Who Does Science Serve?

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Les jeunes femmes qui entrent dans le domaine des sciences rencontreront vraisemblablement certaines influences qui protègent des intérêts directs. Les intérêts académiques, politiques et industriels ont une influence profonde sur la démarche scientifique, de la conception de la recherche à son interprétation, en passant par sa publication et son financement. Ces différentes influences peuvent avoir pour résultat toute une gamme d'actions injustes et même malhonnêtes: corruption dans le domaine de la science industrielle, favoritisme académique ou gouvernemental, préjugés contre telle personne ou tel projet, promotion de toute donnée qui appuie la politique du gouvernement, trucage des données pour avantager l'intérêt de certains scientifiques. Cet article examine comment et pourquoi cette situation existe, et ce qui peut être fait pour que disparaisse ce lien direct entre ceux qui sont intéressés à obtenir certains résultats, et ceux qui recueillent les données et les analysent.

". . . in the long run, what each of us is likely to do will not be remembered as the great individual achievement of John or Mary. Very few names will be remembered in fifty or a hundred years, and probably none in a thousand. But at the end, the reasons for being anonymous lost their naive and symbolic nature, to become instead part of the awareness that out there, there are no share-holders in this human enterprise – as I had believed. Out there we have "competitors," who might see in our published names the great vile chance for self-aggrandizement. And the introduction of these new values – "competition," "me," "fame," "public image" – into Western science is to a large extent the responsibility of this country. Everywhere in science the talk is of winners, patents, pressures, money, no money, the rat race, the lot: things that are so completely alien to my belief in the way of being human in a world threatened by natural and man-made disasters that I no longer know whether I can be classified as a modern scientist or as an example of a beast on the way to extinction, of little use in these new dimensions of human achievement – as no doubt some great television commentator would put it . . ."

(from a letter by Dr. Anna Brito in *An Imagined World*, by June Goodfield)

"A novice must stick it out until he discovers whether the rewards and compensations of a scientific life are for him commensurate with the disappointments and the toil; but if once a scientist experiences the exhilaration of discovery and the satisfaction of carrying through a really tricky experiment – once he has felt that deeper and more expansive feeling Freud has called the "oceanic feeling" that is the reward for any real advancement of the understanding – then he is hooked and no other kind of life will do."

(Peter Medawar, *Advice to a Young Scientist)*

Science can be inspiring stuff. Scientific research conjures up images of dedicated investigators pursuing truth with no further goal than to find it and to offer it up, freely and openly, for the service of society at large. It's an appealing image, and one which, I hope, will attract more and more women. Women have much to offer – in new visions of what to look for, how to find it, and how to use what they find. And scientific research can certainly be rewarding – intellectually, at least. But women should have a healthy scepticism concerning the organization and practice of science, if they want to survive doing it. And along with all scientific researchers, they also have a responsibility to change it, to work toward a more liberated, independent, and socially responsible way to try to understand the world. Because the image of the disinterested researcher, pure of heart and clear of mind, is a mirage.

I am writing this article because I think young women entering science should learn this before they get in so deep that the discovery is painfully disillusioning. I found out the hard way, when I nearly failed my Ph.D. exams, after years of uncomfortable and frustrating research. My field was a branch of animal ecology and I was trying to model, mathematically, how predators choose their prey. Like most post-graduate research work, my project was just a minor extension of a model developed by others and widely accepted as a very good description of animal feeding behaviour. So when my experiments showed flaws in the original model, I was amazed and traced their origins, naively expecting my colleagues to welcome my findings. But not only did they not praise my work, they tried to prevent its distribution and publication. My research was far from flawless and, no doubt, could have been much improved by larger numbers of experiments and a more sophisticated statistical treatment. But now that my initial disappointment and confusion are behind me, I'm convinced that the unpopularity of my results had more to do with their implications for my colleagues than with their scientific merit. Finding fault with a widely held theory, which many ecologists had milked for a long string of publications, was asking for trouble. Why should they want to admit they'd been wrong, especially when scientists are trained to subject new ideas to the most rigorous scrutiny? After all, these scientists had based their reputations on a theory's being right – by now they had a strong vested interest in its correctness.

This is just one kind of scientific vested interest. In this article, I'll catalogue and analyse three distinct, though inter-related, vested interestes: academic, political, and industrial. Together they influence, and even control, much of scientific work.

The academic vested interests are the first a young scientist is likely to encounter. In many areas of research, especially in exciting new fields like cancer research, genetic engineering, and nuclear physics, the risks are high but the rewards for successful research are great. As a result, competition for jobs is stiff and getting even stiffer. Young researchers know they have to produce many publications – with important, original results – if they want to secure a permanent job. In addition, the equipment and chemicals needed to conduct the research are expensive, and it can be terribly difficult to get results published in reputable journals.

All these problems can be overcome when a junior researcher works in the lab of a more established, reputable scientist. I believe a kind of unwritten contract operates between the junior and senior scientist: the senior scientist secures the research funds – his reputation ensures generous grants to cover the cost of equipment, chemicals and even the junior researcher's salary. Likewise, publication is easier when the senior researcher co-authors the papers. In exchange, the junior researcher churns out data and drafts the papers to provide the senior researcher with proof that his research funds are well spent.

In this way, a hierarchical lab structure is established, with the senior scientist embroiled in administration, writing grant applications, reviewing papers, serving on departmental, professional, charitable, or grant-allocation committees, and the junior researchers working in the lab, largely unsupervised, and meeting with the senior scientist only to discuss experimental results and revisions to publications. This relationship is borne out by a quick survey of the top five Canadian Medical Research Council grant recipients in 1979 and 1980. The science-citation index lists several papers per year for each one (three to thirteen), but in each case the vast majority of these papers are co-authored, often with more junior researchers in their labs.

In such a situation, heavy pressure to produce results, combined with scant supervision, could easily tempt ambitious young scientists to provide interesting data extra quickly. In this context, recent scandals in which junior scientists have been caught tampering with data (W. Broad and N. Wade, *Betrayers of Truth*, New York, 1982, and B. Savan, *Science and Deception*, CBC *Ideas*, 1982) are less than surprising.

This inequity in many research labs is perpetuated by a funding system that frequently favours a small elite of established scientists. As an example of the operation of one such elite, I investigated the membership of the Medical Research Council of Canada (MRC) grant-award committees during the period from 1971 to 1981. Modern medical research is expensive, and, given the average rejection rate of about 70 new applications out of every 100 proposed, the people on the committees deciding who gets support, and who doesn't, wield considerable power. I found that the group of scientists on these committees is rather small – approximately six to eleven scientists on each of nineteen committees, with a usual turnover of two or three members per committee year. The couple of hundred committee members represent only a tiny fraction of the 1,600 or so researchers funded each year, out of approximately 4,000 applicants. Moreover, several individuals served on more than one committee over the ten-year period, or served on the same committee for more than one term. I also looked at the editorial boards of several Canadian medical-research journals listed in *Index Medicus* – nearly every editorial board included scientists who were or had recently been on an MRC grants committee.

Having established that a relatively small number of research scientists controlled the funding and publication decisions in their fields, I looked into who and what proposals were getting support. Not surprisingly, I found that the committee members themselves do very well by the MRC. The average MRC grant in 1979-1980 was $34,041. The average grant to a grant-committee member during that same year was $52,376 – half again more than the average. Furthermore, in the same year, all but one of the recipients of the ten largest MRC grants were on grants committees themselves during the period from 1971 to 1981. Seventy-three per cent of all grants over $100,000 and 65 per cent of all grants over $75,000 in 1979-1980 were awarded to scientists on an MRC grant committee during the period from 1971 to 1981.

So perhaps not surprisingly, the projects given the most generous support are proposed by those making the grant decisions themselves. It can be argued that the best scientists propose the most worthwhile projects and therefore get the most support, and that they are also the most experienced and far-sighted individuals to recruit for the grant committees and journal editorial boards. But it can equally well be argued that the process of peer review which the funding agencies and publications depend on is inherently conservative – it supports the established, the familiar, the well-known ideas and people. Those who adhere to the prevailing dogma and work in established fields with respected scientists are much more likely to get funded and published. As they gain a reputation, they get a disproportionate share of the research pie, making it more difficult for the less well-known researchers with unorthodox ideas to get support.

This systematic bias has some rather alarming implications. Daring, original research on new theories is probably starved for support or refused publication, while safe, orthodox research which will reliably produce predictable results is encouraged. In this way established fields, theories, and even values in science are systematically favoured. A striking example of this is the predominant use of male-only subjects in behavioural research, the results of which are frequently generalized to apply to women as well (C. Stark-Adamec, *Sex Roles*, Montreal, 1980). Despite the disproportionate numbers of women receiving psychiatric care, only a very small fraction of the Canadian grants awarded for mental-health research went to projects directly related to female problems.

In this way the direct vested interests of academic scientists operate to promote certain fields, theories, and members of established research labs. In addition, the extreme risks and potential rewards of a junior career in science, combined with the hierarchical structure of many large labs, permits and may even encourage irresponsible reporting of findings or, on occasion, outright fraud.

The government is the second vested interest which influences scientific research. There are often direct political rewards for certain findings. The government is one of the largest supporters of scientific research, and a scientist pushing politically embarrassing results is unlikely to find favour in high places. Two studies conducted in 1978 to determine the health effects of seepage of toxics from Hooker Chemical at Love Canal exemplify the influence of political vested interests on research findings.

Beverly Paigen, a molecular biologist at the Roswell Park Memorial Institute in Buffalo, New York, surveyed 850 families to determine whether their health problems related to their location in the community. The study was conducted on a shoestring and wasn't academically unassailable. She did, however, find a significant threefold increase in miscarriages in residents of homes on the historically "wet" areas, where streams, swamps, and ponds used to exist. Rubble had been filled in in these depressions, forming a route by which toxics could be transported underground. She also found an increase in birth defects and a significant increase in admissions to mental hospital in "wet" house residents. In a separate study, carried out by the State Health Department, the opposite claim, that the chemicals hadn't migrated to local homes, was made. In its study, the Health Department analyzed whether the miscarriage rate decreased for residents farther away from the canal, and since it didn't, it concluded that miscarriages weren't related to chemical exposure. While Dr. Paigen's study counted all miscarriages reported by the women themselves, the Health Department only counted those in Love Canal when a doctor had independently confirmed them. These two studies clearly asked the same question in different ways and got different answers as a result.

The opportunity to influence research results by choosing convenient hypotheses and methods, combined with the inadequate or ambiguous data which are usually available, encourages scientists to impose (often unknowingly) their own views on their data. Especially when strong vested interests exist which make certain conclusions more politically attractive, it's easy to see how these interests colour scientific research. Often several camps of scientists emerge with different views on an issue, each using different data to support its views. Thus the spectacle of scientists battling it out publicly in courtroom or media debates has become more common.

Science supporting industrial or corporate interests provides even more blatant examples of a powerful vested interest influencing scientific conduct (N. Hildyard, *Cover Up*, London, 1981). One of the most serious examples of corporate fraud to be uncovered recently is the case of Industrial Biotest Ltd. (IBT), a commercial biological testing lab, based in the United States. This firm is one of several that was contracted by chemical manufacturers to test their products to find out if they posed any potential health hazards. On the basis of these tests, the products were then registered to permit their sale in the U.S. Canada relied on these tests too and, by and large, permits use of pesticides which have been registered south of the border.

In the case of IBT, U.S. investigators found that many of the tests on over 100 chemicals, mainly pesticides, had been fabricated or grossly distorted. In some cases sick test animals were replaced with healthy ones, and in others serious problems like high numbers of fetal deaths, miscarriages, and numerous cancerous tumours were not reported. As a result of these unduly optimistic results, provided by IBT, nearly 100 pesticides are now in use in the United States and around the world, since few governments are inclined to duplicate the expensive battery of tests. Captan, possibly one of the most serious of the pesticides that was registered on the basis of the suspect experiments, is used as a fungicide here in Canada. It has been found to cause cancer, birth defects, and genetic damage in animals, and Health and Welfare Canada has recommended restrictions on its use.

The problems associated with the tests on these pesticides surfaced back in 1977. The U.S. government, assisted by the Canadian Health and Welfare Department, has been painstakingly reviewing about 1,600 IBT tests, to determine whether they are valid or not. As a result, about one-third of the chemicals have been cleared, but most of the remaining suspect ones are still on the market and in use. Unfortunately, this incident is not isolated. The U.S. Environmental Protection Agency is investigating over thirty labs for sloppy or deliberately careless work. Tens of millions of dollars and many years of careful research will be needed to double-check on the thousands of suspect tests. And in the meantime, what will the cost be in terms of human health as we continue to use these pesticides?

All the above examples illustrate the corrupting influence that comes with the direct dependence of scientists on their benefactors, be they other scientists, government, or industry.

To liberate science from these pressures, I believe that several changes are necessary. A deliberate effort should be made to disengage those collecting the data from those with a vested interest in the results. For academic researchers, the lab hierarchy could be undermined by awarding more, smaller grants to the individual researchers who do the work, not the lab chiefs. Government and industrial science should be provided with secure, long-term funding by a body which is not answerable to hose needing the research results. Other reforms, like limiting co-authorship of research papers to those with an active involvement in the work and including more lay people on the grant-awarding committees, would also help to democratize the research community. Women entering science must actively contribute to these reforms, to help establish a research community in which they can participate freely and equally.

Further Reading:

Jane Goodfield, *An Imagined World*. Markham, Ontario: Penguin Books, 1981.

Adeline Levine, *Love Canal: Science, Politics, and People*. Lexington, Massachusetts: Lexington Books, 1982.

Beth Savan, *Science and Deception*. CBC *Ideas* series, 1982.

Beth Savan, "A Cautionary Tale," *Proceedings of the First National Conference for Women in Science, Engineering, and Technology*, 1983, pp. 67-71.

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