a scientist would have said what I quote the scientist as saying, they accuse me of having fabricated it! The real explanation for why Russell and Nicoll were originally unable to find

¹ To whom requests for reprints should be addressed at Centre for Human Bioethics, Monash University, Clayton, Victoria, Australia 3168.

the quotation is the obvious one: a simple typo. My book gives the date as *West County Times* (California) January 17, 1988. The correct date is January 27, 1988. I regret the error, naturally, but only those who have never made a slip in typing or proofreading have a right to be highly critical here; and even then, an accusation of having fabricated the quotation goes well beyond the evidence.

Given this approach, consider Russell and Nicoll's assertion in their abstract that "Of the 132 different references, we attempted to verify 49 of them. Of these 16 (33%) were incorrect or we could not find." To this, two points need to be made:

(i) Russell and Nicoll provide no information about how the selection of 49 out of 132 references was made. It was presumably not random. Thus, the percentage figure they give "33%" is scientifically worthless. It lends a spurious precision to a figure that in fact can say nothing whatsoever about how many of my references as a whole are "incorrect or we could not find." Scientists should know better.

(ii) Russell and Nicoll provide no information at all about how many of the references they checked were incorrect, as distinct from being ones that "we could not find." Nor do we know how hard they looked for the ones that could not find, or why they were unable to find them. (For example, one reference was from a South African publication which their library may not have carried.) Thus, the information does not indicate anything about the accuracy of my references, although it is clearly intended to give the impression that many of them were inaccurate.

If Russell and Nicoll's use of statistics here were a specimen of the scientific method generally used in the biological sciences, it would be no wonder that people cast doubt on the validity of many of the experiments conducted on animals. But it would be unfair to accuse biological scientists in general of being as scientifically slipshod as Russell and Nicoll are in this article.

Now let us go back and look at what Russell and Nicoll still say, and do not say, in the final version of their paper, about my use of the quotation from Whelan:

- First, they no longer "doubt that a knowledgeable person such as Dr. Whelan" would refer to tests as infallible. Plainly, she did.

- Second, they do say "there was no article by Dr. Whelan in the cited issue of that newspaper, nor in others around that date." Well, they obviously did not look very hard here.

- Third, they imply that I have deliberately cited a source that would be difficult for most of my readers to check. But I cited the *West County Times* article for the wholly innocent reason that it was sent to me

by a friend, and I did not know where else it had appeared.

- Fourth, Russell and Nicoll quote a personal communication from Dr. Whelan in which, by putting scare quotes around the word "quotes" she suggests that I have not quoted her accurately. But my quotation is totally accurate.

- Fifth, the passage they quote from Dr. Whelan says that the quotations I use are "taken from a media report of one of my many speeches." But the article from which I am quoting has the byline "by Elizabeth Whelan," and there is no indication that any other journalist has had a hand in it, nor that it is a report of a speech. Russell and Nicoll themselves now contradict Whelan here, by describing the article as a "syndicated column."

- Sixth, although Russell and Nicoll had, by their revision of May 1994, withdrawn the only "evidence" they had that I had made up a quotation, they did not then alter the level of the invective that they use against me, and still concluded their article by making the (now utterly baseless, even on their own showing) claim that I was in the business of "apparently, inventing 'quotes'" (unpublished manuscript, p45). Only after I had pointed this out, did they finally admit that this too was mere abuse that they could not defend, and alter their conclusion.

- Seventh, Russell and Nicoll claim that I have misrepresented Whelan. They write, "The article is not a blanket criticism of animal use in toxicity testing, as Singer suggests. . . ." This is the classic trick of erecting a straw man in order to knock it down. I never suggested that the article was a blanket criticism of animal use in toxicity testing. I introduced the quotation from Whelan by saying "After decades of mindless animal testing, there are now some signs of second thoughts. Dr. Elizabeth Whelan . . ." There is not the slightest indication here of a blanket condemnation, and the quotation amply bears out what I said about it. In fact, a reading of the section from which this quotation comes, which is on the desirability of developing nonanimal using alternatives to current methods of toxicity testing, shows that not only do I not present Whelan as making a blanket condemnation of animal testing, but I myself do not make any such blanket condemnation of the scientific value of animal testing for establishing toxicity. Quite the contrary: at the end of the section, just three pages after the quotation from Whelan, I conclude:

Developing completely adequate alternatives to all tests for toxicity will take longer [than eliminating redundant tests by computerized access to existing test data], but it should be possible. Meanwhile there is a simple way to

cut down the amount of suffering involved in such tests. Until we have developed satisfactory alternatives, as a first step we should just do without any new but potentially hazardous substances that are not essential to our lives. (1, p61)

Obviously, no one who believed that all toxicity testing using animals was completely pointless would propose a staged phase out along these lines. If something were of no scientific value, we would not need to develop adequate alternatives to it. (The question of the ethical justifiability of animal testing is, of course, a separate matter from whether animal testing is, and if so to what degree, a scientifically valid guide to the effect of a substance on humans.)

A Time-Saving Proposal

Russell and Nicoll's critique of Chapter 2 of *Animal Liberation* is long and contains a large number of allegations and innuendos. As I showed in replying to their previous totally unsuccessful effort to discredit my work (2, pp59–60), these allegations and innuendos are invariably distorted, misleading, and misdirected. But it would take a long time and a lot of effort to show this of every allegation—see, for example, the amount of space needed already to rebut what they say about my use of just one quotation. The resulting paper would undoubtedly be longer than Russell and Nicoll's, because it always requires more space to explain why an allegation is false than it takes to make the allegation. Not only would a comprehensive reply be very long, it would also be even more tedious than Russell and Nicoll's paper, and I guess that few readers would have sufficient interest in whether I did or did not misrepresent someone to persevere to the bitter end. In any case, I do not have so much time to spare, because I have more interesting lines of academic research to pursue. On the other hand, I do not want to leave myself open to the accusation of having answered Russell and Nicoll's allegations selectively, picking out only the ones that were false. I have therefore adopted the following time-saving proposal. I will discuss Russell and Nicoll's opening general remarks, and then the very first example of their criticism of my account of an actual experiment. Coupled with what we have already seen in the case of the Whelan quotation, this will be quite enough to show the falsity of their claims, and the blatantly selective way in which they misread almost everything I say. Moreover, since I take the first example, rather than the one that might suit me best, I trust that I shall not be accused of having selected the weakest point for my reply. In all, then, I will be discussing only the first three typescript

pages of their paper, but the quality of their critique is, unfortunately, no better in the remaining 52 pages.

Opening Remarks: Smear Tactics

Right from the start, the language that Russell and Nicoll use indicates clearly that they are not interested in serious discussion of an ethical issue, but only in polemics. For an example, look at the way they use scare quotes in their abstract. Not only do they put this around words like “documents” and “evidence,” where they are presumably seeking to imply that what I have provided is not really evidence that documents my claims, but they also use it around “notes,” as if the endnotes to my chapter were not really notes at all! One can only wonder what else they might be. They conclude the abstract by saying that I “present myself” as an ethicist and a moralist, as if I really am not one. All I can say about this is that they should take the matter up with their colleagues in philosophy departments, who use my books and articles in ethics courses all over North America, in many European countries, and in Japan. Or maybe they should tell the appropriate advisory committee of the *Encyclopaedia Britannica*, since that august body was so misguided, a few years ago, to ask me to write the major article on “Ethics” for their recent revision of the encyclopedia (3). Others who would undoubtedly benefit from the advice of Russell and Nicoll on my qualifications in ethics would be my publishers, Basil Blackwell, for whom I have edited *A Companion to Ethics* (4) in their “Companions to Philosophy” series (which they were misguided enough to think so well of that they have now commissioned me to co-edit another volume, *A Companion to Bioethics*); and the Delegates of Oxford University Press, for whom I have edited *Applied Ethics* (5) in their Oxford Readings in Philosophy series, and more recently, the volume *Ethics* (6) in their Oxford Readers series.

The petty and ignorant nature of Russell and Nicoll's attack can also be seen by their statement referring to the fact that I do not make a practice of citing page numbers for items appearing in newspapers. They write:

We do not believe that this sort of bibliographic nonchalance is common among philosophers in their writings, and we are disappointed to find such poor citation techniques.

Russell and Nicoll here show themselves to be both ignorant of philosophy, and too busy heaping abuse on their opponents to stop to check on common citation practices in the humanities. It is quite common, in works of undoubted scholarship in the humanities, not to use page numbers when citing newspapers. To check my perception of this, after receiving Russell

and Nicoll's article, I picked up three books that I had on my desk. I was doing some work on the ethics of abortion at the time, and so all three were on that topic.² The first of the books on my desk was James Mohr's *Abortion in America*, probably the outstanding scholarly work on nineteenth century American attitudes to abortion. Although Mohr cites mostly from nineteenth century books and journals, when in his "afterword" he brings the story of abortion up to date, he has several citations to the *New York Times*. They do not have page numbers (10, p314nn4–9). The second book was a collection of essays. Most of them did not refer to newspapers, but I noticed one newspaper reference in an essay by H. L. A. Hart, who for many years held the Chair of Jurisprudence at the University of Oxford, and is probably the most distinguished legal philosopher of the twentieth century. Hart's reference to *The Sunday Express* has no page number (11, p207n 19). Finally, I also happened to have on my desk a book by a distinguished scientist from Russell and Nicoll's own field, Clifford Grobstein, who is Professor of Biological Science and Public Policy, Emeritus, at the University of California, San Diego. Grobstein's *Science and the Unborn* is, like *Animal Liberation*, aimed at a general readership rather than at the author's professional colleagues. Its notes have a number of newspaper references. They do not have page numbers (12, p194nn2, 4, 6; 194n2, 10).

Russell and Nicoll also say that my notes are "haphazard" and often do not give the authors of a cited source. This is highly misleading, because it fails to point out that in most of these cases, the author is named in the text. A large number of the notes are to experiments described in the text. A typical description begins:

At Kenyon College in Ohio, J. Williams and D. Lierle performed a series of three experiments to study. . . . (1, p48)

The note then gives the journal, volume, page numbers, and year. It does not needlessly repeat the name of the authors. (With remarkable indifference to consistency, Russell and Nicoll later do mention the fact that I give the authors' names in the text, but not when complaining about my notes; they mention it only in order to imply some other dark motive that I must have had. So what do they want: names or no names?)

² Incidentally, Russell and Nicoll say that animal liberationists like myself "prefer not to think about" the issue of abortion. This is another example of their invincible ignorance of the subject they are discussing. My widely-used text *Practical Ethics*, contains a long chapter on the abortion issue (7, pp135–174); I have also co-edited, and contributed essays to, an entire volume on human embryo experimentation, which inevitably raises many of the same issues as abortion (8), and I have discussed the ethics of abortion in other publications as well. (9).

Russell and Nicoll have carefully calculated the number of pages of Chapter 2 of *Animal Liberation* devoted to particular areas of experimentation on animals, and then compared this with National Institutes of Health expenditure. They seem to find something sinister in the fact that there is no correlation between these figures. But I never claimed that the chapter gave equal time to all areas of animal research. Given the chapter's place in the book as a whole, it should have been obvious—and I did in fact explicitly state (1, p22)—that I was using experimentation and factory farming, as "examples of speciesism in practice"—that is, as illustrations of the kind of behavior made possible by the prejudice we have against taking seriously the interests of nonhuman animals.

An appropriate comparison here would be an Amnesty International Report against the treatment of political prisoners in a jail in, say, Ruritania. Suppose that in the prisons in Northern Ruritania, political prisoners are routinely tortured. On the other hand, in Southern Ruritania, while political prisoners are still locked away for many years, they are not tortured. If the Ruritanian Government then complained that Amnesty International's Report on Political Prisoners in Ruritania was biased, because it gave more space to accounts of what happened in Northern Ruritania than it did to what happened in Southern Ruritania, would anyone think that such an objection discredited the Report? Of course not. Russell and Nicoll's objection is equally beside the point.

Russell and Nicoll claim that the chapter of my book gives "many examples of luridly portrayed research in the same tones one might use to describe an ogre under the stairs in a story designed to frighten children." When I read this, I really wondered if they had read the wrong book. There are books about animal experimentation that use highly emotional language, but mine is definitely not one of them. When *Animal Liberation* was published, several independent reviewers went out of their way to make just the opposite point. In the *New York Times Book Review*, for example, the reviewer used phrases like "unhysterical yet engagingly written" and "unrhetorical and unemotional" (13, p5) *Kirkus Reviews* wrote: "Without hysteria, Singer points out the suffering much scientific research involves for animals. . . ." (14). In Cleveland, the *Sunday Plain Dealer* reported that "The approach [Singer] selects is, with few lapses, unemotional. The book, therefore, is stark and economical and analytical to a fault. . . ." (15). In fact, as the *Plain Dealer* review goes on to point out, it is the nature of the material itself that can be horrifying, not my style of writing, which is deliberately low-key, and closely based on the experimenters' own reports in scientific journals. The problem is that there simply is

no way to describe what happens to animals in some experiments without arousing emotional responses. (And it would be a matter of deep concern if there were). Here, for example, is one of the most emotionally arousing passages from the chapter, a description of what happens to a dog in an experiment conducted by one of the experimenters that Russell and Nicoll defend, Martin Seligman (together with Steven Maier and James Geer):

When a normal, naive dog receives escape/avoidance training in a shuttle-box, the following behavior typically occurs: at the onset of electric shock the dog runs frantically about, defecating, urinating, and howling until it scrambles over the barrier and so escapes from shock. On the next trial the dog, running and howling, crosses the barrier more quickly, and so on, until efficient avoidance emerges.

Seligman altered this pattern by strapping dogs in harness and giving them shocks from which they had no means of escape. When the dogs were then placed in the original shuttle-box situation from which escape was possible, he found that

such a dog reacts initially to shock in the shuttlebox in the same manner as the naive dog. However in dramatic contrast to the naive dog it soon stops running and remains silent until shock terminates. The dog does not cross the barrier and escape from shock. On succeeding trials the dog continues to fail to make escape movements and thus takes 50 seconds of severe, pulsating shock on each trial . . . A dog previously exposed to inescapable shock . . . may take unlimited shock without escaping or avoiding at all.

The point to note about this passage, which I have taken directly from *Animal Liberation* (1, p.46), is that nearly all the information is in the two doubly indented passages, which are direct quotations from the researchers' own publication (16, p256). This includes descriptions like "runs frantically about, defecating, urinating and howling," and "severe, pulsating shock." My own words add nothing in the way of emotionally laden content.

On the nature of my prose style, I am happy to leave the judgment to the reader, after reading the chapter in question.

The First Experiment—Who Is Doing the Distorting?

Now we come to the first experiment I describe in my chapter, which is also the first that Russell and Nicoll discuss, and as I have already indicated will be the only example I shall use to analyze their accusations regarding my account of experimentation. The experiments in question were carried out by Brooks Air Force Base in Texas.

The first accusation that Russell and Nicoll make in regard to my description of these experiments is that, by referring to the fact that the film *Project X* was based on this research, I intend to mislead readers into believing that the actual experiments were carried out on chimpanzees. Again, I can only look at these words in amazement, and wonder how Russell and Nicoll could have so totally misread what I actually wrote. My description of the actual experiments consistently uses the term "monkeys." For example, I write "The monkeys sit in a chair. . . . Once monkeys have been trained to do this" By my rough count, the words "monkey" or "monkeys" appear 24 times on pp25–29, in which the experiment is described and discussed, and a quote from Dr. Donald Barnes, the experimenter who was for many years in charge of these experiments, refers explicitly to "rhesus monkeys." The book also includes a photograph of the experiment, which clearly shows the monkeys in the Primate Equilibrium Platforms, and has a caption that begins "At Brooks Air Force Base in Texas, monkeys are trained" There is no further reference to chimpanzees in these pages after the first two paragraphs of the first page of the chapter, which discuss the film, not the actual experiments. Accordingly I cannot imagine why Russell and Nicoll think that I am trying to pass off the experiments as being done on chimpanzees. A chimpanzee, as even Russell and Nicoll, must know, is not a monkey.

Russell and Nicoll then say that my description

omits any mention of the *rationale* behind the experiments. . . . Singer says not one word about why such experiments might be performed, except to deny their utility for estimating "probable force strength or second strike capability" (in other words, to estimate how human pilots might be able to perform) after a nuclear attack.

If I still taught first-year logic students, I would probably use this as an illustration of self-contradiction. After saying that I say "not one word" about the rationale, Russell and Nicoll go on to quote several words that I do say on this topic. But in fact these are by no means the only words that I say on the topic. Among other words that appear on these pages are:

Once the monkeys are regularly keeping the platform horizontal most of the time, they are exposed to lethal or sublethal doses of radiation or to chemical warfare agents, to see how long they can continue to “fly” the platform. (1, p27)

I also quoted extensively from Don Barnes:

“For some years, I had entertained suspicions about the utility of the data we were gathering. I made a few token attempts to ascertain both the destination and the purpose of the technical reports we published but now acknowledge my eagerness to accept assurances from those in command that we were, in fact, providing a real service to the U.S. Air Force and, hence, to the defense of the free world.” (1, p28)

Finally, the caption to the photograph of the experiment reads:

At Brooks Air Force Base in Texas, monkeys are trained through electric shocks to keep these platforms level by means of controls that simulate the flying of air force bombers. They are then gassed or irradiated to test how long they can continue to keep the platforms level under simulated conditions of chemical or nuclear attack. (1)

Not one word, except for the seven words they quote? After going back and reading over the appropriate pages from *Animal Liberation*, and then re-reading what Russell and Nicoll have written, I am at a loss to know what their problem really is. Are they being deliberately dishonest? That seems unlikely, since they would have known that I would have the opportunity to refute their statements, and would be able to do so decisively. I can only assume, therefore, that they are so blinded by hostility to my ideas that they read selectively, finding only what confirms their opinion, simply not seeing what contradicts it. This is speculation, of course. But I can think of no better explanation for the fact that, in an article that complains (without adequate reason) that I misrepresent experimenters, they should so palpably, so blatantly, misrepresent what is in *Animal Liberation*.

The Real Issue

That is enough by way of point-by-point rebuttal. We have reached my self-imposed limit—the description of the first experiment. Instead of continuing with the tedious and apparently endless process of displaying Russell and Nicoll’s boundless capacity for misreading my text in order to blacken my reputation, I would prefer to use this opportunity to address the

readers of *PSEBM* on the much more important issue of what can be done to overcome the problems of which Russell and Nicoll’s paper is a symptom—the extreme polarization that exists in the United States between experimenters who use animals and their critics. What I find really depressing about Russell and Nicoll’s paper is their failure to acknowledge that even a few of the experiments that I describe might not have been ethically justifiable. In Australia, it is easy to find researchers who will admit that many of the experiments I describe are indefensible. Australian researchers have been prepared to take serious steps to prevent such experiments. In the United States, the hostile and defensive response of experimenters has prevented that kind of dialogue developing, except occasionally on a local level. This polarization is not good for those who wish to conduct research on animals, since it breeds violence and forces researchers to spend large sums on elaborate security measures. It is also not good for animals, because it means that the discussion of animal experimentation becomes a debate between those who believe that all experiments on animals should be stopped immediately, and those who believe the situation should continue exactly as it is now. This does not make for progress in science or animal welfare.

In a number of other countries, including Britain, Sweden, Switzerland, the Netherlands, and Australia, the climate of discussion is entirely different. In Australia, for example, the animal movement—including animal liberation organizations with which I am involved—has for many years regularly been consulted on proposals for revision to the *Australian Code of Practice for the Care and Use of Animals for Scientific Purposes*, a document produced by the National Health and Medical Research Council (Australia’s equivalent of National Institutes of Health) together with the Commonwealth Scientific and Industrial Research Organisation and the Australian Agricultural Council (17). As a result, this Code is far in advance of regulations in the United States. Typically, Russell and Nicoll misread what I said about this in *Animal Liberation*. They report me as saying that Australia and Sweden “have research oversight committees that contain lay members” and they then add “but he ignores the fact that the United States does as well.” In fact what I say is:

A minimal first step would be a requirement that no experiment be conducted without prior approval from an ethics committee that includes animal welfare representatives and is authorized to refuse approval to experiments when it does not consider that the potential benefits outweigh the harm to the animals. As we have seen, systems of this kind already

exist in countries such as Australia and Sweden and are accepted as fair and reasonable by the scientific community there. (1, p87)

Note how Russell and Nicoll transform my suggestion that ethics committees should include "animal welfare representatives" into one that these committees should contain "lay members." This enables them to claim that the United States has these committees already. But the United States has no requirement that animal experimentation ethics committees should have animal welfare representatives. The Australian Code clearly marks the distinction. It specifies that ethics committees must consist of at least four persons, including a separate person appointed from each of four categories (17, p11). Category A is a person with qualifications in veterinary science. Category B is a person with substantial recent experience in animal experimentation. Category C is described in the Code as:

A person with demonstrable commitment to, and established experience in, furthering the welfare of animals, who is not employed by or otherwise associated with the institution, and who is not involved in the care and use of animals for scientific purposes. The person should where possible be selected on the basis of membership of an animal welfare organisation.

Category D is the equivalent of the United States' "lay person," that is:

An independent person who does not currently and has not previously conducted experiments using animals, and who is preferably not an employee of the institution.

In other words, Australian animal experimentation ethics committees must have *both* an independent lay member *and* an animal welfare representative, and the two roles must be filled by separate people.

The Australian committees are guided by a statement of "General Principles" that is more balanced than equivalent United States principles, and they also have wider and less equivocal powers. As a result, many of the experiments that I describe in my book could not take place in Australia. I know of no evidence that Australian scientific research is harmed as a result; indeed, although such matters are extraordinarily difficult to quantify, my admittedly subjective impression is that on a per capita and per research dollar basis, Australian medical research certainly matches and quite possibly surpasses United States research in the significance of its results.

Despite the fact that the Australian Code is so far in advance of that in the United States, the Australian

National Health and Medical Research Council is currently undertaking another revision of the Code, and this time it is going beyond mere consultation with animal welfare organizations. This time a representative of the Australian and New Zealand Federation of Animal Societies—the umbrella body for most of the Australian and New Zealand animal welfare and animal liberation organizations—has been invited to be a member of the working group that will prepare the revision. As it happens, I am the President of the Australian and New Zealand Federation of Animal Societies.

The United States regulations on animal experimentation are widely regarded as weak, and not only in Australia or by adherents of the animal movement. Writing in *New Scientist*, not a journal hostile to science, Helen Gavaghan has described the United States legislation covering animal experimentation as "remarkably weak" (18, p32). Indeed, as long as the majority of animals used in experimentation—mice and rats—are not even covered by the United States Animal Welfare Act, the United States will continue to lag far behind more civilized nations. There are other influences at work within the United States scientific community (19, 20). Only if they prevail over the kind of blind hostility that Russell and Nicoll so vehemently manifest are there grounds for a more fruitful discussion, of the kind that is possible in Australia today. This could then bring about some real progress, both for animals and for science.

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