ANZJS Vol. 18 No. 3
November 1982

MEASURING FERTILITY: PURPOSES AND PROBLEMS— A REPLY TO CARMICHAEL

What are our purposes in constructing measures and undertaking social research? This, to me, seems to be the central question that Carmichael's paper raises.

In my previous note I wished to demonstrate the overall distribution of ex-nuptial and 'bridal' fertility by age of mother, the tendency for rates to decline during the seventies, and the relatively lower rates that teenage girls experience when compared to older women. I felt that this exercise might provide a useful base for those of us who are interested in trying to construct theories about why birth rates alter. I concentrated on ex-nuptial and 'bridal' births because many of these will be unplanned, unwanted, and a focus of individual and social stress, with particular implications for the life chances of the women who experience them. I was also concerned about the fairly widespread misapprehension of the nature of the problem.

For example, the sense of alarm about a demographic event that has not happened (an 'increase in the ex-nuptial birth rate' and especially

an 'increase in the teenage ex-nuptial birth rate') appears to be a product of misleading measures. That is, it is a common practice of the media to express ex-nuptial births as a percentage of all births and to call this an 'ex-nuptial birth rate', and though ex-nuptial fertility has been declining, nuptial fertility has been declining more rapidly. This means that an 'ex-nuptial birth rate' so expressed will show an increase. Thus *The Age* (11 July 1981) reports:

> The latest Australian Bureau of Statistics figures show that ex-nuptial birth rates have crept steadily upwards in the past decade from 9.3% in 1971 to 11.7% in 1979 . . . In the under 20 age group, the proportion of ex-nuptial confinements rose from 33% in 1971 to 48% in 1978 . . .

It is understandable that journalists should adopt such a measure. It is the easiest one to calculate when all one has is raw data on births. But the practice is completely misleading and, worse than this, creates the impression that attempts to provide young people with sex education and information about birth control have been useless, or even counterproductive. It may even lead to increased feelings of anger and impatience directed towards those unmarried women who do become pregnant.

Yes, it is indeed demographic common sense to express events like ex-nuptial births in terms of the population 'at risk' or experiencing them, but misunderstandings of this order make it imperative that demographic common sense be restated, and that the reasoning behind it be presented in a form which interested people, lay and professional, can understand. It would be absurd to claim originality for a measure expressing ex-nuptial births as a rate per 1000 unmarried women and I do not do so. To express 'bridal' births in the same straightforward way, as a rate per 1000 unmarried women, may be unusual but it enables us to compare and combine the two measures. This is helpful, if only to further demonstrate that those efforts that have been made to de-privatise sexuality, and to increase the availability of birth control, do seem to be associated with a decline in 'bridal' as well as ex-nuptial fertility, especially among the younger age groups. It is therefore a useful measure and I am not convinced that it is invalid merely because Carmichael asserts that it is 'simply not acceptable to purist demographers'.

I was, then, trying to describe a way of presenting data on ex-nuptially conceived births that allows us to make some general sense of the trends. I wished to do this because I felt that the data might be useful for those with an interest in sociological, and social problems. For, while the methods employed and the trends they reveal may be commonplace to the initiate, they are clearly not a part of everyone's taken for granted stock of knowledge.

Carmichael's interests on the other hand seem to lie rather with the technical niceties of measurement than with making data accessible and useful. He has two central concerns. Firstly he is concerned with the way in which the measures I use are flawed, and secondly he is concerned with the possibility of constructing original and superior measures of his own.

Problems with an ex-nuptial birth rate

Carmichael dismisses the validity of a 'bridal' birth rate in a summary fashion and concentrates his attention on the problems, too long unacknowledged and too little discussed, that afflict an ex-nuptial birth rate.

He has two major quarrels with a simple age specific ex-nuptial birth rate per 1000 unmarried women (where 'unmarried' is defined as never married, widowed or divorced). Firstly such a measure assumes that all ex-nuptial children are born to unmarried women and this is an unjustifiable assumption; married women may well bear children whose biological father is not their legal husband. And secondly, the measure assumes a common definition of marriage in the numerator (derived from vital registration data) and in the denominator (derived from census data), but in fact the definitions are different. Both these problems will, he claims, introduce bias into the measure.

The first difficulty contains not one but two problems. Assuming that a married woman registers her extra-maritally conceived child as an 'ex-nuptial birth', the ex-nuptial birth rate, as a rate per 1000 unmarried women, is biased upward because the denominator of women 'at risk' is too small. That is, some married women, those who are 'at risk' of ex-nuptial births, should be included in the denominator. But if this is so we could well argue that some, or many, unmarried women should be excluded from the denominator. For, as has already been acknoweldged, in assuming that all unmarried women are 'at risk' the measure assumes too much. So, if the denominator should have an 'adultery co-efficient' to control for extra-marital sexual activity among the married, it should also contain a 'chastity co-efficient' to control for the sexual inactivity among the unmarried. But, in the face of the demanding prospect of constructing such measures, it is as well to remember that our central concern lies with measuring trends in births not with the variations in sexual behaviour that may or may not explain them.

The other problem concerns those cases where a married woman bears an ex-nuptial child but may wish to misrepresent its natural parentage, at least to the Government Statist, if not to those more

intimately concerned. Carmichael acknowledges that this may happen, but bases his argument on the assumption that it will not. Can we make this assumption so easily? While much may depend on what the people involved wish to conceal from each other, we should also take into account the manner in which the data are collected and the degree to which this may influence people to give face saving answers.

In 1968, at the Royal Women's Hospital in Melbourne, I observed a hospital official in a crowded public ward helping newly delivered women fill in the birth registration forms for their babies. 'Are you married?' she would ask. This question itself clearly elicited embarrassment and annoyance. The reaction to her second question, 'Is your husband the father of this child?' was even stronger. In those circumstances it would have been a brave and committed truth teller who answered 'no'. Greater privacy might possibly facilitate the collection of the sort of data Carmichael presupposes, but, given that the signature of a third party is required on the birth registration form, complete privacy cannot be secured.

The first major difficulty Carmichael raises amounts, then, to this. If married women do register children conceived in extra-marital relationships as 'ex-nuptial', the ex-nuptial birth rate is biased upwards, because the denominator is too small, while if they do not, the rate, as a measure of 'real' ex-nuptial fertility, is biased downwards, because the numerator is too small. Short, however, of more inquisitorial practices on the part of the Government Statist, there seems to be little that we can do about either problem.

Carmichael's second major objection to an ex-nuptial birth rate is that the definition of marriage used in the numerator is a strictly legal one, while the definiton of marriage in the denominator permits a de facto couple to declare themselves as 'married'. This, too, is a nice point. The form from which vital registration data are derived is certainly more specific and searching in its questions about marriage. The mother's maiden name, the date and the place of marriage, must all be provided. It may be more difficult in these circumstances to make up a convincing tale, especially as the information must be witnessed by a third party. The census question 'what is each person's present marital status?' (with the alternatives: never married, now married, separated but not divorced, divorced, widowed) does of course 'allow' those in a de facto relationship to report themselves as 'married'.

But, given Carmichael's assumption that the legal status of the child will be correctly recorded, it is hard to see why he is so concerned about this presumed difference in the definition of marriage. For if it is the case that the marital status of the mother does not affect the way in

which the legal status of the child is recorded, the way in which marriage is defined for purposes of vital registration is immaterial. This being so, we would be left with one definition of marriage only, the one employed by the census, and the problem of the rate being contaminated by two different definitions of marriage evaporates.

Nevertheless, even though he begins by indicating that the possibility of conflicting definitions of marriage is the cause of his disquiet, it seems that this is not entirely the case. It appears rather that the focus of Carmichael's anxiety is that many women in the denominator will be incorrectly labelled. Some, or many, will be in de facto relationships and some, or many, of these may misrepresent themselves as married. Systematic bias in an ex-nuptial birth rate could arise if large numbers of these women regularly reported themselves as 'married', while all married women reported their status correctly (restraining such impulses as they may have to realise their fantasies of celibacy on the paper before them). The problem now becomes rather intriguing.

Some women who are in fact unmarried may say they are married, and some who are in fact married (and reporting themselves as married) may be living with someone other than their legal spouse. For let us not assume that all de factos are unmarried. While we have already considered appropriate corrective measures (of a statistical kind) for this latter group, and have rejected them as impractical in a world that does not as yet revolve around demographic purity, what of the former?

Yes, they may introduce some bias, and it may be more pronouced among older women. The question of how much bias is, however, an empirical one. But the possibility of this raises two questions. Would such a phenomenon be likely to introduce substantial bias into an age specific measure? Could it for example explain the apparent fact that unmarried women aged 25-29 have experienced ex-nuptial birth rates some three times greater than girls aged 15-19 in 1961, 1966, 1971 and 1976? And, is it practical, or even feasible, for us to attempt to gather the kind of data that would allow us to control for this effect?

Carmichael suggests that women who are reported by the census to be 'separated but not divorced' be included in the denominator. While this could in no way control for any systematic tendency for the unmarried to misrepresent themselves as married, he feels that it would make the denominator more nearly inclusive of those who are really 'at risk' of ex-nuptial births. He is happy to so include them because 'presumably such women generally are not at risk of becoming pregnant by their legal husbands'. The validity of this assumption is, however, doubtful and reliable annual estimates of the numbers of women, by age, who are married but permanently separated are not available.

Ex-nuptial 'conception' rates?

Although he has drawn our attention to all these difficulties, the measures Carmichael proposes do no more to contain them than does a simple ex-nuptial birth rate.

His proposed 'ex-nuptial conception resulting in ex-nuptial confinement rate' and 'ex-nuptial conception resulting in nuptial confinement rate' appear to address problems that are rather different from those that concerned him in his discussion of ex-nuptial birth rates. The central one, and the one that we may wish to take seriously is this. If we express ex-nuptial births as a rate per 1000 unmarried women in the year in which the births occurred, we have not taken into account the fact that some of the children will have been conceived in the previous year, when the number of women 'at risk' may very well have been different.

Given Australia's relatively rapid and continuing population growth we may feel that we should take this into account. Measures that express births as a rate per 1000 women (of a certain age and marital status) in the year in which the children were born, will be biased downward because the population assumed to be 'at risk' in that year will be somewhat larger than the population 'at risk' during the time span in which the children were actually conceived. (This, of course, is true for all measures, nuptial and ex-nuptial, that express births as a rate per 1000.) Carmichael's formula, based on another assumption, that of a constant gestation period of 266 days, allows us to make some attempt to control for this difficulty.

Again, however, we should ask: Are the population changes during this time span great enough to seriously affect our measures? And does the quality of the base data describing the population by age, sex and marital status justify the attempt to control for the bias that such changes may produce?

Problems with the denominator, the base data, are far more extensive than the varying probability of extra- or pre- marital sexual behaviour. The denominator is derived from census data, and this itself is subject to undercounts and errors in reporting. Some of these have already engaged our attention. More than this, however, if Carmichael is proposing to compute annual rates he will have to rely on intercensal estimates which, in my experience, are not generally available broken down for age, sex, and marital status, and, if they are, are inevitably subject to greater error than the census itself. In effect this means that he will be building a sophisticated statistical superstructure (the numerator) on a less than wholly reliable base (the denominator). I doubt that this will produce measures of any greater accuracy.

Nonetheless, the potential to control for the effect of changing population size between conception and birth is the main advantage that Carmichael's measures offer and it is therefore a pity that he does not draw our attention to it. Rather, we are left with the impression that these statistical procedures must be endured simply because it is illegitimate to compare 'bridal' pregnancies with ex-nuptial ones at the point of birth. To maintain demographic integrity we must journey back to the time of conception.

Conclusion

It is not, then, immediately clear just what advantages these measures have over ex-nuptial and 'bridal' birth rates. As we know, a large proportion of zygotes fail to implant, miscarry, or are still born. For some purposes, could it be calculated, it might be useful to have a 'conception' rate so that we could compare it with a birth rate. But Carmichael does not claim that his formula produces a 'conception rate'.

True, his measures remind us that women are invariably younger when they conceive than when they give birth, and that they may have conceived in the previous calendar year, but apart from this they are in no way different from simple confinement rates. (A confinement rate is the same as a birth rate, except that it does not include the extra children resulting from multiple births. These, however, form but a small proportion of the total and for stylistic reasons I have tended to treat the two rates as one and the same.) But though Carmichael's measures have no essential features distinguishing them from 'bridal' and ex-nuptial birth rates, they are considerably harder to understand and to compute.

This is not to say that they cannot be computed. Clearly they can. The real question revolves around whether they should be. What use would such measures be? This question reconfronts us with the continuing dilemma of the social sciences. What is the purpose of pursuing social research? To this I have been able to find no more satisfactory answer than that proposed by C. Wright Mills in 1959: that the purpose of social science is to understand personal troubles and public issues and to look for the relationships between the two. If this is what we are trying to do we will rest content with measures that have some imperfections, but that are nonetheless workable tools, and we will avoid those measures that are time consuming, arcane, and no less imperfect, for they can only divert us from our central task.

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