

“Unfortunate Experiment or Medical Uncertainties?”

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The Cartwright Inquiry – or the 1988 Inquiry into the Treatment of Cervical Cancer at National Women’s Hospital – was an investigation into medical practice over a period of thirty years – it was in effect an inquiry into medical history. And it was as an historian of medicine that I came to question this document, resulting in the publication of my book, *A History of the ‘Unfortunate Experiment’ at National Women’s Hospital*, in 2009. As I will explain, some of the Inquiry’s pronouncements about past medical practice were clearly ahistorical – failing to acknowledge the changing context of medicine, portraying it as black and white, and not appreciating the ever-changing nature of what is regarded as standard or conventional medical practice. So-called ‘proper’ treatment is constantly subject to revision, it is not immutable or a fixed body of knowledge. Medical practice is filled with uncertainties, as Tony Baird noted in his recent Presidential Address, whereas the public (and it seems the court-room) demand straight black-and-white answers. Those who have defended the Cartwright report have continued to present the issues in black and white terms. In my talk this evening I will share with you how I came to question some of the conclusions of the Cartwright Inquiry, and my responses to the more recent criticisms of my book.

In 1987 a Committee of Inquiry was set up, presided over by District Court Judge Silvia Cartwright, to investigate the treatment of carcinoma in situ of the cervix at National Women’s Hospital during the previous three decades. This was sparked by an article in the

Metro magazine by feminist activists Sandra Coney and Phillida Bunkle which claimed that gynaecologist Dr Herb Green had endangered the lives of women coming to National Women's with a positive cervical smear, by not treating their condition but simply monitoring it in the pursuit of scientific research.¹ Cartwright concluded in her Report that the details of the *Metro* article had proved to be correct, that the authors had gone (she said) to extraordinary lengths to discover the truth, and that Green and his colleagues at National Women's Hospital had 'failed in their duty to patients'.

First, to provide some historical background – in the 1950s there had been tremendous excitement in New Zealand as elsewhere that cervical cancer could be eradicated by the use of a new test, called the Pap smear test, to detect an early pre-invasive form of the disease and treat it before it became invasive. The test began to be widely used around the Western world and those who tested positive were surgically treated, usually by hysterectomy, or increasingly by the local removal of the lesion using cone biopsy under general anaesthesia. However, some gynaecologists were already beginning to wonder whether the treatment of this disease was too aggressive and radical. Given that there were many more diagnoses of this condition called carcinoma in situ (or CIS) using the Pap smear than there were cases of cervical cancer in the community they began to wonder what exactly they were treating. Some estimated that as few as five per cent of CIS cases would advance to cancer even if untreated.²

Dr Herb Green had joined the staff of National Women's Hospital in 1956, and from 1961 until he retired in 1982, he was Associate Professor at the hospital's Postgraduate School of Obstetrics and Gynaecology. Green was one of the gynaecologists who by the 1960s

¹ S. Coney and P. Bunkle, 'An Unfortunate Experiment at National Women's', *Metro*, June 1987, pp.47-65.

² L. Bryder, *A History of the 'Unfortunate Experiment' at National Women's Hospital*, Auckland, 2009, pp.8-12.

questioned an aggressive approach to CIS. Two of his colleagues, cytologist Bill McIndoe and pathologist Jock McLean, disagreed. These disagreements about case management continued for many years and in 1984, two years after Green had retired, McIndoe and McLean published a paper in an American journal, analysing the outcome for about 1,000 women who had presented with a positive cervical smear at the hospital over a twenty year period.³ As this article became the basis of the claim that there had been an ‘unfortunate experiment’ at National Women’s, I’ll focus on it briefly.

Coney and Bunkle explained in their *Metro* article that the 1984 paper showed how Green had divided the 948 women with CIS at National Women’s Hospital in the period 1955 to 1976 into two groups: those treated ‘conventionally’, and those treated ‘conservatively’ or ‘not at all’. ‘Conservative treatment’ was defined as treatment consisting of less than hysterectomy or cone biopsy; it involved the use of a less invasive procedure, called a ‘wedge’ or ‘punch’ biopsy.

Yet the authors of the 1984 paper had made it clear to Coney and Bunkle before the Inquiry that there had been no division of patients into ‘treated’ and ‘untreated’. As McIndoe told Coney, ‘the two groups we discuss result from a method we have applied to analyse data.’ It was the authors of the 1984 paper who divided the patient data into two groups retrospectively – those with a negative smear two years after initial diagnosis and those with a positive smear two years after diagnosis. Following the inquiry, one of the authors, statistician Peter Mullins, also stressed that the idea of two groups, with the latter untreated or

³ W.A. McIndoe, M.R. McLean, R.W. Jones, P.R. Mullins, ‘The Invasive Potential of Carcinoma in Situ of the Cervix’, *Obstetrics and Gynecology: Journal of the American College of Obstetrics and Gynecology*, 64, 1984, 451-8.

conservatively treated, was false. This is borne out by the treatment offered to the two so-called groups - in both about 70% had cone biopsies and 25% had hysterectomies.⁴

So there were no two groups. Why is this important? It effectively means there was no experiment with two groups being treated differently. Yet this was how it was portrayed in the Cartwright Report. Cartwright referred to Group 2 women as having limited or no treatment and Group 1 as fully treated. She believed that this description by Coney and Bunkle accurately reflected the McIndoe paper. Some of the attacks on me have been along the lines that 'of course Cartwright knew that there were no two groups'. This quotation from her report suggests otherwise:

Twelve of the total number of women died from invasive carcinoma. Four (0.5%) of the Group-one women, and eight (6%) of the Group-two women who had limited or no treatment. Thus the women in the limited treatment group were twelve times more likely to die as the fully treated group.'

I accept that [this] accurately reflects the findings of the 1984 McIndoe paper.⁵

Medical statistician Peter Mullins, who could have dispelled this myth, was not called to the Inquiry. As Mullins wrote to Kevin Ryan on 15 June 1990, 'The implication that the abnormalities were untreated is, on the information presented in our 1984 paper, quite false: the group was defined as "continuing to produce abnormal cytology", not as having been untreated.'⁶

In her Report Cartwright confused outcomes with treatments. Consider the illogicality in the following statement:

Those women whose lesion had not been eradicated, as evidenced by continuing positive cytology, had at least a 10 times higher chance of developing invasive cancer than those who had been treated by generally accepted standards.⁷

⁴ Bryder, 2009, pp.32-3.

⁵ *The Report of the Committee of Inquiry into Allegations Concerning the Treatment of Cervical Cancer at National Women's Hospital and into Other Related Matters*, Auckland, 1988, p.95

⁶ Copy of letter in author's possession.

⁷ *The Report of the Committee of Inquiry*, 1988, p.54.

‘had not been eradicated, as evidenced by continuing positive cytology’ is an outcome not a treatment. It does not mean they were *not* ‘treated by generally accepted standards’. It says nothing about *how* they were treated. The comparison is not valid. This confusion between outcomes and treatments has continued in the defences of the Cartwright Report and recent follow-up studies. Charlotte Paul, medical advisor to Cartwright, recently wrote of the McIndoe paper: ‘Bryder’s claim that there was no difference in treatment between Groups 1 and 2 also leaves her with no explanation for the fact that the rate at which cancer appeared in Group 2 was twenty-five times higher than in Group 1.’⁸ The reality is that women were placed in Group 2 by McIndoe retrospectively because they had persistent positive smears and so they would be more likely to develop cancer than those whose smears had returned to normal by whatever means. The groups were constructed according to outcomes. Treatment did not enter the study, as McIndoe had told Sandra Coney in 1985.

The confusion persists in recently published follow-up studies which were described in the *Otago Daily Times* as ‘settling the debate forever’, and confirming that Green conducted unethical research. Two Oxford epidemiologists, Sandercock and Burls, questioned the methodology of this research as outlined in a paper written in 2008; Sandercock and Burls wrote, ‘This paper states clearly that the authors divided their sample into adequately and inadequately treated groups. However, a major problem in their methods is that they use the outcome following treatment as part of the classification system’.⁹ Sandercock and Burls cite the 2008 paper which stated:

‘Any procedure followed by a positive smear in the following 6–24 months was classified as inadequate treatment (Figure 1). Four women who developed cancer within 2 years of CIN3

⁸ Charlotte Paul, ‘Medicine in context: A response to Linda Bryder’s arguments,’ In: J. Manning (ed.). *The Cartwright Papers: Essays on the Cervical Cancer Inquiry 1987-88*, Wellington, 2009, p130.

⁹ Josie Sandercock and Amanda Burls, ‘Congratulations to the NZMJ for the Unfortunate Experiment theme issue’, *NZMJ*, 13 August 2010, 123, 119-120.

diagnosis, but who had no follow-up cytology, were assumed to have had inadequate treatment.¹⁰

Sandercock and Burls rightly comment that,

‘Given that they ... base the classification of adequacy of treatment on outcomes, it is totally unsurprising and uninformative that the authors should find that women who are classified as “inadequately treated” have poorer outcomes. It is difficult to follow exactly what this paper was trying to prove, but as a means to demonstrate that conservative treatment led to worse outcomes, the methods are wholly inadequate.’

To return to the Cartwright Report; Cartwright argued that Green’s so-called unfortunate experiment began in 1966 when he took a proposal to the Hospital Medical Committee to treat women with positive smears by methods less than hysterectomy and cone biopsy. She argued that he was trying to ‘prove a personal belief’, as she put it, ‘ignoring virtually all the existing literature which assessed the likelihood of progression to invasion’.¹¹ She portrayed him as out-of-touch with international trends.

What exactly did the ‘existing literature’ state about the medical status of CIS and its management? One person who has become involved in recent debates about the Cartwright Inquiry is Sir Iain Chalmers, a co-founder of the Cochrane Collaboration based in Oxford,¹² who became interested following an approach by Charlotte Paul to elicit his views. Chalmers complained that when Cartwright attempted to review the existing international practice she relied totally on the viewpoints of four witnesses at the Inquiry and one interviewee. He declared, ‘there is no evidence in the report, or in any of its Appendices, that any attempt was

¹⁰ Margaret R. E. McCredie, Katrina J. Sharples, Charlotte Paul, Judith Branyai, Gabriele Medley, Ronald W. Jones and David C. G. Skegg, ‘Natural History of Cervical Neoplasia and Risk of Invasive Cancer in Women with Cervical Intraepithelial Neoplasia 3: A Retrospective Cohort Study’, *The Lancet Oncology*, vol. 9, 5, 2008, pp. 425–34.

¹¹ *The Report of the Committee of Inquiry*, 1988, p.33.

¹² The Cochrane aims ‘to help people make well informed decisions about health care by preparing, maintaining and ensuring the accessibility of systematic reviews of the effects of health care interventions’; see www.cochrane.org

made to conduct the international review of management that might have justified use of the term ‘conventional management’. He went on to point out that such reviews of international practices over time were readily available, referring in particular to a 1983 Scandinavian study of changing practices in different countries.¹³ This study included a bibliography of over 400 articles, and the author concluded that there had been dramatic differences of opinion about how the condition should be managed over time and in different countries. The publication also referred to the debates as to whether cone biopsy was overtreatment, which was the current view in Scandinavia in the 1980s. Cartwright had access to this book (I know that because I found it in the Cartwright archives), but she chose not to draw upon it.

As part of my research I conducted a review of the literature in specialist medical journals from the 1950s to the 1980s, which was what led me to question the conclusions of the Cartwright Report in the first place. I too found many articles and much debate, which I then placed into a broader historical context. Some of you may be aware of an intellectual movement in Britain in particular in the 1960s which began to question interventionist medicine, led by Thomas McKeown, Professor of Social Medicine at Birmingham University. McKeown became famous for showing the minimal impact of modern medicine on health status through his epidemiological studies spanning 200 years.¹⁴ He was also among those who questioned the value of an interventionist approach to CIS, as was Professor Archie Cochrane, doyen of evidence-based medicine, who claimed in 1972 in relation to CIS that, ‘Never has there been less appeal to evidence and more to opinion’. Sir Richard Doll, Professor of Medicine at Oxford and the internationally renowned epidemiologist who made the link between smoking and cancer, attended a Medical Research

¹³ Iain Chalmers, ‘Why won’t defenders of the Cartwright Inquiry provide evidence to justify their use of the term “conventional treatment” for carcinoma in situ?’ *NZMJ*, 30 July 2010, 123, 109-11; Iain Chalmers, ‘Defendants of the Cartwright Inquiry are unable to provide a description of “adequate care” for cervical carcinoma *in situ*’, *NZMJ*, 10 September 2010, 123, 85-87.

¹⁴ T.S. McKeown, *The Role of Medicine: Dream, Mirage, or Nemesis*, London, 1976

Council Symposium in Auckland in 1973. Summing up the conference, he made special mention of Green's questioning approach, stating that this was 'the most useful thing that a scientist [could] do', for, Doll explained, Green had 'succeeded in making us realise once again the disastrous power of words to constrain thought. We label a lesion carcinoma in situ and regard it automatically as cancer, whereas we ought to ask ourselves continually, what is the reality behind the words and what it is that has actually been observed.'¹⁵

Since the Inquiry there have been exaggerated claims about what exactly Green's views were. Charlotte Paul claimed recently that Green 'wrote repeatedly of his belief that CIS was a benign condition'.¹⁶ While she provides no reference for this statement, in the 2008 *Lancet Oncology* article, of which she was a co-author, the same claim was referenced to an article Green wrote in 1966. There he wrote, 'These then are still the two uncertain factors – the length of the pre-invasive phase and the proportion going on to invasion. Clinical evidence is tending to show, but cannot prove, that the latter is small – probably much less than 10 per cent'.¹⁷ In other articles he also addressed the current state of knowledge and the uncertainty as to whether 'the invasive potential in in-situ cancer is as high as has been claimed'.¹⁸ Nowhere did Green state that he believed CIS to be a benign condition; like many others he did believe, however, that it did not invariably lead to cancer.

A major influence on Green was an Australian gynaecologist called Malcolm Coppleson. Coppleson wrote an award winning book on conservative treatment of CIS in 1967. Ten years later he noted that since the 1960s there had been – in his words – 'a general disquiet

¹⁵ Bryder, 2009, 25-7

¹⁶ Paul, 'Medicine in context', p136.

¹⁷ G. H. Green, 'The Significance of Cervical Carcinoma In Situ', *Am JI of Obste & Gynecol*, 94, 7, 1966, 1009–22.

¹⁸ G.H. Green, 'Invasive potentiality of cervical carcinoma in situ', *Intern J of Obstet Gynaecol* 1969, 7, 4, 168-9; G.H. Green, J.W. Donovan, 'The natural history of cervical carcinoma in situ', *J of Obstet Gynaecol of the British Empire*, 1970, 77, 1, 1.

with the large number of *cone biopsies and hysterectomies* which [had] been the legacy of many years of loose interpretation' of abnormal Pap smears. He noted that management had tended to be radical and that many healthy asymptomatic women with a normal-looking cervix but with an abnormal smear were given a cone biopsy followed by hysterectomy, based on the smear. It was only now, he said, that 'many thinking gynaecologists' questioned this 'in the light of the increasing numbers of follow-up studies reporting excellent results from minimal interference in patients with abnormal smears and pathology'.¹⁹ Green, in other words, was far from alone in questioning a radical or aggressive approach to the condition.

In her report Cartwright wrote, 'One outstanding fact ought to have been clear to [Green] and to others – following (without treating) patients with positive smears, whether after cone biopsy, or after hysterectomy, was unsafe, as a proportion of those women would subsequently be shown to have invasive cancer.'²⁰ Here she is acknowledging that these women had already been treated with cone biopsy or hysterectomy. So what were the further treatment options? Once the patients had had hysterectomy, they would be looking at radiotherapy or additional surgical intervention. Dr Joe Jordan, a British medical witness at the Inquiry, reviewed Green's patients' case notes and sympathised with what he called Green's 'dilemmas' when these two options had known side-effects, and there was no clear evidence that the lesion would become malignant in any case.²¹ Modern studies have continued to question interventions relating to CIS. Dr Angela Raffle and Sir Muir Gray warned in a recent British book on screening that most positive smears were transient minor cell changes which would never advance to cancer.²² This reaffirmed the conclusions of a

¹⁹ Bryder, 2009, p.39.

²⁰ *The Report of the Committee of Inquiry*, 1988, p.55.

²¹ Bryder, 2009, p.40.

²² A. Raffle and M. Gray, *Screening: Evidence and Practice*, Oxford, 2007, pp. 20, 25

1988 *Lancet* article, published at the time of the Inquiry, which announced that new studies showing the links with human papilloma virus had estimated that even without treatment, only about 5% of CIS cases would advance to cancer. Oxford medical sociologist Tina Posner argued in 1991 that the medical dilemma for CIS was to know when to treat the abnormality and when to leave it alone because no harm would result from doing so, whereas intervention could lead to a variety of unintended negative consequences.²³

What were these ‘unintended negative consequences’? Hysterectomy was a major operation. Even cone biopsy was not risk-free. In his gynaecology textbook Norman Jeffcoate, a professor of obstetrics and gynaecology in Britain, advised that cone biopsy should only be resorted to if smears repeatedly contained cells with severe dysplasia, warning that this procedure was ‘not free from immediate and late hazards’ and that deaths were reported.²⁴ Some authors estimated severe side effects in 30 per cent of cases of cone biopsy. It could cause severe haemorrhage, leading to hysterectomy. Other complications of cone biopsy included uterine perforation, infection or damage to the cervix causing painful periods, dysmenorrhoea or amenorrhoea, infertility, future miscarriage or spontaneous abortion, premature labour, low-birth weight babies, and higher rates of caesarean sections – all of these were particularly important as many of these positive smears were being identified in young women.

There was also the question of whether radical treatment made a real difference to the outcome. A Norwegian specialist who came to the Inquiry as an expert witness, Professor Per Kolstad, followed up over 1,000 CIS cases, and concluded in 1976 that radical treatment had not made a difference to the outcome. The 1984 McIndoe paper itself also stated that

²³T. Posner, ‘What’s in a Smear? Cervical Screening, Medical Signs and Metaphors’, *Science as Culture*, 2, 2, 11, 1991, p.173

²⁴ Sir Norman Jeffcoate, *Principles of Gynaecology*, 4th edn, London and Boston, 1980, p.492

‘Whether or not the lesion is completely excised does not appear to influence the possibility of invasion occurring subsequently.’²⁵ The authors of this paper also questioned whether ‘adequately treated CIS is a totally curable lesion’. Despite these reservations, those who critique my book have continued to argue that CIS is totally curable.²⁶

Recognising the risks of intervention, the dilemma for doctors then was when to intervene, balancing risks and benefits. Careful monitoring of cases was important, and when Green took his proposal for conservative treatment to the Hospital Medical Committee in 1966 he advocated the additional safeguard of what was then a new diagnostic tool, the colposcope. As Kolstad wrote in 1972, when urging a conservative approach, ‘By the complementary use of cytology, colposcopy and histopathology, the risk of overlooking invasive carcinoma of the cervix is minimal.’²⁷

Such an approach required safeguards, as Green explained in 1970: ‘clearly patients treated in this manner must be assessed and followed carefully, and if clinical, cytological or colposcopic evidence requires it, be subjected to more radical diagnosis and treatment’.²⁸

This indicates that Green treated patients on a case by case basis and belies the notion that he followed patients without treatment in the interests of scientific research.

Again, disregarding international debates, Cartwright made some fairly damning statements about Green’s professional competence. She wrote, ‘An analysis of Dr Green’s papers points

²⁵ McIndoe et al., 1984, p.457.

²⁶ See for example Michelle Coffey who wrote: ‘CIS is and was a totally curable lesion’: Michelle Coffey, ‘The Unfortunate Experiment: Revisiting the Cartwright Report’, *New Zealand Skeptics*, 97, Spring 2010 (www.skeptics.org.nz).

²⁷ Per Kolstad, ‘Diagnosis and Management of Precancerous Lesions of the Cervix Uteri’, *International Journal of Gynecology and Obstetrics*, 1970, 559.

²⁸ G.H. Green and J.W. Donovan, ‘The Natural History of Cervical Carcinoma in Situ’, *The Journal of Obstetrics and Gynaecology of the British Empire*, 77, 1, 1970, p.2.

to misinterpretation or misunderstandings of some data on his part'. She wrote that the evidence of invasiveness 'appears to have been disregarded by [Green] or not fully understood.'²⁹

These assessments, however, reveal not so much misunderstanding on the part of Green and his colleagues, but rather medical disagreements and conflicts. Diagnosis of CIS was widely recognised as problematic throughout the period Green was working. In the 1960s one American gynaecologist described an experiment in which twenty slides of lesions were submitted to 25 eminent pathologists. They were asked to allot each of these to one of ten diagnoses ranging from normal to invasive cancer and they invariably came up with different diagnoses. As an example, he said, 'one slide was called negative by one observer, various intermediate diagnoses by twenty-three and invasive cancer by the remaining observer.' There were numerous other studies of slides in the following decades which showed similar divergences. American pathologist Ralph Richart, who was an expert witness to the Inquiry, admitted in 1981 that a woman's chance of having a hysterectomy depended upon which pathologist was on duty on the day the sample was submitted to the laboratory. In Australia Malcolm Coppleson noted around the same time that the opinion of the histopathologist had always been considered definitive in determining treatment, and that the decision to remove a woman's uterus had frequently been made on differences in the appearances of a few surface cells. He claimed that there was now sufficient evidence that faith of this sort was unwarranted, given 'the same slide proffered among cytopathologists of great expertise was rated from benign to invasive cancer'. Reflecting on this, Professor Jeffcoate wondered 'how

²⁹ *The Report of the Committee of Inquiry*, 1988, p.198.

many errors must be made taking the world as a whole'. He added, 'The toll in respect of unnecessary surgery and patient worry is inestimable.'³⁰

With experts of international standing in the United States, Britain and Australia disagreeing on the diagnosis – which ranged from mild dysplasia to invasive cancer - it is little wonder that disputes arose in New Zealand. However, they appeared to reach a pitch at National Women's which they did not elsewhere. Was this related to personality clashes? Some staff certainly thought so. McIndoe and McLean were fiercely antagonistic towards Green. Green himself was gruff and, according to some, arrogant, though as Cartwright also noted, a very caring practitioner. Green had a longstanding interest in pathology, and according to another pathologist at the hospital, James Gwynne, had developed an expertise equivalent to any pathologist. But, as Sir Graham Liggins commented, challenging a pathologist's diagnosis was like a red rag to a bull.³¹

Cartwright also appeared to be unaware of wider international debates relating to population-based cervical screening. She wrote that, despite a worldwide consensus on its effectiveness, its importance 'has been consistently undermined by the confused statements from Dr Green and some of his colleagues at National Women's Hospital... Even in 1987 [they] believed that they were right and the rest of the world was wrong in its assessment of the benefits of a mass screening programme.'³²

Again a review of the literature suggests otherwise. The opening sentence of a 1988 Oxford University study on the prevention of cervical cancer in Britain reads: 'The prevention of cervical cancer through the implementation of efficient screening programmes has been the

³⁰ Bryder, 2009, pp.74-6.

³¹ *ibid.*, p.78

³² *The Report of the Committee of Inquiry*, 1988, pp. 198-9.

subject of much debate and media coverage.’ Also in 1988 a history of the British Imperial Cancer Research Fund concluded that, ‘Despite its long history, the organization of a screening programme for cervical cancer remains controversial at the present time. Doubts are frequently expressed about the effectiveness of the existing screening programme in Britain, reinforced by the reported increases in mortality from the disease in younger women.’³³ Clearly there was plenty of debate on this issue at the time of the Inquiry. During the Inquiry National Women’s medical superintendent Gabrielle Collison sent Cartwright a letter which had recently been published in the *British Medical Journal* about cervical screening. In his letter Professor Alwyn Smith of the Department of Epidemiology and Social Oncology, University of Manchester, England, criticised what he called ‘the absurdity of conducting a screening programme in such a way that nearly 40 women are referred for an expensive and possibly hazardous procedure for every one who is at risk of developing serious disease’.³⁴ Despite receiving this letter, Cartwright asserted that in questioning the benefits of screening, Green and his colleagues were totally out on a limb.

In the final part of this talk I will turn to the most recent follow-up study of women who presented at National Women’s Hospital with a positive cervical smear from the 1950s to the 1970s. This was published last year in the *Australian and New Zealand Journal of Obstetrics and Gynaecology* by researchers Margaret McCredie and Charlotte Paul from Otago University, Ron Jones from National Women’s Hospital and others.

While the 2010 article claims to show that Green behaved unethically, there is no evidence in the article that the patient records reviewed were Green’s patients, relating as they did to a hospital where there were up to 20 consultants and many registrars. The authors no longer

³³ Bryder, 2009, p.90.

³⁴ *ibid.*, p.94

suggested there had been no treatment or limited treatment, which was the view expressed in the Cartwright Report. Instead, they wrote of an ‘excess of inappropriate follow-up interventions for women diagnosed in 1965-74’.³⁵ In fact these interventions continued up to 2000, and were provided by a wide range of doctors at the hospital, including Ron Jones (remember that Green himself retired at the beginning of 1982). Yet, to quote Jones’s own evidence to the Inquiry in 1988:

Mr R Jones said that since the mid 1970s the management of CIS and its results have “by and large followed internationally accepted principles”... Jones identified just three women of 400 files examined who had developed cancer, and reported that ‘he had no reason to believe that their management was anything other than correct’.³⁶

At that time he clearly did not consider the follow-up interventions ‘inappropriate’.

The authors of the 2010 article also declared, ‘Our findings show that inclusion in [Green’s] clinical study subjected women to many medical interventions designed to observe rather than treat their [CIS].’ Elsewhere in the article they wrote, ‘follow-up biopsies were often intended to exclude invasive cancer rather than to diagnose and treat’ CIS.³⁷ But where is their evidence of ‘intent’? A retrospective study of outcomes cannot be used in itself to prove unethical behaviour, which implies *intent* not to do best for one’s patients. Management choices in medicine might not always be the right ones in retrospect, and there are many examples of this in medical history, but it does not prove unethical behaviour. Moreover, attributing intent not to cure is contrary to Cartwright’s claim in her Report where she wrote, ‘I do not accept that Dr Green had no intention to cure his patients.’³⁸

³⁵ M.R.E. McCredie, C. Paul, K.J. Sharples, J. Baranyai, G. Medley, D.C.G. Skegg, R.W. Jones, ‘Consequences in women in participating in a study of the natural history of cervical intraepithelial neoplasia 3’, *Aust & NZ J Obstet & Gynaecol*, 2010.

³⁶ Submissions on behalf of the Auckland Hospital Board to the Committee of Inquiry into Allegations concerning the Treatment of Cervical Cancer at National Women’s Hospital and into other related matters’, BAGC A638/34a, Archives New Zealand, Auckland, pp. 8-9.

³⁷ McCredie et al, 2010.

³⁸ *The Report of the Committee of Inquiry*, 1988, p.96

To conclude then: The debates about what happened at National Women's Hospital raise wider issues about medical practice which are overlooked by those who try to present the past as one-dimensional, portraying Green as a 'villain', and McIndoe for example as a 'hero'. The wider issues, still relevant for medical practice today, are concerned with how medicine deals with uncertainties, how doctors are more likely to be criticised for under-treatment than over-treatment, even if the latter does more harm than good, and how medicine and society interact, particularly where there is any hint of the possibility of the dreaded disease, cancer.

As an exposition of past medical practices the Cartwright Report was seriously flawed. In saying this I realise I am ranged on the side of the baddies; in her speech on the occasion of the twentieth anniversary of the Report Cartwright declared, 'I realise now that this was a drama unfolding in the nation's living rooms, a drama in which there were goodies and baddies, and for all time, I was placed with the goodies, if not by the medical profession.'³⁹ This may of course simply reflect the power of the media, as buried in the archives I found a large number of letters written during the Inquiry by the general public including many women, to the judge, to the hospital, and to the Minister of Health, in support of the doctors of National Women's Hospital. But Cartwright's division of the world into 'goodies' and 'baddies' is not one that historians would subscribe to in any case; history is filled with complexities of which the historian seeks to make sense, as I did, through the use of multiple sources, published and unpublished.

Since my book appeared I have been subject to some fairly ferocious criticisms and attacks on my personal integrity and professionalism, and have been forced to defend my so-called position. None of those attacks has led me to cast doubt on my conclusions which I believe

³⁹ Dame Silvia Cartwright, 'Personal Reflections by the Commissioner on the Inquiry and Report', in *Twenty Years After The Cartwright Report: What Have We Learnt: A Conference hosted by the University of Auckland Faculty of Law*, Friday, 29 August 2008

are robust and sustainable. I reached those conclusions following several years of careful research and, while I am open to reviewing them in the light of new historical evidence or sound analytical studies, these *ad personam* attacks on me do nothing to move the understanding of historical events forward, and indeed leave me more convinced that there were, and continue to be, emotional and psychological drivers that are distorting the interpretation of events.