

PORNOGRAPHY, HARM AND WILLIAMS

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The possibility that pornography might be in some way socially harmful is not new. Long-standing legal prohibitions indicate that this has hitherto been a fairly general assumption. The belief has rested not only on the notion that pornographic materials have something about them likely to "deprave and corrupt" but also on the belief that people are capable of being depraved and corrupted. If, however, one adopts the assumption that people are not so susceptible, together with the belief that the themes of pornography actually represent rather healthy views of sexuality, it is easy to see the whole matter as a non-issue.

It was this set of assumptions which coloured the U.S. Report on Obscenity and Pornography of 1970, minimising public concern, emphasising other issues as more serious and concluding there was no evidence of harm from pornography.

Williams no less leads us to assume that terms such as 'deprave', 'indecent' and 'deprave and corrupt' have outlived their usefulness (p. 159) and concludes that the printed word shall not be prohibited since it is incapable of producing relevant harm (p. 160) I say 'leads us to assume' rather than 'proves' since I frankly doubt that the claim to "objective assessment of likely harm" (p. 160, #5) is justified. Williams rejection of such words appears to spring more from a belief that man has matured sufficiently to make such distinctions obsolete. I do not subscribe to that view, but those who do would be a great deal more honest to make their assumptions explicit and not hide behind phrases like "objective assessment" with their connotations of scientific proof.

It was when I was last in England, in 1977, that I became aware the Williams Committee was soliciting submissions for consideration in relation to the issues of obscenity and film censorship. As I had been involved in the research in this area since 1970, it seemed appropriate to make a submission. My work had been presented progressively at professional seminars, conferences and lectures. It had been published in journals and

books. It had also been subject to examination and cross-examination in a notable pornography trial at Snaresbrook in 1976. I took the view that the central issue in this whole matter is whether the availability of pornography and obscenity is in some way associated with harm - if so, at what level and of what kind. I now know that this conviction was also central for the Williams Committee.

This point of agreement may perhaps explain in part why so much of the Report is devoted to my work. It is however practically the only point of agreement since, to my surprise, the evidence I submitted is treated to phrases like "totally unsubstantiated surmise" (6.37) and the case cannot "even survive as a plausible hypothesis" (6.42). Since the Committee had access to a substantial amount of evidence of mine, and indeed devoted the majority of pages 69-86 to responding to it, one might suppose it to have been an important contribution. I wonder, therefore, why no-one on the Committee thought it worthwhile to contact me for clarification or discussion on any point? It has been my practice to communicate with those with whom I disagree prior to publication and that includes Dr. Kutchinsky of Copenhagen in spite of allegations by Williams that I have misrepresented him (6.56).

It is clearly impossible in the course of one lecture to do justice to the many issues I have raised in some 150 pages of my response to Williams. Some of the essential points are philosophical and methodological and hence quite unsuited to an occasion like this. I propose therefore to settle for some of the more specific issues on which I still claim to be correct. By way of preface, and to give structure to what follows let me quote from the end of my response.

"For all the negative evaluations and adjectives thrown at my submission, I have still not found one example of an error in my data which needs correction..... The dishonesty of the Report's recommendations lies in their claim to be supported by

evidence. My case is that the evidence has been reviewed selectively, evaluated with bias, ignored when convenient and critiqued tendentiously."

I will seek to illustrate each of these five points.

1. Not one example of error

In para. 6.58, Williams has drawn on a critical paper by Cochrane in which numerous errors in my work are alleged. Cochrane's allegations are taken seriously, even though the Committee received a copy of my reply to them. I can best illustrate the quality of the criticism of my work by referring to South Australia.

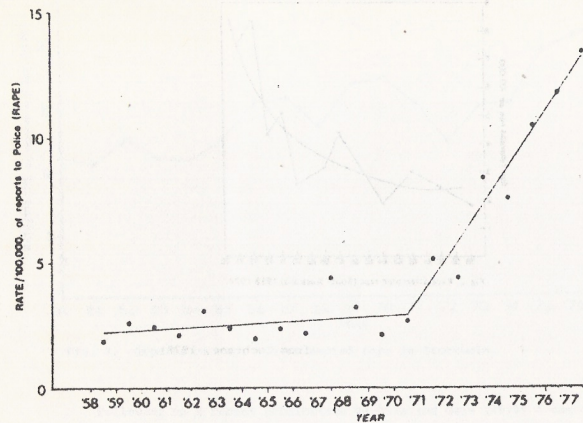


Fig. 1. Reports of rape and attempted rape coming to South Australian police with best fitting regression lines.

My case, illustrated in Figure 1, was that rape reports in S.A. remained at a stable level until 1970-71, and then departed significantly from linearity from 1971-72, and I related this to the marked change in policy relating to pornography late in 1970.

My critic claimed I was wrong, and asserted that the rise had been gradual, conforming to a quadratic function, and hence indicated that a rise was occurring before the time I claimed.

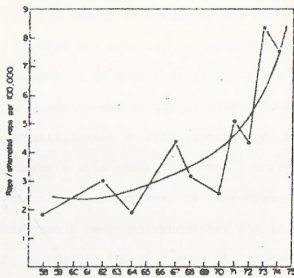


Fig. 1. Rape-attempted rape (South Australia) 1958-1974.

(from Cochrane, 1978)

His representation of the data looks convincing until one realises that, in order to achieve his solution, he carefully excluded seven of thirteen data points which proved inconvenient for his hypothesis. Had he applied adequate statistical analysis he would have found that my solution has a more satisfactory error mean square (.607) than his (1.151).

Admittedly, I now have the advantage of using more recent data than when I first contacted Williams, but this merely confirms more strongly what was already emerging then.

Williams actually prefers to highlight Cochrane's criticisms relating to Singapore in relation to London, and to Sweden. It was truly said that I had insufficient (but not inaccurate) evidence on Sweden. I had referred to that country since it, like Denmark, is often vaunted as the great success in social legislation and sexual liberation.

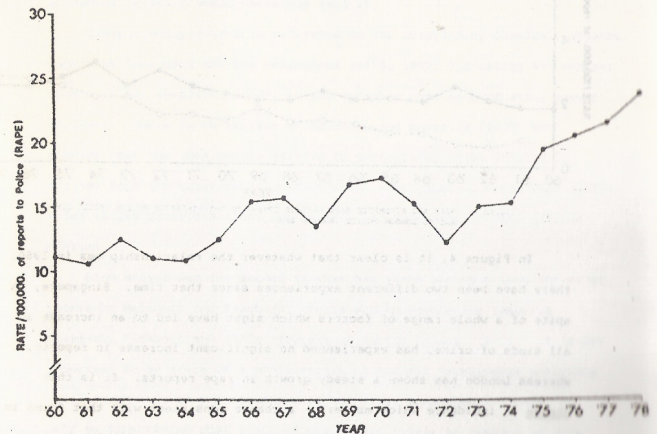


Fig. 3. Reports of rape and attempted rape in Stockholm.

Following on a recent publication by Geis and Geis (1979) I can now show (see Fig. 3) that my prediction of an upward trend in rape reports is fully confirmed. It was similarly suggested that I had not done justice to the relationship between rape reports in Singapore (where pornography is tightly controlled) and London (where pornography has been widely available for a decade and more). Cochrane criticised me on the grounds

that "rapes are ... actually more common in Singapore than London" (Williams 6.58). Indeed, reports to the police were more common but I can quite properly, as Williams admits, emphasise that base levels of reporting cannot be safely compared from one place to another in this way.

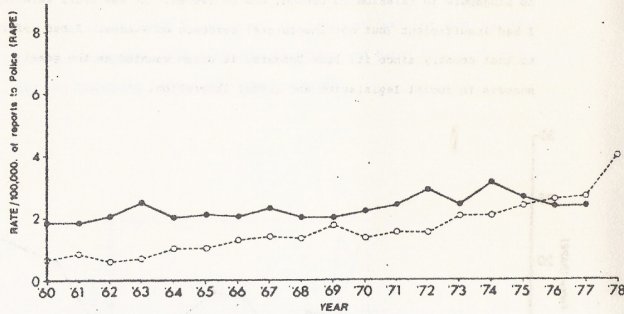


FIGURE 4. RAPE AND ATTEMPTED RAPE REPORTS COMING TO THE SINGAPORE POLICE (SOLID LINE) AND TO LONDON POLICE (BROKEN LINE).

In Figure 4, it is clear that whatever the relationship was in 1960, there have been two different experiences since that time. Singapore, in spite of a whole range of factors which might have led to an increase in all kinds of crime, has experienced no significant increase in reports, whereas London has shown a steady growth in rape reports. It is that change in incidence which matters - a change consistent with that found in numerous other places too, but which Williams chooses to dismiss by saying "we do not think it necessary to deal in detail with the situation in all of those countries" (6.57).

2. The evidence has been reviewed selectively

This is a major criticism I have, and so extensive that again I can only present examples. The Committee was to some extent at the mercy of its chosen reviewer, Maurice Yaffé, in assuming that he had provided an up-to-date account of the evidence relating to pornography and its effects.

Their choice of one who has over the years been associated with 'Mortimer's circus' in obscenity cases, could explain the selection of materials eventually summarised in Appendix 5. It really does not excuse the errors and omissions which abound in his review. He claims to review the relevant literature for the period 1972-77 though this does not deter him from including material from 1978 and 1979. As an academic who often reads undergraduate reviews of this kind, I suspect that the work was hastily finalised resulting in so many inaccuracies that if I were reading it as a student review I would certainly fail it.

Conspicuously absent is reference to the outstanding clinical research papers by Gene Abel and his colleagues (1975, 1977) indicating how rapists show a rather specific arousal to pornographic cues incorporating sexual violence. There is no mention of Schaefer and Colgan's (1977) work showing that the idea people satiate to pornography is an artefact derived from the experimental method used. The paper by Dienstbier (1977) which raises questions about the validity of Kutchinsky's work gets no mention.

Also missed was the second in what has since become a long series of papers by Malamuth and Feshbach. There was no excuse for ignoring Malamuth, Feshbach and Jaffe (Journal of Social Issues, 1977). It is now important to be aware of a whole series of careful experiments analysing those components of pornography which generate potential for rape, as well as identifying what kinds of people are likely to respond in this aberrant way. This work, appearing in such journals as Journal of Research in Personality (Malamuth, Haber and Feshbach, 1980) Journal of Sex Research (Malamuth and Spinner, in press) and Archives of Sexual Behavior (Malamuth, in press) is of a kind to make William's conclusions obsolete.

One would have to conclude that whatever else it is, Yaffe's review has not been 'brought up to date for our benefit' as Williams claimed (1.9). Hence his conclusion that "there does not appear to be any

strong evidence that exposure to sexually explicit material triggers off anti-social behaviour" must be limited by the fact that significant research remains unconsidered.

3. Biased evaluation of evidence

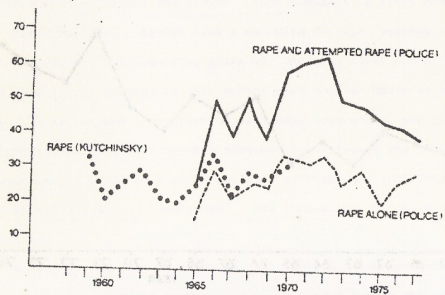
In a controversial area such as this there are bound to be disagreements about what constitutes relevant evidence, and which is to be preferred when conflicting findings are reported. That does not, however, justify selectivity such that unwelcome findings are disparaged and dismissed, while those consistent with one's prior expectations are espoused without criticism.

My reading of William's treatment of the situation in Denmark is that Kutchinsky's work is accepted as "more impressive, it is comprehensive, detailed and scrupulously careful" (6.56) while my own is rejected as lacking "a sound scientific basis" (6.59).

One gains little indication that Kutchinsky's work has even been questioned. His case is very fully accepted even though Williams acknowledged that much of it is yet to appear in published form (though it was foreshadowed as 'in press' as long ago as 1973). A number of serious authors (e.g., Bachy, 1976; Cline, 1974; Dienstbier, 1977) have raised objections to his work. It is also notable that Cochrane (1977) while taking every opportunity to attack my paper on the Danish situation (Court, 1977) raised no quibble about my critique of Kutchinsky's work - which had indeed already been sent to Kutchinsky for prior comment so that I should not inadvertently misrepresent him.

This issue must be pursued for the evidence continues to be hotly disputed. Williams presents evidence from Kutchinsky that rape reports are declining.

FIGURE 5: RAPE AND ATTEMPTED RAPE REPORTED TO THE POLICE IN COPENHAGEN



(from Williams, 1979)

In spite of the title given to Figure 5 by Williams, it should be added that in the text there appears the explanation that "Dr. Kutchinsky has a statistical record of sexual offences which differs from that issued by the police but which he regards as more accurate and reliable." (emphasis mine) (6.50)

I wonder how my own evidence would have fared had I chosen to re-arrange official figures so that instead of rising they went down? I prefer not to do that, but instead offer Figure 6 as evidence expressed as rate/100,000 population obtained from the Copenhagen police.

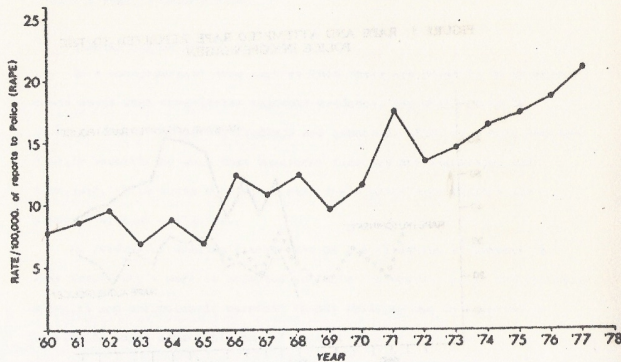


FIGURE 6. Reports of rape and attempted rape in Copenhagen (based on police figures, 1979).

This trend has the virtue of being subject to comparison with other police data, and shows a consistency with trends in comparable places (e.g., Sweden). In considering the importance of this trend, I want to stress that this growth in serious sex offences has exceeded the figures for sex offences generally, such that, while they represented 8.7% of the Danish total in 1970 they were 20% of the 1978 total. This distinction between major and minor offences is something I have developed at length in my response since it is a point where Williams' critique of my work is most misguided.

4. Evidence is ignored when convenient

I am sure that my experience of having my evidence dismissed is one shared by many others who made submissions. Williams in paras. 6.60-80 refers to the lines of argument advanced by a quite diverse assortment of witnesses and groups. Each argument is first presented and then demolished. Rather like a man with a rifle, sniping at oncoming troops, the Committee picks off one case after another. What is missed in the process is that behind each soldier there is a whole army. In other words, by treating each case separately, and seeking some objection to it, it becomes possible to disregard the overall convergence of evidence - psychological, psychiatric, sociological, criminological, moral, ideological - all indicating that harm is occurring.

Within my own area of evidence, this approach was most obvious in relation to anecdotal evidence of cases coming before the courts where pornography had been implicated.

First, the criterion of cause-effect relationships required as the standard of proof "beyond reasonable doubt" showed an abysmal lack of understanding of the complexity of human behavioural responses. Apparently the offender was required to be a 'tabula rasa', exposed to one pornographic experience, only to rush off and commit an offence. Such naivete totally disregards social learning theory and reinforcement principles, as well as predisposition and opportunity.

As Goldstein (1977) puts it,

"We can see that most of the reported imitation occurred some time after the exposure to erotica, suggesting that a type of latent learning took place which was not expressed until a later time when an appropriate partner was available" (p. 11).

In para. 6.6 Williams explains that anecdotal evidence was rejected

because the links between pornography and an offence were claimed in many cases by defence counsel, and "there were fewer cases in which the trial judge offered his own comment on the influence of pornography". It is not at all clear that because defence counsel introduces a link with pornography in order to mitigate the seriousness of the offence, the link must therefore be rejected as false. One may well recognise their reasons for introducing such evidence, but to reject it as false is a most serious suggestion.

Two particular cases (the Moors Murderers and the Cambridge Rapist) are dismissed by Williams (6.7 and 6.8) with the note that in neither case did the suggested link with pornography come from the trial judge (6.6). So what happened to those cases where the judge did comment? They are all conveniently ignored. I have therefore devoted twelve typed pages of my responses to documenting cases in which a link with pornography has been remarked upon, including a number where the trial judge saw fit to comment.

For example, in an Australian case in 1974 it was the Crown prosecutor not the defence who said

"Despite what psychiatrists and other people say, the danger of pornographic material becomes more apparent to those of us dealing with these crimes."

The judge agreed and added that the change which occurred in the accused from "a lonely hitchhiker to a rapist was probably triggered by sexual excitement aroused by this literature."

In January 1980, in South Australia a case of rape came before Mr. Justice Jacobs who, in his summing-up, said

"Despite public utterances to the contrary by some persons who claim to be well informed, cases from time to time do arise in this court, and this is such a case, which strongly suggests that

there is a connection between pornography and criminal conduct by persons who have failed, for one reason or another, to establish a satisfactory and stable pattern of sexual behaviour."

In Tasmania, in February 1980, a case came before Mr. Justice Cosgrove of a youth who raped a young mother after watching pornographic films. The judge said

"he hoped the case would stand as an example to those people who claimed there was no connection between pornography and rape."

These and many other such examples lead me to believe that police, solicitors and judges, as well as rapists and their victims are more firmly in touch with the real world than are the members of the Williams Committee.

5. The evidence critiqued tendentiously

A distance between my own work and that of Dr. Kutchinsky has been falsely created at many points by Williams. Para. 6.45 indicates the area of dispute while at the same time purporting to show that I support Kutchinsky's findings.

"We think it right to make the point that even Dr. Court, who has sought to detract from Dr. Kutchinsky's work, made it clear to us that he regards those reports as careful, detailed and appropriately cautious about the conclusions which might be drawn."

That is a true statement based on my submission but without citing the rest of what I said creates a false impression. While truth, it is not the whole truth. My statement to Williams continued -

"While attention has been drawn to what I believe to be errors, these are only detectable because of the thoroughness and detail which characterized the presentations. It is in the secondary reporting of his work, and in later papers of a conference type

that the cautious conclusions have been transmuted into the myths of our time."

In seeking to polarise our conclusions, Williams has incorrectly inferred that Kutchinsky and I were both emphasizing the same information. This is quite untrue, and insofar as comparisons are drawn between quite different kinds of information, they are invalid. Whereas Kutchinsky's published work has been concerned with sex offence data in Denmark prior to 1970 (and a limited extension beyond that), my own work has referred to rape data in many countries before 1970 but also most significantly after 1970.

A great deal of Williams critique is directed to sex offence reports and reports of other crimes (violence, robbery) seeking to show that sexual offences have not escalated. That proposition is not only tangential to my case, but also positively misleading. For example, I do not dispute that comparing all violent offences and all sexual offences for England and Wales, the trend from 1970-78 is up by 112% and down by 7% respectively. That makes sexual offences look relatively unimportant.

However, a better evaluation of my thesis would take serious violent offences and compare them with serious sexual offences (rape and attempted rape). That way, the figures for violence are up 30%, but rape is up 41%. The clear implication of this very different relationship is that Williams hides the rise of serious sexual offences by masking them with heterogeneous data greatly affected by changing social attitudes to sexual behaviour.

I can most readily illustrate my point with data from South Australia. (See Court, 1980(a))

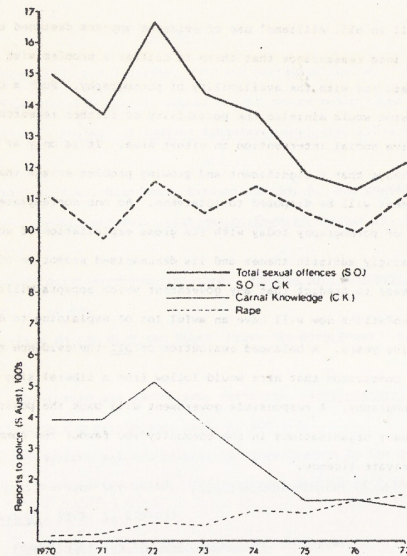


Figure 7. Sex offences separated into major and minor to illustrate opposite trends.

If we look simply at total sex offence reports, Figure 7 suggests a decline over the period 1970-77. However, the major component contributing to this is reports of carnal knowledge. When these are separated out there is no decline among the remainder, while the sub-category of rape reports has risen so far as to exceed carnal knowledge reports. Few would doubt that this reduction arises because carnal knowledge is now much less likely to be reported than it was in 1970.

All in all, Williams' use of evidence appears designed to lull people into reassurance that there is neither a problem with sexual offences, nor with the availability of pornography. Such a dangerous conclusion would minimise the possibility of further research or effective social intervention in either area. It is only as we acknowledge that a significant and growing problem exists that the Parliament will be disposed to intervene. As one contemplates the nature of pornography today with its gross exploitation of women, its increasingly sadistic themes and its dehumanised promotion of perversion, it is easy to predict that any government which accepts Williams recommendations now will have an awful lot of explaining to do over the next five years. A balanced evaluation of all the evidence must lead to the conclusion that harm would follow from a liberalising approach to pornography. A responsible government will back the police and those many organisations in the community who favour the common good over private licence.

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