

Chapter 10

Corporatisation and intellectual property

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The Case of Ines Carrin

In January 1994, Dr. Ines Carrin took up a postdoctoral Fellowship in the Centre of Molecular Biotechnology at Queensland University of Technology, “The University of the Real World,” as its logo proclaims. She was to do research on banana genetics.

Carrin was born in Argentina and educated first in Brazil, then at Macquarie University where she took her Honours degree, and at Cambridge, England, where she obtained her Ph.D. She came to QUT from a research post at Princeton, USA, with very strong references. A minor blip on her appointment was that she was offered a post below that advertised, but after negotiations was appointed within the advertised range. This post ended in February 1996, but she continued employment on research grant money until August 1997.

The Centre of Molecular Biotechnology had an ongoing research programme on banana genetics under the direction of Professor James Dale, Head of the School of Life Sciences. The focus of the work was Banana Bunchy Top virus (BBTV), which is endemic in Australia and damages the banana industry throughout the world. To eradicate the disease would have major economic consequences.

She proposed in her application to work on inserting genes into banana plants, by a process known as “transformation,” using a micro projectile bombardment technique. The ultimate aim was to attack BBTV by creating a virus resistant banana. Prior to her appointment there was in the Centre no work on transformation, of the kind she

was proposing. It took her until April 1995 to establish a successful tissue culture, and by August, she had sufficient material to see if a plant could be generated. She then experimented to see if foreign genes could be inserted into the cells and whole plants produced. By October 1995, it looked as though she was successful. These experiments are recorded in lab books.

In April 1996, Carrin says she was told by Dale to hand her work, using micro projectile bombardment, over to a Ph.D. student, AB. He had started work on the transformation project at the beginning of 1995, using agro-bacterium, but without much success. He was now to pursue the bombardment technique developed by Carrin, leaving her, as Dale put it, “free to concentrate on the real important stuff,” another process, infectivity. She felt very resentful over this, as she claimed ownership over the important aspects of the bombardment process, and wanted to see it through herself.

Dale insisted, but did agree that her name appear on any publications arising from the work on transformations. She later came across a multi-authored manuscript in preparation in which her procedures were being used, but her name was not as a co-author but footnoted as having provided relevant material. This was not co-authorship as she saw it.

She then got on with the infectivity aspect of the project, as instructed. She inserted the DNA from the Bunchy Banana Top virus itself into the banana cells, using a technique she had brought from Princeton. This was an exciting breakthrough. She told Professor Dale about it. He too was excited, and in his excitement asked her to hand the work over to another graduate student. Carrin expected to be working with the student, CD, and gave her the DNA primers she had designed, her “recipes” for making the long chains of multimers, showed her how she introduced the DNA, and gave CD photocopies of her lab book.

However, in two presentations by the students in July and September, 1996, she received no acknowledgment for her contributions. She complained to Dale, who told her AB had simply forgotten to do so.

The Diary of Professor James Dale, Banana Geneticist

In May 1997, an advertisement entitled “The Diary of Professor James Dale, Banana Geneticist” appeared in the *Weekend Australian* and the *Brisbane Courier Mail*.

In diary format he outlined the progress of the project:

- August 1994. His aim: to conquer BBTV by developing a virus resistant banana by inserting the right gene.
- June 1996. "...after months of trial and error, it's only today that something quite brilliant happened. Inspecting the cell culture we saw actual banana embryos. Now, if we can get the embryos to germinate, if they do take then maybe, just maybe, there's hope..."¹
- September 1996. Germination noted
- December 1996. Regeneration of transformed plantlets and the assay for the marker gene inserted
- He also announced his research team: Lecturer GH, and two of his Ph. D. students, AB and EF.

Similar statements appeared in *Inside QUT*, the university paper, except that AB was reported to have "master-minded" the project. There was again no mention of Dr. Carrin; nor was there in another article in *Natural Life Review*.

Carrin had some problems with all of this:

1. She was a member of the team, and in her view had done the important groundwork, in both the transformation and infectivity aspects of the project. Her name was not mentioned in the advertisement or in the articles, except as a footnote.
2. AB was announced as having master-minded the project, when in 1994 AB was an Honours student working on tea plants, and had had nothing to do with the start of project. This statement was particularly bitter to her given that he had been appointed to take over from her against her will.
3. The order of events was seriously in error. Carrin's lab books record that she had already produced the embryos, transformed the cells, and got the embryos to germinate, in 1995, the previous year. The dramatic expression of hope in June 1996 that all this would take place in the future would appear to give a misleading impression of what had happened, and of her contribution.

Her perception by now was that she had not only been deprived of ownership over what she saw as her intellectual property, but of any association with two crucial contributions she had made to the work of the Centre. And by the end of August her contract was due to finish. She had contributed three years of fruitful work, with nothing solid in the way of publications — essential for an academic's career — to show for it, only assurances that her name would appear on some papers, and three minor poster presentations at conferences.

She went to the Chancellor, Dr. Cherrell Hirst, who promised her that her interests would be protected, and that she would be given the opportunity to finish her work. She also approached the Director of the Centre for Molecular Biology, which housed the department of Life Sciences, but was told that he did not want to get involved.

She was by now very upset. She confronted the students, AB and CD, to see if she was receiving any acknowledgment of her work in their ongoing lab books. AB refused point blank to show her anything. CD showed her a partial copy of her lab book, and Carrin found that neither the idea nor the procedures were acknowledged as coming from her. Further, the pages she saw had all been countersigned by CD and GH, CD's supervisor. This is most unusual practice. Technically, one has a lab book countersigned by a witness once a fortnight or so, but not every day by both parties. It further seemed that these were all signed on the one occasion, and back-dated. Carrin was furious and photocopied the whole of CD's lab book — to find that the only pages to have been co-signed were those that had been shown to her. This elaborate routine only convinced Carrin that she was to be excluded:

Actually, I could see a pattern developing. I do the work, the students get the credit, you know so this year AB is the banana master-minder transformer; next year it's going to be CD, the infectivity breakthrough author.

By now the atmosphere in the Centre was very tense. Carrin felt marginalised. She was no longer allowed to order any materials, or to use the electron microscope, unless approved by Dale or GH. She found her materials had been tampered with, and her equipment switched off in her absence. Strangest of all, a book was left on her desk. It was about people who were difficult to get along with.

University grievance procedures

Dale complained that Carrin was harassing his students, which he saw as a matter for Human Resources to deal with. Vice-Chancellor Dennis Gibson agreed.

Carrin however insisted this was a matter of scientific misconduct, falling under the National Health & Medical Research Council (NHMRC) Guidelines, which universities must accept if they receive NHMRC research funding. The relevant misconduct here is defined as: the lack of appropriate acknowledgment of work primarily produced by a research student, trainee or associate.

Under NHMRC rules, a university must have advisers on integrity in research, who can give confidential advice to staff or students with these kinds of concerns, and have a designated person to receive formal complaints.

So she tried to institute formal proceedings. She asked who was the Science Faculty's adviser on integrity. It was the Director of the Centre for Molecular Biotechnology, whom she had already approached, and he had refused to help her on the grounds that he did not want to become involved. He had not then revealed that his role was as adviser on integrity.

The University set up an enquiry, but there were delays because Dale was overseas, and a letter informing Carrin that the enquiry was about to start was sent to the wrong address. However, her perception was that the enquiry was not addressing her complaints, but Dale's.

Accordingly, days before her employment ceased and with no other prospect in sight, she went to the ABC. Norman Swan went to air with her story on 16th August 1997.

Vice-Chancellor Gibson sternly reprimanded her for this: "The person has chosen to go public with her version of events before the investigation is finalised. This may be seen to have compromised the investigation."²

What then is this person supposed to do when her employment finishes within days, and her deeply felt grievances are not, to say the least, being treated sympathetically? This is the classic dilemma of the victim of establishments, as we have seen in Chapters 3 and 9, where Orr and Spautz had exactly this experience.

The pivotal issue: The question of ownership

Norman Swan tried to clarify the matter of ownership in the *Science Show* report. The following interchange with AB occurred:

Swan: And who gave you that study, that second part, the redirection of your work?

AB: It's a group decision, since the agro-bacterium was not producing results.

Swan: Well, whose idea was it?

AB: The micro projectile bombardment is a technique that's been around since 1987, so it's a logical step that once we have a suitable target tissue for micro-projectiles to use that system.

Swan: And whose system was it that you took over?

AB: Oh. Can we stop the recording for a moment?

Swan: Well, what I'm asking you is who owned that original research. My understanding is that the research was ...

AB: Oh, I see what you're getting at. Um, well, um well I wasn't aware that you were going to take this line of questioning.

Swan: Well I did say that I was focusing on the ad, and your work at QUT.

AB: So I'm really, um. I'm not really authorised to speak about, or really...

Swan: I mean wasn't it Dr. Ines Carrin's work that was handed to you in 1996?

AB: I mean you realize the position I'm in. There is absolutely no way I can comment on things like that.

Swan: Why not? In an open lab surely it's pretty open sharing of ideas?

AB: I have responsibility to QUT to, not to comment on, to leave those sorts of things to more senior people.

Swan: But it's your research, your Ph.D. that you're currently working on. You must know where the ideas and the techniques originally come from?

AB: I can't comment on any of this, and I think we should just terminate the interview, really.

Swan: I've got a paper in front of me called 'Promoter activity associated with the intragenic region of Banana Bunchy Top Virus and transgenic tobacco and banana cells' in which you're the second author. I notice that Ines Carrin's name does not appear in this letter although I've got quite a lot of lab evidence that a lot of the work in here is based on her work. Can you explain why she's not an author of this paper?

AB: No, I said more senior people would have to respond to questions like that.

Swan: And what senior people would have to respond?

AB: You would have to speak to Professor James Dale.

Professor Adrian Gibbs, who was one of Dale's Ph.D. supervisors at ANU, wrote to *The Science Show* afterwards: "I was appalled at the way that (Swan) tried to trap the young Ph.D. student into making comments about his supervisor. It was disgraceful."³ Possibly so, if that was what Swan was trying to do, but it was necessary in order to clarify Carrin's input into the ideas on which AB's thesis was based.

A crucial issue in the dispute is the nature and extent of Carrin's contribution. This is not easy to adjudicate. A researcher is likely to have a different view of his or her own contribution to a team project than other members of the team. The team leader theoretically has a better overview of the relative contribution of individuals. It is also extremely difficult for non-specialists — the writer of this chapter for instance — to assess the importance of an individual's contribution to highly specialised research work.

Carrin herself was in no doubt of the originality of her contribution, and neither was Swan, who had seen her lab books. But AB's interview reveals that micro projectile bombardment had been used in Belgium since 1987. Carrin did not invent the process, she had gone to Belgium to look at the technique, but could not import it as fully operational because it would have risked bringing in other diseases with the tissue culture involved. Consequently, in Brisbane she had to start from the beginning. So the question becomes: Was her role a technical matter of replicating an existing procedure under different and untried conditions, or was it a matter involving genuine creativity?

Dale saw it as the former, Carrin as the latter. Dale also claims that Carrin was proceeding too slowly, whereas AB was making "excellent progress." He accordingly allocated the project to AB, transferring Carrin to the infectivity project. Her Fellowship had now run out, and she had to be employed from dedicated research funds. Dale said this meant that she had to work in line with what the funds had been provided to do. However, this too is open to interpretation. When there is a series of inter-related projects, it is often difficult in practice to separate the funding for one project from that for another; they tend to feed each other. Dale said that in moving her to infectivity, he was

offering her the opportunity of senior authorship in a new field. And indeed she certainly developed this aspect successfully — only once again to be removed from it soon after.

Carrin herself seems not to have realised that from February 1996 she was employed on different terms, and so she saw the move as part of an overall pattern to exploit her ideas.

“I didn’t get a chance to defend myself”

Dale had told Swan, but off the record, that the advertisement “The Diary of Professor Dale, Banana Geneticist” was designed by an agency and that Dale had originally supplied 20 names to be listed, but he was restricted to mentioning only four. This is likely to account for the overly dramatic, and inaccurate, retailing of the progress of the project and its membership. However, as Dale had been forbidden to speak to Swan, this information, which gives quite a different view of Dale’s actions, could not to be broadcast.

Dale was naturally angry and upset at the broadcast: “I didn’t get a chance to defend myself!”: the University prevented him from talking until its own enquiry had been completed, and Swan would not delay the broadcast.⁴ The Director of the University’s Public Affairs Department had informed Swan that neither QUT as a body, nor staff members individually, would be making any comment. However, if Swan had delayed the broadcast, it would have been too late for Carrin. She would be already out of a job.

The Pitman Enquiry

One result of the *Science Show* was that the University aborted its own enquiry, and appointed Professor Michael Pitman, chief scientist with the Department of Industry, Science and Tourism, to investigate. Another result was that Carrin’s contract was extended for a few months, which rather vindicates Swan’s decision to go to air when he did.

Pitman was given two terms of reference:

1. to investigate the allegations by Dr. Ines Carrin to determine whether a sufficient case exists for formal charges of misconduct to be laid.

2. to investigate and report to QUT Council on broader issues associated with the management of research at QUT and other matters arising from (1).

His tasks were to determine if Carrin had any substance in her allegations within the framework of the NHMRC guidelines; and if the procedures operating in QUT to investigate such problems as Carrin had found were satisfactory.

The first Pitman Report addressed the first term of reference, and an advisory committee chaired by Pitman and three distinguished external academics addressed the first part of the second term of reference. The second part of the last term of reference was addressed by an internal committee, convened by Associate Professor Jan Lovie-Kitchin, Chairperson of the University's Research Management Committee.

The first Pitman Report reported back within weeks, and concluded that "no case exists for formal charges for misconduct or serious misconduct to be laid."⁵ The account of events given by Carrin and Swan on the *Science Show*, and by Pitman, are very similar. As was mentioned on the *Science Show* on the 8th September, QUT demanded an apology from the ABC for the errors contained in that programme (of 16th August), "but the trouble is that the Pitman Report has not identified any. The differences are over the interpretation of the facts." A major difference was the interpretation as to Carrin's contribution, and hence whether the acknowledgments she received were in fact appropriate, and not constituting "misconduct."

Pitman saw Carrin's contributions as "a substantial contribution to the group's work."⁶ He agrees it would be "appropriate" that papers on transformations should be recognized with her as joint author. Likewise, while papers on infectivity should not be "solely" authored by her, "there are very strong grounds for Dr. Carrin being included in papers following up this discovery but until the papers are conceived no case can be entertained for organisation of authorship."⁷ He notes that Professor Dale agreed with this analysis and had given assurances that "her work would be recognized in publications from the group." By August 1997, however, no publications according this recognition had emerged, except for one poster presentation in 1994, and two posters scheduled for September and October, 1997, and her name had not been associated with any patents or patent applications.⁸

The paper on the extraction of promoters from BBTV acknowledges Carrin in the methods section. But she claimed that she provided the idea of working on banana cells and the material; further, Dale had agreed her name should appear as author. Pitman did not believe that “either of these grounds provides strong reasons for authorship.” Her complaint that AB had not acknowledged her in the two presentations in 1996 “was an honest error and there was no intent to deprive Dr. Carrin of recognition.”⁹

The lack of any recognition of her work in the “extracts” from Professor Dale’s Diary is noted by Pitman, as are the errors in fact, for example that Carrin had already done the work that Dale had been reported as dramatically claiming to be the next steps. She was part of the team, Pitman agreed, and that it was “unfortunate hyperbole” that AB had been allotted the role of “master-mind” for the project. However, unfortunate as all this was, these articles are not grounds for misconduct “because they cannot be construed as scientific publications and set out to be illustrative not definitive. None the less, I can understand Dr. Carrin’s grievance with the advertisement.”

Dale was exonerated from any misconduct in removing her from the transformation work on the grounds mentioned above. Basically, as team leader, it was up to him to judge who was most suited to what aspect of the project, given that he claimed that Carrin had to change direction anyway, according to the designated funding for her salary.

As to Dr. GH’s role, Pitman found that he had signed CD’s laboratory books using incorrect dates. The reason given was that CD was worried about providing copies of her notes to Carrin. However, as Pitman points out, this predictably and reasonably reinforced Carrin’s view that there was a conspiracy by the group against her. Pitman found that Dr. GH’s behaviour was “totally unprofessional ... when the essence of scientific credibility is accuracy in reporting.” However Pitman did not see it as appropriate to lay a case for misconduct “as there was no impact on acknowledgment of Dr. Carrin and no attempt to change the recording of (or alter the significance of) research data.”¹⁰

Other allegations of lack of acknowledgment by Carrin were not considered to be “sufficiently substantial to warrant being treated as misconduct.”¹¹

So there is no argument about what happened. Pitman’s conclusions were simply that what happened in each of the allegations was not enough, individually, to warrant action under the ethical guide-

lines. They were honest mistakes, or dishonest but little mistakes. The only person who was severely criticised was GH, but his actions did not affect Carrin's position.

The research climate at QUT

This method of dealing with the allegations does not address what Carrin (and no doubt many listeners to *The Science Show*) saw as the problem. The little mistakes can be seen to form a consistent pattern, which is further consistent with what Carrin referred to as the "atmosphere" at QUT, and the Centre for Molecular Biotechnology in particular:

There was a secrecy within the group I had never experienced in my whole career ... In other labs and people were producing papers and they would give you copies of their manuscript in preparation for you to comment ... When people were doing experiments they would come and talk to you.

Carrin was not the only one who felt this. Professor Jim Burnell, now at James Cook University, had worked in the Centre for Molecular Biotechnology at QUT but quit the job without a job to go to, and risked the dole rather than stay on. One reason was a disagreement over intellectual property rights, and another was the general atmosphere in the Centre for Molecular Biotechnology:

... I was not happy with the way I had been excluded from being involved in academic matters at QUT and that my future employability would be seriously disadvantaged if I continued at QUT. There was no atmosphere of inclusion. Our input into research discussions was not sought ...

The second Pitman report recognises these problems existed in the School of Life Sciences. This aspect appears to have been dealt with mainly by the internal committee, comprising eight academic staff, and one each of a laboratory manager, a postdoctoral fellow, and a graduate student.

The School of Life Sciences as a whole is identified as needing special assistance to overcome uncertainties and low morale: "... there is a feeling of tension and disquiet among academic staff, researchers, and postgraduate students."¹² It is unclear whether this is directly

referring to the Carrin case, or to an incident involving the CMB earlier in 1997, when there was widespread public concern over unsafe procedures involving the deadly Japanese encephalitis virus, an enquiry leading to the suspension of a senior scientist,¹³ or to both incidents. At all events, Recommendation 12 of the *Review of Research* reads: “That the Vice-Chancellor address, as a matter of urgency, issues about the research environment and staff morale in the School of Life Sciences.” Another point with a possible bearing on the Carrin case was classified as “advice received,” rather than as a formal recommendation. This was the option of appointing postdoctoral fellows for three instead of two years.

The second report contained nothing else relevant to “issues arising from” the Carrin case. All the other Recommendations are general, concerning the management and state of research at QUT. One would have thought that such an enquiry into the management of research would have been more specifically directed towards the terms of reference, and to have addressed seriously such issues as had been identified by Swan. Why, for instance, did the individual appointed to the School as an adviser on integrity in research refuse to become involved when approached by Carrin specifically in that role? Why did an enquiry sought by Carrin to address her complaints about Dale, proceed instead to address Dale’s complaints about Carrin?

Recommendation 12 suggests that the Vice-Chancellor investigate, albeit as a matter of urgency, the very issues in which he had been involved in part. This is hardly likely to allay Carrin’s concerns.

In summary, the second report was essentially a broad-ranging and no doubt useful document to the University. When however it could have addressed the real, structural problems that allowed the Carrin Case to happen, it became very coy about the details.

The fruits of corporatisation?

Bessant (Chapter 4) described QUT as having a very authoritarian style of management. If the “inner circle” wanted something done quickly, structures were largely ignored, as Carrin herself discovered.

QUT originally had no research culture. But with developments in the 1990s, one had to be developed quickly. This was done with an emphasis on “practical” research, with close ties to industry. Patents were important revenue earners. It seems very likely that in this

atmosphere the issue of intellectual property also became corporatised, so that the idea of the individual scholar having ownership over her or his creativity gives way to corporate ownership. This seems an appropriate way of interpreting the experiences of Carrin and of Burnell, from his statement quoted above. The introduction of a commercially sensitive note breeds secrecy, competition, and exclusion, bolstering the public image with grand public statements, and references to the “real” world. Dale was also a victim of this, when the University did not allow him to defend himself.

This kind of ethos is markedly different from the traditional academic atmosphere, in which seeking knowledge is a public, collegial activity, conducted and reported according to agreed criteria that attest to the validity of what is reported. Accordingly “publication” is an important index of scholarly activity; once discovered, knowledge is to be shared with the scholarly community, due recognition being granted to individuals according to their contribution. Academics set up networks of knowledge-sharing, so each can benefit from the work of others, once recognition of that work has been established in the conventional ways.

In the corporatised university, it is the institution that matters. Research is done collectively, which requires an authoritarian leader who allocates the roles, and makes the big decisions; Middle Kingdom-style research, if you like. Recognition of the individual contribution is less important than the good of the institution as a whole, what is “good” being decided by senior management, not by the community of scholars.

QUT is not the only university the research in which may have been affected by corporatisation. Both La Trobe University and the University of Melbourne have signed agreements with commercial developers, which prohibit academics at these universities from engaging in “competitive” research.¹⁴ The effect, in the words of Dr. Gideon Polya, a La Trobe biochemist, is that “I am subject to some kind of constraint on scholarly enquiry by an agreement to which I am neither privy nor signatory.”¹⁵ This is anathema to genuine scholarship. But it is the way Australian universities are being required to go; it is a predictable and inevitable consequence of private funding.

Another aspect of this is that the two kinds of ethos, the academic and the entrepreneurial, attract or mould different kinds of people. Carrin, from the academic tradition she experienced previously at Macquarie, Cambridge and Princeton, noted and did not like what she

saw at QUT. By the same token, QUT did not like what they saw: an individualist, a trouble-maker, and “difficult” person. Swan asked her honours supervisor from Macquarie, Professor Keith Williams, if he thought she was a “difficult” person:

... she’s a refined person, she’s very cautious and careful about how she approaches things, and she pays tremendous attention to detail so she’s a terrier if you like when she gets hold of something, and some people find that intimidating I guess ... Personally I find it quite delightful because I think it’s the way good science is done.¹⁶

What we seem to be seeing in both Carrin and Burnell are not “difficult” persons, but ones whose way of doing research in the old scholarly tradition did not fit the ethos developed at QUT. Under these conditions, it is likely that matters become personalised and highly charged. The villains in these two cases were not Dale and his colleagues, or Carrin and Burnell; all are victims. No person or group of persons was the villain. Rather, problems grew out of patterns of organisational dynamics that prevailed at QUT due to procedures that did not work, structures that invited short-circuiting, and policies of secrecy that not only damaged Carrin, but also Dale, and the University itself, when these dynamics prevented entry into public debate on air.

Pitman’s report on Carrin’s allegations does not appear to take this big picture into account. He treats each such incident as isolated, a series of molehills, not the mountain Carrin had perceived, or indeed the mountain that is recognised — albeit enshrouded in fog — in the second *Report on research at QUT*.

Thus, while Carrin’s work on infectivity deserved recognition, until the papers are conceived “no case can be entertained for organisation of authorship.” “Having the idea” in itself does not provide strong reasons for authorship. “Acknowledgment” does yeoman’s service for co-authorship. Her omission from the student presentations is an “honest” error. Even the errors in Professor Dale’s Diary become unimportant because the Diary cannot be construed as a “scientific” publication. AB’s role as “master-mind” is “unfortunate hyperbole.” Thus, Carrin’s case is dismantled, piecemeal. The major problems at the University, particularly in the School of Life Sciences, are therefore likely to remain unaddressed, despite Recommendation 12.

It seems a pity that Justice Green did not adopt this piecemeal approach in his ruling on *Orr versus the University of Tasmania* in 1956. If he had, Australian academic history would have been very different, and it is even possible this book need not have been written.

Acknowledgements

The details of this account derive from several public domain sources: a transcript of the Science Show, presented on ABC Radio National 16 August 1997; two reports to the Council of Queensland University of Technology by Professor Michael Pitman; and several press reports as acknowledged below. I am much indebted to Dr. Norman Swan who presented the programme, and who provided some background details; to Professor Pitman, for facilitating access to QUT sources, and to Dorothy Illing of *The Australian* and Tania Ewing of *The Age* for providing copy of articles they had previously published. I had asked Professor James Dale and the Vice-Chancellor of QUT, Professor Dennis Gibson, to point out any factual errors and/or to make a statement, which I undertook to publish. Professor Dale had no comment to make, on the grounds that he had no confidence that they “would be reported in any way other than that reflected by the general tone and direction of the chapter.” Vice-Chancellor Gibson expressed disappointment at the inaccuracy and unfairness contained in the chapter, and suggested that the contents were defamatory. In a reply, I assured him that I was indeed trying to be fair to all parties, which was precisely why I had asked him and Professor Dale for comments. He replied that it was not incumbent on the University to correct my work, and repeated his concern that the chapter was based on the ABC programme about which the University had raised serious concerns to the ABC. He refused to correspond any further on the matter. I was thus unable to find out what *specifically* concerned QUT, about either the ABC programme or the present Chapter. In light of Professor Gibson’s reaction, I have modified the text in several places, and refer to junior staff and to students by sequential initials. My intention is not to damage individuals, however high or low in the hierarchy, but to argue how, as a matter of public interest, the climate of corporatisation is distorting academic processes, and how individuals working on traditional academic lines are likely to be hurt.

Notes

1. As quoted in The Science Show, Australian Broadcasting Corporation, 16 August 1997. Unless attributed otherwise, all subsequent quotations are from the Science Show transcript.
2. *The Australian Higher Education Supplement*, 20 August 1997.
3. Tania Ewing, "Weird science: how the media made the professor mad," *The Age*, 15 November 1997.
4. Science Show transcript, op. cit., pp. 10-11.
5. *Pitman Report*, p. 6.
6. *Ibid.*, p. 10.
7. *Ibid.*, p. 11.
8. *Ibid.*, pp. 10-11.
9. *Ibid.*, p. 11.
10. *Ibid.*, Appendix 2, p. 1
11. *Ibid.*, Appendix 3.
12. *Review of Research at Queensland University of Technology*.
13. *The Australian Higher Education Supplement*, 15 April 1998.
14. *The Australian Higher Education Supplement*, 21 October 1998, p. 38.
15. *Ibid.* Gideon Polya is the son of J.B. Polya, Professor of Biochemistry, University of Tasmania (1953-1988), who played an important role as critic of that University's administration during and after the Royal Commission (see Chapter 3).
16. Science Show transcript, op. cit.