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The limitations of English family reconstitution: *English population history from family reconstitution 1580–1837*

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English population history from family reconstitution 1580–1837 (Cambridge, 1997) is an impressive volume. This ambitious study represents the culmination of a quarter-century of laborious research by four of the most accomplished practitioners of English historical demography, E. A. Wrigley, R. S. Davies, J. E. Oeppen, and R. S. Schofield. The sheer volume of information is overwhelming; the book contains 121 tables and 73 graphs, and it weighs in at almost $2\frac{1}{2}$ pounds. The study is a landmark in the field of pre-industrial population history. It contributes important new evidence on long-run trends in fertility, mortality, and marriage behaviour. Even more exciting than the refinement of the aggregate results contained in previous work by the Cambridge Group, however, is the new kinds of analyses made possible by the existence of microdata. The book marshals an array of innovative methods to address a remarkable assortment of demographic issues. The authors address dozens of topics previously hidden from view, ranging from an ingenious analysis of the relative mortality of monozygotic and dyzygotic twins, to an important investigation of lifetime fecundity, to an exhaustive analysis of the seasonality of mortality.

A work of this magnitude invites close scrutiny. Like the previous equally substantial volume by the same research team, *The Population History of England, 1541–1871* (1981) this one is sure to generate controversy. Much of this, I expect, will be stimulated by the expansive claims made by the authors about the representativeness and reliability of

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their national estimates of demographic behaviour. This essay will explore these issues in detail. In particular, I will describe and evaluate the main potential sources of error in the English family reconstitutions. The focus on sources of error necessarily gives this essay a negative tone. I do not wish to detract from the tremendous accomplishment of the authors, and I am certain that achievement will be universally recognized within the field. It is the importance of the work that justifies and demands the careful evaluation of its limitations.

We can conveniently group the major sources of error in family reconstitution into five general categories:

1 Non-representativeness of selected parishes
2 Selection bias (non-representativeness of selected individuals because of the exclusion of migrants and nonconformists)
3 Censoring (mis-specification of at-risk population)
4 Linkage failures and under-registration of vital events
5 Random error.

The following sections discuss each of these potential problems in turn.

NON-REPRESENTATIVENESS OF SELECTED PARISHES

English Population History from Family Reconstitution generalizes about the population of England as a whole on the basis of information derived from 26 parishes, about a quarter of 1 per cent of the ancient parishes of England. These parishes were not selected randomly; rather, 'a small band of volunteers' working with the Cambridge Group chose parishes 'in their neighbourhood' and undertook the tedious process of transcribing by hand the registers of baptisms, marriages, and burials, and then linking them together to create life histories.[1] Despite the non-random selection of parishes, the authors maintain that they are remarkably representative of England as a whole, and thus permit reliable national generalizations about demographic behaviour. In the case of mortality estimates, they go even farther, and argue that many results of the family reconstitution studies may be viewed 'with almost equal confidence' as the official vital statistics published by the registrar-general in the period after 1837.[2]

The volunteers contributed 34 parishes, but Wrigley and his collaborators rejected 8, mostly because they suspected them to be of low quality or because they showed unusually high childnessness. Among the 26 parishes remaining, the authors excluded particular chronological sections of the reconstitution in 14 cases. There do not seem to have been clear criteria for including or excluding a reconstitution or part of a

TABLE 1
Population density in 1801 of family reconstitution parishes and England as a whole

Place	*Persons per square mile*	*Total persons*
26 included parishes	235.56	56,867
12 fully included parishes	288.18	38,175
14 partially rejected parishes	171.54	18,682
8 fully rejected parishes	136.30	17,227
England	172.18	8,671,439
England without Greater London	150.73	7,556,795

Sources: Wrigley et al., *English population*, 22–3, 614; Karl Gustav Grytzell, *County of London: population changes 1801–1901* (Lund, 1969), 123–5.

reconstitution; the language the authors use to describe those decisions indicates that they were seat-of-the-pants judgements about the quality of the register. The reasons given for excluding data are most frequently a period with a drop-off of births or deaths, but sometimes data were eliminated because the authors felt that other demographic measures were implausible. For example, they write that 'the completeness of burial registration appears to have deteriorated in Bridford towards the middle of the eighteenth century'; in Hartland, 'it may be somewhat harsh to reject data from the early decades, but there are several periods in the burial register when the number of events recorded appear suspiciously low'; in Earsdon, 'there was probably a marked deterioration in registration towards the end of the eighteenth century', because the reconstitution shows a substantial decline in infant and child mortality.[3] In every case, the authors describe the exclusion in impressionistic language – 'appear', 'probably', 'suggest' – rather than citing clear-cut measures of data quality.

The 26 parishes that form the basis of the book are on average about two-and-a-half times larger in population than typical pre-industrial English parishes. It is not clear how this might contribute to non-representativeness, but one might expect that the larger parishes would tend to be places that had experienced more growth, and might also tend to be more urban. There is evidence in support of these hypotheses. Wrigley and his collaborators provide the acreage of each parish, so it is possible to calculate the population density of each parish in 1801 (the time of the first census) in comparison to that of England as a whole, An analysis of these data appears in Table 1.

As one might expect, the 26 parishes had somewhat higher density than England as a whole. More striking, however, is the relationship between rejection of a parish and population density. The 12 parishes that were included in their entirety had an overall population density more than twice as great as the 8 rejected prishes. The 14 partially rejected parishes had an intermediate population density. This suggests that the quality of parish registers is related to density, perhaps because larger parishes – which typically paid better – were able to attract more responsible or better-educated clergy. The finding that the quality of the parish registers varies according to parish density is cause for concern. It suggests not only that the family reconstution parishes are biased towards dense populations, but also that the previous study of 404 parishes carried out by the Cambridge Group in 1981 could be biased.[4]

No London parishes were included among the 26 accepted parishes. Despite the apparent association between high population density and high-quality parish registers, the highest-density place in England apparently has no parish registers of sufficient quality to allow reconstitution. The authors note that the demography of London differed radically from that of the rest of the country, but surprisingly they never discuss how their estimates might be affected by exclusion of the metropolis. The non-representativeness of the 26 parishes with respect to population density is even more striking when we compare their density to England outside of London, a figure given in the final row of Table 1. The reconstitution parishes had an overall density 56 per cent greater than the density of England excluding London.

In sum, the 26 parishes exclude London but are otherwise apparently biased towards places with higher-than-average population densities. This raises two concerns with quite different implications. First, as pointed out by William Farr in the nineteenth century and readily acknowledged by the Cambridge authors, population density was a key determinant of mortality levels.[5] Second, high density in 1801 was doubtless associated with rapid growth during the eighteenth century; thus, we would expect such places to have been demographically atypical.

The composition of parishes included in the analysis varies according to time period. Wrigley and his colleagues defined four overlapping groups of parishes according to period of coverage. Some parishes appear in all four groups; a few appear in only one. Table 2 shows the population density in 1801 of each of the four groups of parishes. The lowest density group (Group 1) consists of the parishes available for the earliest period. The group of parishes for the most recent period (Group 4) also has low density, perhaps because parish registration broke down soonest in towns. The most important group, however, is Group 3, which covers the period

TABLE 2
Population density in 1801 of four groups of family reconstitution parishes

Group	*Persons per square mile*	*Total persons*
Group 1, 1580–1729 (15 parishes)	203.48	26,608
Group 2, 1600–1729 (20 parishes)	251.19	48,749
Group 3, 1680–1789 (18 parishes)	279.26	44,864
Group 4, 1680–1837 (8 parishes)	217.30	14,953

Source: Wrigley et al., *English population*, 22–3, 26.

1680 through 1789. Because the eighteenth century is the period when England's population took off, Group 3 is the source for many key analyses. It is also the group with the highest overall population density, some 85 per cent greater than in the rest of England outside London.

The authors maintain that 'as a result of serendipity rather than initial design, it is reasonable to regard any findings relating to the four groups of reconstituted parishes as likely to reflect national characteristics'.[6] They base this claim of representativeness on two analyses. First, they compare the occupational structure in 1831 of the 26 parishes with that of England as a whole. Second, they compare the raw time series of vital events in the 26 parishes with that of a broader group of 404 parishes that they used in their previous study. *The Population History of England, 1541–1871*.[7]

The occupational results are consistent with the evidence on population density; Wrigley and his colleagues found that, in 1831, the 26 selected parishes had significantly more men engaged in manufactures than did England as a whole.[8] To correct for the high proportion of manufacturing in the reconstitution parishes compared with the rest of England, the authors adopted a crude weighting procedure. They gave a 'half weight' to the parish with the second-highest percentage of the 1831 population in manufactures (Shepshed) and to the parish with the third-highest percentage in manufacturing (Birstall). No explanation is given for not applying the weight to Gedling, the parish with the highest percentage of men engaged in manufacturing. This weighting scheme was then applied for all periods in every subsequent tabulation in the book.

Weighting is of course a common practice in population analysis. Ordinarily, however, it is done more systematically. The authors arrived at the weighting system by experimenting with a variety of different weights until they got overall results they liked. The *ad hoc* flavour of the

TABLE 3

Over- and under-representation of men in manufactures, 1831: proportion of men in manufacturing in family reconstitution parishes as a percentage of proportion in England as a whole

Population	*Percentage*	*N*
26 parishes, unweighted	138.7	20,340
26 parishes, weighted	98.0	17,213
Group 1, weighted	51.0	9,822
Group 2, weighted	112.2	14,675
Group 3, weighted	126.5	12,674
Group 4, weighted	136.7	5,076

Source: Wrigley et al., *English population*, 49.

weighting scheme is characteristic of the style of methods employed throughout the book. The origin of the weights is purely arbitrary; we are asked to accept them as valid because they yield plausible results.

Unfortunately, the results of the weighting system are far from ideal. Although the weights improve the overall percentage of persons engaged in manufactures in the 26 parishes considered as a whole, the fix does not have even effects across the four groups of parishes. Moreover, the weighting scheme actually *worsens* the fit between the reconstitution parishes and England as a whole for some other occupational groups, such as retail trade and handicrafts, and does not fully correct for the bias in population density.

The impact of the *ad hoc* weighting scheme on the percentage of men in manufacturing can be seen in Table 3. Unweighted, the 26 parishes had 38.7 per cent more men in manufactures in 1831 than did England as a whole. When Shepshed and Birstall are counted at half weight, the reconstitution parishes have almost identical participation in manufactures as the rest of the country. The weighting is much less effective, however, when we look at the four groups of parishes used for the analysis, shown at the bottom of Table 3. The first group of parishes – those used to analyse the earliest period – had half the population engaged in manufactures, as did the rest of England. All the other groups continue to over-represent manufacturing even after Shepshed and Birstall are counted at half weight. In the critical Group 3, the weighted percentage of men engaged in manufactures is 26.5 per cent higher than in England as a whole. Surprisingly, the relatively low-density parishes that comprise Group 4 have the highest over-representation of men in manufacturing.

The weighting system only modestly reduces the bias in population

TABLE 4
Population density in 1801 of family reconstitution parishes and England as a whole, using weights

Place	*Persons per square mile*	*Total persons*
26 included parishes	212.44	48,215
12 fully included parishes	252.48	30,846
14 partially rejected parishes	165.76	17,369
8 fully rejected parishes	136.30	17,227
England	172.18	8,671,439
England without Greater London	150.73	7,556,795

Sources: Wrigley et al., *English population*, 22–3, 614; Grytzell, *County of London: population changes*, 123–5.

TABLE 5
Population density in 1801 of four groups of family reconstitution parishes: weighted figures

Group	*Persons per square mile*	*Total persons*
Group 1, 1580–1729 (15 parishes)	203.48	26,608
Group 2, 1600–1729 (20 parishes)	233.08	39,837
Group 3, 1680–1789 (18 parishes)	247.70	36,222
Group 4, 1680–1837 (8 parishes)	210.85	13,640

Source: Wrigley et al., *English population*, 22–3, 26.

density of the reconstitution parishes. Tables 4 and 5 are the same as Tables 1 and 2, except that Birstall and Shepshed are counted at half weight. Overall, the 26 parishes are still over 40 per cent denser than the rest of England outside London, and the parishes that make up Group 3 are about 64 per cent denser than the rest of the country.

In addition to the occupational composition of the reconstitution parishes in 1831, Wrigley and his colleagues offer a second proof of the representativeness of the 26 reconstitution parishes. For their earlier study of the population of England, the Cambridge Group had compiled information on the annual number of baptisms, marriages, and burials in 404 parishes over the period 1541–1871. Like the family reconstitutions, these parish totals were originally gathered by volunteers and then subjected to various adjustments and tests of reliability by the Cambridge

TABLE 6
Estimated number of demographic events in the 1690s and 1780s: aggregate series and reconstitution series (Group 3)

	1690–1699	*1780–1789*	*Percentage growth*
Baptisms			
Aggregate series	55,597	86,603	55.8
Reconstitution series	51,149	93,531	82.9
Marriages			
Aggregate series	13,661	23,740	73.8
Reconstitution series	11,202	26,114	133.1
Burials			
Aggregate series	50,569	67,454	33.4
Reconstitution series	43,489	76,898	76.8

Sources: Wrigley et al., *English population*, 60–1, 64, 66; Wrigley and Schofield, *Population history*, 537–60.

Group. To test the representativeness of the 26 family reconstitutions, the authors prepared graphs of the annual totals of baptisms, marriages, and burials in the family reconstitution parishes compared with the larger set of 404 parishes. As noted, it seems likely that the 404 parishes may also be biased to some extent by systematic exlusion of low-density parishes.

The comparison between the 26 reconstitution parishes and the larger set of 404 parishes (the aggregate series) is informal. The authors carry out no statistical analysis of differences between the two series, instead relying on a subjective visual appraisal of whether or not the trends in the reconstitution series differ significantly from those of the aggregate series. They conclude that the differences are trivial: in fact, they describe the correspondence between the reconstitution and aggregate series as so 'remarkable' as to be surprising. 'The fact that, against reasonable expectation, the match between the two series proved to be so good is not easy to explain, but it represent san opportunity too promising to be neglected.'[9]

There was little secular trend in either the reconstitution or the aggregate series in the seventeenth century; for both series, there was approximately the same number of births, deaths, and marriages around 1600 as there was around 1700. There are, however, differences in the fluctuations from year to year; in some instances, the relative number of events in the two series is off by as much as 85 per cent.

The larger problems with the representativenes sof the reconstitution

sample begin in the early eighteenth century, when the number of demographic events began to rise. Table 6 shows my estimates of the numbers of baptisms, marriages, and burials in the 1690s and the 1780s in the aggregate series and the reconstitution series. Unfortunately, the authors do not provide the numbers underlying their graphs, so I had to be creative. The aggregate series data come from the earlier work by Wrigley and Schofield.[10] The reconstitution number of baptisms is based on figures given by the authors in the text. To obtain the reconstitution estimates for numbers of marriages and deaths, I enlarged the graphs and used a ruler to estimate the data points.

The results are disheartening. By every measure, the number of demographic events in the reconstitutions went up far more rapidly than the number of events in the aggregate series. Overall, the reconstitution baptisms grew 48.6 per cent faster than the aggregate baptisms; the reconstitution marriages grew 80.4 per cent faster than the aggregate marriages; and the reconstitution burials grew a stunning 130 per cent faster than the aggregate burials. Bear in mind that this analysis makes use of the weighting system described above to minimize the impact of Birstall and Shepshed; without that adjustment, the results would be even worse. It is worth nothing that the proportional discrepancy between the reconstitution population and the aggregate population is higher for marriages than for baptisms, and higher still for burials. This suggests that the rapid growth of these parishes was probably fuelled by migration, some of which occurred before marriage and some of which occurred after marriage.

The evidence is clear. The reconstitution population lived in significantly larger and denser places than did the rest of the English population at the beginning of the nineteenth century. It should come as no surprise that these parishes grew far more rapidly during the eighteenth century than did the rest of the country. It is remarkable that after decades of work with these parishes, the authors faied to appreciate these basic facts.

Wrigley and his collaborators write that they had originally envisaged a volume stressing the *range* of demographic experience within the country, by pointing out the differences in the demographic experience of different parishes. When they discovered that the 26 parishes closely mirrored the demographic experience of England as a whole, however, they abandoned this strategy and decided to pool the information from the parishes and describe the demographic experience of England as a whole. This was clearly a mistake.[11] The 26 reconstitution parishes do not represent England as a whole. By focusing almost exclusively on the combined parishes, the authors lost the opportunity to assess how local conditions affected demographic behaviour.

SELECTION BIAS

The previous section was concerned with non-representativeness of the 26 parishes used to represent the English population; this section focuses on the issue of non-representativeness of individuals within those parishes. The bulk of the literature written on the limitations of the family reconstitution method stresses this problem of selection bias. Family reconstitution methods usually exclude most of the parish population from analysis. The largest excluded group is composed of persons who migrate across parish boundaries. In addition, religious dissenters and anyone else whose baptisms, marriages, or burials took place outside the established church are excluded from analysis.[12]

There has been considerable debate among historical demographers about the extent to which non-migrant religious conformists are representative of the population as a whole. Some analysts – many of them practitioners of family reconstitution – argue that the biases are minor. Others point out that the non-migrant population over-represents farm occupiers, artisans, and fishermen, while under-representing both the rich and the poor. It would indeed be surprising, I think, if the non-migrant minority of the population – a group known to be atypical with respect to one aspect of demographic behaviour – turned out to be identical to the population as a whole with respect to all other aspects of their demographic behaviour.[13]

The potential seriousness of selection bias depends on the proportion of cases excluded from analysis. French family reconstitutions ordinarily include at the outset a table giving the proportion of cases available for each demographic measure, so that the reader may judge the potential for selection bias. Wrigley and his colleagues do not provide this sort of information. Indeed, as far as I can determine, nowhere in the book do they even acknowledge that the analyses are based on a minority of the parish populations.

In general, Wrigley and his colleagues do not provide sufficient information to estimate the proportion of excluded cases in any particular analysis, but we do have a few clues. Table 7 provides estimates of inter-parish migration based on the percentage of Family Reconstitution Forms (FRFs) that could not be linked to baptisms and burials. The percentage of FRFs unlinked to a baptism is roughly the percentage of persons migrating between birth and marriage or childbirth, whichever is observed. Similarly, the percentage of FRFs unlinked to a burial is essentially equivalent to the percentage migrating between marriage or childbirth, whichever is observed, and death. Almost four out of five women and almost three-fourths of men, then, moved by the time that they reached

TABLE 7
Percentages of persons in English family reconstitutions who are apparent migrants, by timing of migration

	Females	*Males*
Percentage migrating before FRF	79.2	73.3
Percentage migrating after FRF	56.3	55.1

Source: Ruggles, 'Correcting sources of bias', 510.

their late twenties or early thirties; over half of both sexes also moved after that age. If one limits analysis to women born in the community, marrying there, and remaining there until at least age 50 – the population used for most measures of fertility – then reconstitution analyses must exclude 95.4 per cent of all FRFs.

Although precise figures are not available, these figures suggest that roughly 70 to 80 per cent of the population is excluded in the analysis of marriage, and over 95 per cent is excluded for most fertility measures. No hint of this extraordinary selectivity appears in the family reconstitution volume; my estimates are based on tabulations created for me in 1990 by Jim Oeppen. If the authors consistently provided the number of cases that underlie their calculations, we might be able to estimate the proportion of cases excluded from analysis for any particular measure. Unfortunately, the authors do not give the number of cases used for many of their measures, and I cannot even find a mention of the total numbers of baptisms, marriages, and deaths recorded on the FRFs. In the absence of such basic statistics, it is impossible to estimate the proportion of excluded cases.

As Roger Schofield pointed out in 1972, there is no single 'reconstitutable minority'.[14] Depending on the particular demographic measure, reconstitution methods exclude a widely varying proportion of the population. I see this variability of the universe across demographic measures as a real problem. Wrigley and his colleagues treat the data as a single coherent demographic system, but in fact they are analysing a variety of different populations depending on the particular demographic measure used. This would not create a problem if migrants were identical to non-migrants with respect to every aspect of their demographic behaviour, but we will never be able to determine if this is in fact the case. Patching together the demographic experience of different subpopulations could yield highly misleading results. It would be far better, I think, to base all the measures on the narrowest group: completed families, defined as those in which the marriage remains intact until the wife reaches age 50.

Then, at least, one has a clearly defined population, even if it may be a non-representative one. The problem with this approach is that – if I read the tables correctly – the entire set of 26 reconstitutions contains only 2,060 completed marriages, and represents perhaps 3 per cent of all Family Reconstitution Forms.[15] This is a slender reed on which to base 250 years of English population history.

A casual reader of *English Population History* would be completely unaware that most of the parish population is excluded from the analyses. The authors simply do not address the potential effects of selection bias resulting from migration. Given the length of the book and the great attention given to some other potential sources of error, this omission is surprising. However, in a recent article written after the book was published, Wrigley argues that selection bias resulting from migration is insignificant in the case of the English parishes. Wrigley maintains that internal evidence supports this claim:

> For some measures comparison is possible between those in a given parish who were born there, and those in the same parish who were born elsewhere, that is between migrant and non-migrant families. The levels of infant and child mortality in these two groups were virtually identical in the twenty-six parishes contributing data to the family reconstitution study. Similarly the average interval between births in the two groups was almost identical. If mortality in the first fifteen years of life and marital fertility were effectively the same in the two groups, it is probable that they were little different in other respects, since these are two of the most important measures helping to define their demography generally.[16]

Wrigley does not cite any source for his generalization about the similarity of mortality between migrants and non-migrants, and I assume it is based on an unpublished tabulation. He does, however, cite a table from *English Population History* to support his generalization that migrant and non-migrant birth intervals are essentially identical.[17] I have graphed a simplified version of that table in Figure 1. The figure does not support Wrigley's statement; in every period, non-migrants had longer birth intervals than did migrants. The average difference in birth intervals between the two groups is 1.2 months.[18] Although the authors do not provide the standard deviations or case counts that would be needed to carry out a formal test, given the large number of cases involved this difference is certain to be statistically significant. It is also substantively significant. Wrigley and his colleagues regard the 1.3 month drop in birth intervals between 1550–1619 and 1780–1837 as key evidence of rising fertility.[19] Given that the average gap between migrants and non-migrants is 1.2 months, it is hard to understand how Wrigley could regard the migrant and non-migrant intervals as 'almost identical'.

Ironically, I suspect that the differential in birth intervals shown in Figure 1 may not reflect a true difference in fertility between migrants and

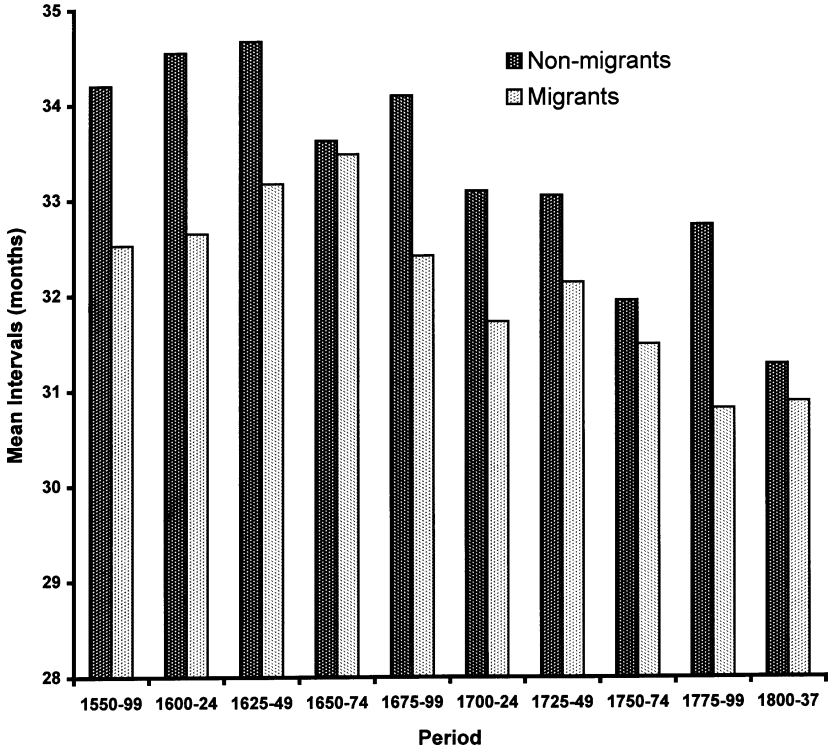

FIGURE 1. Mean intervals between successive births: intervals in which elder child is parity 2+ and survived infancy (based on figures in Table 7.35, pp. 438–9, in Wrigley et al., *English Population*).

non-migrants. More likely, I think, it is an artifact of a measurement problem known as 'migration censoring'. That issue is the subject of the next section.

CENSORING

In 1992, I wrote an article about censoring bias in English family reconstitution studies, and the authors of *English Population History* have responded to my analysis at length.[20] I am not persuaded by their discussion of migration censoring, for reasons I will explain. First, however, I must describe the nature of the censoring bias problem.

The purpose of the method of family reconstitution is to estimate appropriate denominators for the estimation of vital rates. For example, the number of births in many pre-industrial parishes is readily available from parish registers, but to translate the number of birth into a marital fertility rate we would need to know how many married women of childbearing age were present in the parish. Since no census data are

available for England before 1801, we have no means of estimating the number of married women of childbearing age. Family reconstitution studies link together the records of marriage, baptism, and burial. Thus, reconstitution allows us to define a population of women known to be present in the community from their marriage until at least age 50. Fertility rates can then be calculated directly for this subpopulation, since we know both the numerator and the denominator.

Unfortunately, the reconstitution methods developed by Henry to analyse French parish registers do not always translate well to England, for two main reasons. First, unlike the French marriage registers, the English ones do not include the ages of the bride and groom; thus, any analysis involving age must be linked to a baptism record as well as a marriage record. Second, the English population seems to have migrated across parish boundaries to a much greater extent than did the French. These limitations of the English parish registers magnify the problem of migration censoring.

Migration censoring occurs when, because of migration, the denominator for a vital event is specified incorrectly. Consider, for example, the calculation of age-specific marriage rates. Ordinarily, an age-specific marriage rate is defined as the number of marriages at a given age divided by the number of single persons of that age. In the English family reconstitutions, marriage age is known only for persons with a baptism record linked to a marriage record. Thus, to calculate an age-specific marriage rate in an English reconstitution, the numerator would be the number of linked persons marrying at each age, and the denominator would be the number of linked persons not yet married at each age. The problem arises because at any given age there are some persons present in the population who have not yet married and are 'at risk' of marriage, but who ultimately migrate out of the parish before they marry. The number of such eventual migrants present at each age cannot be estimated from reconstitution data, because we have no information on the date of migration. Thus, because of out-migration, the denominator for the calculation of age-specific marriage rates will always be understated, and the rates will therefore be underestimated. If marriage ages are calculated from underestimated age-specific marriage rates, they too will be understated.

The authors of *English Population History* do not actually calculate age-specific marriage rates. Instead, they simply take the average age at marriage for all marriages linked to a baptism record. This is a poor measure for two reasons. First, it is influenced by the age composition of the population and is therefore not comparable over time or between populations. Second, and far more serious, it is subject to severe migration

censoring. If there are any local-born persons in the population of marriageable age who have not yet married but who eventually marry in a different parish, the population at risk of marrying is understated and marriage age will be underestimated.

Thinking of this problem in terms of denominators and population at risk may be confusing for non-demographers. There is a more intuitive way of thinking about it: the later a person marries, the greater the odds that he or she will have migrated before marrying, and thus will be excluded from analysis. The problem does not occur in French reconstitutions because there the marriage records include information on age, and therefore the migrants do not have to be excluded.

The same basic problem can affect the analysis of birth intervals. If a mother migrates between the birth of two successive children, that birth interval will be excluded from reconstitution analysis because the two baptisms will be recorded in separate parish registers. Migration is much more likely to take place during a long birth interval than during a short one; all things being equal, one would expect a six-year birth interval to be excluded by migration about six times as frequently as a one-year interval. Therefore, because of migration censoring, if you include any eventual migrants in the analysis then birth intervals will be biased downwards. This, I think, is the explanation for the pattern shown in Figure 1. The only practical means of addressing the problem is to restrict analysis of birth intervals to completed families in which the mother remains in the parish until age 50. This means rejecting approximately 97 per cent of Family Reconstitution Forms and basing the entire analysis of fertility on the experience of 2,060 women.

Migration censoring also affects the analysis of adult mortality. This issue has been a recognized problem from the very beginning of family reconstitution, and like all family reconstitution studies the Cambridge authors make an attempt to adjust for it. I do not think those adjustments are adequate, for reasons I will explain presently.

Since I raised the problem of migration censoring in 1992, a number of historical demographers and statisticians have addressed the issue.[21] Most of the work has attempted to measure empirically the magnitude of migration censoring in various different pre-industrial populations by comparing the marriage age or mortality of migrants with that of non-migrants. The results have been mixed; some authors have found minimal censoring effects, and others have found large effects. But there is a basic problem with this approach: it is impossible to distinguish censoring bias from selection bias. Thus, for example, if one finds that the marriages of non-migrants in a population occur at the same average age as the marriages of migrants, it might be because selection biases the marriage

TABLE 8
Revision history of estimates of the effects of migration censoring on age at first marriage: English reconstitution parishes

Date of estimate	*Conventional measure*	*Unbiased measure*	*Difference*
May 1990	25.3	27.7	2.4
September 1990	25.5	27.1	1.6
January 1991	25.6	26.9	1.3
January 1994	26.0	26.8	0.8

Sources: Jim Oeppen, personal communications; E. A. Wrigley, 'The effect of migration', 86.

age of non-migrants upwards while simultaneously censoring biases it downwards. Therefore it is extremely difficult to develop an empirical test for censoring bias even when you have information available on the behaviour of migrants. A conclusive test for censoring problems would require data on age at migration for each individual in the population.

Wrigley and his colleagues argue that in the English case there is no migration censoring of marriage age. To explain how they reached this extraordinary conclusion, I must provide some background. I started working on the problem of migration censoring in the Spring of 1990 while on sabbatical at the Cambridge Group. To estimate the potential magnitude of the problem and to test alternate solutions, I used a microsimulation model. I found that, given the range of English migration levels, one would expect, *ceteris paribus*, that the standard family reconstitution methods would understate mean age at marriage by an average of two to four years. I recommended that to avoid the bias, English family reconstitutions should limit the analysis of marriage age to persons who remained in the community until the age of 50.

In May 1990 I asked Jim Oeppen, one of the co-authors of *English Population History*, to tabulate the unbiased measure of marriage for women who remain in the parish until age 50. According to Oeppen's tabulation, by the conventional measure marriage age in the reconstitution parishes was 25.3, compared to 27.7 according to the unbiased measure.[22] This 2.4 year difference was very close to the difference predicted by the microsimulation. I regarded it as confirmation of the effects of migration censoring and included this statistic when I submitted my paper on the topic for publication to *Population Studies*.

During the next few years, Oeppen and Wrigley repeatedly revised these figures; the most significant revisions are given in Table 8. For the first

revision, made shortly after I submitted the paper for publication, Oeppen and Wrigley added several parishes and removed several that were considered of low quality. According to the new tabulation, the difference between the conventional measure and the unbiased measure came to only 1.6 years. Shortly before the paper went to press, I received a second revision from Cambridge: some of the periods of coverage in some of the reconstitutions were of low quality, and therefore had to be cut from the analysis. The new estimates showed a difference of only 1.3 years between the conventional measure and the reconstitution measure, and this was the figure I published in 1992. But the revisions were not over. In 1994, Wrigley published a paper on the issue in *Population Studies*, in which he argued that some additional refinements of the data together with correction of some tabulation procedures had the effect of reducing the difference between the conventional and unbiased measures still further, to only 0.8 years. Wrigley maintained that such a modest difference would be expected from the effects of mortality alone; thus, the English population was not subject to migration censoring.[23]

In fact, Wrigley misunderstood my reasons for comparing the biased and unbiased measures of marriage age. It was intended as an illustration of the problem, not as a test of whether or not a problem exists. Even if there were no differences whatsoever between the conventional measure and the unbiased measure, that would not demonstrate that England was immune from migration censoring of marriage age. The two measures refer to dramatically different populations; the unbiased measure allows use of only a fifth of the cases used in the conventional measure, and 5 per cent of all Family Reconstitution Forms. If this tiny subpopulation of lifetime non-migrants actually married at a younger age than the rest of the population, then we would be unable to observe any effect of migration censoring. The unbiased measure is unbiased only with respect to migration censoring; it is still subject to severe selection bias. If both the conventional and the unbiased measures are distorted – one from censoring and the other from selection – the fact that the difference between them is modest is of little comfort.

As long as there are any unmarried eventual migrants in the population at risk to be married, then there must be some degree of downward bias in marriage age according to the conventional measure. Wrigley and Oeppen whittled away my original illustration of migration censoring, so I now provide a more persuasive one. Table 9 presents regression models of age at first marriage across the 26 reconstitution parishes. The data are taken from Wrigley's 1994 article on migration censoring, and thus represent the final revision of the marriage data shown in Table 8. The dependent variable in Models 1 and 2 is the conventional measure of age

TABLE 9
Regressions of age at marriage on selected community characteristics: English family reconstitutions

A. Conventional measure of marriage age

	Model 1			*Model 2*		
	B	*SE*	*Sig.*	*B*	*SE*	*Sig.*
Proportion migrant	−8.979	3.040	0.007	−9.263	2.885	0.003
Population density per acre				−1.029	0.574	0.087
Agricultural community				−0.829	0.422	0.062
Constant	33.357	2.516	0.000	34.563	1.04	0.000
R-square	0.267			0.401		
N	26			26		

B. Unbiased measure of marriage age

	Model 3			*Model 4*		
	B	*SE*	*Sig.*	*B*	*SE*	*Sig.*
Proportion migrant	−1.727	5.617	0.761	−2.647	5.568	0.639
Population density per acre				−1.473	1.107	0.197
Agricultural community				−1.181	0.815	0.161
Constant	28.575	4.649	0.000	30.298	4.71	0.000
R-square	0.004			0.113		
N	26			26		

Source: Wrigley, 'The effect of migration', 86.

at first marriage, and the dependent variable in Models 3 and 4 is the unbiased measure, which is restricted to women who remain in the parish until the age of 50. The proportion migrant is measured as

$$1-\frac{\textit{female baptisms linked to marriages}}{\textit{total female baptisms}}$$

This measure is influenced by mortality as well as by migration, but the bulk of variation in the measure across parishes doubtless results from migration.

As shown in Model 1, migration is an excellent predictor of the conventional measure of marriage age. Persons in high-migration parishes married substantially earlier than those in low-migration parishes, just as one would predict if the conventional measure were biased by migration censoring. The significance is very high: even though there are only 26 cases, there are only seven chances in a thousand that this result can be ascribed to chance. Even more striking is the close correspondence

between the coefficient for migration in the regression model compared with the results of the microsimulation model that I published in 1992. The microsimulation predicted that women in the highest migration parishes would marry 1.9 years earlier than women in the lowest migration parishes, all things being equal. If we use the identical migration parameters to predict marriage age using the regression model, the difference between the highest and lowest migration parishes also comes to 1.9 years. Thus, the empirical evidence from the 26 family reconstitutions shows an identical sensitivity of migration level to marriage age as does the theoretical calculation.

It is conceivable that the close agreement between the family reconstitutions and the microsimulation is purely coincidental. The apparent strong association between migration and marriage age could be the result of intervening variables. In particular, one might suspect that migration would be highest in the largest places, and that marriage could occur earlier in such economically developed places because wage workers reached peak earnings early in life. To test this hypothesis, I included two additional variables in Model 2. Population density is the same measure described above in the section on representativeness, and therefore refers to parish density in 1801. Agricultural community is a dummy variable that identifies those parishes classified by the Cambridge Group as predominantly agricultural. The authors also identify trading and manufacturing communities, but since those variables had no effect I eliminated them from the model. Both density and agriculture proved marginally significant at the .10 level, but they did not reduce the powerful effect of migration; on the contrary, the effect of migration is actually slightly stronger in Model 2 than in Model 1.

Models 3 and 4 are the same as Models 1 and 2, except that the dependent variable is the unbiased measure of marriage age rather than the conventional measure. The proportion migrant has no significant effect on the unbiased measure of marriage age. This reinforces the conclusion that the powerful effect of migration on the conventional measure of marriage is primarily the result of migration censoring.

This analysis indicates that the severity of migration censoring of marriage age in the reconstituted populations is consistent with the theoretical predictions I published in 1992. If those predictions are correct, then the estimates of marriage age presented in *English Population History* are two to four years too low. In the absence of information on date of out-migration for the never-married population, it is impossible to be certain about the severity of the problem. However, I think we can be reasonably certain that the conventional measure adopted by Wrigley and his co-authors is significantly biased.

As noted, when it comes to the estimation of adult mortality the authors acknowledge the issue of censoring and take steps to correct it. They adopt a procedure originally suggested by Alain Blum in 1989. Unlike Blum and most other previous analysts, Wrigley and his colleagues do not provide analyses to test the sensitivity of their estimates to alternate assumptions. Essentially, their procedure estimates the period during which individuals remain in the parish after their last observed vital event (marriage, birth of child, death of child, or widowhood) as one-half of the average interval between successive demographic events among persons who remain in the community. The problem with this method, as I pointed out in 1992, is that those intervals are themselves subject to migration censoring, and thus understate exposure. The authors are clearly aware of this problem; they write that 'on average the intervals will be longer among leavers than among stayers because the longer the interval to the next event, the more likely it is that the individual in question will have left the parish'.[24]

In 1992 I proposed two alternate methods for correcting the bias in Blum's approach. The authors do not explain why they chose instead to use the method that I demonstrated to be biased. They write that although Blum's approach 'is open to criticism, it is no easy matter to devise an alternative that is unequivocally better'.[25] They do not mention the improved methods I proposed in 1992. Based on my theoretical analysis together with the data provided in Appendix 6 of *English Population History*, I estimate that on average the authors added only about 40 per cent of the additional needed exposure to risk of death.[26] As a result, I expect that the estimates given for life expectancy at age 25 are biased downwards by an average of 1.5 to 6.5 years, depending on the prevailing level of migration. It is possible, however, that there may be a countervailing selection bias.

In sum then, because of censoring problems *English Population History* significantly understates marriage age, life expectancy, and birth intervals. To the extent that there was significant change in migration over time or between parishes, this problem also affects the analysis of trends and differentials in marriage age, mortality, and fertility.

LINKAGE FAILURES AND UNDER-REGISTRATION OF VITAL EVENTS

Family reconstitution is based on linkage of records of baptisms, marriages, and burials. Record linkage is tricky business, and there are many possible ways it can go wrong. The previous sections cover the implications of failures to link vital events because of migration; in this

section I will briefly discuss the implications of failures to link for other reasons.

Part of the failure to link is the consequence of limitations of the records themselves. There may be damage to the centuries-old registers – from water, rodents, insects, mold, and simple wear-and-tear – that render some entries partially or wholly illegible. Names can change, and on occasion the parish registers may record names incorrectly. Some individuals on the fringes of Anglican society had religious ceremonies for some vital events but not for others. And, perhaps most important, the local religious authority might sporadically neglect to record some events.

Additional linkage failures are introduced during the process of converting the records into machine-readable form. In the case of the English family reconstitutions, the entries were transcribed by hand in two separate stages before being entered, again by hand, into the computer. There was no verification at any of these stages. My own experience with professional data-entry operators suggests that one might expect an 0.2 to 0.4 per cent error rate for each transcription.

Any failure to link a baptism or a child death to the appropriate parental Family Reconstitution Form will result in underestimated measures of fertility and child mortality. Fertility, for example, is measured as the number of married women of each age who are linked to a baptism divided by the overall number of married women of that age; fertility measures are thus biased downwards in direct proportion to the frequency of failed baptism links. Wrigley has compared the problem to under-enumeration in modern censuses, but in fact it is much more serious.[27] Missing cases in a census only distort estimates of demographic behaviour to the extent that the unenumerated population differs from the enumerated population. For example, if a census misses 10 per cent of the population and the unenumerated population has 10 per cent lower fertility than the enumerated population, then overall estimates of fertility will be in error by about 1 per cent. By contrast, if in a family reconstitution 10 per cent of baptism links are missed, then fertility will be underestimated by fully 10 per cent.

Louis Henry recongized the seriousness of the problem, and developed a set of methods to adjust for the under-registration of births and deaths. In the case of Crulai, for example, Henry adjusted infant mortality upwards by 11.5 per cent to account for defective registration.[28]

Wrigley and his colleagues on several occasions acknowledge that the English parish registers are far inferior to those of the French; they refer to the English registers as 'a low grade of reconstitution ore, containing much dross'.[29] They carry out several tests designed to detect deficiencies in registration or linking, and conclude that their data 'are unlikely to be

seriously defective'.[30] They therefore make no adjustments to their fertility or child mortality data whatsoever.

I am not equipped to evaluate the evidence on the reliability of registration and record-linkage. I am nonetheless skeptical. The authors implicitly assume that there are zero linkage failures and zero under-registration. Although I have never carried out a family reconstitution, I have enough experience with historical record linkage to know that even under the most favourable circumstances a few percentage points of loss are inevitable. Thus, I suspect that the figures for fertility and child mortality presented in *English Population History* are generally biased downwards by linkage failures.

RANDOM ERROR

The data from the 26 parishes are not independent random samples, but they are still subject to random error. Demographic processes are stochastic, and they approximate random behaviour. Thus, the precision of demographic estimates based on the family reconstitutions is proportional to the square root of the number of cases used. It is therefore appropriate to calculate confidence intervals for the demographic estimates and to carry out conventional tests of the statistical significance of trends and differentials.

The authors provide no confidence intervals and carry out virtually no tests of statistical significance, despite the tiny number of cases that underlie many of their results. Even more problematic, they do not provide sufficient information for other researchers to do their own significance testing. The majority of the measures given in the book are expressed as means, but as far as I can tell the authors do not provide a single standard deviation, so no tests are possible in these cases.

Even where the measures are percentages, rates, or proportions, statistical tests are often impossible because the tables leave out the number of cases. In a few cases, cells with few cases are simply omitted. For example, Table 5.18 leaves cells blank when they are based on fewer than 25 cases. In other instances, a total number of cases is provided, but not the number of cases that underlie each cell. Thus, in Table 7.35 we get the total number of cases for each type of marriage, but the table is broken down by time period as well as marriage types and there is no way to determine the number of cases in each time period. But the most common procedure was simply to make no reference whatsoever to the number of cases; by my quick count, only about 40 per cent of the 121 tables in the book include any indication of the size of the underlying population. In some cases the number of cases may be calculable from a different table,

but I was unable to locate appropriate case counts for most of the tables I studied closely. Moreover, much information is presented only in the form of graphs, and these also provide no indication of number of cases.

The limitations might be acceptable in a trade book aimed at a popular audience. *English Population History*, however, is clearly aimed at an audience of specialists. It aims to be an authoritative text in its field. Thus, it should provide a higher standard of documentation.

CONCLUSION

The authors stress that by surprising good fortune the potential biases and pitfalls of family reconstitution do not exist in the English case. They argue that the 26 communities are remarkably representative of England as a whole in all periods. They argue that there is no selection bias caused by eliminating, for most measures, between 70 and 97 per cent of the Family Reconstitution Forms from the analysis. They argue that, in the English case, there is no censoring of age at marriage. They maintain that the registration of births and deaths is so good that they need apply no adjustment for under-registration or other linking failures.

I have argued that these claims are unconvincing. I think we should be wary of the precision of virtually every estimate in the book. Given the complex combination of potential biases – non-representativeness of the parishes, selection bias, censoring, and under-registration – we in general cannot be certain of the net direction or magnitude of error for any particular measure. I do not mean that the demographic estimates presented in *English Population History* are necessarily wrong; I am just unpersuaded that they are right.

In a recent article, Wrigley indicates that the machine-readable reconstitution data will soon be made publicly available. He notes that 'the advent of electronic forms of data storage and retrieval will make it easy for others to satisfy themselves with a variety of tests if they so wish'.[31] I applaud this initiative. Public release of the data is entirely consistent with the generosity and support that the Cambridge Group has shown the profession since the late 1960s. It will stimulate new research in an area that might otherwise atrophy in the wake of *English Population History*. It will greatly mitigate many of the criticisms I have made about documentation and methods. And it will ensure a permanent active legacy for the creators of the data. Ideally, the authors will release all years of all 32 parishes, even those that are considered inferior. If the data are made freely available through the Data Archive at Essex University, they will inevitably become the basis for many doctoral dissertations at universities around the world.

It is much easier to criticize a study like this one than actually to do the work. Whatever limitations I have pointed out, I want to make clear my admiration for the tenacity and perseverance of the authors in the face of challenging data limitations. Moreover, I want to be entirely clear that the biases I have described do not invalidate the study as a whole. Much of the book is not concerned with providing national estimates of the levels of fertility, mortality, or marriage age; rather, it focuses on the relationships among different aspects of demographic behaviour. In most cases, these analyses will stand even if the underlying evidence is not really representative of the English population.

I also have no special quarrel with the general outline of English demographic history presented by the authors. In general, the story they tell is much the same as the one they proposed in 1981 in *The Population History of England*, except that now they postulate a modest rise in marital fertility during the eighteenth century. While I view the scenario they present as plausible and perhaps correct, I do not regard it as proven. There are simply too many potential sources of error – in both the family reconstitutions and in the aggregative analysis – to be certain of the motors of population change in the pre-statistical era. Thus, I think we should view the Cambridge Group findings as the leading hypothetical model of English population history, and should work to develop appropriate methods and materials for testing it.

ENDNOTES

1 E. A. Wrigley, R. S. Davies, J. E. Oeppen, and R. S. Schofield, *English population history from family reconstitution 1580–1837* (Cambridge, 1997), 8.
2 Wrigley et al., *English population*, 353.
3 Ibid., 33–5.
4 E. A. Wrigley and R. S. Schofield, *The population history of England, 1541–1871* (Cambridge, 1981). Wrigley and Schofield did provide partial control for this problem by standardizing the 404 parishes according to the distribution of parish sizes in England as a whole in 1811. Given the wide differential in growth rates of different parishes, however, this would not prevent biases in earlier periods.
5 Wrigley et al., *English population*, 202.
6 Ibid., 70.
7 Wrigley and Schofield, *Population history*, passim.
8 Table 3.3 in the book also provides figures for England outside of London. By stressing the comparison with England outside of London, the authors almost seem to suggest that they intend the statistical generalizations presented in the book to apply only to England outside of London. They never explicitly state this, however, and on numerous occasions they assert that the numbers are representative of England as a whole.
9 Wrigley et al., *English population*, 70.
10 Wrigley and Schofield, *Population history*, 537–60, column 4.
11 The crude weighting system was also a mistake. It would have been far preferable, I

think, if the authors had abandoned weights altogether and also abandoned the claim that their results are representative. Alternatively, they could have developed a more sophisticated weighting system. In addition to correcting the problem of occupational distribution more effectively, a better weighting system could correct the over-representation of high-density parishes, and could also adjust for known demographic differences in every year between the 26 reconstitution parishes and the larger group of 404 parishes previously studied by the same authors. Thus, a full set of weights could have made the data representative of England with respect to occupation in 1831, density in 1801, and number of births, deaths, and marriages in all years. As a side benefit, such a weighting system would eliminate the need for the clumsy system of four overlapping chronological groups of parishes.

12 T. H. Hollingsworth, *Historical demography* (Ithaca, NY, 1969), 181–96; R. S. Schofield, 'Historical demography: some possibilities and some limitations', *Transactions of the Royal Historical Society*, 5th ser., **xxi** (1971), 119–32; Schofield, 'Representativeness and family reconstitution', *Annales de Démographie Historique* **1972** (Paris, 1972), 121–5; Jacques Dupâquier, 'Problèmes de représentativité dans les études fondées sur la reconstitution des familles', *Annales de Démographie Historique* **1972** (Paris, 1972), 82–91; Dezso Danyi, 'La migration et les méthodes nominatives: l'exemple Hongrois', *Annales de Démographie Historique* **1972** (Paris, 1972), 69–82; T. H. Hollingsworth et al., 'Discussion: representativeness and family reconstitution', *Annales de Démographie Historique* **1972** (Paris, 1972), 127–46; D. Levine, 'The reliability of parochial registration and the representativeness of family reconstitution', *Population Studies* **30** (1976), 107–22; J. Knodel and E. Shorter, 'The reliability of family reconstitution data in a German village', *Annales de Démographie Historique* **1976** (Paris, 1976), 115–53; S. Akerman, 'An evaluation of the family reconstitution technique', *Scandinavian Economic History Review* **XXV** (1977); S. L. Norton, 'The vital question: are reconstituted families representative of the general population?', in B. Dyke and W. T. Morrill eds., *Genealogical Demography* (New York, 1980); J. Rogers, 'Family reconstitution: new information or misinformation?', Reports from the Family History Group, 7 (1988), Department of History, Uppsala University; John Knodel, *Demographic behavior in the past: a study of fourteen German village populations in the eighteenth and nineteenth centuries* (Cambridge, 1988), 461–502.

13 See for example Knodel, *Demographic behavior*; Norton, 'The vital question'.

14 Schofield, 'Representativeness and family reconstitution', 121.

15 The number of completed families is calculated from Wrigley et al., *English population*, 414–15. The estimate of 3 per cent is based on a total count of FRFs prepared for me in 1990 by Jim Oeppen.

16 E. A. Wrigley, 'How reliable is our knowledge of the demographic characteristics of the English population in the early modern period?', *The Historical Journal* **40**, 3 (1997), 578.

17 Wrigley et al., *English population*, Table 7.35, pages 438–439.

18 The data in Figure 1 are an approximation; the authors do not provide sufficient information to calculate the mean birth interval for migrants within specific time periods, so I was forced to assume that there was no chronological change in the type of migration. This is unlikely to materially affect the results.

19 Wrigley et al., *English population*, 449.

20 Steven Ruggles, 'Migration, marriage, and mortality: correcting sources of bias in English family reconstitution studies', *Population Studies* **46** (1992), 507–22. The main response is E. A. Wrigley, 'The effect of migration on the estimation of marriage age in family reconstitution studies', *Population Studies* **48** (1994), 81–97.

21 J. David Hacker, 'Trends and determinants of adult mortality in early New England: reconciling old and new evidence from the long eighteenth century', *Social Science History* **21**, 4 (1997), 481–519; Alice B. Kasakoff and J. W. Adams, 'The effect of migration on ages at vital events: a critique of family reconstitution in historical demography', *European Journal of Population* **11** (1995), 199–242; Bertrand Desjardins, 'Bias in age at marriage in family reconstitutions: evidence from French-Canadian data', *Population Studies* **49** (1995), 165–9; Wrigley, 'The effect of migration' 86; E. Voland and R. I. M. Dunbar, 'The impact of social status and migration on female age at marriage in an historical population in north-west Germany', *Journal of Biosocial Science* **29** (1997), 355–60; R. D. Gill, 'Nonparametric estimation under censoring and passive registration', *Statistica Neerlandica* **51** (1997), 35–54.

22 The difference between the two measures was actually even greater for the first run Oeppen undertook, but that turned out to be the result of a truncation problem.

23 Mortality censoring works much like migration censoring: some people who would ordinarily marry late in life fail to do so because they die before they get a chance to marry. I estimated that under pre-industrial mortality conditions this would bias marriage age downwards somewhere in the range of 0.5 to 0.7 years. I have argued that age-independent, mortality-free measures of marriage age are highly desirable. Such measures are widely used in demography; they include Singulate Mean Age at Marriage (SMAM), the indirect median age at marriage used y the US Census Bureau, and mean marriage age calculated from marital-status life tables, as well as the unbiased reconstitution measure of marriage age I proposed. See Ruggles, 'Migration, marriage, and mortality', 511–12.

24 Wrigley et al., *English population*, 584.

25 Ibid., 584.

26 The estimate of 40 per cent underestimated exposure is based on comparison of the exposure that would be added by the method given in Ruggles, 'Migration, marriage, and mortality', 517–20, with the figures given in Appendix 6 of Wrigley et al., *English population*.

27 Wrigley, 'How reliable is our knowledge', 576–7.

28 Cited in Wrigley et al., *English population*, 101.

29 Ibid., 74.

30 Ibid., 118.

31 Wrigley, 'How reliable is our knowledge', 595.