



Looking back at the 1987 Cervical Cancer Inquiry

The Auckland Women's Health Council (AWHC) would like to respond to the article by Barbara Heslop entitled '*All about research*'—*looking back at the 1987 Cervical Cancer Inquiry*, which appeared in the NZMJ on 6 August 2004 (<http://www.nzma.org.nz/journal/117-1199/1000/>).

The article is of considerable concern to AWHC members as it represents a misguided attempt to rewrite history and casts doubt on a significant event in the history of the medical profession and the development of patient rights. The AWHC has among its members women who sat through the whole Inquiry into the Treatment of Cervical Cancer at National Women's Hospital as well as others who gave evidence or attended some of the sessions. The AWHC was formed at the beginning of 1988 and has always had a special interest in the issues that arose during what has become known as the Cartwright Inquiry.

Over the past 16 years, the Council has been actively involved in the implementation of the recommendations contained in the Report of the Cervical Cancer Inquiry including actively supporting the establishment and ongoing development of the National Cervical Screening Programme, the establishment of the office of the Health and Disability Commissioner, the development of the Code of Rights, and the nationwide patient advocacy system.

It is simply not possible to respond in the form of a letter to the editor to the number of unsubstantiated claims in Barbara Heslop's article. Most of the claims made in the article are in fact refuted in both the Cartwright Report and Sandra Coney's book, *The Unfortunate Experiment*. The AWHC would also draw attention to the fact that the 5000 pages of evidence contained in the transcripts of the Inquiry are also publicly available and prove that the issues Ms Heslop raised were all thoroughly canvassed during the Inquiry.

For example, it was irrefutably demonstrated at the Inquiry that there were medical researchers both here in New Zealand and throughout the world who not only knew about "the unfortunate experiment" that was underway at National Women's Hospital but that during the 1960s and 1970s some of them actually visited the hospital and met with Herbert Green. Many in the medical research community were horrified by their observations during their visits, and word of what was going on spread far and wide among the research community. People like Ralph Richart openly challenged Green at international symposia.

It simply isn't true that Professor Green worked in isolation. The evidence presented during the Inquiry revealed that he was in fact supported in his views by other senior doctors at the hospital—Bonham, Jamieson, and Liggins.

The AWHC would also point out that Bill McIndoe and Jock McLean were not as ignorant as Ms Heslop attempts to make them. It was demonstrated very clearly at the Inquiry that, contrary to Ms Heslop's assertion that neither man "had given much serious thought to scientific hypotheses," both knew exactly what they were doing and had spent decades fighting Green and attempting to protect women whom they

knew to be in serious danger of developing cervical cancer because they were not being treated. The families of these two men believe that the stress they were under contributed to their early deaths. This is why a decade ago the Auckland Women's Health Council and Women's Health Action held a special ceremony during which a pohutukawa tree was planted and a plaque referring to their work placed at the foot of the tree. So their efforts would not be forgotten—or misinterpreted and maligned by those who come after.

We must not forget that over 30 women died as a result of being part of “the unfortunate experiment at National Women's Hospital” and their untimely deaths were entirely avoidable. The 1960s and 1970s were not the Middle Ages of medical research that Ms Heslop's article would make them out to be. There was in fact an international framework arising from the Nazi experiments during World War 2 that set a clear standard for ethics of research that 50 years later meets the test of today.

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Response

I fail to see why I was “misguided” in recording my perception of medical research over the half century during which I was involved with it. Nor do I delude myself that I, or anybody else, can “re-write history”. Because I am not quite sure what it means, I make no comment on the assertion that I “cast doubt on a significant event...”. I should perhaps point out that medical scientists, among whom I number myself, are apt to see life as long on doubts and short on certainties.

It is also difficult to comment on the “number of unsubstantiated claims” that I am said to have made, since the single example cited by Ms Williams was not a claim that I had made. I certainly did not maintain that doctors and researchers in NZ during the 1960s and 70s did not know that Herb Green held unorthodox views about cervical pathology. I was well aware of it, as were many of my colleagues. What many of the NWH staff lacked was the scientific “know how” that might have prompted them, when presented with a hypothesis, to ask “Is this hypothesis testable? What sort of evidence will it take to falsify it? Will the evidence be easy to get? How long will it take? Is this the best way of getting the evidence? Are there other ways in which the hypothesis could be falsified? How feasible are they?” and so on. Those with service commitments—clinical or laboratory—usually have neither the time nor the need to ponder on the best ways of dealing with research problems.

There is nothing wrong with holding unorthodox scientific views as long as one can justify them. Much productive research, after all, involves disagreeing with somebody or something. This is the way the scientific world operates—disagreement and questioning are its lifeblood. Trouble arises not because of differences of opinion per se, but when the accuracy of the data on which the opinions are based becomes suspect. There is little doubt that the NWH hierarchy took far too long to take a hard look at the quality of Herb Green's data. Nevertheless the main point of my article, which I reiterate, is that had the scientific “know how” of the main protagonists been more sophisticated than it was, Green's hypothesis could have been disproved

(falsified) in a few months, if not by Green himself, then by McLean. I don't for a moment doubt that McIndoe and McLean knew exactly what they were doing (most of us do). It is unfortunate that they (and especially pathologist McLean) missed seeing that they were almost certainly sitting on archival hospital material that would have allowed them not only to disprove Green's hypothesis quite quickly, but also to publish the relevant findings without reference to Green. My tentative answer to the question "Why didn't they do it?" (because they were not researchers and it did not occur to them) is rather more charitable than Ms Williams' assessment (that they did indeed know, but chose to take a longer and incomparably more stressful route to their destination).

Pointing this out does not entail "misinterpreting or maligning" anybody living or dead, nor does it detract from the significance of what they eventually did. It is no more derogatory than commenting that my parents' generation could have communicated more effectively had they been familiar with today's information technology, or that I could have approached yesterday's immunogenetic problems more effectively had I used today's molecular biological techniques. It merely serves as a reminder that yesterday's research is apt to have obvious shortcomings when viewed from the vantage point of today's knowledge.

Being challenged by the leaders in the field on one's home ground or at conferences does not necessarily mean that one's opinions are wrong, a point well illustrated by Bryan Sykes in *The Seven Daughters of Eve* (Corgi Books 2001, pp 190–193). What it does mean is that one's evidence had better be pretty good, and freely available to anybody who wants to scrutinise it minutely. Those who present material at international conferences—from plenary sessions to posters—expect to be challenged. For heaven's sake, why would anybody go to a conference if this were not going to happen?

Finally, the assertion "The 1960s and 1970s were not the Middle Ages of medical research that Ms Heslop's article would make them out to be". Actually I specified those years as occupying the dawn of the current golden age of biology, and noted the shortcomings of "a lot of medical research and especially clinical research" at that time. Those who are familiar with the allocation of money for medical research will know that for the last 30 years or so, clinical research has experienced difficulty attracting funds in competition with scientifically more sophisticated biomedical research projects. This has been a world-wide phenomenon, and has prompted various solutions, including the establishment of multidisciplinary collaborative research groups whose members have complementary skills. Whatever label one chooses to attach to Green's working years, today's clinical research is hugely different from most of that practised in the 1960s and 1970s. It is not too hard to see why the change had to happen.

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