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Maire Ni Bhrolchain

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Period Paramount? A Critique of the Cohort Approach to Fertility

MÁIRE NÍ BHROLCHÁIN

CONFUSION LIES AT THE HEART of discussions of the cohort and period approaches to fertility. The confusion has several roots and goes back over 40 years to the earliest discussions of the subject. This article aims to clarify the issue and to make progress toward resolving the apparent conflict between the two approaches. The question is more complex than it appears.

In brief, the conventional view of the period versus the cohort approach to fertility runs somewhat as outlined in the following paragraph:

Traditional measures of period fertility, such as the total fertility rate, fluctuate more than do comparable cohort indices. This is because changes in the timing of cohort fertility distort period measures of the quantum, or level, of fertility. When the pace of cohort fertility is accelerating, conventional period measures of fertility quantum will be excessively high and when cohort tempo is decelerating, period indicators will be too low, relative to the corresponding measures for the cohorts affected. This is seen repeatedly in time-series of period and cohort fertility indices. Period measures of fertility are therefore unsatisfactory and must be treated in one of two ways: either they should be deployed with extreme caution or they should be adjusted or transformed so as to approximate more closely to the true, underlying level of fertility observable in the cohort mode. In formal terms, period and cohort measures need to be "translated" from one to the other. A particular practical need is to remove the "tempo" component from period indicators because this inflates the implied level of fertility. Cohort fertility is a more fundamental or more "real" and period fertility a more transient and "unreal" phenomenon. The trends depicted by period indices give a misleading account of fertility change through time. The true time path of fertility is to be observed in inter-cohort change. Cohort fertility is ultimately what we are trying to estimate and period fertility is but an imperfect guide to it. We are interested in period fertility largely because cohort fertility indices have the disadvantage of being out of date, since they require a cohort's childbearing to be complete. For this reason we sometimes use period fertility measures, reluctantly, to get some indication of current levels and trends. Period fertility measures are a useful way of establishing a population's current reproductive performance. Interest in the period perspective is also driven by policy concerns.

Though conventional now, this set of ideas has not always been the prevailing demographic orthodoxy. Its currency owes much to Norman Ryder's championing of the cohort approach, but the earliest clearcut and comprehensive discussion of the issues was advanced by Hajnal (1947).

Statistical evidence

The main reason for questioning the validity of the cohort approach is the recurrent finding in empirical statistical investigations using age-period-cohort type models that of the two dimensions of calendar time—period and cohort—period is unambiguously the prime source of variation in fertility rates. A series of inquiries,[1] using a variety of approaches and a range of data, have concluded that, in Brass's words, "cohort completed family sizes reveal no significant feature which distinguishes them from time averages" (Brass, 1974: 558). There is more consistency between age-specific fertility rates in the period than in the cohort domain. Stated otherwise, there is little evidence of consistency in the behavior of particular cohorts at different time periods. Page (1977: 104) concludes that "at any given time, all birth and marriage cohorts react, in some sense, as a single unit to whatever factors determine the general level of fertility at that time." Foster's study, detailed and wide-ranging in coverage, analyzes fertility time-series in eight developed countries. He finds no more than "a couple of isolated bits of evidence that would suggest that cohort indices of quantum and tempo are anything other than averages of period indices" (Foster, 1990: 309), a conclusion echoing that of Brass nearly two decades before. There are fewer studies of nuptiality, but one—by Rodgers and Thornton (1985)—finds a clear period rather than cohort patterning to marriage rates. The absence of distinctive cohort-based fertility patterns is in marked contrast to the clear presence of cohort mortality regularities in many contexts. Some of the relevant studies of mortality are discussed in Hobcraft, Menken, and Preston's (1982) review of the age/period/cohort issue in demography.[2] More recently, evidence of cohort mortality effects has been presented by Mason and Smith (1985), Caselli and Capocaccia (1989), Caselli (1990), and Wilmoth (1990). That clearly observable cohort effects have been uncovered in relation to mortality should perhaps alert us to the likelihood that their absence in fertility time-series is a decidedly negative result.[3]

Why does the cohort idea continue to flourish?

Despite the weight of statistical evidence favoring the period perspective, demographers continue to have recourse to the cohort mode in analyzing fertility. In other words, a tension appears to exist between the statistical primacy of periods and the day-to-day demographic utility of cohorts. Why is this? One answer is that the cohort approach has offered dividends from an interpretive standpoint. It is, for example, indisputable that period indicators overstate movements in fertility, evaluated on a cohort basis. This feature is perhaps the most persuasive argument for the cohort approach. In addition, the approach has brought considerable practical benefits. The pioneering analyses of Hajnal, Ryder, and Whelpton remain illuminating. Time and again, the cohort perspective has complemented the period representation. Another contributory factor may be that the arguments for the cohort have been forcefully put, principally by Ryder, and, demographically if not statistically, they remain unchallenged.[4] This article investigates the resilience of the cohort idea in the face of the comprehensively negative statistical evidence of cohort effects. Are there perhaps fundamental demographic arguments in favor of the cohort approach that override the verdict of statistical studies? To assess the demographic role of the cohort perspective, the next section attempts to identify the components of current thinking on the demography of the period/cohort issue.[5] These are sought in the historical roots of the subject and are examined individually in later sections.

Historical background

The first investigator to consider the matter in any detail was John Hajnal.[6] In his 1947 article he addressed some methodological questions about fertility measurement arising from the sharp fluctuations in period rates that had occurred in most developed, low-fertility countries in the 1930s and early 1940s. He argued that reproduction rates, commonly employed at the time to summarize the trend in period fertility, presented a misleading picture of fertility trends. Reproduction rates suggested very large changes in fertility within relatively short time periods, an account not greatly modified when the rates were standardized for marital status and duration of marriage.

Hajnal was dissatisfied with these results, arguing that a "reasoned discussion of population trends" could not be based on "irregular fluctuations in fertility rates" but rather assumes "a reasonably orderly and smooth development of fertility rates" (ibid., p. 141). He went on to show that "by analysing the fertility statistics of any given year in relation to those of past years, it is possible to appraise more correctly the significance of changes in fertility rates" (ibid.). He argued that where use of contraception is widespread in a population, the fertility of couples will be strongly influenced by the number of children they already have. Furthermore, couples with access to contraception may alter the timing of their births and so "a change in the rate at which people are having children in a given year [could] no longer be taken as an indication of a change in the number of children they [would] bear altogether in the course of their reproductive lives" (ibid., p. 143). This meant that contemporary methods of demographic analysis were defective. It also meant, argued Hajnal, that the analysis of the rate of childbearing in a given year was not "the most important task of demography." On the contrary, "from the point of view of the ultimate prospects of population growth, what is of importance is the size of completed family which married couples ultimately have," and this "cannot be deduced from any analysis of fertility data relating to single years or periods of years" (ibid., pp. 143–144).

Hajnal went on to show that in Germany the cumulative number of births to marriages of each duration changed only very slowly between 1933 and 1939, despite the sharp year-to-year fluctuations in fertility rates. These fluctuations, he suggested, were due to the "postponement" and subsequent "making up" of postponed births.[7] Reviewing the implications for demographic analysis, Hajnal concluded that attention should be directed in the future to the number of children born over their lifetimes to marriage or birth cohorts, and that annual fertility rates should be specific by order/parity and cohort.[8] Hajnal closed his discussion with a critique of gross and net reproduction rates. He argued that no indicator based on a single year's rates is suitable for assessing long-term growth prospects. Because any individual year's rates reflect what are necessarily temporary conditions, they cannot be maintained indefinitely, and so the hypothesis implicit in the use of a net reproduction rate (NRR) above or below 1 to reflect long-term population growth or decline is unrealistic. It is futile, he suggested, to ask "To what extent is the population replacing itself according to the rates of *this* year?" (ibid., p. 162; italics in original). Replacement prospects, he believed, should be evaluated on the basis of cohort rather than period indicators.

Two central assumptions guided Hajnal's exposition. First he assumed that fertility trends evolve in a smooth and orderly way; an index that suggests otherwise must therefore be misleading. Second is the implicit assumption that the chief purpose of examining trends is to evaluate long-term growth prospects. These two assumptions are probably closely related. That is, if we are seeking from statistics on current or recent fertility an indication of a population's growth prospects, we will naturally expect our chosen indicator to change only gradually, because growth prospects can change only slowly.

Ryder developed these themes further, though his adherence to the cohort approach has been more emphatic than Hajnal's. Beginning with an early joint paper (Stolnitz and Ryder, 1949) and his Ph.D. dissertation of

1951 (published in 1980), Ryder conducted a concerted and effective campaign of advocacy for the cohort approach to fertility (see listings under References). His arguments for the cohort perspective are based on a complex web of associated facts and propositions. To evaluate their cogency we must disentangle the various strands and examine each individually. They appear to be as follows:

(a) Fertility rates are influenced by an individual's past reproductive history. A related set of ideas is that fertility is influenced by life-cycle stage and therefore, it is argued, by cohort factors.

(b) Cohorts are distinctive, socially and demographically, and their distinctiveness persists through time.

(c) Theories of fertility are formulated in a cohort framework.

(d) Time-series of cohort total fertility rates are smoother than the period equivalent.

(e) The cohort framework is the natural way to approach behavior, such as family formation, that takes place over time. A related claim is that cohorts are "primary" and take precedence over the period perspective.

(f) Changes in cohort fertility timing are manifested as changes in period quantum. Combined with the view that the cohort approach has primacy, this implies that period measures mislead with respect to fertility levels and trends. An associated claim is that although there are period influences on fertility, they are essentially transient and operate by shifting a cohort's births from one period to another.

Other related propositions, not in themselves arguments for a cohort approach, include the views that: (g) period fertility measures should be "isomorphic" with cohort measures—that is, period and cohort measures should be of the same type; (h) cohorts are the vehicles of causation and periods reflect the consequences of cohort behavior; and (i) cohorts operate primarily with a target family size and tempo from the start of childbearing and only deviate from this in response to strong period-based stimuli.

Taking issue with the cohort perspective

In examining components of the demographic case for the cohort approach to fertility, the question to be asked in each case is whether the argument is reasonable and, if so, whether it necessitates a cohort as opposed to a period perspective.[9]

Allowing for "past history"

The "past history" argument is that current fertility is in part dependent on previous fertility history. It is necessary, therefore, to ensure that a fertility indicator reflects the fertility of the year or period in question and not that of preceding years or periods. Age alone is insufficient as a standardizing factor for this purpose. This consideration applies to the analysis of both individual-level and aggregate (time-series) information. For Hajnal, the need to remove the effect of previous fertility history arose in a period context, and the requirement was met by use of parity-specific measures, in some cases in conjunction with a control for marriage duration. Ryder is not fully consistent about the significance of past history. In one passage, he claims that "the case for a cohort approach lies in the importance of a woman's history to the explanation of her current behavior" (1968: 549; 1975: 9). In making this idea concrete, Ryder at times comes close to acknowledging that parity-specific rates take complete account of past reproductive performance (e.g. 1959a: 408; 1975: 9),[10] while elsewhere he argues that specificity for cohort is needed as well. This is either because the individual stages of family formation, alone, lack the coherence conferred by a cohort framework (1965b: 296) or because cohorts have distinctive time-patterns of fertility, so that any set of rates which is not cohort-specific will be distorted by this fact (1956: 14; 1973: 499).[11]

There is no doubt that in order to arrive at sensible measures of fertility performance, in whatever mode, previous reproductive history needs to be taken into account. But to do so does not require a cohort approach.[12] Introducing specificity for parity and interval since previous birth (rather than marriage duration) is an excellent way of removing the effect of past history. The feasibility of specifying such measures in a period context has long been recognized, though data limitations have somewhat restricted their currency. If further aspects of previous history are needed beyond parity and interval since previous birth, they too can be incorporated, in principle, into period measures by increasing the level of specificity, subject only to data constraints. Nothing in the period approach per se precludes the incorporation of past history into its measurement methods. Indeed, it was precisely in the context of period measures that Hajnal suggested the need for a parity control. Ryder's claim that however specific a set of rates is, the derived measures will always be influenced by distinctive cohort timing patterns is not justified by the statistical evidence. So, the case for the cohort approach cannot rest on the past history argument.

A related argument is that the need to take a longitudinal or life-cycle approach to reproductive and other demographic behavior carries with it the implication that a cohort approach is required. Exactly the same argument applies here as applies to past history. If life-cycle factors, plans, and expectations infuence fertility, this need not imply a cohort approach; as is true of parity, life-cycle stage can be represented in either cohort or period measures. As with the past history argument, there is no necessary link with a cohort approach.[13]

Cohort distinctiveness

The need to take account of past history would imply a cohort approach only if each cohort behaved in a reproductively distinct way that persists throughout its childbearing history. There is no evidence of this and much evidence to the contrary. This issue has effectively been settled by the statistical investigations discussed earlier. These lead us to conclude that various cohorts respond in similar ways to period influences.

Leaving aside the statistical grounds for doubting the existence or significance of cohort effects on fertility, there are substantive grounds for doubting this also. Characteristic marriage and fertility behavior on the part of cohorts is absent in spite of several twentieth-century events providing ample historical opportunity for cohort effects to appear. Several major upheavals have occurred in the last century or so—two world wars, the 1930s depression—that, if "transformations of the social world" truly operated in this way, should have left their mark on some of the generations that experienced them. Yet, as is frequently pointed out, cohort-to-cohort change in fertility is extremely smooth. This is not what would be expected if period-by-age interaction effects were operating to shape distinctive cohort behavior patterns with consequences for nuptiality or fertility plans, preferences, or behaviors, supposing that war and depression are indeed the sorts of events that would create cohort distinctiveness. And if world wars and economic depression are not the kinds of events that would be expected to shape distinctive cohort orientations, what would be expected to do so? Apart from the Easterlin hypothesis, there has never been a clear statement of how and why twentieth-century cohorts would be expected to be distinct from one another in their fertility. As Isaac et al. (1982: 186) remark: "precise expectations for cohorts do not generally exist." The hypothesis survives in spite of, or perhaps because of, considerable vagueness.

Also, it is not at all clear how distinctive cohort orientations to family formation could arise. Ryder's original view (1951: 64) was based on the belief that "attitudes toward childbearing are basic determinants of levels of fertility," that "[e]ach cohort is unique and distinctive in the temporal milieu in which such attitudes are acquired," and that "[t]hese attitudes are not readily changed once they have become established." But he rejected the idea that attitudes to family formation would remain literally fixed, preferring to believe instead that cohorts would display inertia in moving from their original attitudes, and so respond differently to similar period conditions. In later work, he did not elaborate, empirically, on the origin of cohort distinctiveness. On the contrary, his experience with the US National Fertility Studies led him not only to doubt the fixity of fertility intentions but also to espouse a view implying almost unlimited adaptability of attitudes. For example, Westoff and Ryder (1977: 449) conclude that "reproductive intentions are tailored to conditions at the time of interview and, thus, share the same possibilities of misinterpretation as other period indices. . . . They seem little better than conventional period indices. . . ." Intentions, in other words, were as changeable as period fertility itself. The argument for cohort distinctiveness cannot rest on the early development of family size preferences, since these appear to be neither fixed nor resistant to change.

The biological differences between fertility and mortality are also instructive. There are sound biologically based reasons for the existence of cohort effects on mortality. Lifetime susceptibility to particular disease processes can be influenced by intrauterine conditions determined by maternal health and nutrition and may also be acquired in early childhood (Preston and van de Walle, 1978; Barker, 1992; Elo and Preston, 1992). In recent years, Barker and colleagues have been accumulating evidence on what they term the "programming hypothesis," and biological mechanisms through which such programming may operate are being identified. Thus, a sound foundation exists in epidemiological and medical evidence for the idea that a cohort may, so to speak, carry its future mortality with it throughout life, since each cohort bears the imprint of the period influences obtaining during gestation and the first few years of life. But fertility is rather a different matter. There does not appear to be any empirically verified mechanism by which a cohort could acquire a relatively fixed propensity to a particular level or pattern of childbearing. Indeed, such a propensity seems contrary to the expectation we might have on sociobiological grounds. The ability to adapt reproductive behavior and strategies to current circumstances is a key to reproductive success and the survival of species. It seems unlikely that the reproductive behavior of the human species is geared to anything other than this adaptive pattern. On sociobiological grounds we should expect fertility to be heavily influenced by current conditions—that is, by period factors. While sociobiological arguments must be deployed with caution, they may nevertheless help to guide our view of what kinds of hypothesized processes are plausible. In all, the substantive case for cohort effects in the demographic domain outside of mortality seems, on present evidence, doubtful.

Theories formulated in cohort mode

Another claim favoring the cohort approach is that theories and ideas about fertility are usually formulated in cohort mode, though data are usually available in period format (e.g. Ryder, 1982: 19; 1983: 755). In the strictest sense, this is not the case, with the notable exception of the Easterlin hypothesis.[14] It is well known that the demography of low-fertility societies is, for the most part, bereft of fully fledged theory. Explanations for the baby boom and subsequent bust relate to such factors as female labor force participation, the availability of highly effective contraception and access to safe and legal abortion, rising autonomy of women, trends in real incomes, equal pay legislation, increasing marital instability, rising secularism, ideational change, and so on. These are, by and large, period factors, though Lesthaeghe and Surkyn (1988) find ideational change to be cohort-specific. The only fully developed cohort theory in demography—the Easterlin hypothesis—has not fared well when measured against the facts (Smith, 1981; Russell, 1982; Wright, 1989). So, it seems that the cohort approach is underpinned neither by compelling theoretical ideas nor by successful theory.

There is a loose sense in which the aforementioned claim could be described as having validity. Until recently, fertility research has been overly concerned with accounting for family size as a whole, rather than with its components—for example, the progression to particular parities, or the timing of births—and with doing so relative to fairly stable determinants. The fertility survey, the standard investigative medium in the field of fertility research, to some extent reinforces this focus. Survey-based studies typically examine the relationship between family size or other features of the fertility history and characteristics that are relatively fixed attributes of individuals, such as education, social background, income, religion, urban-rural residence, and so on. This is true even of surveys that collect maternity history data. In other words, the explanatory factors in most empirical, individual-level models of fertility are personal attributes. While such explanatory attributes are not generationally based, neither are they period-based, since they relate to factors that—with the exception of the tangled question of women's economic activity—are fairly constant over the life-cycle.[15] Survey-based studies have not sought to investigate the impact of factors that change year by year through individuals' lifetimes. Indeed, the major gap in the fertility research of recent decades could be that it has ignored period-based, macro-level determinants.[16] Although ideas about societal-level period factors have been part of the fertility determinants literature, they have not been the subject of much formal investigation.[17] Also, the principal vehicle of fertility research—the retrospective survey—is not well suited to addressing period influences. None of this amounts to a picture of a strong cohort orientation in empirical or theoretical demography, indicating, rather, a lack of a committed period orientation.

It is fascinating to notice Ryder's tendency to adopt period-based explanations when he directly addresses the task of accounting for fertility trends in the United States. In one instance (1969: 102ff.), for example, his discussion is guided by the assumption that period factors—depression, war, prosperity—are the natural explanations of the distinctive features of each cohort's fertility performance. Four years later, reviewing the findings of the National Fertility Studies, he writes that "despite [his] penchant for viewing the world from cohort eyes, [he] must admit that our recent demographic past has been so distinctively marked by the period-specific stimulus of the introduction of modern contraceptives that the pristine elegance of the cohort approach must bow before the weight of stubborn facts" (1973: 499).

Another of Ryder's themes is that whereas the causes of fertility change are to be sought in the cohort domain, the consequences of such change are to be seen in period fertility outcomes (1959b and 1971). In the perspective of the present article, the causes of fertility change are likely to be found in the period domain, since it is here that the variance lies. As we have seen, Ryder is himself drawn to period factors as the likely source of explanations for developed-world fertility movements in this century. In a sense, one could see the outcomes as residing in the cohort domain, in that the level of fertility in a period is a determinant of the size of the cohort born in that period and also in that the fertility of individuals is the outcome of a series of period rates. In another sense, consequences are in the period dimension also, in that the impact of fertility on population age structure is observed in period mode.

In all, the cohort perspective is not central to demographic thinking about fertility determinants and trends. Faced with the task of explaining actual events, rather than dealing with the formalities of the issue, Ryder is clearly drawn to the period framework.

Trends in cohort total fertility are smoother than the period equivalent

In one passage Ryder (1968: 548) has asserted that the smoothness of the time-series of cohort fertility rates is the "lone justification" for preferring a cohort perspective. Why should smoothness of change be a recommendation in a fertility indicator? Ryder seems not to have elaborated on this question. Hajnal was a little more explicit—writing, as noted earlier, that sensible discussion of fertility change assumed smoothness in the trend. Though Hajnal does not make the connection directly, it seems likely, in context, that the expectation that fertility series should display gradual change was linked with the then central aim of studying fertility—namely, the evaluation of long-run population replacement prospects. The other objective was and is rather less well defined—to study the trend in fertility, unclear though this is in meaning. The indices used for these two purposes in the 1930s and 1940s were both reproduction rates. The gross reproduction rate and the net reproduction rate were thought to reflect, respectively, "fertility in itself" and its consequences for population growth. It is inconsistent with what is known of population movements to suppose that growth prospects should change markedly within a short timespan. Nevertheless that is what was implied by certain rapid shifts in period reproduction rates in the 1930s and 1940s in industrialized countries. Thus, the gradual nature of the change revealed by examining cohort mean family size must have suggested that cohort total fertility was a more reliable quantity on which to base statements about replacement, since cohort fertility rates behaved as reproduction rates would be expected to. It seems that the perceived superiority of cohort fertility series for this purpose became generalized into a view which suggested that the smoothness of the cohort trend was indicative of greater reliability in a more general sense. That is, cohort measures were taken to reflect the true trend in fertility as well as in reproductivity. But none of this is explicitly stated in the writings of the time.

As I have suggested, one can claim that cohort measures are useful for estimating long-run replacement prospects. The matter is not straightforward and is discussed later. But the evaluation of long-run replacement is only one of the purposes for which "current levels" of or time trends in fertility are assessed. A key purpose is to describe change over time in fertility and to examine the causes of such change: that is, to define an indicator (or a set of indicators) of temporal change in fertility that would be suitable for use as a dependent variable(s) in analytical studies of change. There is no reason why the index or indices that are appropriate for describing and explaining fertility change should display the properties desirable in a reproduction rate. If we relax the assumption that a measure of temporal change in fertility should behave like a reproduction rate, there seems no good reason for expecting a fertility time-series to be smooth in appearance. Indeed, for explanatory purposes, the regularity of cohort time-series can, on the contrary, be seen as a defect in contrast with the often substantial fluctuations in period series. As suggested earlier, it is where greater variability is to be found that causes are to be sought. On this principle, the smoothness of cohort series should call into question the value of the cohort approach for investigating the causes of change over time. Ultimately, of course, the utility of either representation, from an explanatory viewpoint, depends on the empirical substantiation of causal hypotheses expressed in one or the other mode. To date, the major cohort theory—the Easterlin hypothesis—has not been empirically validated. By contrast, current evidence favors period as a vehicle of causation since there are numerous instances of a clear response in fertility time-series to period conditions. Moreover, such conditions seem to affect all cohorts simultaneously. Straightforward examples include the effects of famine (see e.g. Feeney and Yu, 1987; Dyson, 1991), the year of the firehorse in Japan (Biraben, 1968; Kaku, 1972; Weeks, 1989), the abolition of legal abortion in Romania in late 1966 (Teitelbaum, 1972; Lévy, 1990), and fears surrounding use of the oral pill (Murphy, forthcoming). There are less exotic examples too—the effects of war, the bulge in births characteristic of the immediate postwar period, and the 1930s depression.

Most of these are short-run situations. The long term is, however, ultimately of signal importance. That we do not have ready examples of period-driven long-run change may stem from the absence of accepted and verified explanations of long-term trends. Another possible contributory factor is that long-run change tends by convention to be assigned to cohorts. It is often assumed that longer-run movements in family size are identifiable through the cohort trend, while period fluctuations reflect short-term influences having the effect of altering the tempo of (cohort) fertility (e.g. Ryder, 1965b: 291; 1968: 547; 1983: 740). Several points are worth noting here.

First, the habit of assigning long-run change to the cohort domain ensures that period factors will appear to influence only timing, and thus short-run, decisions. There seems to be no compelling argument for assigning long-run change to cohorts. One could, with equal justification, allocate both long-run and short-run change to the period domain. The final arbiter is the success or failure of empirical attempts to explain the trends. Descriptively, the statistical evidence points to period as the dominant force, but successful explanatory empirical work in the period mode remains to be done. Second, a long-run trend extracted from an otherwise fluctuating period series will be very similar to the corresponding cohort series. But this does not mean that the two approaches are of equal value for delineating the long view. Fertility series are not only a description, they are also the explanandum in theoretical inquiry. If the mechanisms of change operate period-wise, then fertility should be represented correspondingly.[18] Finally, there is the claim that the cohort/period distinction breaks down when stretches of very long duration—for example, aggregations of 25 or 50 years—are examined, since the two types of measure will then be very similar. If this is so, it reflects little more than limitations of data. It is hard to conceive of circumstances where, in the presence of long runs of complete yearly information, an analyst would choose to employ 25-, 50-, or 100-year aggregates in preference to single years. The long-run trend extracted from full period data will almost certainly be approximated fairly well by a cohort series, suitably displaced. Nevertheless, the two series could also differ in some significant respects, particularly in the location of turning points. So, the considerations set out above apply here too, and the "lengthy aggregations" argument does nothing to rehabilitate the cohort perspective.

It seems reasonable to conclude that the smoothness of cohort series is not, in itself, an argument for the priority of cohorts. Indeed, it may simply reflect the mechanism suggested by the statistical studies referred to earlier: that cohort fertility series arise as averages of (more fundamental) period movements. In addition, the argument of the present article applies with equal force to long-run and to short-run trends.

The cohort framework as natural

A central set of ideas favoring the cohort approach is that it is, in some poorly defined sense, the "natural" way to view fertility or that the cohort is the "primary descriptive target" of the demographer (Ryder, 1969: 100; 1980: 16). The cohort is portrayed and widely accepted as somehow the preferred form of presentation. The idea arises from the central importance Hajnal (1947) accorded to cohort mean family size from the point of view of evaluating population growth prospects. Nevertheless, as argued earlier, the evaluation of growth prospects is only one of several purposes for examining time trends in fertility. The idea of the cohort as natural may also have its roots in the notion that "it is only common sense to study consecutive human behavior when the data are available" (Ryder, 1968: 548). But the cohort approach does not have a monopoly on the representation of consecutive human behavior. As discussed above, one may construct period measures that are specific to parity and duration. Such measures need not be summarized by a synthetic cohort calculation, and, provided they are not, they retain the natural ordering of events. If cohorts in fact had distinctive fertility patterns throughout the life cycle, then the cohort might be the natural unit of observation. We have seen, however, that this is not so.

There are further reasons for doubting the general claim that the cohort perspective has a natural priority. By Ryder's own admission, the assertion of cohort primacy carries with it at least one implication contradicted by existing evidence. A consistent cohort position implies a fixed-target model of reproductive decisionmaking, a model that is known not to fit the facts. Indeed, on the evidence of a series of sophisticated fertility inquiries in the United States from the 1940s to the 1970s, Ryder opts for the view that family formation decisions are taken, more or less, month by month (Ryder, 1973: 504; 1980: 44). There could hardly be a more period-oriented model of the basics of reproduction than this. The cohort primacy view implies a consequence known to be false. This being so, it cannot be considered a natural analytic perspective.

Period quantum reflects cohort timing

In the issue of cohort timing and period quantum, we come to the nub of the argument for the cohort approach. Ryder (1971: 115) suggests that the basic source of the preference for a cohort perspective is that period parameters are "distorted versions" of cohort parameters, wherever the age distribution of cohort childbearing is changing. A passage from one of his most-quoted papers summarizes the key elements of his position: "The fundamental flaw in research based on the period mode of temporal aggregation is simply that changes in cohort tempo are manifested as changes in period quantum." Such an approach produces "results which mislead with respect not only to the magnitude and direction of change but also to the dimensions of the reproductive process which are involved" (1980: 16). This issue is, in my estimation, the core of the problem because the apparent vulnerability of period measures to "distortion" through changing tempo has been perceived as their central weakness, a perception that seems to have weighted the balance so heavily in favor of the cohort approach.

Consider first the assertion that "changes in cohort tempo are manifested as changes in period quantum." In a purely arithmetical sense, this statement is true. But, again arithmetically, the reverse is also true: in some circumstances changes in period quantum, accurately conceived, will appear as changes in cohort tempo. Which is the correct depiction of reality? Ryder suggests that the way in which we view the data—in period or cohort mode—could be considered an arbitrary matter, though he prefers the cohort representation (1982: 23). This is not altogether consistent with the earlier statement, since if it is a purely arbitrary matter, then there is no "fundamental flaw" in the choice of the period mode—either approach would do.

Is it truly arbitrary whether we adopt the cohort or the period perspective? From the standpoint of explaining changes in fertility it seems to matter substantially. The framework in which temporal change is portrayed will determine what research questions are asked and what data are brought to bear in answering them. Instead of asking, for example, why particular cohorts are beginning childbearing at earlier ages, a period perspective would lead us to ask why birth rates at younger ages are particularly high in a specified year or period. Correspondingly, we would look to period conditions for an explanation. So, there appear to be good reasons for attempting to establish which approach reflects the process by which change takes place.

So far, we have encountered no evidence that gives the cohort approach a natural advantage. How is it, then, that in the issue of cohort timing versus period quantum, the cohort perspective appears preferable? The answer lies, I believe, in the way in which period fertility is routinely measured—that is, through the total fertility rate. As is now well known, the period TFR frequently overstates or understates the level of childbearing of "underlying" cohorts. That it misleads us about the "current level" of fertility appears to be an out-and-out defect in the period approach. But we need to consider whether the period approach per se is at fault or the manner in which period fertility is measured. As I see it, it is the measurement of period fertility that is erroneous and not the period approach itself.

The claim that period fertility misleads with respect to the degree, direction, and dimensions of change depends on two false and unnecessary assumptions. The first assumption is that period fertility measures should mimic cohort quantities. They should, in Ryder's terminology, be "isomorphic" with cohort indicators. This supposition ensures that period measures must, by definition, be badly specified. A measure that reflects meaningfully the experience of 35 years of exposure to childbearing cannot appropriately represent what happens in just a single year or quinquennial period.[19] The second assumption is that period indices should be evaluated with reference to how closely they approximate cohort levels. This assumption guarantees that period fertility is misperceived. If, as the statistical evidence suggests, cohort fertility represents, in the long run, no more than the average of the rates of the associated periods, this expectation is tantamount to suggesting that the true period time-series should be abandoned in favor of an average of the series. Together, these assumptions have the effect of, so to speak, wrongfooting the period approach to fertility so that it appears to be a mistaken way of describing reality.

In all, there is nothing to sustain the argument that cohort level and timing are primary (and correct) and that their period equivalents are secondary (and mistaken). Its success is almost certainly due to the contrast between period total fertility and cohort total fertility, combined with the assumption that the first of these is in error to the extent that it deviates from the second. Because the period TFR is frequently out of step with corresponding cohort TFRs, and because the latter are seen as "primary," the claim arises that changes in (the allegedly more fundamental) cohort tempo distort levels of period fertility. This chain of reasoning relies on inappropriate measurement of period phenomena. To take this argument further, we need first to examine what is wrong with the way period fertility is measured and then to propose an alternative to current measurement techniques.

What is wrong with current measures of period fertility?

The conventional age-based total fertility rate, and period measures constructed on similar lines, have several faults. These are of two types—problems of measurement and problems of misuse.

The TFR is faulty not because it deviates from cohort fertility, but because, being incompletely standardized, it gives a distorted comparison between different periods. Even if the synthetic cohort principle is accepted, the TFR is unsatisfactory because it is not properly standardized as between periods. This is because, as Hajnal pointed out more than four decades ago, period measures should be standardized for parity so as to take account of fertility in previous years. Moreover, the same arguments leading to a parity control also imply the need to control for duration of interval since previous birth. In a comprehensive approach, a control for age may also be necessary.

A second flaw in the TFR is that it is constructed on synthetic cohort lines. The hypothetical cohort embodies what Ryder has called "sequential incoherence": it implicitly strings together sequences of events that, in times of change, are known not to occur.

Because of the synthetic cohort principle, the TFR misrepresents what occurs in a period. Clearly, individual women do not have 1.8 births in a year, and this figure does not describe anything actually experienced in the population of women during a year or period. As noted above, quantities that summarize fertility experience over a cohort's childbearing years are, if they are performing their cohort role effectively, definitionally unsuitable for describing the characteristics of a single period's fertility. One can argue that the synthetic cumulation of rates is no more than a useful device to compress into a single figure what is otherwise an unwanted plurality of a set of rates. Nevertheless, the resort to summarizing period rates in a metric that is false to period phenomena means that we routinely misrepresent what happens in a time period. The practice is symptomatic of, and reinforces, the perennial inadequacy of the way we think about period fertility. To persist with this type of measurement is to ensure that we continue to distort reality.

Problems of misuse also surround the TFR, one of which is that it is measured against the wrong criterion. The TFR is evaluated by how closely it approximates cohort values, and it is thought to be wide of the mark to the extent that it departs from them. This is a mistake. A period fertility measure should be judged by how well it represents period, not cohort, levels and trends. As suggested above, the conventional TFR does not do this very well because of inadequate standardization and because it is expressed in the wrong metric.

The conventional TFR is subject to several forms of misinterpretation. It is seen as representing, in some sense, a "current level" of fertility, a concept that in most situations is completely meaningless because a population's "fertility" cannot be gauged from a single year's or period's rates unless those rates do not vary (see e.g. Hajnal, 1959: 31). Period fertility indicators over a longer stretch of time are, however, meaningful as a whole. So, this problem can be solved by abandoning the term "current fertility" and desisting from using a single year's or period's rates to represent it. Instead, we should adopt a concept such as "recent" in place of "current" fertility and, correspondingly, quantify this with reference to either a time-series of period indices or some summary of such a series.

A second mistaken use made of the period TFR is as an indicator of long-term growth prospects—for instance, whether it is above or below 2.1, and so implies long-term population growth or decline. This figure is based on the same principles, and subject to the same objections, as the reproduction rates that were criticized in the 1940s, with the single difference that it is not confined to births of one sex. Hajnal drew attention in 1947 to the futility of asking what the implications of current rates are for population replacement. He returned to the issue twelve years later, asking (1959: 29): "In what sense is it useful to speak of measuring the reproductivity of an actual population that is not stable?" Nevertheless we persist in using the TFR as a kind of reproduction rate. No period measure can satisfactorily fulfill this function where conditions are not, in fact, stable. Where conditions are stable, there is no need to calculate such a measure since it is a known feature of the system. But this is not a deficiency of the period perspective per se; it reflects rather a misconception about what can be inferred from a single period's fertility. The question of evaluating population replacement is highly problematic and is discussed below.

An alternative approach to period fertility measurement

For the present purpose, the assumption is that period fertility measures are sought to describe temporal change in fertility and to provide the dependent variable(s) in analytical studies of change. If this article could offer a comprehensive alternative to the standard methods of quantifying period fertility, its argument would be complete. I do not have a fully developed alternative to offer, merely some suggestions as to the direction it seems sensible to take. Nevertheless, the general principles of such measurement can be readily stated.

We should approach the issue of period measurement by imagining an ideal world where there is access to whatever information is required to measure fertility. In such a world, in contradistinction to the classic demographic ideal world of stable populations, what measures would we use? Having specified the perfect measure(s), we can consider how best to approximate this in the real world of imperfect and incomplete data.

Period measures should be standardized for previous reproductive history, in whatever sense this is necessary. It seems certain that they need to be specific for parity and duration of interval since previous birth, and it is likely that age-specificity is necessary also.

Period measures should be expressed in a metric that is appropriate for period measurement—most likely, a set of rates or probabilities. This means that they cannot be synthetic in character, since measures constructed on synthetic cohort principles misrepresent what occurs in a period. To be authentic, period measures should be such that the phenomena represented by them can, in reality, take place in a period.

Period measures should accurately reflect the differing levels and patterns of fertility between different periods. If a "change in pace" occurs during a particular period, this should be reflected in the fertility measure(s) for that period. Such change should not be ascribed to some notional underlying "cohort"—it is a property of the period. Provided there is sufficient specificity in a period measure for past fertility history, the removal of pace components from period fertility measures would misrepresent the level of fertility in that period.[20]

Period measures should be evaluated according to how well they represent the period trend, rather than how closely they approximate cohort values. There is, of course, no absolute criterion against which we can gauge our measures. The definitive test is operational—does the adoption of a particular measurement approach lead to sensible explanations of fertility change through time? Identifying good fertility indicators and developing sound explanations are interdependent.

Period measures should be expressed in quantities that are appropriate for explanatory analysis.

The natural candidate is the period parity progression approach, albeit with one important reservation. The merits of this approach, originally attributable to Henry (1953), are discussed and developed in Feeney and Yu (1987) and Ní Bhrolcháin (1987). In the present context, parity- and duration-specific birth probabilities satisfy the key requirement that measures of period fertility should take the past history of fertility into account.[21] In low-fertility societies, fertility rates are very closely related both to parity and to duration since previous birth. The probabilities obtained in the period parity progression framework are close to being pure measures of fertility in the period to which they relate. They are superior to age-based rates—even to age-parity-specific rates—since they relate births of each order to the appropriate population at risk, and because standardization is based on the factors—order of birth and duration since previous birth—that most strongly influence birth rates. It is worth re-emphasizing that their superiority to age-based measures does not result from their being closer to cohort levels, though they may be so because of the shorter timespan over which rates are cumulated. They are better period measures than age-based rates because they are properly standardized and therefore do not give distorted comparisons between different periods. This does not necessarily mean that a control for age can be dispensed with: it is possible that specificity for age should be brought back into the period parity progression framework.[22]

It is widely assumed that tempo effects cannot be observed with the data of a single period. I believe this to be untrue. The shifts that are currently thought of as changes in cohort timing can be observed in period mode as shifts in birth probabilities that are differential by duration. An example is presented in Ní Bhrolcháin (1987: Figure 2B and Table 2). For women of parity one, the duration-specific birth probabilities show, during the late 1950s and early 1960s, much more substantial upward shifts at earlier durations (12–47 months) than at durations of 48+ months. In other words, what is usually described as a cohort timing phenomenon can be identified in the period framework. So, the cohort framework is not essential for the identification of such phenomena.[23] A schedule of period parity- and duration-specific birth probabilities (q_xs) might be accompanied by summary measures of the extent to which the q_xs are moving in concert—thus implying a quantum effect—and of the extent to which year-by-year change (up or down) is specific to particular durations—thus implying a tempo effect. It is, of course, possible to summarize differential change in the schedule of rates by calculating period estimates of "birth intervals," as was done in Ní Bhrolcháin (1987: Table 3).

This brings us to the reservation that I wish to enter in relation to the period parity progression perspective. Wherever possible, we should resist the temptation to cumulate the duration-specific probabilities into a synthetic parity progression index for a period, but rather use the schedule of duration-specific probabilities as they are. The objective of this injunction is to avoid recourse to the hypothetical cohort idea, since it is a potentially misleading concept.[24] One may reply that this will leave us with an awkwardly large number of quantities with which to represent fertility at a given time.

There are several points here. First, with further investigation the number of indicators needed might well be reduced through, for example, amalgamation of durations that are found empirically to move together. Second, it is instructive here to make a comparison with mortality analysis. No detailed study of mortality trends relies on expectation of life at birth (e_0) alone. Detailed analysis of mortality addresses the components of e_0—infant mortality (or, in more refined analysis, neonatal mortality and post-neonatal mortality), child mortality, mortality in young adulthood, and so on. The synthetic expectation-of-life calculation is used as a summary, and it is well recognized that a given value of e_0 is consistent with a variety of age patterns of mortality. To focus exclusively or primarily on either synthetic parity progression ratios or on the synthetic TFR—whether age or parity based—can be seen as the fertility equivalent of directing primary attention in mortality analysis to e_0. This is not how the study of mortality has made advances—rather, progress has come through the detailed examination of age-specific mortality. If the study of mortality has gained from directing attention to detailed age-specific rates, it seems reasonable that a similar advantage may accrue to fertility analysis from detailed study of parity- and duration- (and perhaps age-) specific rates. The proposal here is, thus, twofold. I suggest both that synthetic indicators be avoided because they are misleading and that viewing fertility through detailed parity- and duration-specific (and perhaps also age-specific) rates or probabilities may turn out to be as fruitful as the study of age-specific mortality.

Whether synthetic measures of fertility should be proscribed entirely is a moot point at this stage. As we saw above, the period "birth interval" is a convenient summary indicator, and observing changes in the period birth interval seems an efficient way of capturing shifts in period rates that are differential by duration. On the other hand, reference to a period "birth interval" is as meaningless as to a period mean family size of 1.8: there cannot be an average birth interval within a single period and the concept does not represent what happens in the period. Also, change in a period "birth interval" can be interpreted reliably only when information is available about the differential changes that have been occurring. Much the same is true of synthetic parity progression ratios: they too seem an effective way of describing the (quantum) components of period fertility, but full interpretation of their movements requires reference to the constituent probabilities. There is, thus, a case for using the disaggregated data where feasible, since synthetic quantities may well obscure as much as they reveal. While summary indicators may be helpful in a presentational context, such considerations should clearly not influence the choice of measures for analytical purposes. Finally, summary indices need not be in synthetic cohort form—perhaps other, conceptually less hazardous, ways of summarizing the probabilities can be found.

The greater variability of period in contrast to cohort measures has been mentioned in discussing the gradual nature of cohort series. I have argued that the smoothness of a time-series cannot, in itself, be considered an advantage for a measure of change over time. A time-series should be just as variable as the phenomenon it purports to represent. One reason why the traditional, age-based TFR fluctuates more than its cohort equivalent is that it is poorly constructed. Because the traditional TFR is not parity-specific, acceleration or deceleration in fertility across a number of parities can influence age-specific fertility rates. So, changes in the population at risk by parity and duration since last birth are not properly controlled for. One outcome is that the TFR fluctuates more than it needs to. The (multiplicative) parity progression–based period TFR is less variable than the age-based TFR because changes in pace are confined to each progression individually (see Feeney and Yu, 1987; Murphy and Berrington, 1993).[25] But it is equally clear that period parity progression ratios, and their associated TFR, fluctuate more than do cohort values, however the cohort is defined. As I have argued, this is not in itself a weakness. Period indicators should fluctuate just as much as period fertility itself varies. There are plausible reasons for believing that conditions favorable or unfavorable to childbearing can alter fairly rapidly. Since actual fertility behavior almost certainly responds to such conditions, it is natural that our measures should reflect this. There are purposes for which indicators that change more slowly may be needed, as discussed in the next section. Such measures can be defined, but they should not be promoted to the status of preferred measure because of their utility for one particular purpose.

Ryder (1980: 15) has written that "inept measurement procedures have misled us into concentrating on the wrong questions." I would agree but for reasons that are subtly different from Ryder's. He had in mind that the use of the TFR had misled demographers into focusing on the quantum of period fertility while ignoring the tempo of cohort fertility. In the perspective of the present article, the customary use of the TFR has misled us into examining an undifferentiated level of fertility instead of disaggregated parity-and duration- (and perhaps also age-) specific rates. On generating these one can, as I have argued, observe and quantify movements in both quantum and tempo of fertility.[26]

Data issues

Inasmuch as current methods of collecting and collating data are well established, it is likely to take time to change existing practices. Nevertheless, progress has been made toward collecting routine data suitable for the methods of fertility analysis advocated above. France and Italy currently collect, at birth registration, the requisite data for following the present approach, and there are draft plans in the United Kingdom to add true birth order (including both marital and nonmarital births) and interval since the previous birth to the information collected at birth registration.[27] In principle, survey-based maternity histories provide, for periods close to the survey, the information needed to calculate period parity- and duration-specific rates; in practice, however, the sample sizes in most such surveys are so small that period parity progression indicators calculated from them are subject to substantial sampling error.[28]

Following Henry (1953), indirect methods can be used in the absence of comprehensive data (see e.g. Feeney, 1986, 1990, 1991). Brass (1990) takes a different approach to generating indirect estimates of period parity progression ratios. But indirect methods cannot, almost by definition, provide empirical observations of the component parity- and duration-specific rates, since if these were available an indirect method would not be necessary. A better remedy for lack of data, in light of the present argument, is the use of the own-children method applied to large-scale household surveys to estimate the parity- and duration-specific rates. Luther and Cho (1988), Luther, Feeney, and Zhang (1990), and Murphy and Berrington (1993) have successfully employed such data to estimate period parity progression ratios. Luther, Feeney, and Zhang (1990: Table 2) present a set of period- and duration-specific rates for women of parity one. Household data are, thus, clearly a promising source of the disaggregated rates. A further possibility, by analogy with model life tables, is the compilation of model fertility tables of period parity- and duration-specific rates—a feasible option in view of the large number of fertility surveys that have been conducted worldwide in the last two decades.

The availability of large bodies of data in traditional format may well inhibit the search for better types of evidence on fertility. Ryder's remark (1959a: 413) that "the scientific advance of demography has been hobbled by the wealth of data" is all too relevant here. He had in mind that the abundance of certain kinds of period data tended to discourage the adoption of a cohort perspective. In the present perspective, the risk is that the profusion of the wrong kind of period data may curb the demand for what I see as the right kind of period data.

What role for the cohort?

The preference argued here for the period approach does not require us to abandon the insights arrived at through the cohort perspective. The undoubted productivity of the cohort approach can be reconciled with the lack of support for the approach on statistical and substantive grounds. There are several points here.

The cohort has a useful role in representing a kind of average across period fluctuations. It is not essential for this purpose, since other kinds of time-period averages could be specified as Brass (1974) has noted. Nevertheless, provided we ignore the problem of differing male and female indicators, it has a kind of demographic coherence.[29]

The cohort perspective has also been fruitful because it has addressed a need not met by the sorts of period measures in use in the 1930s and 1940s and by those in use today. In short, it provides a convenient and easily understood framework by which to represent secular trends in the timing of fertility. This convenience arises largely because of the general unavailability of the kind of data required to specify sensible fertility measures in period mode (i.e. parity- and duration- and perhaps also age-specific period rates). A further advantage for the cohort is that tempo phenomena are easy to describe in cohort mode but have greater complexity in the period format. If data resources continue to be strained, the cohort approach will retain this practical utility.[30] Nevertheless, it would be sensible from an intellectual point of view that our measures be designed in the framework for which there is greatest justification—in the period mode, according to the argument of the present article. None of these practical uses need imply that the cohort has any greater significance other than as a convenient way to aggregate the data. If clarity is maintained regarding the roles of the various representational modes, problems should not arise.

A final role for the cohort is in providing measures for estimating future growth prospects. The cohort's utility in this respect is not altogether clear, since the notion of reproductivity is more complex than is generally acknowledged. Hajnal (1959) contended that, except in conditions of stability, the concept of population replacement is not straightforward. And it is precisely when conditions are not constant that reproduction rates of some kind are needed. Pending progress on the issue, cohort or cohort-like quantities may be useful in this context, though time-period averages can be just as satisfactory. It continues to be dubious to use a single year's fertility rates as the basis for a stable long-run calculation, and to infer that "current fertility" lies above or below replacement level. It is less dubious to use a cohort figure or an average of recent periods. On the other hand, it is arbitrary, as Ryder (1971: 121) notes, to use the fertility of any particular cohort for the purpose.[31] It is not straightforward to operationalize the concept of population replacement. Whether cohort values truly play a part in evaluating it is an open question. We do not yet have answers to the points raised by Hajnal (1959) in his discussion of reproduction rates.

Some final comments are needed on the point raised by Rindfuss, Morgan, and Swicegood (1988: 226ff.) in contrasting the "strong theoretical rationale" for and the "intuitive plausibility" of cohort effects with their absence in the US fertility data they examined. It seems reasonable now to question the intuitive plausibility of cohort effects in the demographic domain. And while the theoretical rationale offered by Ryder for the existence of cohort effects was attractive, surely it was and continues to be insufficiently grounded in empirical observation. Should we not reject as unfounded the expectation that cohort effects are likely to exist in the fertility domain and turn our attention to the search for, and quantification of, the period factors that influence childbearing? As Rindfuss, Morgan, and Swicegood conclude (1988: 241): "An important goal of future research is to understand better how these pervasive period effects operate."

Reprise and conclusion

The confusion referred to at the start of this article is, I believe, twofold. First, time trends in fertility and in reproductivity have been confused. Currently, we have a single indicator, the total fertility rate, to represent these two distinct concepts, though the net reproduction rate is sometimes used for the second of them. We require, at minimum, a system of measurement that accurately tracks fertility through time: that is, fertility measured so as to be an appropriate dependent variable in valid and successful empirical models of change through time.[32] When a measure or set of measures, whether or not rooted in the approach recommended here, is ultimately identified as the system of choice, it may turn out to be suitable for evaluating population growth prospects as well. But there seems no reason at present to expect a dual-purpose solution. The second source of confusion lies in the twofold nature of Hajnal's resolution of the "problem" of strong short-term fluctuations in period fertility indices. He proposed both that cohort mean family size would be a better guide to trends in replacement, and that, to avoid period-to-period bias in indicators, period measures should be specific by parity. The recommendation to use cohort-based measures was taken up as "the" resolution of the problem. The proposal that period measures be parity-specific was not overlooked[33] but seems to have been accorded a secondary place.[34] Our present difficulties could be seen as resulting in part from the lopsided reception of Hajnal's message.

A tabular representation may help to clarify the structure of my argument in favor of the period approach. Table 1 outlines the perspective favored by the so-called classical arguments in support of the cohort approach. The position at the start of our discussion is as set out in the first column of the table. Justification for the details of this classification can be found in preceding sections and is not elaborated here. On considering the evidence and arguments, I conclude that none of the classical arguments for the cohort is sound, hence the position at the end of this inquiry is as represented in the second column of the table. Recall that the statistical evidence discussed at the outset favors the period approach. So, present evidence and knowledge are either neutral with respect to the cohort/period issue or favor the period perspective. This article does not purport to establish definitively that the period format is the correct way to represent fertility trends. To do so would require extensive empirical investigation. I propose rather that, of the two, the period approach currently holds the greater promise and has the more solid justification.

It is startling to find that none of the demographic grounds for the cohort approach to fertility stands up to close examination. It is also surprising that they have been so little discussed. The statistical evidence indicates that if there are cohort effects in twentieth-century developed-country fertility series, they are so subtle as to be extremely difficult to detect. This fact does not preclude the existence of some cohort effects of a theoretically interesting kind, even though they may not account for much of the variance in fertility.[35] By the same token, however, it must rule out the idea of the cohort as having explanatory primacy in relation to contemporary developed-country fertility. And, as we have seen, a cohort perspective is not necessary in principle for demographic purposes, even though it can be useful where period parity-

TABLE 1 Classical arguments favoring the cohort approach to analyzing fertility, contrasted with the perspective advanced by the author

	Perspective favored by	
Argument based on	Classical argument	This article
Past history	cohort	neutral
Cohort distinctiveness	cohort	period
Theories/theoretical ideas about fertility	cohort	period or neutral
Cohort trend is smooth; period is variable	cohort	period or neutral
Cohort approach is natural	cohort	neutral
Cohort timing distorts period quantum	cohort	neutral

and duration-specific rates are unavailable. Virtually everything that is achieved through the use of the cohort can be done in other ways, except of course the cohort aggregation itself. Cohort data are not necessary for the effective analysis of fertility change; on the contrary, they may actually hinder progress by directing research to the wrong questions. Whether the period approach to fertility has greater validity than the cohort view is almost certainly more than a matter of formal correctness. Perhaps our slow progress toward defensible explanations of fertility trends arises, at least in part, from the rarity of a thoroughgoing and carefully considered period perspective in empirical studies.

Notes

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1 See especially Brass (1974), Page (1977), Pullum (1980), Namboodiri (1981), Smith (1981), Isaac et al. (1982), Rindfuss, Morgan, and Swicegood (1988), and Foster (1990).

2 Although Hobcraft, Menken, and Preston (1982) discuss most of the statistical investigations of fertility considered here and note that they are largely supportive of the period approach to fertility, they do not come to a clearly stated conclusion in its favor.

3 Indeed, the radical difference between mortality and fertility in this respect was noted and documented in one of the earliest papers on the subject. Barclay and Kermack (1937) found unambiguous cohort regularities in age-specific death rates from the mid-nineteenth century to the 1930s and equally clear period regularities in age-specific fertility rates.

4 Another possible reason for the durability of the cohort view, mentioned by Foster (1990), is the tendency to associate long-run change with cohorts and short-run change with periods. This is discussed in a later section.

5 This article concentrates here and throughout on the English-language literature. There have also been substantial contributions in French to this subject (see, for example, Vincent, 1947; Pressat, 1969; Festy, 1979). The issue of cohort and period fertility has recently been the subject of intense debate in French demographic circles: see Calot (1990); *Le Monde* (1990), *Le Figaro* (1990), and a number of other press reports and popular articles in mid-1990; and Le Bras (1991).

6 It is not generally appreciated that Hajnal was the first to present a systematic solution—with both cohort and period components—to the problems posed by using reproduction rates to track fertility trends. His work on the demography of marriage and of the household has been cited extensively, his papers on fertility much less so. They show an acute analyst at work and have much to offer the present-day demographer.

7 Hajnal notes that the idea of postponement does not require the assumption that couples have relatively fixed family size desires, nor that families are completely planned. It is necessary to assume only that "a substantial proportion of couples attempt to exercise control over the time at which they have children with such success that they can substantially reduce the rate at which, as a group, they have children in a given period" (Hajnal, 1947: 151).

8 There is a link, albeit a weak one, in Hajnal's paper between the perceived need for parity-specific rates and the need for cohorts: annual fertility rates were to be related to "the number [of children] already born to the marriages in question" (1947: 153). For Hajnal it was the measurement of period rates especially that needed to be parity-specific.

9 In taking issue with the cohort view, one inevitably finds oneself engaged in a detailed study of the work of Norman Ryder, the most eloquent and influential proponent of the cohort perspective. This article is based on a close reading of Ryder's work, which is consistently interesting and thought-provoking on the subject of fertility measurement. My critique as a whole is intended as a constructive endeavor. Ryder (1973) has himself noted that criticism is "a form of compliment to the researcher." From my discussion it will become clear also that there are many issues on which I am in agreement with Ryder.

10 Ryder (1975: 9) neatly summarized the point when he stated that parity-specific fertility rates were "more purely a reflection of fertility itself . . . and more purely a specification of current behavior, since the residue of history crystallized in the parity distribution has been abstracted."

11 This conviction regarding the necessity of a cohort reference goes back to Ryder (1951: 117), who comments that "[r]egardless of the level of specificity for which fertility is studied from period to period, the integrity of cohort behavior, the interdependency of the reproductive experience . . . of the same cohort of people in successive periods" must be respected in analysis.

12 Hobcraft, Menken, and Preston (1982: 10) comment that "[c]ohort effects occur whenever the past history of individuals exerts an influence on their current behavior in a way that is not fully captured by an age variable." If what is meant by "past history" here is the past history of fertility, then "cohort" would be merely a proxy for previous fertility experience. This cannot be a sensible meaning for "cohort." If, on the other hand, "past history" refers to previous experience outside the fertility domain—e.g., formative experiences rooted in social, cultural, or economic conditions—then the statement makes more sense. In the perspective of the present article, a cohort effect occurs when a cohort term is needed in descriptive statistical models of fertility rates that include, along with period, age, parity, duration, and perhaps other factors designed to standardize for "past history of fertility." Of course, problems arise in the statistical identification of such models, but that is a separate issue.

13 The implications of life-cycle factors for demography need to be clarified. For example, wealth might be considered a life-cycle factor since it is relatively constant over the life course and, for any given individual, is not as variable from year to year as earnings. However, this does not mean that wealth must be viewed in a cohort framework. Only if variation in personal wealth can be shown to be subject to cohort-specific influences can one argue that it is best analyzed in cohort mode.

14 Lesthaeghe and Surkyn (1988) have recently argued for cohort distinctiveness and present evidence of persistent cohort differences in the ideational, though not the demographic, domain.

15 The problems associated with investigating the impact, if any, of women's economic activity on fertility have been well documented. No extended treatment of the issue is possible here except to note that although women's labor force involvement varies over their life cycle, it is erroneous to treat participation itself as a time-varying covariate where fertility is the dependent variable. For a recent discussion of this and associated issues see Ní Bhrolcháin (forthcoming).

16 For an exception and discussion of some of these issues see Murphy (1992a), who uses male and female real wages as time-varying covariates, alongside more standard factors, in a proportional hazards model of progression to third birth in Sweden. The period factors that have, by and large, been overlooked in empirical work are macro- or societal-level, perhaps even global-level variables (see Dyson and Murphy, 1985).

17 Of course, there have been some time-series investigations, but most of these have been conducted within the framework of economic theory and suffer both from technical shortcomings and from dissociation from the demographic realities. Factors such as the introduction of the contraceptive pill, for example, have not figured in econometric time-series models. See Murphy (1992b and forthcoming) for a discussion of this issue.

18 Clearly, if cohort data are available and period figures are not, long-run trends can be approximated by the cohort series. Nevertheless, demography needs to be wary of allowing itself to be driven by the available rather than the requisite information.

19 Of course, a single-year or a five-year age-specific fertility rate, or a parity- and duration-specific fertility rate, can be calculated meaningfully in both cohort and period frameworks. That is, most authentic period-style measures can be generalized meaningfully to cohorts, but the reverse is true only if synthetic measures are allowed in the period mode.

20 To refine or transform period measures so that they more closely approximate cohort values is to denude them of part of their essential period character. It may be possible to do this, but the resulting quantities should be thought of as period-based estimates of cohort quantities.

21 The term "duration" is used here to refer to duration since previous birth (in the case of second or higher order birth probabilities), or since the beginning of marriage/cohabitation (in the case of first birth probabilities), or since the start of exposure to risk of a first birth (say age 16) where first birth probabilities are considered irrespective of partnership status. See Feeney and Yu (1987), Ní Bhrolcháin (1987), and Murphy and Berrington (forthcoming).

22 Murphy and Berrington (forthcoming) present recent age-specific period parity progression ratios for the United Kingdom; their results suggest the possibility of significant variation in period parity progresssion ratios with age. See also Rallu and Toulemon (forthcoming), who have carried out an extensive investigation of a range of summary measures of period fertility; several of these measures are of the period parity progression type, including a multiplicative total fertility figure based on parity progression ratios standardized for parity, duration, and age.

23 Another instance of change in birth probabilities that is differential by duration is seen in the England and Wales postwar baby bulge of 1946–48, with 1947 as the peak year. An analysis of this in terms of parity- and duration-specific birth probabilities is presented in Table B, Appendix B of Ní Bhrolcháin (1985), a longer and more detailed version of Ní Bhrolcháin (1987). Because of the nature of the data set, it was not possible to compare 1947 birth probabilities with those of previous years. Instead the 1947 duration-specific birth probabilities were compared with the 1951 figures, 1951 being chosen as approximating "normal" levels for the time. Table B shows clearly that the decline between 1947 and 1951 in the birth probabilities of women of parity one was much larger at longer durations (5+ years) than at shorter durations. This was also true of women of parity two. This confirms the 1947 bulge as, to an important extent, a catching-up of postponed births. The principles on which this analysis is based are the same as those in Hajnal (1950: Tables 14–16), which relied on marriage cohorts. However, the present approach does not require a cohort approach to allow the observation of a postponement effect.

24 Furthermore, it seems probable that the caveat mentioned by Hajnal is not usually observed in the case of synthetic cohort measures. He pointed out that "[t]he computation of a reproduction rate from a set of frequencies and the decision to regard this set as characteristic of the current demographic situation are two separate processes and there is no necessary connection between them" (1950: 343).

25 Feeney and Yu (1987) demonstrate that each of the parity-specific components of the period parity progression–based TFR fluctuates less than its age-based equivalent; Feeney and Saito (1985) show this to be true of the progression to marriage as well.

26 Implicitly included in the "parity" designation here is the progression to marriage, no longer as critical in low-fertility societies as it used to be in identifying the start of exposure to risk of childbearing, but certainly still significant in the recent past. The marriage progression is evaluated in the parity progression framework as a conventional gross nuptiality table. In the absence of a marriage progression, the progression to first birth is evaluated in an age- rather than duration-based period life table.

27 Of course, countries with comprehensive population registers can readily generate these measures from existing data.

28 For a comment on the sampling error associated with survey-based parity progression measures see Murphy and Berrington (forthcoming).

29 The alternative measures of fertility

proposed here do nothing to resolve the discrepancy between male and female fertility rates.

30 Since cohorts may have the role of presenting in simplified format what are otherwise complex period phenomena, it is inaccurate to describe the cohort as a "con-trick" (Hobcraft, 1991). The cohort has its uses, and may well continue to do so. Hobcraft favors the period approach for statistical reasons but touches only lightly on the demographic aspects of the issue.

31 The same applies to averaging recent periods. At present there is no theoretical or even practical justification for choosing any particular set of periods, nor do we have any basis for averaging in any particular way.

32 One could argue that a weakness of much of the formal demography of fertility is that it has been conducted in an explanatory vacuum. Not enough thought is given to the need for measures of temporal change in fertility that are appropriate on both formal and substantive grounds. Ryder, by contrast, has attended carefully to the twin tasks of describing and accounting for fertility trends (see especially Ryder, 1980).

33 For example, Ryder (1951) devotes considerable attention to parity-specific measures, as does Whelpton (1954). Henry's 1953 monograph is the classic study in this respect and a superbly full treatment of the subject. Parity-specific period measures had, of course, been in use before Hajnal's 1947 paper—see e.g. Quensel (1939), Lotka and Spiegelman (1940), Galbraith and Thomas (1941), and Whelpton (1946).

34 It is clearly difficult to reconstruct the thinking of the time, but a contributory factor here may well have been Hajnal's resort to marriage cohorts in generating parity-specific measures.

35 It is possible also that cohort effects may be found in contemporary less developed countries or in historical populations.

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