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TIME-PERIOD vs GENERATION: HOW SHOULD TRENDS IN FERTILITY BE MEASURED?*

MÁIRE NÍ BHROLCHÁIN
University of Southampton, United Kingdom

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1. INTRODUCTION

This paper starts by presenting the contrast between two approaches to depicting time-trends in fertility - the period and cohort perspectives - and discusses why it is that the existence of two approaches presents a problem for fertility analysis. It then considers the arguments for and against the period and cohort approaches to representing fertility trends. The conclusion reached is that there are clear grounds for preferring the period approach to the cohort perspective, but that the period measures currently used to depict fertility trends are defective and need to be replaced. I go on to examine what the criteria are for a sensible system of measurement for period fertility and present a (partly) new approach to constructing fertility indicators that meets these requirements. Recent trends in fertility in Ireland are considered briefly in the context of the potential uses of data collected by the current Irish birth registration system. Finally, some objections to the view advocated in the paper are discussed. The paper's aims are, thus, both destructive and constructive, arguing against the currently conventional approach and suggesting an alternative system of measurement.

Period vs cohort seems perhaps, at first encounter, an arcane subject, the stuff of truly academic argumentation. The issue has a bearing, however, on some decidedly concrete matters. In the context of mortality, for example, the question arises as to how far it is the conditions of the moment that determine the morbidity and mortality of individuals and how far their mortality is the result either of particular conditions in the past or of the accumulated set of exposures to which they have been subject through time. The question at stake in a fertility context is the extent to which childbearing is governed by the here and now and how far by past experience that gives individuals, and perhaps cohorts, their distinctive history. One could see the matter in everyday terms, perhaps only a little fancifully,

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as a question about whether time-trends in childbearing are driven by "expedience" or by "authenticity"?

2. THE PROBLEM

There are two related aspects to the period vs cohort issue: a measurement problem and a substantive one. The question of substance is whether social, cultural and economic factors influence fertility in period or in cohort fashion, or, indeed, in both modes. The measurement issue is about which approach gives the best representation of time-trends. It is the measurement problem that is of primary concern in the present paper, though these two facets are, of course, not independent of each other.

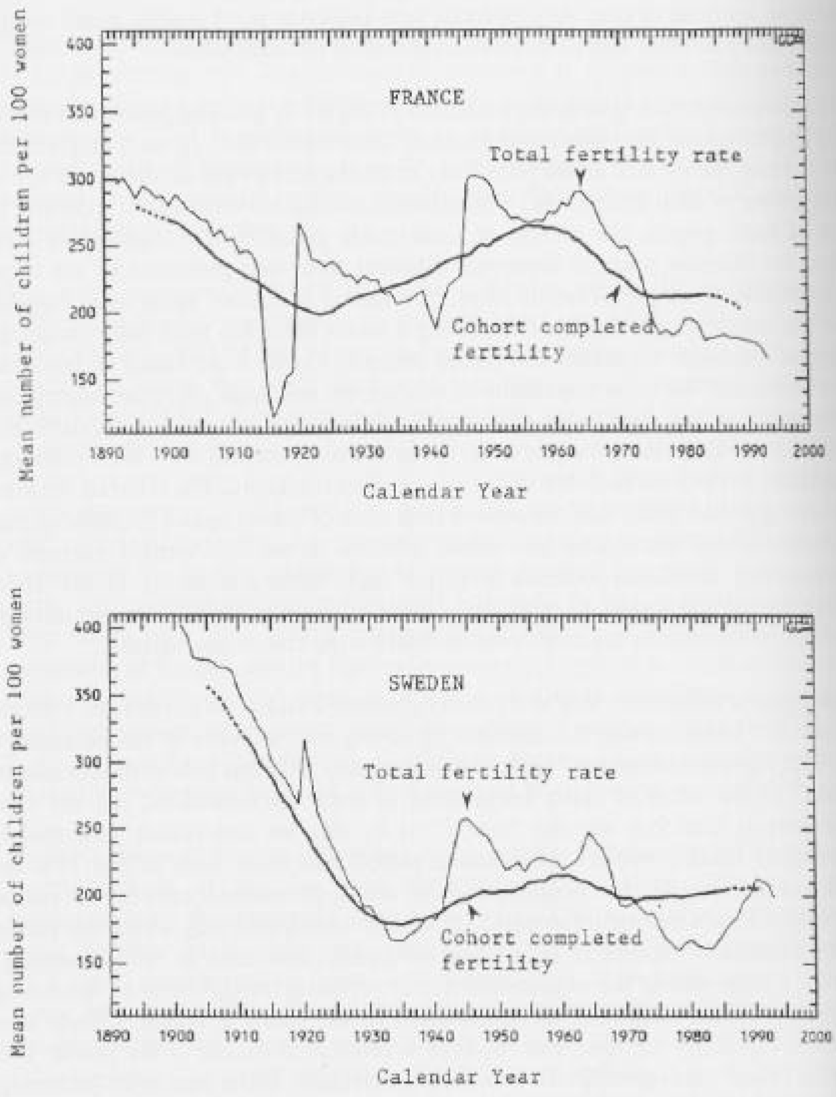
The most common approach to tracking fertility trends is to calculate some indicator of fertility performance for calendar years (e.g. 1993, 1994 etc.) or periods (e.g. 1985-89, 1990-94 and so on). The indicators chosen could be e.g. the absolute number of births, the crude birth rate, age-specific fertility rates[1], or the total fertility rate (TFR)[2]. A succession of such indices, assembled in time-series, depicts changes from year to year, or period to period, in fertility. This is what is known as the period approach. That it is by far the most widely used method of depicting trends may stem from the ease with which data can be compiled in this form from vital registration systems. An alternative way of looking at change through time is to examine the fertility experience of women grouped according to their year of birth, the so called generation or cohort approach. In this, a fertility indicator of some kind - most often, cohort completed family size[3] - is calculated for each birth generation of women, and the figures for successive cohorts are assembled as a time series. In calculating the mean family size of a birth generation of women, we obtain an average for an actual, identifiable group of women, unlike the TFR which is an average for a hypothetical or synthetic group - i.e. the imaginary group of women for whom fertility rates remain constant at a particular year's level[4].

Probably the most striking expression of the contrast between the period and cohort perspectives is in the fact that these two modes allow different ways of operationalising the concept of mean family size - TFR vs cohort completed fertility - and that time-trends in the two variants of this concept do not agree. In drawing up a visual comparison of period and cohort series, it is customary to locate the figure for cohorts in the calendar year when the cohort members were at their mean age of childbearing. The displacement may be either by a constant interval for all cohorts (more convenient for plotting) or it may be specific to each cohort, that is, each cohort figure can be displaced by its exact mean age of childbearing. Note that this is merely a diagrammatic device to allow visual comparison between the two series and produces a rough comparability in the time to which the period and cohort series relate. However, the correspondence is, by the very nature of the two approaches, only approximate. Any one cohort figure is derived from summing selected fertility rates (those relating to the cohort in question) across 35 calendar years (the years during which the cohort members aged, progressively, from 15 to 49). Hence, locating a cohort completed family size figure against a single calendar year is an artificial device. Any calendar year indicator is, of course, based on the rates of that year only, and so its location in time is not ambiguous.

Two examples of such graphs are presented in Figure 1, plotting period and cohort total fertility series[5] for France and Sweden, 1892/1901-1993[6]. In each diagram the cohort mean family size series (thin line) is displaced forward by the mean age at childbearing of each generation[7]. The problem arising is immediately apparent. In each of these graphs, the picture of time-trends given by the sequence of TFR values for calendar years is somewhat different from that presented by the time-series of cohort completed family sizes. The period series are much more variable than the cohort series, the latter following a much smoother path, with no abrupt changes. Although the period and cohort series in Figure 1 are based on the same birth events and the same population of women, by and large[8], they are aggregated differently and their time paths do not agree. In a rough and ready way, there is a similarity between the two types of series in the diagrams shown - each rises and then falls - but beyond that, the lines diverge quite markedly. The contrast between fluctuating period series and the smooth time-path of cohort values is characteristic of fertility trends throughout the world, whether in the low fertility regimes of contemporary developed societies or in the high fertility countries of the Third World. Period and cohort do not tell the same story about fertility change through time and period indices are more volatile than are the cohort equivalents.

Which then is the correct way to represent the time trends - in a cohort or a period framework? Some answer this question by saying that a choice is not necessary - that both representations are “useful”. It is certainly true that cohort figures can be “useful” in the sense of being informative in some circumstances, but the view taken here is that they are not “useful”, if by this we understand “essential to interpreting fertility trends”. If synthetic period indicators such as the TFR are used, and that data are not available in more detail, the contrast between the period and cohort trends can be informative. It is well established that where the period TFR substantially exceeds or falls below the family sizes seen in “corresponding” cohorts, a large part of the explanation lies in variation through time in the timing of fertility. In the first case, an acceleration in the speed at which women have children, especially the first, can result in a substantial excess in the period TFR over the cohort equivalent. In the second, a deceleration in the pace of childbearing, and particularly delay in the age at first birth, can result in a shortfall in the period TFR by comparison with its cohort counterpart. Thus, the period/cohort contrast can be informative. However, the view taken in this paper is that cohort series are not necessary to identify timing changes provided period data are sufficiently detailed and, indeed, they may not be necessary for any other purpose either.

Figure 1 Total Fertility Rate and Cohort Completed Fertility, France and Sweden, 1892/1901-1993



Source: INED, France

Note: Each cohort figure is plotted against the calendar year at which the cohort was at its mean age of childbearing.

The argument that both approaches are "useful" (in the sense "necessary") and that we can get along perfectly well with two, discrepant, accounts of fertility trends is, I believe, fundamentally flawed. This is because in attempting to explain fertility trends, we need to choose one or other of the two accounts as a dependent variable to work with. If we wish to build models (whether formal or not) incorporating social, economic and cultural variables as explanatory factors, it is necessary to know what it is that we are attempting to "explain". Is it the period or the cohort version of events that is the explanandum or, indeed, some combination of the two? The need for clarity about what is the appropriate dependent variable for explanatory purposes is the principal academic argument for attempting to resolve this question.

For policy-making and other applied purposes, however, the question of interest is more likely to be what the implications of current fertility are for future population size and structure. This introduces another reason for wishing to analyse and resolve the discrepancies between the period and cohort perspectives on fertility. In assessing future prospects for a population - in e.g. making population projections - it is customary to extrapolate from current conditions and/or recent trends. Simple fertility assumptions might be that current fertility levels will remain constant, or that fertility will rise or decline from current levels in the decades to come. But how do we evaluate the "current level" of fertility? On a period or on a cohort basis? In the case of the French figures seen earlier, was fertility performance in France in the early 1990s at or below the replacement level of 2.1 children per woman[9]? The figure from the most recent French cohort having completed its fertility is close to 2.1 while the period figure in 1993 is 1.65: a crucial difference in demographic terms since it represents the difference between long term stability and long term decline in population numbers[10]. Hence, for practical purposes as well, a choice must be made between the two representations since both fertility assumptions and the projection process itself must be formulated in one or other mode.

3. RECENT ORTHODOXY

Recent orthodoxy is probably best summarised by a series of distinct propositions, as follows[11]:

1. Period fertility fluctuates more than comparable cohort indices because changes in the timing of cohort fertility distort period measures of the level of fertility. When the pace of cohort childbearing is accelerating period fertility measures are "too high" and when cohort timing is decelerating period fertility indices are "too low", relative to their cohort counterparts.
2. Period measures are therefore unsatisfactory. We need especially to remove the tempo component from period indicators.
3. Cohort fertility is more "real" and period fertility more "unreal" or "transient".
4. The true time-path of change is to be seen in the cohort series.

5. Cohort fertility is ultimately what is of interest and period fertility is but an imperfect guide to it.
6. Period fertility is of interest largely for pragmatic reasons, because cohort figures are out of date by the time cohorts have completed their childbearing.
7. Period measures are a useful way of establishing a population's current reproductive performance.
8. Policy issues lead to an interest in period fertility.

The only one of these propositions with which I agree fully is the last of them. In relation to the first, while it is true, as already noted, that period measures are more volatile than cohort indices, it is a misrepresentation to suggest that changes in cohort timing are a prior, more fundamental cause of this volatility. Propositions 2 to 5 are, in my view, false. It is true, as statement 6 has it, that completed cohort figures are out of date but the present paper argues that period fertility is of a great deal more than pragmatic value, and is, in fact, the more fundamental of the two. Finally, I argue that the notion of a population's "current reproductive performance" is meaningless and misleading. The remainder of the paper is given over to substantiating these points, and to discussing some of the objections to the view advocated.

4. WHY RECONSIDER?

The starting point for the re-evaluation of the period versus cohort fertility issue is the evidence provided by a number of statistical analyses of sets of fertility rates classified by age, period, and cohort and of time-series information on total fertility and mean age at childbearing. By the early 1970s, when Brass (1974) was writing on the subject, it had become the established orthodoxy that the period approach was misleading with respect to fertility trends and that the picture of temporal change presented by cohort time-series was a more reliable one. Brass's 1974 paper seems to have been the first to draw attention to the likelihood that cohort rates were generated as little more than the average of period rates. His results suggested that period fluctuations were dominant and that cohort indicators were of "no help for assessing future fertility levels" (Brass, 1974, p. 561). This conclusion dealt a severe blow to hopes for practical benefits claimed for the cohort perspective: viz. that cohort effects could be evaluated from incomplete cohort experience and used for projection purposes. If cohorts were distinctive in their childbearing throughout their lifetimes, then an indication of the future fertility of cohorts that had not yet completed their childbearing could be obtained from their experience to date and used to project forward. On the other hand, if birth cohorts have no special properties or propensities in relation to fertility, other than those that can be explained through period factors, then the past fertility histories of cohorts whose fertility is as yet incomplete hold no information that would help in anticipating their future behaviour.

Brass's concerns were primarily with the potential practical utility of cohorts for prediction purposes. Later statistical investigations, however, addressed the more academic issue of arriving at a satisfactory statistical description of fertility time series or with testing for the existence of cohort effects per se[12]. A number of analysts, using different data sets and a variety of methods, have examined the statistical evidence for cohort effects (Brass 1974; Page 1977; Pullum 1980; Smith 1981; Namboodiri 1981; Isaac et al 1982; Rindfuss et al 1988; Foster 1990). The overwhelming consensus from these investigations is that a period-based model of variation in fertility rates over time is to be preferred to a cohort model, and that cohort effects are not evident in the fertility series of contemporary developed societies. The situation in relation to mortality is quite different: there is equally clear evidence of pervasive cohort effects on mortality. Indeed the distinction between fertility and mortality in this respect was noted as early as 1937 (Barclay and Kermack 1937). Cohort effects in mortality tend, furthermore, to be specific to particular causes of death, reflecting the differing physical mechanisms to which they owe their origins. Recent studies include those of Osmond et al, 1980, Mason and Smith 1985, Caselli and Capocaccia 1989 and Caselli 1990.

It can be claimed that the statistical identification problem precludes strong conclusions about the relative merits of a period- versus cohort-based model of fertility rates[13]. However, there are substantive grounds for preferring the period formulation. This is particularly clear in Foster's 1990 time-series investigation which finds that, in a cohort formulation, cohort fertility timing leads cohort quantum by 9-10 years, with an index of cohort variability displaying a somewhat longer lead over cohort quantum. If a cohort version of the model is chosen in preference to a period version, it would be necessary to find substantive reasons for such patterns. This would be a difficult if not impossible task: there simply is no known mechanism — social, economic or cultural — that can be specified at present as capable of inducing changes in cohort timing that predate changes in cohort quantum. On the other hand, Foster's period model, which incorporates a fluctuating level of period fertility and, in developed countries since 1945, a decline followed by a rise in the mean age at childbearing, gives a satisfactory and parsimonious account of these patterns.

It is interesting to note that although there are authors with strong views about the existence and significance of cohort effects, there has never been a serious challenge to the verdict of the wide range of statistical studies regarding the absence of cohort fertility effects. The implication is that the statistical findings favouring period as the predominant source of temporal variation in fertility have been accepted not only by the broad reach of unengaged demographic opinion but also by advocates of the cohort approach, whether positively or by default.

5. WHY ARE COHORTS STILL IN USE?

Cohorts are, however, extensively used in present-day demography. It is intriguing that the demographic orthodoxy in relation to the importance of cohorts has been little affected by the absence of a statistical case for the existence of cohort fertility effects[14]. No doubt, Norman Ryder's championing of the cohort case was influential in this respect and the widespread acceptance of his ideas has probably been helped by the undoubted practical utility of the cohort approach, wherever period measures of traditional type are in use. As seen earlier, a time-series of cohort TFRs is a useful adjunct in indicating the extent to which the period TFR is influenced by timing changes. Another possible reason for the cohort's endurance as an idea and as a mode of analysis is that the measures usually generated in cohort mode are quantities that have real significance in the life cycle of individuals: family size can be observed at the individual level and is experienced by individuals, as are birth intervals and other timing phenomena. A cohort approach has affinity with forms of thinking that are rooted in the detail of individual experience and that represent macro-level observations as simple aggregations of such individual-level experiences. The cohort's persistence may also be in part the incidental outcome of the manner in which computer software for survey data analysis is structured. In analysing survey event history data in any of the commonly used statistical packages - SAS, SPSS etc. - it is much simpler to produce trend information by cohort than to generate rates that are period-specific. Trends across cohorts can be produced by tabulating by year of birth of woman or by age at interview. On the other hand, while the numerators of rates in a period perspective can be produced fairly readily by standard packages, the denominators are rather more complex to compute. Available software may, thus, be having the incidental effect of biasing users towards a cohort representation of trends. This bias may, of course, also reflect lack of demand, since a period perspective is not particularly prominent in the social sciences in general. A further contributory factor to the success with which the cohort idea has taken root and flourished in demographic thinking may well be that the case for the cohort did not rely on a single argument. The cohort perspective has been sustained by a range of arguments, though in recent times these have not always been explicitly recognised.

6. HISTORICAL BACKGROUND

To understand the basis of currently conventional thinking on fertility measurement, and the persistence of the cohort perspective, we must consider the history of the subject and the context in which it arose. The cohort versus period issue in relation to fertility came to light initially in the 1940s. In the 1930s, fertility had been in apparent free fall in industrialised countries for half a century and had reached what was then thought of as below-replacement levels by the 1920s. Population replacement was, thus, a live issue. The fear of population decline was perhaps as topical a question then as concern for the environment is nowadays. The outlook in most developed nations was of medium- to long-term population decline as a result of low fertility. Hence, fertility and its implications for population growth became a central focus of attention, both in demography and in wider public discussion.

The measures of time-trends in fertility in use at that time were of the period type: the gross and the net reproduction rates (GRR and NRR)[15]. The first of these was considered an indicator of "fertility in itself", that is time trends in childbearing per se, and the second was thought of as a measure of long-range replacement prospects, since it allowed for mortality of female children born. Because of the topicality of population replacement prospects, a particular aspect of fertility rates of that time was troubling to demographic observers: the rates were prone to displaying considerable variation within a relatively short time-span. For example, in Germany the NRR fell to 0.698 in 1933 and climbed back to 0.934 by 1936: within four years, prospects of severe long-term decline in population seemed to have given way to a much more modest expected decline (Hajnal 1947, Table 1). In England and Wales, the NRR stood at 0.81 in 1939 but had declined to 0.74 two years later; there followed a rapid rise to 0.99 in 1944 and to 1.11 in 1946. Such sudden shifts were seen as anomalous because population growth prospects were known to change only slowly: it was unreasonable to suppose that a population facing long-term decline in one year could, a year or two later have long-term growth in prospect. Yet this is what the then conventional interpretation of these indices suggested.

A landmark paper by John Hajnal, the Anglo-Hungarian demographer and statistician, was much celebrated in its day for providing a solution to these difficulties (Hajnal 1947). Hajnal proposed using generation-based reproduction rates rather than time-period indicators. The cohort cause was later taken up by Norman Ryder in his Ph.D. dissertation (Ryder 1951) and in a series of papers spanning almost four decades that have been extremely influential[16].

7. CLASSIC ARGUMENTS FOR THE COHORT

Hajnal's 1947 paper put forward two key arguments for adopting a generation rather than a time-period framework for the depiction of fertility trends. The first of these was that period measures of reproductivity, particularly synthetic indices such as the GRR and the NRR (see footnote 15 above), were inadequate because they fluctuate too much. A satisfactory measure of fertility change would, he argued, change more slowly. It is clear that what Hajnal had in mind in evaluating fertility trends was how fertility was changing in its role as contributor to the future reproduction of the population, that is to "reproductivity"[17]. Generation total fertility evolved more slowly, and hence behaved more as one would expect of an indicator of reproductivity. There are two flaws in this argument, as I see it, whether in Hajnal's original or Ryder's later formulations.

Fertility as dependent vs independent variable

On the face of it, the argument seems reasonable. Where the purpose of monitoring fertility is to evaluate future population growth/decline, an indicator suggesting rapid year-on-year change in this respect will have little plausibility, since growth prospects change only gradually. The first problem is, however, that this is not the only objective of keeping track of fertility. We should distinguish two quite separate purposes for which time-trends in fertility are of scientific and practical interest. Fertility can be considered, on the one hand, as dependent variable, but, on the other, as independent variable. Fertility is of interest to us as a dependent variable when we are trying to find explanations for how it changes through time. Viewed as an explanandum, there is no reason why a measure that indicates volatility in fertility series should be disqualified from consideration, since there is no a priori reason for expecting the phenomenon to be slow-changing. Indeed, it appears empirically plausible, on sociological as well as on socio-biological grounds, that human populations should have the capacity to respond rapidly to current conditions since it would seem to be highly advantageous to a group to be able to restrain reproduction in unpropitious times and to take advantage of favourable circumstances when they arise[18]. From a statistical perspective, the presence of substantial variance in period fertility series suggests that it is in the period domain that the mechanisms determining fertility change are to be found. For these reasons, the variability of period fertility rates appears indicative of the likely primacy of the period mode, where fertility is of interest as explanandum.

The second purpose for which we track fertility trends is to try to gauge growth prospects. Probably because of contemporary concern, it is this that Hajnal had principally in mind when he specified that a fertility indicator should change only slowly through time. It is certainly true that growth prospects change only slowly and any indicator of the reproductivity of a population would therefore be expected to display only gradual change. This does not imply, however, that all indicators of time-trends in fertility - whether as dependent or as independent variable - should display a smooth trend.

"Current" replacement prospects

The second flaw in the Hajnal/Ryder argument regarding the characteristics appropriate in a reproduction rate is the question whether such a rate can defensibly be calculated from current fertility rates. Although demographers routinely talk and write about a population's prospects of reproducing itself on the basis of "current" fertility - evaluated on a single year's or period's rates - this idea could be considered an absurdity. At any given time, the replacement prospects of a population depend very little on the birth rates of a single year, but rather on surrounding fertility levels - childbearing in the recent past as well as in the immediate future. The calculation of an annual figure to represent the current replacement prospects of a population is a convenient fiction which is, however, an error if interpreted literally. Demographers are well aware that any single year's TFR or NRR has little or no significance in itself for future population prospects[19], but the routine calculation of such indices almost certainly leads to distorted habits of thought. Furthermore, it is well established in demographic analysis that fertility is only one of a number of determinants of future growth prospects. A population's current age structure can be of immense importance for future prospects: population momentum means that where, for example, a population has a very young age structure, it will continue to grow, in some cases for many decades, well after fertility has reached "below replacement" levels. Mortality trends too can be of major significance for future population size and structure. Except where unusually heavy, migration is not, in general, of cardinal importance to population prospects at national level[20]. It is, thus, quite unjustified to emphasize exclusively the level and behaviour of just one element - fertility - of the several that determine future growth prospects. Yet, this occurs implicitly in demographic commentary that focuses on whether fertility is above or below replacement.

The implications of my argument here are first that the notion of "current" fertility is misleading in the context of replacement prospects if it is evaluated on a single year's or short period's fertility performance. We should replace it with a concept of "recent" fertility, evaluated over a longer stretch of time. Furthermore, measures of the reproductivity of a population are needed that do not incorporate the supposition, known to be false, that fertility is the only determinant of future growth prospects.

The distortion argument

The second of Hajnal's arguments arises from his demonstration that sharp fluctuations in period-based reproduction rates may result not from changes in the ultimate number of children born to birth or marriage cohorts of women but from alterations in the timing of marriage and birth. This suggested that time-period reproduction rates - the precursors of our present day TFR - could therefore, in some circumstances, mislead about the "level" of fertility: being too low when cohorts were slowing down in reproduction (postponing) and too high when they were accelerating (bringing forward) their births. This is the "distortion" argument. It was elaborated further and at length by Ryder and is, perhaps, most clearly expressed in Ryder (1980). The claim that period measures misrepresent the true fertility levels in a population has probably been the most persuasive point in favour of the cohort approach.

The difficulty with this account is that it takes period measures to task, so to speak, for doing badly something that they should not be asked to do at all: viz. pronounce on completed family size. To say that the period TFR distorts the truth about fertility is to suppose both that fertility measures should be formulated in "mean family size" terms and that the cohort version of this quantity is, so to speak, the gold standard. Neither of these assumptions is necessary. In my view, it is not the period framework which is distorting, but the inappropriate type of indicator generated within it and the faulty expectations regarding what can be inferred from period figures. Family size is built up over a number of years, in general, and it is inappropriate to characterise the fertility out-turn of a single calendar year by means of an indicator (mean family size) appropriate only to a lengthy stretch of childbearing. Period fertility should be represented by period-style indicators that are an authentic reflection of what happens in a period. The suggestion that the metric of period fertility measures should be true to the period format almost certainly leads to the use of straightforward birth rates per 1000 women, specific for a variety of structural factors, in preference to synthetic or hypothetical cohort[21] measures such as the TFR, the GRR or the NRR. If alternative measures of this kind were available, period rates could not be described as being distorted versions of cohort quantities. It is the measures, and their misuse, that are faulty and not the period approach itself.

Past history

A further ground that has been used to support the cohort view is that "past history" influences current fertility, and that this implies the need for a cohort perspective. In its modern form, this was one of Ryder's claims. The reasoning was that since individuals' current fertility behaviour is influenced by their fertility in preceding years, aggregation into groups - i.e. cohorts - assumed to be homogeneous with respect to past history would remove the influence of previous events, thus providing more contemporaneous measures. However, we know from the statistical evidence that cohorts are not distinctive and so the assumption of homogeneity with respect to past experience is invalid. Furthermore, while it is certainly true that "past history" - both of individuals and of aggregate populations - influences present behaviour, such influence can be controlled for by introducing appropriate forms of specificity in fertility rates. In particular, since one of the principal features of past history that has a bearing on current rates is how many children a woman has already borne, making fertility rates specific to the parity of women[22] is an excellent way of removing the influence of previous fertility experience on current rates. Indeed, this was one of Hajnal's proposals: he suggested that annual fertility rates should be specific to the number of children already borne by women. This was the second leg of his solution to the problem of period vs cohort (the first being the use of cohort data to evaluate reproductivity) and its aim was to remove the worst of the distortions present in annual reproduction rates. Thus, the difficulties arising from past history can readily be handled in period format and do not call for a cohort approach.

Cohort distinctiveness

While the smooth trend, distortion and past history arguments do not, I believe, lead us to a cohort formulation, none of them is affected by the statistical evidence on the absence of cohort effects. The statistical evidence against the existence of cohort effects would not, one supposes, have been interpreted as countering these arguments. Hajnal's paper did not, in fact, argue that each cohort was special in itself. It was Ryder who elaborated this aspect of the case for the cohort. Ryder's claim regarding the distinctiveness of cohorts is effectively negated by the statistical evidence discussed earlier: that is, the fertility behaviour of women does not vary systematically according to their year of birth, in any dataset analysed hitherto[23]. Rather, it is periods that appear to exert strong and distinctive influence on fertility rates. There are furthermore many well-known examples of a clear response in fertility time series to specific period conditions - war and its aftermath, famine, economic depression, changes in legislation and so on.

Summary

These four points are the central planks of the classic case for the cohort[24]. On detailed scrutiny, none of the substantive arguments for a cohort approach to fertility leads inexorably to a cohort perspective. Indicators of fertility change do not need to be smooth in appearance, period fertility measures cannot reasonably be expected to do the job of cohort measures, and the influence of “past history” can be removed from current rates by making these specific by parity and other factors to be discussed below. Above all, the statistical evidence indicates that cohorts are not distinctive. All the grounds on which the cohort has been viewed as superior to the period approach are either neutral between the two or favour the period perspective. Our analysis therefore leads to a conclusion in favour of period rather than cohort as the preferred mode of fertility analysis.

8. ALTERNATIVES TO CURRENT METHODS

While the verdict here is in favour of the period approach, the recommendation is a qualified one. The measures of period fertility in current use are unsatisfactory as are conventional expectations about what can be inferred from period figures. The TFR, in particular, is deficient in several ways. Its primary defect as a measure is not that it does not approximate well to the cohort trend in total fertility. This is the charge that has been levelled at the TFR for several decades but it misses the mark, in the view taken here, because the cohort series is a false criterion. The problem with the TFR is that it is inadequately standardised, largely because it is not generated from parity-specific rates. It therefore gives a biased account of period change in overall fertility. A further difficulty with the TFR is that it is based on the hypothetical cohort principle: it generates an estimate of the result of a life-time’s childbearing on the basis of a single year’s rates. While synthetic indices of this kind are convenient as summaries, in the case of fertility analysis they misrepresent the true nature of period fertility phenomena, since they are expressed in the wrong metric. Finally, the interpretation of the TFR has been faulty, since it has been seen as providing the basis for statements about long-term growth in a population. As we have already seen, no single year or period’s fertility can be used defensibly for this purpose.

The alternative I wish to recommend is that time trends in fertility be quantified by means of indices grounded in the period parity progression system of measurement (Henry, 1953; Ní Bhrolcháin, 1987; Feeney and Yu, 1987; Rallu and Toulemon, 1993a, 1993b). In this approach, period indices are constructed on the basis of rates that are specific for parity, duration since previous birth and/or age. The general rationale for such rates, which has been touched on already in the preceding section, is elaborated a little further here.

In a population where contraception is widespread, couples can and do choose both the numbers of children they have and the timing of their births[25]. In a contracepting population, couples will usually have some idea of a desired family size, though this need not be a very precise idea and need not function as a very definite behavioural goal. In these circumstances, couples' fertility intentions are closely related to the number of children they already have (the parity of the woman) and fertility control is, correspondingly, exercised in a parity-specific way. Couples who have reached their (approximate) desired family size will have a greater incentive than others to avoid a further birth, and will act accordingly. Hence, the number of children already born to a woman (her parity) influences the probability of a further birth in a way that is not true under a natural fertility regime. Furthermore, decisions to "postpone" a birth, and especially the first birth, almost certainly occur, when conditions dictate, for a variety of reasons: simply "for the time being", or until e.g. there is a greater sense of economic security or that the costs of setting up an independent household have been met, or until one or both partners have established themselves firmly in the occupational sphere and so on[26].

All in all, this means that no ineluctable train of events is set in motion by the decision to take the first step in family formation - to marry or, as is increasingly the case in recent years, to cohabit. Decisions about marriage and the formation of a family can be and are taken one by one, and decisions about each succeeding stage of family formation can be made independently of decisions about previous stages, particularly because of the availability of fertility control. This means that fertility, and especially marital fertility, is no longer all of a piece, so to speak, but has separately articulated phases. The component elements of overall fertility - union formation, birth of first child, birth of second child etc. - may, as a result, display differing time trends and need not be in step with one another. Divergence between the component time-trends will occur to the extent that the separate stages of family formation are influenced by different external factors - whether social, cultural or economic - and that time-trends in these external determinants are not uniform. In concrete terms, fertility trends may result from fluctuations in any of: (a) the frequency and timing of union formation[27]; (b) the frequency and timing of the first birth; (c) the frequency and timing of the second birth; and (d) the frequency and timing of third and later births. In these circumstances, we need as many indicators of fertility trends as there are independently varying components to fertility.

There is now ample evidence that time trends in these constituents of overall fertility do not always move in concert. In recent British series, time trends in the parity-specific rates tend to be grouped into two: the earlier events in the family formation sequence (marriage, first and second birth) which tend to move together and later events (third and fourth birth) which display quite distinct trends. Trends in very high order births - fifth and later, in a developed country context - tend to show yet further patterns. It is particularly noteworthy that the onset of the “baby-bust” - the rapid decline in fertility starting, in Britain, in 1964 - was marked by a rapid decline in the propensity to have a third or fourth child, while the propensity to have births of lower orders remained fairly stable until the late 1960s (Ní Bhrolcháin, 1987). There is a similarly marked contrast between the relative stability, in Britain, of first and second birth rate indicators in the 1970s and 1980s with the initial sharp drop and subsequent slight recovery of the series representing third and fourth births (Murphy and Berrington, 1993). The component parts of “overall fertility”, then, do not move in concert: fertility is not a unitary phenomenon. The same general points could be made about a number of other European countries, though the groupings by order may vary between countries.

The rates proposed, then, are of the following type:

$$\frac{B_{t,x,i,d}}{W_{t,x,i\text{-}1,d}}$$

where t is the calendar year in question, x = age, i = order of the birth/parity of woman, d = duration since previous birth or since marriage in the case of first birth. $B_{t,x,i,d}$ thus represents births in year t of order i to women aged x whose i-1th birth occurred d years previously, where i is at least 2, or whose marriage occurred d years ago where i is 1[28]. The denominator of each rate $W_{t,x,i-1,d}$ represents, correspondingly, women at risk of such a birth: that is, women who in year t were aged x, were of parity i-1, and had their previous birth (or were married; but see footnote 28) d years ago[29].

The need for parity specificity has already been discussed. The rationale for rates that are specific also by duration and age is similar. Just as death rates need to be made specific by age to remove the effect of age-structure on mortality indices, so also structural (i.e. compositional) influences on fertility rates need to be removed. Age is the traditional factor according to which fertility rates are specified, and there is a clear rationale for so doing both in biological variations in female fecundity with age and in the social origins of the strong patterns of differentiation of fertility rates by age[30]. Along with age-specificity, there is a strong argument for making rates specific by duration since the previous birth (where the birth is second or later) or since marriage/start of union (where the birth is the first). The reason for introducing duration as an additional dimension to the rates is both because birth rates are highly differentiated by duration since previous birth (or marriage) and because it is through delays/advances in this dimension that timing changes occur in contracepting populations. Without a duration control, alterations in the population at risk resulting from previous decisions to have/not to have a child at a particular stage could influence, in an extraneous manner, birth rates calculated for a particular calendar year: that is, a duration control has the same rationale as parity-specificity, in removing the compositional influence of previous history on the population at risk. The calculation of age- and/or duration-specific rates allows, in particular, period timing effects to be observed, in the occurrence of movements in parity specific rates that are differentiated by duration and/or age.

9. SUMMARISING THE DISAGGREGATED RATES

My recommendation of rates characteristic of the period parity progression approach to fertility measurement is, however, qualified by the proviso that we should base our measures not on the synthetic quantities - parity progression ratios, birth intervals, TFR[31] etc. - that are standard to this methodology but on the disaggregated parity- and duration-, and perhaps also age-specific rates themselves. This is a potentially controversial suggestion since it appears to present us with a very large number of rates with which to describe year-on-year change. For example, if for economy we restrict analysis to 6 parities, 10 years of duration since previous birth and, say, 35 single years of age we would need 2100 indicators to describe the level and pattern of fertility in each year. Even if age were to be rationalised into 5-year bands, fertility movements would have to be represented by 420 distinct time series; ignoring age altogether would still leave us with 60 indices per year.

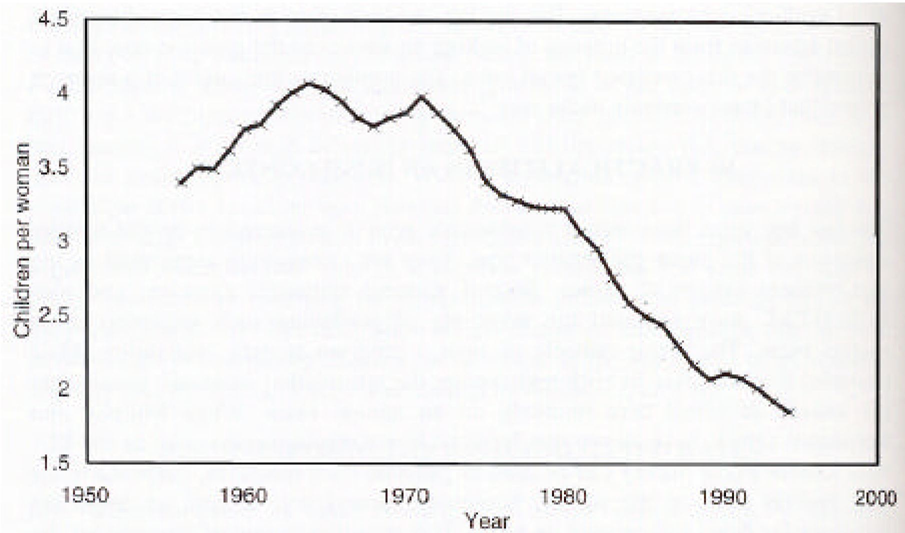
Large arrays of rates of this kind by parity, age and duration since last birth are clearly an impractical way to represent fertility trends. There is little doubt that such arrays would be heavily patterned and could readily be summarised. There is, however, no a priori reason why this should be done by means of synthetic cohort-type indicators of the traditional type - TFR, period parity progression ratios[32] and the like - and every reason to avoid these. While the argument that hypothetical cohort-type indicators are the natural way to summarise the rates is an attractive one, its attractions should, I believe, be resisted. The reason why is because any synthetic cohort indicator — whether summarising in terms of a period parity progression ratio, a period birth interval or a total fertility figure of some sort — is definitionally expressed in a metric that is inappropriate for a period indicator. As long as we use such indicators, we avoid facing the problem of what the true metric for year-on-year fertility change should be. Certainly it is not clear, at present, what the metric should be, and so perhaps a sensible interim procedure is to continue using synthetic-type measures. But the convenience of such measures should not deflect attention from the urgency of seeking an answer to the question how best to summarise the disaggregated period rates. The problem is the subject of a research project that I have currently under way[33].

10. PRACTICALITIES IN AN IRISH CONTEXT

The last few years have seen a considerable growth in interest in period fertility indicators of the parity progression type. They are increasingly recognised as the measurement system of choice. Several national statistical agencies, and also EUROSTAT, have explored the feasibility of producing such indicators on a routine basis. The major obstacle to such a program is data availability. Most countries do not collect, at birth registration, the information necessary to calculate the indices described here routinely on an annual basis. While Murphy and Berrington (1993) have shown that large-scale household surveys such as the EU-wide Labour Force Survey can be used to generate such measures, particularly for time periods close to the survey, household surveys are, at best, an imperfect substitute for direct information on births. It is therefore of special interest that the combined birth registration and perinatal reporting system established in Ireland in 1981, the joint system being 94% complete since 1984 (Planning Unit, Department of Health, 1993), collects all the details necessary to generate the numerators of the rates discussed above. The two key items of information collected under the current registration arrangements, and unusual in an international context, are the question on the date of the previous birth and the inclusion in the recorded number of previous live births of extra-marital births and any children not born to the present marriage[34]. These data would make it possible, in principle, to generate what could be a fascinating and detailed picture of Irish fertility trends since 1984, in parity-, age- and duration-specific terms. However, preliminary work would be needed to estimate denominators for the rates.

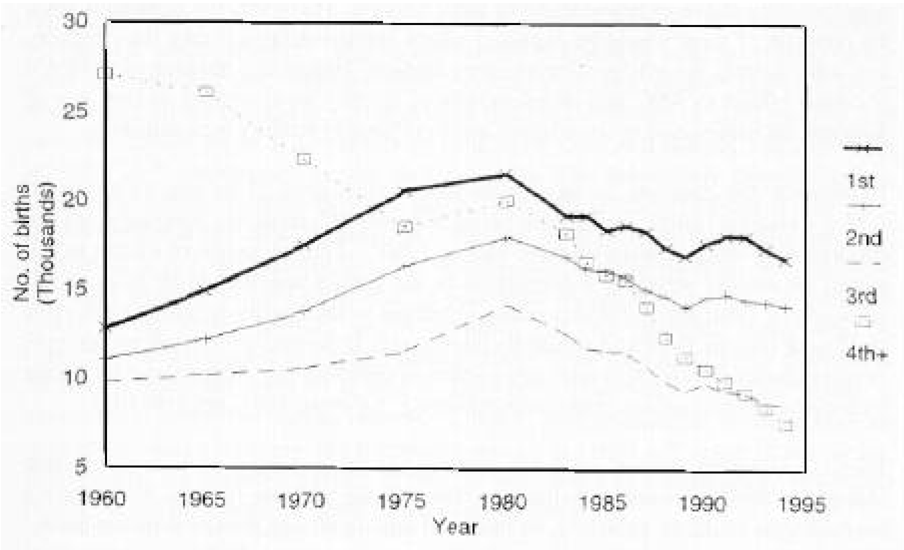
A brief look at the TFR in Ireland in the last four decades reveals a picture similar to that in other parts of the developed world (Figure 2). As elsewhere, Ireland experienced a substantial baby boom in the two decades after the second world war, the TFR peaking in 1964 at just over 4 births per woman, a high figure in the European context. In common with other Western nations, Ireland has seen a massive subsequent decline in the TFR, punctuated only, in the Irish case, by a brief plateau in the late 1970s. The TFR in Ireland is now below the critical replacement level of 2.1 children per woman[35]. Irish fertility finally crossed this threshold, according to Council of Europe figures, in 1989 (Council of Europe, 1995, p. 153), substantially later than the rest of Western Europe, much of which had reached below-replacement fertility by the mid-1970s.

Figure 2 Total fertility rate, Republic of Ireland 1956-1994



That the fertility decline in Ireland since 1971 is unlikely to have been uniform across women of all parities is suggested by Figure 3 which plots the total numbers of births by order in the Republic of Ireland 1960-94[36]. The total numbers of births of orders 1 to 3 increased between 1970-75 and 1975-80, and the numbers of 4th and later births, after a precipitous decline 1960-75, also rose slightly 1975-80. There is then a decline in the numbers of first, second and third births, with a small dip in 1989 followed by a small recovery in 1991. However, we cannot fully interpret these data as they are, since absolute numbers of these kind do not tell us about any changes that may have occurred in the propensity of the population at risk to have children of a particular order. How far, for example, is the rise and fall in the number of first births throughout the period due to changing numbers of women in the prime childbearing years, and/or changes in the frequency of marriage among those of childbearing age and/or variation in the propensity of married couples to have a first child and/or trends in the timing of first births within marriage? Similarly, how far can the trends in second births be accounted for simply by the changing numbers of women who have had a first birth and are, therefore, at risk of having a second and to what extent has the probability of a second birth, given the first, been changing? It is precisely to answer questions of this kind that the detailed parity- , duration- and age-specific rates discussed earlier are designed. Such an array of rates, or indices summarising them, would give a much more pointed and focused account of recent trends in Irish fertility than is possible at present.

Figure 3 Number of births by order, Republic of Ireland 1960-1994



11. DOING WITHOUT THE COHORT APPROACH: SOME FURTHER POINTS

The arguments of this paper are directed at the cohort as a vehicle for the description of fertility trends, rather than other forms of social and economic behaviour. The view taken here is not a general, philosophical objection to the cohort approach *per se*, but to its representation as essential and fundamental to fertility change through time. With present knowledge, no generalisations can be made about the role of cohorts in shaping social behaviour. The question in any particular context is an empirical one, not a question of principle. Examples available from the analysis of mortality indicate that while cohort effects are often present, they can be distinctive to each individual cause of death - being quite different in form for e.g. cancers at different sites (Osmond et al, 1980). There is no basis at present for discounting the role of cohort processes in general in social matters. Lesthaeghe and Surkyn (1988) present, for example, suggestive evidence of cohort effects in relation to ideational factors.

Postponement and linkages through time

Some argue that the “brought forward” or “postponement” effects observed in some fertility series are essentially cohort linked and are, themselves, evidence of “cohort effects”. The “cohort effect” tested for in statistical age-period-cohort (APC) analyses is specified as a tendency for particular cohorts to display rates systematically above or below those of other cohorts. These are, the argument goes, the opposite of what would be expected where cohorts delay/advance their fertility and subsequently behave in compensatory fashion. Hence, the absence of evidence of cohort effects in APC and allied models of fertility is, according to this way of thinking, an irrelevance to the issue of a cohort basis to fertility movements.

To consider the question, let us suppose that women aged 25 in, say, 1990 put off having a (further) child for the time being, but, in 1992, have, for simplicity, all the children they would otherwise have had in 1990[37]. This sequence of events would appear in fertility series as a depression in the (parity specific) birth rates of 25-year-olds in 1990 and a corresponding elevation in the (parity-specific) birth rates of 27-year olds in 1992 and would be interpreted, if general to several age groups, as representing a move to a later age pattern of childbearing. Some insist on seeing such a process as a cohort phenomenon (see e.g. Le Bras, 1991, pp. 100-101).

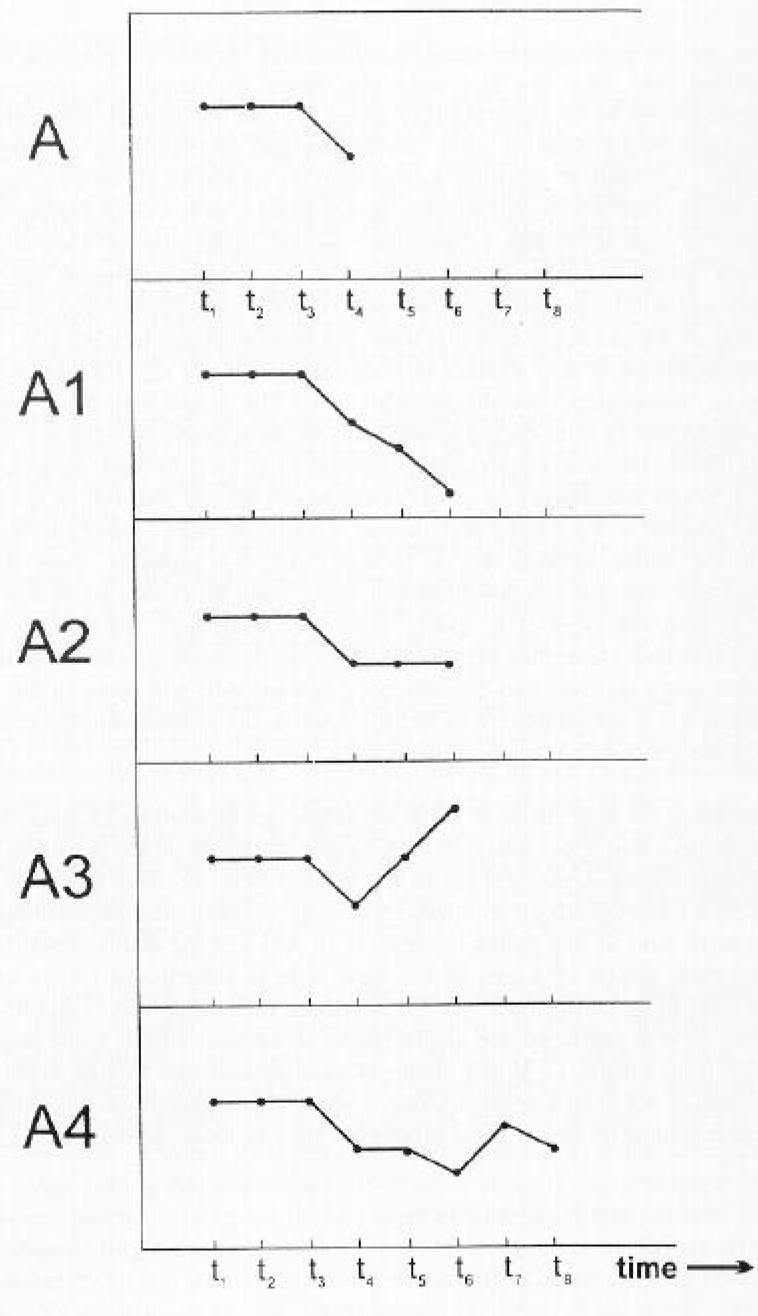
There is, however, neither need nor justification for ascribing such compensating changes to distinctive cohort behaviour. The reasons why are twofold. First, such a phenomenon could, in principle, be observed among all age groups simultaneously, and usually occurs in at least several adjacent age groups. If there were a sharp decline in the fertility of all age groups in, say, 1990 followed by a sharp and compensating rise in the rates of these same women two years later, nothing that could be described as "cohort specific" would have occurred. The phenomenon would be a period feature, one extending over a 3-year time-span.

There is, furthermore, no need to employ an age-reference in describing such a process, if the process is not specific to a particular age group. The situation might be perfectly adequately described as one in which a change downwards in birth rates in year t among women of parity i whose i-th birth occurred d years previously was followed by an upward shift in birth rates in year t+x among women of parity i whose i-th birth occurred d+x years previously, where x is the time-span over which compensating movements are observed to occur in any particular instance. In this description, no reference to the cohort of women affected needs to be made. That compensating movements of this kind can occur is not in dispute. They were documented by Hajnal in his early work on fertility analysis (Hajnal 1947, 1950). Further documentation of the phenomenon in one of its most spectacular manifestations - the post-war baby bulge of the late 1940s in Britain - is given in Ní Bhrolcháin (1985, Appendix B, Table B and 1987).

However, the key point to note about "postponement" is that the phenomenon can be identified only after the fact since the future occurrence of compensating movements cannot be forecast. Fertility series are not, in general, self-correcting. This straightforward point is often overlooked and is illustrated in simplified, stylised form in Figure 4. Ignoring the fluctuations typical of all fertility series, let us suppose that after a period of stability during years t_1 to t_3 we see a sharp drop in birth rates in year t_4. Is this a "genuine" decline in fertility or does it merely represent a “postponement” - that is, are women in t_4 simply putting off their births until later and will birth rates therefore recover in the near future? The view taken here is that all we can say at t_4 is that there has been a decline in birth rates in that year. We cannot know at t_4 whether in later years rates will recover and overshoot in order to “compensate” for the shortfall in t_4. The future may bring with it a continuing decline in rates during t_5 and t_6, as in the scenario labelled A1, or rates may stabilise at the new (lower) level reached in t_4 (A2) or they may return to normal in t_5 and rise sharply in t_6, to “compensate” for the shortfall in t_4 (A3), or they may proceed to vary from year to year with no apparent pattern or structure (A4). If the series behaves as in scenario A3, we probably have what is conventionally regarded as a postponement effect[38], but we cannot know that this is what it is until the rates of t_5 and t_6 have been observed[39]. In other words, a statement at t_4 that the decline observed is due to postponement is no more than an assumption that a recovery and compensating upward shift will occur in the future. Empirically, the temporal pattern of fertility rates is not so mechanically structured that A3 is the inevitable outcome of the pattern seen at A.

This appears to me to be the flaw in the argument put forward by Le Bras, who, in advocating the cohort approach, provoked a vigorous debate on the period vs cohort issue among French demographers in the early 1990s. Le Bras (1991, p. 101), arguing for a cohort perspective, draws an analogy between the compensating links between birth rates at two points in time (as in A3) and the distance between the front and back wheels of a car. In his view it is as superfluous to say that the elevated rates in t_6 “compensate” for the depressed rates of t_4 as it would be to say that, when a car is displaced, the displacement of the back wheels compensates for that of the front wheels. Le Bras’s choice of analogy indicates that he assumes the t_4-t_6 scenario of A3 to be a necessary one, a view which ignores the possibility that the series may actually follow some other path such as those depicted in A1, A2 or A4[40].

Figure 4



As I see it, “postponement” is a cohort phenomenon only in the trivial sense that in order for the term to have any meaning, the same group of women who put off having children in a given year eventually make good the deficit in some later year or years. “Postponement” and the “making up” of births is, nevertheless, a longitudinal phenomenon in the sense that it results from the relationship between behaviours, both at the individual and at the aggregate level, at different points in time. But a “longitudinal” process does not necessarily imply a “cohort” process. Relationships can be longitudinal in character without being cohort-specific, though it is often convenient to employ the cohort as a measurement framework in making such observations. Often, it is from follow-up cohort studies that evidence is provided of links between behaviour or experience at different points in time. In many cases, however, it is the longitudinal - i.e. follow-up - nature of the data collection rather than its origin in a study with a cohort design that is critical[41]. It may well be that the tendency to think of postponement and similar phenomena as essentially cohort-based results from persistent confusion between the related but distinct concepts: “cohort” and “longitudinal”. This confusion is probably not confined to demography, being fairly general in the social sciences.

Mean number of children per woman/mean family size

A further objection to the view that cohorts are unnecessary is that the only sensible way in which to arrive at a figure for the mean number of children actually born to women in a particular society is by observing a cohort mean family size. It is true that a genuine, non-synthetic estimate of the mean number of children per woman can only be obtained from data organised in some kind of cohort framework, whether birth or marriage cohort. One answer to this point is that genuine estimates of mean family size and the distribution of family sizes, not based on a synthetic cohort calculation, could be produced from a time-series of period rates, if these are specified within the period parity progression framework: that is a sequence of indicators from each year in a span of years could be used to replace the cohort-derived figure.

Beyond this, however, it is worth stepping back to consider whether the idea of mean family size or mean number of children per woman is, indeed, of crucial importance? It is entirely possible that when demographic analysis develops further this quantity may not be at all central, or even of any significance at all, to formal population analysis. The population models currently in use - stable and quasi-stable population theory - incorporate an indicator of the mean number of children per woman in the form of the gross reproduction rate or the net reproduction rate. But currently available population models, while prodigiously productive, are known to be incomplete, in particular in the fact that they cannot handle the problem of two sexes: available population models are one-sex models because there is as yet no solution to the problem of modelling the marriage market. New forms of population model are bound to be specified in the future that do not suffer from this limitation, and they may not bear much resemblance to current versions. It is possible to envisage sensible models that do not, in particular, incorporate a quantity corresponding to a mean family size or mean number of (female) children per woman. Demographic interest in the determinants of individual-level variation in family sizes may well be misplaced: one can foresee a time when the distribution of family sizes, and its derivatives, are seen as no more than an epiphenomenon of other, more fundamental, demographic processes. However, that time has not yet come and these comments are acknowledged to be highly speculative. I am not arguing that mean family size is of no consequence, merely that it may be instructive to consider the possibility that it may not be essential. Family size is of undoubted personal importance to individuals. Does its role in present day demographic thinking owe more to an anthropocentrism in our concepts than to its scientific significance?

12. RESUME

The paper argues that the relative merits of the period and cohort approaches to fertility depend on the purpose in hand. For the evaluation of long-term replacement prospects, cohort or cohort-type fertility indices may be useful, though they are not essential since time-period averages could be substituted for them. For other purposes, period fertility is more important than cohort fertility. Its statistical primacy has long been known. However, the statistical verdict in favour of periods has been curiously under-appreciated, perhaps because there appear to be unassailable demographically-based objections to the period approach. These arise because period fertility suffers from problems of measurement and misuse. In my view it is not the period approach itself that is faulty but the indicators that have been used to represent it and the interpretations of such indicators. The basis for an alternative system of measurement is suggested to be fertility rates specific by parity, duration and age. Before such a representation can be of practical utility, however, methods of summarising the resulting array of rates are needed that are true to the period format. The logic of disaggregation nevertheless brings us to the question whether the concept of mean family size is, indeed, a crucial one for population analysis, an issue which is a long way from our starting point.

Footnotes

1. An age-specific fertility rate is the number of births per 1000 women of a given age in a particular year. The age-specific fertility rate, $f_{x,t}$, of women aged *x* in year *t* is, thus: 1000 * $b_{x,t}/w_{x,t}$ where $b_{x,t}$ is the number of births occurring to women at age *x* in year *t* and $w_{x,t}$ is the total number of woman-years at risk at age *x* in year *t*, usually estimated by the number of women aged *x* at mid-year.

2. The total fertility rate, or TFR, is obtained for any given calendar year by summing the fertility rates at each age of woman (conventionally, 15 to 44 or 15 to 49) in that year. In year *t* it is, thus, $\Sigma f_{x,t}$, i= 15, 49 where $f_{x,t}$ is as defined in footnote 1. The result of the summation is conventionally interpreted as the mean number of children a woman would have if she experienced through her lifetime the age-specific fertility rates prevailing in the calendar year in question.

3. Cohort completed family size is defined as the mean number of children born to a birth generation or cohort of women. Other types of cohort can be specified also - e.g. marriage cohorts (people married in a given year) or parity cohorts (women having had a birth of a particular order in a given year). The issue surrounding period and cohort fertility has been discussed largely in relation to birth cohorts.

4. For discussion of the TFR and other types of fertility index, see e.g. Pressat (1972) or Shryock and Siegel (1976).

5. The terms “cohort total fertility”, “cohort completed fertility” and “cohort mean family size” are used interchangeably to refer to the mean number of children born per woman or per 100 women of a given birth generation.

6. One would, ideally, have wished to present time series of Irish period and cohort series in the present context. However, I have not been able to locate a series of cohort completed family size values that appear reliable. Such data appear not to have been published by CSO and while a series of this kind has appeared in Council of Europe sources, the behaviour of the cohort series leaves substantial room for doubt as to its reliability. The France/Sweden comparison here has been chosen because it is in many ways a celebrated one, having sparked off an intense controversy in French demographic circles in 1990 and after. The diagrams of Figure 1 are, in fact, updated versions of those that appeared originally in Calot (1990) and have been kindly made available by Jean-Paul Sardon of the Institut National d’Etudes Demographiques, Paris.

7. Thus, the figure for e.g. the generation of 1920 is plotted against the calendar year 1920 + m_{1920} where m_{1920} is the mean age of childbearing among women born in 1920.

8. While this statement is entirely true of the central part of the period covered, correspondence is not complete at the start and finish of the graphs. The earliest period figures here will be based in part on events (and rates) of some (older) women belonging to cohorts born earlier than the earliest depicted here; the most recent period figures will incorporate events (and rates) occurring to women belonging to cohorts born later than the latest cohort plotted in the graph.

9. Low mortality populations will replace themselves in the long run if each woman has on average approximately 2.1 children and that this level of fertility is maintained over the long term. The additional 0.1 of a child over what might be thought of as a more intuitively obvious replacement level of 2 children per woman (one child to replace each of the two partners) is to allow for mortality: not every child born will survive to reproductive age. In higher mortality societies the replacement level will be greater than 2.1 since many more children will die before reaching the age at which they themselves would reproduce.

10. It can of course be argued that this comparison is "unfair", in some sense, since the cohorts currently having children have, as yet, not completed their families. In 1996 e.g. we can observe the TFR for 1995, subject only to delays in compiling the statistics. But in 1996, the most recent completed cohort figure available is that for women born in 1950, who reached their 45th birthday in 1995. By far the largest part of the childbearing of these women will have taken place between around 1970-1985, when they were aged 20-35. Clearly the most recent completed cohort figure reflects, in the main, fertility events that occurred some time in the past. We could, of course, examine the fertility of a cohort of women who are in their prime childbearing years - say those who reached 30 in 1995; but these women have at least a decade's further exposure to childbearing before them. Thus, we see one of the key deficiencies in cohort statistics. If complete, they are not "current"; if "current", they are incomplete and can only be completed by making assumptions about future fertility.

11. Demographic opinion on the subject is, however, by no means uniform. Informal reaction to an earlier account of the views presented in this paper (Ní Bhrolcháin, 1992) has ranged from surprise at the apparently cavalier rejection of the cohort, well known as a corner-stone of demographic methodology etc., to claims by others never to have had any use for cohort indices. What I describe as "demographic orthodoxy" here is the large swathe of opinion between these extremes, influenced particularly, as will be seen presently, by Norman Ryder's advocacy for the cohort.

12. Statistically, a "cohort effect" is a tendency for members of a birth generation to display event rates (in this case, birth rates) systematically different from those of other birth generations, in a way that cannot be accounted for more parsimoniously by means of age and period effects. The existence of cohort effects became a key issue in the 1970s both because of American demographer

Norman Ryder's concerted advocacy of cohort distinctiveness and because of the interest in Richard Easterlin's theoretical ideas on relative cohort size and its hypothesised cyclical effects.

13. The identification problem is a statistical difficulty which arises when attempting to estimate the coefficients of a regression model in which a dependent variable is expressed as a function of a number of predictor variables, one or more of which can be expressed exactly in terms of two or more of the others (that is, that there is linear dependence among a set of the predictors or any subset of them). This occurs when demographic rates are modeled as a function of age (a), period (p) and cohort (c). The year of a person's birth (c) plus their age at a specified time (a) equals the period (p) in which observation is made. In general, fixing any two of these three variables fixes the third: c=p-a, a=p-c and p=c+a. As a result, if a regression includes all three factors, their parameters cannot be determined uniquely. There are an infinite number of solutions to the equation, and the solutions possible may carry quite different substantive implications. Holford (1991) gives an excellent account of this issue in an epidemiological context.

14. Nevertheless, the absence of cohort effects has not been unmarked at demography's "coal-face", in the practical world of population projections. A recent study by Keilman and Cruijsen (1992) indicates that few national statistical agencies in industrialised countries currently use a cohort model to project fertility. See also Keilman (1990) on this subject.

15. The Gross Reproduction Rate (GRR) is a period measure constructed along identical lines to the Total Fertility Rate (see footnote 2) except that it is confined to female births. It is equal to: $\Sigma f^{f}_{x,t}$ for x = 15, 49 and $f^{f}_{x,t}$ is the age specific rate of female births to women aged *x* in year *t*. The Net Reproduction Rate (NRR) is similar to the Gross Reproduction Rate but makes an allowance for mortality. The NRR is $\Sigma l_x f^{f}_{x,t}$ where l_x is the life-table probability (in a life table of radix 1) of a female child's surviving from birth to age *x*. Each of these rates is an attempt to quantify the extent to which a population is replacing itself at current rates, with the NRR being the more refined of the two, and more generally used, since it corrects for losses through deaths of female children before they reach reproductive age, specified in the calculation as the age at which their mother bore them.

16. Ryder (1980) is probably the most complete and up to date account of Ryder's views on the period *vs* cohort issue in relation to fertility. Ryder (1965) was an attempt to provide a theoretical sociological basis to Ryder's arguments in favour of the cohort perspective in general in human affairs and has been an influential source on this subject among sociologists.

17. Reproductivity is a term used to refer to population replacement. The term can be a source of confusion since it tends to be seen as synonymous with *reproduction* whereas, as will be discussed, this is not the case.

18. In relation to human social behaviour, socio-biological arguments need, of course, to be used with care. The point being made here is simply that it seems biologically plausible to suppose that human reproductive behaviour is flexible and responsive to external circumstances.

19. Except, of course, in cases where there are exceptional increases or decreases in the number of births, resulting in a substantial bulge or indent in the population pyramid. In such cases, the implications are for population age structure rather than for population size *per se*.

20. Migration can be, nevertheless, of great importance in the dynamics of national populations that are small and of local populations.

21. The terms *hypothetical* or *synthetic cohort* are used interchangeably to refer to an imaginary group who experience throughout their lifetime the rates current in a particular year or period. The expectation of life derived from a period life-table is probably the best known index constructed on the synthetic cohort principle.

22. The "parity" of a woman is the number of live children she has had; thus women who have had no children are described as being of parity 0, those who have had one child as being of parity 1 etc. The corresponding term used in relation to the births is the "order" of a birth: a first birth is described as of order 1, a second birth as of order 2 etc.

23. An interesting possible exception to this "rule" was presented recently by Street (1995) in an account of fertility trends in Singapore. It appears that the cohort of women born in 1937 have, throughout their childbearing years, always shown a dip in their age-specific rates relative to surrounding cohorts. This is interesting because there are suggestions that a biological cause may be at work. Street hypothesises that the lowered fertility of children born in 1937 may be linked to a synthetic anti-malarial treatment and prophylactic that was used during a major malaria outbreak that occurred in Singapore in 1935. Since we know that biological factors are the root cause of cohort effects observed in relation to mortality, the discovery of a cohort fertility effect due to possible biological causes suggests that it may be only biological, rather than economic, social or cultural, mechanisms that are capable of generating such effects in relation to fertility behaviour.

24. The issues are discussed in greater detail, along with some subsidiary elements of the case for the cohort approach to fertility, in Ní Bhrolcháin (1992).

25. That choice is exercised in contracepting populations in relation both to number and timing of births is almost certain, though this does not mean that couples behave according to "rational choice" principles nor that active decision-making occurs in relation to each and every birth, nor that the precise outcome of childbearing decisions is actively chosen.

26. Whether "postponement" is a reasonable concept in the fertility context has been a matter of debate. It may be that all that can be done is to decide against a birth at a particular time, with the expectation that it will occur later. A "postponed" birth may, in the event, never take place. The couple's situation may alter, or hoped-for changes in circumstances may not occur. The couple may change their minds about wanting a (further) child, they may become infertile, the union may founder, a partner may die, they may have difficulty in establishing their own household, and so on.

27. Two or three decades ago it was conventional to refer in this context to the level of and age at marriage. With the increasing prevalence of cohabitational unions, "union formation" has become the more generally used term to refer, inclusively, to the start of formal (i.e. marital) or informal cohabitation.

28. In analysing first births, marriage or the start of cohabitation may also be ignored and the birth rates considered simply by reference to age, or equivalently years since 15th birthday.

29. In practice the denominator would be an estimate of "woman years during t" during which all of these attributes held, since individual women can, of course, change their state in respect of any or all of these factors during year t: i.e. they may have a birthday and so reach age $x+1$ and/or they may have a birth of order i during year t and so reach parity i and/or the duration since their $i-1$th birth may reach $d+1$ completed years.

30. The appropriateness of age as a natural control in relation to fertility rates is, however, beginning to be questioned in relation to low-fertility, contracepting populations.

31. The parity progression approach can be used to generate an alternative version of the "total fertility rate", sometimes styled the parity progression TFR. It is obtained by estimating the mean family size that would result from the series of period parity progression ratios implied by the parity- and age- and/or duration-specific rates of a given year. In general, the parity progression TFR differs from the conventional TFR based on summing the age-specific rates.

32. The parity progression ratio of order i is the proportion of women who have had i births who go on to have at least one more birth. Annual or period estimates of a

series of parity progression ratios for each order of birth, calculated on a synthetic cohort basis, are the normal way in which rates characteristic of the period parity progression approach are summarised.

33. This is an ESRC (UK) funded project which aims both to generate time series of annual parity-, duration- and age-specific rates for England and Wales 1941-1991, using the OPCS Longitudinal Study, and to explore ways of summarising the disaggregated rates. The project will be reporting results between now and 1997-8.

34. Most developed countries collect information on the order of each registered birth, but the definition of "order" does not often include children born outside of marriage and is sometimes confined to the present marriage. The order of birth classified in the current Irish registration system is the true order of birth within the mother's complete childbearing history.

35. Note that discussion here has slipped into conventional terms. Sceptical comment in an earlier section on the notion of "replacement level fertility" and its evaluation on annual figures needs to be borne in mind.

36. The figures are at 5-year intervals (1960, 1965 etc.) to 1980 and annual from 1983. Note that the line for 4th+ is the sum of all births of order four and above and so is well above the other lines in the earlier part of the series.

37. This is a simplification since it is unlikely that births deferred from a particular year would all occur during any one subsequent year. It is more probable that the shortfall would, if it were compensated for in later years, be made up over several years rather than within the confines of a single calendar year. Also, this simplifies matters because of the difficulties with the concept of "postponement" discussed in footnote 23 above.

38. Whether this is truly so will depend, however, on the mechanism giving rise to the dip and recovery observed, and subsequent observations in the time series might be informative for this purpose. For example, if what happens is that in t_5 and for a further, say, ten years there is an economic boom and that birth rates go on rising over that period, the apparent t_4-t_6 link might be entirely spurious.

39. It could be argued further that we cannot defensibly claim to have identified a "postponement" effect until we have made a convincing link between the fertility rates in question and the socio-economic conditions that determine such short-term movements. This point is not generally acknowledged in the demographic literature.

40. The situation is a little more complicated than this in that Le Bras's point relates to a decline in the fertility of a particular age group, say aged a in year t, followed by a recovery and rise in the fertility of age group $a+x$ in year $t+x$: i.e. compensating movements occurring within a birth cohort. The argument of the text applies equally to this situation. One cannot know until after the fact that the fertility of a particular generation of women will rise to compensate for a current shortfall. Furthermore, if "compensation" of this kind always occurred, cohort total fertility would be invariant, which is clearly not the case.

41. That a longitudinal study need not be a cohort study is clear from such instances as the American Panel Study of Income Dynamics, and similar household panel studies recently established in several European countries.

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DISCUSSION

John Power: A comprehensive critique is given of the cohort approach to fertility and the flaws identified make a valuable contribution to developing awareness of the methodological difficulties encountered in the study of fertility. The author presents a framework for the analysis of period data which may make a significant contribution to demographic analytical methods. However, the methods presented have yet to be fully specified and empirical results are awaited. For this reason my comments on the paper focus specifically on the author's comparative evaluation of the two approaches to fertility - cohort and period.

The author presents a binary view of cohort and period approaches to the study of fertility. Cohort measures are set against period measures such that they are in competition with one another instead of being complementary reflections of the same phenomenon.

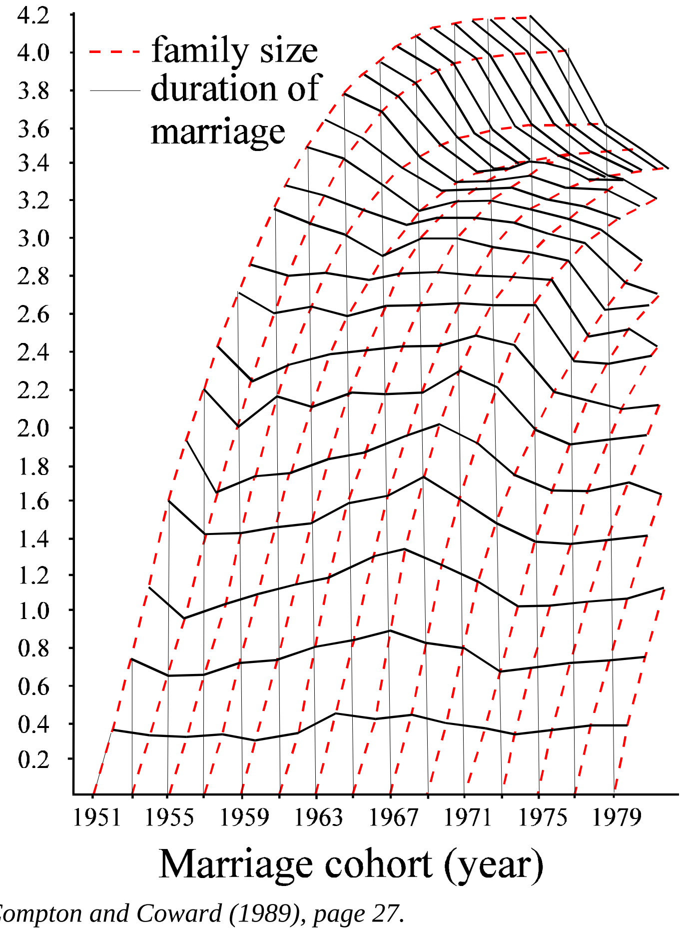
Statistical studies of the influences of age, period and cohort upon fertility consistently identify the largest explanatory power to age effects. The importance of period and cohort can be difficult to grasp since a given age at a specific point in time identifies a birth cohort. The influence of cohort at a particular point in time, 't', is that ascribed to the influence of earlier experiences upon a specific age group beyond those influences present to that age group at time 't'. The cohort effect is therefore a component of the age-period interaction with the focus upon the influence of past experience.

Cohort influences, though more pronounced in studies of mortality, are not conceivably absent from Irish fertility histories. Consider for example, the position of Protestant women in Southern Ireland in the aftermath of World War I. The loss of young Protestant males left unmarried females with a shortfall of potential co-religionist partners. Young married women were left widowed. The effects were felt in nuptiality, fertility, migration and inter-marriage. The influence of the War would not have been identical across all ages and marital states of women and persisted beyond the 1914-1918 period. Age, period and cohort influences all appear relevant with outcomes residing in the cohort domain.

Much of the behavioural differences between a cohort and period series can disappear as the time period from which TFRs are calculated is lengthened and erratic annual fluctuations are smoothed. However, as the author acknowledges, the greater variability in TFRs need not be considered a disadvantage - particularly in the context of analysis where fertility forms the dependent variable. The period approach offers advantages in locating the determinants of temporal fertility change. That period rates can be erratic from year to year while cohort fertility rates move more slowly is reflective of completed family size changing more gradually than births counted at specific points in time. In modernised societies, the propensity of couples to postpone births and vary completed family size cause considerable difficulties in relying upon period measures as a basis for population projections. The smoother trend offered by the cohort approach appears a more reliable indicator of long run population replacement.

The most recent study of fertility in Northern Ireland (the N.I. Fertility Survey, 1983) examined the relationship between rates of family formation and period rates. Figure 1 plots cumulative fertility by marriage cohort from which period measures were calculated by summing the annual birth increments across marriage cohorts.

Figure 1 Cumulative fertility patterns; linkage between cohort and period trends

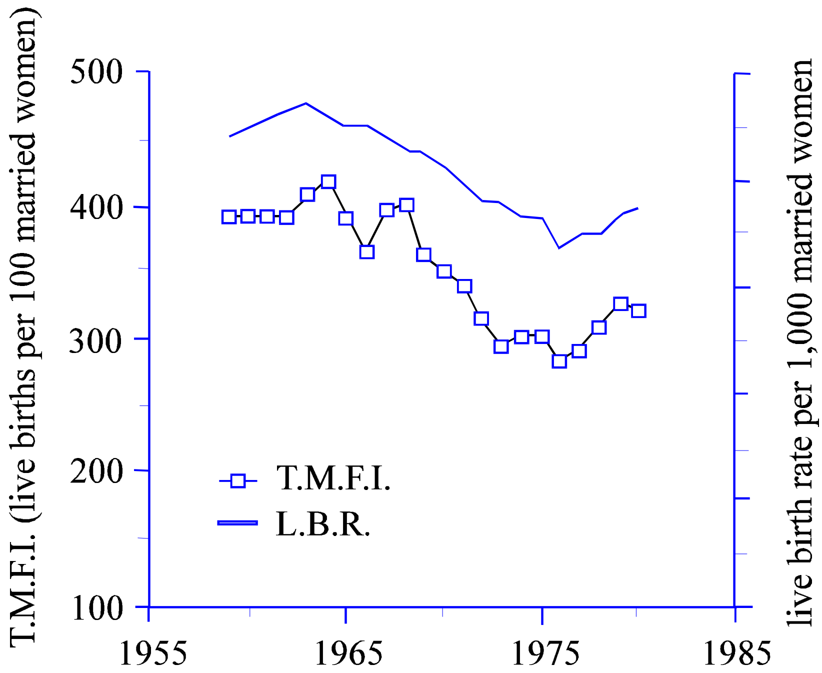


Source: Compton and Coward (1989), page 27.

Summing the annual cohort increments for each year provided a period fertility rate which Compton and Coward referred to as the Total Marital Fertility Index (TMFI).

The TMFI index when compared to annual live birth rates were found to correspond closely (see Figure 2).

Figure 2 Comparison of the total marital fertility rates and crude birth rates: 1959 to 1982

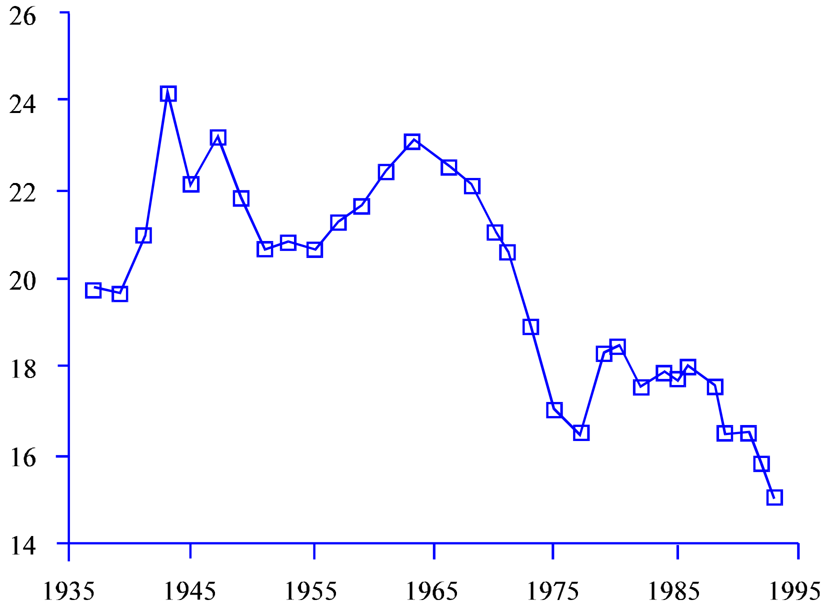


Source: Compton and Coward (1989), page 28.

Compton and Coward concluded that close correspondence between the rates pointed to changes in annual birth rates having arisen due to real fluctuations in marital fertility.

Birth rates in Northern Ireland tracked over the 1935 to 1993 period have shown a cohort oscillation trend approximately one generation apart up to the late 1970s.

Figure 3 Northern Ireland Birth Rate, 1935-1993



Source: Compton (1995), page 47.

The small birth cohorts of the 1930s went on to produce small birth cohorts in the 1950s which then again repeated the trend in the 1970s. Conversely the large birth cohorts of the mid 1940s themselves went on to produce large birth cohorts twenty years later. These oscillations indicate that the demographic experience of one generation was echoed in the next (Compton, 1995). More recently this trend has broken down with a notable decline in the amplitude of the short term recovery in birth rates during the late 1970s, relative stability in the 1980s followed by the recent continuous decline in birth rates.

In writing about Northern Irish changes in fertility patterns Compton concluded:

> *"Women in Northern Ireland generally plan their families, sometime postponing their births or bringing them forward in time depending upon personal circumstances and the general socio-economic climate within the Province. .... Average family size, therefore, tends to change slowly over time and conveys an impression of greater continuity when set against the much shorter fluctuations in annual birth rates. .... Needless to say the two approaches to fertility analysis* (cohort and period) *complement each other."* (Compton, 1995, page 62).

The arguments presented by the author propose improvements to the analysis of fertility via extension to the detail held in registration records (period measures).

Improvements to the birth registration system, and the methods whereby the extension of data could be utilised are to be welcomed. Influence of past history can be incorporated as a period measure such as in the use of birth parity for example. As other influences beyond parity are included, such as birth interval, marital status and age, the matrices become large. These demographic influences are not the only ones significant to the study of fertility - social class, religion and their interaction are also important. Incorporation of all of these factors into aggregate period matrices presents a serious statistical problem. Moreover, the age period interaction term is in itself comprising a cohort influence that may also interact with other factors.

Statistical data sources exist in Northern Ireland which may aid this research. The censuses of population for Northern Ireland in 1971 and 1991 recorded information on marital births. In 1971 the dates of marriage and births were recorded and hence birth parity and parity intervals could be calculated. Northern Ireland was the only region in the UK to record birth data as part of the 1991 census of population - though only the quantum of births was recorded. Detailed cross-tabulations of the fertility trends from the 1971 census have been archived and may assist the development of the period based methodology advocated here.

Annual birth statistics by parity have been available for some time and in conjunction with additional interval measures may well provide greater sensitivity to short-term shifts in births and changes in fertility. Research developing the methodology is formative and so it is difficult to assess the utility of the new approach. Problems persist in the estimation of denominators. Additionally, a balance needs to be set against the importance of nuptiality and the rapid growth in the numbers of births outside of marriage - most specifically in the context of births of first child. Short-term shifts in such births can have serious implications in the context of future population projections.

While acknowledging the value of the author's research aimed at advancing the period approach to fertility there remain concerns about the comprehensive criticism of the cohort approach. Neither cohort or traditional period approaches to projecting birth rates are able to fully anticipate short-term shifts in the birth rate. Recent projections 30 years into the future utilising age specific period birth rates for Northern Ireland require downward adjustment.

The criticism of cohort measures sets two approaches to fertility in competition when they are more normally considered complementary. Cohort approaches continue to be important and, as yet, little would appear to have emerged by way of new methodologies to challenge Woods' observation that, "*It is of fundamental importance in any population analysis that as many perspectives as possible are sought and in achieving this goal cohort studies of fertility are crucial.*" (Woods, 1978, page 127). In setting the two approaches to fertility analysis against one another the author appears to be knocking down a straw-man.

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Brendan Walsh: The paper we have heard contains a very thorough discussion of an interesting topic. The author has set out in some detail the case for the use of period, as opposed to cohort or generation, measures of fertility. She is generally critical of what she calls the "recent orthodoxy" to the effect that, for many purposes, cohort measures are superior to period ones and proposed the use of more refined period measures, such as parity-duration specific rates, as a better guide to trends in fertility.

An initial reaction to the paper is to wonder whether we need to choose between the alternative measures. Both contain interesting and valuable information and, to the extent that they can readily be calculated, why should not both be presented, as well as, indeed, the additional measures proposed by the author. I cannot agree with the assertion that "we need to choose one or other of two accounts as a dependent variable to work with". Clearly, both are useful descriptive measures and it would be foolish to ignore either. I also see no reason why we should not try to explain both measures - it seems reasonable to invoke short run, economic factors more in accounting for the wider fluctuations in the TFR, whilst longer run forces, economic as well non-economic, might be expected to play a larger role in accounting for fluctuations in the cohort measure.

In the third section of her paper, the author lists eight propositions which she says have been accepted as orthodox. She agrees with the proposition that "policy measures lead to an interest in period [fertility]". She also accepts that "period measures fluctuate more than cohort measures" - which is indeed obvious from a casual inspection of the two examples, for France and Sweden, that she uses as illustrations in Figure 1. But she queries the suggestion that "changes in cohort timing are a prior, more fundamental, cause of this volatility". On this point she offers some evidence in the concluding sections of her paper, but the issue is clearly one of emphasis and interpretation.

The other six propositions she associates with the current orthodoxy all relate to whether period or cohort measures are more "satisfactory", "real", "true", "interesting, etc. In reading this debate I felt there is a danger of becoming embroiled in arguments of more emotional than substantive content. It would be a good discipline if the proponents of the alternative sides in this dispute were to specify tests by which their positions could be either supported or refuted. In the absence of such tests, why not examine carefully the information about fertility behaviour being furnished through both measures?

However, forced to choose which is the "better" measure of fertility in some deep sense, I suppose I must declare that I am not convinced by the author's criticisms of the cohort measure. It seems pretty clear that there is a lot of noise in the TFR, and a much higher signal to noise ratio in the data on completed fertility. How much weight, for example, should we attach to the precipitous drop in the French TFR during the First World War, which was followed by a sharp recovery in the immediate post-war years? In the same vein, surely Hajnal did advance knowledge by switching attention away to calculations of the NRR based on synthetic cohorts and focussing on actual cohorts. An interesting, and important, contemporary illustration of the strengths and weaknesses of the two measures is provided by the Swedish data. The pronounced rise in the TFR since 1985 is yet to be reflected in the cohort measure - and I am willing to bet that most of the behaviour in the TFR will eventually be seen as due to timing. The author shows (Figure 4) that we simply cannot decide this issue until all the evidence is in, but she is also correct to point out that more detailed data on parity progression would shed more light on the matter.

The author claims that “period and cohort [indices] do not tell the same story about fertility change through time. . .” except that, “in a rough and ready way, there is similarity between the two types of series - each rises and then falls”. I think this understates the extent to which the two series might be regarded by a time series expert as being generated by similar underlying processes. Much of the contrast to which the author draws attention could be eliminated by a simple process of moving averaging. It is clear that a moving average of the French and Swedish TFRs would track the corresponding cohort fertility rates quite closely.

A distinct, but related, point is raised as to whether a *cohort effect* can be discerned in fertility data. This effect is defined

> *as a tendency for a generation to display birth rates that differ systematically from those of other generations, in a way that could not be accounted for more parsimoniously by means of age and period effects. . .*

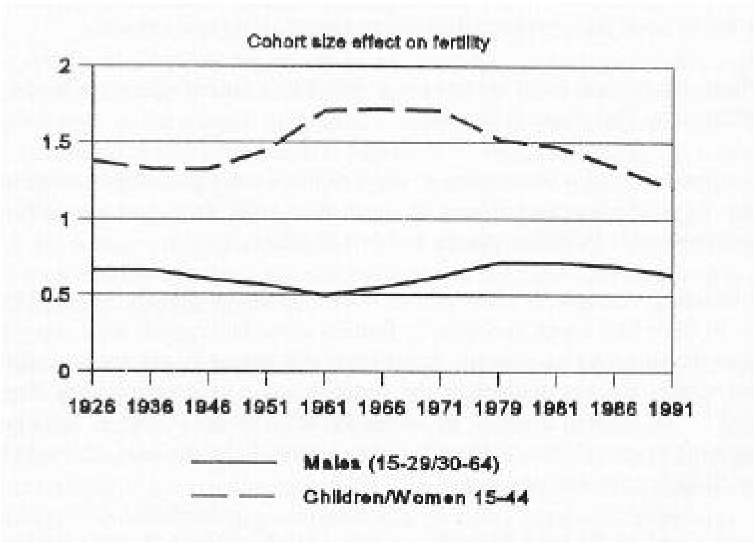
An interesting example is cited of the women born in Singapore in 1937, who appear to have had lower age-specific fertility rates throughout their reproductive age than the surrounding cohorts. Apart from this example, the author claims that “cohort effects are not evident in the fertility series of contemporary developed societies” - in marked contrast to mortality data, where “there is equally clear evidence of pervasive cohort effects”. Granting this to be the case, it would hardly lead us to ignore the cohort series.

As emphasised by Richard Easterlin, one aspect of a cohort that is potentially an important influence on marriage patterns and hence, indirectly, on fertility is the size of the cohort itself. He posits three channels through which higher birth rates may adversely affect a cohort’s economic and social fortunes, namely, through the crowding mechanisms operasting within the family, the school and the labour market. Of relevance to the present paper is Easterlin’s claim that

> *.... the 1940s and 1950s saw the emergence of relatively favourable conditions in the labour market for young adults, a shift towards accelerated family formation (the “baby boom”) . . .Then in the 1960s and 1970s as the size of the young adult population soared upward relative to the older, younger workers’ realtive*

*wage rates, unemployment rates, and upward job mobility deteriorated. Marriage was deferred and childbearing reduced...**

It is of great interest to ask whether a similar effect could be established for Ireland. We have to bear in mind, however, that the small, open nature of our labour market means that the the ratio of the numbers aged 15-29 to those aged 30-64 in the population is endogenous, reflecting the effect of emigration - which in turn reflects economic opportunities relative to those abroad - rather than simply the size of different birth cohorts. None the less, the Chart below is intriguing, suggesting that there has been some correlation between the relative size of a birth cohort and its subsequent fertility. The measure of fertility used here is rough and ready - extracted quickly from the Census - and I have no doubt that if we had a more refined measure of *cohort* fertility, and if we extracted a negative secular trend from the series, the Easterlin effect would shine through.



The Irish children born today have only two thirds as many coevals as those born in 1980. This will affect their educational chances from primary school to third level, as well as their employment prospects when they leave the educational system. It is difficult to believe that the sharp downturn in the marriage rate, and in the number of births that began in 1980 was not related to the rise in youth unemployment (both absolutely and relative to total unemployment) in that period. While this was mainly due to broad economic trends, it is not unreasonable to look to the peak in the TFR some 16 years earlier. The young people coming on the labour and marriage markets throughout the 1980s belonged to a crowded cohort, especially compared with that which reached maturity in 1960s. These types of echo effects of demographic fluctuations have been studied in depth by Easterlin in the United States, who argues that the depleted cohort of Depression babies enjoyed very favourable labour market conditions in the immediate post-war economy, which encouraged them to marry early and have large families, but when this "baby boom" generation in turn reached school-leaving age in the 1960s, the relative labour market situation deteriorated and marriage and fertility rates plummeted. In this model cohort effects are cited as very important influences on the long run swings in fertility. I believe that until we have explored his approach in depth on Irish and European data it would be premature to conclude that there are no pure cohort effects on fertility.

Finally, French demographers were for a long time fascinated with the effects of the extraordinary fluctuations in the birth rate during the First World War on marriage and hence, indirectly at least, on family formation patterns. The dearth of births during 1914-18 made it difficult for men born in 1910-14 to find wives younger than them by the "normal" gap in age between groom and bride. The consequences were a narrowing of the age difference gap between grooms and brides, a high marriage rate among the women born during the war, and a lower marriage rate among the relatively large number of women born after the war. While these changes were not momentous, they are interesting examples of how membership of a cohort can affect demographic behaviour.

* RichardA. Easterlin, "Easterlin hypothesis", in *The New Palgrave: A Dictionary of Economics*, ed. by John Eatwell, Murray Milgate and Peter Newman, London, 1987, Vol. 2, p3. (Italics added)

Reply by Dr. Ní Bhrolcháin: I would like to thank the two discussants of my paper for their detailed comments. There are few issues on which either of them is in agreement with the view I present. While I regret having made such a poor job of convincing them, I am happy to take this second opportunity to clarify my approach and arguments.

Response to Mr John Power's comments

1. *Empirical results are awaited*

I acknowledge that "the proof of the pudding is in the eating". Several authors, including myself, have published estimates of period parity progression ratios and analogous fertility measures - for England and Wales, France, Italy, Sweden, China and Japan - and there appears to be general agreement that these indicators are a decided advance on previous accounts of time-trends. But the work I propose on disaggregating the rates and looking for summaries of them, is still in progress. Thus far my arguments in favour of parity-progression type period measures as an alternative to cohort indices are arguments in principle. I would like to think that future work with indices of this kind will, ultimately, result in concrete progress in both measurement and explanation. While I think the arguments in principle unassailable, they are more likely to be accepted as such when they have been shown to be useful in practice, especially when it comes to *explaining* trends. Work is in progress on an attempt to do just that.

2. *Further measures*

I do not see the point of the measures presented in Power's Figure 2. Yes, they show that change in fertility over the period was not due simply to marriage trends, but that can be shown more effectively in other ways - e.g. via parity progression type measures.

3. *Cyclical fluctuations*

Power misrepresents the idea of countercyclical cohort fluctuations in fertility. This idea relates not to a positive relation between the absolute size of a given cohort and the absolute size of the cohorts they produce but to an inverse relation between the relative size of a cohort and its fertility *rates*.

4. *Period and cohort complement each other*

This statement is common enough among those who defend the cohort analysis of fertility but it is utterly unspecific. What precise information in cohort form is needed that cannot be obtained from period indicators at the level of detail I propose, and what purpose does it fulfil?

Response to Professor Brendan Walsh's comments

5. "Both [period and cohort] contain interesting and valuable information".

This statement is only partially true. The time-series of period and cohort fertility in virtually all empirical situations look different. The cohort series appears thus to add to the information present in the period series. However, the apparent information given by cohort indices can be obtained quite adequately as a function of period indices: e.g. a moving average of period indicators produces all the information we need about long-run as distinct from short-run trends in fertility. This has been shown to be so by the statistical analyses of Brass, Page, Foster, Isaac et al, Namboodiri, Pullum, Rindfuss et al and Smith, referred to in section 4 of my paper. On the principle of scientific parsimony, the cohort representation is unnecessary to demographic description because all the information it appears to contain is already available from period series. If there is need of e.g. a total fertility figure that is relatively "free" of short-term fluctuations, it can be obtained by averaging over period figures. Information on timing change can be obtained from checking for interactions between period and age in a series of age-specific rates.

6. "Clearly, both are useful descriptive measures and it would be foolish to ignore either".

Naturally enough, I disagree. If this were the case, then the arguments in my paper would be foolish since I propose that we can indeed, with no loss, ignore the cohort perspective on fertility, if we have period information at a sufficient level of detail. I considered the four main justifications that are normally advanced for the cohort approach and argued that they are mistaken; two more minor arguments are considered and rejected in an earlier paper (Ní Bhrolcháin 1992). As Walsh does not specify what aspect of my detailed arguments on these six points he finds unconvincing and does not offer any other justification for cohort measures except that they are "clearly...useful", I can only ask for further information on *how* he considers them useful: i.e. what for? At a guess, Walsh is attracted by the apparent role of the cohort as an indicator of long-run trends. But, as argued further below, we do not need cohorts to assess long-run trends.

7. *Arguments of more emotive than substantive content*

Up to a point, I agree with Walsh that the discourse of comparisons of the pros and cons of cohort vs period measures is often, to use a word less emotive than "emotional", evaluative. But, to me, it is the pro-cohort arguments that are presented in a "cohort is better/truer/more reliable" wrapping. The word "discourse" in English is imprecise and maddeningly suggestive of the vague and even vacuous discussions characteristic of the various intellectual relativisms of our day. But the concept is, I think, relevant in the present context. What I have tried to do is to look beneath the evaluative surface of the arguments for the cohort in order to identify and assess their substantive content. My assessment is that, in each case, the substantive part of the argument lacks justification.

8. *Specifying critical tests*

I agree with Walsh that the ultimate question at issue is an empirical one. The issue has been repeatedly put to the test in an empirical sense (Brass, Foster, Isaac et al, Page, Pullum, Smith: citations in the paper above). The statistical evidence from age-period-cohort and time-series analyses overwhelmingly supports the period approach. What is odd is that adherence to the cohort approach nevertheless lives on, though not a single proponent of the cohort idea has ever published a statistical analysis giving evidence of cohort effects. It can be argued that the published statistical analyses address the hypothesis of cohort "main effects". This is indeed so. "But", the cohort argument goes, "there are other kinds of cohort effect". In principle, many varieties of "cohort effect" could be imagined and specified. But, with the exception of Easterlin, discussed below, advocates of the cohort approach rarely if ever take the trouble to specify cohort effects other than the classic main effect. The principal alternative type of cohort effect implicit in many discussions is the idea of "postponement". With the exception of Hajnal, nobody has yet specified clearly what these are, much less produced empirical evidence of them.

9. *"How much weight... should we attach to the precipitous drop in the French TFR during the First World War.....?"*

I am not too sure what is meant by this question. How much weight for what? The TFR declined precipitously during the First World War in France, and indeed elsewhere too. It also rose sharply after the war. On this much, we no doubt agree. Why then the question "how much weight..."? My guess is that what Walsh means is that it would be wrong to infer from a brief decline in the period TFR that there would be a long-term decline in the period TFR. If this is the origin of his question, I agree, but with some puzzlement. Surely it goes without saying in any time-series that short-term fluctuations can be, and often are, superimposed on long-run trends? Time-series of period fertility indicators can be analysed to show both long-run and short-run features. Walsh appears to assume that a cohort measure is necessary to depict the long run. But long-run fertility change can be assessed perfectly adequately from the period series. What is it about fertility that leads Walsh, and other proponents of the cohort, if I read them correctly, to abandon the style of thinking they would adopt in relation to any workaday economic time-series? Why do we need cohorts to measure long-run trends in fertility, when no such analogous concept or measurement framework is needed to identify long run-trends in e.g. prices, rainfall, temperature, agricultural production and so on?

10. *"Much of the contrast to which the author draws attention could be eliminated by a simple process of moving averaging"*.

I would agree with this statement. A time-series behaving almost exactly like the cohort time-series can be produced from a moving average of the period time-series. That being so, what need is there for the cohort, since it does not provide us with additional information?

11. *"...I have no doubt that if we had ... a more refined measure of cohort fertility, ... and if we extracted a negative secular trend from the series, the Easterlin effect would shine through"*.

And I have no doubt that the Easterlin effect would be absent! I am confident in this assertion since the Easterlin effect has been disconfirmed time after time in empirical statistical investigations. What is odd is how the elegance of Easterlin's idea continues to attract adherents long after it has been shown not to fit the facts. Everyone would like the Easterlin idea to be true, since it is such a nice idea. But it is not true, and it is long since time that we moved on. Nevertheless, I await with interest empirical evidence of its operation in Ireland.

12. *Cohort size and the marriage market*

Cohort size appears indeed to be associated with some aspects of the marriage market, but nobody has yet been able to show that the absolute or relative numbers of men and women available for marriage influences the marriage rate. The "marriage squeeze" is another of those nice demographic ideas that has been able to survive empirical disconfirmation. The role of cohort effects in the marriage market is, in any case, an entirely separate issue from the issue of cohorts in fertility analysis.

13. *Hajnal*

I agree wholeheartedly that Hajnal made an enormous contribution to fertility analysis in drawing attention to the volatility of period measures and clarifying the misinterpretations of reproduction rates current in the 1940s. His analysis of the problems of tracking and interpreting fertility trends was wonderfully acute. I admire enormously his diagnosis of the difficulties associated with measuring fertility, and have emphasised both that it was he who first suggested the use of cohort measures as a potential solution to the difficulty of volatile period rates and that it was he who originally proposed parity-specific measures as a way of removing the effects of "past history" from period rates. But I disagree with the role given by Hajnal to cohort fertility.

[1]An age-specific fertility rate is the number of births per 1000 women of a given age in a particular year. The age-specific fertility rate, $f_{x,t}$, of women aged *x* in year *t* is, thus: 1000 * $b_{x,t}/w_{x,t}$ where $b_{x,t}$ is the number of births occurring to women at age *x* in year *t* and $w_{x,t}$ is the total number of woman-years at risk at age *x* in year *t*, usually estimated by the number of women aged *x* at mid-year.

[2]The total fertility rate, or TFR, is obtained for any given calendar year by summing the fertility rates at each age of woman (conventionally, 15 to 44 or 15 to 49) in that year. In year *t* it is, thus, $\Sigma f_{x,t}$, i= 15, 49 where $f_{x,t}$ is as defined in footnote 1.The result of the summation is conventionally interpreted as the mean number of children a woman would have if she experienced through her lifetime the age-specific fertility rates prevailing in the calendar year in question.

[3]Cohort completed family size is defined as the mean number of children born to a birth generation or cohort of women. Other types of cohort can be specified also - e.g. marriage cohorts (people married in a given year) or parity cohorts (women having had a birth of a particular order in a given year). The issue surrounding period and cohort fertility has been discussed largely in relation to birth cohorts.

[4]For discussion of the TFR and other types of fertility index, see e.g. Pressat (1972) or Shryock and Siegel (1976).

[5]The terms "cohort total fertility", "cohort completed fertility" and "cohort mean family size" are used interchangeably to refer to the mean number of children born per woman or per 100 women of a given birth generation.

[6]One would, ideally, have wished to present time series of Irish period and cohort series in the present context. However, I have not been able to locate a series of cohort completed family size values that appear reliable. Such data appear not to have been published by CSO and while a series of this kind has appeared in Council of Europe sources, the behaviour of the cohort series leaves substantial room for doubt as to its reliability. The France/Sweden comparison here has been chosen because it is in many ways a celebrated one, having sparked off an intense controversy in French demographic circles in 1990 and after. The diagrams of Figure 1 are, in fact, updated versions of those that appeared originally in Calot (1990) and have been kindly made available by Jean-Paul Sardon of the Institut National d'Etudes Demographiques, Paris.

[7]Thus, the figure for e.g. the generation of 1920 is plotted against the calendar year 1920 + m_{1920} where m_{1920} is the mean age of childbearing among women born in 1920.

[8]While this statement is entirely true of the central part of the period covered, correspondence is not complete at the start and finish of the graphs. The earliest period figures here will be based in part on events (and rates) of some (older) women belonging to cohorts born earlier than the earliest depicted here; the most recent period figures will incorporate events (and rates) occurring to women belonging to cohorts born later than the latest cohort plotted in the graph.

[9]Low mortality populations will replace themselves in the long run if each woman has on average approximately 2.1 children and that this level of fertility is maintained over the long term. The additional 0.1 of a child over what might be thought of as a more intuitively obvious replacement level of 2 children per woman (one child to replace each of the two partners) is to allow for mortality: not every child born will survive to reproductive age. In higher mortality societies the replacement level will be greater than 2.1 since many more children will die before reaching the age at which they themselves would reproduce.

[10]It can of course be argued that this comparison is "unfair", in some sense, since the cohorts currently having children have, as yet, not completed their families. In 1996 e.g. we can observe the TFR for 1995, subject only to delays in compiling the statistics. But in 1996, the most recent completed cohort figure available is that for women born in 1950, who reached their 45th birthday in 1995. By far the largest part of the childbearing of these women will have taken place between around 1970-1985, when they were aged 20-35. Clearly the most recent completed cohort figure reflects, in the main, fertility events that occurred some time in the past. We could, of course, examine the fertility of a cohort of women who are in their prime childbearing years - say those who reached 30 in 1995; but these women have at least a decade's further exposure to childbearing before them. Thus, we see one of the key deficiencies in cohort statistics. If complete, they are not "current"; if "current", they are incomplete and can only be completed by making assumptions about future fertility.

[11]Demographic opinion on the subject is, however, by no means uniform. Informal reaction to an earlier account of the views presented in this paper (Ní Bhrolcháin, 1992) has ranged from surprise at the apparently cavalier rejection of the cohort, well known as a corner-stone of demographic methodology etc., to claims by others never to have had any use for cohort indices. What I describe as "demographic orthodoxy" here is the large swathe of opinion between these extremes, influenced particularly, as will be seen presently, by Norman Ryder's advocacy for the cohort.

[12]Statistically, a "cohort effect" is a tendency for members of a birth generation to display event rates (in this case, birth rates) systematically different from those of other birth generations, in a way that cannot be accounted for more parsimoniously by means of age and period effects. The existence of cohort effects became a key

issue in the 1970s both because of American demographer Norman Ryder's concerted advocacy of cohort distinctiveness and because of the interest in Richard Easterlin's theoretical ideas on relative cohort size and its hypothesised cyclical effects.

[13]The identification problem is a statistical difficulty which arises when attempting to estimate the coefficients of a regression model in which a dependent variable is expressed as a function of a number of predictor variables, one or more of which can be expressed exactly in terms of two or more of the others (that is, that there is linear dependence among a set of the predictors or any subset of them). This occurs when demographic rates are modelled as a function of age (a), period (p) and cohort (c). The year of a person's birth (c) plus their age at a specified time (a) equals the period (p) in which observation is made. In general, fixing any two of these three variables fixes the third: c=p-a, a=p-c and p=c+a. As a result, if a regression includes all three factors, their parameters cannot be determined uniquely. There are an infinite number of solutions to the equation, and the solutions possible may carry quite different substantive implications. Holford (1991) gives an excellent account of this issue in an epidemiological context.

[14]Nevertheless, the absence of cohort effects has not been unmarked at demography's "coal-face", in the practical world of population projections. A recent study by Nico Keilman and colleagues indicates that few national statistical agencies in industrialised countries currently use a cohort model to project fertility.

[15]The Gross Reproduction Rate (GRR) is a period measure constructed along identical lines to the Total Fertility Rate (see footnote 2) except that it is confined to female births. It is equal to: $\Sigma f^{f}_{x,t}$ for x = 15, 49 and $f^{f}_{x,t}$ is the age specific rate of female births to women aged *x* in year *t*. The Net Reproduction Rate (NRR) is similar to the Gross Reproduction Rate but makes an allowance for mortality. The NRR is $\Sigma l_x f^{f}_{x,t}$ where l_x is the life-table probability (in a life table of radix 1) of a female child's surviving from birth to age *x*. Each of these rates is an attempt to quantify the extent to which a population is replacing itself at current rates, with the NRR being the more refined of the two, and more generally used, since it corrects for losses through deaths of female children before they reach reproductive age, specified in the calculation as the age at which their mother bore them.

[16]Ryder (1980) is probably the most complete and up to date account of Ryder's views on the period v cohort issue in relation to fertility. Ryder (1965) was an attempt to provide a theoretical sociological basis to Ryder's arguments in favour of the cohort perspective in general in human affairs and has been an influential source on this subject among sociologists.

[17]*Reproductivity* is a term used to refer to population replacement. The term can be a source of confusion since it tends to be seen as synonymous with *reproduction* whereas, as will be discussed, this is not the case.

[18]In relation to human social behaviour, socio-biological arguments need, of course, to be used with care. The point being made here is simply that it seems biologically plausible to suppose that human reproductive behaviour is flexible and responsive to external circumstances.

[19]Except, of course, in cases where there are exceptional increases or decreases in the number of births, resulting in a substantial bulge or indent in the population pyramid. In such cases, the implications are for population age structure rather than for population size *per se*.

[20]Migration can be, nevertheless, of great importance in the dynamics of national populations that are small and of local populations.

[21]The terms *hypothetical or synthetic cohort* are used interchangeably to refer to an imaginary group who experience throughout their lifetime the rates current in a particular year or period. The expectation of life derived from a period life-table is probably the best known index constructed on the synthetic cohort principle.

[22]The "parity" of a woman is the number of live children she has had; thus women who have had no children are described as being of parity 0, those who have had one child as being of parity 1 etc. The corresponding term used in relation to the births is the "order" of a birth: a first birth is described as of order 1, a second birth as of order 2 etc.

[23]An interesting possible exception to this "rule" was presented recently by Street (1995) in an account of fertility trends in Singapore. It appears that the cohort of women born in 1937 have, throughout their childbearing years, always shown a dip in their age-specific rates relative to surrounding cohorts. This is interesting because there are suggestions that a biological cause may be at work. Street hypothesises that the lowered fertility of children born in 1937 may be linked to a synthetic anti-malarial treatment and prophylactic that was used during a major malaria outbreak that occurred in Singapore in 1935. Since we know that biological factors are the root cause of cohort effects observed in relation to mortality, the discovery of a cohort fertility effect due to possible biological causes suggests that it may be only biological, rather than economic, social or cultural, mechanisms that are capable of generating such effects in relation to fertility behaviour.

[24]The issues are discussed in greater detail, along with some subsidiary elements of the case for the cohort approach to fertility, in Ní Bhrolcháin (1992).

[25]That choice is exercised in contracepting populations in relation both to number and timing of births is almost certain, though this does not mean that couples behave according to "rational choice" principles nor that active decision-making occurs in relation to each and every birth, nor that the precise outcome of childbearing decisions is actively chosen.

[26]Whether "postponement" is a reasonable concept in the fertility context has been a matter of debate. It may be that all that can be done is to decide against a birth at a particular time, with the expectation that it will occur later. A "postponed" birth may, in the event, never take place. The couple's situation may alter, or hoped-for changes in circumstances may not occur. The couple may change their minds about wanting a (further) child, they may become infertile, the union may founder, a partner may die, they may have difficulty in establishing their own household, and so on.

[27]Two or three decades ago it was conventional to refer in this context to the level of and age at marriage. With the increasing prevalence of cohabitational unions, "union formation" has become the more generally used term to refer, inclusively, to the start of formal (i.e. marital) or informal cohabitation.

[28]In analysing first births, marriage or the start of cohabitation may also be ignored and the birth rates considered simply by reference to age, or equivalently years since 15th birthday.

[29]In practice the denominator would be an estimate of "woman years during t" during which all of these attributes held, since individual women can, of course, change their state in respect of any or all of these factors during year t: i.e. they may have a birthday and so reach age $x+1$ and/or they may have a birth of order i during year t and so reach parity i and/or the duration since their $i-1$th birth may reach $d+1$ completed years.

[30]The appropriateness of age as a natural control in relation to fertility rates is, however, beginning to be questioned in relation to low-fertility, contracepting populations.

[31]The parity progression approach can be used to generate an alternative version of the "total fertility rate", sometimes styled the parity progression TFR. It is obtained by estimating the mean family size that would result from the series of period parity progression ratios implied by the parity- and age- and/or duration-specific rates of a given year. In general, the parity progression TFR differs from the conventional TFR based on summing the age-specific rates.

[32]The parity progression ratio of order i is the proportion of women who have had i births who go on to have at least one more birth. Annual or period estimates of a

series of parity progression ratios for each order of birth, calculated on a synthetic cohort basis, are the normal way in which rates characteristic of the period parity progression approach are summarised.

[33]This is an ESRC (UK) funded project which aims both to generate time series of annual parity-, duration- and age-specific rates for England and Wales 1941-1991, using the OPCS Longitudinal Study, and to explore ways of summarising the disaggregated rates. The project will be reporting results between now and 1997-8.

[34]Most developed countries collect information on the order of each registered birth, but the definition of “order” does not often include children born outside of marriage and is sometimes confined to the present marriage. The order of birth classified in the current Irish registration system is the true order of birth within the mother’s complete childbearing history.

[35]Note that discussion here has slipped into conventional terms. Sceptical comment in an earlier section on the notion of “replacement level fertility” and its evaluation on annual figures needs to be borne in mind.

[36]The figures are at 5-year intervals (1960, 1965 etc.) to 1980 and annual from 1983. Note that the line for 4th+ is the sum of all births of order four and above and so is well above the other lines in the earlier part of the series.

[37]This is a simplification since it is unlikely that births deferred from a particular year would all occur during any one subsequent year. It is more probable that the shortfall would, if it were compensated for in later years, be made up over several years rather than within the confines of a single calendar year. Also, this simplifies matters because of the difficulties with the concept of “postponement” discussed in footnote 23 above.

[38]Whether this is truly so will depend, however, on the mechanism giving rise to the dip and recovery observed, and subsequent observations in the time series might be informative for this purpose. For example, if what happens is that in t5 and for a further, say, ten years there is an economic boom and that birth rates go on rising over that period, the apparent *t4-t6* link might be entirely spurious.

[39]It could be argued further that we cannot defensibly claim to have identified a “postponement” effect until we have made a convincing link between the fertility rates in question and the socio-economic conditions that determine such short-term movements. This point is not generally acknowledged in the demographic literature.

[40]The situation is a little more complicated than this in that Le Bras’s point relates to a decline in the fertility of a particular age group, say aged *a* in year *t*, followed by

a recovery and rise in the fertility of age group $a+x$ in year $t+x$: i.e. compensating movements occurring within a birth cohort. The argument of the text applies equally to this situation. One cannot know until after the fact that the fertility of a particular generation of women will rise to compensate for a current shortfall. Furthermore, if "compensation" of this kind always occurred, cohort total fertility would be invariant, which is clearly not the case.

[41]That a longitudinal study need not be a cohort study is clear from such instances as the American Panel Study of Income Dynamics, and similar household panel studies recently established in several European countries.