THE POPULATION HISTORY OF ENGLAND

1541–1871

The Population History of England

1541–1871

A RECONSTRUCTION

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with contributions from Ronald Lee and Jim Oeppen

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To the local population historians of England

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An introductory note

The reprinting of *The population history of England* seven years after its first publication affords us an opportunity to respond to some of the comments and criticisms that have been made about the book, and to draw attention to writings published since 1981 that modify or extend the analyses and arguments that we then advanced. The list of publications at the end of this essay, though selective, is evidence of the interest aroused by the book; it includes the more important published reviews and the articles which in our judgement might be consulted with the greatest benefit by a reader who wished to discover how the book has been received by those best able to probe its strengths and weaknesses, and subject its findings to further critical interpretation. Several of them were included in a special issue of the *Journal of Interdisciplinary History* that we edited, and which appeared in spring 1985.[1]

There are three major aspects to any judgement of the validity of the work embodied in *The population history of England*; the accuracy of the empirical data employed, the appropriateness of the techniques of analysis used, and the schemes of interpretation offered. We shall say something about each in turn.

The data

The data were the entries of baptisms, burials and marriages recorded in 404 Anglican parish registers. Because the registers were not a random sample of all Anglican registers, because baptisms and burials cannot be equated with births and deaths, because more and more of the population ceased to be conforming Anglicans, and for many other reasons, the original data, consisting of about three and half million monthly totals of the events listed in the registers, required much manipulation before they could plausibly be represented as national totals of vital events. A description of the work involved took up the first five chapters of the book. Because the conversion process was so important and so complex, we set out the steps in great detail, providing full numerical information (see appendix 4). Our intention was to make it possible for anyone who demurred at any of our correction factors to produce alternative estimates embodying different assumptions. In the event, although some scholars have emphasized the complexity and, at some junctures, the rather arbitrary nature of the correction procedures that we used, the resulting annual totals of births,

1. The issue has been re-published in book form by the Cambridge University Press, under the title of *Population and economy: population and history from the traditional to the modern world*, edited by R. I. Rotberg and T. K. Rabb.

deaths and marriages have been widely treated as acceptable. Lindert, however, has suggested an alternative birth series that would, if accepted, substantially change the population history of England in the later eighteenth century.[2]

It is a striking feature of the fertility estimates arising from the birth series in this book that the gross reproduction rate rises to a very high peak early in the nineteenth century. During this period the national totals of Anglican baptisms are very substantially inflated to offset the effects of the several causes of discrepancy between the number of baptisms registered and the total of births taking place. The conclusion that fertility reached a high peak in the second decade of the nineteenth century is not in doubt. The information about age structure contained in the 1821 and 1841 censuses pinpoints fairly precisely the size of the birth cohorts in the early nineteenth century. Nor does Lindert challenge our estimates for this period. He does, however, suggest that the steep rise in birth totals and fertility rates in the later eighteenth century resulting from our birth total series (see, for example, fig. 7.6) was probably mistaken. His alternative series differs very little from ours after 1800 but his estimates of annual totals of births at earlier dates are much larger.[3]

Lindert's first criticism of our series related to the apparently cavalier way in which we had used the age data from nineteenth-century censuses to produce estimates of birth cohort totals earlier in that century, but had not used the same method to construct estimates for earlier decades. He argued that if this had been done the birth cohort totals for decades in the mid-eighteenth century would have had to be substantially larger than those used in this book. However, to derive mid-eighteenth-century birth cohorts from survivors in the nineteenth-century censuses means using the recorded totals of people at advanced ages and inflating them suitably. We argued strongly that the age data for the older age groups become progressively less trustworthy with rising age, until above the age of 70 very substantial corrections to the published totals are necessary. In extreme old age the inaccuracy was striking. Above the age of 90 the early census figures must be reduced by more than half to offset overcounting. Subsequent work by Lee and Lam has confirmed the presence of a substantial overestimate of the numbers of the elderly.[4] If the census totals of those claiming to be of advanced age are reduced to more plausible levels, the figures we estimated for birth cohorts in the mid-eighteenth century are in fact consistent with the age data from the censuses.

Lindert further argued that it was appropriate to test the plausibility of the birth series by modelling fertility behaviour as a function of real wage trends. He considered that the relationship between economic trends and fertility behaviour itself changed early in the nineteenth century, so that the model should be differently specified before and after 1815. If it were correctly specified, the error term in the equation would not be significantly different from zero. On the other hand, if the error term were significantly different from zero, it would suggest that the birth totals had been incorrectly estimated. The results of implementing the model persuaded Lindert that the post-1815 birth totals were substantially correct, but that the earlier totals were too low by a substantial margin.

2. Lindert, 'English living standards'.
3. *Ibid.*, fig. 1, p. 145.
4. Lee and Lam, 'Age distribution'.

The force of this test depends, of course, on the validity of the formulation of the model, as indeed Lindert himself stressed.[5] In particular the test would be inappropriate if the relationship between economic circumstances and fertility were not adequately captured by the variables used in the model, or if the relationship changed over time in ways not incorporated in the model. There is reason to think that Lindert's model is too simple to do justice to reality. For example, it does not take account of independent influences on fertility such as those related to the ideology of marriage or family formation, or those arising from changes in occupational structure. In so far as it fails to capture reality, any discrepancy between its predictions and our series of births may reflect its own inadequacy rather than cast doubt on the plausibility of the birth series.

Lindert went on to propose an alternative series in which the birth totals at earlier dates were substantially increased. A difficulty with inflating birth totals in this way is that either the death totals must be raised commensurately, or the net emigration totals will rise dramatically. There seems no warrant for the latter as a plausible historical scenario. The former would imply that the gap between recorded burials and deaths was massively larger than has previously been supposed. While this cannot be ruled out *a priori*, it would be very surprising if such a serious breakdown in registration coverage had escaped all comment at the time. An inflation of earlier burial totals would also imply a substantial fall in mortality in the eighteenth century. However, the results of family reconstitution studies, based on independent parish register data, are broadly consistent with the fertility and mortality findings of *The population history of England*, and do not lend any support to the view that mortality fell precipitously during this period.[6] The scale of the mortality change implied by Lindert's revision is substantial. If a series of death totals is constructed to match Lindert's new birth series in the sense of preserving the original estimate of population size at various dates, back projection finds a massive increase in expectation of life of nine years between 1780 and 1830. Since independent evidence of the occurrence of so great a transformation of mortality during this period is conspicuously lacking, we conclude that Lindert's alternative birth series is unlikely to be well founded.

To prefer the original estimates is not, however, to assert either their perfect accuracy or their immunity to improvement. It may be, for example, that the assumption of comparatively complete coverage in the early decades of baptism registration is unduly optimistic. All registration systems have deficiencies. The body of Anglican clergy cannot be supposed to have been free from the weaknesses that have affected the performance of officials administering other systems. But it is well to remember that mistakes and deficiencies did not all lie in one side of the ledger. Some events escaped registration; others are recorded more than once in the same register or in several registers. There is plenty of scope for further work on the issue of registration coverage, and it may well ultimately justify alteration to the totals which we derived, but at present it would be premature to do so.

Similarly the other steps in converting raw register totals into the national estimates will bear further scrutiny and may need amendment. For example,

5. Lindert, 'English living standards', p. 144.
6. Wrigley and Schofield, 'English population history from family reconsitution', pp. 175–83.

Hodges has pointed out that Rickman's figures for the baptism and burial totals for the year 1700 in the London Bills of Mortality, on which we relied, in fact relate to the year 1701.[7] We, therefore, inadvertently used figures from the wrong year in correcting the figures for 1700 that Rickman reported as recorded in the parish registers. Consequently, our baptism and burial totals for London are likely to be 5.3 and 6.7 per cent, respectively, below the true figures during the period 1660–1700, and the figures for the period before 1660 and for the decade 1700–9 will also be somewhat affected. However, since events in London comprised only 12.6 per cent of national events in the period 1675–99, the effect of this error on our estimates of national totals is less serious than might appear at first sight. Moreover, we anticipated that our corrections to the data might well contain errors, and might legitimately be open to challenge on substantive grounds. Accordingly, we repeated some of the key demographic estimations using six different series of births and deaths that incorporated much greater departures from our preferred series than are at issue here. As is reported in chapter 7, the results generally lay within a range of about 3 per cent either side of those obtained with our preferred series.

The main outlines of the population history of England seem to be remarkably robust in the face of quite large errors or uncertainty in the data. At present, therefore, we can see no persuasive reasons for making any significant revisions to our estimates of the totals of vital events or to the picture of the course of population change that we based upon them.

Methods of analysis

In the absence of a census, as was the case before 1801, totals of events can only be converted into estimates of standard measures of fertility and mortality if a way can be found to estimate the stock of individuals at risk (i.e. the size and age structure of the population) from the flows of events themselves. Some years ago Lee showed how this could be achieved in the case where migration was zero, or could be assumed to be constant.[8] We felt that this assumption was too restrictive, suspecting that migration was likely to have been far from negligible in England in the past, and to have varied considerably in importance between the sixteenth and nineteenth centuries. Accordingly, we devised an alternative approach to the problem, which we called 'back projection' because it proceeded by successively back-dating and revising the known age structure of the 1871 census, thus deriving earlier 'censuses' at five-year intervals. This was achieved by taking into account the flows of previously occurring events. Our procedure followed Lee's 'inverse projection' in using model mortality schedules to allocate deaths by age, but it went further in finding a way to deduce the magnitude of the unrecorded flows of migrants of different ages from inconsistencies between the population age structures and the intervening flows of births and deaths.

Back projection was a complex system, an example of a technique whose operation was made feasible only by the advent of high-speed computing. Although its nature and operation were described in some detail, it cannot have

7. Hodges, 'The population of London', p. 1.
8. Lee, 'Estimating series of vital rates'.

been easy for most readers to make a considered judgement about its merits, or about its defects or limitations. It was therefore especially fortunate that Lee, better positioned to make such a judgement than anyone else, should have taken the trouble to subject back projection to a thorough examination. His verdict was that 'I see no reason to alter any of the estimates in their volume. On the other hand, I do not believe that those estimates should be taken to provide *independent* evidence about the general level of population size and vital rates in the early eighteenth century or before.'[9]

Lee was troubled by two problems that he thought must prevent back projection from providing independent evidence for a period in which only totals of births and deaths were known. The problems were those arising from 'under-identification', and from 'weak ergodicity'. The former refers to the existence of more unknowns than equations in the estimation procedures. Lee's objection is undoubtedly formally correct, But because he believed that back projection was under-identified in this sense, he argued that, although the method could produce population estimates, 'it achieved this through the use of additional *ad hoc* relations, such as the initial assumption about the ratio of the sizes in the oldest age group in adjacent periods'.[10] Lee's second objection to back projection, 'weak ergodicity', refers to the tendency of a population to 'forget' any particular age structure as time elapses when moving forward or backward from a date at which the age structure is known. The latter consideration led Lee to assert that 'consequently very different demographic pasts could be consistent with the same birth and death series, and the same terminal age distribution'.[11]

The issues that Lee raised are complex and cannot be dealt with briefly. Nevertheless, work carried out recently by Oeppen, and shortly to be published, has greatly clarified the nature of back projection and its logical status.[12] Oeppen stresses the importance of recent advances in the understanding of the non-stable dynamics of open population systems which have enabled him to show that inverse projection and back projection are variants of a more general class of problem in demographic estimation, and can be subsumed within a more inclusive system that he terms generalized inverse projection. Once *additional* constraints have been placed on the demographic processes, solutions can be obtained using standard numerical estimation algorithms. When back projection is viewed in these terms, it becomes clear that the algorithm does not suffer from under-identification.

Furthermore, both back projection and generalized inverse projection impose significant constraints on migration which enable them to overcome the problems raised by ergodicity and extract a consistent progression of population age structures through time from the data available. The tendency for a population to 'forget' its original age structure through the operation of ergodicity is the result of the repeated application of fertility and mortality *rates*. But what are fixed in a projection exercise are the *numbers* of vital events. The only way in which different starting age structures could have generated the same series of frequencies of vital events through time with the same crude rates (as the ergodic objection assumes to be the case) is through compensating

9. Lee, 'Inverse projection', p. 246. 10. *Ibid.*, p. 237. 11. *Ibid.*, p. 238.
12. Oeppen, 'Back-projection and inverse projection', and 'Generalised inverse projection'. Lee's objections can be appreciated in his 'Inverse projection and demographic fluctuation'.

changes in the age-specific patterns of fertility and mortality. Wachter has shown formally that this is actually feasible in the case of mortality, but only subject to a constraint on the relationship between the elements of the life-table system.[13] This condition is restrictive, however, and the ergodic property may well fail with conventional life tables in quite plausible demographic conditions. Unfortunately, there are as yet no formal methods that would enable the limits of the application of ergodicity in such circumstances to be specified. But, in our opinion, the further constraints that are in practice imposed by back projection, and by the optimization procedure used in generalized inverse projection, are likely to make the compensating adjustments referred to above impossible, and so effectively rule out the problem of indeterminacy associated with ergodic behaviour.[14]

Oeppen has used generalized inverse projection to dispose of two further criticisms that have been made of the original back projection algorithm. First, some scholars were uneasy that in order to obtain population estimates for the sixteenth century from back projection, it was necessary to concoct imaginary birth and death series for a period of 95 years before registration began in 1541.[15] This was necessitated by the problem of estimating the size of the age cohorts comprising the population at the start of registration who were born before the data series begin. Lee's inverse projection distributed the given starting population according to a stable-growth assumption, but generalized inverse projection provided more flexibility in formulating a solution. The dominant rôle of the starting conditions in generalized inverse projection has been identified by Lee, and confirmed by Oeppen.[16]

Second, concern was expressed that a great deal depended on the accuracy of the estimated totals of persons aged 90–4 in each five-year period, which were partly based on the numbers in the same age group in the terminal census of 1871, probably the least accurately enumerated age group at any date. With generalized inverse projection this problem no longer arises because it is no longer necessary to proceed sequentially backwards through time. In the circumstances it is reassuring that when generalized inverse projection is run on the same data, and with the same mortality and migration schedules, as back projection, it produces estimates of the population size and age structure, and of fertility and mortality, as far back as 1541, that are virtually indistinguishable from those based on back projection. We have, therefore, seen no reason to alter any of the demographic estimates reported in this volume, although we do not claim that they represent the only possible reconstruction.

It remains true that methods such as back projection or generalized inverse projection need more information than totals of births and deaths and a terminal age structure drawn from a census if they are to produce useful estimates of population trends and demographic characteristics over time. For example, if age at death is not stated in the registers, as was the case in England before 1813, deaths need to be allocated to different age groups using some other means, such as a model mortality schedule, as has been indicated above. But to choose an appropriate schedule it is necessary to have additional information about the age-specific structure of mortality in the population,

13. Wachter, 'Ergodicity and inverse projection'.
14. It is, of course, possible that a sufficiently weakly constrained general inverse projection procedure might be open to the objection of ergodic indeterminacy.
15. For example, Moodie in Gaunt, Levine and Moodie, 'Review symposium', pp. 160–1.
16. Lee, 'Inverse projection and demographic fluctuation', and Oeppen, 'Generalized inverse projection'.

information which can usually be obtained only by the alternative, and laborious, method of analysis known as family reconstitution. Reconstitution also provides valuable information about other population characteristics which cannot be studied directly from aggregate data, such as age at marriage and the age-specific profile of fertility. Such complementary information can be either incorporated into the estimation procedures of generalized inverse projection, thereby narrowing the margin of error, or used to supplement its findings, enhancing the scope of the results.

Interpreting the results

For most historians esot ic arguments about technical demographic matters may be uninteresting and often unintelligible, whereas the question of the nature of the links between society, economy and population in the past have a significance which is easier to grasp. We turn finally, therefore, to criticisms that have been advanced of the later chapters of this book which were largely devoted to such interrelationships. Attention has been focused chiefly on the link that we found between long-term trends in prices and wages on the one hand and nuptiality on the other. Since marital fertility rates were remarkably stable over the whole period from the mid-sixteenth to the mid-nineteenth century, this relationship also implied that there was a link between secular trends in the economic variables and fertility.

Both Weir, and Henry and Blanchet, have shown independently that the nuptiality data in chapter 7, relating to the proportions never marrying (drawn from back projection), and age at marriage (taken from a dozen parish reconstitution studies), were not fully compatible with the reported gross reproduction rates.[17] Weir devised an ingenious graphical method of identifying any such inconsistencies and offered revised nuptiality estimates that did not suffer from this defect. Subsequently Schofield has investigated the issue further and has both confirmed and sharpened Weir's suggestion that in pre-industrial England there was a marked change in nuptiality behaviour from a period when movements in the proportion never marrying were dominant to one in which changes in age at marriage determined nuptiality trends, a transformation that was only tentatively alluded to in our original text.[18] It now seems clear that until the middle of the eighteenth century the substantial swings in nuptiality that occurred were produced almost exclusively by wide variations in the proportion of women never marrying (between about 5 and 22 per cent), whereas after this date there was little change in this aspect of nuptiality but a rapid and substantial fall in age at marriage, followed by a compensating rise in the nineteenth century.[19]

Although the problems in interpreting the relationships between the long-run movements in the components of nuptiality on the one hand, and in the economic variables on the other, are largely technical, nuptiality is so profoundly important in English demographic and economic history that any additional clarification of marriage behaviour cannot fail to have a wider significance. As it happens, work done by other scholars after the original publication of this volume poses some of the chief problems of interpretation in

17. Weir, 'Rather never than late'; Henry and Blanchet, 'La population de l'Angleterre'.
18. See below, pp. 263–7.
19. Schofield, 'English marriage patterns revisited'.

a particularly vivid manner.[20] It is clear that nuptiality varied so substantially over time that it exercised a great influence on population growth rates in all periods, and a dominant influence as the eighteenth century wore on. Marriage in England responded to economic circumstances very much as Malthus had thought possible and desirable (see figure 1 below).

The nature of the feedback relationship between real wages and nuptiality has proved to be the issue that, rightly, has attracted more comment than any other, and to this issue we now turn. The first point that arises is the length of the lag between turning points in real wages and in nuptiality or fertility. It is reasonable to suppose, on Malthusian grounds, that rises and falls in real wages should be followed by sympathetic changes in nuptiality and fertility, but also natural to expect that the lag between the two series should be fairly brief. Accordingly, there was widespread disbelief in the plausibility of lags of about half a century between turning points in the two series, such as we reported at one juncture.[21] Assuming the disbelief to be justified, there are two obvious reasons why the apparent lag might be spurious – that its length was a function of the method used to define it rather than a 'real' finding, and that the turning points in the real wage series were wrongly located because the Phelps Brown and Hopkins data on which they were based were unreliable.

Both these possibilities were raised by us when the series were first published and compared, and have been extensively debated subsequently. To avoid any possibility of continued misunderstanding, we now emphasize that we never supposed that the PBH estimates were authoritative. Both the price and wage series suffer from serious flaws, acknowledged by Phelps Brown and Hopkins themselves and rehearsed in our discussion of their data. Although we were aware of the problems associated with the PBH series, there was no alternative to using them for the bulk of the period studied, and we were reluctant to 'doctor' the results we obtained from their data because of the danger of appearing to make use of them only when they afforded support for our general model. For example, using the Bowden series for agricultural wages in the early seventeenth century suggests no upturn in real wages before the middle of the seventeenth century, a conclusion buttressed by the results of considering proxies for real wage trends, such as the use of the ratio of the prices of agricultural and industrial products, as suggested by Goldstone.[22] Shifting the upturn in real wages towards the middle of the seventeenth century in this way removes any problem about an inconveniently long lag between the turning points in the two series at this date.

Similar plausible manipulations can be made to remove other lag anomalies. In our original text we illustrated the uncertainties surrounding national real wage levels and trends in the eighteenth century by 'splicing' PBH wage data, which were drawn from southern England, with Gilboy's northern data to form a composite national series. This showed that even such a simple and uncontentious change could transpose the real wage peak as much as 40 years, and in so doing radically reduce the length of the lag.[23] The results of making these

20. This literature is reviewed in Goldstone, 'The demographic revolution in England'.
21. See below, pp. 430–5.
22. Bowden, 'Agricultural prices, farm profits, and rents', tab. XVI, p. 865; Goldstone, 'The demographic revolution in England', pp. 8–9.
23. See below pp. 430–3.

changes to the real wage series are shown in figure 1.[24] The solid line shows the revised series; the original series is represented by the broken line. It is also possible to modify the graph of the crude first marriage rate in ways that improve the closeness with which it is likely to mirror reality, though this is not shown in the figure. For example, between about 1660 and 1720 clandestine marriage was widespread. Correcting for this would increase the CFMR moderately and would place its turning point about 1650 rather than later. Again, the substantial 'hump' in the CFMR in the period from about 1710 to 1740 disappears if allowance is made for the increase in remarriage caused indirectly by the very high death rates of the late 1720s.

The continued absence of improved estimates of real wage trends between 1550 and 1850, and still more of individual and family earnings over the same period, remains an obstacle to an authoritative estimate of the lag between real wage changes and nuptiality responses. The balance of probability, however, is that it was about 15 to 20 years at each of the major turning points.[25] If the lag is both comparatively brief and uniform, it ceases to be a 'problem' in that it then conforms to the models, formal or intuitive, used by most scholars in considering the question. But it does not thereby cease to warrant further intensive research. Work at a much more disaggregated level, stretching down to the individual, is needed to throw light on such matters as whether the income effect on marriage decisions was the same for income drawn from capital assets as from wage-paid employment, or what the independent influence of female earnings before marriage may have been, or how the decay of the institution of service in husbandry affected the timing of marriage.

The more accurate measurement of the relative influence of changes in the timing of marriage and in celibacy in altering the incidence of nuptiality, and thus the level of fertility, has raised some most interesting issues. Even though nuptiality as a whole may have remained responsive to real wage changes throughout a period of more than a quarter of a millennium, the nature of the response clearly changed fundamentally in the middle decades of the eighteenth century. The clarification of these nuptiality changes by Weir and by Schofield has sharpened interest in the possible explanations of the change.

24. It was an oversight not to have included a graph of the relationship between these two variables in the book originally.

25. Ideally, the lag between the two series should be estimated taking account of all the annual values, not just the turning points. Using a regression model Olney has estimated that the mean lag between real wages and *fertility* was 16 years (Olney, 'Fertility and the standard of living'). In our opinion technical defects in the analysis make this a spurious result. First, Olney derived annual fertility values by linear interpolation between the quinquennial values of GRR reported in appendix 3 of this volume. This short-cut biases the result; if true annual values of GRR are used a mean lag of only 2 years is obtained. Second, the model specification leads to autocorrelated errors, which were not allowed for in the estimation method. Third, Olney used a second-order Pascal distribution of weights (corresponding to an inverted 'v') to model the lag structure, which is particularly inappropriate since it is almost exactly the reverse of the actual lag structure in the data. If a more appropriate third-order polynomial form (corresponding to an 'S'-shape) is used with annual data, a mean lag of 40 years is obtained between real wages and fertility, and a lag of 22 years between real wages and nuptiality. Although these values are not far from those obtained by considering the turning points in the series alone, we are disinclined to place any weight on these results since, in our view, this kind of statistical model is inappropriate for estimating lagged effects of this length.

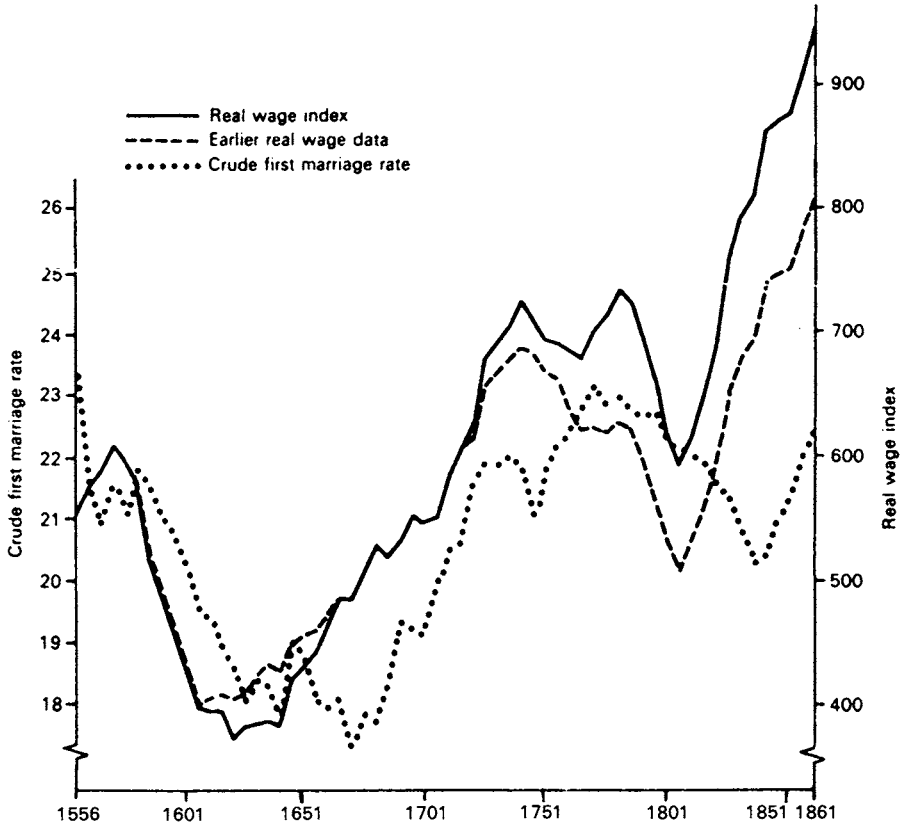

Figure 1: Real wage trends and crude first-marriage rates (both 25-year moving averages centred on the dates shown)

Note: The real wage data are as in fig. 10.7, except for the periods 1581–1661 and 1716–1861. In these periods the data from fig. 10.7 form the broken lines. The revised data for these periods (shown by the solid line) were derived from Goldstone, 'The demographic revolution in England', table A1, p.32 (for 1561–1661) and from fig. 10.12 (for 1716–1861). Goldstone's real wage estimates were obtained indirectly from the ratios of the prices of agricultural to manufactured goods taken from a publication of Phelps Brown and Hopkins. They were converted to 30-year averages centring on 1576, 1586, etc. and the Goldstone series was then 'tilted' to join the original data in 1576 and 1666, while preserving its shape. The data in fig. 10.12 can be used directly to provide revised estimates for the period 1716–86. Thereafter the series was extended by increasing each reading from the original series by the ratio of the revised series to the original in 1786.

Sources: Real wages, table A9.2. Crude first marriage rate, table A3.1 modified as described on pp.426–8.

Goldstone, for example, has argued that between 1551 and 1751 the secular movements in fertility and mortality, when plotted on demographic terrain maps, display the characteristic 'clockwise signature' of a preventive-check homeostasis. Thereafter, in his view, there was a change so profound and abrupt as to deserve to be termed a demographic revolution, when neither the preventive-check nor the positive-check paradigm will 'save the phenomena'.[26] Goldstone went on to develop a model that was intended to explain the observed changes in the incidence and timing of marriage underlying the sharp increase in nuptiality in the eighteenth century and the abrupt change in the relative importance of the two components governing its overall level. He suggested that adult males could be divided into three groups defined according to how they made their living, and whether they were able to find work. The three groups he labelled traditional (T), proletarian (P), and unemployed or underemployed (U). The first group (T) consisted of all those, whether engaged in agriculture or manufacture, who were dependent for a living upon access to a capital asset, such as a land-holding or workshop, or whose entry into marriage was preceded by an apprenticeship or service in husbandry. The second group (P) might also work either on the land or in industry but were wage-paid and served no extensive period of training. Both these categories could command the resources conventionally regarded as necessary before embarking on marriage, but the second characteristically made the transition into marriage at a substantially earlier age. The third group (U) included all those whose earning capacity left them short of the resource threshold which could enable them to marry. Clearly, the higher the ratio of U to T and P, the higher proportion of those who never marry, while equally the higher ratio of P to T, the lower the age at marriage of those who do marry.

Using this model, Goldstone suggested an explanation for some of the observed trends in nuptiality. During the late sixteenth and early seventeenth centuries, for example, falling real wages were accompanied by a rise in U, swamping any effect from increasing proletarianization (which would cause $P/(P+T)$ to rise). In the eighteenth century, in contrast, the large increase in the proportion of the labour force that was wage-paid took place at a time when the demand for labour remained strong, and U therefore remained low. Hence the combination of a substantial fall in age at first marriage and low celibacy which caused fertility to grow substantially and population growth to accelerate sharply.

Goldstone advanced his model with suitable caution, regarding it primarily as a useful stimulus to further reflection and empirical testing. But his model faces significant difficulties if it is to explain marriage patterns throughout the period. For example, since dependence upon wages for income probably affected a steadily growing percentage of the labour force during the seventeenth and early eighteenth centuries it is surprising that age at marriage did not fall since the ratio $P/(P+T)$ is likely to have been rising. It is also difficult with this model to account for the *rise* in the age at first marriage after the early decades of the nineteenth century, since proletarianization continued to gather momentum in this period.

26. Goldstone, 'The demographic revolution in England'.

Although the mechanisms linking economic change to demographic behaviour need much further elucidation, we remain impressed by the strength of the evidence of a link between secular changes in economic circumstances and those in nuptiality and fertility. A time lag of 15 to 20 years between the turning points in the real wages and nuptiality series does not pose any problem in most interpretative schemes, and the further link between nuptiality and fertility was also close, though there may be a problem with the lags between the series in the late eighteenth century.[27] It is premature, however, to describe the changes in nuptiality in the eighteenth century as constituting a demographic revolution. Evidently there were great changes in the way in which the different components of marriage behaviour responded to 'signals' in the economy, but overall the gross response of nuptiality to changes in real wages remained impressively similar. And progressive proletarianization as an explanation for rising nuptiality seems doomed to failure until it can be shown either that proletarianization went into reverse early in the nineteenth century or that the apparent decline in nuptiality in the period is spurious.

In the course of discussing English population history we found it useful to make use of two contrasting models of the interaction between the economic and demographic functioning of society, calling them 'high-pressure' and 'low-pressure' regimes. In the former, where both fertility and mortality are high, population is large relative to available resources and growth is curbed principally by the positive check: in the latter the preventive check is more important and its other characteristics are the opposite of those in a high-pressure regime. The terms have attracted attention and some criticism.[28] It may therefore be helpful to emphasize that we had in mind the long term rather than the short when making use of them, and that they were intended primarily as 'ideal types' rather than as exact representations of empirical reality.

If it is agreed that prior to the industrial revolution population growth was necessarily very slow or non-existent because the capacity of a society to increase the level of material production was slight if present at all, then birth and death rates were necessarily at closely similar levels in the long term, though not necessarily in the short.

Three limiting cases may be conceived. In a 'west African' situation mortality was always high because the disease environment was so unfavourable, often including diseases like malaria that might even be *more* prevalent at low population densities than when densities were high. In such circumstances high fertility was essential for a population to survive; in this sense high mortality could be said to have 'caused' high fertility. In a 'Chinese' situation, on the other hand, the disease environment was less deadly but social conventions made early and universal marriage mandatory. As a result, fertility was high and, because rapid growth had to be short-lived, mortality was high too. In the 'Chinese' case high fertility 'caused' high mortality. Thirdly, where social conventions required couples to have amassed substantial resources before marrying, nuptiality would be lower than in the Chinese case and would be responsive to changing economic conditions. In these circumstances, providing the disease environment was not so severe as to force fertility to be high in order

27. See table 10.11 and pp. 426–30.
28. Weir, 'Life under pressure'.

to ensure survival, both fertility and mortality could be low. This was the 'west European' case, in which a low and responsive fertility might produce an economic environment in which low mortality was also possible.

Precisely because it was a flexible system, there was much variation within western Europe. England was an especially intriguing case since it combined relatively low fertility and mortality with unusually rapid population growth rates. France, at least for the two centuries down to about 1750, had higher fertility and mortality but a slower population growth rate, and some French scholars have viewed France as suffering from a population total well above the optimum.[29] Though both England and France were clearly 'west European' in nature, it is plausible to suppose that in a long-run perspective France was somewhat closer to the 'high-pressure' end of the spectrum than England.

How closely and in what way short-term demographic characteristics correlated with long-term ones in such circumstances is not clear *a priori*. Weir has suggested, for example, that in a 'low-pressure' regime in which the preventive check dominates, nuptiality should display greater sensitivity to price changes than in a 'high-pressure' regime more subject to the positive check, and, finding stronger evidence of such sensitivity in France than in England, has argued that this tells against the view that England exemplified the 'low-pressure' case.[30] But it is equally reasonable to interpret the evidence differently. The existence of a greater tension between production and reproduction may have been the reason for the greater sensitivity in question. A regime that is comparatively successful in accommodating the tension between production and reproduction in the long run, may for that very reason be better able to 'ride' price shocks, leaving nuptiality relatively little affected, in much the same way that the same success may reduce the sensitivity of mortality to similar shocks.[31]

If economic circumstances powerfully affected marriage and ultimately population growth rates, the reverse was also true in England until about 1800. Since there were quite long lags between the interlinked variables we christened this process 'dilatory homeostasis'. We were taken to task over this by Flinn who wrote that our 'discussion of the relationship between real wages and the rate of population growth runs into a logical tangle', involving a circular argument because we argued that population growth rates greatly influenced real wages but equally that real wages indirectly determined population growth rates. 'It is a hen-and-egg argument that leaves the main questions about the determinants of population growth effectively unanswered.'[32]

Flinn's comment amounts to a denial of the possibility that any explanation of a phenomenon that depends upon the use of a negative feedback model can be valuable. Such a view may be apposite if it is held that only what might be termed a hierarchical model is appropriate, as when A causes B which in turn

29. Sauvy, *Théorie générale*, I, pp. 186–7. Or see the argument of Goubert, *Beauvais et le Beauvaisis*, I, pp. 604–16.
30. Weir, 'Life under pressure', pp. 38–9.
31. Weir presents evidence for the existence of this pattern in the seventeenth and eighteenth centuries (that is, of a stronger impact of price shocks on mortality in England than in France). It is a problem for him that satisfactory demographic data for testing his argument are available only after 1740 for France. Not long after this there were great changes in both mortality and marital fertility which make it problematic whether it is legitimate to draw from this evidence firm conclusions about the existence of a 'high-pressure' regime in early modern France (*ibid.*, pp. 42–3).
32. Flinn, '*The population history of England*', pp. 455.

causes C, but C is debarred from appearing as a determinant of A. But in seeking to understand the functioning of a *system*, rather than a simple linear hierarchy of relations, identifying the nature and strength of feedback paths within the system is likely to figure largely in any satisfactory account of its operation. Indeed the use of the term 'system' to describe the *explicandum* implies that feedback relationships are likely to prove important. Thinking in this manner may be less familiar to historians than to biologists or social scientists, but there are many topics in social, economic and demographic history that can be approached with profit in this way.

In chapter 11 of this volume we outlined a model of the ways in which the various components of demographic and economic processes may have interacted in the past as elements within a negative feedback system. Rather than strive for the formal rigour of an abstract expression of the model couched in the form of a series of equations, we contented ourselves with a verbal description supplemented by a diagrammatic representation. Our aim was to explore how the evolution of different parts of the network of links in the system could be integrated with, and advance further, our understanding of the course of the social and economic history of the period. While Malthus's original insights formed the basis of our thinking, our understanding of the nature of the several possible paths connecting the various demographic and economic components in a Malthusian system owed an immense debt to our collaborator and co-author, Ron Lee. In several papers preceding the publication of this volume he had greatly clarified the logical structure of the various theoretical sets of relationships linking population and the economy by expressing them in symbolic form and testing them against earlier estimates of demographic and economic variables.[33] It is, therefore, fortunate that he and other scholars have taken up the challenge of applying econometric techniques to the new data in order to estimate the relative strengths of the various links in the model.

However, as Lee points out, while short-run relationships can be estimated fairly unambiguously, there are technical difficulties in estimating the strength of long-run relationships in a feedback system which oscillates so slowly that less than two complete cycles can be observed in the historical record. Furthermore, the usual logical difficulties in drawing causal inferences from statistical associations are aggravated in an interacting cyclical system in which any one variable may be influenced by several other variables with differing periodicities. In the circumstances it is difficult to specify and estimate a formal mathematical expression of all the possible relationships between the various components of demographic and economic change; and it is no less difficult to interpret any results. For example, we cannot tell from a statistical study of the covariation of the series of population totals, real wages and nuptiality alone whether nuptiality was responding to the economic consequences of earlier population growth, as in the Malthusian preventive check, or whether, alternatively, it was responding to exogenous influences, and so 'causing' population growth and changes in the real wage rather than responding to them. In the latter case the apparent lag between real wages and nuptiality would be spurious, the real relationship being between nuptiality and real wages occur-

33. For key references see Lee, 'Population homeostasis', p. 77, n. 4.

ring 'round the back' of the cycle, as it were.[34] To infer causal relationships in a cyclical interacting system, it is even more necessary than in the case of simple linear systems to go beyond the measurement of statistical associations and appeal to substantive theory.

The model summarized in figures 11.4 to 11.9 in this volume represents a distillation of our understanding of such theory. Since we could think of no good reasons to expect secular fluctuations in nuptiality to be driven by forces exogenous to the system, we treated them as being determined endogenously as part of the preventive-check mechanism. Indeed, it is rather difficult to imagine a set of exogenous forces that would be capable of generating the regular cycle of nuptiality that can be observed in the data. Thus, while it is logically possible that nuptiality was driven by exogenous forces, and that the real wage to nuptiality link was consequently spurious, the suggestion seems too improbable to be taken seriously. In the event we accepted the historical plausibility of Malthus's stylization of the relationships between economic change and demographic behaviour in the guise of the preventive check. And, like Malthus, we expected that an equilibrating system of this kind would be unable to monitor its progress with precision and so would be likely to overshoot the mark, like a central heating system in which some tolerance is built into the sensitivity of the room thermostat before any signal for action is sent to the central heating boiler.[35] In this sense we regarded the oscillations produced by the 'dilatory homeostasis' of the system as comprising at least in part a self-generating cycle.[36] But we also saw the system as being affected by exogenous changes in mortality and in the demand for labour, although these two components were also affected by movements of other elements in the system.

Despite the difficulties of formal specification and inference, the econometric evaluations that have been made of the data presented in this volume now enable us to make a better assessment of the functioning of the demographic–economic system in pre-industrial England. So far as the central control system is concerned, the influence of population growth rates on food prices and real wages has been confirmed, as has the absence of any impact of real wages on the long-run course of mortality. On the other hand, the influence of real wages on population growth rates, *via* nuptiality and the preventive check, appears to have been relatively weak.[37] Lee has estimated that only about a quarter of the population growth occurring over the span of a generation would be self-correcting through the preventive check responding to the real wage consequences of that growth. In these circumstances the preventive check could be effective only because it exerted a persistent tug over a very extended time scale. But it was too weak to be able to cause significant overshooting of the 'target'

34. Lee, 'Population homeostasis', esp. pp. 77–91.
35. Malthus's statement is worth quoting in full: 'This sort of oscillation will not be remarked by superficial observers; and it may be difficult even for the most penetrating mind to calculate its periods. Yet that in all old states some such vibration does exist; though from various transverse causes, in a much less marked and in a much more irregular manner than I have described it, no reflecting man who considers the subject deeply can well doubt' (*Essay on the Principle of Population*, p. 15).
36. See below, p. 451.
37. Lee, 'Population homeostasis'; Lindert, 'English population, living standards and prices'.

equilibrium population size, a conclusion that casts doubt on the plausibility of treating the oscillations in the system as self-generating cycles.[38]

However, it should be remembered that changes in the rate of population growth had wider economic consequences than an impact on real wage rates, some of which may also have influenced nuptiality. For example, since women and children were discriminated against in the labour market, fluctuations in the effective demand for family labour, and hence family real incomes, engendered by population change are likely to have been of considerably greater magnitude than the fluctuations in adult male real wage rates which stand proxy for changes in the standard of living in the econometric models.[39] Thus the fact that these models find only a weak self-correcting effect of earlier population growth through its influence on real wages may simply reflect their failure to take into account the full economic impact of population growth relevant to nuptiality decisions. Their findings, therefore, are not necessarily inconsistent with the hypothesis that nuptiality varied considerably over time in response to changing economic conditions more widely construed.

Ironically, although the econometric evaluations of the data treat the economic factors involved in population processes in such restricted terms, they none the less provide powerful indirect evidence that the nature of the economy, and especially its productivity, changed very substantially over time. For example, Lee calculates that the equilibrium population growth rate increased from 0.2 to 0.6 per cent per annum between the sixteenth and nineteenth centuries, and if a measure of urbanization is incorporated into the model as an indicator of technological advance, this factor emerges as a significant element in accounting for the course of population growth over time.[40] Changes in the economy clearly mattered: developments such as improvements in material technology, more efficient use of land and material resources, greater specialization and the development of more efficient marketing and distribution networks all raised total factor productivity and increased the effective demand for labour, enabling the economy to absorb, and make more effective use of, the increasing supplies of labour that population growth provided.[41]

In accounting for population growth we placed some emphasis on our finding that it was movements in fertility, driven by changes in nuptiality, rather than movements in mortality that had the greater impact.[42] Yet econometric evaluations appear to suggest the opposite when they point to the importance of exogenously determined swings in mortality as ultimately 'driving the system'.[43] In fact there is no conflict. Since the system has been modelled in such a way that only mortality and the demand for labour can vary autonomously, by definition only variations in these two components can 'drive the system', the other components being constrained to respond only to movements within it.

38. Lee, 'Population dynamics', esp. pp. 451–4.
39. For sex- and age-specific discrimination in the labour market see Snell, *Annals of the labouring poor*, ch. 1.
40. Lee, 'Population homeostasis', p. 97. The measure of urbanization was the percentage of the population living in towns of 10,000 or more and was introduced by Stavins, in 'Model of English demographic change', pp. 105–9, and was utilized later, with additions, by Tsoulouhas 'A new look at demographic and technological changes'.
41. Wrigley, *People, cities and wealth*, chs. 2 and 7. For technology see Simon and Sullivan 'Population size, knowledge stock and other determinants'.
42. See esp. pp. 236–48, below.
43. Lee, 'Population homeostasis', p. 100; Stavins, 'Model of English demographic change'. p. 113, Tsoulouhas, 'A new look at demographic and technological changes', pp. 196–7.

Mortality did indeed exhibit a substantial secular fluctuation, rising during the seventeenth century, and falling between the mid-eighteenth and mid-nineteenth centuries, so that the system could be seen as comprising an elaborate equilibrating response mechanism that was nudged into action by exogenous fluctuations in mortality, but in which nuptiality played the dominant role in securing demographic adjustment. There is much to be said for this perspective, subject to some caveats.

The first caveat concerns the degree to which it is proper to characterize the secular fluctuations in the background level of mortality as being exogenously determined. It was certainly not an endogenous response to changes in the standard of living, since the standard of living was improving while mortality was worsening, and vice-versa. But it could be argued that the secular fluctuation in mortality was itself the product of wider changes in the economy, in which the development of a more integrated network of markets in goods also acted as a more effective distributor of micro-organisms, increasing the exposure of a greater proportion of the population to infectious disease.[44] In this perspective, mortality changes over the long run are not wholly autonomous; they are at least partly determined endogenously as a consequence of structural changes in the economy.

The second caveat relates to the unfortunate absence of structural change both from the formal models of the demographic-economic system that were represented diagrammatically in chapter 11 and from subsequent attempts to evaluate them by econometric analysis. The force of this point becomes even more apparent when the history of the English society and economy of the early modern period is compared to that of other pre-industrial European societies at that time. Indeed, the more deeply the English experience is probed, the more unusual it appears to be. Population growth was much faster than in the main countries of continental Europe, and yet real income per head probably grew significantly faster than elsewhere, which implies, of course, a still more marked contrast in the rates of growth of the gross national product. Moreover, it was before, rather than after, the industrial revolution that this contrast in rates of change was so pronounced.[45] Why was England different?

Perhaps because its characteristics can be established with some precision, much of the debate has centred on the peculiarities of the English marriage pattern. This immediately raises the issue of how social attitudes and personal value systems structured marriage decisions and thereby affected the operation of the economic–demographic system. For example, in contrast with most other west European pre-industrial societies, in which access to a farm or craft practice was a prerequisite for marriage, the preventive check in England was a much more diffuse affair, mediated by an intricate network of economic and social factors. It was a less stringent and a less automatic control mechanism than that provided in peasant societies by the 'niche' variant of the preventive check. Consequently, while it could prolong the agony when population growth rates were 'too high' and pressing on resources, it could also provide a breathing space in the opposite phase of the cycle by delaying the return of pronounced population growth. In the latter phase this would have the effect of prolonging

44. This point was first made in n. 29 on pp. 416–17, below. It is developed further in Walter and Schofield, 'Famine, disease, and crisis mortality', and discussed in Landers, 'Mortality and the metropolis'.
45. Wrigley, *Continuity, chance and change*; de Vries, 'The decline and rise'.

the period in which the standard of living remained relatively high, and so help promote structural change in the economy. Moreover, the presence or absence of a particular version of the preventive check, and the degree of economic differentiation and development, may themselves reflect systematic differences in familial, social and political power structures that are in turn institutionalizations of very different ideologies of inter-personal relations.[46] In this way understanding the process of demographic change in the past is inextricably bound up with an exploration of the interrelations between many aspects not only of the social and the economic spaces that populations inhabited, but also of their political and psychological environments.

The findings of this book, and the debate that they have provoked, have raised issues that are fundamental to a better understanding of the transition from the traditional to the modern world, from the agricultural to the industrial, from poverty to relative prosperity. There is much that forms part of this vast topic that is not covered within these pages, or is touched on only in passing. But the functioning of the socio-economic and demographic system is nonetheless a central matter: it both made possible and greatly facilitated the process of change and growth. The reprinting of this text is therefore an initiative we greatly welcome, especially as we have been able to make use of the opportunity to remove a number of errors that went undetected before the original publication. Otherwise the text remains in the form in which it was originally printed.

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46. This argument is developed in Schofield, 'Family structure, demographic behaviour and economic growth'.

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