

Suppression Stories  
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# Introduction

Throughout history, dissidents have often come under attack. They have encountered censorship, harassment, slander, dismissal, banishment, even prison, torture and execution. In liberal democracies today, intellectual freedom is celebrated. Yet it remains dangerous to disagree with conventional wisdom. Inside corporations and government departments, most employees know it is not wise to criticise official policies or the boss—at least not openly. Those who speak out are often victimised. Suppression of dissent is commonplace. Yet this suppression receives little attention.

My aim in this book is not to document the methods or extent of suppression. There is plenty of information already available about that. Rather, my aim is to describe some of the experiences and insights that I've had in more than 15 years of research and action against suppression. In many of the following chapters I draw on my own studies and experiences even though there are others who have a deeper understanding and more extensive experience. I do this because, when I know the case personally, I'm more confident about the insights. It's easy to be seduced by someone else's account of a case in some other country.

The first five chapters deal with the problem of suppression: a detailed case study in chapter 1, a range of illustrative cases in chapter 2, patterns of suppression in chapter 3 and the roles of the law and peer review in chapters 4 and 5. The final five chapters deal with responses to suppression. Chapter 6 argues that official procedures for dealing with suppression seldom work. Chapter 7 treats the complex role of the media. Chapter 8 gives examples of the challenges facing someone trying to publish accounts of suppression. Chapter 9 describes some of the sorts of people who take action against suppression. Finally, chapter 10 summarises what a person under attack can do to respond.

Out of the many cases I've come across over the years, only some are mentioned here, and not much detail is given for any particular one. To give a detailed account of a single suppression case can easily require a book, and there are quite a few books that do this. I've used cases to provide insights about opposing suppression. I hope those who are not mentioned here will realise that this is not due to lack of interest.

Memories can be deceptive. I've relied throughout on my detailed written records and previously published accounts.

In studying suppression, I have accumulated more than the usual number of intellectual debts. I thank all those who have contributed information, insights and inspiration over the years. Mark Diesendorf, Peter Drahos, Don Eldridge, Isla MacGregor, Wes Shrum and Wendy Varney gave helpful comments on the entire manuscript. For comments on individual chapters or points I thank Tim Anderson, Eric Bachelard, Ann Baker, Penelope Canan, Tom Curtis, Bill De Maria, Tim Doyle, Jeremy Evans, Ned Groth, Carolyn Hayes, Ed Herman, David Hess, Bernadette Hince, John Hookey, Ian Hughes, Jo Kamminga, Jean Lennane, Clyde Manwell, Brian O'Brien, Louis Pascal, Mel Reuber, Alan Roberts, Dhirenda Sharma, Mike Spautz and Richard Sylvan. Sharon Beder provided valuable technical advice.

# 1

## A tenure battle

In 1976 I moved to Canberra to take up a job at the Australian National University. One of the people I met there was Jeremy Evans. Little did I imagine that I would be helping to campaign for his tenure only a few years later. This campaign was my first real introduction to suppression issues.

After completing my PhD in theoretical physics at the University of Sydney and spending a year mostly unemployed, I was lucky to obtain a job as a research assistant in the Centre for Resource and Environmental Studies at the Australian National University (ANU) in Canberra. I had developed a strong interest in environmental issues and also in social alternatives, so on arriving in Canberra I immediately began asking around to find interesting people to meet.

The ANU, though a relatively new university at the time, was dominated by orthodox perspectives. There were only a few pockets of exciting innovation. One of them was the Human Sciences Program. It was not long before I became a regular visitor to the members of the Program.

The Human Sciences Program was an undergraduate teaching programme. It might simplistically be called environmental studies, but there was a strong emphasis on the human side of the picture, both the dynamics of society and the dynamics of the psyche. Students in the Program took one or both of a sequence of two full-year courses, both taught by the small staff: a second-year course called Human Ecology and a third-year course called Human Adaptability. Students took Human Sciences as a small component of an otherwise orthodox science or arts degree with a major in a conventional discipline.

The Human Sciences staff was indeed small. There was Jeremy Evans, senior lecturer and head of the Program; Ian Hughes, lecturer; Val Brown, tutor; and Rosemary Brissenden, tutor. This

tiny unit was able, though, to upset a lot of powerful people on campus.

The Program dealt with current social issues, such as environmental degradation and ways to respond to it, including social and personal change. This may not sound like anything special from the perspective of the 1990s, when environmental issues are everyday stories and even conservative politicians voice their concerns. But in the 1970s, the environment was still a relatively new and radical issue.

However, what made the Human Sciences Program really stand out from the crowd was its commitment to interdisciplinary study. It attempted to bring together approaches from a range of fields, including sciences such as zoology and geography and social sciences such as sociology, anthropology and psychology. Whether you call this interdisciplinarity, multidisciplinary or something else doesn't really matter. The Human Sciences Program was a threat to some traditional academics not so much for what it taught but because of what it was in organisational terms.

To understand why the Program came under attack, I need to discuss how universities are organised. The ANU was set up like most universities, as a series of departments such as philosophy, physics and psychology. The model department could call itself a discipline or at least part of a discipline. Members of a discipline make the intellectual claim of being the only ones with the specialised knowledge to make judgements about scholarship in the field. If disciplinary barriers are high, universities become fragmented, with each department/discipline zealously guarding its boundaries, keeping out interlopers and maintaining the purity of the canon.

There are actually numerous exceptions to the discipline model of universities, such as law and medicine. These are areas of application and necessarily draw on a number of disciplines. But because they are allied to powerful professions, their organisational and intellectual status is seldom questioned.

However, when there is no powerful outside group to support a field, it has a more difficult time. Women's studies and peace studies are two good examples. Human Sciences had the same problem. The main outside group to which it might appeal was

the environmental movement. Given the reputation of environmentalism in the 1970s as a radical fringe, this was hardly the basis for gaining intellectual respect in a hide-bound university.

My discussions with staff in Human Sciences helped me gain an understanding of why they encountered hostility from some powerful members of the ANU. Val Brown and I had many stimulating conversations. I had my own radical and largely untested ideas about education. For her PhD, Val was doing something more practical, namely studying the Human Sciences Program itself as a form of interdisciplinary education.

In retrospect, it is amazing that the Program was set up at all. The key driving force behind it was Stephen Boyden, a researcher working in the John Curtin School of Medical Research at ANU. I knew Stephen because he had moved to the new Centre for Resource and Environmental Studies to head up the Human Ecology group. His efforts in the late 1960s and early 1970s, with support from many others, led to establishment of the Human Sciences Program. But there was serious opposition from some figures in traditional departments, or so I was told. There wasn't much written evidence, since the hostility came out in committees and discussions in the years it took to establish the Program. Finances were tight in the university. The Program was a juicy morsel. It attracted many students who might otherwise study in some traditional department.

### **Jeremy's tenure is denied**

In 1979 Jeremy Evans, senior lecturer in the Human Sciences Program, came up for tenure. At that stage all of the academic staff in the Program were untenured. The reappointments committee recommended against Jeremy's tenure. As word of the decision spread, students and friends of the Program were shocked and outraged. It was not just a personal setback for Jeremy; it was an attack on the Program.

In Australian universities at that time, just about everyone who held a potentially tenurable post and applied for tenure was successful. The more difficult part was getting a tenurable post. Jeremy had been appointed to a tenurable senior lectureship. After the usual three years of probation, he put in an application for tenure.

*If* he had published nothing at all *and* been a terrible teacher *and* been an unpleasant colleague, then it was just possible that tenure would have been denied. But Jeremy had a modest though not meagre publication record, got on reasonably well with his colleagues, and was highly acclaimed as a teacher. The teaching was the key in this case, since Human Sciences was a new teaching operation and involved a very heavy load of both educational innovation and face-to-face teaching.

Jeremy and others believe that an important factor in the decision to deny him tenure was his introduction of “experiential” sessions in his course Human Adaptability in 1976, including the occasional guest lecturer who advocated revelation as a means for seeking the truth. Several members of the Program’s supervisory committee, including Frank Fenner and Stephen Boyden, advised against this, but Jeremy went ahead in the face of their disapproval. His “disobedience” in this regard was never mentioned in any official context but in Jeremy’s view it aroused considerable fury among committee members and almost certainly triggered the decision to deny tenure.

In official terms, denying Jeremy’s tenure was not a threat to the Program. He would lose his job, to be sure, but it could then be advertised and offered to someone else. In practical terms, though, students and supporters of the Program came to see the tenure denial as a direct attack. Jeremy was one of the founders of the Program: to deny him tenure was to deny his contribution. In addition, denying Jeremy tenure was in effect to say that his research and teaching in an interdisciplinary area were not sufficiently “scholarly” to merit inclusion in the ANU. This was in effect a comment on everything the Program was attempting to do.

Jeremy Evans was born in 1937 in Hobart. He attended the University of Sydney — one of Australia’s most prestigious universities — where he obtained first class honours in zoology. He went on to get a PhD in biology from Harvard University. He joined the Zoology Department at the University of Melbourne as a lecturer. Then, in 1969, he voluntarily took a step down in rank and pay to work as a research assistant with Stephen Boyden in the Urban Biology Group in the John Curtin School of Medical Research at ANU. When the Human Sciences Program

got under way in 1973, he became a lecturing fellow, and then in 1976 a senior lecturer and head of the Program.

Jeremy thus broke out of his traditional disciplinary background and championed interdisciplinary studies — environmental studies, very broadly interpreted — when universities were only just coming to terms with these issues. Jeremy was primarily oriented to the intellectual endeavour of interdisciplinary exploration. Although he actively supported a number of community initiatives such as foundation of a local Society for Social Responsibility in Science, Jeremy was not prominent as a social activist or public commentator. Thus, he was not denied tenure because of his radical politics or activism.

It's taken me quite a bit of explanation here to tell why people were upset about the denial of Jeremy's tenure. The tenure decision was justified by the committee on traditional grounds of lack of sufficient academic merit. To oppose this assessment meant having a critical understanding of the dynamics of the university. As in most cases, the issues were complex. Indeed, what I've described here gives only a hint of the complexities of the case. I haven't gone into personalities, power plays or the wider dynamics of environmental politics.

### **The campaign**

Following the reappointment committee's rejection of tenure for Jeremy, there was a big campaign to push for his tenure and to defend the Human Sciences Program. The campaign was very effective. Here I tell about some aspects of the campaign without pretending to give a full history.

The first and essential requirement for this campaign was that Jeremy be willing to fight the decision. He was. It sounds easy to say, "I'll fight it," but actually it's not all that common. The first response most people have when they come under attack in this way is to blame themselves and to hide their shame.

Tenure committees meet at universities regularly and deal with case after case. Usually there is no controversy. In the Australian system, if someone is likely to be denied tenure, often they will be told quietly before they apply, so that they can seek another job and not be embarrassed by rejection. When there is a formal rejection of tenure, it is presented as entirely a question of

academic merit. Challenging these decisions is not easy. When a panel of experienced academics pronounces that someone is not worthy of tenure, it is difficult indeed for the rejected applicant to turn around and contest the decision.

One reason Jeremy was willing to fight was because there had been continual discussions in the Program about the forces for and against it. Jeremy and his supporters had a framework for explaining the rejection in terms of the politics of the university. In this framework, academic “merit” was not an objective criterion. Rather, it was influenced, indeed constructed by the reappointments committee and, in this case, used to devalue the sort of teaching and research being undertaken in Human Sciences.

University regulations allowed Jeremy to request his tenure rejection to be reassessed by a review committee. So he prepared a comprehensive application to the committee. The review committee reaffirmed the rejection of tenure. Jeremy persisted by going to an appeal committee, which could look only at procedural anomalies in the review committee’s deliberations. Jeremy prepared an even more impressive submission. It included a critique of the review committee’s procedures, an account of the special difficulties of programmes such as Human Sciences, an account of the performance of his administrative duties, his teaching and research, and a series of appendices, including letters of support from students and academics.

One of the grounds Jeremy used for appeal was that the chair of the review committee, Ted Chapman from the Geography Department, added his signature to a letter to the Vice-Chancellor from members of the Geography Department suggesting that the Human Sciences Program be amalgamated with Geography. Chapman was also alleged to have said in conversation with undergraduate students that this amalgamation would lead to termination of all but one of the positions in Human Sciences and termination of the course Human Adaptability, taught by Jeremy. If evidence was needed of a link between Jeremy’s tenure and the survival of Human Sciences, this was it.

Pursuing justice through formal university channels is a risky business at the best of times. Why would a panel of academics overturn a decision made by their esteemed colleagues? This is where the campaign came in. It was an attempt to demonstrate

the value of Human Sciences and to challenge attacks on it through denial of tenure. Public campaigns are uncommon in cases like this.

Some time after the campaign got under way, Jeremy was contacted by John Hookey, who had previously worked at the ANU, in the Law Faculty. In the early 1970s, Hookey had quickly made his mark. He introduced the first course in environmental and resource law at any Australian university. He developed a high profile in supporting land rights for native peoples, including writing a critique of a prominent judge's decision in relation to Aboriginal land rights and appearing in the High Court as junior counsel in a Papuan land rights case. He thought that everything was going fine.

Then one day Hookey found a note on his desk from the dean of the Law Faculty, telling him that he was unlikely to be recommended for tenure. He was stunned. He quickly took steps to challenge this decision, using internal university procedures. The bitter struggle over his case divided the Law Faculty. Before the issue was formally resolved, he was offered and accepted a high-paying and prestigious job as an environmental hearings commissioner in the Australian public service.

There were a number of similarities between Hookey and Evans. Each of them had undertaken innovative teaching in the environmental area. Each of them had a respectable research and teaching record. And each of them was threatened with denial of tenure. Their cases differed in the public visibility and duration of their struggles against denial of tenure. Hookey and his supporters did not seek media coverage or support from students. Also, the matter was defused when he took another job. By contrast, the campaign for Jeremy's tenure became a public issue.

I use the word "campaign" but don't get the wrong impression. There was no secretariat running a well-funded and well-organised operation. There were meetings of concerned individuals and some degree of organisation among three separate groups: the members of Human Sciences, academic supporters, and student supporters (including former students).

The first and most vital part of the operation was getting accurate information out to key people. Packets of documents were given to quite a number of people. I did my bit by sending information about Jeremy's case to various people. Some supporters took action by quiet lobbying, either talking to others or writing letters to university officials. Others made the issue public, especially by writing letters to the *Canberra Times*, the only daily newspaper in the city and fortunately one recognised at the time for its high quality. The first publication occurred on 5 August 1979. A letter to the editor from R. M. Aitken was published, expressing concern about the possible termination of the Human Sciences Program due to budget cuts. Also in the same issue was a prominent article entitled "'ANU irrelevant' if innovative courses cut." In the opening paragraph, it quoted Fred Emery, a high-profile ANU academic from the Centre for Continuing Education, saying that "The ANU would become irrelevant to the requirements of society if it continued its 'gut reaction' to expenditure by axing innovative courses." Two ANU administrators replied in the *Canberra Times* a few days later, and this led to further letters and comment in the newspaper.

A few letters to the newspaper may not sound like much, but it is a major operation. Most academics are reluctant to become involved in public controversy. Even more than this, they are reluctant to openly question their own university administration, out of both loyalty and self-protection. Students and outsiders have less to lose by joining the debate, but they are also likely to feel insecure when it comes to challenging academic "experts." For anyone at all to speak out or write a letter to the *Canberra Times* was quite something. Jeremy's organised supporters cultivated every possible letter writer.

Even a few letters to the newspaper can have a considerable impact on a university administration. Like most bureaucracies, university administrations loathe bad publicity. They were caught in a dilemma. Should they ignore the complaints and leave them unanswered, or respond and prolong the debate?

I became heavily involved in another initiative, a petition. A group of us got together and drafted a mild statement, something that would not be all that difficult for academics to sign. Here is the statement:

We the undersigned urge the Australian National University to reconsider the issue of Dr Jeremy Evans' tenured appointment, taking into account the special requirements of problem oriented teaching and research and the uncertainties surrounding the future of the Human Sciences Program.

We had spaces for people to print their names, list their positions and institutions, and sign. Richard Barz of South Asian and Buddhist Studies agreed to be the return point for petitions.

Most people on campus didn't know much about the issues behind Jeremy's tenure. Therefore, we produced a background statement with the "facts" about the case. Drafting this document was a challenge. We had to be absolutely accurate, since even the slightest mistake could be used to discredit the case being made. The document had to be clear and persuasive for academics who knew nothing about Human Sciences or Jeremy.

In order to give the document added credibility, we sought a list of signatories who would show the breadth of support for Jeremy's case. Various names were canvassed and various people were approached. Each person listed had to agree to the final text. I spent a lot of time making minor revisions and then checking these with all the signatories. We decided that it would be better for me not to be one of the signatories, since I had a relatively junior position (and therefore less credibility among academics) and also was known for my radical views, especially through my letters against uranium mining in the *Canberra Times*.

The background document is reproduced below. It is worth studying because I believe it is a good model for others to follow.

#### Background to the Issue of Dr Jeremy Evans' Tenure

Dr Jeremy Evans, Senior Lecturer in the Human Sciences Program at the Australian National University, is the first person to be denied tenure under the full review procedures approved by University Council in 1974.

The Human Sciences Program comprises a group of four academics who are responsible for two innovative, problem oriented courses in the School of General Studies, based on the multidisciplinary study of human interactions with the environment. It has also graduated four Honours and two Ph.D. students. The Program has attracted both praise and controversy within the University since its inception in 1973. Its place in the University curriculum has been vindicated in terms of both content and

standards by several evaluations<sup>1,2,3,4</sup> and it enjoys strong student support.<sup>3</sup>

The ostensible primary reason for denying Dr Evans' tenure is inadequate research output. As a Lecturer in Zoology at the University of Melbourne in 1966-68 Dr Evans published seven research papers. During 1969-72 he worked as a Research Assistant in the Urban Biology Group, John Curtin School of Medical Research, ANU, took courses in sociology and psychology and co-edited a book. Since joining the Human Sciences Program in 1973 he has devoted the major part of his time to administration, teaching and course design, as would be expected in establishing and developing new multidisciplinary courses. He has nevertheless since then produced nine publications and written a substantial portion of a book. The result of Dr Evans' unusual devotion to teaching is increasing enrolments of enthusiastic students who, along with many of Dr Evans' colleagues, recognise him as a gifted scholar and teacher.

Even if Dr Evans' research performance falls below the norm for the Faculty of Arts in which the Program is located, which seems most unlikely, the decision to deny him tenure appears questionable in view of the special circumstances surrounding it. Indeed, a U.S. Report<sup>5</sup> concludes that problem oriented environmental programmes in universities cannot be expected to succeed if they are subjected to the prevailing form of tenure review. In the light of Dr Evans' experience in Human Sciences and of this Report's conclusions, it seems that the traditional criteria for tenure may be inappropriate to his case.

In addition, the blocking of Dr Evans' tenure has been closely followed by a move by another department to incorporate the Human Sciences Program. This raises questions about the relation of the issue of Dr Evans' tenure to the survival of the Human Sciences Program as a viable operation, since Dr Evans' post is the only tenurable position in the Program.

On the basis of this and other information, we recommend that university staff and others involved in tertiary level teaching and research add their signatures to the attached statement.

1. Ward, R. Gerard, *Report on the HSP (2977/1974 3.9.74)*.

2. Brown, V. A., 1978, *Holism and the University Curriculum: Promise or Performance*, Vols. 1 & 2, Ph.D. Thesis, ANU.

3. Questionnaire responses in Human Ecology and Human Adaptability, 1976-78.

4. Miller, Allen H. and Ann Porteus, *Student Involvement in Learning*, in preparation.

5. Steinhart, John S. and Stacie Cherniak, 1969, *The universities and environmental quality*, A Report to the President's Environmental Quality Council. Washington DC, Office of Science and Technology, Executive Office of the President.

\* Dr Richard Barz, Senior Lecturer, South Asian and Buddhist Studies, SGS, ANU

Mrs Rosemary Brissenden, Senior Tutor, Human Sciences Program, SGS, ANU

Dr R.K. Darroch, Lecturer, Psychology, SGS, ANU

Dr Ken Gardiner, Senior Lecturer, Asian Civilizations, SGS, ANU  
Dr Ian M. Hughes, Lecturing Fellow, Human Sciences Program,  
SGS, ANU

\* Dr Hugh Saddler, Research Fellow, Centre for Resource and  
Environmental Studies, ANU

Mr F.W. Shawcross, Senior Lecturer, Prehistory and Anthropology,  
SGS, ANU

Dr M.J. Weidemann, Senior Lecturer, Biochemistry, SGS, ANU

\* Contact for further information.

Drafting and printing the petition and the background statement was only the first step. Next it was necessary to get people to sign! I knew a few people who were sympathetic to Jeremy. It was easy to get their signatures. I then got up my courage and took the petition around to my colleagues in the Departments of Pure and Applied Mathematics, where I had been working since 1977.

I think there are two main reasons why doing this was hard. First, having been in the Department of Applied Mathematics for a few years, my relationships with other staff had settled into a standard pattern. To ask someone to sign a petition was to go outside the usual expectations. Would I offend them by asking them to consider the petition? This worry sounds almost silly, but it can be a strong inhibiting force against any behaviour out of the ordinary.

Second, most of the academics in mathematics were not very outgoing or friendly. Perhaps that's why they were attracted to mathematics.

In any case, I shouldn't have worried. Nearly every mathematician I approached signed the statement. Only two declined. One of the two was known as an eccentric, so his choice on this could not be predicted. The other said he had no respect for Jeremy.

I started by approaching colleagues I knew well and who were more likely to sign. Then when I approached others, there were already several signatures on the petition. This created a sort of bandwagon effect. After getting a good response with mathematicians, I felt able to approach people I didn't know. I went to the nearby Philosophy Department and obtained quite a few signatures. Then I tackled the Geography Department. Since many geographers had signed a letter to the Vice-Chancellor suggesting that the Human Sciences Program be taken over by

Geography, I didn't expect to get many signatures but thought it would be good to confront the people concerned. As it turned out, only the department's three cartographers signed.

With this sort of response, we could have obtained support from nearly every academic on campus. The difficulty was finding people to collect the signatures. A number of others, like me, had success by approaching colleagues. But we didn't have all that many supporters willing to do this. After all, I had been heavily involved in getting the petition going and still had to get up my courage to approach people.

Nevertheless, we obtained over 200 signatures, with about 160 of these from ANU. There were some 1000 academics at the ANU, not to mention other staff such as research assistants. Nevertheless, the number of signatures was impressive. It's generally very difficult to obtain support from academics on anything except their salaries and parking places.

The petition was presented to the Vice-Chancellor in November 1979, and there was some accompanying publicity in the media. But the value of the petition was far greater than its impact on university officials or the public. Over 200 people signed the statement and even more read the background document. The petition solidified the commitment of the key people involved, especially some of those whose names were at the bottom of the background document and those who had collected signatures. Finally, the petition project provided a valuable focus for organising support.

Meanwhile, students supporting Human Sciences organised their own petition. They had less trouble gaining support. The main problem was tracking down former students.

During all this activity, Jeremy took a wholly appropriate role. He realised that it would not be for the best if he were an active partisan in organising activities, since he might be perceived only as serving his own career interests. So he clearly stated at an early meeting that he would not be an active participant in initiatives such as the petition but would be available to offer suggestions. That is precisely what happened. Those of us who prepared the petition sought Jeremy's advice concerning details in the background document, but we organised everything independently of him.

Jeremy's role was possible because there was such a depth of support for both him and Human Sciences. Others do not have this luxury and must play a more direct role in any campaign.

The public side of the campaign for Jeremy's tenure and the defence of Human Sciences was straightforward: lobbying, letters, petitions. Behind this was lots and lots of discussion and networking. The real complexity of the case arose with the university's official procedures at the time, which were slow and tortuous. While the public campaign boiled during 1979, decisions by university committees about tenure and budgets proceeded at the usual snail's pace.

To cut a long story short, the four members of the appeal committee disagreed about Jeremy's case. After long negotiations, Jeremy accepted the administration's offer of a two-year extension of his appointment, after which he would go for tenure in the usual way. This compromise effectively dampened down public activity on the case. Jeremy buckled down to do more research and obtained tenure when the time came. No one in the administration ever admitted publicly that the campaign had made any difference, but of course it was the crucial factor.

The problems facing Human Sciences did not end with Jeremy's tenure. There were further threats to the survival of the Program — indeed, this was almost an annual event. Even more seriously, there were increasing tensions among the staff, a common occurrence in any academic unit and especially in interdisciplinary units that are critical of the status quo. Jeremy eventually moved to the Psychology Department and then to Biochemistry and Molecular Biology, where he continues to teach in the Human Sciences tradition. The rest of the Program was eventually incorporated into the Geography Department, where its survival as a source of critical, innovative approaches to problems of society and the environment remains precarious. Along the way there have been continued budgetary problems, student agitation, attacks on and defences of the Program.

The renamed Human Ecology Program in Geography as well as Jeremy's courses now attract more students than ever. Their greatest protection against cutbacks comes from high enrolments and the willingness of students to vocally support them.

### **Conclusion**

In spite of all the difficulties and eventual division of the Human Sciences Program, there are some valuable lessons to be learned from the campaign, which was one of the most effective I've seen.

The foundation for the campaign was a sound social analysis of the situation, in this case of the dynamics of the university and especially the forces both supporting and opposing Human Sciences. This analysis was developed over the years spent in setting up and running Human Sciences. When Jeremy's tenure was denied, lots of people believed they knew what was behind the decision.

Human Sciences had a great number of supporters: students, former students, academics from different parts of the university, and outsiders. They were a crucial resource. The support had been built up over the years through good teaching and outreach.

The campaign was built on a core of people who were willing to take action. This included, most of all, the Human Sciences academics, and also groups of students and other academics.

The campaign was very careful in its claims. Every fact was checked, as in the case of the background document for the staff petition.

The campaign took the case to wider audiences rather than just going through official channels. Letters to the newspaper and the petitions brought the issue to a wider public.

Throughout all of this, there was a clear set of aims and demands: grant Jeremy tenure and ensure the funding and survival of the Human Sciences Program.

The campaign was not perfect, but it was pretty good. It certainly taught me a lot. It also primed me for the investigation of intellectual suppression.

## 2

### Suppression: it's everywhere

The struggle over Human Sciences helped me to see a pattern. I had read about other cases in which environmental researchers or teachers had come under attack. The denial of Jeremy's tenure was part of a larger picture of "suppression of environmental scholarship." It all seems obvious to me now but at the time it struck me like a revelation.

In June 1978, Richard Dunford, then doing a PhD at ANU, gave me a 1976 article from the Australian journal *Arena*. It was by Peter Springell and called "For the freedom to comment by scientists." Springell had worked as a scientist for the Commonwealth Scientific and Industrial Research Organization, the large Australian government research body commonly known as CSIRO. In the early 1970s, while working at the CSIRO Division of Animal Genetics in Rockhampton, Queensland, Springell began to be involved with environmental research. He encountered quite a few obstacles, some of them amazingly unfair.

CSIRO has an internal review system for publications: they are screened within the organisation before they can be sent to journals. Springell wrote some environmental research papers on topics such as beef production and lead in petrol. He was told that he could not submit them as an employee of CSIRO. However, he was allowed to submit them using his home address. From the point of view of editors and readers, a home address rather than an institutional address lowers one's credibility. Springell found out that the chief of his division, J. M. Rendel, who objected to Springell's papers going out under CSIRO auspices, had actually published a paper on "consciousness" — a topic having nothing to do with division's work — using his CSIRO affiliation. Springell did not endear himself to Rendel when he pointed out the hypocrisy involved.

Springell was not one to suffer quietly. He took the issue of the treatment of his environmental work to senior CSIRO officials and then to politicians. He complained publicly about the lack of environmental research in CSIRO. His dissent was met with hostility. Attempts were made to dismiss him for inefficiency, but since Springell published more research than most of his colleagues, these attempts failed. Then he was transferred from Queensland to Melbourne. Springell decided this move was political rather than scientific and refused to go. CSIRO officials then began dismissal proceedings. Springell resisted them. Various tricks were used against him. Eventually he decided to leave and take another job.

Springell's article in *Arena* told his story briefly and effectively. It contained a host of references backing up his claims. His story had received national publicity. It was essentially the story of a scientist who was harassed by his employer because he pursued environmental research and refused to shut up about it.

In February 1978 I received a letter from Clyde Manwell, Professor of Zoology at the University of Adelaide. He had read a recent article of mine in the *Ecologist* magazine and felt considerable affinity with my views. After some letters and phone conversations, he invited me to Adelaide to give a seminar. In October that year I made the trip and gave a talk on "Environmental studies and politics." As a result of this contact, I found out about Manwell's experiences. It is one of the most astounding stories I've encountered.

Manwell moved to Australia from England in 1970 to take up the second chair of Zoology at the University of Adelaide. He introduced environmental issues in his teaching and pursued some research with environmental themes. Then in 1971 Manwell and his wife Ann Baker wrote a letter to the local newspaper, the *Adelaide Advertiser*. They actually wrote it from their home address, but the newspaper, knowing Manwell's position, added his university affiliation. Their letter was a criticism of some aspects of the South Australian government's programme for spraying pesticides against fruit fly. Note that their letter was not a broadside: it only criticised *some aspects* of the fruit-fly spraying programme.

The response to this letter was immediate and dramatic. Several politicians denounced Manwell in the South Australian parliament. Back at the university, the senior professor of Zoology, H. G. Andrewartha — the only other full professor in the department — wrote a letter to the Vice-Chancellor making a number of complaints about Manwell's performance. Some of these were ludicrous, such as a charge that there were four errors in statistics in Manwell and Baker's book on evolution. (It is well documented that errors in statistics are rife in published research. As it turned out, only one of the four alleged errors was actually wrong, and it didn't affect the conclusion.)

The Vice-Chancellor took Andrewartha's letter seriously, and launched proceedings that could have led to Manwell's dismissal from his tenured post. This was the beginning of a four-year struggle for Manwell, in which he defended himself against the charges and against harassment within the Department of Zoology. There was also a lot of support for Manwell, from some colleagues and especially from students, who even on one occasion occupied the Council Chamber in his defence. Eventually in 1975 the charges were dropped.

What was behind all this? One factor was hostility to environmentalism which, in the early 1970s, was seen as a dangerous challenge to prevailing practices. It was also noted by a number of people that H. G. Andrewartha, who made the complaint against Manwell, had strong links with the South Australian Department of Agriculture and its fruit-fly spraying programme.

In the case of Peter Springell, my information was based on a brief but well-referenced article. On Clyde Manwell's case I had much more. There were quite a few documents, including a statement written by the Vice-Chancellor that was published as part of the settlement of the case, articles in the University of Adelaide student newspaper *On Dit*, and various unpublished internal documents. In addition, Clyde told me a lot of things that had never been written down.

At this stage I knew a lot about a few cases: Jeremy Evans, Peter Springell and Clyde Manwell. There was also the case of John Hookey, who had informed Jeremy about his expected denial of tenure in the ANU Law Faculty. In each of these cases, a person had undertaken environmental research or teaching, or

spoken out about environmental issues, in a way that threatened powerful vested interests. In each case they had come under attack as a result.

The Evans, Springell and Manwell cases were prominent. There was a lot of media coverage and activity and lots of publicly available information. The Hookey case was low key and, except for the Evans case, might never have come to my attention. Obviously there weren't all that many high-profile cases, otherwise we would read about them every day. My suspicion was that the high-profile cases were the tip of an iceberg of suppression, and that cases like John Hookey's were more typical. Several things led me to think this way.

Cases where there is a direct attack on people — denying them tenure or threatening to dismiss them — are easy to document. There are procedures for tenure and dismissal. Therefore decisions can be contested, information can be generated and media stories produced. But in many situations there is no easy way to provide documentation. For example, what if there is a bias against environmental research by the editor of a journal? This could even be unconscious bias, as in an assessment that environmental arguments are less scientific than other sorts of arguments. In any event, environmental articles might be rejected where articles on other topics, of similar calibre, are more easily accepted. This could be called suppression of environmental scholarship. But it would be virtually impossible to document.

A similar process occurs in job applications. Often there are quite a few applicants who are good enough to be appointed. The selection is made by a few people and usually no public justification is required for the elimination of certain applicants. Bias — against women, ethnic minorities, political activists, etc. — is quite possible. It is also very difficult to document.

These sorts of abstract arguments make it plausible that prominent cases are the tip of the iceberg. But what really convinced me was something else: Clyde Manwell's experiences and my own.

Clyde Manwell's case received enormous attention in Adelaide over several years. As a result, Clyde received many letters from individuals who wanted to tell him about their own experiences in being attacked for their views. I have no way to assess this

information directly, as I've seen only a few of the letters — Clyde took requests for confidentiality seriously. But I saw enough and heard enough to convince me that it was entirely plausible that for every case like Clyde's, there were tens or hundreds of other cases of suppression which never received any publicity.

### **My experiences at CRES**

So far I may have given the impression that I was a disinterested observer of other people's struggles. But I had my own experiences to draw on. These had primed me to conceptualise the phenomenon of suppression.

When I first arrived at the Centre for Resource and Environmental Studies — CRES for short — at the beginning of 1976, I realised that my views on both environmental and social issues were much more radical than those of my superiors. I was a research assistant — a fairly junior position — in the Applied Systems Analysis group, headed by Peter Young. My view was that environmental problems persisted largely because of the dominance of powerful groups in society, especially governments and large corporations. However, the research in Applied Systems Analysis was concerned with technical aspects of pollution. The most likely use for such research was minor tinkering with environmental standards, not a re-examination of the driving forces behind environmental problems.

I decided to keep a relatively low profile in CRES. By my own standards I did keep a low profile, but it wasn't low enough. Outside of work, I joined Friends of the Earth and became active in the campaign against uranium mining. At CRES I sometimes offered comments at seminars, pointing out the social dimensions of environmental issues. Stephen Boyden made the same sort of comments, as well he might, considering that he had been the driving force behind setting up the Human Sciences Program. Stephen's comments were politely received. He was an experienced researcher, a professorial fellow and head of the Human Ecology group at CRES and could not easily be dismissed. My similar comments caused more consternation. After all, I was just a young research assistant.

In addition, I did not respond well to Peter Young's rather authoritarian managerial style. For example, I was working on a book — published several years later as *The Bias of Science* — and sent sample chapters to some publishers. Peter demanded to know what was in my packages. He insisted that either he be allowed to read my writings prior to posting, or that I pay for the postage myself. I chose the second option. There were a number of other tensions between us, some of them relating to our different disciplinary backgrounds, his in control engineering and mine in theoretical physics.

My initial appointment at CRES was a one-year contract, but I was told at the beginning that I could expect at least another year in the job. But towards the end of 1976, I was told by the head of CRES, Professor Frank Fenner, that he and Peter Young had decided that it would be better not to renew my contract.

There was no way I could contest the decision. After all, my contract was only for one year. In addition, there was no easy way to demonstrate any bias. True, another research assistant in Applied Systems Analysis, Tony Jakeman, who was appointed at the same time as me on a similar basis, had his contract extended. But Tony had done more than Peter had wanted, tackling the control theory modelling most effectively. It could be argued that my performance wasn't up to scratch.

Nevertheless, it seemed to me that my environmental activism was involved in some way. Frank Fenner was not an enthusiast of radical environmentalism. According to a friend at the local environment centre, he had been a supporter of the "old guard" at the Australian Conservation Foundation that was ousted by the "radicals" in the 1973 elections. CRES itself reflected a fairly technocratic orientation to environmental issues. The two main groups were Applied Systems Analysis, headed by Peter Young, and Resource Economics, headed by Professor Stuart Harris, a traditional economist. Stephen Boyden's Human Ecology group had been put in CRES as an afterthought, when Stephen sought a new home for his research.

My case, I felt in retrospect, was one that might have involved suppression, but for which there was insufficient evidence to prove much one way or the other. My experience thus primed me

to recognise cases of suppression and also to appreciate that most possible cases are clouded by ambiguity and uncertainty.

As soon as I found out about the nonrenewal of my contract at CRES, I began applying for other jobs. I was lucky to be offered a research assistant position in the Department of Applied Mathematics at ANU. It was also a one-year contract, but this time I was working in a situation where my radical views were less of a threat. CRES dealt with environmental problems but also set itself up as a centre for scholarly research. Radical views and environmental activism were seen as a threat by some of the senior members of CRES. By contrast, radical views and environmental activism were largely irrelevant in the Department of Applied Mathematics. In addition, my new boss, Professor Archie Brown, seemed to hold the old-fashioned view that as long as I did my work satisfactorily, it didn't matter what else I did. So when I had a letter on uranium mining published in the *Canberra Times*, no one commented one way or the other. It was not seen as having anything to do with applied mathematics!

My experiences at CRES undoubtedly made me more receptive to the plight of Human Sciences and willing to take up the case of Jeremy's tenure. There was also another link. In my final days at CRES, I began to write a critique of CRES itself, with sections on the shortcomings of each group within CRES when it came to analysing environmental problems. I obtained comments on drafts from about a dozen people within CRES. This paper was published in the *Ecologist*, a British magazine, in July 1977, and caused quite a stir in CRES and around Canberra at the time. Forthright published comments about an actual programme were not common. Although lots of people knew about my article, no one told the senior members of CRES, who were shocked when it appeared. I was told by one person at CRES that Peter Young — to his credit — wanted to invite me to give a seminar at CRES, so that my views could be challenged. Frank Fenner apparently ruled against this.

Publication of the article cemented my position as a critic of the establishment. I assumed that I could never again get a position at CRES. Undoubtedly this made me more willing to take up the causes of other challengers of orthodoxy. One of the people who

read my *Ecologist* article was Clyde Manwell. As noted earlier, he wrote me as a result, beginning a long interaction.

### **More suppression cases**

During 1979, as the struggle over Human Sciences proceeded, I thought it might be useful to write an article about the difficulties faced by environmental teachers and researchers who threatened the status quo. I decided to base the article around a series of case studies. I had good material about Jeremy Evans, John Hookey, Peter Springell and Clyde Manwell. I began talking over my ideas with several people. My friend Mark Diesendorf told me about a New Zealand environmentalist, Bob Mann, who had come under attack by the administration of the University of Auckland. The Vice-Chancellor initiated dismissal proceedings. As it turned out, Mann's colleagues rallied to his defence and the attack eventually failed. Mark gave me a few documents about Bob Mann's case, which were enough for me to include a relevant entry in my article.

An article with a series of cases is one thing, but I wanted to do more — to develop a framework for understanding the attacks. I drew upon my ongoing studies of the exercise of power in science. I argued that science — both the practice of science and scientific knowledge — is strongly influenced by the dominant groups that fund research and use scientific findings. The other dominant influence is the internal hierarchy within science, in which some elite scientists, such as lab directors and editors of key journals, have enormous power over the direction of research. The outside influences plus the internal hierarchy make up what I called the “power structure of science.”

I had been reading books and articles on the sociology of science for several years. From my point of view, most of this material was quite uncritical. But there were a few treatments of those scientific elites who exercise power, who I called the “political scientific elite.” Much less interesting to me was the study of intellectual authorities in science, who I called the “cognitive scientific elite.”

I also brought in the familiar idea of paradigms in science. A paradigm is essentially a standard way of doing things in a field, including an accepted framework of ideas and usual methods.

Anyone who challenges the dominant paradigm — such as a supporter of an alternative paradigm — is likely to encounter difficulties. But “difficulties” means that someone else is able to exercise power against the challengers. In practice this means the political scientific elite and its patrons in government and industry.

Different case studies illustrated different aspects of my analysis. For example, the attack on Clyde Manwell came directly from a member of the “political scientific elite,” namely H. G. Andrewartha, senior professor of zoology, who carried weight with the Vice-Chancellor. According to Ann Baker, Andrewartha and some of his supporters had links with the South Australian Department of Agriculture which in turn was committed to the use of pesticides produced by chemical companies, illustrating the ties between scientists, government and industry. On the other hand, the struggle over Human Sciences had more to do with its challenge to the standard model of intellectual endeavour in the university. Outside vested interests were not directly implicated.

To bolster my case, I drew upon a range of material. I had been reading quite a few books and articles about attacks on intellectual dissent. Most of this material concerned experiences in the United States, but it was still relevant. There were excellent books documenting the attacks on dissidents during the late 1940s and early 1950s, under so-called “McCarthyism,” notably Cedric Belfrage’s *The American Inquisition 1945-1960* and David Caute’s *The Great Fear: The Anti-Communist Purge under Truman and Eisenhower*. There were also articles about attacks on radical scholars in the late 1960s and 1970s, a phenomenon not nearly so widely recognised as McCarthyism. The more I looked, the more I found evidence that attacks on dissidents are the rule rather than the exception.

More specific to the issue of academic freedom was the work of Lionel S. Lewis, who had studied cases officially brought to the attention of the American Association of University Professors. He found that attacks from outside the university were more common in the first half of the century, but since then attacks from within — namely from university administrations — became more common. This fitted in with my picture of the

key role of the political scientific elite in suppression. Most suppression operated within the organisation; the local elites acted to protect their own power and status, which in many cases was linked to powerful outside interests.

There were lots of relevant ideas and references that I tried to pack into the article. For example, Joseph Haberer in his book *Politics and the Community of Science* documented how most of the German scientific community — especially the scientific elite — had readily cooperated with the Nazis. Haberer introduced the expression “prudential acquiescence” to describe this phenomenon. The current cooperation between the scientific community and dominant political and economic groups was not so very different.

I couldn't resist using a great quote from C. Wright Mills, the famous radical sociologist. Mills wrote “the deepest problem of freedom for teachers is not the occasional ousting of a professor, but a vague general fear — sometimes politely known as ‘discretion,’ ‘good taste,’ or ‘balanced judgment.’ It is a fear which leads to self-intimidation and finally becomes so habitual that the scholar is unaware of it. The real restraints are not so much external prohibitions as control of the insurgents by the agreements of academic gentlemen.” This sort of social control is the usual mechanism; suppression is only used occasionally, to warn people against stepping out of line.

I worked away at my article, checking details with every individual mentioned. By early January 1980 I had completed a draft, which I promptly sent out to a considerable number of people for comment. At that stage it had the poor title “Functions of the scientific elite structure.” Naturally I sent copies to Jeremy Evans, John Hookey, Peter Springell, Clyde Manwell and Bob Mann. I also sent copies to a number of others who I thought would be likely to give me useful comments.

For several years I had been corresponding with Richard and Val Routley, two radical philosophers who were involved with environmental issues, anarchism and social critique generally. We exchanged copies of draft articles and sent each other detailed comments. They lived near Braidwood, not so very far from Canberra, and Richard actually worked at ANU doing full-time

philosophy research, but at that time seldom visited the campus. So we mainly communicated by post.

Val sent me a long letter with lots of insightful comments from their reading of the paper. She also mentioned that some of their own experiences might be relevant. I arranged to meet them within a couple of weeks to obtain more information. As it turned out, their story was another important case.

In the early 1970s, Richard and Val wrote a book entitled *Fight for the Forests*. It was a frontal attack on standard forestry practice and the assumptions underlying it. Richard arranged for it to be published by the Research School of Social Sciences at ANU. However, members of the Forestry Department at ANU obtained word of the impending publication. Apparently as a result, the Vice-Chancellor wrote requesting that the book be shown to the head of the Forestry Department and revised in accordance with any comments he might make. This attempt at censorship failed. *Fight for the Forests* was published in 1973 and two later editions appeared in 1974 and 1975. It was and still remains the best critique of Australian forestry available. Environmentalists and others sought it eagerly. All three editions sold out, but no money was made available for future printings or editions.

Perhaps the most bizarre part of this story was that Richard Routley was barred from using the Forestry Department library on campus for six months in 1974. David Dumaresq, who worked as a research assistant for Richard, used to use the library on Richard's behalf, almost surreptitiously. David later worked in the Human Sciences Program and obtained an additional taste of the treatment of environmental radicals. When the bar on Richard's use of the library was brought to the attention of a new head of the Forestry Department, it was rescinded.

In her letter to me with comments on my draft paper, Val not only mentioned their own experiences with *Fight for the Forests* but also suggested that I contact Peter Rawlinson, a forests activist who worked in the Zoology Department at La Trobe University in Melbourne. I talked to Peter on the phone and in April received from him a long letter and pile of documents. This case actually involved not only Peter but also Philip Keane, a lecturer in the Botany Department at La Trobe.

In January and February 1977, Peter had given radio and television interviews in which he criticised the Forests Commission of Victoria, especially regarding the spread of a tree disease caused by cinnamon fungus. At the time he was the official spokesperson for the Conservation Council of Victoria. The chairman of the Forests Commission, Dr F. R. Moulds, made complaints to senior officials of La Trobe University. A courier was sent to the university to hand-deliver letters of complaint. Eventually 10 letters were delivered. Moulds also complained about Philip Keane, who had written an article about cinnamon fungus in a weekly newspaper, the *National Times*. Moulds suggested that the administration should take action against Rawlinson and Keane.

This story had a happy ending. The Vice-Chancellor defended the academic freedom of Rawlinson and Keane. The staff association also took a strong line against the attack. The Rawlinson and Keane cases were a nice addition to my list of cases. They showed that attacks can be resisted. They also fitted my provisional conclusion that direct attacks from the *outside* are less likely to succeed than attacks from the inside even allowing that they sometimes serve outside interests.

My contact with Richard and Val Routley led me to look more deeply into the forestry issue. The Forestry Department at ANU was one of the few places in Australia where professional foresters were trained. It had strong links with the government forestry commissions and with the forest industries. These links included shared perspectives, conferences, consultations and even a humorously named international organisation, the Concatenated Order of the Hoo-Hoo. I was referred from one critic of the forestry establishment to another, collecting information and getting comments on a short section in my paper about forestry. Ray Hammond, who had worked for the NSW Forestry Commission, gave me many valuable comments. Ian Penna, who worked in Melbourne for the Australian Conservation Foundation, gave me information on links between the forest industries and government forestry commissions. Ian Penna referred me to John Dargavel, who had worked in the industry for 20 years and who at that time was undertaking a PhD in the Forestry Department, applying a left-wing perspective. And so on.

I diligently collected information and also noted down the names of everyone who had helped me. But there were also a few people who were quite willing to help but didn't want to be mentioned in my paper, not even in the acknowledgments. They were afraid to be associated with any criticism of the establishment, since it might jeopardise their careers.

Throughout the first half of 1980 I kept revising and expanding my paper, showing updated versions of appropriate sections to relevant people. This was before the days of word processors, and I was doing all the typing myself, so an entire new version wasn't such a simple matter. In May, I circulated a new full version to all the key people. By this stage I had collected 10 cases, including the dismissal of John Coulter which I'll describe in chapter 7. I submitted the paper to *Science*, which quickly rejected it, and then to *Social Studies of Science*, which did the same. (My experiences with publishing work on suppression are covered in chapter 8.) Then I tried the *Ecologist*, which had published my critique of CRES. To my delight, the paper appeared in the January-February 1981 issue. I had changed the title to "The power structure of science and the suppression of environmental scholarship," as suggested by Jeremy Evans. The *Ecologist* demoted this to a subtitle under a new title, "The scientific straightjacket."

### **When is it suppression?**

One of the continual challenges in studying suppression cases is to decide whether suppression is actually involved. For example, I've never yet come across an academic administrator who openly admitted to an academic "We dismissed you because you were exercising your academic freedom in a way we didn't like." No, in every case some acceptable-sounding justification is offered: your performance is not good enough; you are derelict in your duties; your publications are not of the right type; your behaviour is improper; and so forth. These sorts of reasons are given because, almost always, those who initiate the action sincerely believe in the reasons. We're not dealing with goodies and baddies with labels attached, where the baddies have broken the law and know it. In suppression cases, everyone is sincere — at least that has always been my working hypothesis.

So how do I determine whether someone is being denied tenure because they don't deserve it in terms of academic merit or whether they are being discriminated against because of their gender, opinions or whatever? One very convenient method is what I call the "double standard test." If the justification for blocking a publication from CSIRO endorsement is that it falls outside the bounds of the organisation's research agenda — as was alleged in Peter Springell's case — then is the same criterion applied to all other staff and all other publications? When Springell pointed out that the chief of his division had published under CSIRO auspices a paper falling outside CSIRO's research agenda, he exposed the double standard involved. The obvious implication was that he was being victimised.

Similarly, in our statement about Jeremy's tenure case, we pointed out his satisfactory research performance and outstanding teaching performance, thereby showing the double standard: other academics with similar or inferior records were routinely granted tenure. One of the justifications for the threat to Clyde Manwell's position was the claim that there were four errors in statistics in his book co-authored with Ann Baker. To use such errors as a reason for threatening dismissal from a tenured position is unheard of — except in Clyde's case. Andrewartha later admitted that two of his allegations of errors were themselves wrong and a third confused and irrelevant. But, needless to say, Andrewartha's mistakes did not put his position in jeopardy.

The double standard test is part of the method. It can be used to show that there seems to be some unfairness. But unfairness occurs all the time, and only in some cases should it be called suppression. In suppression cases, the person involved does something that is threatening to a powerful group, such as carry out radical environmental teaching or research or make public statements on social issues.

In the most obvious cases of suppression, there is a close connection between an action by the dissident and the attack. Immediately after Clyde Manwell and Ann Baker's letter to the *Adelaide Advertiser*, Manwell was vehemently denounced in state parliament and soon afterwards the attempt at his dismissal began. Soon after Philip Keane and Peter Rawlinson publicly

expressed their concerns about cinnamon fungus in Victorian forests, Moulds made his complaints to senior figures at La Trobe University.

When there is no close connection like this, things are a little fuzzier. Jeremy Evans was teaching for years and the Human Sciences Program was under constant threat before his tenure was denied. But of course there were no opportunities to deny tenure before the time arrived to take a decision. Nevertheless, a case like this needs a deeper analysis than a case where an attack comes immediately after a particular act.

Actually, I don't often encounter cases where there isn't at least a strong indication of suppression. The reason for this is simple. When there is a legitimate official reason, it is so hard to argue against it that few people do. To take an example, there are undoubtedly cases in which threatening articles are rejected by an editor on the legitimate grounds that they aren't well argued. The decision is "over determined," to use social science jargon. In these cases, articles might have been rejected on either grounds of quality or grounds of viewpoint. It's difficult to argue that suppression occurred in these cases, since it's hard to find people who are willing to say the articles should have been published.

### **Conclusion**

My investigation of suppression of environmental scholarship was triggered by several factors. First was my involvement in the campaign to defend Human Sciences. Second was having recently read or heard about other attacks on environmental scholars, especially the cases of Peter Springell and Clyde Manwell. Third, my own experiences in CRES had prepared me to recognise the processes of suppression.

In collecting information and writing a paper, I learned as I went along. My studies of the role of power in science turned out to be highly relevant, as was my reading of books and articles about attacks on intellectual freedom.

To obtain more case material, it was helpful to talk to people. But probably most useful was sending people copies of my own writing on the topic, in this case a draft of my article. It showed them what I was trying to do and how I was presenting the information. It made them see how their own experiences might

be relevant. And it gave them confidence in my own abilities and commitment to the cause of dissent. Until you see how someone is likely to use information you give them, you may have some reservations.

Probably the most important lesson was to check, double check, triple check and then do another check. To document a case of suppression is a sensitive issue. To document ten cases is even more risky. If a few facts can be easily challenged, they will be, and this will be used to discredit the entire argument. Accuracy was vital.

Of course, the “facts” are seldom simple issues of being right or wrong. There are always interpretations involved. Sometimes different people gave me different stories. Also, it was my article: I put my own stamp on the selection, organisation and interpretation of material. Nevertheless, I found it immensely helpful to circulate drafts for comment. It led me to new material and helped me hone my argument. It gave me confidence about the whole undertaking. Most of all, it made me realise that suppression was everywhere.

### 3

## Power against dissent

As a result of my paper “The scientific straightjacket,” I acquired a certain reputation as a person who would take up cases of suppression. I had circulated the draft paper to many dozens of people. After the paper appeared in the *Ecologist*, I had 250 copies printed and sent them out to people who I thought might be interested. Also, there was quite a lot of publicity about the issue during 1980, as I’ll describe in chapter 7. As the years went by, I was contacted now and then by people wanting to tell me about their own experiences. Suppression was everywhere, but it wasn’t random. There were patterns, and certain patterns interested me greatly.

### **Nuclear power**

In 1985 I read a short item in the journal *Radical Science* about a researcher in India, Dharendra Sharma, who had been penalised because of his opposition to nuclear power. Suppression of nuclear dissidents interested me because I was one of them myself. I had taken a prominent role in the campaign against uranium mining and nuclear power, for example by helping organise rallies, writing letters to the newspaper and giving talks. I had read about various scientists and engineers victimised for their opposition to nuclear power. John Gofman, one of the very early critics in the US, had his funding cut and his staff taken away. Anyway, I decided to follow up Sharma’s case, especially since *Radical Science* asked people to help out.

In July 1985 I wrote to Sharma, enclosing copies of my articles on suppression. In reply he sent me an enthusiastic letter and a substantial amount of information about his case. The story was familiar. He worked as a science policy analyst at Jawaharlal Nehru University in New Delhi. He was a prominent critic of the Indian government’s nuclear policies, both nuclear power and nuclear weapons. He wrote letters and articles, organised confer-

ences and petitions and in 1983 published a book, *India's Nuclear Estate*, which exposed the role of vested interests — especially the Nehru family — in nuclear policy-making. This was at a time when there was little public criticism of or organised opposition to nuclear developments in India

Sharma had tenure and was a senior academic in the Centre for Studies of Science Policy, where he had worked since 1973. Suddenly, in December 1983, he was transferred to the School of Languages. This was convenient for those who wanted to shut him up — it prevented him from becoming head of the Centre and in formal terms limited his work on science policy. The grounds for the transfer were not just flimsy: the transfer itself was in violation of the university's own regulations. Sharma was an outstandingly productive academic, so there were no academic grounds for the transfer. The obvious conclusion was that he was being harassed because of his outspokenness on nuclear issues. His letter to me told of his latest problem. The university authorities were withholding his salary. In other words, he was not being paid.

I offered to write an article about his case, in order to publicise the injustice. After thinking it over, I decided that the best way to make the case would be to write an article that gave some detail about Sharma's case but also documented other cases of suppression of nuclear dissidents. After all, people could always dismiss a single case by assuming, rightly or wrongly, that there must be some "real" — but unsaid — reason for the transfer. By including other cases, I provided a context. If there was a pattern of attacks on nuclear dissidents, then Sharma's experiences would be easier to understand as simply one more case, rather than as an exception.

It's easy enough to talk about documenting cases of suppression, but doing it is another thing. If you go to any index, such as for titles of articles in journals, you will seldom find an entry entitled "suppression of dissent." That's partly because there is no standard terminology. The term "whistleblowing" captures some cases, but far from all. It's also because many cases are not documented. Finally, calling something suppression depends on an analysis, namely me or someone else saying it fits the

category. Not everyone operates with the same framework for analysing the phenomenon.

Luckily, I had a big head start. Since about 1979, when I first began studying suppression, I had been collecting copies of any article I saw suggesting suppression. I read lots of magazines and books, and once I became sensitised to the idea of suppression, relevant items would spring out of the page to my attention. So I went through my file, picking out items about nuclear power. I also went through my many files on nuclear power. In addition, I had articles and newspaper cuttings on suppression sent to me by various people, especially copies from Clyde Manwell's vast collection. Wendy Varney, who corresponded with me about fluoridation and other issues, had sent me an article about the harassment of five different scientists and engineers working in the British nuclear industry. I also combed through issues of various journals, especially anti-nuclear magazines, but this didn't generate much additional material. After assembling this material, I had a considerable number of cases.

In December 1985 I produced a first draft of an article titled "Nuclear suppression." The draft was a useful stimulus to obtain more information. I sent it to various people. Sharma sent me corrections on the part about his own case and also information about problems faced by other anti-nuclear scientists in India. Kiiiti Siratori sent me information about the harassment of Atsushi Tsuchida in Japan.

In 1980 when I visited the US, Mark Diesendorf recommended that I meet Hugh DeWitt, a physicist at the Lawrence Livermore National Laboratory in California. DeWitt was a courageous public critic of nuclear weapons testing. Recently, he had nearly lost his job after testifying for the *Progressive* magazine over its story on the "secret of the H-bomb." He gave me lots of information about his case.

Actually, getting information about nuclear dissent in the US was the easiest task. The system is more open and the country and nuclear industry are sufficiently large that there are plenty of cases. There is even an excellent book on nuclear dissenters by Leslie J. Freeman, called *Nuclear Witnesses*.

Because I had been involved in the campaign against nuclear power for many years, I had a ready-made analysis of the power

structures in which suppression of nuclear dissidents takes place. In my analysis, the key driving force behind nuclear power is the state, namely governments and government bureaucracies. In most countries, nuclear power plants, enrichment plants, reprocessing plants — namely all the key elements in the nuclear fuel cycle — are owned and run by the state. Why the state? Because nuclear power requires large investments, is potentially dangerous, depends on experts and demands protection against misuse. For all these reasons, centralised control is called for. This both requires state involvement and justifies it. By contrast, measures for energy efficiency and small-scale renewable energy can readily be taken up by individuals and local communities. If households and neighbourhoods are self-reliant in their energy systems, they do not depend on the state. Back in 1952, the US Paley Commission recommended a solar-based energy strategy, but instead the US government poured money into nuclear power.

Another key link in the promotion of nuclear power is nuclear weapons. Nuclear power was an outgrowth of nuclear weapons research, and there continue to be strong links. Any government that sets up a nuclear power programme provides itself with both plutonium and nuclear experts, thus setting the stage for a nuclear weapons programme if desired.

Corporations, namely the nuclear industry, also play a role. In most countries, with the possible exception of the US, the nuclear industry is subservient to the state. When the British government privatised its electricity industry, the nuclear sector had to be kept under state control, since private enterprise wouldn't touch it without government guarantees. Indeed, government subsidies and protection — such as insurance for major nuclear accidents — have always been necessary to keep the nuclear industry going.

But how does all this relate to suppression? The connection comes via another key factor in the nuclear equation: the nuclear experts. Nuclear scientists and engineers have been key proponents of nuclear power, not surprisingly since it provides them with status and jobs. In addition, some of the early proponents had helped to build the first nuclear weapons; nuclear power seemed to be a way to use their skills for peaceful purposes. As

long as the experts all supported nuclear power, it was easy for governments to push the new technology. But then in the late 1960s and 1970s, as nuclear power programmes began to expand, citizen opposition emerged around the world. Citizens could be dismissed as uninformed. They were not experts. But if even a small minority of experts openly opposed nuclear power, this changed things enormously. The situation went from expert consensus to a debate. Nuclear dissidents thus were influential far beyond their numbers. They gave enormously greater credibility to the anti-nuclear movement. In this situation, many of the dissidents came under attack. Indeed, it was more important to attack an anti-nuclear scientist than an anti-nuclear citizen activist. There were fewer anti-nuclear scientists and their role in the credibility stakes was more crucial.

This then was the framework I developed. Suppression of expert critics of nuclear power was a feature of a power struggle between the supporters of nuclear power, found largely in sectors of the state, in the nuclear industry and in the scientific community, and opponents of nuclear power, found largely in citizen movements. Dhirendra Sharma's ordeal could be understood both as part of a pattern of suppression around the world and as part of a wider struggle over nuclear technology.

I put together my article "Nuclear suppression" by first dealing with Sharma's case, then outlining my framework of analysis, and then briefly recounting cases from many countries. I sent a draft to a number of friends and colleagues and obtained useful comments. The publisher of the journal *Science and Public Policy* was interested in the article, but he wanted to be absolutely sure about the evidence.

So I went back and searched for even more cases. This was useful. I found more and more evidence.

Searching out cases is challenging. More frustrating is actually writing up the cases. First I have to decide what counts as a case worth mentioning. Through all my experience in studying the issue, I've come to have a good idea about this. Then, when there are a lot of cases to describe, comes a series of minor challenges: to describe each case in a sentence or a paragraph. Sometimes the description I have, from some magazine or book, is short to begin with. Then the challenge is to say basically the same thing

without copying or misrepresenting the description. Sometimes I have several accounts, from different sources, of the same case. Yet other times I have source documents, such as letters of reprimand. The challenge is to condense all this material into a short, accurate and revealing summary. Brevity is vital because there are so many cases to describe. Accuracy is vital because mistakes can be used to discredit the whole argument. Finally, each summary needs to show the process of suppression. To get this right means lots of checking against source documents, and also sending out drafts and queries to many people. Naturally I sent a draft of my article to Sharma for his comments, and he was most helpful. It was impossible to check out all the other cases directly, since there were too many and in any case getting in touch with dismissed workers can be difficult. But I was able to contact a few.

I thought the final product was impressive: I was able to cite cases from ten different countries, though there were far more documented cases from the US than anywhere else. The article was published in *Science and Public Policy* at the end of 1986.

*Science and Public Policy* does not have a large circulation. But publication of my article “Nuclear suppression” was useful nonetheless as it gave my account the credibility of being in a journal. It was also accessible through libraries. Even so, the biggest impact probably came through direct circulation of copies of the article. Whenever I wrote to someone who might be interested — someone interested in nuclear issues or in suppression generally — I enclosed a copy. Sharma wrote me that an article appeared in the British newspaper the *Guardian* about suppression of nuclear dissidents, drawing heavily on my article. Not least, the article was read and circulated by a number of nuclear dissidents themselves, such as Sharma and Hugh DeWitt. As a result of hearing about the article, a couple of the British nuclear dissidents wrote to me.

### **Fluoridation**

In 1985 my precarious short-term appointments in the Mathematics Department at ANU finally came to an end. I applied for many jobs — mostly in scientific research — and was lucky to obtain one in the Department of History and

Philosophy of Science at the University of Wollongong. In my new job, studying social issues such as suppression was entirely legitimate rather than something I did on the side. I decided to do a study of the fluoridation controversy.

I already knew a fair bit about fluoridation since my friend Mark Diesendorf was a leading critic of it. Fluoridation is the addition of about one part per million of the element fluoride to public water supplies in order to reduce the incidence of tooth decay in children. It was tested out in the US in the 1940s and then strongly promoted in the industrialised world since the 1950s. Fluoridation was backed by most dental authorities but from the very beginning it was opposed by citizen groups.

Fluoridation interested me mainly because the debates over it provide an insight into the links between scientific knowledge and power. Personally I have never thought it a vital issue, compared for example to nuclear power or genetic engineering, not to mention big problems like war and racism.

My study about fluoridation was not just about suppression, but covered a range of social issues. I looked up the numerous social studies of fluoridation that had already been done and interviewed key pro and antifluoridation experts in Australia. My analysis looked at a number of levels of the fluoridation debate, including the scientific arguments, the coherency of the viewpoints of the partisans, the role of the dental profession, the influence of corporate interests (such as the manufacturers of sugary foods) and the role of the social scientist (that is, me). But within this many-layered treatment of the fluoridation issue, I discussed suppression as a central issue.

In the early years after it was first proposed in 1939, the idea of adding fluoride to public water supplies was promoted by only a few enthusiasts in the United States. Most authorities were sceptical. Controlled studies comparing towns with and without added fluoride were begun in 1945. After much lobbying, the proponents won over the key body, the US Public Health Service, which endorsed fluoridation in 1950. Not long after, many other organisations endorsed fluoridation, such as the American Dental Association. Endorsements became a key method of promoting fluoridation. But many of the endorsements came from organisations that had never studied the evidence, such

as the American Federation of Labor and Congress of Industrial Organizations. The proponents had decided that fluoridation was safe and effective. Endorsements were an important means to convince others. They were a key technique used in the “struggle over credibility.”

The proponents had the official backing of all crucial organisations, especially public health, dental and medical bodies. But there were still some opponents, including some dentists, doctors and scientists. The proponents sought to deny them credibility. How? They had several methods.

In the US, most decisions about fluoridation were made in cities and towns. When the issue came to be decided, whether by the local government or in a referendum, pro and antifluoridationists each tried to win over the uncommitted. In many cases, profluoridationists refused to debate with antifluoridationists. The proponents claimed that there was no scientifically credible evidence against fluoridation. Therefore there was nothing to debate. By refusing to debate, they implied that there was only one credible side. To debate would be to admit there was something worth debating. But sometimes this tactic backfired, when proponents were seen as being arrogant.

Sometimes the critics of fluoridation were entirely ignored. For example, dental researcher Philip Sutton published a book in 1959 showing flaws in the methods used in the early controlled studies of the effectiveness of fluoridation. His critique was generally ignored by profluoridationists.

Another technique was to attack the critics in general terms. For example, a book published by the World Health Organization in 1986, edited by leading fluoridation proponent J. J. Murray, mentioned “the often misguided opposition to community fluoridation programmes” but didn’t cite a single source. In such cases, the critics were not given names.

Another technique was to circulate unpublished critiques. John Colquhoun, a New Zealand dental researcher who became a leading critic of fluoridation, published an article in *American Laboratory* in 1985. In response, a dental research officer in New Zealand, Peter Hunter, wrote a letter which alleged that Colquhoun’s article contained mistakes. On the basis of Hunter’s letter, the Director-General of Health sent a statement to local

water supply authorities in New Zealand criticising Colquhoun's work. The Centers for Disease Control in the US incorporated Hunter's letter as part of one of its publications. Neither Hunter nor anyone else from these organisations bothered to send Colquhoun a copy of Hunter's letter. The impact of the unpublished critique was to attack the credibility of the critic of fluoridation without engaging in an open debate in professional or public venues.

Yet another technique was to attack the critics personally. The most astounding example of this was the dossier on opponents of fluoridation compiled by a group within the American Dental Association. The dossier contained derogatory comments — mostly taken from letters or newspaper articles — about a range of critics. Many of the critics, such as the Ku Klux Klan and various purveyors of nostrums, had little credibility. Others were reputable scientists. By being included in the dossiers, the implication was that they also were cranks.

The dossier had a big impact. It was published twice in the prestigious *Journal of the American Dental Association*. It had an especially big impact on the most authoritative critics of fluoridation. Foremost among these was Dr George Waldbott, a doctor and scientist who had a number of important discoveries to his name. Waldbott became critical of fluoridation in the mid 1950s and undertook studies, finding allergic reactions to fluoride in a number of his patients. He was the leading opponent of fluoridation in the United States until his death in 1982.

The material about Waldbott in the dossier was damaging. From his point of view, much of it was also false and unfair. As Waldbott appeared throughout the country and overseas speaking and testifying against fluoridation, the dossier followed him like a "steady companion," to use his description. He had to repeatedly reply to the allegations. The American Dental Association was effective in circulating the dossier but not equally assiduous about circulating the corrections sent to them by Waldbott.

So far, the sorts of techniques that I've described are not what I would call suppression. They are simply rather unsavoury methods for promoting a cause. They are unsavoury because they sidestep an open and honest discussion of the issues by either avoiding debate or attacking the opponent. But there are plenty

of documented cases of suppression too. There are cases of dentists who were suspended from their dental societies for opposing fluoridation. There are cases of researchers who were threatened with loss of research funds if they continued to study fluoride. There are cases of university students who were fiercely attacked by senior administrators because of their studies of fluoridation. There are cases of articles critical of fluoridation that have been criticised by journal referees because they might be helpful to antifuoridation groups.

One of my favourite cases is the response of the *Journal of the American Dental Association* to submissions from Albert Schatz, a scientist known as co-discoverer of streptomycin — in other words, not “just a crank.” Schatz’s letters, sent by certified mail, were refused and returned to him unopened. Apparently the editor knew Schatz was opposed to fluoridation.

Tracking down examples of all the sorts of responses I’ve described, from refusal to debate to formal complaints against dentists for stands against fluoridation, took a fair bit of time. But some of the work was done before me. Several leading antifuoridation scientists had both experienced suppression repeatedly and also, because of their prominence, been informed of many other cases. Waldbott’s 1965 book *A Struggle with Titans* documents numerous cases. Philip Sutton in a 1980 monograph lists several cases. US scientist and leading antifuoridationist John Yiamouyiannis lists many cases in his book *Fluoride: The Aging Factor*. Hans Moolenburgh, a Dutch doctor and campaigner against fluoridation, tells of several cases. There was plenty of evidence. I only had to select the most appropriate material to illustrate my argument, write accurate summaries and verify details.

In collecting information about the fluoridation issue, direct contact with partisans was highly valuable. I wrote to lots of people for different sorts of information. After interviewing leading Australian pro and antifuoridationists, I wrote an article and sent a draft to each one of them for comment. I wrote to dozens of governments around the world asking about the extent of fluoridation in their countries and about their policies on the issue. I also wrote to leading figures internationally, and obtained valuable responses from Albert Burgstahler in the US, Hans

Moolenburgh in the Netherlands and John Colquhoun in New Zealand, among others. George Waldbott's widow Edith sent me documentation on a number of suppression cases mentioned in his books.

One of the most difficult challenges in writing about suppression of anti-fluoridationists is to explain why. From the point of view of some pro-fluoridationists, there is no dilemma. They believe that fluoridation is totally safe and highly beneficial and that there is no credible evidence to the contrary. Therefore, anyone who criticises fluoridation must be irrational, confused or driven by some vested interest. That anti-fluoridationists have been denied funding or blocked from publishing in dental journals is nothing to worry about, because their work is no good. Dozens of social scientists had studied the issue previously and assumed that fluoridation is scientifically beyond criticism and so had not recognised that suppression could be an issue.

My assessment was different. I assumed that simply appealing to science alone was not enough to explain the domination of pro-fluoridation views among dentists and doctors. In accordance with the precepts of the "sociology of scientific knowledge" or SSK, I looked to social factors to explain why scientific claims that fluoridating water supplies was safe and beneficial were so widely accepted. According to SSK, the social scientist — me in this case — examines the arguments on all sides without making any judgements about their validity.

But I didn't have to be an SSK adherent to make my analysis. I had read enough of the anti-fluoridation studies by scientists such as George Waldbott, John Colquhoun and Mark Diesendorf to know that they couldn't be dismissed so very easily. They provided or referred to studies showing that fluoride caused allergic or intolerance reactions in some people, that it was linked to skeletal fluorosis in some parts of the world and that improvements in tooth decay rates might be caused by factors other than fluoridation — among many other criticisms of the case for fluoridation.

There was also another factor. The arguments about fluoridation weren't entirely scientific. There were value judgements built into the debate at all levels. Should fluoride be added to public water supplies, thereby making it hard to avoid even for people

for whom there were few or no benefits — people with no teeth, for example? There are alternative means for people to get fluoride, such as taking fluoride tablets, having fluoride treatments by dentists and buying salt with added fluoride. But if one of these alternatives was adopted, then many people could not afford or would not take the trouble to obtain fluoride. Was it a valid public health measure to add fluoride to public water supplies, or was it a violation of civil liberties to give people a compulsory but uncontrolled dose of a chemical? Furthermore, how should decisions be made about fluoridation? By governments advised by dental experts, as advocated by many profluoridationists, or by referendum as advocated by many antifuoridationists?

The issue certainly has many dimensions. It is also persistent, having been going for half a century with no sign of resolution. The two sides seem entrenched in their positions.

Back to the issue of suppression. My assessment was that there were some solid scientific criticisms of fluoridation that at least deserved to be taken seriously. Yet the more usual response was to ignore the critics or attack them. Why? Why was there such hostility to critics?

My assessment, like that of a number of others who had investigated this issue, is that the prime driving force behind fluoridation was the dental profession. A more conspiratorial view of some antifuoridationists was that corporations that produced fluoride pollution, especially the aluminium industry, were behind fluoridation. I couldn't find much evidence that industry played more than a background role in the debate. There was some funding of fluoridation campaigns by the sugary-food industry, which served to draw attention away from the acknowledged role of sugar in tooth decay. Corporate influences may have helped shape the agenda.

Some elements in the state have promoted fluoridation. The best example is the US Public Health Service. As in the case of nuclear power, the promotion of fluoridation is complex process, involving the dental profession, corporations, government bureaucracies, media and community groups, among others. But within this complexity, the main player was the dental profession.

This was certainly obvious in the suppression cases. The American Dental Association's dossier was a dramatic manifestation of dental profession hostility to criticism by fluoridation opponents. But it all seemed counter-intuitive. If fluoridation reduced tooth decay, this would reduce work for dentists. It was against their interests. Surely they wouldn't support it unless they had the interests of the public at heart.

This sounds plausible and it certainly explains the individual psychology of many dentists. But there is an analysis of professions that gives a different assessment. Professions, such as law, medicine and the ministry, are really just occupational groups like plumbers or farmers. They are different from most other occupations in that the members of professions have a considerable degree of control over their own work and as a group have some control over training and entry into the profession itself. Doctors, lawyers and dentists have long sought to restrict the number of practitioners, in order to keep salaries high.

If salaries are kept high by restricted entry, that means there is plenty of work to do. There are many more dental problems than dentists have time to treat. Reducing the amount of tooth decay means there is more time for dealing with other dental problems such as gum disease.

Fluoridation was attractive to dental researchers because it made dentistry seem more scientific. It involved epidemiological studies of tooth decay as a function of fluoride levels and biochemical studies of the mechanism by which fluoride works in the mouth against tooth decay. Some dental researchers and public health official built their careers on promoting fluoridation. They managed to persuade most dentists, who had no time to study the evidence, that fluoridation was a good thing and that the status of the profession was under attack by know-nothing antifluoridationists.

This is the argument in outline. You can see that it's not easy to explain in casual conversation. Most people believe in the virtue of professions. This contrary view is not that professions are corrupt or anything but well-meaning, but that their assessment of scientific claims and their response to challengers is shaped, in a complex way, by their collective self-interest.

On the other hand, cases of suppression are relatively simple and dramatic. Even some profluoridationists are embarrassed by the “excesses” that are committed against the critics.

My analysis was “balanced” in the sense that I critically analysed the arguments and the vested interests on each side. Because dental and medical authorities have largely supported fluoridation, my analysis thus seemed to them to be opposed to fluoridation. Therefore, it was often difficult to obtain comments from profluoridationists. On the other hand, some of the ardent antifuoridationists, such as John Yiamouyiannis, thought I had given fluoridation too much credibility.

When I came to write a book about fluoridation, I wrote to several leading pro and antifuoridationists to ask if they would comment on a draft. Three leading opponents, Albert Burgstahler, John Colquhoun and Mark Diesendorf, each readily agreed. Getting a similar number of leading proponents to comment was more difficult. I had to approach about a dozen proponents in order to find four who would comment: Brian Burt, Michael Lennon, John Small and Donald Taves. But this effort was worthwhile, because it gave me critical perspectives from both sides of the debate.

The person who gave me the most valuable comments of all was not a partisan but a social analyst like me. His name was Edward Groth III, or Ned to his friends. I had come across his name a few times in my study of fluoridation literature. He did a PhD at Stanford University. His 1973 dissertation covered two issues: air pollution in San Francisco and the fluoridation controversy. When I finally was able to contact him, he was working at the Consumers Union in New York. We struck up a vigorous correspondence. Ned sent me a copy of his dissertation and some other papers he had written. If I had known about Ned’s work earlier, I might never have written my book on fluoridation, because he covered much of the same material that I did — though my treatment was more international, more up-to-date and grounded in a particular analysis of science. More importantly, his dissertation was never published, mainly because he had obtained a job at the Consumers Union where there was little pressure to publish. In any case, Ned was enthusiastic about my

efforts but also a keen critic. He sent page after page of comments on my drafts.

To all those who read and commented on the draft of my book, I made an offer: they could write a commentary that would be included in the book itself. Only one person took up the offer: Ned Groth! From my point of view, this was highly appropriate, given that his work had foreshadowed mine.

Working on the fluoridation controversy was a lot of fun. It was a good conversation topic. Australia has long been highly fluoridated, so most Australians drink fluoridated water but never think about it. The claim it could be causing allergies or even cancer would be greeted with concern, disbelief or even amusement. There are still plenty of people who accept the idea, promoted by profluoridationists, that the only criticisms of fluoridation come from unscientific cranks or right-wingers who believe it is a government plot to poison the public.

My main writings about fluoridation were several articles in scholarly social science journals and a book published by a university press. Still, my analysis was found useful by some people besides academics. The most satisfying response was from a scientist who wrote, in a letter in the magazine *Chemical & Engineering News*, that the dynamics of the fluoridation controversy, as he experienced them, were accurately described in my book. He commented that “Every argument, every claim, every uninformed public health official, and every personality involved in the Tucson controversy was a mirror image of the stereotypes described in Martin’s book.”

### **Pesticides**

Pesticides are chemicals designed to kill things such as insects, plants and fungi. There are special names in some cases, such as insecticides, herbicides and fungicides. The general term is pesticides. The most famous one is DDT. Among hundreds of others are dieldrin, aldrin, 2,4-D and 2,4,5-T. The last two were the main components of Agent Orange, the most well-known herbicide used by US forces to remove foliage from trees during the Vietnam war.

Pesticides can be very valuable in controlling harmful pests that would otherwise destroy crops or forests. But they also have

undesirable side-effects, killing insects and animals that are not pests. They are also a potential danger to human health. The hazards of pesticides were brought to public awareness by Rachel Carson's famous book *Silent Spring*, published in 1962. This book was a key trigger in the rise of the modern environmental movement.

Most pesticides are produced by a small number of chemical companies. Not surprisingly, these companies are strong supporters of their products. They provide lots of money to promote pesticides, including funds for research. Many scientists, both in government and in universities, also support pesticides. Some of them receive research support from pesticide manufacturers, but some don't. Pesticide supporters all believe the benefits outweigh the risks.

Many of the most active critics of pesticides are community activists. They want pesticide use controlled and reduced and also favour development of alternative approaches to controlling pests, such as biological controls or planting certain crops next to each other.

In this situation, there are a few scientists who do research into or speak out about problems with pesticides. Because these scientists undermine the monopoly on scientific credibility otherwise held by pesticide proponents, they often come under attack. No surprise here!

I've already described the case of Clyde Manwell, the professor of zoology at the University of Adelaide who was denounced in parliament and threatened with dismissal after he and his wife Ann Baker simply wrote a letter to the newspaper. What's interesting here is that several people had criticised pesticides in letters to the *Adelaide Advertiser* before Manwell and Baker's letter was published. But they weren't denounced in parliament. The obvious difference is that Manwell was a professor of zoology and therefore had much more scientific credibility.

Beginning with the Manwell case, I came across many examples of attacks on scientists critical of pesticides. In 1980-1982 I was a member of a short-lived group called "Community Action on Science and Environment" or CASE. We did studies, produced leaflets and made public statements on a number of issues, such as the problems with sugar, caffeine and television. At one stage I

wrote a short piece on herbicides, and included a list of some of the attacks on critics.

There are several good sources on the attacks. Frank Graham Jr.'s book *Since Silent Spring*, published in 1970, documents the furious denunciations and attacks on Rachel Carson and other early critics that came from the chemical industry and its allies. Even more revealing is the book *The Pesticide Conspiracy* by Robert van den Bosch, who worked at the University of California at Berkeley. He lists about a dozen cases of attacks on different individuals. Cutting off of research funds is a typical technique. He also tells of the personal abuse he received — being called a variety of names — from university colleagues because of his views.

I had mentioned these sources in my writings but hadn't made a special study of suppression of pesticide critics until I heard about the case of Melvin Reuber. Reuber was a highly productive scientist who worked for the Frederick Cancer Research Center, part of the National Cancer Institute in the US. Among other things, he did research on the possible cancer-causing properties of certain pesticides. In 1980, out of the blue, he received a dressing down and a written denunciation of his work from his boss, Michael G. Hanna, Jr. More seriously, the bulk of Hanna's report was soon published by a petrochemical trade newsletter, *Pesticide & Toxic Chemical News*. Reuber resigned under the stress but then decided to fight in the courts. The story in *Pesticide & Toxic Chemical News* was circulated around the world and used to discredit Reuber whenever his work was cited as part of a case against pesticides. The use of Hanna's letter reminded me of the use of the American Dental Association's dossier against George Waldbott.

I decided to write an article on suppression of pesticide critics, featuring Reuber. This was very much in the style of my paper on nuclear suppression. My approach was the same. I collected information from my files and through obvious sources, and checked out my draft with several knowledgeable people, including Reuber himself. After rejection by several journals — a process that took years — it was published by *Philosophy and Social Action*. But for now I'd like to turn to a different issue.

**Patterns of suppression**

As already described, nuclear power, fluoridation, and pesticides are three fields where I've studied suppression of scientific dissent. Another field where I've observed a pattern of cases is forestry. On the other hand, some areas where you might expect to find many cases, such as automobile safety, seem to have few on record. What's the explanation?

A preliminary generalisation goes like this. For there to be a pattern of suppression in a field, there has to be a powerful set of interests involved, backing a particular stand. This sets the stage for suppression. But there is no need for suppression unless there is opposition. In each of the cases I've studied, there has been a social movement challenging vested interests: the anti-nuclear power movement, the antifuoridation movement, and the community groups opposed to pesticides and to certain forestry operations. In each of these cases, some scientists have done research or spoken out in a way that can be used by community activists. These dissident scientists give credibility to the activists, changing the situation from a monopoly of expert opinion to a debate. In this situation, attacks on the dissident scientists are likely, if they are vulnerable.

In some cases there are expert critics but no social movement. Automobile safety is one example. There are a few cases of suppression, of which the most famous is the attack on Ralph Nader, who came to his initial fame with the book *Unsafe at any Speed*, a critique of automobile safety. But there has been no mass movement against the car culture. Critics have no mass constituency that will take up their work and hence receive less encouragement to become open critics in the first place. In such a situation, there is still suppression but it is seldom publicised. Also, there is a lot of self-censorship.

What about the vested interests? My analysis is that the driving force behind nuclear power is the state, the driving force behind fluoridation is the dental profession, and the driving force behind pesticides is the chemical industry. These are three different types of vested interests: the state, a profession and an industry. But this difference doesn't affect the details of suppression cases as much as might be expected. Suppression isn't done directly by "the state" or by "the dental profession." It is always

carried out by individuals. The ties to what I call the “driving force” can be complicated. There are links between the state, professions and corporations. Nuclear power might be promoted by state bureaucracies, but the nuclear industry and nuclear scientists and engineers are closely involved. The dental profession has ties with some industries and is both certified and regulated by the state. Professional associations and government bodies are tightly involved in promoting pesticides.

Amidst all this complexity, there are some important constants. For patterns of suppression to occur, there need to be vested interests and they need to have power that can be used against dissidents.

One area that I studied showed a revealing reversal and confirmation of this generalisation. The area is “nuclear winter,” the name applied to the global climatic consequences that some scientists predict will occur after a major nuclear war. The idea is that dust and smoke from the explosions and fires will block sunlight, causing a precipitous drop in temperature that could kill much of the world’s population as well as cause major environmental damage. Claims about nuclear winter were developed in the early 1980s by atmospheric and other scientists, the best known of whom was astronomer Carl Sagan. Some of the promoters of nuclear winter were vocal critics of preparations for nuclear war. They argued that because nuclear winter resulting from global nuclear war could lead to the destruction of civilisation or even human extinction, it was imperative that there be massive reductions in nuclear arsenals.

My analysis of nuclear winter was designed to show the linkage between science and politics. I argued that assumptions about politics — such as the assumed type of nuclear war — were embodied in nuclear winter models, and also that the scientific results of nuclear winter models were used for political purposes in the debate over nuclear weapons. I also argued that the same thing applied to the critics of nuclear winter models.

The supporters of nuclear winter conclusions included prominent critics of governmental policies on nuclear war. They were broadly aligned with the vigorous peace movement of the 1980s. They also had much more scientific credibility than the critics of nuclear winter, who were generally defenders of government

nuclear policies. The top officials of the powerful US Department of Defense were critical of nuclear winter. However, I have not heard of any cases of suppression of nuclear winter scientists. That could be because most of the key scientists work for universities, not the military. It could also be because it would be counterproductive trying to suppress a dominant scientific view. It would be hard to discredit so many scientists.

On the other hand, there were a few expert critics of nuclear winter who, by their stand, punctured the appearance of scientific unanimity. One of them in particular, Russell Seitz, then an Associate of the Harvard University Center for International Affairs, was an influential critic because his article in the *National Interest* was circulated widely including being published in the *Wall Street Journal*. Nuclear winter scientists wrote letters to these journals that attacked Seitz on scientific grounds and also made vicious criticisms of Seitz himself, for example referring to him as “a stock investment consultant” who was “dabbling in atmospheric physics.”

I would call this a personal attack but not suppression. Seitz’s position, financial support or ability to publish his views did not come under threat. Nevertheless, criticisms of people’s qualifications are characteristic of cases of suppression. Seitz could have been a victim of suppression if nuclear winter scientists had had power over his job or his opportunities for publication.

But they didn’t. That’s the important difference here. It was the Department of Defense that had enormous power but was in no position to squash a dominant scientific position. The nuclear winter scientists had the most scientific credibility but lacked the power to suppress the few technical critics who they faced.

Suppression is much more likely, then, when the side backed by power and money also has a near monopoly on scientific credibility. This was the case in the debates over nuclear power, fluoridation and pesticides, at least until the critics became more successful.

In my studies of patterns of suppression, I’ve concentrated on social debates where scientists have had an important role. But suppression is found in other sorts of areas. Political dissidents regularly encounter suppression. In the capitalist countries, many socialists, trade unionists and other critics of corporations have

been suppressed. In socialist countries, opponents of the government are prime targets. In dictatorships of any complexion, critics of the authorities are likely to be attacked. Feminists who challenge male-dominated institutions have been attacked. And the list goes on.

There's plenty of documentation of attacks, but in most areas it's not very systematic. Suppression in science has been of special interest to me both because of my background in science and because many people think science is done by rational, objective researchers who are not influenced by social factors.

In spite of all my studies, there are still many basic questions that I can't answer. Often I'm asked, especially by journalists, "How frequent is suppression?" My answer is, "I don't really know and no one else does either. There haven't been enough studies to provide an answer. What I *can* say is that it's much more common than most people realise."

Another question is "Is the amount of suppression increasing?" Usually they think it is, because they've come across some recent cases. I know that there's plenty of evidence of suppression in the "old days." So my answer is "No one really knows. There haven't been enough studies to tell one way or the other."

Then there are questions like this: "Suppression seems to be more common in Australia than other countries. Do you agree?" Some think it's more common in Australia, others that it's more common in the US, or Canada, or wherever. Sometimes it's a comparison between universities in different countries, or the media, or whatever. My answer is always the same. "There isn't enough evidence to say one way or the other."

What I can say is that suppression is much more common than most of us realise. It's under our noses but we don't see it. Few people make a fuss about being suppressed and in many cases they don't even know it has happened. In my experience, if a good investigator goes into virtually any organisation — government bureaucracy, corporation, university, church, trade union, etc. — then it's possible to find many cases. But doing this is not a way to win friends in high places.

Finally there are the sceptics who ask whether suppression really makes any difference. This is easier to answer. The risk of suppression discourages most employees from speaking out

about corruption involving millions or billions of dollars; a few courageous individuals have spoken out, such as A. Ernest Fitzgerald who exposed massive cost overruns in US military contracting. Engineers warned about the risks of defective O-rings in the Challenger spacecraft, but were overruled — and disaster occurred. Dissidents in many countries have been crucial to challenges to repressive governments. They are symbols of freedom and inspire others to oppose tyranny. Even when money and lives are not directly at stake, tolerance of dissent is vital to any society that calls itself free.

## 4

### It's defamatory!

In my studies of dissent, I sometimes like to imagine that I could tell the *full* story, revealing the hidden facets on all sides of the issue. But this is just a dream. I've never been close. Partly it's because my information isn't complete or totally reliable. Partly it's because I don't want to offend someone, especially the person who has been suppressed. Partly it's because of the limitations of the frameworks through which I view the world. But even if I could overcome these obstacles, there's another big problem: the law of defamation.

Many people think of the law as a great protector, as a place where justice is dispensed. If only it were true! Actually, the legal system serves best those who have the most power and money.

For anyone who writes or publishes on sensitive topics, concern about defamation is always there in the background. Here I won't bother with legal details, but just give a general perspective on defamation. Broadly speaking, defamation occurs whenever you say or write anything derogatory about someone and someone else hears or reads it. Most people make defamatory statements several times every day or even every few minutes, just in the course of everyday gossip. If you say that someone is stupid or fat or corrupt or rude, that's defamatory. When you just say it, it's called slander. When it's printed or broadcast, it's called libel. The term defamation covers both types.

If you say something defamatory about someone, they can sue you. You can defend in court on various grounds, depending on the law. In some places, you only have to prove that your statement is true. In other places, you have to prove that it is true and also that it was in the public interest to say it.

Of the untold number of defamatory statements made every minute, only a handful get to court. Few people ever do anything about purely verbal statements unless they are widely broadcast,

such as on radio or television. Similarly, few people ever do anything about written statements unless they are widely circulated, such as in a newspaper. Publication is the usual prerequisite for a charge of defamation.

In a polite society, a person who said something defamatory and wrong would be asked to make a correction, retraction or apology. The law of defamation uses quite a different approach. The person or organisation that is found guilty of defamation — saying or writing it, publishing it or even just distributing it — may be forced to pay thousands or even hundreds of thousands of dollars in compensation. It's also expensive to take a case to court. Lawyers' fees can be hefty. It is the big financial penalties and legal costs that make defamation law a tool to protect those who are powerful from published criticism.

Considering some of the sensitive topics I've dealt with, I've had a relatively easy time, having never been sued for defamation. But there have been some threats. My experiences give me a warm awareness of the dangers but have not left me scorched with a major case. Threats of defamation are far more common than actual cases. Therefore it may be helpful to tell about my experiences in this regard.

### **Jousting with the nuclear knights**

In 1979 I decided to do a study of the views of the leading Australian proponents of nuclear power. I was involved in the anti-nuclear campaign and thought that such a study would be useful to other opponents. I eventually decided to focus on the two leading proponents, Sir Ernest Titterton, then Professor of Nuclear Physics at the Australian National University, and Sir Philip Baxter, former head of the Australian Atomic Energy Commission. They were knighted largely for their contributions in the nuclear field.

I tracked down as many pieces of their writings as possible and then analysed their views on nuclear power, nuclear weapons and the nuclear debate. My argument was that their views reflected their positions as nuclear experts and also that their views on particular issues had changed to suit the convenience of the current debate. When in the 1960s they looked favourably on the prospect of Australian nuclear weapons, they said the Nuclear

Non-Proliferation Treaty was, in Sir Ernest's words, a "worthless bit of paper." But in the 1970s when nuclear proliferation was a key argument against uranium mining and nuclear power, they changed their tune and said the Treaty ensured that ostensibly "civilian" parts of the nuclear fuel cycle could not be used for military purposes.

My critique was detailed and hard-hitting, but also carefully written and highly referenced. I was aware of the possibility of defamation. My analysis was of their views, not a judgement of them as individuals.

On completing the analysis, I submitted it as a paper to the British journal *Social Studies of Science*. The editor, David Edge, told me that they would be most reluctant to publish it if either Sir Ernest or Sir Philip objected strongly. Edge had some reason to be concerned. He was the co-author of a book on British astronomy, and an astronomer had threatened to sue in order to stop the book's publication. I knew for sure that Sir Ernest and Sir Philip would object to my article, so publication in *Social Studies of Science* was not a prospect. (As it turned out, it was rejected anyway as not having enough original sociology.) I also realised that I'd have the same problem at other journals. So I decided to publish it myself as a booklet.

I approached the Rupert Public Interest Movement ("Rupert" for short), an organisation that campaigned for freedom of information legislation and took up other free speech issues, to see if it would be the official publisher. I knew two of the key people in the organisation: John Wood and Kate Pitt. They agreed. Rupert was the official publisher. John and Kate helped out with some useful promotion. John drew some fantastic cartoons, including the cover showing Sir Ernest and Sir Philip tilting at a windmill. I did all the typesetting and layout, covered the costs and did most of the publicity and bookkeeping.

But before we got this far, we took precautions to reduce the risk of a defamation action. Rupert was incorporated — Rupert Public Interest Movement Inc. — which meant that the members were not liable for debts of the organisation. Since Rupert had almost no money, there wasn't much to lose. Indeed, if Sir Ernest or Sir Philip did sue, it might bring helpful publicity to Rupert. But there was no point being rash. John McMillan, a lawyer who

was involved with Rupert, looked over the manuscript. After checking that I had evidence to back up some of my statements, he concluded that there was nothing that couldn't be defended. I sent copies of the manuscript to Sir Ernest and Sir Philip, asking if they had any comments. As we expected, they didn't reply. So Kate Pitt rang up each of them, with John Wood listening in. She asked whether they had read the manuscript and whether they had any comments. Sir Ernest said it was "mainly rubbish — not entirely but mainly." Sir Philip said the manuscript was with his solicitors. He threatened to sue "for very considerable damages" if it was published.

We thought that Sir Philip was just bluffing. But his threat made us wary. In August I sent the typeset version to another lawyer, a friend of Rupert's from Perth who didn't charge for giving his advice. He queried the evidence for a few statements and I made a few adjustments to the text. It was published with the title *Nuclear Knights* in October 1980. As we expected, Sir Philip didn't sue.

There were several lessons from this experience. I learned first hand how the risk of being sued for defamation can inhibit research and publication of material that criticises powerful individuals. I also learned several ways to avoid defamation that have worked well in the years since. First of all, I studied the law of defamation a little bit myself, reading several treatments of the issue. I used my amateur understanding of the law when writing my critique. Second, I sought advice. John McMillan and the lawyer from Perth gave free legal advice. I also sought comment from others on the accuracy of my analysis. Several friends read versions of the manuscript and provided corrections and suggested improvements. Third, I sent the manuscript to the nuclear knights themselves. This put them in the position of remaining silent, in which case it would have been harder for them to successfully sue later, or providing specific requests to remove or change defamatory passages, in which case changes could have been made before publication. One reason we thought Sir Philip was bluffing was that he didn't point out problems with any specific bit of text. One last protection was that Kate Pitt gave me a signed summary of her conversations with Sir Ernest and Sir Philip.

### **An expert bites back**

A couple of years later, I wrote a critique of the writings of another Australian advocate of nuclear power, Leslie Kemeny, who was in Nuclear Engineering at the University of New South Wales. My article, set in the context of a general critique of experts, was hard-hitting but carefully written and well documented. In March 1982 I sent a draft to Kemeny. He wrote back the next month saying that it was “dishonest, devious and actionable.” The word “actionable” means that he expected to succeed in a court action for defamation. I wrote again asking for specific reference to defamatory passages, but he didn't reply.

My article, entitled “The naked experts,” was published in the British journal the *Ecologist*, in the July/August 1982 issue. More than a year later, Kemeny sent a letter to the *Ecologist* which attacked me and said that my article was “dishonest, defamatory and actionable.” Ironically, Kemeny's letter was much more defamatory of me than anything I had said about him. He didn't point out a single statement of mine that was defamatory. Kemeny demanded that I apologise in print. Instead, I wrote a letter in reply for the *Ecologist*. Kemeny's letter, mine and one by Mark Diesendorf were all published in the January-February 1984 issue.

The next communication from Kemeny was a letter to the editor of the *Ecologist*, Edward Goldsmith, in July 1984. He demanded a written apology — he sent a copy for us to sign — from the publisher and editor of the *Ecologist*, from me and from Mark Diesendorf and Rosemary Walters. The only connection that Mark and Rosemary had with my article was that they had read a draft and given me comments, as I indicated in an acknowledgment. This curious inclusion of Mark and Rosemary in the demand, and the style of Kemeny's letter, suggested to me and others who read it that he had not actually received detailed advice from lawyers. For example, he demanded that we admit that the article had been written with malice to defame him and that almost every paragraph contained “a plethora of mendacious, unresearched innuendo.” In addition, it would have been very expensive for Kemeny, living in Australia, to arrange for a defamation action to be launched in Britain where the *Ecologist* is published.

Edward Goldsmith was more rattled. He wanted me to supply documents that would enable the *Ecologist* to defend against a court action. So I made up a big bundle of photocopies of all relevant articles by Kemeny and others that were relevant to my writing of the article. It was a lot of work, but minor in comparison to what would have been involved in an actual court case. But Kemeny never sued. I presumed it was a bluff.

Perhaps when I originally sent “The naked experts” to the *Ecologist*, I should have warned Edward Goldsmith about Kemeny’s threat to sue. I assumed then that it was a bluff. I turned out to be right, but Goldsmith might have preferred to avoid the worry.

### **An advocate of nuclear power?**

In the opening section of *Nuclear Knights*, I gave an overview of the main issue in the debate over nuclear power. To provide a context for my analysis of the views of Sir Ernest and Sir Philip, I included a table of “Advocates of uranium mining and nuclear power prominent in the Australian public debate,” noting that almost all of them were either nuclear scientists, nuclear engineers or had links with uranium mining companies. One of them, Dr Don J. Higson, who worked at the Australian Atomic Energy Commission, had written pro-nuclear letters to newspapers. Higson wrote to me and we had a cordial exchange of letters. At least it was cordial at first. Higson claimed that he was not an advocate of nuclear power, but rather he was simply providing facts to the public about the issue and correcting other people’s mistakes. I disagreed, although I conceded that it might have been more accurate if I said that his letters had the effect of promoting nuclear power. Higson eventually wrote to Rupert concerning my statement that he was a public advocate of nuclear power, saying “I find this allegation offensive and consider it damaging to me.” For those familiar with the language of defamation, this was obviously a threat to sue. At this stage I discontinued the correspondence. Higson never sued. But that someone would find a statement that he was an advocate of nuclear power defamatory — in this case, defamatory to his reputation as an objective scientist — and ask that it be withdrawn, shows how the law of

defamation can extend to even apparently innocuous statements. This undoubtedly has a chilling effect, especially on publishers.

### **The deadly objection**

Through my years of action and writing against nuclear power, I got to know many of the people in the anti-nuclear cause, especially the scientists. One of them was Alan Roberts, who worked in the Physics Department at Monash University in Melbourne. In the mid 1970s, Alan had written some of the earliest critiques of nuclear power with an incisive political analysis.

In 1980 Alan wrote a review of a new book by Lennard Bickel, *The Deadly Element: The Men and Women behind the Story of Uranium*. The review was published in the *National Times*, a prominent weekly newspaper. Bickel sued for defamation. Most of the review caused no problem. The crucial sentence that triggered the suit was this: "I object to the author's lack of moral concern."

The case went to court. Bickel claimed, among other things, that the review meant that he lacked concern about the human consequences of nuclear weapons and nuclear power. The defence — lawyers working for the publishers of the *National Times* — said that the statement was "comment" rather than a statement of fact, and thus was permissible. Without going into details of trials and appeals, suffice it to say that Bickel eventually won in court, being awarded \$180,000 in damages. In a subsequent settlement, he received a somewhat lesser amount. It was not a happy moment for the publishers. Roberts did not have to pay anything. But it was a lesson for him too. He had to answer searching examination of the review in court. Afterwards, naturally, he was much more cautious about what he wrote.

### **A chill in the greenhouse**

My good friend Mark Diesendorf has seen his share of suppression. I met Mark soon after moving to Canberra in 1976. He had just joined the Division of Mathematics and Statistics in the CSIRO, the large Australian government research organisation. He had a PhD in applied mathematics and had worked the previous three years in the Applied Mathematics

Department at the Australian National University. When I joined that same department in 1977, I ended up in Mark's old office!

Mark took a keen interest in issues of health and the environment. In the early 1970s, he played a key role in drawing public attention to the health hazards of low-level ionising radiation, such as from chest x-rays. He was also a leading critic of the French government's testing of nuclear weapons in the Pacific. Mark became a highly effective opponent of nuclear power, using his expertise on both the social and technical issues to powerful effect. He went head-to-head in public debates against the likes of Sir Ernest Titterton and Leslie Kemeny. It was perhaps for this reason that when I contacted Kemeny at the beginning of my study of his views, he twice asked me whether I was working for Mark Diesendorf!

I learned a lot from Mark. We gave each other comments on letters that we wrote to the *Canberra Times* about uranium mining. Mark had a great talent for writing prose that covered the key points, was absolutely accurate and as brief as possible.

Mark was an enthusiastic promoter of energy efficiency and renewable energy technologies, so long as there was good evidence that they were effective. He soon became one of Australia's leading experts on wind power. In the late 1970s, we began a scientific collaboration on the potential role of electricity from wind generators in conventional electricity grids. Two others, John Carlin and John Haslett, were also involved at certain stages. Mark was the leader of the group, providing ideas and guidance but also being intimately involved in data analysis and mathematical modelling.

At the time, there was an organisation that funded energy research and development, called the National Energy Research, Development and Demonstration Council or NERDDC. Energy research in Australia was dominated by fossil fuel interests and the Australian Atomic Energy Commission. Renewable energy took a back seat. Although Australia has a great potential for using solar and wind energy, the existing energy system is largely dependent on coal and oil, and the relevant companies and government bodies preferred it this way. CSIRO was influenced by the same priorities.

Mark put in an application to NERDDC for wind power research. The application had to go first to CSIRO head office. He found out that head office had not even forwarded the application to NERDDC. After NERDDC itself put pressure on CSIRO, the head office forwarded the application and it was successful.

In 1981, Mark suggested that we submit a supplementary application with me as principal investigator, so that it could go through the university rather than CSIRO. Our application was unsuccessful, not too surprisingly, for I had received a copy of a letter from the ANU administration to NERDDC that had accompanied our application. Among other things, it said "Dr Martin's present appointment as Research Assistant in the Department of Applied Mathematics expires in January 1982." Due to financial uncertainties, "it is not possible to affirm that Dr Martin will remain in that position." This was true but also the kiss of death for a grant application.

Mark came under increasing pressure at CSIRO due to his research and public profile on renewable energy. In 1984 he was requested to transfer to Adelaide, a major move that would have separated him from his children. He refused to move and proceedings were begun to retrench him. As a result, in 1985 he lost his job but received a substantial pay-out.

These experiences are typical of situations in which suppression of dissent seems to be a factor, but there isn't really enough evidence to argue a convincing case. Maybe our grant applications weren't really all that good, but maybe hostility to wind power or to Mark's outspoken support for it played a role. Maybe Mark's transfer and retrenchment would have happened to him even if he had been a typical low-profile scientist working on noncontroversial topics. There is no way to know for sure.

Mark also became involved with the fluoridation issue. He undertook a careful investigation of the issue and by the mid 1980s had become one of the world's leading scientist critics of fluoridation. Not surprisingly, there were various attempts to discredit him and his work. Officials from the Australian Dental Association wrote to the chairman of the CSIRO (Mark's employer) and also to the federal government minister responsible for CSIRO, saying that he had "misused his CSIRO

connections to lend weight to his views on subjects outside his expertise” and asking for “all necessary steps to ensure this deceptive practice does not continue.” These officials did not write to Mark himself. He obtained the letters of complaint through requests using Freedom of Information legislation. CSIRO officials in this case defended Mark, pointing out that he had made clear in his public comments on fluoridation that he spoke in his “private capacity” and anyway he was an expert on some aspects of the issue.

Through all of his work on nuclear issues, wind power and fluoridation, Mark had no major encounters with the legal system. I have given this background to indicate that Mark is both a courageous critic but also an experienced and careful scientist. He is always disturbed by the less meticulous critics of nuclear power or fluoridation who get their facts wrong or propose wild conspiracy theories.

After a stint at the Australian Institute of Health, Mark joined the staff of the Australian Conservation Foundation (ACF), the country’s largest environmental organisation. He was in charge of the ACF’s climate change programme, a position that built naturally on his previous work. The key issue in climate change is the so-called greenhouse effect. Most researchers believe that carbon dioxide from burning of fossil fuels, plus other human activities such as clearing of rainforest, may be leading to major changes in climate.

However, there are a few critics of the standard view about the greenhouse effect. One of them is Dr Brian O’Brien, former chair of the Environmental Protection Authority in Western Australia. In 1990, Mark criticised some of O’Brien’s claims about the greenhouse effect. He also pointed out that O’Brien had been a paid consultant for the coal industry — remember that burning of coal produces carbon dioxide, a major contributor to greenhouse warming — and that this should be taken into account in assessing his views. This comment was similar to my analysis of the nuclear knights, in which I criticised their views and pointed out the connection between their views and their positions as nuclear scientists and engineers. Mark did not say anything about O’Brien’s motivations.

O'Brien responded by suing Mark and the ACF for defamation but not, interestingly, the newspapers that had reported Mark's comments. The ACF took on the defence, briefing its lawyers. As in many cases, it never went to court. After many months, a settlement was reached. The ACF published an apology. Why did the ACF agree to publish an apology? Basically, it would have been too expensive even to win the case. The ACF would have had to pay its own legal costs, and there was a slim chance of losing. The ACF is not a rich organisation. It receives most of its income from donations and subscribers. It could hardly afford a major pay-out. The expedient course was to settle the case and avoid further costs.

Naturally this was not welcomed by Mark. He felt he had been on solid ground in criticising O'Brien, but the big penalties available through defamation law were enough to make the ACF give in. The settlement made it difficult for Mark and the ACF to comment further about the key issue, namely O'Brien's links with the coal industry. In February 1992, Senator Peter Walsh wrote a column in the *Financial Review*, a national daily paper, in which he pointed out the ACF's apology, saying that Mark had impugned O'Brien's motives. Mark was constrained by the settlement from writing a reply. I wrote one myself. This is the way that free and vigorous discussion of social and scientific issues can be inhibited by the legal system.

I sent a draft of this chapter to O'Brien, inviting his comments. In reply he pointed out that he had publicly stated and published his views on the greenhouse effect before having any contact with the coal industry. In my view, this does not affect my assessment of the case.

Mark later left the ACF to take a position in the Human Ecology Program at ANU, a descendant of the Human Sciences Program, as described in chapter 1. After a few years in this precarious situation, in 1996 he was appointed professor and head of the new Institute for Sustainable Futures at the University of Technology, Sydney.

### **SLAPPING down critics**

In 1988 two professors at the University of Denver, Penelope Canan and George W. Pring, published an article in the sociology

journal *Social Problems* entitled “Strategic Lawsuits Against Public Participation.” Their acronym for these lawsuits was SLAPP. They examined 100 cases throughout the United States in which the legal system was used to harass and intimidate people who were exercising their right to petition the government.

In one case, a farmer made a complaint to the US Environmental Protection Agency about pollution of a river by a coal company. The company sued him for defamation, claiming \$200,000. In other cases, people who signed a petition against a real estate development were sued. Lawsuits have been filed against individuals and groups for all sorts of everyday actions, such as filing a complaint with a government agency, engaging in nonviolent rallies and marches, speaking critically at a meeting of a school board, or even signing the attendance sheet at a public meeting. The most common legal claim was defamation; others used include business torts, judicial process abuse and conspiracy. The amounts claimed are typically in the hundreds of thousands of dollars, up to many millions.

Very few legal actions of this sort are successful in court. That doesn't matter. Their main effect is to scare people, to make them afraid of speaking out. Those who have plenty of money can initiate legal actions. Those who don't are intimidated.

Canan and Pring's acronym SLAPP caught on quickly. They have continued to work in this area, producing valuable articles, circulating information and providing advice to people who have been SLAPPED. Many people who are subject to a SLAPP are stunned. Some of them back off, withdrawing their public statement or whatever. Others get angry. The value of the concept of SLAPP is that it puts individual cases into a bigger picture. Individuals realise that it's happening to others and that there's a pattern. They also learn how to oppose SLAPPs. Canan, Pring and their collaborators have led a push to oppose SLAPPs in a variety of ways, including laws against them and counterclaims against SLAPPers for injuries caused by the SLAPP.

Canan and Pring define SLAPPs as civil lawsuits, filed against nongovernment individuals or groups, claiming some injury due to communications aimed at influencing government on an issue of public concern. Brian O'Brien's legal action against Mark and the

ACF seems to fit this definition, except that the injury didn't arise from communication aimed at influencing government, but rather more general discussion of a scientific and social issue. It can be argued, though, that one aim of debate in the media is influencing government. There is considerable similarity to Canan and Pring's definition of SLAPP.

SLAPPs seem to be most common in the US, where the legal system is used to deal with all sorts of issues that are handled differently elsewhere. But cases that fit the pattern are found in other countries too. The legal system is used as a tool to inhibit free discussion.

### **Is this book defamatory?**

My aim in this book is to describe some of the things I've learned about how to oppose suppression of intellectual dissent. To do this, there is no special need to make risky statements that could be defamatory. On the other hand, I can explain things best if I'm able to state what I really believe, and sometimes that can lead to trouble.

Undoubtedly, this book has been censored, like many others. I decided to write it initially by including everything that seemed relevant to the points I was trying to make, with nothing excluded due to worries about defamation but nothing added just to be provocative. The text has been subject to two filters.

The first filter was my own judgement. If some points seemed too strong or not backed up by sufficient evidence, I deleted them. The second filter was the people to whom I sent draft copies. If they thought something was inappropriate or too risky, and I agreed with their judgement, I made deletions or changes. I sent draft copies to friends whose judgement I trust, such as Mark Diesendorf, and also to people likely to be critical, such as Leslie Kemeny (who, incidentally, didn't reply). I've also obtained informal legal advice — it's called informal because I haven't paid for it. Formal legal advice would be expensive.

If this book had been commercially published, there would have been a third filter: the publisher. At least one publisher was scared away by worries about defamation. The original idea for this book came from Souvenir Press in London. Editorial assistant Maggie Baddeley wrote in May 1993 saying they had seen my

article on suppression in *Newsweek* and asking whether I might be interested in writing a book on the topic. I wrote back with a list of ideas for such a book. Next I heard from Tessa Harrow, Editor at Souvenir Press, who said “The one big problem likely to arise with this sort of book is libel” and asked how I would handle this issue. I replied at length and later sent a chapter outline. Harrow wrote in October that “the book and its problems have been the subject of intense and lengthy debate” and that it would be impossible to publish it initially in the UK, though they might be interested in UK rights if it was first published in Australia.

This was discouraging, but much later I decided to write the book anyway. I eventually sent a draft to Souvenir Press asking whether they were still interested in UK rights assuming there was an Australian publisher. Souvenir Press didn’t answer my letter. This episode made one thing clear: if a commercial publisher had taken the book, quite a number of changes would have been required for legal reasons. Since *Suppression Stories* is essentially self-published, changes and deletions are due only to the first two filters.

However, rather than leave all these changes and deletions on the cutting-room floor, I’ve kept a record of the original version. It is safely stored with a friend in another country. This version is for posterity. Defaming the dead is not illegal, at least in Australia. I can say whatever I like about Sir Ernest and Sir Philip, but not about some others. Eventually the uncensored version will be available. The irony is that it may be unfair. When dealing with material where people threaten defamation, the usual process of checking facts and arguments is very difficult. Accuracy gets lost in the legal shuffle. Defamation law undermines the search for truth and hence results in greater misrepresentation in the long run.

## 5

# Peer review as scholarly conformity

Quality control. Who could disagree with that? When a scholar sends an article to an academic journal, the editor has to decide whether or not to publish it. The article has to be relevant to the subject matter covered by the journal, and also high quality. In what are called “refereed journals,” the editor relies on other scholars — the referees or reviewers — to judge the article. Each referee writes a report on the article, judging it in various ways, and recommends whether the article should be published unchanged, resubmitted with changes, or rejected outright. The editor weighs up the comments and recommendations of the referees and makes a decision.

The process can be quite elaborate. Some social science journals seek reports from four or five referees, and then after revisions there may be a second or even third round of refereeing. It’s said to be all in the cause of quality control.

I’ve had plenty of experience with the refereeing system as an author. Most of my articles in scientific journals — such as in mathematical modelling and in astrophysics — went through the refereeing process. This was straightforward and seldom caused problems. Scientific journals typically use one or two referees, and most of them publish a majority of articles submitted. The topics on which I was working were fairly orthodox, which may be another reason there were few problems.

In the social sciences, a smaller fraction of submitted articles are actually published. Typically, more referees are used and they are more likely to recommend rejection. Over the years, I’ve had dozens of articles published in refereed social science journals, but an even larger number of rejections.

The process of judging an article by sending it to referees is a form of what is called peer review — the referees and editor are said to be “peers,” namely people in the field with similar values

and standards. Peer review is also involved in assessing grant applications, job applications, promotions and book proposals at scholarly publishers.

Although the rationale for peer review is quality control, it's obvious that the process can be used to suppress dissent. It's a powerful method: peer review can be used to block publications, appointments, promotions and grants. Most importantly, it is very difficult to demonstrate that bias is involved. Usually referees are anonymous: only their reports are made available. Members of selection committees carry out their deliberations in secret: only a decision and perhaps a brief justification is needed.

It is very difficult to collect systematic information about the role of peer review in squashing dissent. But in the course of looking into suppression I've come across a few dramatic cases.

### **Research grants**

As you may remember, Clyde Manwell was threatened with dismissal after he and Ann Baker in 1971 wrote a letter to the *Adelaide Advertiser* about pesticides. Clyde had been a recipient of grants from the Australian Research Grants Committee, the main funder of university research in Australia. But then in 1972, just after the attack was launched on him, he was unsuccessful. He continued to be unsuccessful for the rest of the 1970s, even though his publications put him among the top 1% of scholars in terms of productivity. Clyde documented his experiences in an article published in the Australian science journal *Search* in 1979.

One of the more well-known cases of suppression via cutting off research grants involved Thomas Mancuso, an epidemiologist at the University of Pittsburgh. Mancuso was funded by the Atomic Energy Commission to study the effects of low-level ionising radiation on the health of workers at the AEC's nuclear reprocessing plant at Hanford, Washington. The project began in 1965, before the rise of popular concern about nuclear power. In the 1970s the issue of the health effects of low-level ionising radiation had become a hot potato for the promoters of nuclear power.

In 1974, another researcher, Samuel Milham, published findings showing an increased risk of cancer among Hanford workers. The AEC requested that Mancuso repudiate Milham's findings, but Mancuso refused on the grounds that his study was not yet complete. So the AEC organised a review of Mancuso's project. Citing two unfavourable reviews, one of which recommended termination and transfer of the project, the AEC terminated Mancuso's work and transferred the work to Battelle West, a private contractor.

On the basis of this information, there can be little more than a suspicion of foul play. But because of the politically sensitive issues involved, the termination of Mancuso's project was investigated by Congress. It turned out that there were actually six reviews of the project, not just two. Four of the six reviewers were favourable; the AEC had cited only the two unfavourable ones. Furthermore, the director who took over the study at Battelle West was a former employee at the AEC who was the very same reviewer who had recommended termination and transfer.

In each of these two cases, the person receiving the research money was doing work potentially threatening to vested interests, namely the pesticide establishment in Manwell's case and the nuclear establishment in Mancuso's case. In each case, the denial of research grants occurred in the context of a highly contentious social issue in which there was a pattern of suppression. Finally, in each case the apparent bias in research funding could be exposed through inconsistencies in peer review.

Manwell had an outstanding publication record, which was dependent on favourable reports from journal referees. Why were journal referees so favourable and grant referees negative? A plausible explanation is that the journal referees — most of whom were from other countries judging an article submitted to a journal published outside Australia — knew nothing about Manwell's activities and just judged his work, whereas the grant referees — or the grant body's panel members — were local scientists who were prejudiced by the stigma attached to Manwell's activities or the attack on him.

Similarly, Mancuso's project was judged favourably by a majority of the reviewers, throwing into question the AEC's

action. In each of these cases, ironically, it is peer assessments that can be used to expose apparent biases in peer review.

It is only very blatant cases that can be exposed in this fashion. In many other cases, there may be a suspicion that suppression is responsible, but no way to get further than this.

Peter Springell referred me to a 1974 article in the *British Medical Journal* by David Horrobin, who documented bias by research grant referees against innovative applications. Horrobin was able to obtain referees' reports for quite a few unsuccessful applications. Horrobin's evidence was the best I had seen. Indeed, it's still the best I've seen. It seems that investigations of bias in awarding research grants are few and far between.

Most scholars are unwilling to make a big issue about biases in research funding. They are afraid that they will obtain a reputation as a troublemaker and be unable to obtain funding in the future. A common informal view is that it is easier to obtain funds for conventional projects. Those who are eager to get funding are not likely to propose radical or unorthodox projects. Since you don't know who the referees are going to be, it is best to assume that they are middle-of-the-road. Therefore, a middle-of-the-road application is safer. It's difficult to say whether this view is correct, but many people believe it to be so and the few obvious cases of suppression don't help to change it.

### **Dental profession dominance**

When a particular viewpoint holds sway through an entire field of study, it is difficult indeed for challengers to gain a hearing. The dominance of profluoridation views within the dental profession is a good example. From the 1950s, when fluoridation became accepted and promoted by dental associations in most western countries, until today, it has been extremely difficult for anyone to publish an article critical of fluoridation in any dental journal. This also applies, to a lesser extent, to medical and scientific journals, where profluoridation editors and referees often hold sway as well.

However, it's hard to prove rejections of antifuoridation articles are due to profluoridation bias. After all, the antifuoridation articles submitted may have been no good. And, to be sure,

there are plenty of poorly argued antifluoridation writings around. Nevertheless, there are a few suggestive bits of evidence.

George Waldbott, the most prominent and influential opponent of fluoridation in the US for several decades, wrote numerous scientific papers. He also sometimes encountered difficulties getting his articles critical of fluoridation published. One revealing indication of the source of his problems came at a court hearing in Dublin. Being quizzed by a lawyer on his testimony, he was asked “How did it happen that the *Journal of the American Medical Association*, the *Annals of Internal Medicine*, the *Journal of Gerontology*, and *Annals of Allergy* turned down your articles on fluoride poisoning?” This question was an obvious attempt to undermine his credibility as a competent scientist. But the question revealed something else besides its intent. Waldbott noticed that the four journals mentioned were the only ones that had ever rejected any of his submissions. But how would the lawyer know about the rejections? US Public Health Service officials were there in the courtroom advising the lawyer. Waldbott concluded that the editors must have used USPHS officials as referees and then told the USPHS about the rejections.

One of the leading critics of fluoridation in Australia is dentist and researcher Geoffrey Smith. When I interviewed Australian profluoridationists, one of them told me that Smith couldn’t get anything published in scholarly journals, but only in the unrefereed letters-to-the-editor section of the *New Zealand Dental Journal*. Smith himself told a different story. He said that he was given an extremely hard time by referees for the *Australian Dental Journal* and so was unable to get anything published there. But he said he had no difficulty getting articles critical of fluoridation published in international (that is, non-Australian) refereed scientific journals. He sent me a pile of them.

Mark Diesendorf told me about his difficulties getting antifluoridation articles published. For example, on one occasion he submitted an article to the Australian journal *New Doctor*, which is mildly critical of the medical establishment. A guest editor rejected it because “it might encourage the antifluoridationists.” Mark was told about the rejection over the phone, never receiving a written reply.

Although Mark encountered immense obstacles in getting his articles on fluoridation published, he kept trying. His greatest triumph came in 1986 when his article “The mystery of declining tooth decay” — which argued that the evidence that fluoridation was responsible for major reductions in tooth decay was inadequate — was published in *Nature*, one of the world’s most influential scientific journals. This article, with its visibility and prestigious location, gave the antifuoridation cause an enormous boost. The profluoridationists did what they could to undermine it. Rather than write a reply for publication, which would have admitted that the issue was worth debating, Graham Craig of the Sydney University Dental School wrote a rebuttal that was circulated in profluoridation circles. When I interviewed Australian profluoridationists, one of them told me that Mark’s article was not refereed. Mark sent me the referee’s report. Another profluoridationist told me that Australian scientist Michael Briggs — later exposed for scientific fraud — had published in *Nature*, implying that Mark’s article might be little better.

Even a single article in a key professional journal, such as Mark’s article in *Nature*, can have an enormous impact when the issues are hotly contested and one side is excluded from presenting its case. The antifuoridationists scour the scientific literature looking for any findings that might support their cause. No wonder editors and referees occasionally admit their concern that publication of an article might aid the antifuoridationists. Of course, the usual referees’ reports never say such a thing. They are couched in terms of scientific and other inadequacies of the paper. Furthermore, probably the usual motivation for rejecting an antifuoridation article submitted to a journal is simply that the evidence and logic don’t measure up. No conspiracy theory is required. Most profluoridationists genuinely believe that there is little or no substance behind criticisms of fluoridation. You can call this suppression, but perhaps a better description is domination by a standard viewpoint.

When one viewpoint is so dominant that critics face enormous obstacles getting articles published, the temptation is to not even try. There is a journal, *Fluoride*, that regularly publishes scientific work critical of fluoridation. It is relatively easy to publish criticisms there, so why hit your head against a brick wall by

submitting articles to dental journals? Because so few critical articles are submitted to mainstream journals, it is easy for supporters of the standard view to say that there is really no critical work of substance to publish anyway.

In investigating suppression, it is very difficult to document cases of bias in peer review. The fluoridation debate is a good place to find examples because there is a pattern of other types of suppression and a reasonable explanation of why suppression should occur. Even so, the cases of peer review bias usually depend on revealing discrepancies in peer review. Waldbott was able to publish in lots of journals; the profluoridationists knew about each of his rejections. Geoffrey Smith could publish in numerous non-Australian scientific journals but not in the *Australian Dental Journal*. Mark Diesendorf published his work in *Nature* and other scientific journals but had enormous difficulty when submitting to dental and medical journals.

Fluoridation is only one area out of many where dissenting ideas are prevented from appearing in journals. But who is to say that dissenting ideas are any good? I'm occasionally contacted by people who believe that their brilliant discoveries are being suppressed. I try to be open-minded. But I always keep in mind that just because an idea is rejected does not mean that suppression is involved. Some of the writings that have come my way are incoherent and illogical, at least in my judgement. Nevertheless, there might be a grain of truth in even the least plausible claim. I have only a finite amount of time and energy and therefore have to choose carefully which cases to pursue.

### **An AIDS orthodoxy**

One of the original cases of suppression of environmental scholarship that I studied was the attempt to block publication of *Fight for the Forests* by philosophers Richard and Val Routley. In the years since I have kept contact with each of them. To make things confusing, they each changed their last names. Val became Val Plumwood and Richard became Richard Sylvan.

In 1990 Richard sent me a bundle of material that he had received from Louis Pascal. Pascal had developed a theory on the origin of AIDS, but had had difficulty getting it published. The editor of the philosophy journal *Inquiry* — which had earlier

published two of Pascal's articles — suggested a few people who might help in getting his new work published. One of them was Richard.

I wrote to Pascal in April 1990, telling him about my work on intellectual suppression, making some suggestions for getting his work published and sending copies of some of my writings. Thus began an ongoing correspondence and a major project for me.

The conventional theory of AIDS is that HIV (human immunodeficiency virus), the virus thought responsible for AIDS, came to humans from monkeys or chimpanzees, who have similar viruses called SIVs (simian immunodeficiency viruses). Transmission is thought to have occurred by a hunter getting monkey blood into a cut, through a monkey bite or some other such means. It's supposed to have happened in Africa, probably around the late 1950s according to the rate at which variants of HIV have been evolving. After the initial transfer to humans, HIV spread through human-to-human contact.

Pascal's theory was that AIDS originated from contaminated polio vaccines used in Africa in the late 1950s. The vaccines were grown on monkey kidneys, as they still are. The vaccine thus might have been contaminated by SIVs. A particular batch of Hilary Koprowski's vaccine was given to hundreds of thousands of people in central and west Africa from 1957 to 1959, in the regions that now have some of the world's highest rates of HIV infection. Furthermore, the vaccine was given to many small children, including ones less than a month old. The significance of this is that children's immune systems are undeveloped. Depressing the immune system is one of the ways used to transfer viruses from one species to another. Needless to say, there's much more that could be said about the issue, but this gives a feeling for what's involved.

Pascal developed his theory in 1987 and soon wrote a short article making the case. The issue is not just of historical significance. If AIDS came from contaminated polio vaccines, then there remains a danger that other viruses are being transferred to humans through vaccines. Furthermore, operations such as transplanting a baboon liver into a human — which requires drugs to suppress the immune system — may be a means for other

species-to-species virus transfers, with potentially devastating consequences.

Pascal sent his papers to leading scientific journals: *Nature*, *Lancet* and *New Scientist*. *Nature* rejected it with only brief explanation. *Lancet* rejected it without giving a reason. *New Scientist* replied two years later, saying that the article was being refereed, but didn't write again.

These rejections don't prove a lot. It's very difficult to get published in any of these journals at the best of times. Pascal faced extra difficulties. He wrote from a private address in New York City. Some editors assume that any submission from a private address is no good. After all, they reason, any scholar of quality should be working for a university or some other institutional employer. Another difficulty was that Pascal's papers weren't perfectly in the scientific mould. They weren't exactly in the formal, impersonal, logically structured style that is expected in scientific journals, although they weren't too far off.

Most editors and referees expect that anyone wanting to have their work published must adapt to the system. Authors are expected to write in the standard style, to cite other work in the usual fashion and to prepare their submissions in orthodox fashion. Anyone who does things a different way is likely to be rejected out of hand. That may be what happened to Pascal.

Pascal's view was different. In his view, he had proposed a theory that, if correct, would point to the need for immediate action to stop further diseases through simian-to-human virus transmission. In addition, it offered insights into how to deal with AIDS, for example by finding monkeys or chimpanzees with SIVs similar to HIV and seeing how they survive with the infection. Pascal believed that editors had a responsibility either to reject his theory on the basis of specific refutations or to publish it. Whether or not he wrote in the standard fashion was a side issue when millions of human lives were at stake.

A colleague sent me an article by David Horrobin in the *Journal of the American Medical Association* that made this point well. Horrobin argues that the point of peer review, at least in biomedical science, should be improving care to patients, not quality control.

Although Pascal's submissions may have been rejected just because of his lack of an institutional address and the style of his writing, there was another explanation. Pascal's theory was very threatening to the scientific establishment, especially medical researchers. If it was accepted that polio vaccinations had led to the deadly disease AIDS, it would be an incredible blow to the prestige of medicine. The credibility of vaccinations would be undermined, and many more people might refuse to be vaccinated.

In medical circles, vaccination is virtually unquestionable. There are a few critics, but they are mostly outside the medical research community. Certainly among my peers in the social science community, vaccination is defended most vehemently. On a computer conference dealing with social aspects of science and technology, the subject of vaccination came up in 1993. The very idea that parents might not have their children vaccinated was greeted with outrage by some scholars on the conference. And they are the ones who are supposed to be willing to study the evidence as well as vested interests on both sides of issues.

Pascal wasn't the only one having problems getting his work about polio vaccines and AIDS published. Two professors from South Africa, Gerasimos Lecatsas from the Department of Virology at the University of Southern Africa and Jennifer Alexander from the Department of Microbiology at the University of the Witwatersrand, had also encountered difficulties. Their comments about polio vaccines and the origin of AIDS were much briefer, less specific and more tentative than Pascal's. But even their short comments about a possible link were rejected by several journals. One of their submissions on the topic was published as a letter-to-the-editor in the *South African Medical Journal*. A group of scientists responded in a later issue by calling Lecatsas and Alexander's letter "reprehensibly irresponsible misinformation" and "recklessly wild and unscientific information." This sort of rhetoric gives some idea of the passions aroused by this theory.

One of Pascal's correspondents sent his article to the *Journal of Medical Ethics*, whose editor asked Pascal to write a different article for submission there. Pascal wrote a long and passionate article. It was too long. In May 1991, the editor, Raanan Gillon,

wrote to Pascal in a rhetorical overstatement that “There is just no way that I can publish a 19,000 word paper even if I thought that it was going to save *millions* of lives as you suggest (and I have to say that I remain unconvinced by this speculation).” Gillon underlined the word “millions.”

In my correspondence with Pascal, I offered to arrange publication of his paper if he was unsuccessful elsewhere. This was not an offer made lightly. There were several reasons why I was inclined to help promote Pascal’s work. First, his letters and articles revealed a keen, logical, meticulous intellect. When I or someone else raised a query about some small component of his analysis, Pascal would reply with detailed logical arguments and references to relevant evidence. He had thought through his ideas far more carefully and comprehensively than most scientists I had met.

Second, Pascal’s theory had highly important social implications. It concerned a deadly disease and possible new diseases. It also had implications for medical research and peer review. Third, Pascal’s article was well written, engaging, dealt with both scientific and social issues, and raised perspectives of interest to social analysts of science. Finally, Pascal had tried unsuccessfully to publish his work in scientific journals. Thus there were plenty of reasons for me to put time and effort into publishing and promoting Pascal’s work. At the time, though, I didn’t anticipate how much time and effort would eventually be involved!

At the University of Wollongong, a certain amount of university money is allocated for research, most of which goes to groups of researchers. I was in a group called Science and Technology Analysis. One thing we did was produce a series of “working papers,” usually but not always written by members of our group, which we could circulate to interested people. I arranged for Pascal’s paper, rejected by the *Journal of Medical Ethics*, to be published in the working paper series. I did the work to get the text in the format for working papers, checking it all with Pascal. It was printed and ready for distribution in December 1991.

I began by sending copies to 25 people whose names were given to me by Pascal. I also sent copies to colleagues interested in intellectual dissent, to various journals, to science journalists

and to people who wrote in for their free copy. Pascal's paper was a hit. I sent out hundreds of copies and some recipients made lots of photocopies themselves for further distribution. This was one way around the journal rejections.

Quite independently of Pascal, the same theory was developed years later by Blaine Elswood, an AIDS activist from San Francisco. Elswood knew an investigative journalist, Tom Curtis, and tried to get him interested in pursuing the story. Curtis in turn encouraged Elswood to write up and publish his work in a scientific journal. Curtis finished first. He put in an enormous effort investigating the issues, developing the ideas further and interviewing leading researchers. His article "The origin of AIDS" appeared in *Rolling Stone* in March 1992. *Rolling Stone*? Yes, it's a rock magazine. It does run some "serious" articles. And because of its large circulation, it has an enormous impact. Soon there were stories in major newspapers and scientific journals. *Nature* would not publish Pascal's article but it ran a story about the *Rolling Stone* article. Of course, most prominent scientists who were quoted opposed the theory. But at least it was on the agenda.

I wrote to Elswood and then Curtis to put them in contact with Pascal. In fact I corresponded with lots of people about the issue, as well as sending out copies of Pascal's article. In this way I gained a good idea of what was going on concerning the theory. One key development was that Raanan Gillon, editor of the *Journal of Medical Ethics*, wrote an editorial in which he explained why the *JME* had not published Pascal's article. He recommended it as worthy of serious consideration and gave full details about how to obtain it from us at the University of Wollongong. So although *JME* did not publish Pascal's article, our independent publication of it, followed by the *JME* editorial, made many people seek it out.

Meanwhile, Elswood collaborated with Ray Stricker to produce a technical article describing the theory. They submitted it to *British Medical Journal*, where it was rejected. They then tried *Research in Virology*. In February 1992, famous AIDS researcher Luc Montagnier wrote back very encouragingly, implying that it would be published. Months passed. Eventually the board of *Research in Virology* said that they would only publish a much

shorter, letter-length submission. In addition, Elswood and Stricker were asked to remove all their discussion of monkey virus SV40 which was known to have contaminated polio vaccine given to millions of people worldwide. (SV40 is different from SIV, the virus thought to have been transmitted to humans to become HIV.) Elswood and Stricker complied. After further delays, their letter appeared in mid 1993, accompanied by a note from the editors that challenged and disowned Elwood and Stricker's claims. This theory was not welcome!

Elswood and Stricker sent their original paper to the journal *Medical Hypotheses*, edited by David Horrobin, a long-standing critic of many features of science. I had corresponded with Horrobin concerning intellectual suppression and met him in Sydney one time when he was attending a conference there. Horrobin set up his own pharmaceutical company and yet kept up with his research. He established *Medical Hypotheses* to provide a forum for unorthodox ideas. Elswood and Stricker's article was published in *Medical Hypotheses*, but not until after more than a year's delay.

However, these problems were nothing compared to what happened to Tom Curtis. In December 1992, Curtis and *Rolling Stone* were sued for defamation by Hilary Koprowski, the scientist who developed the polio vaccine used in Africa from 1957 to 1960. This was the vaccine that Pascal, Elswood and Curtis said might be responsible for starting AIDS.

Koprowski's lawsuit had the effect of shutting down most media discussion of the theory. It also was oppressive for Curtis, who had to supply copies of all notes, correspondence and tapes made in researching his article in *Rolling Stone*. After this, he had a hard time pursuing the polio-vaccine-AIDS story, because he had to tell any informants that their comments might end up with Koprowski's lawyers. He was a freelance journalist, and the case took up time when he could have been researching other stories and making money. The one saving grace was that *Rolling Stone* covered legal fees. And they were hefty. A year later, the case was settled before testimony even began. Even so, *Rolling Stone's* legal costs amounted to \$500,000. The settlement involved a payment of the grand total of \$1 to Koprowski and publication

by *Rolling Stone* of a “clarification” that Curtis considered to be grossly misleading.

Can Koprowski’s lawsuit be called a “strategic lawsuit against public participation”? Not according to Canan and Pring’s definition of a SLAPP. But there are similarities. It certainly had the effect of shutting down public discussion. Curtis had prepared a second article on AIDS, but *Rolling Stone* dropped its option to publish it.

I decided that just distributing Pascal’s article was not enough. In mid 1992 I wrote an article called “peer review and the origin of AIDS,” covering some of the problems faced by the polio vaccine theory. After getting comments on a draft, I sent it to the *British Medical Journal*, which promptly rejected it. Then I tried *BioScience*, a general interest journal mainly aimed at biological scientists. *BioScience* has a feature called “Roundtable” in which opinion pieces are presented. To my delight, my article was accepted. The one “adviser” was favourable. Perhaps it is easier to publish an account about the reception to a challenging theory than to publish an account of the theory itself.

*Science*, the most influential scientific journal published in the US, remains hostile to the theory. It published a highly critical news story about the *Rolling Stone* story. Curtis then was able to get a letter published in *Science*. Koprowski responded with a long and condescending letter. *Science* then refused to publish Curtis’s point-by-point rejoinder.

One supporter of the polio-vaccine-AIDS theory is W. D. Hamilton, professor of zoology at Oxford University and an eminent evolutionary biologist. Hamilton wrote a letter to *Science* pointing out errors in Koprowski’s letter and arguing that the theory warranted consideration. Hamilton’s letter was rejected. Then he wrote a personal letter to Daniel Koshland, editor of *Science*, making a strong appeal about the importance of open discussion of the theory. This letter was the most eloquent that I had read for many a month. But it was unsuccessful. Koshland refused to publish Hamilton’s letter.

### **The power of editors**

Koshland, like the editors of other major scientific journals, has enormous power. By choosing referees and by making decisions

about controversial submissions, such editors have a great influence on the credibility of different viewpoints. When John Maddox, editor of *Nature*, accepted Mark Diesendorf's article, he gave a giant boost to the critics of fluoridation. Koshland's rejection of all responses to Koprowski's letter is more typical in its perpetuation of orthodoxy.

Investigating biases in peer review is not an easy task. When there is a wider pattern of suppression, then it is reasonable to expect that there will be biases in peer review, but only sometimes is there any evidence that is more than suggestive. Even then, the biases are most easily exposed when there are inconsistencies in peer assessments, for example between countries or different types of journals.

Editors not only have enormous power, but they seldom are subject to peer review themselves. Some of them keep their positions for decades. Potential authors may complain privately about inconsistencies and bias, but they are seldom willing to say anything openly. Their fear is that if they did, they would be discriminated against. As in the case of other types of suppression, the fear of stepping out of line has a much greater effect than the few attacks on dissidents that do occur.

## 6

### **“Proper channels” just don’t work**

When dissenters first come under attack, often they have a strong impulse to seek redress through “proper channels.” This includes appeal procedures, grievance procedures, writing letters to top management, and seeking support from trade unions or professional bodies, ombudspersons, official tribunals and the courts, among others. Time and time again I’ve seen these methods tried. Time and time again I’ve seen them fail, either by giving a negative decision or by interrupting and diverting the flow of an effective campaign.

When people come to me for advice about challenging suppression, I usually warn them about the limitations of formal channels. Seldom are my warnings heeded. Most people seem to have an intense desire to believe that the formal structures in organisations and society can provide justice. Many dissidents speak out precisely because they believe that if they speak the truth, people will listen and take action. They are shocked when the response is to attack them instead. Yet they retain their belief that someone somewhere is looking out for injustice and can right the injustice. It is a dangerous illusion.

Sometimes, of course, official channels do work. Sometimes it is wise to use them, often as part of a wider campaign. I do not say to *never* use official channels. But it is important to realise all their disadvantages, and not to expect any solutions.

In many cases, official channels seem to work, but actually the success is mainly due to a campaign. When Jeremy Evans applied for tenure at the Australian National University, the reappointments committee denied it. Then he went to a review committee, which reaffirmed the decision. Then, following the official channels, he went to an appeal committee, which couldn’t reach a decision. Jeremy was given two more years and then was successful with a new tenure committee. Was this success

through formal channels? Hardly. Without the massive campaign in defence of Jeremy and Human Sciences — letters, meetings, petitions, media stories — it’s likely that the tenure denial would have been backed up by all committees. Admittedly, there’s no way to prove this. The people on the various committees are hardly likely to admit it, and anyway the influence might not be at the conscious level. It’s simply my experience that without a campaign, the formal channels are usually useless.

With a campaign, formal channels may not even be necessary. Politicians and top administrators can always intervene if the urgency is great enough. A noisy campaign is more likely to trigger their involvement than a case following standard bureaucratic protocol.

### **The Spautz case**

In June 1980 I received a letter from Michael Spautz, a senior lecturer in commerce at the University of Newcastle. At least he had been a senior lecturer. He had been dismissed a few weeks previously. He had heard about my work on “railroading of academics,” asked for a copy of an article of mine and told me a bit about his experiences. I promptly wrote back, expressing interest but commenting that his case was different from the ones I had studied. Spautz then sent a bundle of material, and I became involved with what was to be one of the most tortuous cases at an Australian university.

The events that led to Spautz’s dismissal were connected with Alan J. Williams, who was appointed to a professorship in the Department of Commerce in 1977. Spautz had joined the department a few years earlier. There were no problems until 1978, when Spautz objected to new administrative arrangements that put Williams in charge of one of the two sections in the department. At about this time, Spautz raised questions about Williams’s PhD thesis — completed not long before his appointment — alleging that it was seriously flawed in its methods and conclusions, due to “spurious statistics and inverted causality.”

What are the proper channels for raising concerns about your boss’s PhD thesis? The first and very proper step that Spautz took was to discuss them with Williams himself. This led

nowhere. Spautz wrote two short rebuttals of the thesis and submitted them to journals. *Rydge's* rejected Spautz's article on the grounds that it was defamatory. *Real Estate Journal* rejected Spautz's submission on the grounds that readers would not remember Williams's article in the same journal, published a few years earlier. So much for the proper channels.

It is worth noting that Williams had only recently been awarded his PhD, in 1975. When he was appointed to the second chair in commerce, his publication record was sparse: he had only published a couple of articles. The 750-page thesis was his major piece of scholarly work.

Instead of giving up, Spautz continued to try to expose what he saw as inadequacies in Williams's PhD thesis. In 1979, he added a new charge. He claimed that Williams's thesis contained plagiarised passages. Specifically, he pointed out that Williams seemed to have copied quotes and their sources from secondary sources, instead of looking up the original sources — and that he didn't give citations to the secondary sources.

Spautz went to the University of Newcastle administration with his concerns about Williams's thesis. He was told it was a matter for the University of Western Australia, where Williams had received his PhD. An official at the University of Western Australia replied that responsibility lay with the examiners of the thesis. They were anonymous. Spautz had reached the end of the line.

Getting no satisfaction from Williams, from journals, or from the University of Western Australia, Spautz began spreading his allegations to more and more people around campus. The University of Newcastle administration set up a committee to look into the problem — which it defined as the problem of Spautz's behaviour. At no stage did any committee look into Spautz's allegations about flaws or plagiarism in Williams's thesis.

On the committee's recommendation, the University Council essentially told Spautz to shut up. He didn't respond well to this, and instead escalated his claims. He began his "snowflake campaign," so-called because he circulated memos to academics and others nearly every day, covering the campus like snow.

Since it never snows in Newcastle, some academics there may have a curious view of a snowstorm.

Another university committee was set up. It found that Spautz had disobeyed instructions from Council, the university’s ruling body. The Council dismissed Spautz from his tenured job on 23 May 1980.

There were a lot of problems with the dismissal process. The University of Newcastle Council had dismissed Spautz without formally charging him with misconduct or giving him a chance to make a full and effective defence. To challenge this dismissal, Spautz again tried formal channels. This time it was the courts.

At this stage the story gets messy — and I’ve described only a fraction of what happened before the dismissal. Spautz often acted as his own advocate in court and became a self-taught expert on the law. He launched legal actions against the university for wrongful dismissal and against various individuals for defamation, as well as many other charges.

Spautz sent me lots of documents and put me on his mailing list for his memos, which he continued to produce in great numbers. Every few weeks or months I would receive an envelope stuffed with memos. In certain periods Spautz produced one nearly every day, at least in the early stages.

Not knowing much about small business failures, I couldn’t easily judge the validity of Spautz’s claims about flaws in the methods and conclusions in Williams’s PhD thesis. I could, though, check out the allegations of plagiarism. I wrote to Williams for a copy of his thesis, but he didn’t reply. So I took up Spautz’s offer to loan me one of his copies. I also obtained copies of the sources cited by Williams and of the secondary sources pointed out by Spautz. All indications showed that Spautz was right: Williams had quoted sources that he had, by the evidence, not consulted. As far as plagiarism goes, it was not especially serious, but it could indeed be called plagiarism. I wrote up a document giving specifics about Williams’s use of sources.

In April-May 1981 I made a three-day trip to Newcastle. I stayed with my friend Dave Blatt and his wife Betty and their children. Dave and I did our PhDs in the same department at the same time, in Theoretical Physics at Sydney University in the

early 1970s. Dave had moved out of nuclear physics into computer science. He did a lot of work to arrange my visit. I gave three talks at the university, on wind power, nuclear knights and suppression of environmental scholarship.

While in Newcastle, I took the opportunity to talk to various people about the Spautz case — not least Spautz himself. He turned out to be a confident and articulate fellow who was completely sure about the justice of his cause and about his course of action. I advised him that he was unlikely to obtain justice through the courts, and that it would be better to document his case and build support in order to expose the corruption that he saw at the university. He said others had given him the same advice. Spautz listened and told me that he wanted to do it his way. The courts it was.

Spautz was passionate about his case. Some people called him obsessed. He called it his campaign for justice. Others called it his campaign against Williams. Spautz's intensity about the case soon alienated many of those who would otherwise have been his supporters. He complained to the journalists who reported his case, and consequently coverage dropped away. It could be said that he was his own worst enemy. In taking up the case, I had an advantage. I lived in Canberra, far away from the action and far away from Spautz's strong personality. At a distance it was easier to assess the issues of importance.

In the years since, I've often found that some distance is an advantage in investigating cases. Those who are right in the middle of the action are so affected by the personalities and events that it is hard to focus on issues of principle. On the other hand, being too far away — so that it is impossible or difficult to actually talk to people, either face-to-face or at least by telephone — is also a disadvantage. Assessing a case only via documents is risky, because there are often important things that people will say but not write down. Canberra, about 400 kilometres from Newcastle, was a good distance for my investigation.

Some of Spautz's views made it difficult for others to support him. He refused to join the University of Newcastle Staff Association because it seemed too political to him; as an industrial psychologist, he wanted to be objective on union-

management issues. In spite of this, the executive of the Staff Association made a strong statement about Spautz's dismissal.

Spautz had come to the University of Newcastle in 1973, from the United States. It is easy to say that his challenge to Williams and to the university administration was in some way linked to his status as an outsider. It is certainly my impression that immigrants are more likely to see problems in a society and are less sensitive to the cultural cues that usually inhibit challenges to them. But to trace Spautz's actions just to his background and psychology is to explain away what was really interesting about the case: the response of the administration.

The most significant impression I gained from my visit to Newcastle was how afraid many people were. Some did not want to speak to me at all. Others were cautious about what they said. Spautz had been dismissed. Most of those who saw some injustice in this were reluctant to say so publicly. Perhaps they were afraid Spautz would seek them out as an ally, or perhaps they were afraid that they would be victimised by the administration.

Afterwards, I had enough material for a long article about the Spautz case. I circulated it for comment in the usual fashion. Alan Williams didn't reply, and indeed he never replied to any of my letters. In September 1981, I submitted my article to *Vestes*, the journal of the Federation of Australian University Staff Associations (FAUSA), the national professional body which later became the national union of Australian academics. The editor wanted something shorter. So I divided the paper into two parts. In December 1981, I sent *Vestes* the part about the issues surrounding Spautz's dismissal. It was accepted for publication, but had to be checked by FAUSA's lawyers for defamation. In August 1982 I was told that the legal advice to FAUSA was that my article shouldn't be published until court cases involving Spautz were over. This was a prescription for indefinite delay! Also, I protested, I had received informal legal advice that the only person who could be defamed by my article was Spautz himself, and he was hardly likely to complain. After quite a number of letters and phone calls to George Szlawski, FAUSA's industrial officer, to work out changes in the article to avoid defamation, the article was finally published in May 1983.

The other part of the article was about issues to do with plagiarism, using Spautz's allegations about Williams's thesis as a case study. This was a hot topic, to say the least. I sent it to journal after journal, mostly education journals, and received rejection after rejection, usually with no comments of substance. One of them was *Discourse: The Australian Journal of Educational Issues*, a progressive education journal published out of the University of Queensland. The editor, Ted D'Urso, asked my permission to send it to Alan Williams for comment. I readily agreed, and explained the context of the paper. Before long I received a cold rejection note. I rang D'Urso to ask what had happened. He said that the article would have been published if "everything had been in order," but in light of the reply from Williams and advice from the University of Queensland legal office, the editorial committee had decided not to proceed.

Finally I had success. The *Journal of Tertiary Educational Administration*, published in Australia, agreed to publish a revised version. I had to drastically reduce the material about Spautz and Williams, partly because the editorial board was concerned about the ongoing court cases and thought that "it might be imprudent to publish an article of this nature at this time." But at least the article appeared. It had been rejected by a total of nine journals. As I said in the article, plagiarism is a taboo topic.

Meanwhile, Spautz continued his long march through the courts. He had numerous different cases going at the same time. When one was thrown out, he would appeal. The most dramatic development came after he lost one case. Court costs were awarded against him, which meant that he had to pay \$5000. He refused out of principle. In any case, he had no money, as he was living on unemployment benefits. The magistrate sentenced him to 200 days in prison. But the Supreme Court ruled that the imprisonment was unlawful and he was let out after 56 days. He promptly sued for false imprisonment but for technical reasons was awarded only a token two cents.

Spautz spelled out the latest news about the court cases in his memos, which gradually became less frequent. The courts are slow moving at the best of times. Spautz's cases dragged on and on. It took some ten years for all his avenues of appeal to be exhausted. The university spent a fortune in legal fees, while

Spautz devoted all his energies to this endeavour. What a waste! But there are a few lessons from this saga.

University administrators should have learned to be more careful before dismissing someone. At least that is what I tried to say in my article in *Vestes*. An important lesson for me was that formal channels don’t work when challenging a more powerful person or organisation. When Spautz tried to raise concerns about Williams’s thesis, he got nowhere with Williams, with journals or with the university that granted Williams’s PhD.

Spautz also tried formal channels, namely the courts, in challenging his dismissal. He certainly caused a lot of trouble, but arguably the whole effort was counterproductive. He alienated supporters rather than building a support network. As the cases became more and more complicated, almost no one could understand the technicalities. Journalists couldn’t spend the time to understand the legal niceties, and the cases weren’t very newsworthy anyway. With defamation suits right and left, people were wary that they might be sued. Better to stay out of it altogether. As I learned through trying to publish articles about the case, an ongoing court action greatly inhibits discussion of the issues. Finally, the court cases took the focus further and further away from the key issues of the allegations against Williams, Spautz’s dismissal, and the accountability of various groups for investigating charges. I put a lot of effort into investigating the case, but as the court process became more and more complicated, I lost track and lost interest. All these negative factors would have been bad enough even if Spautz had won in court. But he lost there too. At least he did it his way.

### **Trying to expose scientific fraud**

Spautz came to grief after he criticised the work of a colleague with a higher rank. Others have had a similar experience.

Michael Briggs was professor and dean of science at Deakin University in Victoria. Several people became suspicious about Briggs’s research on contraceptives. But no Deakin academics would openly question it. It fell to Jim Rossiter, a medical doctor with a private practice, to make a complaint to the Vice-Chancellor. Rossiter for his trouble received hundreds of threatening phone calls and saw his medical practice go into

decline due to a lack of referrals. The Vice-Chancellor, Fred Jevons, initiated an inquiry. Briggs was able to mobilise support from FAUSA. An inquiry was finally set up, Briggs resigned and moved to Spain, where he admitted to fraud before he died of natural causes. A subsequent inquiry at Deakin confirmed problems with Briggs's research but exonerated his colleagues.

This is a very long and messy story. A book could be written about it and in fact one has been. Fred Jevons gave me a copy of his manuscript to read. But he hasn't sought publication because it is defamatory of some others in the saga besides Briggs, who is dead and can't sue. The point of the story is that there are no decent procedures for exposing scientific fraud.

There's a similar lesson in the story of William McBride, one of Australia's most well-known scientists ever since he discovered that pregnant women who took the drug thalidomide often gave birth to deformed children. McBride set up a private research institute called Foundation 41. At one stage in the early 1980s he was investigating the drug scopolomine. Junior researchers Phillip Vardy and Jill French discovered that McBride appeared to have altered data in a paper published in the *Australian Journal of Biological Sciences*. They raised their concerns with the director of Foundation 41. Gaining no satisfactory response, they resigned. Seven other junior researchers at Foundation 41 wrote a letter about the allegations. They were retrenched. Vardy and French wrote a letter to the *Australian Journal of Biological Sciences*, which didn't publish it.

I heard about some of this story from Bill Nicol during a visit to Canberra in 1986. Bill had written a book about McBride, but he couldn't get it published due to defamation law. He was toying with the idea of getting it accepted as a submission to a parliamentary committee, so it could be included in the parliamentary record and thus be available for quotation by the media. But he had to wait several years. Norman Swan, a journalist for the Australian Broadcasting Corporation (ABC), with medical training as well, broke the story in 1987. This eventually led to inquiries into McBride's research which concluded that he had indeed committed scientific fraud. Swan arranged for the publication of Bill Nicol's book by the ABC.

Once again, the official channels didn't work. Foundation 41 and the *Australian Journal of Biological Sciences* didn't take action. Only after the media became involved were official inquiries set up. Meanwhile, several researchers who had tried to raise their concerns paid severe career penalties.

The most famous US case in recent years has a similar message. Junior researcher Margot O'Toole tried to raise questions about experimental evidence for results reported in an article in the journal *Cell*. The work was done primarily by Thereza Imanishi-Kari, David Baltimore (a Nobel Prize winner) and David Weaver. The scientific establishment rallied around Baltimore, who denied any problems. It took dogged pursuit by "fraud busters" Walter Stewart and Ned Feder, persistent media attention and a congressional inquiry to squeeze out evidence that irregularities had indeed occurred. Margot O'Toole's courageous efforts led to the virtual destruction of her career as a scientist.

In trying to pull together themes from the Spautz, Briggs and McBride cases, I came upon a provocative argument. I noted that it was usually difficult to take action against scientific fraud, especially when committed by senior and powerful scientists, even though scientific fraud is ritually castigated as a totally unacceptable practice. At the same time, there are many things done by scientists that involve misrepresentation and bias which are widely accepted, such as citing work that has not been read, not giving co-authorship to people who helped out in research, exaggeration of the quality and social significance of research, padding of curricula vitae, "sloppy scholarship," and accepting research money from vested interests.

For example, Briggs had been made co-author of scientific articles to which he had contributed little. No one did anything about this because it's virtually standard practice. Briggs received research money from a contraceptive company whose contraceptives he reported to be superior. No one did anything about this conflict of interest, since it is standard practice to accept research funding from vested interests. Similarly, McBride accepted money from the lead industry and dismissed the possibility that lead was implicated in birth defects. My conclusion was that when a scientific practice, such as accepting funding from vested interests, is of benefit to elite scientists and their

patrons in government and industry, it is not called fraud. The definition of fraud is restricted to things such as manufacturing data that are not particularly useful to elites. Even in these cases, action is difficult to achieve.

When there is a conflict of interest in which a scientist finds results that are favourable to the company funding the research, not much can be done. Formal channels for making a complaint simply don't exist. Only when the scientist is also foolish enough to be caught manipulating data is something done, and then only after enormous efforts. No wonder most people who know about unsavoury activities in science simply keep quiet.

### **A Cambridge connection down under?**

In March 1988, a couple of years after I had moved to Wollongong, I received a letter from Dr Johan Kamminga, a visiting fellow in the Department of Prehistory and Anthropology at the Australian National University. He had been referred to me by Jeremy Evans. He offered to have his name added to my list of people willing to speak to the media about intellectual suppression. He also mentioned that he had submitted a two-volume complaint against the ANU to the Commonwealth Ombudsman.

Given all my difficult times at ANU, I was definitely interested in this complaint and wrote back saying so. Thus began an ongoing interaction with Jo Kamminga. We exchanged quite a few words by post but even more by phone, since Jo is an enthusiastic conversationalist.

Jo essentially alleged that in the Prehistory Department at the ANU, there was a prejudice in favour of graduates of Cambridge University. The Prehistory Department is part of the research schools at the ANU, which have no undergraduates and in terms of research are the most privileged part of Australian academia. Hence, although the Prehistory Department has only a few tenured academics, its role is especially significant.

Jo's complaint to the Ombudsman was a model of scholarly investigation — an investigation into scholarly bias. He provided figures on appointments in Prehistory over 25 years. He also analysed five particular appointments in detail, showing what he alleged were shortcomings in selecting the short list of candidates,

in ruling out certain candidates and in choosing the successful applicant. He argued that there was a bias in favour of graduates of Cambridge University.

Jo had an obvious vested interest in raising the issue. He was an archaeologist with an outstanding record in the field, trained in Australia and singularly unsuccessful in his applications for jobs in the Prehistory Department. But from a career point of view, making a complaint was not a wise thing to do. It would brand him as a dissident and possibly make him unemployable in the field. He knew this. He nevertheless felt it was important to make the complaint. He spent months investigating and preparing his submission.

Jo’s training in archaeology and anthropology turned out to be good training for his submission. He was thorough and meticulous in collecting information about selection procedures, university and government regulations and the like. He kept in touch with members of various selection committees and referees for job applicants. He kept in touch with various sources on campus. He even kept on reasonable terms with several of the members of the Prehistory Department, even after he had made his complaint and had received media attention. This indeed was unusual. In my experience, most dissidents become quite alienated from anyone they believe responsible for their situation.

My recommendation to Jo was the usual one. I didn’t think official channels — in this case the Ombudsman — would give him much satisfaction. It was likely to take a long time and not lead to any changes in substance. Jo realised there would be problems but once he made his decision to proceed, he pursued his course with total commitment. An unanticipated complication was that the newly appointed Ombudsman, Dennis Pearce, was a law professor at ANU, on leave from the university for three years. Would he be willing to take strong action against his employer? Jo thought he had a good case and that he could win. In any event, he was committed to his course of action.

Getting the Ombudsman’s office to move on the issue required a major effort on Jo’s part. He met with various officers as well as the Ombudsman himself, provided additional documents, and pursued them with phone calls and letters. Originally he thought the case would take three months. It ended up taking three years.

Jo was also willing to seek media attention. He talked to various journalists, briefing them on the case and providing them with documents. Arguably, the media attention had a greater impact than the Ombudsman's investigation. But they were not mutually exclusive. Media coverage may have kept the official case from being dropped.

My first aim, as in many such cases, was to gain an understanding of the key elements of the issue. This wasn't easy. Jo's report was clearly written, but the case before the Ombudsman quickly got into technical issues concerning rules and regulations, such as whether a particular selection committee, for a particular appointment, had been legally bound by government regulations, whether university procedures adequately reflected those regulations, and so forth. My interest was much more in the general issues of bias in appointments. I wrote a short article, telling about Jo's complaint to the Ombudsman in the context of discrimination in academic hiring practices. I sent a draft to some key people, including the head of the Prehistory Department and the Vice-Chancellor of ANU. Jo told me that the article stirred up discussion in the Prehistory Department.

I decided to send the article to several student newspapers. Student newspapers are a good venue for critiques of universities, because they are often willing to criticise the establishment and they are read by lots of people. Many academics read them, though not always publicly! After my article was published in some student newspapers — most notably in *Honi Soit* at Sydney University — Jo told me that it had caused a stir at ANU. Having the article in print was useful, since it summarised the general issues conveniently and could be sent to others, such as journalists, to introduce them to the case.

The case went on and on. Jo filed new complaints, such as a complaint to the Ombudsman in Darwin about selection procedures at the University of the Northern Territory. Jo contacted more journalists and obtained more coverage. Sometimes I was asked to comment. Jo's case went to the Council of ANU, the governing body. He contacted various members of Council, providing them with information. He worked through the ANU Staff Association and also through the national organisation FAUSA. Most of this produced very little.

Meanwhile, Jo was juggling his own life. He survived on consultancy work in archaeology. He took trips to Thailand for his archaeological research and attended conferences in Japan and the US. He wrote archaeology articles. When one of his books was published, he organised publicity. He bought a Vietnamese restaurant in downtown Canberra and refurbished it as a Thai restaurant, which he ran with his wife Katai. When I visited the restaurant, I found that Jo had put a copy of my article in *Honi Soit* in the display area near the door.

As ANU administrators seemed to evade the scrutiny of the Ombudsman, Jo filed more complaints. He put in requests for information through Freedom of Information legislation. When items were denied, he tried another official channel: the Administrative Appeals Tribunal.

Jo sent me official reports that dealt with his complaints. They were so couched in administrative jargon that I had to ask Jo to interpret what they really meant. In essence, his case to the Ombudsman led to some changes in appointment procedures at the ANU. His case to the Administrative Appeals Tribunal led to some changes in criteria for releasing documents under Freedom of Information legislation.

It seemed to me that the official channels in this case led to a bottomless pit of administrative detail. But Jo was committed to the case and he perceived a few gains. Many of the gains, though, can be attributed to the publicity that the case attracted. There were many stories in the media. University administrators hate adverse publicity. Making selection procedures appear more rigorous is one way to avoid future bad publicity. Whether things have really changed is another question. Jo believes that the system of cronyism and patronage at the ANU remains intact, and that is his primary regret.

Jo's original complaint to the Ombudsman was one of the best documented accounts of bias in appointments that I've ever seen. It seems a shame that it led into murkier and murkier levels of bureaucratic discourse and manoeuvring. Journalists tried valiantly to make a clear story out of the case. Many academics were quite sympathetic. They are familiar with academic old-boy networks. But it is hard to mobilise support when the issues become exceedingly complex.

I think it might have been more effective for Jo to have written his original complaint as a document for general distribution, and to have arranged publication somewhere. He could have pushed for changes in appointment procedures, using the power of publicity to mobilise support rather than the threat of sanctions to force compliance. Would this have worked? There's no way to know for sure. Jo's ordeal through official channels certainly didn't change my view about their ineffectiveness for a challenge such as his.

There are other cases with a similar message. Remember Melvin Reuber, the US pesticide researcher whose reputation was ruined as a result of the publication of a criticism from his boss in *Pesticide & Toxic Chemical News*? He went to the courts and won big: \$875,000. But the journal appealed. Reuber won again at the first appeal to the Court of Appeals, with the three judges unanimously in his favour. Then the journal appealed to the full bench of the Court of Appeals, and Reuber lost. The US Supreme Court refused to hear the case, which meant the final Court of Appeals decision stood. In the end, after a decade in court, Reuber's case failed. He got nothing.

### **Whistleblowers without recourse**

In mid 1991 I received a call from John McNicol in Canberra. He had set up an organisation called the Social Conscience Group. He set up a hot line for whistleblowers and received 75 calls in the first month. Before long he set up an organisation called Whistleblowers Anonymous. It worked to support individuals, such as government bureaucrats, who had come under attack for speaking out. The word "whistleblower" has come to mean anyone who speaks out in the public interest, typically to expose corruption or dangers to the public. However, the greater danger is often to the whistleblowers, who are attacked by their employers with great regularity.

Because whistleblowing is such a risky business, John McNicol's support organisation allowed whistleblowers to keep their identities out of the public eye: they were anonymous. But the title Whistleblowers Anonymous was not a good one, since it incorrectly suggested, by analogy with Alcoholics Anonymous, that it was composed of people who were trying to kick their

habit of whistleblowing. The name was later changed to Whistleblowers Australia.

There's obviously a close connection between whistleblowing and suppression of intellectual dissent, but there are differences. Not all whistleblowers are attacked as a result of their actions, though many of them are. On the other hand, many cases of suppression do not involve whistleblowing. Jeremy Evans was denied tenure; his teaching in the Human Sciences Program may have been threatening to some people, but he was not a whistleblower. Suppression of dissent can occur by blocking appointments or publications; only in some cases could those who are suppressed be called whistleblowers.

The idea of whistleblowing focuses attention on the person who speaks out and on their action. By contrast, the idea of intellectual suppression focuses attention on the act of suppression and the people who carry it out. For most purposes I prefer to use the concept of suppression, especially since it is more general and more easily leads to an investigation of systems of power and patterns of suppression. A focus on whistleblowing is more individualistic, and it is easy to get diverted into examining the personality of the whistleblowers.

John McNicol invited me to join the board of Whistleblowers Australia. At the first board meeting that I attended, in Canberra on 26 March 1993, the meeting began with introductions. Each board member was invited to say a few words about themselves. Many of the board members were whistleblowers themselves and had been through complex and traumatic experiences. Their stories could not be told in a few minutes. I was used to hearing of corruption and unscrupulous actions, but even so the stories had a big impact on me.

Vince Neary, for example, told about his struggle in the State Rail Authority of the Australian state of New South Wales. Beginning in 1987, he raised the alarm about rorts — large payments to companies without evidence that they had done the work specified — and what he considered to be unsafe signalling practices. He raised his concerns initially with the people concerned, then with the head of State Rail, then with his representative in parliament, then with the state ombudsman, then with the Independent Commission Against Corruption, then

with the state Auditor-General. The management of State Rail denied his claims throughout. For raising these issues he was harassed, sent for psychiatric examinations, demoted, put on menial work and eventually dismissed.

The official channels didn't work for Vince. But he kept his faith in the system and kept trying further channels of appeal. The cruelest twist came two years after he had first gone to the Auditor-General. The person in the Auditor-General's office working on his case was Dick Dunn. Vince kept calling up to find out what was happening. Dunn was preparing to report to parliament about the State Rail's refusal to supply documents. Then in September 1992 Vince found out that Dunn had taken leave of absence from the Auditor-General's in order to work for State Rail at a "senior executive" salary. Talk about disillusionment!

Fortunately, Vince gained support through the media and from other whistleblowers. Years down the track he received a substantial payment from State Rail as part of a settlement — though not substantial enough to compensate for years of harassment and loss of his career. One condition of the settlement was that he not reveal details of the settlement itself.

Many of the stories told by whistleblowers are so astounding that it is easy to be sceptical. Officials don't do such nasty things, surely! Naturally, if I was going to write or comment on any case, I would want to see relevant documentation. But having heard so many cases, I'm familiar with the patterns of suppression.

Certain things are good indicators. There is the whistleblowing itself, of course: the person says or does something that is threatening to powerful interests. Then there are reprisals. Most whistleblowers have exemplary records at work, being unusually conscientious. They believe in doing things properly, which is why they blow the whistle. Afterwards, they come under attack. Complaints are made about them, but they are not told the charges. Their sanity is questioned and sometimes they are sent to psychiatrists (often allegedly to justify their claims to sick leave due to stress). Their work performance is criticised. They are transferred, isolated, demoted, reprimanded, dismissed. When a case fits the pattern, my inclination is to believe the story.

Imaginary stories of suppression would probably contain features that don’t fit the pattern.

Why would anyone make up a story of suppression, anyway? Most people who are attacked for their courageous acts of whistleblowing feel violated. They may feel guilty. They are often reluctant to tell others, much less seek publicity. Talking to other whistleblowers is therapeutic as well as providing practical suggestions for action.

The most frightening cases involve violence. People who have challenged corrupt police or organised crime can be in great danger. In many countries, the government is repressive and dissent is a crime. By definition, the official channels do not work in such situations.

The new president of Whistleblowers Australia was Jean Lennane, a psychiatrist who had herself been dismissed from the NSW Department of Health for speaking publicly against government funding cuts. She did a revealing study of the experiences of 35 whistleblowers, preparing a table listing all the official channels that had been tried, such as internal appeals procedures, ombudsmen, trade unions, parliament and so forth. Since many whistleblowers had tried several of these channels, she could produce a score for each official channel, giving the number of people which any given channel helped, hindered or made no difference. She found that most channels were more likely to be a hindrance than a help, with “made no difference” the most frequent report. Jean’s conclusion is that there is one thing you can count on when you use official channels: that they won’t work. This was a strong message. I agreed wholeheartedly. After all, it was precisely the conclusion that I had come to myself.

Yet no matter how much evidence I might give, most people won’t believe it. The belief that there is justice to be found somewhere is deep seated. A few people seem to be vindicated, which provides hope. Even more than this, though, dissidents know that they are doing the right thing by speaking out. They know in their hearts that if there is such a thing as justice, it should be on their side. They think that although other people may have had bad experiences, their own case is so good that its

justice is bound to be recognised. There is nothing I can say to change such an opinion.

Jean Lennane concluded that there were only two things that helped whistleblowers with any reliability. They are publicity and support from other whistleblowers. I could only agree.

## 7

### The media against suppression

It was in 1980, when I was still in the early stages of my studies of suppression, that I had my most intense experience with the mass media. That year an organisation called the National Science Forum was set up in Canberra. It hosted talks, often by prestigious scientists, on issues relating to science. The audiences consisted of journalists, scientists and other interested people. The idea behind the forum was to give greater publicity to issues relating to science by getting key people to speak out in a situation which allowed and justified considerable media attention. Talks were held about once a month in Canberra. Because it is the national capital, most major newspapers and other media have Canberra-based reporters.

In July 1980 I had discussions with one of the coordinators of the National Science Forum, Wendy Parsons, who arranged for me to speak in September. I titled my talk “Suppression of Australian research — how widespread is it?” and used a drastically shortened version of my article “The power structure of science and the suppression of environmental scholarship.” In both my talk and the written version that I prepared for distribution, I listed the ten suppression cases which were later published in my article in the *Ecologist*.

The forum, on 29 September, was quite an event. During the questions and comments immediately following my talk, several other scientists spoke up and said they had been suppressed themselves or knew of suppression. Such declarations are not common! What happened was that my description of suppression cases had given others courage to speak publicly about their knowledge and experiences. And it was very public, since numerous journalists were present. Some of them sought out these scientists after the meeting.

The next day, there were stories in several newspapers such as the *Canberra Times* and the *Financial Review*. The *Canberra Times* is the only daily newspaper in the city and at that time

was recognised as one of the quality newspapers in the country. Across the top of page one the next day was a major story by Richard Scherer titled “Researchers ‘facing corporate and government suppression’.” It described a number of the cases I had documented in my talk.

One error worried me. Scherer reported me as saying that both John Hookey and Jeremy Evans had been denied tenure. But John Hookey hadn’t actually been denied tenure, only given an indication that he would be. In addition, Jeremy Evans was still going through the appeals procedure, so it would have been more accurate to say, as I did in my paper, that the reappointment and review committees had recommended against tenure. I talked to Richard Scherer about this, but he didn’t think it was worth publishing a correction. In retrospect I realise that I was being overly sensitive. Minor inaccuracies are common in the media, and seldom worth worrying about. It is the major ones that should be of concern.

After the articles appeared, lots of other journalists contacted me, including ones from ABC radio “Morning extra,” the Australian Associated Press, the *Australian* newspaper, the Sydney newspaper the *Sunday Telegraph*, ABC radio “AM,” and Brisbane radio station 4ZZZ. The Australian Associated Press is a syndicated media service and many other stories appeared as a result of its reporting. The callers from radio stations usually recorded interviews over the telephone, or ran them live, whereas the journalists from the print media asked questions about the stories already printed and sometimes asked for further contacts.

The intense interest from reporters died down after a few days. This is the usual pattern. Media attention comes in bursts, and so has to be used when it’s available. It won’t come back easily. However, as a result of the publicity, I was contacted by quite a few members of the public over the following months. But by the time my article was published in the *Ecologist* early in 1981, the issue was fairly quiet.

This was hard to imagine at the height of the media attention. Four of the ten cases I had highlighted involved the Australian National University. The article in the *Canberra Times*, not surprisingly, triggered a response. The Vice-Chancellor of ANU,

Anthony Low, released a statement that was reported in the *Canberra Times* the following day. He said that I had drawn “quite unjustified conclusions” from the four ANU cases, and that “Since they relate to particular individuals I am reluctant to enter into details publicly, since it would seem to me that such issues are primarily matters between the University and the person concerned.” He welcomed a meeting with me. I took up Low on this and arranged a meeting for later the same week, on Friday.

The other big factor during this week was television. Just before my talk to the National Science Forum, I was interviewed by Stephen Taylor of Capital 7 Television, a commercial station and one of the two television stations then broadcasting in Canberra. The interview was broadcast twice that evening, so I’m told — I seldom watch television myself. On Thursday that week, Stephen Taylor called me. He wanted me to make a statement on camera about the ANU Vice-Chancellor’s statement. I said I preferred to wait until after my meeting with the Vice-Chancellor the next day. That evening, so people told me several days later, a story on the developing case was shown on Capital 7, including my talk to the National Science Forum, the Vice-Chancellor’s refusal to comment, someone else’s refusal to comment, and my planned meeting with the Vice-Chancellor.

When I arrived at the ANU administration building for my meeting with the Vice-Chancellor, to my surprise there was Stephen Taylor and a camera operator. Not being prepared, I turned my back and hurriedly went up the stairs. I felt pressured into a confrontation not of my own making. No doubt Taylor felt he was pursuing a good story and was frustrated by my refusal to make a statement: a familiar case of an eager reporter encountering a reluctant academic.

I thought about my position. My view was that the basis for suppression was built into the nature of the university. The ANU administration was not in a position to do much about the cases I had identified there. Therefore, the main goal should be to get action from all parts of the university and the wider community to counter institutionalised suppression resulting from funding decisions, knowledge frameworks and organisational prerogatives. With this perspective, the focus on the Vice-

Chancellor did not seem fruitful to me. On the other hand, another television reporter — Bill Nicol, the same person who later wrote a book about William McBride and his fraud — suggested that I might have done better to use my short-lived access to television to demand a public response and action from the Vice-Chancellor, since that is the sort of thing television is good for.

If this was a mistake on my part, I made an even more serious mistake in my meeting with the Vice-Chancellor Anthony Low — I went alone. Also present was the Assistant Vice-Chancellor, Colin Plowman. Both of them grilled me about the ANU cases, denying that any suppression was involved. Three of the cases could be interpreted either way: two tenure denials and one rejection of a PhD. Were they decided on merit or were other factors involved? It was understandable that administrators would believe that everything was being done properly. The fourth case concerned the Forestry Department's attempt to block publication of *Fight for the Forests* by Richard and Val Routley. The part about Richard Routley being denied access to the Forestry Department library would be especially hard to deny, or so I thought. The Vice-Chancellor gave me a letter from the acting head of the Forestry Department, Professor Eric Bachelard, saying that he had been unable to find evidence that any ban had occurred!

If I had arranged for someone to accompany me, to listen and take notes, Low and Plowman would have had to be more careful in their claims. Afterwards, I realised that Low had received all his information from the individuals who were responsible for what I said was suppression. Before releasing his statement denying suppression at ANU, he had not talked with John Hookey, Jeremy Evans, David Smith, or Richard and Val Routley. However, it took me a while to obtain additional evidence about the library ban from the Routleys, since they seldom came to Canberra. A week later I obtained the names of two other people who would vouch for the ban having occurred, which I sent to Eric Bachelard. I also rang him about it, and we had a useful discussion. But by then the issue was dead.

During the heat of media attention, people will make claims that are hard to refute quickly and obviously. By the time the

patient work is done, the story is no longer newsworthy. From my point of view, the Vice-Chancellor had made the incorrect claim, based in part on misinformation from the Forestry Department, that my conclusions about suppression were unjustified. Only later did Eric Bachelard admit over the phone to me that part of his advice to the Vice-Chancellor was wrong. But things were much worse from the Vice-Chancellor's point of view. My talk and claims about suppression had received widespread media coverage, giving the ANU a black eye, and his statement was reported only in a small article on page 7 of the *Canberra Times* a day later.

### **Media skills**

One thing I've learned from my experiences with the media is to be as accurate as possible. That doesn't mean the published story will be accurate — but it helps. If I can provide documents to journalists, that helps them get the facts right.

I've also learned never to say anything unless I'm happy for it to be reported. Of course it's possible to make comments to journalists and to demand or request that they be "off the record" or "background" or whatever. Most journalists will respect such requests, but some don't, so I don't take the risk. I've never been burned badly, but I've heard of plenty of cases in which people have been.

Anyone who is familiar with the operations of the media knows that misrepresentation of a person's views is common. This is seldom due to malice and mostly due to the operations of the media. When I write for an academic journal, I can be confident that the published text will be almost identical to what I submit. Usually I get to check through the proofs — a copy of the text as it will appear in print — and make any corrections needed. The mass media are quite different. There are never any proofs. Nearly every time, sub-editors change the titles of my articles, occasionally to something irrelevant or contradictory to my original. Sometimes sub-editors make changes in the text. Some sentences may be rewritten. Usually they are deletions to save space: words, sentences, paragraphs and occasionally big slabs of text. Letters to the editor are also likely to be edited. In spite of such editing, if I write an article myself, I'm reasonably

confident that the published version will be relatively accurate. This is not always so when a journalist writes a story based on an interview with me. Sometimes I've been quoted as saying something that I never said. Thankfully, though, the substance is usually reasonably accurate.

The electronic media provide another set of obstacles. In some ways the safest medium is a live radio interview. There's no editing, though the interview can be cut off at any time, and often is. Talking on radio is a skill for which few people have training or natural skills. It's necessary to have an interesting speaking voice, have relevant points at the tip of your tongue and be quick thinking in answering questions. Practice with a tape recorder and a friend who pretends to be an interviewer is invaluable. I've done many radio interviews over the years, and now they seldom worry me or trip me up. Still, I find it useful to have a list of key points — such as examples of suppression — close at hand.

Whenever an interview is taped, it's likely to be edited. With radio this may be light or severe. If you are broadcast on the daily news, it will probably be just one or two sentences, usually taken out of context. These problems are much more severe with television, which is undoubtedly the most manipulated medium, though ironically it has greater credibility with most people. To be effective on television, extensive skills are required, of which I have only an inkling. Television editors like short punchy statements. It is common to be taped for an hour and find that only a minute or less is actually put to air. The potential for manipulation is enormous and you are largely at the mercy of the journalists and producers. Television has a huge impact, mainly through images. Appearing sincere and truthful is often more important than the statements made.

Sometimes I wonder about the impact the media have on people. On dozens of occasions people have said they heard me on radio. I often ask, "What was I talking about?" Usually they can't remember — but they remember hearing *me*!

Many of the problems with the media are due to the way they operate. To make a reasonable wage, newspaper journalists may have to produce several articles every day. They just don't have enough time to check every detail. Academics can spend weeks, months or even years polishing a piece of writing. How would

they fare if they had only a few hours, or even just a few minutes, to write a story? Considering the pressures under which they work, most journalists do an excellent job.

I try to be helpful. If I don't know enough to comment on a particular issue, I say so and suggest someone else if possible. I try to be accurate, to emphasise what I think are the important points, and to provide documentation if required.

### **The media and suppression**

Looking at the mass media in a general way, one might think that there would be little support for dissidents. The mass media are big businesses themselves, and have strong links with governments and corporations. Advertising plays a key role: media are notoriously reluctant to criticise corporate advertisers. A study showed that US magazines and newspapers that accepted ads for cigarettes ran almost no articles critical of smoking. Only a few magazines, such as *Reader's Digest*, regularly had stories critical of smoking, and these magazines invariably were the ones that refused cigarette ads.

The links with governments are no less strong. The mass media depend on governments for news. Reporters cultivate sources in areas of commerce, defence, foreign affairs and the like. As a result, almost all reporting stays within the bounds of conventional political debate, bound by the limits of the major parties.

Most suppression occurs when one group has much more power than another. Much of it is by employers against dissident employees. Inside corporations and government departments, threatening opinions are systematically discouraged or, if that isn't sufficient, squashed. Most people toe the line to keep their jobs. A few whistleblowers openly buck the system.

If the mass media have such strong links and common interests with government, industry and other dominant interests, then why would suppression ever be reported? There are several reasons. Since the mass media are big businesses themselves, they are not totally dependent on government and industry, as would be a small public relations firm. Governments and businesses need the mass media. This gives some scope for independent action.

The mass media are driven by the need to attract audiences. Anything that makes a good story is hard to resist. In addition, journalists make their reputations by writing stories that get published, and by tackling issues that bring attention. Suppression makes a good story. Why? For one thing, it usually involves individuals, and this is very attractive for readers. When I give names and details, journalists are eager to pursue the story. When I talk about institutionalised suppression, most of them turn off.

Suppression and whistleblowing also tap an attractive theme. They are about individuals standing up to powerful interests, getting attacked and yet persisting. I suspect that many people support and identify with the honest employee who takes on corrupt bosses.

A good journalist will try to report on both sides of an issue. This immediately raises the question of what exactly the sides are. But as soon as an issue is defined in terms of suppression, it becomes a question of those who say suppression has occurred and those who deny it. A “balanced” story is seldom entirely satisfactory to either side. But it’s usually much more helpful to the dissident, who in other circumstances would be given little credibility in relation to a powerful organisation.

Yet another factor is that many journalists know all about suppression, because they see it happening in their work every day. Although evidence is not available one way or the other, I suspect that suppression is a more everyday occurrence in the media than just about anywhere else. Since journalists understand censorship and suppression, and are likely to be sympathetic to those who oppose it, they are in a good position to report on it. There are some, of course, who become cynical — an occupational hazard for journalists — and ask why a few more cases of suppression should be of interest to anyone.

I’m being very positive about the role of the media in exposing suppression. Don’t get me wrong. It’s far from perfect — *very far*. But compared to the “proper channels,” the media are often refreshingly open and supportive.

In some areas, the media are almost totally impervious to nonstandard viewpoints. One is terrorism. The standard view promulgated by western governments is that terrorism is violence and intimidation carried out by small groups, usually left-wing. Actually, by far the most terrorism — in terms of the number of people killed, tortured and intimidated — is carried out by governments in wars and by repressive governments against their own populations.

The mass media give enormous attention to a few small terrorist groups and to terrorism sponsored, or allegedly sponsored, by stigmatised governments such as Libya. By comparison, attention to terrorism funded, sponsored or carried out by the governments of the United States, Russia, China, Britain and the like is minimal.

Experts like Claire Sterling who focus on terrorism by small groups or stigmatised governments receive extensive media coverage in the US. Edward Herman is one the few experts who takes a critical view and emphasises government terrorism. He is not invited to appear on major television or radio shows. Instead, he has encountered suppression. A book of his about state terrorism, in collaboration with Noam Chomsky, was accepted by a publisher. When top officials in the publishing firm found out that the book was so critical of the US government, they broke the contract. Both Chomsky and Herman are eminent intellectuals but when their books are published, mainstream US media decline to review them. One reason for Herman and Chomsky's problems is that they are critical of the mass media itself.

As Ed Herman reminded me, there are two types of suppression, and he and Chomsky have been subject to both. One is overt action such as the breaking of their book contract. The other is the routine dismissal of their views because they are considered to be too far from the mainstream. The latter type of suppression, which can be called "institutionalised suppression," is much more common than overt suppression. It's also much harder to document. Most media interest in suppression is in the overt kind. Almost by definition, there is no media interest in the routine dismissal of views that are off the agenda.

In my own experience, I've been able to get articles published in newspapers on topics that are currently "in the news." For example, during the years when the debate over uranium mining and nuclear power was going strong, the *Canberra Times* published several of my articles on the subject, even though — or perhaps because — they took a strongly partisan line. On the other hand, I've had little luck publishing articles in newspapers on either nonviolent defence as an alternative to military defence or on participatory alternatives to electoral democracy. In both these areas I've built up a lot of knowledge over the years but my provocative submissions are consistently rejected. The most likely explanation is that the areas are outside the current bounds of public discussion.

So there are many areas to which the mass media are largely blind. These areas of blindness are insidious. It is far easier to see problems with what is reported than to realise the biases involved in deciding what is never reported at all.

Fortunately, many cases of overt suppression of dissent are newsworthy. Dissidents often raise issues that are of general interest, such as corruption and hazards to the public. But even when the issues are esoteric, the process of suppression can make the case worth reporting, since suppression involves familiar processes such as censorship, harassment and dismissal. Furthermore, sometimes reporting of overt suppression draws attention to the more pervasive process of institutionalised suppression.

### **The Coulter case**

Dr John Coulter worked for 20 years at the Institute of Medical and Veterinary Science (IMVS) in Adelaide. On 30 June 1980 he was dismissed from his medical research job. This was one of the most blatant and publicised cases of suppression in science for many years.

Coulter was a prominent environmentalist in South Australia. He was an effective speaker and campaigner and was not afraid to challenge powerful interests. For example, he made comments on ABC television about hazards of the pesticide dichlorvos. The manufacturer, Bayer, took court action against the ABC over this, only dropping the case two years later. Bayer also applied

pressure to the Director of the IMVS, Dr J. A. Bonnin. This was only one of a number of cases where Coulter's public statements triggered complaints. Perhaps as a result of such events, in March 1980 Coulter was informed that he would be transferred and demoted.

The apparent trigger for his dismissal was something more local. He had tested a chemical, ethylene oxide, used in an IMVS lab as a sterilising agent, and found that it could cause mutations. This meant it might contribute to causing cancer. Coulter submitted his report on ethylene oxide to the proper authority, the IMVS's Fire and Safety Committee, but he also gave copies to the workers in the lab. The Director rebuked Coulter. Coulter's response was to post copies of his report and the correspondence concerning it on IMVS noticeboards. Soon after he was dismissed and the Environmental Mutagens Testing Unit, which he headed, was shut down.

There is much more to the Coulter case, including his court case against the IMVS which produced much revealing testimony, showing that the official grounds for dismissal didn't stand up to scrutiny. Here, though, I'll concentrate on some lessons about publicity.

The IMVS is next door to the University of Adelaide. Clyde Manwell, who had survived a major dismissal attempt starting nearly a decade earlier, was still in the Zoology Department there. He knew John Coulter and was a supporter of his public stands. Furthermore, they had common interests in the environmental and health effects of chemicals. Clyde quickly rallied to Coulter's defence. Clyde sent me information about the case.

Coincidentally, I visited Adelaide in May 1980 to attend the annual conference of ANZAAS (Australian and New Zealand Association for the Advancement of Science), where I gave a talk about suppression. I met John and obtained documents and information. The case was a good one to publicise. Testing chemicals for hazards has an obvious social value. Furthermore, after the dismissal occurred there were letters and stories in the *Adelaide Advertiser* about the reasons behind it — such as Bonnin's claim that Coulter was not publishing enough research articles — that could be easily demolished.

Clyde began writing letters and articles to a number of different places, and I did the same. We exchanged drafts and compared experiences. The court case dragged on and we continued to seek publicity through a variety of outlets. Eventually we realised that we had carried out an unplanned experiment. We had submitted letters or articles to numerous places, from scientific journals to newspapers. Which types of publications were most receptive to our submissions?

The answer was clear, and also startling and worrying. Technical, scientific and medical journals were the *least* receptive avenues for publication. The mass media were the *most* receptive.

Between us we submitted letters or articles to six technical, scientific or medical journals: *British Medical Journal*, *Medical Journal of Australia*, *Nature*, *New Scientist*, *Science* and *Search*. All of them rejected or didn't publish our initial submissions. We had some eventual success in two cases. *Science* didn't publish or reply to my initial letter sent for publication. I sent a revised and updated version a couple of months later, and it was eventually published. My initial submission to *Search*, an Australian general interest science journal, was rejected following hostile comments by two referees. The editor, Edward Wheeler, suggested that I submit a shortened version as a letter, and I did this in May 1981. The letter was delayed due to concerns about the ongoing court case, which provided an excuse not to publish stories about the issue. After the court case was finished, and after several discussions with Edward Wheeler, my letter was published in the April/May 1982 issue of *Search*. So much for timely comment in scientific journals.

With the mass media we were much more successful. I incorporated the Coulter case in my article about suppression of environmental scholarship, and in this way it received considerable attention. The *Canberra Times* published a letter of mine, and the *Adelaide Advertiser* published numerous letters in defence of Coulter from a variety of people, most of whom he had helped in some way. (Scientists did not rally to his defence.) I gave information to Bill Nicol, who produced a programme on suppression for ABC television which included a segment on the Coulter case. On the other hand, Clyde sent an excellent article to the *Adelaide Advertiser* which was not published. Undoubtedly

there were letters that the *Advertiser* did not publish. Sometimes the local media are the least responsive since they have the strongest links to local elites.

We also sent articles and letters to a wide range of nontechnical journals, everything from *Current Affairs Bulletin* to *Metal Worker*. We were sometimes rejected and sometimes published. The response was unpredictable. On average, we were more successful than with the technical journals and less than with the mass media.

One explanation for this discrepancy is that the more technical the journal, the higher its standards. Clyde and I didn't think this was the explanation. *Search* ran a news story on the Coulter case and got many facts wrong. Also, few of the technical journals offered "peer review." In most cases the editor just rejected the submission. High standards or something else? It is also revealing that scientific journals have been happy to publish stories about suppression of scientists in communist countries.

Actually, the mass media have more to lose by inaccuracies than journals, because of defamation law. The mass media are more likely to be sued because they have much more money. This creates a strong pressure to "get it right."

Clyde and I prefer a different explanation. The technical journals are run by and linked to scientific elites. Some of them have strong advertising links with chemical corporations and the like. They are disinclined to back the cause of scientific dissidents because this is a challenge to their own elite positions. The mass media, by contrast, have fewer links to scientific elites (except perhaps local media with local elites) and more reason to make a story out of challenges to vested interests. The irony, of course, is that the technical journals, which you might think *should* be most concerned about suppression of dissident scientists, are the least receptive to submissions about this phenomenon. Our unplanned experiment thus was valuable precisely because the results were not what we might have expected.

### **Conclusion**

To obtain some attention to cases of suppression, persistence is required. I tried journal after journal to get my articles and letters about the Coulter case published. Attention must be

placed on mustering convincing evidence and maintaining absolute accuracy, to convince editors and reviewers of the reality and seriousness of the situation. The same convincing evidence and absolute accuracy is also vital in gaining publicity in the mass media, but for another reason. Reporters are ready to use any material that makes a good story, and careful and accurate presentation is needed to withstand the inevitable distortion that popularisation entails.

“Absolute accuracy” is impossible to achieve, even setting aside misinformation, disinformation and the problem that facts always involve values and can be contested and reinterpreted in various ways. I try for complete accuracy knowing that I’m bound to get some things wrong. The media aren’t perfect but neither is anyone else.

Unlike academia, what makes a good story for the mass media is less often a learned paper — though this may serve the purpose — than an interview or public statement or speech. For those who are more familiar with the niceties of academic discourse, learning how to interact with the mass media is quite a challenge.

## 8

### Up against the gatekeepers

Probably my most useful contribution to the struggle against intellectual suppression has been my writing. Others put more energy into the vitally important tasks of providing advice and moral support, organising campaigns and pursuing cases through various channels. I've done a bit in each of these other areas, but where I've made special efforts is in writing substantial pieces of analysis and getting them published.

Anyone who wants to do this faces several obstacles. One of them, surprisingly, is fear. I speak here of academics, who are expected to publish results of their research. But quite a few are afraid to submit articles to journals. Some of them fear rejection. Others are afraid that when an article is published, everyone will see how inadequate it is.

There is an extra problem in writing about suppression. It means taking up the cause of people who are challenging powerful interests. Many editors and referees are likely to be extra critical. The published work may not win any friends in high places. This is not a prescription for career advancement.

To even use the term "suppression" is to make a value judgement. To express concern about suppression is to take a stand, to be a partisan for a person under threat and to challenge the official story. Many journal editors and referees don't like this. They may believe in objectivity. They may want to know why you haven't also told the story from the other side's perspective. They may be postmodernists who want to know how the meanings of the events are socially constructed by all people involved. In any case, they are uncomfortable with open commitment.

I try to treat publication like a game. If one of my articles is rejected, I try not to take it personally. The question is, what is the next move? I've developed a high tolerance for having submissions rejected — which is not to say that I enjoy it! This is essential for anyone seeking publication of controversial ideas.

I learned the value of persistence from the example of my original PhD supervisor, Bob May, in the Department of Theoretical Physics at Sydney University. Bob was an incredibly talented applied mathematician who started out in physics — for example nuclear physics and statistical mechanics — and was not afraid of moving into other fields. When I joined the department, he was just getting involved with mathematical ecology, and before long he took the top job in biology at Princeton, later moving to Oxford.

While I was there, Bob had the idea of applying statistical mechanics theory to voting in order to show why so many votes in small groups are unanimous or nearly so. He worked out the mathematical model and asked me to write a computer program to calculate the results. After familiarising himself with key writings in the area, he wrote up an article and sent it off, under both our names, to one of the top political science journals, *American Political Science Review*. Three referees' reports came back, with mixed reports. The editor said we could resubmit. We revised the article. It went to one old referee and one new one: rejection. Bob was convinced the article was a good one, and so tried another top journal, *Behavioral Science*. This time it was rejection the first time. The editor sent a copy of the report of referee #5. Bob would have none of this. He sent a strong letter to the editor, saying the referee #5's report was "completely incompetent." The editor sought further opinions but couldn't reach consensus. As junior author of the paper, I watched with fascination, meanwhile revising and augmenting the paper through its journey. Finally, several years down the track, we were successful on the third try with a more specialised journal, *Public Choice*.

This early experience taught me several things. The first was the value of persistence. The second was to believe in my own judgement of the quality of a piece of work. The third was not to be afraid to submit work in fields outside my own training. Perhaps I would have learned these lessons anyway, but the experience with the voting model article certainly set me on the right track. By the time I started writing about suppression, I had enough experience in the publishing game not to get discouraged.

### **Writing about suppression**

In chapters 2 and 3 I described how I collect information about suppression and put it together for an article. I obtain information about the cases described from various sources: through interviews, from source documents and from newspapers and magazines. If there are any important gaps or contradictions, I seek further information. Then, based on my study of social theory, I put the cases in the context of a wider analysis of the issues at stake.

Having written a draft of an article, I send it to various people for comments and use their suggestions to prepare a version to submit to a journal. But which one? This is an important decision. Often I spend time at one or more libraries, browsing through the current periodicals section, looking for journals that might be suitable. After considering the options, I prepare a list of potential places to send the article. Also, I consult with friends, weighing up the pros and cons of different outlets.

Often the choice comes down to a trade-off between impact and the likelihood of publication. I try for the journal with the greatest impact where there is a reasonable chance of publication. Publication in the *New York Times* would have enormous impact, but — even assuming that I wrote a newspaper-style article — there is no chance of being published there, given that I'm an unknown academic writing from Australia. On the other hand, publication in a local newsletter might be easy but have little impact.

For my first article on suppression, "The power structure of science and the suppression of environmental scholarship," I decided to start at the top. I sent it to *Science*, a journal with enormous impact. When I had a letter-to-the-editor published in *Science* about the dismissal of John Coulter from the Institute of Medical and Veterinary Science, several people wrote to me as a result, which is several more than write as a result of most of my major scholarly articles! By all rights, *Science* was the place for an article about suppression of dissent in science. I didn't expect success, but I wanted to give the journal a chance. In writing about suppression I take the view that the response of editors and referees says more about them than about my writing.

On 8 May 1980, the editor of *Science*, Philip H. Abelson, wrote back saying that unfortunately they had a substantial backlog of accepted articles and therefore they could not handle my article at that time, and accordingly he was returning my manuscript. If I had wanted to press on, I could have resubmitted the article saying that I was prepared to wait for publication if it was accepted. But reading between the lines, I assumed that Abelson didn't like my article but didn't have the courage to say so. Would the journal of the scientific establishment publish an article critical of the "power structure of science"? Not this time. I decided to cut my losses and try elsewhere.

Next was *Social Studies of Science*, a prestigious academic journal about the social dynamics of science. The editor, David Edge, wrote on 31 July saying that I might be better to try the *Ecologist*, which I had mentioned to him as an alternative. First, there was a backlog of accepted articles which meant a delay of at least a year. Second, the *Ecologist* would give more visibility than *Social Studies of Science*. Third, the referees might be critical. They were. While most scientists are well aware of the sorts of processes of which suppression forms a part, many social scientists refuse to be convinced of even a single case of suppression without a wealth of evidence, detailed analysis of the social context, attention to how the legitimacy or illegitimacy of actions is socially constructed by those who make them or view them, and so forth. The very term suppression is a signal to be suspicious.

As a result of this hypercritical attitude, most social science journals have never published any studies of suppression, even though suppression is fundamental to their fields of study, or so I would argue. Suppression of dissent in science is crucial to understanding the maintenance of current scientific elites and their patrons, but this is seldom even alluded to in social analyses of science. Suppression was involved in the very foundation of the social sciences. In the 1800s in the United States, sociology, economics and political science gained legitimacy by allowing radical dissidents to lose their jobs and credibility.

In any case, I next tried the *Ecologist*, a British magazine treating environmental and social issues from a general viewpoint critical of many features of industrial civilisation. In 1980 it took on the scholastic apparatus of footnotes and published some quite lengthy articles, though there was no refereeing. So it seemed suitable. Before I even received a letter in response, a couple of copies of the January-February 1981 issue arrived, with my article in it. Success! This was a satisfying surprise. However, it disturbed me that the article had been edited — mainly by being shortened — without my permission, though the deletions and minor changes were well done, with one exception.

The road to publication of my next major article on suppression was much easier. After reading Marlene Dixon's book *Things Which Are Done in Secret* about suppression of radical sociologists at McGill University in Canada, I was able to contact her at the Institute for the Study of Labor and Economic Crisis in San Francisco. She recommended me to Tony Platt, the editor of *Crime and Social Justice*, an academic Marxist criminology journal published in San Francisco. Platt wrote to me in March 1982 inviting me to submit an article about academic suppression. After some correspondence, I submitted in September an article on suppression of dissident experts. It was published in mid 1983.

Being invited to write an article for a journal may seem like a sure road to publication, and perhaps it is for some people. My experience is different. In a number of cases I've been invited to write an article only to have it rejected. For example, Alastair Gunn, a co-editor of a new journal, *Waikato Environment*, published in Hamilton, New Zealand, wrote to me in March 1981 inviting me to write a short article on "environmental research and the establishment." I suggested that he might use one of my existing articles on suppression, such as my talk to the National Science Forum. He replied saying that *Waikato Environment* would not be able to use any of my articles. He consulted several Waikato scientists, of all shades of opinion, and they agreed that my articles did not accurately reflect the situation in Waikato. Indeed, they reported, several university scientists had been studying the adverse effects of lead in petrol and their careers had not been hurt.

This wasn't too convincing to me. I never said that every scientist doing work threatening to a powerful interest group is suppressed. Only some are. And how did these Waikato scientists know that there was no suppression there? Had they really searched? Even if there was none, why didn't they want to hear about suppression elsewhere? I was thankful that I hadn't gone to the trouble of writing a new article for the journal.

### **The book *Intellectual Suppression***

Cedric Pugh worked in economics at the South Australian Institute of Technology. In the late 1970s he publicly challenged the institute's administration concerning a number of its policies. He believed that this was the reason why he was singularly unsuccessful in gaining promotion to senior lecturer in spite of his excellent record of academic performance. I met Cedric during my visit to Adelaide in 1980, and we corresponded at length afterwards. He mounted an effective campaign for promotion, getting supporters to write to the administration and newspapers and stimulating quite a bit of publicity. In the end he was successful.

In the midst of his struggle, Cedric wrote to me in July 1981 suggesting that he, Clyde Manwell and I prepare a book proposal for an edited volume on discrimination in Australian academic and research organisations. I quickly responded with a tentative list of chapters, and suggested adding Ann Baker — Clyde's wife — to the group of editors. Thus began a major enterprise. It is just as well I didn't imagine then how much work would be involved, since otherwise I might have declined to be involved.

We discussed possible contributors and topics. Each of us planned to write chapters. Some articles could be reprinted, such as Peter Springell's article in *Arena* about his difficulties in CSIRO. Others would be invited to contribute. I took the lead in correspondence, setting up the framework of chapters and inviting contributors. Clyde soon suggested that I be "editor-in-chief."

Eventually in August 1982 we had a proposal to send to publishers. It included a summary, outline of contents, information about the authors, and several sample articles illustrating our sort of approach. Our title at that stage was "Suppression of

intellectual dissent in the ‘free world’.” University of Queensland Press quickly rejected it because “the market for such a book would be slight and publication uneconomic.” The Australian office of Oxford University Press followed suit, saying “We do of course see the interesting and controversial nature of your theme and material, but I’m afraid we are not convinced that we are the right house to turn all this into a commercial proposition.” The Australian editorial office of Cambridge University Press simply said they weren’t interested. These sorts of responses did not worry me. I was used to rejections by book publishers.

With Angus & Robertson we struck it lucky. I suspected that the publisher, Richard Walsh, would be sympathetic because of his own experiences. Only a couple of years earlier, in November 1980, he and George Munster had published a book, *Documents on Australian Defence and Foreign Policy 1968-75*. It reprinted secret briefings, cables and memoranda by Australian government bureaucrats concerning sensitive issues such as Australia’s involvement in the Vietnam war and the Indonesian invasion of East Timor. The Australian government heard about it and placed an injunction on the book the day it was published. The injunction also covered two newspapers, the *Melbourne Age* and the *Sydney Morning Herald*, that were running extracts the same day. The injunction was just a little too late, and quite a few copies of the book and the newspapers were sold or distributed.

For the Australian government to place an injunction like this was highly unusual, to say the least. The government obviously wanted to suppress information about its activities on sensitive issues and was willing to engage in open censorship, causing much bad publicity, to do so. The validity of the injunction was decided by the High Court, which ruled that the Crimes Act was not relevant in this case but copyright was — remember that the book reproduced government documents in full. So Munster and Walsh later produced a book which gave the essence of the secret documents by means of summaries and short quotations.

I suspected that Richard Walsh would be sympathetic to our book proposal, because he knew from personal experience what suppression was all about. That’s what happened. After requesting further information, he gave the go-ahead, accompanied by many suggestions, to be sure.

We got to work. I solicited the proposed chapters and wrote the ones that were assigned to me. Ann, Clyde and Cedric wrote their chapters, and we circulated all drafts to each other. It sounds straightforward, but was far from easy. Ann and Clyde were together in Adelaide, while Cedric was working in Singapore most of the time. Actually, Cedric was quite prompt. My biggest problem was keeping the length down. Ann and Clyde could not keep to a word limit. Their writing was always filled with fascinating material, but just got longer and longer. Even though a couple of prospective contributors never came up with their chapters, the book ended up being about 160,000 words. (This one, for comparison, is about 60,000 words.)

The first part of the book was case studies. On several of the chapters dealing with recent cases, I sent a draft to a relevant official with an offer to publish a response. For example, Evan Jones and Frank Stilwell wrote an excellent chapter on the difficulties encountered by proponents of political economy at the University of Sydney. This major academic battle had received considerable attention in the media but no systematic account was available. I sent a copy of Jones and Stilwell's chapter to the Vice-Chancellor of the University of Sydney inviting a reply from him or any other appropriate person. He sent me a letter with some dismissive comments about the chapter — but this was not for publication. I wrote a postscript to the chapter telling in a couple of sentences about my offer to the Vice-Chancellor and the fact that he offered nothing for publication.

Most of the postscripts were like this. In only a couple of cases did institutional representatives provide substantive comments for publication, and in these cases we gave the author of the chapter a chance to reply. After the book was published, several readers said that they found the lack of response from institutions the most damning comment of all.

Sending the draft chapters to institutional representatives also helped to avoid defamation. As a result of circulating drafts of chapters, we received only one threat, from a minor character in one of Ann and Clyde's chapters. They responded by enlarging their account of the relevant events and providing lots of supporting references. Needless to say, Angus & Robertson's

lawyer went through the manuscript most carefully, but didn't find much to worry about.

Richard Walsh didn't like our original title — it was too long — nor a later one, "Academic suppression." He preferred "Intellectual suppression," which tied it less to the academic market. The book finally appeared in 1986 under the title *Intellectual Suppression: Australian Case Histories, Analysis and Responses*. It received quite a few reviews in newspapers and journals, mostly favourable. It resulted in a few media interviews, but a treatment in such length and depth is not the best way to stimulate media coverage. The most important role of the book was to provide a solid reference for those who were really interested — especially people who were subject to suppression themselves.

All but one of the contributors donated their shares of the modest royalties to a newly established Fund for Intellectual Dissent. This came in handy a couple of years later, when Angus & Robertson remaindered the book. The original print run was 2400 copies, with a retail price of \$20. About 1000 were sold and a few hundred went for reviews and promotion. "Remaindering" meant that remaining copies were to be sold off at a nominal price, in our case \$1.20 each. We used the Fund monies to buy up most of the remaindered copies. As it turned out, we had the best of both worlds, since the publisher's stock then was low enough so that they kept the book in print. Some years later Angus & Robertson, Australia's largest publisher, was taken over by Collins, which sold off the remaining stock of *Intellectual Suppression* without even telling me. Tom Thompson, the publisher of Collins whom I met at a conference in 1990 where we were each speaking, said I should have been consulted, but nothing came of his promise to look into it.

Because of our purchase of remaindered copies in 1988, I had hundreds of copies in my office. We made these available free to interested individuals. My main aim was to get copies to people who really needed them, especially people overseas, where it had received almost no promotion by Angus & Robertson. I've sent out many copies in the years since, and now there are only a few remaining.

It seems appropriate that the Fund for Intellectual Dissent, set up with royalties from *Intellectual Suppression*, is the publisher of *Suppression Stories*. For *Suppression Stories*, there are no royalties and all proceeds will be used to provide copies to those who need them the most.

Over the years, several people have suggested that I should edit *Intellectual Suppression, Volume Two*. I politely decline each offer. They have no idea of the work involved, nor of how difficult it is to find a publisher. Mauricio Schoijet, a Mexican researcher who has come under attack by his university, even proposed a project documenting suppression in every country in the world the way that we dealt with suppression in Australia. An amazing project it would be!

Undoubtedly there is an important place for lengthy, highly detailed and highly referenced accounts of suppression. I'm glad that *Intellectual Suppression* saw the light of day. But for writing about suppression, it is usually a more effective use of energy to produce articles.

### **More articles**

As described in chapter 3, my contact with Dhirendra Sharma stimulated me to write the article "Nuclear suppression." After completing it, I first sent it to *Bulletin of the Atomic Scientists*, which by all rights should have been most interested, given that it was set up by nuclear scientists concerned about misuse of nuclear technology and has a mildly progressive stance on a number of issues. But I have never had any success with the *Bulletin*. Years ago, the editor-in-chief, Bernard Feld, had solicited an article from me, which had then been rejected unceremoniously ("we have no space for your article"). "Nuclear suppression" received the same treatment. The editor said nothing about the content of my article, but simply referred to lack of space as the reason for not publishing it. The article was eventually published in *Science and Public Policy* in 1986 and led to a fair bit of media coverage.

Around the same time, I worked on an article called "Dissent and its difficulties." I took three areas — nuclear power, fluoridation and terrorism — and described the obstacles facing those who presented dissident views. I argued that several "principles

of scholarly practice” were violated by proponents of certain standard views, leading to “asymmetries” in public debate.

First, proponents often fail to provide information, as when proponents of standard views of terrorism fail to provide sources for their statements. Second, they often decline to enter into rigorous debate, as when Edward Herman’s critiques of standard views on terrorism are ignored. Third, they rely on their formal status, such as being head of a government. Fourth, they attack the personal credibility of their critics, as when critics of state terrorism are called dupes of communists.

I sent my paper to *Social Theory and Practice* in September 1985. The chair of the editorial committee wrote back saying that it wasn’t appropriate for their journal, which had a circulation of only about 650 and was read mainly by philosophy academics and students. He said that “Since your paper deals with political and moral issues of wide concern, it would reach a much larger and more suitable readership if it were published in a high circulation journal of politics.” The trouble is, there are no such high circulation journals that publish lengthy articles with lots of footnotes. I also sent it to *Social Science Quarterly*, but it was rejected out of hand for being too long. Dharendra Sharma was enthusiastic and quickly published it in his journal *Philosophy and Social Action* under the title “Science policy: dissent and its difficulties.”

Few academic journals have a large circulation. Over a few thousand subscriptions is doing very well. The impact comes through visibility. A prestigious journal is read by more people than ever subscribe. Library copies are scanned by some people and others search through data bases for titles in areas that interest them. *Social Theory and Practice* was certainly selling itself short by suggesting that I would be better to try elsewhere.

Wide circulation magazines do have an impact, sometimes an enormous impact. But getting something published in one of them is no easy matter.

*Habitat Australia* is a monthly magazine distributed to all members of the Australian Conservation Foundation. It has a circulation of some 25,000 — enormous by Australian standards. Many of its articles are accompanied by photographs of beautiful nature scenes.

The editor, Marilyn Julian, was interested in an article on suppression. I talked with her at some length about it during a trip to Melbourne. My article included many cases of suppression of Australian environmental scientists, with some general analysis. I tried to write it in an accessible style. But it wasn't exactly what Marilyn — who also consulted others — wanted. I ended up doing two major rewrites of the article. At least it was published. I've been told by others that they did major rewrites only to have their articles rejected by *Habitat*. Sometimes it's easier to deal with refereed journals!

My *Habitat* article, published in 1992, had a big impact. Mark Diesendorf, then working for the Australian Conservation Foundation, said it generated more correspondence than anything published in *Habitat* for several years. Quite a few people wrote directly to me. At least *some* of the readers appreciated a story without any pretty nature pictures.

My offer to write for *Newsweek* came through personal contact. When in New York in 1991, I met Sharon Begley, science editor of *Newsweek*. She contacted me in early 1993 to see if I would be interested in writing a guest opinion column for the new monthly science supplement that goes to US subscribers and is picked up by the international editions of the magazine. My first thought for a topic was intellectual suppression.

Not only did *Newsweek* sub-editors go through my prose, fixing it up and putting it in house style, but *Newsweek* researchers checked up the facts on every case I mentioned, contacting key people to check details. One example I had listed, involving the International Rice Research Institute, had to be dropped because I had no way of contacting the author of the story on which I had based my thumbnail sketch. I had a lengthy phone conversation with a *Newsweek* lawyer, which led to further modifications in the text. I thought I was pretty thorough, but this level of scrutiny, for a story just 1000 words long, was something new. The article appeared in the 26 April edition, at least in the New Zealand edition a correspondent sent me. By publishing this article with names and claims, *Newsweek* showed a lot more courage than most academic journals I've dealt with. Of course they have more money to have people check facts and provide legal advice, but they also have much more to lose.

Surprisingly, the *Newsweek* article didn't generate a big response, so far as I could tell. A few people told me they saw it, and a few people wrote to me. Having a big international circulation, it's much harder to know what impact the article had.

My guess is that discussions of suppression have greatest impact when they deal with cases that are directly relevant to the experience or interests of the readers. This was the case with my articles in the *Ecologist* and *Habitat* — they are about suppression of environmental scientists, in journals read by people concerned with environmental issues. But in many cases the journals that would be most relevant are controlled by people who don't want discussion of this topic. This seems to have been the case with *Science* and *Bulletin of the Atomic Scientists*, among others.

### **I'm a gatekeeper too!**

So far I've described some of the methods I've used to get past editors and referees, the "gatekeepers" in publishing. Basically it boils down to working hard to present a strong case and write a good article, and being persistent. But there's another way to view my efforts. From the point of view of those who think they have been suppressed and are looking for a champion, I am a sort of gatekeeper too.

For anyone who wants to oppose suppression of dissent, there is a virtually unlimited choice of topics and cases. Choosing where to devote your efforts can be difficult. My choices have been influenced by the types of cases that I've happened to investigate and sometimes by invitations to write articles. For example, I wrote my article "Nuclear suppression" after finding out about Dhirendra Sharma's case but wrote for *Newsweek* after receiving a general invitation from the science editor.

But what about topics? What about suppression of feminists, suppression of free-market fundamentalists, suppression of radical theologians and suppression of police whistleblowers? Looking back at areas I've studied over the years, there seem to be some guideposts for choosing topics. I prefer to tackle areas that I know about personally, hence my special interest in cases in science and academia. I prefer areas where there is a pattern of suppression, such as fluoridation and pesticides, since these sorts

of cases lend themselves to a more general analysis of the role of power and special interests. I prefer areas where dissent has some social value, for example in defending the environment or oppressed groups. I also prefer areas that are interesting. The issues raised by the theory of the origin of AIDS from polio vaccines are fascinating, and there's social value involved too. A case of blocked promotion in a government bureaucracy would not be nearly so intriguing — though the dynamics of bureaucracy can be surprisingly engrossing.

Whatever my preferences, they can be overridden by circumstance. Sometimes someone tells me about a case and I decide to follow it up. The Spautz case is an example.

This brings up a vexing issue: so-called “fringe” areas. Occasionally I'm asked to have a look at a scientific theory that allegedly has been suppressed. Of course, most of the cases I've looked at are “fringe” from someone or other's point of view. Early environmentalists were thought by establishment scientists to not really be doing science. Antifluoridationists have been stigmatised as cranks. The theory that AIDS came from contaminated polio vaccines is thought by many scientists to be crazy. The question is not whether to take up cases or theories that are considered “fringe” topics by the establishment. It is to decide how far I'm willing to go myself.

In the AIDS field itself, there are numerous competing theories. One that received a fair bit of attention, though still encountering enormous hostility, is that HIV — the so-called AIDS virus — is neither necessary nor sufficient to cause AIDS. Another is that AIDS originated from a biological warfare lab. And so it goes.

Then there are the inventors who claim to have discovered a source of unlimited energy. Out further on the fringe — at least by my reckoning — are claims that the moon landings did not occur but were simulated in a television studio and claims that the earth is hollow and populated on the inside too.

In deciding which topics to take up, at some stage I have to rely on my own assessment of the merits of the theory. I look for evidence that an unorthodox viewpoint is backed up by logical arguments and evidence. And I listen to what the defenders of orthodoxy say about it. Often their arguments make sense.

My view is that the craziest claims should be given some opportunity to be expressed and should be rejected on the basis of reasoned argument rather than arbitrary censorship. Take the case of Velikovsky, who proposed that many historical events and observed phenomena were due to other planets in the solar system moving out of their normal orbits and coming close to the earth. From the time of his first book, *Worlds in Collision* published in 1950, his writings were greeted with scorn by the scientific establishment. There were a number of instances when supporters of Velikovsky's ideas lost their jobs or were otherwise suppressed. Then and now, all but a few scientists would say that Velikovsky was wrong. But the way they attacked Velikovsky's work may actually have been counterproductive. Leroy Ellenberger, a Velikovsky supporter who later changed his mind, wrote me that the scientific community made a big mistake by not putting the necessary work into a detailed refutation of Velikovsky's theory — something that only happened decades later.

Scientists "knew" that Velikovsky was wrong because his theories violated established principles in several fields. They denounced Velikovsky because his books gained a wide popular following. This did not improve the image of the scientific community. The scientific establishment behaved like defenders of dogma rather than adherents of the "scientific method" involving organised scepticism, careful experiment and logical argument.

The Velikovsky affair points to an important aspect of suppression: it can occur even when the suppressed views are wrong — according to some later assessment. Of course, scientists have to get on with their work and don't have time to investigate every unorthodox theory that comes their way. But the dilemma is not really as difficult as scientists make out. Most of them never investigate any unorthodox theories with an open mind. If most of them just spent a little time doing this, it would do wonders for the public image of scientific open-mindedness and might even reveal a few worthwhile ideas. The suspicion of those who are wary of science is always that those few worthwhile ideas are actually a threat to vested interests within science.

Even for someone like myself who arguably spends a fair bit of time examining “fringe” ideas, choices have to be made. I can only follow up a small fraction of the areas that are brought to my attention. Often I have to say, “Thanks for sending me material about your case, but unfortunately I’m too busy to help.” What I can do is describe how to go about challenging suppression.

When people contact me about suppression, often I end up giving the same sort of advice. So I thought it would be useful to write down the basic points and get them published. I wrote an article titled “Letter to a dissident scientist” and submitted it to the journal *American Scientist* in January 1994. The editor replied shortly thereafter saying that it would take a few weeks to deal with it. Months passed and I wrote to find out what was happening. The editor assured me that I would be notified when a decision was made. Months passed. I wrote again. No response. I sent another letter that was not answered. Finally I asked a US colleague, David Hess — who has written about suppression of parapsychologists — to ring the editor. He did so in September 1995. This prompted a response and a promise by the editor to try to deal with referees’ comments in the next month. After hearing nothing for several months, I wrote again, pointing out that it had been two years since my article was submitted. The editor responded this time, apologising for the “extraordinary delay.” But the decision was to reject the article, since it did not fit the format expected by the journal.

### **Conclusion**

Publishing articles and books about suppression serves several functions. It provides support to dissidents: it is greatly encouraging for them to understand that their experiences are part of a wider pattern. Publishing provides legitimacy to the suppressed points of view and more generally to dissent. Publishing helps to build networks between dissenters, since articles provide a trigger and mechanism for them to find out about each other.

Publishing articles and books about suppression is often difficult. Great care is needed to make sure everything is as accurate as possible and that relevant people have a chance to comment. Probably the most important thing is persistence. Rejections can be depressing. All one can do is keep trying.

Is there suppression of information about suppression? It's hard to know. From my experience, getting something published is often difficult even when there is nothing controversial about it. I've often been impressed by how many editors and publishers are keen to take up the issue. The greatest difficulty is not a lack of sympathetic publishers but a lack of people to investigate suppression in the first place.

## 9

# Opponents of suppression

In my years opposing suppression, I've met a variety of individuals engaged in the same cause. Most of them are concerned only about their own case. This is understandable. Even a seemingly trivial case — a minor matter as perceived by outsiders — can shape the entire perspective of the person concerned.

A few survivors of major suppression cases then go on to support others. Ann Baker and Clyde Manwell are good examples. While it was Clyde who was the focus of attention during the years his position as professor of zoology was under threat, Ann was closely involved. Like Clyde, she was a talented and productive scientist. In terms of merit she might have expected a job far better than her position as casual tutor in the Zoology Department at the University of Adelaide.

Ann was also the target of attack. The university tendered in court a carbon copy of her job contract, referring to a clause at the bottom and arguing that she had worked too many hours. Ann was able to produce the original letter, which didn't have the added clause. In other words, someone had altered a document in an attempt to discredit her. This was only one example of the tactics used against Ann and Clyde.

Their intense experiences of suppression from 1971 to 1975 had a deep impact on them. Unlike many, they survived, and even more remarkably, they then began taking up the causes of others. Because of the prominence of their case, they were contacted by dozens of other people, and this helped them gain a wider perspective on suppression. They collected an enormous file of clippings from newspapers and journals with information on a variety of cases. They wrote letters of support to individuals and on their behalf. They wrote articles for journals.

I benefited enormously from Ann and Clyde's wisdom. They weren't always the most prompt of correspondents, but the

quality of their letters made up for the wait. Many of them were like miniature essays, filled with insights, information and references.

In 1985 Clyde came under a new sort of attack. The new professor and head of the Department of Zoology, W. D. Williams, claimed that Clyde's teaching load was "by far the lightest" in the department. This was the first complaint about his teaching. It so happened that it came shortly after Clyde obtained a letter from his doctor saying that he was suffering from hypertension and should have his teaching rescheduled (not reduced) to avoid stress. Williams's complaint did nothing to reduce the stress on Clyde, who was able to produce figures that suggested he had one of the highest teaching loads in the department.

Clyde decided to negotiate early retirement in 1986. He and Ann sold their property and moved to Selby, England, where I visited them in 1990. Since leaving Australia their letters have become less frequent.

Like all of us, Ann and Clyde saw the world in the light of their own experiences. The traumatic years when they were under fierce attack made them focus especially on dismissals, on organised attacks, on trumped up charges, and on the importance of proving one's case through documenting research productivity and the like. They were extremely good at these sorts of cases, but tended to interpret other sorts of cases in the same light.

Jean Lennane is another opponent of suppression who became involved after her own experience of being dismissed from the Health Department for speaking out. As a psychiatrist, Jean came in contact with many other whistleblowers, some of whom were referred to her. This gave her a broader picture of suppression than just her own experiences. In particular, she became aware of the severe health consequences for many whistleblowers. As president of Whistleblowers Australia, she became an effective public voice, making submissions and public statements, while also providing invaluable personal support to numerous individual whistleblowers.

While some people become opponents of suppression as a result of their own experiences, others take up the cause without any personal experience of being suppressed. Isla MacGregor is

one of these. I came in contact with her in 1991. She lives in Hobart, Tasmania and had become involved in a new group, United Scientists for Environmental Responsibility and Protection (USERP), which had been set up by some scientists and citizens who protested about environmental aspects of development proposals in Tasmania. Isla is not a scientist. She could better be described as a community organiser. She scrapes by on a supporting parent's benefit — for single unemployed parents with small children — and devotes every spare moment to organising on free speech and public interest issues. Indeed, she is one of the most efficient organisers I've ever met. She plans itineraries for visiting speakers and organises public meetings, media briefings and conferences.

Isla has taken a special interest in laws, found in every Australian state as well as federally, that make it an offence for government employees to speak out in public about matters relating to their work or agency. Although these laws are almost never invoked, they do a wonderful job of inhibiting free speech because workers fear that the law might be used to victimise them.

On the weekend of 27-28 March 1993, the first national conference on intellectual suppression and whistleblowing was held in Canberra. Sunday, the second day of the conference, was organised by Whistleblowers Australia. Saturday, the first day, was organised by Shirley Phillips, Isla and me. None of us lived in Canberra. Isla was in Hobart, I was in Wollongong and Shirley lived in Bendigo, Victoria, where she was fighting the administration of the local campus of La Trobe University. Isla took the lead in much of the planning for the conference. She also coordinated collection and production of a set of brief case studies which were circulated before the conference to all participants. Generally speaking, I'm a highly organised person, but with Isla looking after things I learned how tempting it can be to leave things to someone else.

As a result of the conference, Shirley, Isla and I set up the Network for Intellectual Dissent in Australia. There is no official organisation — it really is a network. We have a list of names of people willing to offer support to dissidents, talk to the media, write articles, etc. Isla, with help from me, handles the bulk of the

administrative load concerning the list. We also produced a leaflet: I wrote it and Isla distributes it. Isla periodically circulates "Media watch," a collection of articles from newspapers and magazines relating to suppression.

Some people might say just a few people like Jean Lennane and Isla MacGregor must be enough to handle all the cases of suppression around the place. If suppression was restricted to only a few prominent cases, that might be true. Unfortunately it's much more pervasive than that.

William De Maria teaches in the Social Work Department at the University of Queensland. He became interested in whistleblowing. He managed to obtain \$23,000 in funding for a three-year study. He set out to study whistleblowing, not to solve the problems of individual whistleblowers. His team produced leaflets and published advertisements asking for whistleblowers to contact them so that they could collect statistics. They were inundated with phone calls. Just as I and others had been saying for years, the publicised cases are just the tip of an iceberg of suppression.

De Maria wasn't interested in whistleblowing for purely academic reasons. He wanted to do something about it. Even just studying it was important, because it provided academic legitimacy to the phenomenon. As a result of the enthusiastic response of whistleblowers to the project, the Whistleblowers Action Group was set up. Soon they were swamped by people with stories to tell. There seemed to be a social movement waiting to burst out, a movement against the pervasive corruption and service to vested interests that is found in government and corporate bureaucracies.

Adding together the number of whistleblowers known to key people such as Bill De Maria and Jean Lennane gives, by extrapolation to all of Australia, a total of perhaps one thousand. This underestimates the total number, since some whistleblowers do not make themselves known, and is undoubtedly far less than the number of people subject to suppression, many of whom are not whistleblowers in the formal sense.

Bill notes that public interest activism cannot be built solely on whistleblowers, since so many of them are traumatised by their experiences. Others need to be involved too. Bill thinks that

lawyers and journalists together with whistleblowers would pose a very powerful challenge to corrupt government bodies.

### **Selective opposition**

There are many people who will say they believe in free speech but who are only willing to defend the speech of people with whom they agree. Unfortunately this applies to opposition to suppression.

One day I was describing the harassment of Patrick O'Brien, a right-leaning political scientist from Perth, to a left-leaning colleague, also a political scientist and a person I respect greatly. My colleague had been subject to fierce and relentless character assassination because of his own political views. But he disappointed me when he said words to the effect, "You shouldn't be defending O'Brien. He's a ratbag and deserves everything he gets."

There is plenty of evidence that for many decades in capitalist countries, left-wingers have been subject to far more suppression than right-wingers, and not just during the McCarthy era in the US. Left-wing trade unionists are prime targets of employers, and left-wing newspaper columnists are few and far between. But just because they have been victims of suppression does not mean that left-wingers are any more tolerant. Every indication is that most of them would welcome the opportunity to suppress their critics, if only they had the power to do so.

The same applies to many other issues. Antifluoridationists have been subject to fierce attack, but I've seen little evidence that they would be any more tolerant of profluoridationists. Most of the partisans on each side believe wholeheartedly in their own cause. What they want most of all is to have their views reign without opposition. A free and open discussion in which people make up their own minds may be a means to this goal, but it is not the goal itself. The side in any debate that doesn't engage in suppression is usually the side without the power to do so. My view on this was confirmed when I read a book by Nat Hentoff with the self-explanatory title *Free Speech for Me — But Not for Thee: How the American Left and Right Relentlessly Censor Each Other*.

Suppression can also occur within social movements, which is

ironic since social movements are so often subject to suppression themselves. I'm not talking about suppression of the other side, but of people within the movement — heretics. I've heard first-hand about liberal feminists who have been harassed and slandered because radical feminism was the dominant version at a particular workplace. I've heard about environmentalists who have lost jobs at environmental organisations because they didn't follow the right line. Within movements, though, it's not popular to reveal internal strife, since this is thought to provide ammunition to the opposition.

One of those who overcame the inhibitions is Tim Doyle, an environmentalist and academic researcher who studied the role of powerful figures in the Australian environmental movement, especially in relation to the campaign over the wet tropical forests of north Queensland and the national election in 1987. He named key individuals who he said were the "professional elites" in the movement and described their actions and interactions. He told about connections with the Australian Labor Party, about large corporate donations to environmental organisations, and about the alienation felt by many grassroots activists in the movement.

This critique of the movement could have provided a welcome opportunity to reflect on goals and organisational structures. Instead, Tim's analysis triggered fierce attacks on him that persisted for many years. Eventually he was able to write an article cataloguing the many different arguments used to say why he shouldn't have published his critique. Needless to say, the ideas of freedom of speech and tolerance for dissent did not play a big role in these arguments.

Tim did not work for an environmental organisation, but his prospects would not have been good if he had. His critique was not a great asset in getting an academic job in environmental studies either. Nevertheless, he thinks it was helpful in getting Australian environmentalists talking and to some extent altering their practices.

I played a small role in this debate. Dharendra Sharma invited me to be guest editor for several issues of his journal *Philosophy and Social Action*. In one of them, I collected together Tim's article "Environmental movement power brokers," an article by

Lorna Salzman about the centralisation of power in the US branch of Friends of the Earth, and a critique of Greenpeace by an environmental activist who adopted the pseudonym Hazel Notion. Later, *Chain Reaction*, the magazine of Friends of the Earth Australia — a much more radical organisation than the US branch — drew on material from this issue of *Philosophy and Social Action* for a special issue of its own. This led to a predictable set of attacks, especially on “Hazel Notion.”

I’ve been an active supporter of environmental causes since the mid 1970s and know and respect a great number of environmental activists. The environmental movement is, I believe, one of the great inspirations of recent decades. Yet in any list of its virtues, tolerance is not high. Many leading environmentalists, I’m convinced, believe in democracy and popular participation only if they help environmental causes. If they were running the show, critics wouldn’t stand a chance. This is speculative, of course, since environmentalists have seldom had much real power to organise society, in spite of complaints made by captains of industry.

My editorial in the special issue of *Philosophy and Social Action* was titled “Power tends to corrupt,” taken from Lord Acton’s famous aphorism, “Power tends to corrupt and absolute power corrupts absolutely.” This, in essence, is the root of the problem. Those who have power find it enormously tempting to use it against dissidents. David Kipnis, a psychologist, has provided empirical support for Lord Acton. In numerous ingenious experiments, Kipnis has shown the mechanisms by which power affects the beliefs and behaviours of those who exercise it. It does indeed tend to corrupt.

### **Defending their right to speak**

It is a classic statement of tolerance to say, “I disagree with your statements but I defend your right to make them.” In my studies of suppression I’ve made some attempts to provide balance, but it’s impossible to be perfect in this. As just mentioned, I’ve drawn attention to suppression in social movements with whose goals I sympathise. But what about suppression of people or views with which I disagree? That’s more difficult.

In all my studies of suppression of nuclear experts, it was almost always critics of nuclear power who were suppressed. The nuclear establishment had most of the power, after all. I know of only one case to the contrary. Bob Phelps of the Campaign Against Nuclear Power in Brisbane wrote in July 1979 to the Vice-Chancellor of the Australian National University, arguing that Professor Sir Ernest Titterton's pronuclear writings contained errors and should not be distributed by the ANU. (Many of Sir Ernest's articles, which later appeared as newspaper articles, were first published as working papers in the Department of Nuclear Physics.) It's characteristic of attempts at suppression for complaints to be made to a person's boss, not directly to the person.

I agreed that Sir Ernest's writings were filled with dubious claims, but I didn't want their publication blocked. That wouldn't be a good precedent for me, since I was writing further outside my official field of employment than was Sir Ernest. So I wrote to the Vice-Chancellor saying I defended Sir Ernest's freedom to publish his work without censorship. I expected, correctly as it turned out, that the Vice-Chancellor would defend Sir Ernest, but I wanted my views to be on record. A policy of tolerance is most helpful to those who have the least power.

Similarly, when researching the fluoridation issue I looked for cases of suppression of profluoridationists, but found only one case: a health magazine had rejected a profluoride response to an anti-fluoride article. For the book *Intellectual Suppression*, I compiled a chapter called "Archives of suppression" which contained thumbnail sketches of various suppression cases, mostly based on accounts in articles and books. Very few of those subject to attack could be said to be conservatives. Most of them were radicals in one sense or another. In the Australian section, I was able to document three cases that went against the usual pattern. One involved Patrick O'Brien, who I already mentioned. O'Brien made biting attacks on left-wing thought and action, in both print and on radio. An attack was mounted on him, including a court case over his behaviour at a social event, concerning some trivial matters.

Another case was that of Frank Knopfelmacher, a psychologist at the University of Melbourne who was well known for his

commentary on social issues and his opposition to Soviet communism. In 1964 he applied for a position in philosophy at the University of Sydney. The selection committee unanimously recommended Knopfmacher but, after much organising by left-wing opponents, the appointment was blocked by the Professorial Board in an unprecedented move.

The third case was that of Professor Arthur Burns, a political scientist at the ANU and a prominent anticommunist. The ANU Council terminated his appointment in 1981 on medical grounds, but never gave an official reason. Burns went to court to seek the reason, and won. The ANU appealed and won — so it didn't have to give a reason. Burns battled on and eventually his case was settled out of court, so in a sense he won. In a fitting epilogue, the ANU administration's intransigence came back to haunt it. In 1994 ANU students occupied administration offices to protest against introduction of high fees for certain graduate courses. Burns joined the students and stiffened their resolve by telling them of his own experiences. Burns died the next year.

For each of these three cases, I spent many hours collecting documents — I visited O'Brien in Perth in 1984 — writing accounts and checking them with the individuals concerned. The end product was a few paragraphs about each case, all for the sake of "balance." But it was useful to be able to show that anyone can be subject to suppression, and that the same sorts of processes are used whatever the view suppressed.

It would be an illusion to imagine that it is possible to defend dissent in a completely balanced way. I certainly haven't done this. I've looked especially at suppression of environmental scientists, and of scientists and academics generally. I haven't paid as much attention to suppression of critics of psychiatric orthodoxy, suppression of dissent within churches, or attacks on lesbians, though there is evidence in each of these areas, as well as many others. I'm reluctant to put lots of energy into certain types of cases, such as defending researchers who come under attack for investigating racial differences in intelligence. What I do try to do is emphasise that suppression is a general problem, that it affects a wide range of issues and individuals and that it is likely to occur whenever groups have the power to carry it out.

Some critics of suppression are much more selective in their

outrage. The case of PC illustrates this well. The term “political correctness” was originally used as a humorous and gentle reminder within the left to beware of becoming too self-righteous about stands on issues such as sexist language or views on certain issues. “PC” has now become a term by which to attack policies aimed at reducing sexual or ethnic inequalities, among others.

The anti-PC bandwagon has not had much influence in Australia, and therefore there are no prominent cases of alleged PC-inspired suppression for me to investigate at first hand. From what I read about North American cases, there seem to be a few excesses, but relatively little in the way of what I call suppression. There are cases of university professors being criticised for their use of language. But criticism is not suppression. There are cases of official investigations of professors for statements they’ve made in class, and some of these seem to fit the usual pattern of suppression. But I haven’t heard about many being demoted or losing their jobs.

Certainly I oppose the squelching of dissent by “PC police.” But the concern seems one-sided. There have been headline stories in major newspapers and magazines about the great danger of PC. What about all the documented cases of left-wing activists losing their jobs? What about all the lesbians who are harassed out of their positions? What about the environmental activists who are physically attacked and in some cases killed? What about the US government surveillance and harassment of the Central American solidarity movement? What about the attacks on those who try to expose government corruption? Like I said, I oppose suppression under the guise of PC, but it is wise to keep this in perspective. This example shows, if there were any doubt, that “suppression” is a category that is constructed by the people who are concerned about it. To take action against suppression does not automatically serve the cause of those who need help the most.

### **Suppression of opponents of suppression**

Opposing suppression can be a risky business. You could come under attack yourself. Those who have independent means are in the best position to take up the cause. Jean Lennane, once she set up a private psychiatric practice, was in a good position to take a

prominent role in Whistleblowers Australia. Similarly, Isla MacGregor, though as hard a worker as anyone, is not in paid employment and thus is relatively invulnerable to attack.

But there is no ultimate protection. John McNicol set up the organisation that became Whistleblowers Australia and worked tirelessly on many cases. He also earned a few enemies. John organised a media conference in Canberra for 26 March 1993, the day before our national conference in Canberra on intellectual suppression and whistleblowing. There were several of us there before the cameras. Most of the journalists were quite sympathetic. But one wasn't: Norman Abjorensen from the *Canberra Times*. Out of the blue, he asked a series of hostile questions about John's credentials and business affairs, and then promptly exited. The next day there was a damaging article about John in the *Canberra Times*, entitled "Campaigner coy at the sound of the whistle." It cast a shadow over the intellectual suppression conference. There are still a lot of people who believe that if something is in the newspaper, there must be some truth in it. The main point, though, was that John's credentials and business affairs were largely irrelevant to his activities in defence of whistleblowers. I'm told that the *Canberra Times* did not publish John's reply, but it did publish a letter from Jean Lennane about the general issues.

There are several lessons in this. No one is invulnerable. An attack may come at the most unexpected time and from an unexpected source. Anything about your life can potentially be used to attack your reputation, and of course false allegations or insinuations may be used as well. In these circumstances, it is best to be open and honest in advance rather than keep dirty secrets. Best of all is not to have dirty secrets at all! But this is unrealistic. Defenders of dissent are no closer to perfection than anyone else, although people seem to expect them to be.

Fortunately, attacks such as by Norman Abjorensen are rare. No other journalists took up this story, and privately several people told me their opinion of him. Attacks like this can easily backfire, causing sympathy for the person attacked.

People occasionally comment to me, "Surely you must come under attack, considering the stands you take." In my present job I've had no problems. My colleagues have been supportive and

there have been no murmurs from the administration. That's the way it should be! Of course there might be some complaints that no one's told me about.

Things were different when I was a research assistant in applied mathematics at the ANU. As described in chapter 7, in September 1980 my talk to the National Science Forum was featured on television that night. Later that week, Archie Brown, head of my department, called me in. He said that he had received a call that same night from another academic in the department, complaining about my comments. Later I was told by someone in the ANU's publicity office that this same colleague had also called the Vice-Chancellor that night to complain, saying I shouldn't be allowed to say such things. However, he never made the same comments to me then or in the years following. I never told him that I knew about his calls.

A more serious issue arose a few years later. I was finally in a position to apply for tenure as a research assistant. The Dean of Science rejected my application. It so happened that the Dean at the time was Eric Bachelard, the Professor of Forestry who had written to me via the Vice-Chancellor claiming he could find no evidence that Richard Routley had been barred from the Forestry Department library. Bachelard knew about my critical remarks about forestry. In my view at the time, there seemed to be a conflict of interest for him to judge my application for tenure. But from his point of view — as stated in a letter to me in response to a draft of this chapter — he had to act with the approval of the Resources Committee, which was axing all internally funded research assistant positions. So it didn't matter who was the dean of science, especially since I didn't have sufficient support from my department. Archie Brown has retired and the pure and applied mathematics departments had been merged. I think the real source of my problems was that I was an applied mathematician and hence not wanted by the pure mathematicians who were in charge. There had been a long period of animosity between some members of the two groups.

Early in the 1980s, the ANU changed its regulations and for the first time allowed nonacademic staff to inspect their files held at administration. (Academics still weren't allowed to see their files.) As a research assistant I was a member of "general staff"

and so went to look at my file. It contained lots of junk, including job applications, notes requesting holiday leave and so forth. But it also had some interesting bits. I had written an article about academic power structures for the *ANU Reporter*, a university publication. There was a copy of my published article in my file, with many sentences underlined and with various comments in the margins. There was a copy of a memo from one university bureaucrat to another concerning my claim about the number of people denied tenure before Jeremy Evans. And there was a copy of Eric Bachelard's letter to me with the claims about Richard Routley. (There was no reason for such a letter to be on my file, except that Bachelard gave the letter to the Vice-Chancellor to give to me.) I couldn't get items removed, so I wrote a letter to the Personnel Manager giving my side of the story and asked that it be put on my file. (I doubt that it was put on Bachelard's file too!) This was no trivial matter. In those days the file was used whenever a person applied for a job at ANU.

To check my account of these events, I asked Jo Kamminga to look through my ANU file on my behalf. He sent me copies of several letters on my file. One of them was a reply by the Vice-Chancellor to my letter to the Personnel Manager. This was the first I knew about it. He hadn't bothered to send me a copy.

I recall these minor incidents to illustrate the sort of responses that may be made to opponents of suppression. In my case, I found out a little about the responses. Some people find out a lot, but most learn little or nothing. That, in some ways, is the worst part about it. If direct charges are made, then you can defend against them. It is the behind-the-scenes activity that is hardest to counter. Of course it is behind-the-scenes activity that is most characteristic of suppression.

### **Where are they?**

Studying suppression is not a popular activity. In the hope of finding some others in my field who were or might become involved, I sent out the following message on a computer conference on science and technology studies (STS):

To: sci-tech-studies conference  
From: Brian Martin

Date: Mon, Jul 11, 1994 1:36 PM

Subject: sts: analysts needed for suppression studies

For many years I've been studying cases of suppression of intellectual dissent. Due to my writings and through networks in which I'm known, from time to time I'm contacted by individuals about particular cases and issues. Some of these are worthy of a significant STS analysis. For instance, there are cases in which a scientific theory is ignored or dismissed for allegedly "inappropriate" reasons. Three recent instances: a critique of the standard idea that there is a bee "language"; a theory that tooth-grinding behaviour is tooth-sharpening behaviour; a theory that group differences in IQ may be linked to a high load of proviruses among African blacks. I do not have the time to undertake a proper study of all these issues.

If you are potentially interested in studying controversies, dissident ideas and/or cases of suppression of dissent, I would be most happy to put you in touch with people who are looking for someone to investigate and/or document their cases. Please tell me the particular types of issues you are interested in.

Only five people contacted me in response. One of them, Jay Ou, was willing to look into certain types of cases. The other four were too busy to look into cases of suppression, but either wanted information from me or wanted to tell me about situations that they thought I might want to investigate. It was not exactly what I had in mind! Perhaps the most surprising response was from Dick Sclove, who told me about cases of suppression of dissent in the STS field, some of which I already knew about. But the situation in STS is not quite as bleak as it may seem. I have met a few others in the field who have studied suppression in one way or another, especially David Hess who is studying the field of alternative health and Bart Simon who has mirrored my web site on suppression. This is more than in many areas of study.

There's certainly plenty worth studying. There are, of course, numerous cases of suppression, many of which provide a fascinating insight into the dynamics of an area. Few cases sit clearly and obviously in the sun, waiting for somebody to examine them. They have to be sculpted out of the raw materials, just as the Jeremy Evans tenure case was transformed by many people from a routine administrative decision into an indictment of the university's harassment of the Human Sciences Program.

### **Conclusion**

I originally titled this chapter “The making of suppression opponents,” but after writing it I realise I don’t really know what makes people into such opponents. Many people take up their own cases, and often a few of their friends and colleagues provide support. But few of these people take the step of regularly helping others.

Rather than investigating the supposedly unusual psychology of dissidents and of defenders of dissidents, it may be more useful to study conformists and conformity. Why are most workers afraid to speak out? There are various factors, including bureaucratic structures that stifle criticism, career paths that reward conformists, political structures that discourage genuine popular participation, and the general belief that experts and officials really know best. Looking at all these obstacles, it’s a miracle there are any dissidents at all.

## 10

# Taking action

If you have come under attack for your dissent or are thinking about speaking out, the first thing you should do is seek advice from people who have experience with opposing suppression. But often there's no one available, in which case you should consult some writings on the topic. The best single publication on this is *Courage Without Martyrdom: A Survival Guide for Whistleblowers*. It's published by two groups based in Washington DC, the Government Accountability Project and the Project on Government Procurement, both of which have tremendous experience in defending whistleblowers. Much of this handbook is specific to the US, but the general points have wide applicability. *Courage Without Martyrdom* at the beginning lists eight "survival strategies," which are worth listing here. They are very much in tune with my experience.

- Before taking any action that may lead to attack, consult with family and close friends. You need their support.
- Before taking action, see if there is some way to achieve your goal by working within the system.
- Try to find out if there are other people, especially co-workers, who share your concerns.
- Behave fairly with other staff. They may also have encountered harassment from bosses and be able to testify on your behalf.
- Keep a detailed record of events. When something really important happens, write up a statement and sign it in front of a witness if possible.
- Make copies of all important documents that you can. Your case depends on them.
- Find honest supporters, including politicians, journalists and community organisations. Develop a plan for taking the initiative; don't just respond to actions by the other side.

- Obtain advice about taking legal action. Don't overstate your case.

*Courage Without Martyrdom* gives more detail on each of these points, plus much more valuable information. The advice is aimed at whistleblowers who work in government or corporations, but many of the suggestions are helpful for those who encounter suppression outside these particular circumstances.

In the first half of 1996, I visited all the capital cities in Australia and took the opportunity to interview whistleblowers individually and in groups. Of the questions I asked, the two most revealing were "What were the most important lessons from your experience?" and "What do you wish you'd known at the time?" As expected, different individuals gave different answers. After all, each person's experience is unique. But there were also some recurring messages, often expressed with great passion.

- Don't trust the system. "The system" here refers to the organisational hierarchy of the workplace and the external agencies for pursuing complaints. This sentiment was expressed in various ways: "don't trust the organisational hierarchy"; "don't rely on the system"; "it's a waste going through the system"; "external agencies are useless." A special place was reserved for trade unions: "don't trust your union." A few, on the other hand, found their unions helpful. But the basic message was the same. One person said "don't trust anyone!"

- Be prepared for any conceivable attacks. Many whistleblowers learned a bitter lesson: that when they spoke out, there was almost no limit to what might be done to shut them up. "Realise the depths they'll go to protect themselves." "They will do anything to beat you." "Anything is possible." "Ethics don't mean a thing." "Expect the worst."

Many whistleblowers expected to be listened to openly and treated fairly. Instead, they found they had few allies and were attacked in unexpected ways. This was summed up in another lesson: "don't be naive."

- "Document everything." Many whistleblowers wished that they had collected more records and held off speaking out until their documentation was greater. "Collect more records." "Hang in longer, collecting information before blowing the whistle." "Store documents first."

There were lots of other useful hints mentioned by one or more people, including:

- use more tact;
- don't despair in adversity;
- never give up;
- timing is crucial;
- don't blame yourself;
- publicity is valuable.

When I asked whether there was written material that was helpful to them at the time, many said there was nothing, or perhaps a newspaper article. The document most commonly recommended was *Courage Without Martyrdom*.

What if you are not under threat yourself, but want to help those who are? Simply listening to dissidents is immensely important. You can suggest different options, mentioning their strengths and weaknesses, and thus help the person make a better decision.

If you want to become overtly involved, you must take advice from the person most affected — the dissident. Action might be “just” writing a letter of support. This is more important than it seems. It shows outsiders and the dissident that someone else cares. There are many other things that can be done, including attending meetings with or on behalf of the dissident, helping prepare submissions, collecting and checking information, finding witnesses, writing leaflets and articles, investigating claims and organising media coverage. If there is a campaign around the case, there is the task of coordination. This is a major commitment. It involves finding and keeping in contact with supporters, collecting and distributing information, holding meetings to work out strategy, making links with journalists, and perhaps organising action such as petitions, public meetings, vigils and rallies. In order to be inspired to take up a suppression case, a potential organiser needs to know why it's important. There needs to be a goal.

### **Strategy**

A strategy can be thought of as a plan for getting from the present situation to a future goal. To develop a strategy, it's first necessary to understand the present. This includes details about

what happened in a suppression case but also an analysis of the vested interests involved, the various alliances and contradictions within the social structure, the meanings that people place on events, and so forth. Then there's the goal, namely what is being aimed for. The strategy is the plan to get there, taking into account everything known about the present.

What is the goal? In many cases, the goal is to stop the suppression. This might mean ending harassment on the job, getting controversial articles published, or obtaining compensation for wrongful dismissal. In other cases the goal is to stop a corrupt activity or fix a hazard. Be careful. The goal may not be as obvious as it may seem at first.

In the case of Jeremy Evans and the Human Sciences Program, one goal was tenure for Jeremy. But this was only part of a wider set of goals, which might include — depending on who you ask — putting the Human Sciences Program on a secure, stable and autonomous footing, establishing interdisciplinary environmental education as a valid part of the university, and contributing to a stronger and more effective environmental consciousness throughout society. Getting tenure for Jeremy seems a reasonable short-term goal but, some might argue, only if it contributes to achieving the broader, long-term goals concerning environmental education and social awareness.

To take another example, consider Brian O'Brien's legal action for defamation against Mark Diesendorf and the Australian Conservation Foundation. One goal of the ACF was to win or settle the suit without massive cost. A wider goal was to be able to continue its campaign on climate change and for its employees to be able to make statements about the greenhouse effect without being met by threats of legal action. A wider goal yet might have been to change defamation laws so that honest discussion of scientific and social issues is fostered rather than inhibited.

People may disagree about long-term goals. Whatever they are, it is important to keep them in mind when planning campaigns. The big danger is to put all the attention on the short-term goal and to lose sight of the long-term goal.

Sometimes dissidents haven't worked out even their short-term goals. One of the first things I ask people is what they want to

achieve. Sometimes they can't provide a very good answer. They want justice, they want to expose the nasty things the other side has been doing, or they want to get everything back the way it was before. This isn't specific enough or realistic enough. The short-term goal needs to be clearly defined and achievable.

Probably the most important thing to do to develop a strategy is to actually spend time working on it. Rather than act on the basis of anger or inspiration, sit down and examine the situation. How do things stand at present? Who are current supporters? Who are potential supporters? Who are actual and potential opponents? What actions can be taken to win greater support or to undercut opposition? What actions are people willing to take? What will be the financial costs? How much time will it take? Is it worth the effort? What are the different options? What are their pluses and minuses? What happens if everything fails? Is it worth taking the risk? What are the options down the track? Is this the best time and place to make a stand? Does this action contribute to long-term goals?

For big campaigns, it is worth doing some study into methods of social action. It's also worthwhile to contact experienced social activists and obtain their advice.

### **Prevention**

One of the most creative challenges for a dissident is to avoid suppression in the first place. At the national conference on intellectual suppression and whistleblowing in Canberra in 1993, several of the whistleblowers made the same point: don't just leap to make a protest. Instead, figure out the situation. Talk to others and build support. Collect documents, make copies and save them in safe places. Carefully analyse who has power and who is likely to use it. Be prepared to use the media if necessary. Finally, choose the right moment to speak out. Don't just be principled — be effective too. *Courage Without Martyrdom* has the same message, as the title indicates.

Hugh DeWitt has worked for many years as a theoretical physicist at Lawrence Livermore National Laboratory, where US nuclear weapons are designed. Hugh has been the Lab's most vocal critic of US nuclear weapons policy, writing articles, giving testimony, and speaking to journalists. It seems incredible that he

has been able to survive in his job for so long. He has certainly had some close calls, when pressures were put on him. What has protected him is the support he has built up from a variety of outside groups: individual scientists, anti-nuclear groups, politicians, journalists and others. Of course, he is careful in what he does, for example in not revealing secret information. But while being careful, he is also courageous. When he comes under threat, he is able to call on supporters. The possibility of massive negative publicity for the Lab if he were ever dismissed is undoubtedly the reason he has survived in his job without compromising his principles.

Suppression will certainly continue to occur so long as some groups have much more power than others. As long as organisations like the Lawrence Livermore National Laboratory exist, then life will be difficult for dissenters like Hugh DeWitt. For he is the exception. Most of the lab's workers have no intention of speaking out, and indeed few of them see any reason to dissent in the first place.

The same problem applies wherever suppression occurs: corporations, government bureaucracies, political parties, media, churches, trade unions, and so on. You can dream up all the procedures that you like, but as long as a few people at the top have lots of power, they are likely to use it to squash dissent.

### **Formal channels to the rescue?**

Confronted with the abuse of power, most people think the solution is to get rid of the corrupt individuals and bring in honest rulers and bosses. People vote governments out of office, companies introduce enlightened management, and bureaucracies institute reforms. This may provide some temporary relief, but it doesn't solve the fundamental problem. As noted in the previous chapter, power tends to corrupt. The new rulers may start out with the best of intentions, but are likely to succumb to the temptations of power.

As described in chapter 6, many whistleblowers put high hopes in official channels, even after the official channels have failed time and time again. Is it any wonder that great hope is placed in a new official channel, whistleblower protection legislation? Unfortunately, there's little basis for hope. To begin, most

governments are reluctant to pass such legislation. When they do, it is usually hedged with restrictions. For example, the Queensland government's whistleblower law only gives relief if the whistleblower goes through "proper channels" — and this means not going to the media. The experience of whistleblower protection legislation in the US is basically that it doesn't work. It has been sabotaged by the people running the agency. Few cases are ever pursued and hence few whistleblowers are protected. *Courage Without Martyrdom* discusses the various US channels that whistleblowers can use, such as hot lines, Inspectors General, Congress, and laws. Not a single one of them works well. The Office of the Special Counsel was set up to protect whistleblowers, but it actually has served more to give them false hope and even harass them. One head of the Office of the Special Counsel, Alex Kozinski, even went so far as to help others attack whistleblowers: "Using the OSC's own investigative manual as a guide, he taught a course for federal managers on how to fire employees without OSC interference."

It almost looks like a conspiracy. When popular concern about bureaucratic corruption reaches a peak, a government may pass some legislation to protect whistleblowers and hence give the appearance of doing something even though not much has changed. But it doesn't require a conspiracy for this to happen. Governments are very reluctant to make it possible for their employees to freely make criticisms of policies and managers, since the whole government apparatus is built on control from the top. Those politicians who genuinely want to do something must operate within the standard parameters, which means setting up another government agency. If an agency ever gets set up, it is starved of funds and in any case soon makes its peace with the other bureaucracies with which it must work. If it stirs up too much trouble, it will be squashed. Finally, whistleblower protection only ever helps those who fit the official definition of a whistleblower. Many of those who are suppressed do not fit this definition.

There's no denying that whistleblower legislation may help a few individuals. The danger is to imagine that such legislation is *the* solution to the problem, and to put lots of energy into pushing for it.

Bill De Maria, who headed Australia's largest study into whistleblowing, is severely critical of whistleblower laws. After scrutinising the provisions of whistleblower protection laws passed or proposed in Australia and New Zealand, he concluded: "Whistleblower legislation is an exercise in damage control. Whistleblowers are fettered by rigid legislation which defines wrongdoing and public interest disclosures, and sets out the narrow pathways they must travel in order to receive 'protection.' In other words, the state has effectively colonised the potentially subversive activity called whistleblowing. Or so it thinks."

The weaknesses of whistleblower legislation are one aspect of the weakness of formal channels generally, to return to the theme of chapter 6. In a study of the responses of external agencies to whistleblower disclosures, De Maria and Cyrelle Jan found that the most common were no action, a negative response and referral to another agency. In less than one out of ten cases was the response positive, such as protecting the whistleblower.

If a government really wanted to do something to help the cause of intellectual freedom, it could abolish financial penalties for defamation, get rid of laws prohibiting free speech by government employees, and provide arms-length funding for free speech organisations and whistleblower support groups. But recommending what governments should do is a prescription for frustration, since it so seldom succeeds and in any case avoids the more useful response of taking action oneself.

### **The strategy of mobilisation**

Quite a few of the whistleblowers who I talk to have already tried formal channels, almost always without success. For example, they might have talked to their boss about stealing at the workplace, only to find that they come under attack themselves. Should they contact top management? Should they make a complaint to the ombudsman? Should they write to the prime minister? Based on the evidence, I say, it's probably a waste of time proceeding this way.

So what's the alternative? I think there is a much greater chance of changing undesirable practices and avoiding reprisals by trying to win support. This can be called a strategy of mobilisation,

since the goal is to mobilise people to provide passive or active support.

The first step is to see if there are others in the organisation who will support you. If even two or three are willing, you can meet informally with them to discuss tactics. (Be careful. Remember, some whistleblowers advise “don’t trust anyone.”) If you have even one other who is willing to support you openly, it can make quite a difference.

However many are willing to act from within — just yourself or a group — it is difficult to bring about change without support from the outside. The next step is to bring outsiders into the picture. This can be done by individual discussions, but often it is valuable to produce a written account of the problems and recommended solutions. A one-page summary of the issue is often most effective. It can be accompanied, if appropriate, by a package of supporting documents. This might be, for example:

- documentation of misuse of an organisation’s monies;
- information indicating the likelihood of scientific fraud;
- evidence that a government department is covering up sexual exploitation;
- statements about police frame-ups;
- a collection of newspaper clippings about animal disease outbreaks and cutbacks in disease surveillance services.

Every fact in the document should be checked and double-checked and the finished product should be seen by someone knowledgeable about defamation. It should also be read by someone not familiar with the issues to ensure that it is well written and easily understood by outsiders.

The document can initially be circulated only to people likely to have a special interest in the issues. The aim should be to expose the wrongdoing to people who have no vested interest in hiding it. The document could be given to, for example:

- an organisation’s employees;
- an organisation’s clients;
- scientific peers;
- families and neighbours of those covering up corruption;
- influential figures in the community with an interest in the area;
- all people entering a relevant building.

Distribution of the statement needs to be carefully thought through. Taking action this way can be extremely powerful. It can mobilise support but also lead to greater attacks. So be prepared. The way to respond to attacks is to expand circulation of documents, or produce new ones exposing the attacks.

Another step is to contact the media. Journalists may find out about the issue anyway once a statement is circulated. So have a media strategy. Know what you want to say — including a 30-second grab — and what action you want official bodies to take.

You are not guaranteed of media coverage. Sometimes the media will not touch the case, perhaps because it is not considered newsworthy, is too complex, is too dangerous due to the risk of defamation suits, or because the media have strong links with the wrongdoers. Media coverage can be very helpful for mobilising support, but it should not be relied upon. By circulating your own document to people you choose, you have some degree of control.

There are many variants on this approach, since each case has its own particular characteristics. It might be that e-mail provides a useful method of mobilising support. It might be that you can get someone on the outside to write an account for you. You might have contacts in high places, such as parliament, who can act on your behalf. But the basic approach should be clear. It is to mobilise support by alerting ever more people about what is going on.

What about using formal channels at the same time? This is certainly possible. The dynamics of formal channels and mobilisation are quite different, though. Sometimes they conflict. If you are using formal channels, people may think that will provide a solution, so why be concerned? On the other hand, a sophisticated campaign can be built by using formal channels as a tool for mobilisation. For example, the presentation of a complaint to an official body — or the body's failure to act — sometimes can be used to generate publicity. The danger is that formal channels will suck up so much energy and hope that there is not enough left for a strategy of mobilisation.

Some whistleblowers are long-time activists and use the mobilisation strategy as a matter of course, with great skill. Others, by contrast, instinctively use formal channels and only

later, after disillusionment with the system, are willing to consider other approaches. For such whistleblowers, circulating leaflets, organising meetings, giving speeches and talking to the media are not easy. In my view, this approach has a far greater chance of success than relying only on formal channels. If anything is likely to make the formal channels responsive, it is popular exposure and outside pressure. Anyone can learn the skills. It's a matter of getting advice from activists who are familiar with the techniques. There's no guarantee of success, but at the very least quite a lot of people will become aware of the problem. That in itself is a significant achievement.

### **Structural change?**

Sometimes a campaign against suppression contributes to a wider challenge to the social structures that lead to suppression. A good example is the campaign in defence of Tim Anderson, an activist wrongly accused of a major crime. In February 1978 a bomb exploded outside the Hilton Hotel in Sydney during a meeting of the Commonwealth Heads of Government. Three people were killed and others injured. The Prime Minister immediately called out troops to protect the visiting officials. The police and government blamed "terrorists." But terrorists usually claim responsibility and on this occasion no one did.

The police spied on a socio-religious group called Ananda Marga. In June 1978 they arrested three members of Ananda Marga for conspiracy to murder the leader of a neo-fascist party. The trial received enormous attention, for the police alleged the three had confessed responsibility for the Hilton bombing, although this was not pursued in court. Based on testimony of a police informer, Richard Seary, the three went to prison.

Many people thought this was a frame-up. Seary's evidence was dubious. Defenders of the three in prison mobilised. After years of appeals, inquiries and hearings, the last of which exposed lying by Seary and police officers, in 1985 they were freed by the government, pardoned and paid compensation.

Two of the three retired from the public eye. The third was Tim Anderson. He became a prominent social activist, especially in exposing police corruption. In 1989 he was arrested and charged over the Hilton bombing. He went to trial and prison. An

even more massive campaign was launched to prove his innocence. Eventually it succeeded and he was acquitted in 1991.

Political trials are not common in Australia, and the victimisation of Tim Anderson angered many people. An entire organisation was formed, Campaign Exposing the Frame-up of Tim Anderson or CEFTA. It held meetings, organised research, raised money, generated publicity, produced a newsletter and generally did everything possible to support Tim. After two years of action, CEFTA was successful and Tim was exonerated. After his release from prison the second time, CEFTA became Campaign Exposing Frame-ups and Targeting Abuses of Authority. Eventually the name was changed to Justice Action. It continues to expose frame-ups and police corruption. This important on-going effort was triggered by opposing a single case of political victimisation. It shows how a campaign against suppression can be linked to struggles to change the social structures that produce suppression.

### **Being self-reliant**

Just as it is not wise to rely on honest rulers to dispense justice, I think that it is best not to rely on a small number of individuals to be the main defenders of dissent. It's a taxing role, and it's also virtually impossible to consistently defend dissent in every possible circumstance. But my main point is that it will be more effective for many more people to become defenders of dissent. As whistleblower researcher Bill De Maria says, we need to create a "culture of dissent."

When you are involved in a suppression case, whether your own or someone else's, it should be a top priority to build up skills in effective dissent — your skills and those of others. That means learning how to articulate ideas, organise a campaign and work with like-minded people. It also means developing and exercising principles.

Don't rely on "experts" to do everything for you, whether they are lawyers, trade union officials, knowledgeable friends or others. By all means seek advice but try to be self-reliant. Learn as much as possible yourself and help others to learn. Write letters and get advice on improving. But also help others to write letters. Organise meetings. Plan campaigns. These are all skills

that can be learned.

I know that I can only help a few people. I also know that often I can't be of much help, since I'm too far away, not knowledgeable enough, or not familiar with the local situation. Furthermore, dealing with suppression is not my full-time job. Other research, teaching and activities take priority. The best advice I can offer you is to have the confidence to do things on your own.

If you want to take action against suppression, there's plenty to do. I can recommend it. It's challenging, revealing, stimulating and worthwhile — and you get to meet the most interesting people.

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# Suppression Stories

## Brian Martin

In spite of the prevailing rhetoric of freedom, it can be risky to question the established way of doing things. Whistleblowers, dissidents and others who run foul of powerful interests are potential targets of attack. They are harassed, ostracised, threatened, reprimanded, transferred, censored and dismissed.

In *Suppression Stories*, Brian Martin describes experiences and insights from years of studying and opposing suppression of dissent. The book covers patterns of suppression, the problem of defamation, peer review, formal channels, the role of media, difficulties in opposing suppression and advice for dissidents. It uses numerous case studies to illustrate suppression and methods of dealing with it. *Suppression Stories* provides a personal account of how to go about investigating and resisting suppression.

**Brian Martin** has a PhD in theoretical physics from Sydney University and now works as a social scientist at the University of Wollongong. He is the author of numerous articles and books in diverse fields including numerical methods, astrophysics, wind power, scientific controversies, environmental issues, strategies for social movements, participatory democracy, information technology and nonviolent defence. He has been active for many years in the radical science, environmental and peace movements. He became involved in suppression issues in the late 1970s, was lead editor of the book *Intellectual Suppression*, helped found the Network for Intellectual Dissent in Australia and in 1996 became national president of Whistleblowers Australia.

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