Essays in English Population History

To Lyn Melville-James

Essays in English Population History

Peter Razzell

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Introduction

I was first introduced to the subject of English population history as an undergraduate at Birmingham University in the early 1960s, during a series of lectures by David Eversley on eighteenth-century population growth. Eversley quoted Malthus's belief that population increase "had arisen more from the diminution of deaths than the increase of births",[1] and told us how most writers on the subject since Malthus – Rickman, Farr, McCulloch, Griffiths and Buer – had explained population increase in terms of falling mortality.[2] This emphasis on a fall in the death rate had been based on official population and parish register returns, but Marshall in the late 1920s and Habakkuk in the early 1950s, questioned this interpretation of the evidence, and suggested that fertility may have played a central role in population growth.

There was not only disagreement about the mechanisms of population change – the relative importance of mortality and fertility – but the traditional explanation of population increase had also been found wanting. Up to the 1950s, the consensus had been that population had grown mainly as a result of falling mortality, which in turn was due to improvements in medical and public health provision. In 1955 this consensus was challenged by Thomas McKeown and R. G. Brown. They argued that improvements in medicine did not occur as described by Griffiths, Buer and others, and had been ineffectual in treating disease and illness before the twentieth century.[3]

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[1] Quoted in T. H. Marshall, "The population problem during the industrial revolution: a note on the present state of the controversy" in D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965).

[2] See Marshall, *op. cit.* and D. V. Glass, "Population and population movements in England and Wales, 1700 to 1850" in D. V. Glass and D. E. C. Eversley, *op.cit.*

[3] See Thomas McKeown and R. G. Brown, "Medical evidence related to English population growth in the eighteenth century", in Glass and Eversley, *op. cit.*

McKeown and Brown concluded that if medical improvements could not explain population growth, then economic factors – by default – must have been responsible.

Population was generally accepted to have grown most rapidly at the end of the eighteenth century. Yet there was serious doubt as to whether the average standard of living had been increasing during this period. In the early 1960s the controversy over the standard of living was in full swing, with no obvious resolution to the argument one way or the other. I left Eversley's lecture both fascinated and frustrated by the lack of an intellectual resolution to a problem of such obvious central importance: a major historical shift in population linked to the industrial revolution which had transformed English society – but a shift which could not be explained by current knowledge or thinking.

After graduating with a degree in sociology from Birmingham, I spent a post-graduate year at Chicago University, and with the help of Professor Janowitz, began investigating the social origins of army officers. I was interested in the transformation of English society during the industrial revolution period, and felt that an examination of patterns of social stratification in the army would reveal key elements in the changing social structure. I found that there had been an influx of sons of the gentry into the army at the end of the eighteenth century,[4] and became intrigued as to why this influx had occurred. This question led me to analyse the expectation of life of gentry families in Hertfordshire and Northamptonshire, which revealed a marked increase in life expectancy from the middle of the eighteenth century onwards.[5] This confirmed T. H. Hollingsworth's earlier finding of a significant fall in infant, child and adult mortality amongst the aristocracy during the eighteenth century.[6]

I searched the literature for factors which might explain increasing expectation of life amongst the gentry and aristocracy. For such wealthy groups, increases in the per capita consumption of food were unlikely to be relevant. The introduction of smallpox inoculation in the early 1720s, and its practice on a wide scale after the 1760s, seemed to fit the known

[4] P. E. Razzell, "Social origins of officers in the Indian and British Home Army: 1758–1962", *British Journal of Sociology*, Vol. 14 (1963).

[5] See Chapter 1.

[6] See T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies*, Vol, 11 (1957).

evidence. I wrote up my findings,[7] elaborating the thesis that inoculation could account for most of the reduced mortality and increased population of late eighteenth century England.

This work attracted interest and attention, but it suffered from a lack of reliable demographic evidence. The central problem for all work in this field was the unknown quality of the raw data – baptisms, marriages, and burials – and the lack of reliable statistics of nuptiality, fertility and mortality. Without reliable evidence, it was impossible to critically assess the role of inoculation: was population growth mainly due to a fall in mortality, and was the chronology of this fall compatible with the introduction and practice of inoculation? Or was population increase mainly due to an increase in fertility, independent of the introduction of inoculation?

I decided to tackle one aspect of this problem of unreliable demographic evidence by comparing information in the 1851 and 1861 censuses with that listed in the parish registers. A sample of 45 parishes was selected with which to evaluate the reliability of baptism and burial registration.[8] From this research, I found a sharp fall in mortality during the first four decades of the nineteenth century, well after the period in which inoculation had been generally introduced. As a result, I began to revise my earlier conclusions about inoculation and its role in the reduction of mortality.

The problem of population growth became more rather than less complex. For further fruitful work, it was necessary to gather further reliable demographic evidence. I engaged in research on the reliability of baptism registration by comparing census, parish and civil register data, but this work was never published. I left academic life for about ten years to work in publishing, and in effect left unresolved an intellectual problem which had fascinated me for nearly twenty years.

Subsequently, I was reading through historical journals for a publishing project I was working on, when I came across an article by Tony Wrigley in *Past And Present*, in which he claimed to have resolved the conundrum of eighteenth century population growth.[9] On the basis of the Cambridge Group's research findings, he and Roger Schofield argued that there had been a marked fall in the age at marriage, leading to an increase in fertility

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[7] See Chapter 1 of this book, and Peter Razzell, *The Conquest Of Smallpox* (1977).

[8] See Chapter 4 of this book.

[9] E. A. Wrigley, "The growth of population in eighteenth-century England: a conundrum resolved", *Past And Present*, Vol. 98 (1983).

and population during the eighteenth century,[10] and this overall conclusion had become something of an orthodoxy in the field of early modern English population history.

The Cambridge Group's presentation of reliable raw material on baptisms, marriages and burials, along with family reconstitution data, was clearly an important contribution to the history of population. I had, however, been unconvinced by the methods used to process this data when Wrigley and Schofield's early findings were published in the 1970s, and indeed this had been part of the stimulus for my own work on parish register reliability. The claim that the population conundrum had been resolved presented a challenge, and I set out to assess its validity. Although impressed by the empirical and intellectual scope of the Cambridge Group's enterprise, I found myself unconvinced by the central arguments of *The Population History Of England.* This was partly the result of my own earlier work on parish register reliability – see Chapters 4 and 5 of this book – which had cast doubt on the assumptions made by the Cambridge Group on the adequacy of parish registers in the late eighteenth and early nineteenth century.

My new research culminated in a critical review of the Cambridge Group's work, which has been recently published as an article in the *Journal Of Economic History.* (Chapter 7 of this book.) The essence of this critique is that Wrigley and Schofield have made a large number of theoretical assumptions which are not supported by empirical evidence. For example, they have made major adjustments to the 1871 Census figures on age structure – which is the starting point of their back projection programme – and yet the detailed comparison of censuses with parish and civil registers suggests that age statements in nineteenth century censuses were of a high order of reliability. (This is discussed in Chapters 4 and 5.) I concluded that the balance of evidence did not support the Cambridge Group's argument, but on the contrary favoured the classical view that mortality was the most important factor in population change. I have argued in Chapter 7 that much of this fall in mortality occurred at the *beginning* of the eighteenth century, and that the most likely explanation of the fall during this period is an improvement in domestic hygiene associated with the rebuilding of houses in brick and tile.

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[10] E. A. Wrigley and R. S. Schofield, *The Population History Of England* (1981).

The conclusion about falling mortality was mainly based on non-parish register evidence, as parish registers are of such questionable reliability. David Glass had begun detailed empirical research on the quality of parish registers by comparing them with information on births and deaths from tax returns. Unfortunately, these sources of data were not independent of each other, and Professor Glass's results were of limited value. However, the methodology of comparing data from a number of sources – the principle of triangulation – is very appropriate for the evaluation of parish registers. I have begun a programme of checking the quality of burial registers by comparing them with wills, poor law records, bishops transcripts and monumental inscriptions. I have also developed a method – the same-name technique – to measure both baptism and burial registration reliability. The preliminary results of this research will be found in Chapters 7 and 8. As a result of this work, I believe that it will eventually be possible to come to definite conclusions about the population history of England.

In the meantime, I am presenting my essays on English population history, primarily to re-open the debate on eighteenth century population growth. This edited collection of essays, written over the last thirty years, is presented in the sequence in which the essays were written. As I have modified a number of my views, I have rewritten parts of the original articles, but most of these changes are relatively minor, involving style and presentation, rather than substance. I have included a previously unpublished essay on the further evaluation of baptism registration, which forms Chapter 5. In addition, I have written a concluding chapter specially for the book, which discusses the questions of the reliability of demographic evidence and explanations of eighteenth-century population growth, and draws together a review of work by other scholars with some of my own recent research findings.

Most of the essays in the book are of a non-technical nature, and are suitable for the reader with a general interest in English economic and social history. Others – in particular Chapters 4 and 5 – are rather technical, and the non-demographic reader may prefer to read them lightly, or perhaps skip them altogether. I hope I have succeeded in conveying some of the intellectual excitement involved in pursuing a question that has been so central to English economic and social history, and which still remains unresolved.

Peter Razzell

Chapter 1

Population Change in Eighteenth-Century England: A Reappraisal[1]

This was my first essay on English population history. I argued in this article that the practice of smallpox inoculation could in principle account for the whole of the increase in population at the end of the eighteenth century. Since this article was written, I have modified my view and no longer believe that inoculation was the sole major determinant of population growth. I have changed my thinking for two reasons. First, new research on the comparison of census and parish registers (see Chapter 4) suggested that there had been a significant fall in mortality during the first four decades of the nineteenth century, after the general introduction of inoculation. Second, new evidence has emerged (see Chapter 7) to suggest that there was a substantial fall in adult mortality at the beginning of the eighteenth century, before the introduction of inoculation.

The article still stands as a detailed empirical investigation of one highly effective prophylactic eighteenth-century medical practice well before the twentieth century, which clearly had a significant impact on mortality.

Two traditional explanations have been proposed for the acceleration of population growth which occurred in the middle of the eighteenth century. First, the neo-Malthusian view that it was a consequence of the industrial and agricultural revolutions through an improved standard of life. Second, that it was the result of various medical innovations independent

[1] First published in *The Economic History Review*, Vol. 18 (1965).

of these revolutions. The problem posed by these competing interpretations is central to English economic and social history: did the industrial and agricultural revolutions create their own future labour force and expanding numbers of consumers, or were they themselves children of a population revolution which preceded them?

Economic historians have attempted to answer this question by estimating population, birth- and death-rates at decennial intervals throughout the eighteenth century. Professor Krause, however, has questioned the validity of this method for the period before 1781 when national aggregate statistics of Anglican baptisms and burials are available only for every tenth year from 1700 to 1780. He has pointed out that the use of one conventional assumption about English demographic data with reference to Sweden would exaggerate the amount of actual increase of population in that country between 1750 and 1780 by over 61 per cent.[2] Krause has attempted to use the statistics of annual baptisms and burials from 1780 onwards by making certain questionable assumptions about changes in the baptism/birth and burial/death ratios during the period 1781–1850. He concluded that a rise in the birth-rate rather than a fall in the death-rate was "the major variable in English demography".[3]

This has led the medical historians McKeown and Record to state that "the data [on mortality and natality] are so treacherous that they can be interpreted to fit any hypothesis, and it seems preferable to rely on assessment of the sensitivity of the birth-rate and death-rate, and their relative effectiveness, in a period when both rates were high."[4] This they had done in their own work and after reviewing the history of all the major diseases and preventive measures taken against them, concluded that the "fall in the death-rate during the eighteenth and nineteenth centuries was not the result of medical treatment as Griffiths and others had supposed. Only in the case of vaccination against smallpox is there any clear evidence that specific therapy had a substantial effect on the prevention or cure of disease earlier than the twentieth century. The decline in

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[2] J. T. Krause, "Changes in English fertility and mortality, 1781–1850", *The Economic History Review*, Vol. 11 (1958–9), p. 53.

[3] *Ibid*, p. 69.

[4] T. McKeown and R. G. Record, "Reasons for the decline in mortality in England and Wales during the nineteenth century", *Population Studies*, Vol. 16 (1962), pp. 94–5.

mortality from diseases other than smallpox was due to improvement in living conditions, and to changes in virulence and resistance upon which human effort had no influence."[5]

Krause, however, has pointed out that vaccination did not become really widespread until the 1840s and has argued that the average standard of living probably deteriorated slightly between 1780 and 1821 when population was increasing very rapidly.[6] Chambers, in his study of the Vale of Trent region, examined the relationship of food-supply to mortality-rates and concluded that population "was vulnerable to disease, but not as a result of famine. Epidemics could do their own work without its aid, nor, it would seem, did they require the assistance of gin."[7] A similar conclusion was reached by Pickard after analysing the relationship between food prices and changes in mortality and natality in eighteenth-century Exeter.[8] It should also be remembered that from 1838 to 1875, when the standard of living was undoubtedly rising rapidly, the overall death-rate was virtually constant.[9] It is in the light of all these contradictory facts that McKeown and Record have been reduced to making the following desperate statement: "When we have eliminated the impossible [medical explanations of population growth], whatever remains [economic explanations], however improbable, must be the truth."[10]

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[5] McKeown and R. G. Record, "Medical evidence related to English population changes in the eighteenth century", *Population Studies*, Vol. 9 (1955), p. 139.

[6] Krause, *op. cit.*, pp. 63–5.

[7] J. D. Chambers, "The Vale of Trent, 1670–1800", *The Economic History Review, Supplement 3*, p. 29.

[8] R. Pickard, *Population And Epidemics Of Exeter* (1947), p. 67.

[9] B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), pp. 36, 343–58.

[10] McKeown and Record, *op. cit.*, pp. 94, 95.

I

This paper is intended as a summary of research to date on the causes of the increase in population in eighteenth-century England.[11] Before discussing these causes it is necessary to estimate the size of population during the eighteenth and early nineteenth centuries, in order to appreciate the magnitude of change during this period. The estimates of population used in this paper are those derived from the returns of marriages made from several thousand parishes which were published by Rickman in 1841.[12] These estimates have several advantages: (a) unlike baptisms and burials, the overwhelming majority of dissenters' marriages took place in the Anglican church;[13] (b) the registration of marriage is generally considered to have been the most reliable;[14] (c) the estimates are based on three-year clusters of returns rather than single years, a procedure which is much more likely to reduce fluctuations of the marriage-rate from one time to another.[15] The basis of Rickman's own estimate was the assumption that the ratio of the number of marriages to total population in the eighteenth century, was the same for the periods 1699–1701 and 1749–51, i.e. that

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[11] The paper is really a series of hypotheses illustrated occasionally by statistical and other evidence. It is hoped to incorporate detailed evidence into a monograph at a later date. (1994): Published as *The Conquest Of Smallpox* (1977).

[12] Rickman's figures for marriages were generally derived from over 4,000 parish registers. See G. Talbot Griffiths, "Rickman's second series of eighteenth century population figures", *Journal Of The Royal Statistical Society*, Vol. 92 (1929), p. 263.

[13] The best confirmation of this is to be found in the *Report On Non-Parochial Registers*, (Parliamentary Papers 1837–8, XXVIII), where it is seen that there were virtually no non-Anglican marriage registers kept for the eighteenth century.

[14] See J. C. Cox, *The Parish Registers Of England* (1910), p. 76; W. E. Tate, *The Parish Chest* (1946), p. 65; G. Talbot Griffiths, *Population Problems Of The Age of Malthus* (1926), p. 33.

[15] An examination of the Swedish statistics for the eighteenth century, for example, shows that three-yearly clusters fluctuated far less than single years in terms of the marriage-rate. See *Historical Statistics of Sweden, 1720–1950* (1955), pp. 39–41. The long-term marriage-rate in Sweden was remarkably stable between 1751 and 1825. See G. Sundbarg, *Sweden, Its People And Its Industry* (1904), p. 96.

the marriage-rate was constant between 1700 and 1800. It is impossible to test this assumption in any detail, although there are a few scattered statistics available to suggest that it is not too unreasonable.

Table 1. The Marriage Rate Throughout the Eighteenth Century[16]

Place	*Total Population*	*Approximate Period*	*Marriage Rate Per 1,000 Population*
7 market towns	27,043	1724–36	8.7
54 villages	19,607	1724–36	8.4
11 towns	37,541	1770s	8.5
England & Wales	8,892,436	1795–1805	8.8

These figures must not be taken too literally, as they refer to places of different sizes and locations; the figure for 1795–1805 is somewhat arbitrary because of the flaws in the registration of both marriages and population.

However, the figures for marriage-rates indicate that there were no marked long-term changes in the marriage-rate throughout the eighteenth century. This conclusion is confirmed by at least one local study of population change during the same period.[17] The estimates of population size from the returns of the number of marriages are as follows:

[16] Thomas Short, *New Observations On Bills of Mortality* (1751), p. 133; J. Howlett, *Observations On The Increased Population ... Of Maidstone* (1782), p. 82. I have excluded from the 1795–1805 population figure the numbers in the army and navy; also I have not corrected for under-enumeration, as a few marriages were also not registered because of the non-Anglican marriage of Quakers, Jews, and Roman Catholics, as well as various illicit marriages in sea-ports and elsewhere. For the source of the population figure see *Census Of Great Britain, 1851*, pp. xxiii, xxvi.

[17] Chambers, *op. cit.*, pp. 54, 55.

Table 2. Estimated Population Size in Eighteenth-Century England and Wales[18]

Period	*Estimated Population (nearest 1,000)*	*Average Annual Rate of Percentage Increase*
1700	5,307,000	
1750	5,895,000	+ 0.2%
1801	9,337,000	+ 1.1%
1851	17,719,000	+ 1.8%

Although we have indicated that the marriage-rate was only stable during the eighteenth century, it is possible to check the earlier population estimates with figures derived from an independent source. Gregory King estimated the population of England and Wales to be 5.5 millions in 1695, an estimate which Professor Glass thinks may be slightly too high.[19] King's estimate was based on hearth-tax returns and local censuses conducted in connection with the tax on marriages; it is similar to the one we have made for 1700 on the basis of the marriage returns. The population increased relatively slowly up to 1750, after which it increased rapidly and steadily right through to the end of the nineteenth century. It is the causes of this rapid and consistent increase which are the subject of this paper.

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[18] These estimates are recomputations of Rickman's figures. The following adjustments were made: (1) 5 per cent was added to the 1801 enumerated population because of estimated under-enumeration. See Krause, *op. cit.*, p. 60. (2) Rickman took the number of marriages in the single year 1800 as the basis of his marriages/population ratio. This has been recomputed on the basis of the years 1800–02 so that the basic ratio is derived from a three-year cluster of marriages like all the previous periods. The original estimates are those Rickman arrived at by treating England and Wales as one unit, and may be found in Griffiths's article in *Journal Of The Royal Statistical Society*, Vol. 92 (1929), p. 263. See also J. Rickman, *Parishes Possessing Registers Extant 1570 And 1600 With Their Population In 1801*, (Document M. 74 10 in the General Register Office Library). (3) No allowance was made for the numbers in the armed service. The population figures are not intended as exact estimates, but rather as indications of the magnitude of change in the size of the population during the eighteenth and early nineteenth centuries. For the source of the 1801 and 1831 figures, see *Census of Great Britain, 1851*, pp. xxii, xxiii, xxvi.

[19] D. V. Glass, "Gregory King's estimate of the population of England and Wales, 1695", *Population Studies*, Vol. 3 (1950), p. 358.

Ideally, we should want to analyse the aggregate birth- and death-rates, age-specific fertility and mortality-rates. Unfortunately, the paucity of accurate information means that we can only collect data of a piecemeal kind, which at least points in the direction of certain conclusions. It has already been indicated that the aggregate marriage-rate changed but little during the eighteenth century. This conclusion is consistent with the fact that the age at marriage of spinsters appears to have been virtually constant during the same period.

Table 3. Mean Age at Marriage of Spinsters, 1615–1841[20]

Period	*Region*	*Mean Age at Marriage*	*Number in Sample*
1615–1621	Wilts., Berks., Hants. & Dorset	24.6	280
1662–1714	Yorkshire	23.8	7,242
1701–1736	Nottinghamshire	24.5	865
1741–1745	Surrey	24.9	333
1749–1770	Nottinghamshire	23.9	700
1796–1799	Sussex	24.1	275
1839–1841	England & Wales	24.3	14,311

These provisional findings indicate that eighteenth-century population increase was not brought about by a lowering of the age at marriage or by an increase in the marriage-rate.

If changes in marriage patterns were not responsible for population growth, what is the evidence that a reduction in mortality was involved? Although no reliable information for the general population is available, it is possible to construct good-quality data for special groups. Two detailed genealogical volumes for county families have been published in the Victoria County History series for the counties of Hertfordshire and

[20] Rev. E. Nevill (ed.), *Marriage Licences Of Salisbury, 1615–1682*; M. Drake, "An elementary exercise in parish register demography", *The Economic History Review*, Vol. XIV (1962), p. 444; T. M. Blagg and F. A. Wadsworth (eds.), *Nottinghamshire Marriage Licences*, (The Index Library, British Record Society); R. Bax (ed.), *Allegation For Marriage Licences Issued By The Commissary Court of Surrey, 1673–1770* (1907); D. Macleod (ed.), *Sussex Marriage Licences, 1775–1800*, (Sussex Record Society, Vol. XXXV, 1929); *The Registrar General's Fourth Annual Report* (1842), p. 10.

Northamptonshire. The following calculations of average age lived from birth were made from the information in these volumes.

Table 4. Changes in the Average Age Lived From Birth (County Families)[21]

Cohort born	*Average Age Lived From Birth (Males)*	*Number in Sample*
1681–1730	37 years	138
1731–1780	48 years	130
1781–1830	50 years	162

The results of this study were compared with those published by Hollingsworth in his paper on the demographic history of ducal families, as well as the results of his unpublished research into the whole of the aristocracy.[22] All these studies point to the same conclusion: that expectation of life for cohorts born from circa 1740 onwards rose significantly, the saving of life occurring mainly among infants, children, and young adults.[23] A more detailed analysis of the 'county family' material illustrates the sharpness of this rise.

Table 5. Changes In The Average Age Lived (County Families)

Cohort Born	*Average Age Lived From Birth (Males)*	*Number in Sample*
1680–1699	36 years	92
1700–1719	38 years	89
1720–1739	35 years	86
1740–1759	48 years	76

Unfortunately it is impossible to construct similar tables for the general

[21] Samples were taken from the Northants and Herts genealogical volumes of the *Victoria County History* series published in 1906 and 1907. Figures were computed to the nearest year.

[22] T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies*, Vol. XI (1957).

[23] Hollingsworth's figures for the whole aristocracy, which are based on much larger cohorts, indicate that the rise in life expectancy was somewhat more gradual than this, and began at the beginning of the eighteenth century. The chronology of the rise in expectation of life is dealt with in more detail in the Chapter 7 of this book.

population during the same period. It is probable that there was an equivalent rise among the general population, for the mean expectation of life at birth derived from Gregory King's life-table for Lichfield in about 1695 was 32.0 years,[24] whereas according to the English life-table constructed by Farr in 1841 it was 41.2 years.[25] If these figures are representative, the aristocracy and gentry always had a higher life expectancy than the general population but managed to increase their relative advantage slightly throughout the eighteenth and early nineteenth centuries.

What are the possible causes of the increase in expectation of life throughout the eighteenth century? For obvious reasons, an explanation in terms of increased food supplies is inappropriate for social groups such as the gentry and aristocracy. There is one major plausible explanation which fits the known evidence: the introduction and use of inoculation against smallpox during the eighteenth century. Inoculation must formally be contrasted with the nineteenth-century practice of vaccination. Inoculation is the injection of smallpox virus taken from the vesicle of a person suffering from smallpox, whereas vaccination is the injection of cowpox virus. The two injections are conventionally distinguished by the different symptoms they produce. Inoculation is thought of as giving rise to pustular eruptions in different parts of the body as well as at the site of injection, and is viewed as a mild form of natural smallpox, inasmuch as it is believed to spread the natural disease from the inoculated person to other unprotected people. Vaccination only gives rise to a vesicle at the site of the injection and is not infectious to other unprotected people.[26]

II

Inoculation was originally practised sporadically and on a very limited scale as a part of folk medicine, mainly in Oriental and African countries. It was introduced into England in 1721, when Lady Mary Wortley Montagu had her daughter inoculated in London, although it had been known by report for some years previously. It was practised on only a very limited

[24] See Glass, *op. cit.*, p. 368 for the reliability of this figure.

[25] *Fifth Annual Report Of The Registrar General* (1843), p. 29.

[26] The relationship between vaccination and inoculation is discussed in detail in the next chapter.

scale during the 1720s and 1730s, owing mainly to the fact that the very severe technique of inoculation caused several deaths. Between 1721 and 1728 there were 897 people known to have been inoculated, 17 of whom were suspected to have died from inoculated smallpox. In the early 1740s the practice was revived again mainly as a result of the use of a safer technique involving milder injections of virus. However, because the medical profession had elaborated inoculation from its original simplicity into a very complex operation involving both a fortnight's preparation and convalescence, often in a special isolation hospital, the practice became very expensive, and was consequently restricted to the rich. Although the London Smallpox Hospital was founded in 1746 to offer charitable inoculations to the poor, most of its clients in the early period tended to be servants of the subscribers to the foundation of the hospital.

During the 1750s the overseers of the poor began to pay the cost of inoculation for all the poor within their parish; this usually took place as a response to the threat of a smallpox epidemic which provoked mass inoculation among all members of the parish. In addition to these mass inoculations there were many individuals who were inoculated at their own expense. Thus Kirkpatrick wrote in 1754: "But since we have certain accounts that the populace, who were at first strongly predisposed against this practice, and who so rarely stop at the Golden Mean, are rushing into the contrary extreme; and go promiscuously from different distances to little Market Towns, where without any medical advice, and very little consideration, they procure inoculation from some operator, too often as crude and thoughtless as themselves ..."[27] This popularisation of inoculation was made possible by its cheapness through the activities of local surgeons and apothecaries.[28]

However, inoculation did not become really widespread until after the 1760s, for, according to one source, only 200,000 people had been inoculated in England by 1766.[29] The main reason why inoculation was not more

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[27] J. Kirkpatrick, *The Analysis Of Inoculation* (1754), pp. 267, 268.

[28] This was achieved through the simplification of inoculation, culminating in the abandonment of preparation and convalescence by Lewis Paul Williams (a Leicestershire surgeon) in 1763. See *Northampton Mercury*, 15 December 1768; *The British Medical Journal*, Vol. 11 (1910), pp. 633–34.

[29] See A. C. Klebs, "The historic evolution of variolation", *Bulletin Of The John Hopkins Hospital*, Vol. XXIV (March 1913), p. 82. The basis of this estimate is unknown.

widespread was the occasional mortality still associated with the operation. This situation was changed in the 1760s when the Sutton family began to inoculate by injecting the minimal amount of virus into the arm with the very lightest of scratches. The result was that "if any patient has twenty or thirty pustules he is said to have the smallpox very heavy",[30] thus ensuring a negligible risk of death. The Suttons claimed in 1768 "that about fifty-five thousand had been inoculated by them since the year 1760; of which number only six had died".[31] The 'Suttonian Practice' consisted of Robert Sutton, an apothecary and surgeon at Framlington Earl, Norfolk, and several of his sons, as well as a very large number of non-family partners; the practice extended to most counties and several foreign countries.[32] The most famous son was Daniel Sutton, who, because of his very spectacular feats of inoculation,[33] was chiefly responsible for popularizing the Suttonian method. By the end of 1776 they claimed to have inoculated 300,000 people,[34] a claim which is very plausible in the light of the very large number of partners they had. They offered to inoculate the rural poor gratis on the condition presumably that the rest of the parish were also inoculated by them; certainly the Suttons appear in the account books of innumerable overseers who paid them for mass inoculations in their parishes.

The Suttonian method was soon taken up by the rest of the medical profession, as well as by amateur inoculators who began to proliferate very rapidly. Thus Houlton wrote in 1768 "that in every county of England you meet advertisements of these pretenders and itinerants. ... Some of them as before observed, advertise that they inoculate according to the Sutton method; while others have the modesty to deck their imposition with the style of 'The Suttonian art improved'."[35] Some of these "pretenders and itinerants" were undoubtedly professional surgeons and apothecaries, such

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[30] Creighton, *op. cit.*, p. 476.

[31] R. Houlton, *Indisputable Facts Relative To The Suttonian Art of Inoculation* (1768), p. 10. The negligible risk of death from inoculation after the 1760s is confirmed by a great deal of evidence.

[32] *Ibid*, pp. 21–23.

[33] During a mass inoculation at Maldon, Essex, he inoculated 487 people in one day, none of whom died.

[34] W. R. Clayton, "Notes on the history, incidence and treatment of smallpox in Norfolk", *Norfolk Archaeological Society*, Vol. XXX, p. 7.

[35] Houlton, *op. cit.*, p. 24.

as Dimsdale, who was converted to the Suttonian method by its superiority over the older technique; another professional medical practitioner who later inoculated with the Suttonian method before discovering vaccination was Edward Jenner, who had been inoculated in the old method as a boy during the mass inoculation at Wootton-under-Edge in 1756. Others of the imitators of the Suttonian method were "a certain tribe of empirics and other unexperienced Practitioners",[36] such as the livery servant who left his employment in about 1768 to become a full-time inoculator,[37] and the farrier and blacksmith who inoculated 170 people in the neighbourhood of Norwich in 1769.[38] The occupations of the amateur inoculators ranged from farmer to customs-officer, and some set up schools in their own method of inoculation.

Inoculation was practised much more extensively and earlier in rural areas and small towns than in large towns and cities. Haygarth, writing in 1780, stated that

> whole villages in this neighbourhood (Chester) and many other parts of Britain, have been inoculated with one consent. And it cannot be supposed that the inhabitants of towns are more ignorant or more obstinate. There is not a reasonable doubt that our poor fellow citizens would eagerly and universally embrace a proposal to preserve their children from death and deformity, if the intelligent and the opulent would humanely exert their influence and assistance to carry it into execution.[39]

Although the relative lack of provision of charitable inoculation was one of the major reasons why it spread only slowly in the large towns, another reason was because of the differing structure of smallpox epidemics in town and countryside. In the large towns where the disease was endemic the majority of smallpox deaths were of infants and young children; this tended to engender a fatalistic attitude about the inevitability of catching the disease. This was recognized by Haygarth:

> the lower class of people [in Chester] have no fear of the casual [natural] smallpox. Many more examples occurred of their wishes

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[36] M. G. Hobson, *Otmoor And Its Seven Towns* (1961), p. 20.

[37] W. Watson, *An Account ... Of Inoculating The Smallpox* (1768), pp. 71, 72.

[38] *Gentleman's Magazine*, Vol. XXXIX (1769), p. 16.

[39] J. Haygarth, *An Enquiry How To Prevent The Smallpox* (1785), p. 164.

> and endeavour to catch the infection, than to avoid it. This ... prejudice ... probably prevails in other large towns, especially in those which are so large as perpetually to nourish the distemper, by so quick a succession of infants as constantly to supply fresh subjects for infection.[40]

This he contrasted with "small towns and villages, especially where placed in remote situations, the young generation grow up to have a consciousness of the danger before they are attacked by the dreadful disease."[41] This consciousness was also based on the greater fatality of smallpox in isolated areas. One of its results was seen at Blandford, Dorset, in 1766 when a very malignant epidemic of smallpox broke out and "a perfect rage for inoculation seized the town."[42] In the small town or village it was possible for everybody to compare the spectacular differences in mortality of the inoculated and uninoculated during a smallpox epidemic, whereas in a large town it was very difficult to familiarize the poorer classes with the benefits of inoculation owing to the dispersed and piecemeal nature of smallpox mortality.

The relatively slow spread of inoculation in the large towns should not be exaggerated in importance, for only a small minority of the total population lived in such areas. It appears that inoculation was making rapid headway in towns by the very end of the eighteenth century.[43] In the small towns and villages inoculation appears to have been universally practised well before the end of the century. There are innumerable references to mass inoculations in local histories and medical writings for every decade from about 1750 onwards.[44] One of the reasons why parish authorities were so willing to pay for inoculation of their poor was because of the great expenses involved in isolating and nursing the

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[40] J. Haygarth, *A Sketch Of A Plan To Exterminate The Casual Smallpox* (1793), p. 186.

[41] *Ibid*, p. 186.

[42] Creighton, *op. cit.*, p. 513.

[43] Many of these large towns founded dispensaries during the late eighteenth century which provided charitable inoculation. Although the London Smallpox Hospital only inoculated 36,378 people between 1746 and 1805, practitioners such as Daniel Sutton specialized in the inoculation of "the families of artificers, handicraftsmen, servants, labourers" in the Metropolis.

[44] See the appendix on pp. 36–7.

sick during an epidemic of the natural smallpox. The costs were sufficiently great to make many parishes compel everyone within their jurisdiction to be inoculated.[45]

One observer noted in 1771 "that inoculation, which was heretofore in a manner confined to people of superior ranks, is now practised even in the meanest cottages, and is almost universally received in every corner of this kingdom".[46] According to Dimsdale, writing in 1776,

> in the county of Hertford, there have been two methods of public or general inoculation; one to inoculate, at a low price, as many of the inhabitants of any small town or village, as could be persuaded to submit to it, and at the same time were able to pay, refusing all those who had it not in their power to procure the money demanded. The other method has been, where the inhabitants of a town, or a district, of all denominations, have agreed to be inoculated at the same time, the parish officers or some neighbouring charitably disposed persons, having first promised to defray the expense, and provide subsistence for such of the poor, as unable to pay for themselves.[47]

To some extent the emergence of the amateur inoculators served the needs of the poor, who were unable to afford the price of professional inoculation and whose parish was unwilling to pay for a mass inoculation. A supporter of inoculation summed up the extent of the practice by writing in 1805 that "smallpox inoculation was a well-known, proved, and absolute prevention from receiving the natural Smallpox infection, as millions of people now living can testify".[48] Inoculation did not disappear with the introduction of vaccination. On the contrary, it remained very popular, especially with the poorer classes, who were very prejudiced against vaccination. Ironically, inoculation and vaccination appeared to have supplemented one another, in that virtually all of the population during the first half of the nineteenth century were protected by one injection or the

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[45] See S. and B. Webb, *English Local Government - English Poor Law History*, 1 (1927), p. 306; M. F. Davies, *Life In An English Village* (1909), p. 74; E. G. Thomas, *The Parish Overseer In Essex, 1597–1834* (London M.A. Thesis, 1956), p. 394.

[46] *Medical Transactions*, Vol. 11 (1772), p. 279.

[47] T. Dimsdale, *Thoughts On Partial And General Inoculations* (1776), p. 29.

[48] W. Rowley, *Cowpox Inoculation No Security Against Smallpox* (1805), p. 4.

other, sometimes by both.[49] Inoculation was eventually banned by law in 1840 at the instigation of the supporters of vaccination, who believed inoculation spread natural smallpox to the unprotected.

Inoculation was very extensively practised in other countries, in which it was encouraged by legal enactments during the latter half of the eighteenth century – such as Sweden, Russia, and Austria. It appears to have been particularly popular in Ireland where itinerant tinker inoculators proceeded "from village to village several times during the year for the purpose of inoculating the infantile population."[50]

III

In order to determine the significance of inoculation it is necessary to discuss the history of smallpox mortality prior to its effective introduction. By smallpox mortality we mean the proportion of every 100 children born who died from the disease during their lives. There are two methods of estimating such smallpox mortality: (i) multiplying the extent of the disease by its case-fatality rate (allowing for children who would have died before they had a chance to catch the disease); (ii) counting the number of smallpox deaths and expressing it as a proportion of the number of births. Such information is occasionally to be found in parish registers. In a period of static population growth the proportion of smallpox deaths to all deaths will approximate to the ratio of smallpox deaths per number of births. In order to estimate smallpox mortality we will use both methods outlined above. First, however, it is necessary to discuss the problem in interpreting smallpox statistics.There are five major difficulties in using figures of smallpox mortality:

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[49] For an invaluable description of the history of inoculation and vaccination during the first two decades of the nineteenth century, see Dr J. Forbes, "Some account of the small pox lately prevalent in Chichester and its vicinity", *London Medical Repository* (September 1822), pp. 211–15. Vaccination was not introduced into the area until 1812, although all the population appears to have been protected by inoculation at least as early as the beginning of the nineteenth century.

[50] W. Wilde, "Reports on tables of deaths", *Population Census Of Ireland 1851*, (Parl. Pap., 1843, XXIV), p. xii.

(1) The existence of a type of smallpox, known as fulminating smallpox, which does not manifest the classical pock symptoms because of the rapidity with which it kills its victims. It has been discovered only relatively recently, for as a current medical authority on smallpox has observed, "this is 'sledge-hammer' smallpox, and the diagnosis both clinical and at autopsy is impossible unless smallpox is thought of and unless laboratory facilities are available and used to grow the virus".[51] It is impossible to estimate what proportion of all smallpox deaths were of the fulminating kind; generally it would be highest in very isolated communities which lacked a pool of antibodies derived from frequent epidemics.

(2) The variation in the fatality from smallpox in different types of area. This was recognized by Lettsom when he wrote "that in some countries, and even some counties of England, the infection does not appear for the space of some years; but when it does appear, it is more fatal; owing probably to this, that in great towns the infection being always prevalent, it is caught without the accumulated changes of air peculiarly favourable to epidemics; whereas when it comes at stated periods its malignity seems to be augmented by some unknown but deleterious state of the atmosphere".[52] This, we now know, was due to the creation of a pool of antibodies in the large towns through constant recurrence of smallpox epidemics, which it has already been noticed occurred to a lesser extent in isolated areas.

(3) A large number of smallpox deaths were unregistered for other reasons. Lettsom, who had a great deal of experience with the health of the poor in London, estimated that smallpox mortality was nearly twice that recorded in the Bills of Mortality. One major reason was the confusion of disease with the symptoms that it gave rise to, so that a number of young infants dying from smallpox were registered as convulsions deaths.[53] Very young infants are known to be vulnerable to fulminating smallpox[54] and it appears that this could be partly the explanation of

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[51] C. W. Dixon, *Smallpox* (1962), p. 9.

[52] T. J. Pettigrew, *Memoirs Of The Life And Writings Of The Late John Coakley Lettsom* (1817), Vol. 2 , pp. 121, 122.

[53] Creighton quoting Lettsom, stated that "the genetic article 'convulsions' ... swallowed up, in his opinion, a large number of the smallpox deaths of infants." Creighton, *op. cit.*, p. 534.

[54] Dixon, *op. cit.*, p. 324.

this mis-registration.[55] Lettsom also pointed out that from smallpox "some have been deprived of sight; many have been afflicted with the evil and scrofulous complaints, to which they had previously been strangers; many have been disabled in their limbs ... at length, emaciated and debilitated, they have sunk under their miseries, and filled up the amazing list of consumptions; many of which originated from the violence of Natural Smallpox."[56] Smallpox mortality was also much higher when the disease converged with epidemics of other diseases; and, obviously, some of the increased mortality would be ascribed to other diseases.

(4) Pregnant women are particularly vulnerable when attacked by smallpox,[57] the great majority of their children dying because of such an attack. According to Dixon, "in forty-six cases where the infant's condition is recorded [when the mother has been attacked by smallpox], twenty-six were stillborn, and of the twenty born alive, eleven died later".[58] Most of the stillborn children and many infants who died soon after birth were probably not recorded in the parish registers, as they would not have been baptized. Those deaths which were recorded were probably often attributed to some causes other than smallpox, for example, convulsions. According to a doctor of the Bristol Royal Infirmary during the middle years of the eighteenth century, "the female sex whose cases from about 12 years of age to 50 become more dangerous on account of their menstrual discharges, which sometimes coming on in the beginning or State of the Disease proves fatal".[59] Thus the group of potential mothers was particularly vulnerable to death from smallpox, a fact that we shall discuss later in connection with changes in the birth-rate.

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[55] See J. Haygarth, *A Sketch Of A Plan To Exterminate The Casual Small-Pox* (1793), p. 141: "The disease most fatal to infants is convulsions, arising from various causes; one of them is the small-pox. The two circumstances will explain the reason why, under one year old, the proportion of deaths by the smallpox is less than in subsequent periods ...".

[56] Pettigrew, *op. cit.*, Vol. 1, p. 6.

[57] Dixon, *op. cit.*, p. 326.

[58] *Ibid*, p. 113.

[59] *Bristol Infirmary Biographical Memoirs*, Vol. 1, p. 59.

(5) Many people who died of smallpox appear to have been buried in non-consecrated burial pits near the pest-houses or infirmaries used for isolating those sick of the disease. In the Maidstone parish register the incumbent summarized the burials for the year 1760 with the following entry: "Total Burials – 223. Of the Small Pox from Dec. 13 – 59 besides. These carried out of Town 102." It is quite clear from examining the average number of burials in Maidstone that these 102 smallpox victims were not a part of the total 223 burials, a conclusion confirmed by examining the ages of those buried in the churchyard. It is thought that they were buried out at the pest-house because it was quite common practice in the eighteenth century for hospitals to bury their own dead. Both the Northampton and London bills of mortality had yearly returns of the number of people buried in local infirmaries. People responsible for isolating and nursing smallpox victims were also considered responsible for burying them[60] and this was because people were so terrified of smallpox that they feared contact with the corpses themselves; there are references in the literature of incumbents refusing to perform the burial rites and relatives refusing to attend funerals.[61] The existence of non-consecrated burial grounds not only poses a problem for the construction of smallpox mortality statistics but also for demographic studies which assume that burials entered in parish registers represent the total number of deaths.

We are now in a position to estimate total smallpox mortality. There are two methods in arriving at such estimates, the first being to multiply the extent of smallpox by its case-fatality rate. As to the extent of the disease, most writers regarded it as a universal affliction to which all were subject at some time or other, for example, D'Escheray, in his writings on smallpox in England, observed in 1760 that "this distemper spares neither Age nor Sex, Rich and Poor are equally exposed to its influence. What is the most unaccountable, and so wide from all other fevers, is, that the Difference of Constitution is no preservative against its Attack, insomuch, that very few escape it, at one time or other."[62] This universality of smallpox is consistent

[60] See for example W. Le Hardy (ed.), *Calendar To The Herts Sessions Books, 1752–1799*, VIII (1935), p. 226.

[61] See for example Document I.C.1185, 1679 in the Northampton Record Office.

[62] D. D'Escheray, *An Essay On The Smallpox* (1760), p. 2.

with what we know about the nature of the disease; for example, Dr J. F. D. Shrewsbury, the bacteriologist, has written that smallpox is "the most highly infectious of the transmissible diseases of man".[63] It appears from statistical evidence that smallpox was endemic in London as early as at least the sixteenth century; in fact, the disease was so endemic as to be found regularly every week in the bills of mortality during the seventeenth and eighteenth centuries. Smallpox deaths occurred in other large towns during the eighteenth century at least every year. Thus London, and other large towns to a lesser extent, served as smallpox reservoirs from which the disease was constantly exported to the countryside.

The case-fatality rate of smallpox may be estimated from a series of smallpox censuses sponsored by the Royal Society during the 1720s. The figures compiled were for the number of total cases of smallpox sickness with the resulting numbers of deaths in thirty places. Of the 13,192 cases of people suffering from smallpox, 2,167 died: an average case-fatality rate of 16.5 per cent.[64] This figure should be interpreted in the light of the difficulties in using smallpox statistics which we have already discussed. Three of the difficulties are relevant: (i) the figures would exclude cases of fulminating smallpox, the mortality from which is nearly 100 per cent; (ii) large numbers of unregistered deaths would have been excluded, in the ways described by Lettsom; (iii) variations in the fatality of smallpox varied from one type of area to another. With reference to the last difficulty, most of the censuses were conducted in market towns, many of them in Yorkshire and centres of industrial activity. These were towns of very frequently recurring epidemics, which consequently had a lower case-fatality rate than places such as the isolated villages in Worcestershire studied by Eversley. He has written that during the smallpox epidemic of 1720–30 in the area of Bromsgrove "a conservative estimate of the net loss of population at Hanbury is 164 out of the 716 alive in 1715".[65] This was similar to the epidemics in the Shetland Islands, where "formerly the smallpox occasioned the most dreadful ravages in these islands frequently carrying off a fifth part of the inhabitants."[66] In 1720, "the disease was so

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[63] Private Communication, 1964.

[64] For details of the censuses, see Creighton, *op. cit.*, pp. 518, 519.

[65] D. E. C. Eversley, "A survey of population in an area of Worcestershire", *Population Studies*, Vol. 10 (1956–7).

[66] J. Sinclair, *The Statistical Account Of Scotland*, Vol. 2 (1792), pp. 569–70.

fatal as to be distinguished by the name of the mortal pox. On this occasion tradition tells us, in the remote Island of Foula, probably inhabited by about two hundred people, it left only four to six to bury the dead".[67] This type of spectacular smallpox mortality was to be found in other extremely isolated places where the population had no pool of antibodies to protect them.[68]

One contemporary medical observer noted "that when the smallpox is epidemic, entire villages are depopulated, markets ruined, and the face of distress spread over the whole country".[69] Certainly epidemics of the fatality of the one in Hanbury occurred quite often.[70] As about 23 per cent of the total population of Hanbury was wiped out, the case-fatality rate must have been considerably higher than this, for many of the older members of the village must have had smallpox when they were younger. Thus it appears that the case-fatality rate of 16.5 per cent derived from the smallpox censuses in the market towns is too low for the country as a whole. It is impossible to estimate total smallpox mortality using the present method; suffice it to say that smallpox was a universal disease with a recorded case-fatality rate varying from 16.5 per cent to 97 per cent.

The other method of estimating smallpox mortality is to use the parish registers and bills of mortality. Ideally, we would like to express the number of smallpox deaths as a proportion of the number of births. This is not always possible because of the lack of information about births and the deficiencies in registration. When it is not possible the proportion of smallpox deaths to all deaths will be used, as it will generally approximate to the smallpox deaths/births ratio because of the relatively equal number of births and deaths during a period of static population. The smallpox mortality-rate in the eighteenth century varies from 11.6 smallpox deaths

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[67] Robert Cowie, *Shetland: Descriptive & Historical* (1871), pp. 73–5. See also Sinclair, *op. cit.*, Vol. XX (1798), p. 101, for another description of this epidemic.

[68] See E. W. and A. E. Stearn, *The Effect Of Smallpox On The Destiny Of The American Indian* (1945); also *Royal Commission On Vaccination, 1st Report* (1889), pp. 109, 110.

[69] James McKenzie, *The History Of Health* (1760).

[70] See the *Parish Register Of Burford* in 1758; also *Gentleman's Magazine*, Vol. XLII (1772), p. 542. Many of the mass inoculations suggest that a very large proportion of village populations were vulnerable to smallpox. For example, at Irthlingborough, Northants, "upwards of Five Hundred People" were inoculated in 1778, whereas the total population was only 811 by 1801.

per 100 births in London during 1730–39,[71] 20 per 100 deaths in Dublin during the two approximate 30-year periods 1661–90 and 1715–46,[72] to an extreme proportion of 50 per 100 deaths in Great Chart, Kent, during 1688–1707.[73] The majority of records (mainly for towns) yield an average figure of about 15 per cent of all births and deaths due to smallpox during the first half of the eighteenth century. All of the difficulties outlined earlier in the paper apply to these statistics, and all of them would tend to increase actual smallpox mortality over recorded mortality, for example, Lettsom's estimate of the true smallpox mortality in London would raise the figure for 1730–39 from 11.6 smallpox deaths per 100 births to over 20 per 100, this being in an area where smallpox mortality was at its lowest due to the endemic nature of the disease. Once again it is impossible to estimate exactly the magnitude of smallpox mortality, but for the time being it will be sufficient to note that recorded smallpox deaths accounted for between 11.6 and 50 per cent of all those born and dying, and that actual smallpox mortality was possibly twice as large as that actually recorded.

IV

The possible effectiveness of inoculation in reducing smallpox mortality has been rejected by previous historians on two basic grounds: (i) the argument that inoculation spread natural smallpox to the unprotected; (ii) the continuance of smallpox deaths in the bills of mortality of some of the large towns. There are several grounds for questioning the argument that inoculation spread natural smallpox: (a) smallpox was already a universal disease before the introduction of inoculation; (b) inoculation had become so widespread by the end of the eighteenth century that only a relatively small proportion of the population was left unprotected; (c) experimental and other evidence is available to show that inoculation

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[71] J. Marshall, *Mortality Of The Metropolis* (1832).

[72] J. Fleetwood, *History Of Medicine In Ireland* (1951), p. 65; Dr J. Rutty, *A Chronological History Of The Weather, And Of The Prevailing Diseases In Dublin* (1770).

[73] M. C. Buer, *Health, Wealth, And Population In The Early Days Of The Industrial Revolution* (1926), p. 190.

did not spread natural smallpox to the unprotected.[74] This conclusion is supported by the fact that early vaccination was in reality a more attenuated form of inoculation.[75]

Smallpox did continue to kill substantial numbers of children in some of the large towns during the late eighteenth century. This fact has misled medical historians for two reasons: (i) the total population increased very rapidly in these places, and if the number of smallpox deaths is expressed as a proportion of the number of children at risk a reduction in smallpox mortality is seen to have taken place; (ii) as we have already seen, these large towns were atypical, in that inoculation spread much later in them than elsewhere. This was stated quite explicitly by Howlett in 1781:

> It may be thought, at first sight, that the healthiness of London is more increased than that of country towns ... But it must be remembered that the diminished mortality in the latter appears to be chiefly owing to the salutary practice of inoculation; whereas in the former, for want of universality, it has hitherto been of little advantage ... In provincial towns and villages, as soon as this disorder makes its appearance, inoculation takes place amongst all ranks of people; the rich and poor, from either choice or necessity, almost instantly have recourse to it; and where two or three hundred used to be carried to their graves in the course of a few months, there are now perhaps not above 20 or 30.[76]

An illustration of this reduction of smallpox mortality is to be found at Maidstone in Kent.

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[74] (1994): I have changed my view on this subject. The very severe forms of inoculated smallpox were probably capable of spreading natural smallpox.

[75] It is impossible in this paper to document this very controversial statement. The subject is of sufficient importance to warrant a separate paper. Inoculators were able to produce a single vesicle at the site of injection identical to that of vaccination, through a process of attenuation. Inoculation was superior to vaccination in that it conferred life-long immunity against further attacks of smallpox owing to the larger amount of virus injected. (1994): See the next chapter of this book, and Peter Razzell, *Edward Jenner: The History Of A Medical Myth* (1977), for a further discussion of this subject.

[76] Rev. J. Howlett, *An Examination Of Dr Price's Essay On The Population Of England And Wales* (1782), p. 94.

Table 6. Smallpox Mortality at Maidstone, 1754–1801[77]

Period	*Smallpox Burials*	*All Burials*
1752–1763	252	1,703
1762–1771	76	1,426
1772–1781	60	1,549
1782–1791	91	1,676
1792–1801	2	2,068

A mass inoculation in Maidstone was conducted by Daniel Sutton in 1766, and its effects were described by Howlett in a pamphlet by him in 1782:

> Upon casting an eye over the annual lists of burials, we see that, before the modern improved method of inoculation was introduced, every 5 or 6 years the average number was almost doubled; and it was found upon enquiry, that at such intervals nearly the smallpox used to repeat its periodical visits ... in the short space of 30 years it deprived the town of between five and six hundred of its inhabitants; whereas in the 15 or 16 years that have elapsed since that general inoculation it has occasioned the deaths of only about 60. Ample and satisfactory evidence of the vast benefits the town has received from that salutary invention.[78]

Many other statistical sources could be cited to prove the effectiveness of inoculation,[79] the most detailed being for Boston, USA, during the

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[77] Taken from the Parish Register Of Maidstone, lodged in All Saints Church, Maidstone. Smallpox deaths disappeared from the register after 1797. This gradual decline of smallpox cannot be attributed to a decrease in the virulence of the disease, as all the evidence points to the opposite conclusion, i.e. an increase in its virulence, e.g. the case-fatality rates at the London Smallpox Hospital were as follows in 1746–63, 25%; 1775–99, 32%; 1836–56, 35%. See the *Royal Commission On Vaccination, 1st Report* (1889), p. 74, and the *Royal Commission On Vaccination, 3rd Report* (1890), p. 100.

[78] J. Howlett, *Observations On The Increased Population ... Of Maidstone* (1782), p. 8.

[79] For the sources of these statistics see: the parish registers of Basingstoke (Hants.), Calne (Wilts.), Milton Ernest (Beds.), Whittington (Salop.), Selattyn (Salop.), Boston (Lincs.). For other statistics see "An abridgement of the observations on the Bills of Mortality in Carlisle 1779–87" by Dr Heysham in W. Hutchinson, *The History Of Cumberland* (1794), pp. 668–75.

eighteenth century, from which it is possible to attribute the reduced mortality directly to inoculation.[80]

Contemporaries were very well aware of the effect of inoculation; for example, in *She Stoops To Conquer* written in 1773, Mrs Hardcastle says to Hastings: "I vow since Inoculation began, there is no such thing to be seen as a plain woman. So one must dress a little particular; or one may escape in the crowd." Arthur Young, in his essay on population in 1781, wrote:

> In several of these parishes where population had for some periods been rather on the decrease, a great change has taken place lately, and the last ten years are found to be in a rapid state of progression; as considerable drains of men have been made from almost every parish in the kingdom for the public service in that period, I should not have expected this result, and know nothing to which it can be owing, unless the prevalence of inoculation, which certainly has been attended with a very great effect.[81]

References to the effects of inoculation on mortality appear in the reports on agriculture made by local observers to the Board of Agriculture at the end of the eighteenth century; for example, Plymley writing on Shropshire observed in 1795: "I may further add, that since the year 1782, when these observations were made, the population of this parish has been increasing: most certainly inoculation for the Smallpox ... has been most essential to population throughout this kingdom".[82] John Holt of Lancashire wrote in 1795: "One reason, why persons in large manufacturies in Lancashire, do not frequently die in great numbers ... is that they have (in general) been inoculated in their infancy. Inoculation is the most effectual

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[80] The number of inoculations in this town increased from 287 in 1721 to 9,152 in 1792, which was the vast majority who had not had smallpox before. Smallpox mortality fell from 175 smallpox deaths per 1,000 living population in 1677–8 to 15 per 1,000 in 1792, and this was in spite of the fact that the virulence of the disease generally increased throughout the period. See J. Blake, *Public Health In The Town Of Boston (Mass.), 1630–1822* (1959), p. 244; H. R. Viets (ed.), *A Brief Rule To Guide The Common People Of New England* (1937), p. xxxv; *Royal Commission On Vaccination, 6th Report* (Parl. Pap. 1896, XLVII), p. 762. See the Table on this subject in the next chapter.

[81] A. Young, *Annals Of Agriculture*, VII (1786), p. 455.

[82] J. Plymley, *General View Of The Agriculture Of Shropshire* (1803) pp. 343, 344.

of all expedients for preserving the short lived race of men – many gentlemen pay for inoculation of the children of the poor in their own neighbourhood".[83] In 1796 a correspondent to the *Gentleman's Magazine*, observed:

> the increase of people within the last 25 years is visible to every observer. Inoculation is the mystic spell which has produced this wonder ... before that time it may be safely asserted, that the malady, added to the general laws of nature, did at least equipoise population. It is now 30 years since the Suttons and others under their instructions, had practised the art of inoculation upon half the kingdom and had reduced the chance of death to 1 in 2,000.[84]

Another gentleman observed later in 1803 that "one very great cause of increasing population may be ascribed to the success of inoculation for the Smallpox. One in four or five, or about 200 to 250 in a thousand, usually died of this loathsome disorder in the natural way of infection ... so that this saving of lives alone would account for our increasing number, without perplexing ourselves for any other cause".[85]

The claims made by some contemporaries made on the beneficial effects of inoculation on population growth require careful evaluation. Unfortunately there is virtually no reliable demographic data by which this can be done. An analysis of the 'county family' life tables suggests that a reduction of about 25 per cent in mortality among the younger age-groups could account for the whole increase in expectation of life between 1681–1730 and 1781–1830. The same conclusion probably applies to both the ducal families and the whole of the aristocracy. For the population as a whole there is no data sufficiently reliable to test the hypothesis directly. However, it is possible to construct a simple hypothetical model whose limits are defined by the small amount of reliable information available to us. In 1697 Gregory King constructed a 'life table' for Lichfield; Professor Glass has written that "it would appear that by taking Lichfield as a basis, King began with a collection of statistics which were probably not markedly untypical, and then adjusted more acceptably as an indication of

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[83] J. Holt, *General View Of The Agriculture Of Lancaster* (1795), p. 208. n. 2.
[84] *Gentleman's Magazine*, Vol. LXVI, 1 (1796), n. 112.
[85] *Gentleman's Magazine*, Vol. LXXIII, 1 (1803), p. 213.

national structure".[86] Using King's 'life table', it is possible to construct a hypothetical population reproduction model for our period.

Table 7. Female Population Reproduction, 1750–1855[87]

Numbers Surviving to the Following Ages (Years)	*Numbers Surviving in the Following Years*							
	1750	*1765*	*1780*	*1795*	*1810*	*1825*	*1840*	*1855*
0	1,000	1,071	1,237	1,468	1,762	2,116	2,538	3,045
15	620	680	793	952	1,138	1,366	1,640	1,967
30	450	480	559	659	798	956	1,146	1,376
45	315	325	357	422	498	603	722	866
60	190	190	196	215	255	300	364	435
75	50	50	50	52	57	67	79	85
90	0	0	0	0	0	0	0	0
Population Index	2,125	2,260	2,573	3,034	3,627	4,350	5,220	6,251

The above model was constructed on the following assumptions: (i) increase in the female population was proportionate to the increase in total population; this ignores the effects of the relationship between the number of males and females, for example, the proportion of married women who were widowed; (ii) of 1,000 female children born before 1750, the numbers surviving to various ages were the same as in King's 'life table'; (iii) the population was static before 1750, based on an age-specific birth-rate of 1 female child born for every 13.7625 women living between 15 and 45; (iv) the age-specific birth-rate remains constant throughout the whole period; (v) of every 1,000 born, lives were saved in the following manner:

[86] D. V. Glass, "Gregory King's estimate of the population of England and Wales, 1695", *Population Studies*, Vol. 3 (1949–50), p. 568.

[87] This population index is the sum of the average number of people living in each age period, i.e. I have not bothered to multiply by 15 throughout.

Ages	*Period*			
(Years)	*1750–65*	*1765–80*	*1780–95*	*1795–1810*
Under 15	60	60	20	15
15–30	30	30	5	5
30–45	10	10	5	0

In all, it is assumed that 250 lives were saved out of 1,000 born. According to our earlier estimates of population growth, it almost exactly trebled between 1750 and 1851. In our model it does not quite do this, but we assumed that population was static before 1750, whereas according to the earlier estimates it was increasing about 0.2 per cent per annum between 1700 and 1750. If an allowance is made for this pre-1750 growth, population in our model increases by 3.2 times between 1750 and 1851; the greater the allowance made for pre-1750 growth, the more the model population increase will exceed that as estimated.

The point of the model is not to describe exact changes in the population structure, but rather to estimate the magnitude of lives required to be saved in order to generate the rate of increase in estimated population. The assumptions are thought to be realistic because: (a) the crude birth-rate appears to have been very similar between the 1690s and the 1840s;[88] (b) the saving of life (250 out of 1,000 born) assumed is very similar to that which took place among the gentry and aristocracy.

In order for inoculation against smallpox to account for the whole of the population increase, smallpox mortality before inoculation must have been about 310 deaths per 1,000 born, for of the 250 lives saved of every 1,000 born in our model, about 45 would have died of other diseases during the same period, while smallpox accounted for about 1.5 per cent of deaths

[88] The birth-rate was estimated as 34.5 births per 1,000 living during the 1690s by Gregory King and 35.2 per 1,000 during 1841–5 by Professor Glass from civil registration returns. See G. King, "Natural and political observation 1696" in George Chalmers, *An Estimate Of The Comparative Strength Of Great Britain* (1804), p. 44; and D. V. Glass, "A note on the under-registration of births in Britain in the nineteenth century", *Population Studies*, Vol. 5 (1951), p. 85. Professor Glass has written about the basis of King's estimate: "the statistics collected were more comprehensive than any provided previously and, indeed, than any subsequent statistics prior to the establishment of the full mechanism of censuses and civil registration in the nineteenth century." See D. V. Glass, "Gregory King and the population of England and Wales", *Eugenics Review*, XXXVII (1946), p. 175.

of all born during 1838–40,[89] when civil registration was first introduced. It is impossible to state definitely that smallpox mortality before inoculation was as high as 310 deaths per 1,000 born, but we may conclude from our earlier discussion that this is certainly a plausible figure. It must be remembered that much of this saving of life would have been indirect, in so much as the elimination of smallpox attacks probably increased the expectation of life of those who did not die of the disease. Also the vulnerability of mothers and other young adult females to smallpox could have meant that the elimination of the disease led to an increase in the birth-rate; for example, at Basingstoke (Hants.) the average number of baptisms in the ten years before the smallpox epidemic in 1741 was 69.6, whereas in the following ten years it fell to 45.5 (a much greater fall than the average number of deaths and therefore presumably the population), which was possibly due to the fact that one-half of the smallpox deaths occurred among adults.[90]

Although it is not possible to analyse in any detail the history of other diseases, it is possible to draw some conclusions from bills of mortality. For example, in Northampton there was no major epidemic of any disease, other than smallpox, during the hundred-year period after 1736 when records were kept.[91] Smallpox epidemics occurred every seven years on average in Northampton before the introduction of inoculation; the listing of diseases and epidemics was very similar in a place like Maidstone; that is to say, recurrent severe smallpox epidemics were the only causes of sharp rises in mortality rates. This would indicate that the peaks in mortality found in many local studies were due to smallpox and that they disappeared only with the introduction of inoculation.

Ideally one would like to trace the history of all diseases in order to evaluate their importance in contributing to total mortality, but unlike smallpox, most other diseases prevalent in the eighteenth century are not

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[89] See Creighton, *op. cit.* This figure includes chickenpox deaths, which are assumed to approximate to omissions due to fulminating smallpox, etc.

[90] See the Basingstoke Parish Register. A rise in the age-specific birthrate was not allowed for in the population reproduction model for two reasons: (i) simplicity and economy; (ii) the very long-term stability of the estimated crude birth-rate. Thus any increase in the birth-rate has been absorbed for analytical reasons into a fall in the death-rate.

[91] See the Northampton Bills Of Mortality in the British Library.

sufficiently distinctive to be analysed statistically. In their returns to Sir John Sinclair for the *Statistical Account of Scotland*, many incumbents discussed the history of diseases in their parish. No disease, other than smallpox (due to inoculation), was described as having declined or disappeared, except ague, which is very frequently mentioned as having diminished during the latter half of the eighteenth century. Recently one medical authority has questioned whether malaria was ever endemic in Britain.[92] However, the incumbents so consistently mention that the disappearance of ague was linked with the draining of marshes and the reclamation of swamp-land, that one is led to suspect that the disease they described was malaria, a suspicion confirmed by their descriptions of the disease. Buèr, in her discussion of malaria, maintained that although "its direct effect on the death-rate was small, its indirect effect must have been great".[93] Certainly it rarely appeared in the bills of mortality and parish registers as a cause of death even during the early eighteenth century. Malaria in England is a subject which warrants further investigation.[94]

Although this paper stressed the importance of inoculation against smallpox as a cause of population growth during the eighteenth century, this does not rule out the role of other factors and explanations.[95] However, other explanations currently lack evidence in their favour. Inoculation against smallpox could theoretically explain the whole of the increase in population, and until other explanations are convincingly documented, it is an explanation which must stand as the best available.[96]

Although the industrial and agricultural revolutions probably did not bring about the increase in population in the eighteenth century, they did at least enable population to grow unchecked. In Ireland, where such revolutions did not take place, the Malthusian check of mass starvation was the result of a rapidly increasing population without concomitant changes in the structure of the economy. The main achievement of the

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[92] McKeown and Brown, *op. cit.*, p. 124, n. 4.

[93] M. C. Buer, *op. cit.*, p. 212.

[94] (1994): Since this was written, Mary Dobson has embarked on a major study of malaria in the marshland area of south-east England.

[95] For example, the effects of the changing distribution of population between rural and urban areas.

[96] (1994): Recent evidence has come to light – considered in later chapters – which suggests that a fall in mortality may have predated the introduction of inoculation.

industrial and agricultural revolutions in their earlier phases was the maintenance of the standard of living in a period when population was growing for reasons largely unconnected with the revolutions themselves.

APPENDIX

In order to indicate the extent of mass inoculations, a sample was taken of those references to them in local histories, medical commentaries, accounts of the Overseers of the Poor, local newspapers, and other sources. It is in no sense comprehensive or representative, but merely a series of isolated examples culled from the literature, mainly from the South of England. The name of the town is given first, followed by the date of the mass inoculation:

Guildford, Surrey, 1740s. Salisbury, Wilts., 1751–2. Bradford-on-Avon, Wilts., 1752–3. Blandford, Dorset, 1753, 1766. Wootton-under-Edge, Glos., 1756. First Regiment of Foot Guards, 1756. Beaminster, Dorset, 1758, 1780, 1791. Maldon, Essex, 1764. Maidstone, Kent, 1766. Marnham, Notts., 1767. Rye, Sussex, 1767. Neighbourhood of Norwich, 1769. Burton, Lincs., 1770. Berkhamstead and surrounding villages in Herts., 1770. Corsley, Wilts., 1773; Meopham, Kent, 1776. Bedford, Beds., 1777. Ware, Herts., 1777. Great Clivall, Essex, 1778. Irthlingborough, Northants., 1778. Villages in the neighbourhood of Carlisle, Cumberland, 1779, 1781. Cricklade, Wilts., 1783. Painswick, Glos., 1786. Knowle, Kent, 1787. Weston, 1788. Northwold, Norfolk, 1788. Cowden, Kent, 1788. Luton, Beds., 1788. Bozeat, Northants., 1789. Chislehurst, Kent, 1790, 1799. Toddington, Beds., 1790, 1801, 1824. Weston, Norfolk, 1791. Eaton Socon, Beds., 1793, 1800, 1808. Hevingham, Norfolk, 1794. Berkeley, Glos., 1795. Hastings, Sussex, 1796–7. Dursley, Glos., 1797. Three villages near Gillingham, 1797. Tenterden, Kent, 1798. Rayne, Essex, 1806. Chichester, Sussex, 1806, 1812, 1821.

Under Dimsdale's influence, mass inoculations increasingly became 'general' rather than 'partial'.[97] General inoculations usually involved a degree of compulsion, as was described by Cowper, the poet, in 1788:

[97] See, for example, T. Dimsdale, *Remarks On 'A Letter to Sir R. Barner ...'* (1779), p. 13; and Walker, *op. cit.*, p. 467, n.

"The smallpox has done, I believe, all that it has to do at Weston. Old folks, and even women with child, have been inoculated. ... No circumstances whatsoever were permitted to exempt the inhabitants of Weston. The old, as well as the young, and the pregnant, as well as they who had only themselves within them, have been inoculated ..."[98] An example of the effects of general inoculation is to be found at Calne, Wilts. A local surgeon, Mr Wayte, described in 1795 a general inoculation as follows: "In September, 1793, when the poor of the parish were inoculated. ... We inoculated six hundred and upwards ... Besides the poor, I inoculated about two hundred (private) patients.... Now in inoculating a whole parish, we have no choice of patients, all ages, and the sickly as well as others, were inoculated; but these were mostly children, as I assisted in inoculating the whole parish, about twelve or thirteen years ago."[99] According to the Calne parish register, the number of smallpox deaths declined as follows: 1723–42: 205; 1743–62: 122; 1763–82:54; 1783–1802: 8. The last mention of smallpox deaths is in 1793 when there were 6; previous to this there had been a minor epidemic in 1782 involving 10 deaths (this was the epidemic which provoked the earlier general inoculation mentioned by Wayte). These late eighteenth-century epidemics should be compared with the major ones in the early eighteenth century, for example, in 1732 there were 173 people in Calne registered as having died from smallpox.

[98] S. & B. Webb, *op. cit.*, p. 306, n. 2.

[99] Thomas Beddoes, "Queries respecting a safer method of performing inoculation", in Don A. De Gimbernat (Beddoes translated), *A New Method Of Operating For The General Hernia* (1795), pp. 56–9.

Chapter 2

Edward Jenner: The History of a Medical Myth[1]

This article derived from research on the history of smallpox inoculation (variolation). I argued in this essay that Jenner's vaccines were derived from smallpox virus, and that early vaccination was a more attenuated form of inoculation.

I believed that traditional inoculation did not spread natural smallpox, but I have since modified my view on this subject. I revised my view on the basis of evidence on the use of Jenner's vaccine in the United States. One of Jenner's vaccine threads was sent in 1800 to Benjamin Waterhouse, who practised in the Boston area. This appears to have produced a severe reaction, with all the classical symptoms of a heavy case of inoculated smallpox. Subsequently, the person "vaccinated" appears to have communicated natural smallpox through respiratory infection to other people. Eventually, Jenner's vaccine became attenuated by taking virus from a previous site of injection through arm-to-arm transmission. A full treatment of this subject is dealt with in my book, ***Edward Jenner: The History Of A Medical Myth.***

Note by the Editor of *Medical History*, Dr F. N. L. Poynter

The provocative title of Mr Razzell's article will doubtless shock many readers, but it is the duty of the historian to take nothing for granted and to put to the question periodically the major assumptions of history, just as it is an editor's duty to give space to iconoclasts as well as to idolists.

[1] First published in *Medical History*, Vol. 10, No. 3, July 1965.

The following article is frankly controversial and the editor considered its implications so important, both for medical history and current practice, that he has invited Professor A. W. Downie, M.D., F.R.S., of Liverpool University, an acknowledged authority in this field, to comment on Mr Razzell's arguments. The latter has claimed the right to reply to Professor Downie's criticisms and both comment and reply will be found at the end of the article. Discussion is now open to readers and any further discussion, by Professor Downie or others, will be published in forthcoming issues of *Medical History*. The editor confines himself to remarking that the October issue will contain an interesting account of smallpox in Ethiopia which may be read as an implicit refutation of Mr Razzell's case. Despite the long-continued use of inoculation in this close community, epidemics of smallpox raged until Jennerian vaccination was introduced in the nineteenth century. If Mr Razzell's article and the ensuing debate prove nothing else we are given a lively demonstration that medical history is by no means a dead subject but is concerned with issues which are very much alive.

F.N.L.P

The main aim of this paper is to argue that early vaccination was a more attenuated form of the eighteenth-century practice of inoculation.[2] In a paper on eighteenth-century population change,[3] I have argued that inoculation was effective in gradually eliminating natural smallpox, well before the advent of vaccination at the beginning of the nineteenth century. It is not possible to present the full evidence for this conclusion here, but selected statistics will serve to illustrate the nature of the hypothesis.[4]

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[2] Throughout this paper inoculation is used to mean variolation (except where stated), as this was the term used by eighteenth-century contemporaries, some of whose writings we shall be considering.

[3] See Chapter 1.

[4] None of the figures in this paper ought to be taken too literally, as there are many problems with regard to the classification of disease. However, smallpox is a sufficiently distinctive disease to enable us to use these figures as indications of trends.

Table 1. Smallpox Mortality from Epidemics in Boston, Mass., USA, in the Eighteenth Century[5]

	1677–8	*1702*	*1721*	*1730*	*1752*	*1764*	*1776*	*1788*	*1792*
Population	4,000	6,750	10,700	13,500	15,684	15,500	—	—	19,300
Natural Smallpox									
Cases			5,759	3,600	5,545	699	304	122	232
Smallpox Deaths	700	213	842	500	539	124	29	40	69
Deaths per 1,000 cases			146	139	97	177	95	328	298
Inoculated Smallpox									
Cases			287	400	2,124	4,977	4,988	2,121	9,152
Deaths			6	12	30	46	28	19	179
Deaths per 1,000 cases			21	30	14	9	6	9	20
Total Smallpox Deaths	700	213	848	512	569	170	57	59	284
Smallpox Deaths per 1,000 population	175	32	79	37	36	11	—	—	15
Left the town					1,843	1,537			262
Escaped disease in town					174	519			221
Had smallpox before					5,998	8,200			10,300

Three important conclusions are to be derived from this table. First, that the smallpox death rate was reduced from 175 smallpox deaths per 1,000 living in 1677–8 to 15 per 1,000 by 1792. Second, this was achieved in spite of an increase in the virulence of the disease. Third, the reduced mortality may be directly attributed to inoculation, which protected the vast majority of the vulnerable population by the end of the eighteenth

[5] J. Blake, *Public Health In The Town Of Boston (Mass.), 1630–1822* (1959), p. 244. *Royal Commission On Vaccination, 6th Report* (Parl. Papers 1896/47), p. 762. H. R. Viets (ed.), *A Brief Rule To Guide The Common People Of New England* (1937), p. 35. The figures in this table do not balance, as some people inoculated were not inhabitants of the town, and were therefore not included in the total population.

century. An example of the effects of inoculation on smallpox mortality in England is to be found in eighteenth-century Maidstone.

Table 2. Smallpox Mortality at Maidstone, Kent, 1752–1801[6]

Period	*Smallpox burials*	*All burials*
1752–1761	252	1,703
1762–1771	76	1,426
1772–1781	60	1,549
1782–1791	91	1,676
1792–1801	2	2,068

A mass inoculation was conducted by Daniel Sutton in 1766 and its effects were described by Howlett in 1782:

> Upon casting an eye over the annual list of burials we see that, before the modern improved method of inoculation was introduced, every five or six years the average number was almost doubled; and it was found upon enquiry, that at such intervals nearby the smallpox used to repeat its dreadful periodical visits ... in the short space of thirty years it deprived the town of between 500 and 600 of its inhabitants; whereas in the fifteen or sixteen years that have elapsed since that general inoculation it has occasioned the deaths of only about sixty.[7]

The main reason why most historians thought that inoculation had been ineffective against smallpox was the set of smallpox mortality statistics for London. These were faulty in several ways,[8] but must be reinterpreted in the light of the fact that inoculation was utilized on a large scale much later in London than in the rest of the country, especially outside large towns.[9] Howlett stated this quite explicitly in 1781:

> It may be thought, at first sight, that the healthiness of London is more increased than that of country towns. ... But it must be remembered that the diminished mortality in the latter appears to be chiefly owing to the salutary practice of inoculation, whereas in the

[6] Figures compiled from the Maidstone Parish Register.

[7] J. Howlett, Observations On The Increased Population ... Of Maidstone, (1782), p. 8.

[8] For example, no account is taken of the increased number of births.

[9] See Chapter 1.

> former, for want of universality, it has hitherto been of little advantage. ... In provincial towns and villages, so soon as this disease [smallpox] makes its appearance, inoculation takes place amongst all ranks of people; the rich and the poor, from either choice or necessity, almost instantly have recourse to it; and where 200 or 300 used to be carried to their graves in the course of a few months, there are now perhaps not above twenty or thirty.[10]

It is in the light of these findings that we must re-examine the relationship between inoculation and vaccination. One aspect of the conventional medical view of the relationship is that inoculation differs from vaccination inasmuch as it gives rise to pustular eruptions other than at the site of injection and is consequently a source of infection to an unprotected population.[11] There is contemporary eighteenth-century evidence to suggest, however, that this was not always the case. None of the hundreds of incumbents making returns in the *Statistical Account Of Scotland* at the end of the eighteenth century, specifies a case of inoculation spreading smallpox, in spite of the partial inoculations of the gentry and farmers in some parishes.[12] According to a letter sent from the Council of Geneva in 1791:

> An epidemic of smallpox is of almost regular occurrence every five years, and between the epidemics it frequently happens that we have no natural smallpox whatever, little in the city or its vicinity. Inoculation began to be practised here in 1751, since which date we have inoculated a very large number of children annually, and with such marked success that the deaths have not exceeded 1 in 300. Although we have often had to inoculate with pus brought from a distance at times when there was no smallpox to be found in the city, and although children so inoculated have gone freely into the streets,

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[10] J. Howlett, *An Examination Of Dr Price's Essay On The Population Of England And Wales* (1781), p. 94.

[11] The traditional medical view of the relationship is that inoculation uses smallpox virus, whereas vaccination uses cowpox virus.

[12] See Sir J. Sinclair (ed.), *The Statistical Account Of Scotland*, 21 Vols. (1791–99). (1994): Since I wrote the above in 1965, I have modified my view: I now believe that in very severe cases of inoculated smallpox, inoculation did on occasions spread the respiratory form of the disease.

> walks, and other public places, before, during, and after the eruption, we have never observed that they were sources of contagion, nor that they produced any intermediate epidemics, nor that they accelerated the return of the periodical epidemic.[13]

An almost identical description was sent from the Hague:

> The 200 persons who were inoculated at the Hague, about the end of the year 1768, without much regard to themselves or others, frequented all places of public resort; notwithstanding which no epidemic was produced, nor in the whole year did more than eight persons die of the smallpox, and of these three died in the spring, one by inoculation, and two by the natural disease, which they had caught at some other place and carried with them to the Hague, and the remaining five died towards the end of the year.[14]

There were similar experiences noted at Chester[15] and at Ware, Herts.,[16] and many inoculators were well aware that their patients were not a source of contagion. The most convincing evidence of the relative non-contagiousness of inoculation is provided by a series of experiments conducted during the late eighteenth century by Dr O'Ryan, Professor of Medicine at the College of Lyons, France, part of which he described as follows:

> I placed a person in the eruptive fever of the smallpox by inoculation at the distance of about half a yard from four children properly prepared; each exposure continued one hour, and was repeated daily for a fortnight, reckoning from the commencement of the fever till the pustules were become perfectly dry: not one of the four received the infection. Two months afterwards, I inoculated three of these children; they had the distemper in a very mild manner and recovered without difficulty.[17]

O'Ryan concluded from his experiments

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[13] J. Haygarth, *A Sketch Of A Plan To Exterminate The Casual Smallpox* (1793), pp. 472–5.

[14] *Gentleman's Magazine*, Vol. 47 (1777), p. 224.

[15] J. Haygarth, *An Inquiry How To Prevent The Smallpox* (1785), p. 588.

[16] J. C. Lettsom, *A Letter ... Upon General Inoculation* (1785), p. 11.

[17] J. Haygarth, *A Sketch Of A Plan ...*, pp. 82, 83.

> that there is no risk of contracting it [smallpox], provided the person who is liable to the infection, keeps himself at a very little distance from patients in the smallpox, or from things which they have touched.[18]

Although we now know this view to be erroneous, we must still explain the results of his experiments. A clue to the answer to our problem is to be found in Dixon's recent text on smallpox. In discussing the infectivity of scab virus he writes: "... in practice scab virus seems to lack epidemic potential. I have suggested (Dixon 1948) that the virus extruded through the skin, perhaps modified by its passage, is in some way different from the virus from the respiratory tract."[19] Logically, the opposite also applies, that is to say, the virus injected through the skin is also modified in some fundamental way. Therefore an inoculated person, would be less infectious, as all the smallpox viruses in his body would have derived from a stock of modified virus extruded through the skin of another person's body (the person from whom the virus was originally taken) and then passed through his own skin. As the degree of infectivity of smallpox is probably connected with the degree of severity of the disease,[20] we would expect the transmission of the virus through the skin to produce milder forms of smallpox. This is in fact what happened, as all the inoculators well knew. Mowbray, Gatti and the Suttons all produced much milder and safer results from inoculation by arm-to-arm transmission. Gatti ran into difficulty over his inoculation in 1765 of the Duchess de Bouffle, who had no pustular eruption except at the site of inoculation and suffered an attack of natural smallpox two-and-a-half years later,[21] a problem which would occupy the vaccinators forty years later. Gatti appears to have achieved these very mild results by taking the smallpox virus for his inoculations from the site of a previous inoculation, rather than from one of the pustular eruptions around the body.[22]

Fortunately, we have some experimental evidence on the degree to which smallpox virus can be attenuated. In 1777 John Mudge, a Plymouth surgeon, reported the following experiment:

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[18] *Ibid*, pp. 78, 79.

[19] C. W. Dixon, *Smallpox* (1962), p. 298.

[20] *Ibid*, p. 298.

[21] C. Creighton, *The History Of Epidemics* (1894), Vol. 2, pp. 495, 496.

[22] A. Gatti, *New Observations On Inoculation* (1768).

> Messrs. Longworthy and Arscott, surgeons, in the spring of 1776, inoculated at Plymton, a neighbouring town, forty patients; of which number, thirty were injected with crude matter from the arm of a young woman [from the site inoculation before the eruption of pustules], five days after she herself had been inoculated with concocted matter [from a pustule around the body], which eventually did produce in her a pretty smart fever, and a sufficient number of eruptions. The other ten were inoculated with matter of another kind, which I procured, in a concocted state, from a pustule of the natural smallpox. The arm of all the forty patients took the injection; and the latter ten, after the eruptive fever, had the smallpox in the usual way. Of the other thirty, though the injection took place on their arms, so as to inflame them considerably, and to produce a very large prominent pustule, with matter on it, on each of them, yet not one of them had any eruptive fever, or a single subsequent eruption, on any part of the body. ... It is to be remarked too that the matter which was in those pustules having been used to inoculate others produced on them exactly the same appearances, unattended also with either fever or smallpox.[23]

In other words it was possible to attenuate the smallpox virus to such an extent that only a single pustule was produced at the site of inoculation and this was achieved by taking the virus from the site of a previous inoculation. Adams repeated the same experiment at the beginning of the nineteenth century and was able to produce a whole series of cases in which there was only an eruption at the site of inoculation. He compared the latter with typical vaccine vesicles and claimed that they were identical.[24] This was a conclusion confirmed by Guillou, who was also able to produce a typical vaccine vesicle at the site of inoculation.[25] Dr John Walker, Director of the Royal Jennerian Society, wrote to Lettsom in 1813:

> I have, from the first introduction of vaccination, after having observed its symptoms and progress, entertained an opinion respecting its native difference from those who suppose it a substitute only for

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[23] J. Mudge, *A Dissertation On The Inoculated Smallpox,* (1777), pp. 20–22.

[24] See *Royal Commission On Vaccination, 4th Report* (Parl. Papers, 1890–91/44), p. 52.

[25] *Ibid,* p.53.

> the [inoculated] smallpox.... Now I have from an early part of my practice, been in the habit of diluting the smallpox virus with water, previous to its introduction into the system, and in every instance I have then always found the disease mild, and the fever slight: this led me to the conclusion above hinted at.... I believe the variola and vaccine (so called) to be, at bottom, the same disease, and could wish that the term variola mitior were employed instead.[26]

Walker was using smallpox virus as the source of his 'vaccine' and as Creighton observed, "the very Director of the Jennerian Institute was among the prophets of the old inoculation."[27] However, from our present point of view what is significant is that Walker was able to produce the single local vesicle typical of vaccination, through a process of attenuating smallpox virus.

It is in the light of these neglected facts that we must reinterpret the history of vaccination itself. After a few initial experiments with cowpox in 1796 and 1798, Jenner's original vaccine lymph was lost, and it was not until the end of January 1799, when cowpox was discovered in Gray's Inn Lane by Woodville, that experiments were resumed. Woodville immediately sent Jenner some lymph to check its suitability. With this lymph, Jenner operated on twenty persons and reported to Woodville: "Berkely, February 1799. The rise, progress, and termination of the pustules created by the virus were exactly that of the true cowpox".[28] Woodville was completely confused about the relationship between vaccination and inoculation, and later wrote: "The virus which Dr Jenner declared to be perfectly pure and genuine was taken from the arm of a [smallpox] hospital patient who had 310 pustules, all of which suppurated".[29] Woodville, who was a doctor at the London Smallpox Hospital, had found that a majority of 500 people vaccinated by him had pustular eruptions similar to those that took place during inoculation. The conventional medical explanation of this is that repeated recently by Dixon:

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[26] T. J. Pettigrew, *Memoirs Of The ... Late John Coakley Lettsom* (1817), Vol. 3, pp. 350, 351.

[27] Creighton, *op. cit.*, p. 590.

[28] Whilliam White, *The Story Of The Great Delusion* (1885), p. 147.

[29] *Ibid*, p. 149.

> Unfortunately Woodville vaccinated his cases at the Smallpox Hospital, and at least two-thirds of them showed some general eruptions. It is almost certain, that under these circumstances the patients were either inoculated [injected] with a mixture of vaccine and variola virus from contaminated lancets, were vaccinated and naturally infected with smallpox at the same time, or, in some cases, were vaccinated and then variolated from three to five days later, when they again had a double infection.[30]

This interpretation neglects a considerable body of evidence to the contrary, particularly that supplied by Jenner himself. At the beginning of 1800 he wrote a letter to Lord Egremont, one of his patrons, who had complained that some of the vaccine sent from London had produced pustular eruptions when used on his family at Petworth. Jenner wrote:

> In many places where the [vaccine] threads were sent, a disease like a mild smallpox frequently appeared; yet, curious to relate, the matter, after it had been used six or seven months, gave up the variolous character entirely, and assumed the vaccine; the pustules declined more and more, and at length became extinct. I made some experiments myself with this matter, and saw a few pustules on my first patients; but in my subsequent inoculations [vaccinations] there were none.[31]

It is quite clear from this letter that the conventional medical explanation (e.g. Dixon's) of the pustular eruptions in Woodville's cases of vaccination is incorrect, for pustular eruptions occurred outside the London Smallpox Hospital where contaminated lancets, mixed injections or natural smallpox cannot be invoked as explanations (this is particularly true of Jenner's own cases). These pustular eruptions gradually disappeared as the new vaccine was transmitted from arm-to-arm, using the site of a previous inoculation. Thus Jenner's vaccine was probably smallpox virus, which was attenuated in a manner already familiar to some of the inoculators. The vaccinators were producing results similar to those produced by Arscott and Longworthy, Gatti, Adams and Walker, through taking smallpox virus from sites of previous inoculations and transmitting

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[30] Dixon, *op. cit.*, pp. 119, 120.

[31] J. Baron, *Life Of Dr Edward Jenner* (1827), vol. 1, pp. 314, 342.

it from arm-to-arm; the only difference being that they thought that they had discovered a new process. Arscott, Longworthy and Mudge had rejected this attenuated technique which produced only a local pustule, as they felt it would give insufficient protection against future attacks of smallpox (in this they were right) and it was left to the vaccinators to utilize unknowingly the same technique twenty-four years later. However, we must still try to explain what the relationship is between the cowpox and smallpox viruses. Unfortunately, the virologists do not seem to be in a position to settle this problem and it is not even agreed whether the one virus is autonomous of the other.[32] According to one authority:

> At the present day the general opinion agrees with that held by Jenner, that cowpox is simply smallpox much modified by passage through the cow. It might be supposed that this fact would be one easy of demonstration, and cows have by many observers, e.g. Woodville in 1799, by Ceely, by Badcock, and by Thiele of Kazan in 1838, been experimentally inoculated with smallpox but in most cases the disease, when thus artificially produced in cows, appears to retain a considerable degree of virulence, and to produce general though slight symptoms when again communicated to human beings, instead of the purely local symptoms of ordinary vaccinia.[33]

Copeman attempted to explore the relationship between smallpox and cowpox experimentally:

> He first inoculated a monkey with smallpox virus and then inoculated a calf from such an infected monkey. This resulted in typical vaccine, from which good strains of vaccine lymph were obtained. On the basis of this experience Copeman suggested that cowpox may actually have originated in the eighteenth century from inoculated smallpox, as the local sore produced by the inoculated incision frequently was very itchy, and milkers who scratched their arms may easily have conveyed infectious matter to the cow's udder.[34]

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[32] Dixon, *op. cit.*, pp. 119, 120, 163.

[33] W. A. R. Thomson, *Black's Medical Dictionary,* 1963, p. 942.

[34] G. Miller, *The Adoption Of Inoculation ... In England and France,* (1957), pp. 19, 20.

From our point of view the transmission of the smallpox virus through a cow or any other non-human animal, is an irrelevance, inasmuch as smallpox inoculation can be attenuated into vaccination merely by arm-to-arm transmission of the virus, using the previous sites of inoculation. This hypothesis is the only one to explain the manifold contradictions contained in all the evidence. This includes the phenomena of generalized vaccinia which on the present hypothesis is nothing other than what eighteenth-century contemporaries would have considered a typical inoculation. It would also explain why "although vaccinia and cowpox have common features of wide host range, serologically variola is more closely related to vaccinia".[35]

Does this conclusion mean that the reputation of Jenner is undeserved? He, who had been inoculated in the old method as a boy during the mass inoculation at Wootton-under-Edge in 1756, was an inoculator using the Suttonian method before he claimed to have discovered vaccination.[36] The only advantage the latter had over the more traditional methods of inoculation was that it appeared to cause fewer direct deaths. The problem in evaluating this claim is that many deaths were attributed to inoculation, which were probably due to the fact that many people had caught smallpox before being inoculated. Thus for example, in Boston, Mass., inoculation was forbidden by law and was only allowed when the presence of an epidemic created such panic as to make it inevitable. As several thousand people were inoculated, some of them would have caught smallpox before being inoculated, and their subsequent deaths would be incorrectly attributed to the inoculation. In more controlled conditions the death rate from the mild Suttonian method of inoculation was virtually nil. The Sutton's claimed in 1768 "that about 55,000 had been inoculated by them since the year 1760, of which number, six only had died".[37] Among the 5,694 people inoculated at the London Smallpox Hospital during the years 1797–9 there were only nine deaths. By the beginning of the nineteenth century the inoculators had attenuated their viruses sufficiently to be able to eliminate the risk of death altogether; for example Dr Forbes, a supporter of vaccination and an opponent of inoculation, had to report that of the 2,500 people

[35] Dixon, *op. cit.*, p. 163.

[36] J. J. Abraham, *Lettsom* (1933), p. 192.

[37] R. Houlton, *Indisputable Facts, Relative To The Suttonian Art Of Inoculation* (1768), p. 10.

inoculated in the Chichester area in 1821 not one died.[38] Inoculation had the advantage over the more attenuated vaccination of conferring a much longer period of immunity against future attacks of smallpox, and this was of course because of the larger numbers of antibodies produced. This much greater period of immunity was no mean advantage at a time when smallpox was such a constant threat.

Generally we must conclude that Edward Jenner's contribution to the history of medical innovation has been greatly over-estimated, and at most he was one of many innovators in the technique of inoculation against smallpox.

Comment by Professor A. W. Downie, M.D., F.R.S.

I have read through this paper carefully and it appears to me that the author has been very selective in quoting sources to uphold his thesis.

In his general proposition that the reduction in the smallpox death rate between 1677 and 1792 was due to smallpox inoculation, he has ignored the importance of other factors. It is true that the figures from Boston (Table 1) would appear to lend some support to his thesis, but he ignores the fact that in Boston very strict quarantine regulations were enforced to prevent the introduction of smallpox into that City. Isolation of cases when they occurred was strictly enforced. This and the quarantine regulations introduced to prevent the importation of smallpox into the town, were probably more important measures than inoculation in determining the diminution in incidence of the disease over the period covered in Table 1.

The author appears to believe that by the end of the eighteenth century inoculation of smallpox was very widely and generally applied. This would seem very far from being the case. (Up to 1764 only 5,554 persons in the whole of Scotland had been inoculated with smallpox according to Alexander Monro Senior.) It is obvious from Haygarth's correspondence published in his *Sketch* (1793) and in the letter to Percival of Manchester, that after the first few years of the introduction of inoculation against smallpox in Chester, the poor people in the town would not avail themselves of this measure. Indeed, he regrets that no-one had come forward

[38] Dr J. Forbes, "Some account of the smallpox lately prevalent in Chichester and its vicinity", *London Medical Repository*, (1822), pp. 211–15.

at all for inoculation and that the poor preferred to acquire the disease in the natural way. With reference to the diminution of smallpox as a result of inoculation towards the end of the eighteenth century, he states that in 1774 only $\frac{1}{14}$th of the population of Chester had not suffered from the disease. This was at the time when inoculation of the smallpox was not available to the poorer people in the town. Similar observations were made in Leeds and Newcastle. So much for the author's suggestions that inoculation had greatly lessened the ravages of smallpox by the end of the eighteenth century! It seems much more likely that the diminished mortality from the disease at this time was due to the recognition of the infectious nature of the disease and measures of isolation being introduced to prevent its spread, such as the provision of isolation wards in hospitals and isolation of patients at home, together with improved housing and nutrition of the poor.

The author also quotes reports apparently showing that the inoculated disease was not infectious. This, however, is not supported by evidence from other sources. Maitland recorded in 1722 that the little girl, Mary Batt aged two years, who was inoculated by himself, infected six domestic servants with typical smallpox from which one of them died. It is also apparent from Haygarth's *Sketch* (1793) that the disease was frequently spread from inoculated to susceptible persons. Indeed, it was an essential part of Haygarth's plan that those inoculated with smallpox must isolate themselves at home to avoid spreading infection. He quotes several instances where such spread did in fact occur. The author of the manuscript has made selections from the letters published in Vol. 2 of the *Sketch*, choosing those purporting to show that the inoculated disease was not infectious. He has ignored other letters in the same volume which provide evidence of spread of infection from inoculated persons. He also ignores the fact that even the casual smallpox is not as highly infectious as many people think – a point also stressed in all Haygarth's writings.

There is evidence from the observations of the Suttons, Dimsdale and others, that the introduction of the Suttonian technique, of taking material from the site of inoculation of the smallpox after four or five days for further inoculations, produced a milder type of inoculation smallpox than had previous practice. When this technique was followed the mortality from the inoculated disease became much less than in the earlier years (1721–30), but even at the end of the eighteenth century most authorities agreed that the mortality from inoculated smallpox was still of the order of $\frac{1}{200}$ to $\frac{1}{500}$.

The author's main contention that the vaccinia virus now employed has been derived from smallpox virus attenuated by repeated passage through arm vaccination, may be true, but proof of this is not available at the present time. The strains at present in use for vaccination have been so long passed in laboratory animals that the history of their origin is uncertain. It has, however, I think been established from Jenner's experiments and those carried out with fresh stocks of cowpox in 1799, that cowpox infection did protect against smallpox. It is true, as Dixon has maintained, that Woodville's experiments were unreliable in that his inoculations of cowpox were carried out in a smallpox hospital and many of the subjects were subsequently tested by variolation a few days later. These two facts made it very difficult to be sure that Woodville's observations had much bearing on the value of cowpox virus as an immunizing agent. It is, however, also clear from the observations of Ceely, published from 1839 onwards, that inoculation of genuine cowpox virus would protect against smallpox. Ceely gave very clear descriptions of the effects of inoculating cowpox virus on humans and, indeed, isolated fresh stocks of virus from the natural disease in cows or persons infected from them. In my opinion, Dixon's comments on Woodville's work are quite justified.

The author mentions the adaptation of smallpox virus to propagation in the cow. Many such observations of this kind were recorded in the nineteenth century but they are all of doubtful value because cowpox was sometimes inoculated on the same animals and the later experiments were carried out (e.g. Copeman's) with variola virus in institutes where strains of vaccinia were also in use. French workers showed many years ago that vaccinia virus spread very readily amongst cows and suggested that many of the reported successful inoculations with variola in cows were, in fact, cross infection of the animals with strains of vaccinia in use in the same establishments. All recent attempts (in the last twenty-five years) to infect cows with smallpox virus and to pass the virus to successive animals, have failed, even when the monkey has been used as an intermediate host. (Our own attempts to convert variola to vaccinia by inoculation of animals have been completely unsuccessful.) (See also Herrlich *et al. Arch. ges. Virusforschung,* 1963, 12, 579.)

We have no doubt that cowpox is a natural disease of cattle and is not derived from variola. We have isolated at least a dozen strains of cowpox virus from the natural disease of cattle or from lesions on the hands of those working with infected animals. All these strains of virus are quite

different from strains of variola virus and also from current strains of vaccinia virus. However, these strains of cowpox virus are immunologically practically identical with vaccinia virus and with variola virus. Immunisation of animals with cowpox virus produces antibody which is apparently effective against variola and vaccinia viruses. Our cowpox strains have the same host range as vaccinia strains and can be readily passed on to a variety of laboratory animals. This feature is not shown by a number of strains of variola virus which we have tested in this way.

I apologize for writing to you at such length, but I cannot agree with much of the argument in the enclosed manuscript. The author has selected to support his thesis only such evidence as would suit his purposes and has neglected many other works which would appear to refute his arguments.

Mr Razzell's reply

I will deal with Professor Downie's points in the order that they were raised. He writes: "This [the isolation of smallpox cases] and the quarantine regulations taken to prevent the importation of smallpox into the town, were probably more important measures than inoculation in determining the diminution in incidence of the disease over the period covered by Table 1". Yet if you look at Table 1 you will see that the numbers escaping the disease in and out of town amount to only 483 people out of a total population of 19,300 in 1792. Table 1 unequivocally demonstrates that the diminution in the number of smallpox deaths may be directly attributed to the effects of inoculation.

The second point raised concerns evidence for the hypothesis that inoculation did spread natural smallpox to an unprotected population. Maitland's example of an inoculated two-year-old girl spreading the disease to six domestic servants is cited. This incident occurred in 1722 when the English inoculators invariably used natural cases of smallpox as the source of their virus. As Dixon has written:

> In spite of the warnings in the earlier writings of the desirability of sending someone else to collect the smallpox matter so as to avoid infecting the inoculated person simultaneously with the natural disease (from respiratory virus on clothing, or in other ways from an infectious patient), it seems clear that Armyand as well as Maitland did not realize the effect of inoculating simultaneously with, or after

> contact with, natural smallpox in confusing the statistics of inoculation.[39]

The standard practice of later inoculators was to take smallpox matter from previously inoculated cases or to carry it with them dried on threads, thus avoiding the problem of transmitting the infection from natural cases. Even if we reject Dixon's point, Maitland's example of the danger of inoculation is very suspect, because many cases of natural smallpox were to be found in London every week of every year during this period (see the London Bills of Mortality) – therefore it is quite possible for the domestic servants to have caught the disease naturally from another source. A much better type of evidence is that referring to a situation where a partial inoculation takes place in an isolated rural area in response to the threat of an epidemic. In the twenty-one volumes of the *Statistical Account Of Scotland* many of the incumbents described the recent history of diseases in their parishes – of the 234 incumbents who mentioned that inoculation has taken place in their parishes not one specified an instance of it spreading the natural disease to vulnerable members of the population. An even more convincing example of this point is supplied by Dr John Forbes (a supporter of vaccination and an opponent of inoculation), who in his description of the smallpox epidemic of 1821 in the Chichester area had to admit that

> during the winter months he [a local inoculator] inoculated upwards of 1,000 persons [around the country area outside Chichester] ... not more than 130 or 140 cases of natural smallpox were witnessed by all the surgeons during the course of the epidemic. Of these, by far the greater number occurred in Chichester, owing to the continued resistance of the surgeons to inoculate.[40]

Professor Downie goes on to point out Haygarth's belief in the contagiousness of inoculation. All contemporaries believed that inoculation spread smallpox, inasmuch as they believed it to be itself a mild form of natural smallpox. However, when it came to a question of empirical evidence rather than theoretical belief, there is no doubt about the

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[39] C. W. Dixon, *Smallpox* (1962), p. 232.
[40] Forbes, *op. cit.*, pp. 213, 215.

conclusion to be drawn. Haygarth himself concluded in 1781 from his experience in Chester that

> Inoculation did not, as some might apprehend, spread the contagion, but appeared to produce a quite contrary effect. For in the districts where most patients were inoculated, there remained the fewest in the natural smallpox, and, in the districts where the smallest number were inoculated, the distemper was afterwards most general.[41]

The most conclusive evidence for the relative non-contagiousness of inoculation is the series of experiments by O'Ryan which were quoted in the text of my paper. Early 'vaccines' were directly derived from smallpox virus without transmission through a cow (e.g. Walker's 'vaccine'), and it has never been suggested that such 'vaccines' spread natural smallpox.

According to Professor Downie the Suttonian technique consisted "of taking material from the site of inoculation of the smallpox after four or five days for further inoculation". This was not the case – the Suttons took their material from any pustule around the body and not just from the site of inoculation; they also took their material from pustules at every stage of development.[42]

The essence of their technique was the use of a lancet, making the lightest of scratches and inserting the minimal amount of material. As for the mortality from inoculation, it is very difficult to assess independently of mortality due to natural smallpox before inoculation had time to take effect. As I have already indicated, in the controlled conditions of the London Smallpox Hospital its mortality was negligible, particularly in the later period – e.g. of the 431 in-patients inoculated between 1808 and 1813 not one died.[43] Pearson, one of Jenner's chief supporters, conceded that the mortality from inoculation was negligible and quoted two examples:

> Dr William Heberden informs me, that at Hungerford, a few years ago, in the month of October, 800 poor persons were inoculated for the smallpox, without a single case of death. No exclusion was made on account of age, health, or any other circumstances, but pregnancy;

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[41] Haygarth, *An Inquiry ...*, p. 188.

[42] R. Houlton, *Indisputable Facts Relative to the Suttonian Art of Inoculation* (1768) p. 40.

[43] R. Hutchinson, "A historical note on the prevention of smallpox in England", *Health Annual Report* (Ministry of Health, 1945) Vol. 46. Appendix A, p. 122.

> one patient was eighty years of age; and many were at the breast, and in the state of toothing. Dr Woodville acquaints me, that in the current year (1798), from January to August inclusive, out of 1,700 patients inoculated at the Inoculation Hospital, including the in and out patients, only two died, both of whom were of the latter description.[44]

It should also be remembered that Walker's 'vaccine' which was the one most widely used in early nineteenth-century England, was in fact diluted and attenuated smallpox virus – and it gave rise to a negligible rate of mortality.

I have not disputed the power of cowpox to protect against smallpox, but have argued that vaccinia was directly derived from smallpox. Professor Downie counters this point by stating that it has been impossible during the past twenty-five years to infect cows with smallpox virus, i.e. produce cowpox from smallpox. He suggests that the very many previous successes in doing this were due to "cross infection of the animals with strains of vaccinia in use in the same establishments." This argument is implausible given that the purpose of trying to infect cows with smallpox was not experimental, but was an attempt to produce vaccinia which was otherwise not available. Vaccines were difficult to maintain and acquire, hence the attempts to produce them 'artificially'.[45] This being so it is highly unlikely that vaccinia was present in these establishments. If "cowpox is a natural disease of cattle" why is it not to be found in New Zealand where there is little or no smallpox and vaccination, and why do not cases of human cowpox arise in slaughterhouse workers ? As Dixon has said: "This would suggest that cowpox is not a natural disease of bovines".[46] Cowpox appears to have increased considerably with the advent of inoculation in the eighteenth century and declined during the nineteenth and twentieth centuries when inoculation disappeared and the amount of vaccination diminished. This would suggest that Copeman was right in thinking that for smallpox to be suitable for adaptation to the cow it must be taken from an inoculated rather than a natural case (it should be noted that there were several mass inoculations in Gloucestershire at about the time that Jenner

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[44] G. Pearson, *An Inquiry Concerning The History Of The Cow Pox* (1798), p. 79.
[45] See for example J. Jones, *Vaccination* (1884), pp. 401–3.
[46] Dixon, *op. cit.*, p. 162.

discovered his first cases of cowpox, for example in 1795 at Berkeley and at Dursley in 1797 when over 1,100 people were inoculated). However, for the purposes of my paper it is not necessary to demonstrate that cowpox derives from smallpox, but merely to show that the early 'vaccines' were directly derived from smallpox without using an intermediary host such as the cow.

As for Professor Downie's last point about inoculation not being very widespread at the end of the eighteenth century, I have dealt very fully with this question in the paper to be published in the *Economic History Review*.[47] In fact the best evidence is to be found in the writings of Jenner and his early supporters, e.g. Jenner wrote: "... the common people were rarely inoculated for the small-pox, till that practice was rendered general by the improved method introduced by the Suttons ..."[48] These early writings are full of references to mass inoculations, and most of Jenner's cases of people with natural cowpox had been inoculated at some time during their lives. Professor Downie takes the experience of the towns as typical for the country as a whole, but only a small minority of the total population lived in such towns. In a country village or market town epidemics of smallpox were very infrequent, sometimes occurring only every twenty or thirty years. When such an epidemic did occur it struck such a large proportion of the total population (children and adults) and was so virulent (lack of a pool of antibodies) that the resulting panic drove everyone to be inoculated, for example, when an epidemic broke out in Blandford, Dorset, in 1766 "a perfect rage for inoculation seized the whole town".[49] In a place like Chester only a fourteenth of the population (all infants) had not suffered from smallpox, because it was virtually endemic, that is to say, in the town nearly every year. This bred a fatalistic attitude amongst the parents of poor children, particularly as the piecemeal nature of smallpox mortality did not lead to a spectacular demonstration of the effects of inoculation as it did in the country areas. Inoculation was virtually universal in such areas after about 1770, and was also making rapid headway in the large towns by the end of the eighteenth century.

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[47] See the previous chapter.

[48] *The Medical Repository*, Vol. 5 (1802), p. 239.

[49] C. Creighton, *The History Of Epidemics*, Vol. 2, (1894), p. 513.

Chapter 3

Population Growth and Economic Change in Eighteenth- and Early Nineteenth-Century England and Ireland[1]

Much more has been written about the history of inoculation in England than Ireland. Other than the additional material in my book, ***The Conquest Of Smallpox,*** *I am not aware of any further discussion of Irish inoculation history. The evaluation of inoculation in Ireland suffers from an absence of reliable demographic evidence for the eighteenth-century period.*

I did not give sufficient weight in this essay to the colonial relationship between England and Ireland, involving the transfers of wealth from the latter to the former. However, this is implicit in my discussion of Irish social structure, and the analysis of the effects of population growth could be broadened to take account of this omission.

In papers on this important and controversial subject Professor David Chambers has eloquently argued that although population growth and economic change were linked in eighteenth-century England, the increase in population cannot be explained directly in economic terms.[2] Traditional

[1] First published in E. L. Jones and G. E. Mingay (eds.), *Land, Labour And Population In The Industrial Revolution: Essays Dedicated To David Chambers* (1967).

[2] See particularly J. D. Chambers, "The Vale of Trent 1670–1800", *Economic History Review*, Supplement 3. He concluded from this study that population "was vulnerable to disease, but not as a result of famine. Epidemics could do their own

'medical' explanations of falling mortality have been discredited by medical historians, which has led them to an assume that economic growth must have preceded and brought about population expansion. In this essay I try to deal with some of the important problems raised by Professor Chambers, and will argue that the increase in population during the eighteenth and early nineteenth centuries was not primarily brought about by economic factors, but on the contrary, itself shaped the pattern of economic change.

I

The neo-Malthusian view of eighteenth-century population growth is that it was the consequence of increased fertility, resulting from earlier and more frequent marriage due to expanding employment opportunities and a rise in the general standard of living. However, there is evidence to suggest that both the age at marriage and the marriage rate were roughly constant throughout the eighteenth century.[3] Professor Chambers himself has published statistics for agricultural villages which suggest that both the birth and marriage rates may have declined between 1743 and 1801 in the Vale of Trent region.[4] In 1751 Thomas Short published statistics of population, baptisms, marriages, and burials during 1724–36 for seven market towns and fifty-four rural parishes.[5] According to his figures, the baptism rate was 33.8 per 1,000 and the burial rate 29.4 per 1,000.[6] If we

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work without its aid, nor it would seem, did they require the assistance of gin ... For reasons which are far from clear, [disease] severity was mitigated from the middle of the [eighteenth] century in this region, especially in regard to the lower age groups ..."

[3] The figures for the age at marriage are derived from marriage licences which are not entirely satisfactory. However, figures from parish registers suggest a similar conclusion. See C. C. Morell, "Tudor marriages and infantile mortality", *Journal Of State Medicine*, XLIII (1935), p. 179. [1994] For a further discussion of these issues, see Chapter 7.

[4] Chambers, *op. cit.* p. 55.

[5] T. Short, *New Observations On Bills of Mortality* (1751), p. 133.

[6] Undoubtedly some births and deaths were not registered owing to the presence of Dissenters, particularly in the market towns. This, of course, would raise both the 'true' birth and death rates.

compare these rates with those computed from civil registration returns in the 1840s, it appears that the long-term birth rate was more or less constant, while there was a sharp fall in the death rate. This overall conclusion is confirmed by the figures for agricultural villages published by Chambers.[7]

One of the weakest points in the neo-Malthusian argument is that the fairly reliable figures of the 1840s indicate no particular association between the distribution of industry and high fertility rates. The counties with the highest age-specific birth and marriage rates and the lowest age at marriage during the early 1840s were Cambridge, Bedford, Huntingdon, and Northamptonshire, all largely agricultural counties. Although Lancashire had a high crude birth rate, its age-specific birth rate and age at marriage appear to have been about average.[8] Furthermore, the age at marriage of spinsters appears to have varied little between different social strata during the eighteenth century, suggesting that economic considerations were not paramount in determining the age at marriage at least for women.[9]

It is difficult to reach reliable conclusions from the statistics derived from the Anglican parish registers. The figures for burials are probably more accurate than those for baptisms, as few Nonconformists were buried outside the Anglican Church,[10] and the main reason for the under-registration of deaths was probably the existence of private burial grounds in the

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[7] Chambers, *op. cit.*, p. 55. The reverse of these trends applied, however, in the town of Nottingham.

[8] The age at marriage in Lancashire in the 1840s was about the same as for the country as a whole. The ranking of age-specific birth-rates varies considerably according to which age group of women is considered; if the age group 20–30 is taken the age-specific birth-rate was below average, for the age group 15–45 it was above average. See the *Registrar-General's 4th Report*, (1842), p. 9; *R.-G.'s 8th Report* (1847), pp. 37, 187.

[9] The mean ages at marriage of spinsters calculated from the Nottinghamshire marriage bonds and allegations for the period 1701–70 were as follows (number in sample is given in brackets): Farmers and yeomen: 24 (285); Husbandmen: 24.5 (235); Labourers and servants: 25 (390); Artisans and tradesmen: 23.5 (290); Gentlemen: 24 (210).

[10] There were four baptism birth registers to one burial register kept by religious nonconformists before 1810. Few Methodists buried outside of the Anglican Church before 1810. See *Report On Non-Parochial Registers*, (Parl. Pap. 1837–38, XXVIII).

large cities[11] If we exclude urban industrial counties from the analysis, it is clear that there was a substantial fall in the death rate during the eighteenth century. According to Deane and Cole, the death-rate in eighteen southern counties fell from 30.6/1,000 in 1701–50 to 20.6/1,000 in 1801–30.[12] Although these figures should not be taken too literally, the long-run trend is probably fairly accurately described by them.

In addition to this evidence, several studies of the aristocracy and gentry indicate that there was a sharp drop in mortality during the middle of the eighteenth century.[13] Hollingsworth's study of the aristocracy yielded the following increase of expectation of life at birth for females during the eighteenth century:

Table 1. Expectation of Life (Years) at Birth for Aristocratic Women[14]

Period	*1700–24*	*1723–49*	*1750–74*	*1775–99*	*1800–24*
	36.3	36.7	45.7	49.0	51.71

Most of the increase in life-expectancy was due to the saving of life amongst younger age groups. These statistics are derived from sources sufficiently reliable for us to be sure that they describe a genuine decline in mortality. Although it is not justifiable to generalize about the total population from such a finding, we must attempt to explain it in terms which might be relevant for the whole population. Obviously an explanation in terms

[11] This was reflected in the death/burial ratios for different counties, e.g. the 1839–40 ratio for Lancashire was 1.61, as against the national average of 1.18. P. Deane and W. Cole, *British Economic Growth 1688–1959* (1962), pp. 108, 109. [1994]: However, although the non-registration of burials in non-conformist and private burial grounds was important in the nineteenth century, I now believe that burial registration was highly deficient in the eighteenth century and earlier, and that this was primarily due to clerical negligence. See Chapters 7 and 8 of the present book.

[12] *Ibid,* p. 127.

[13] See Chapter 1; T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies,* XI (1957); T. H. Hollingsworth "The demography of the British peerage", *Supp., Population Studies.* Vol. 18, No. 2 (1964). New evidence presented in Chapter 7 of this book indicates that this fall in mortality began at the beginning of the eighteenth century.

[14] Hollingsworth, *op. cit.* (1964), p. 57.

of the quantity of food supply is irrelevant to groups such as the gentry and aristocracy. Mortality diminished so rapidly during 1750–74 that one must seek an explanation more radical than those usually given.

The elimination of smallpox amongst the aristocracy could explain much of the rise in the expectation of life for that group[15] and indeed for the whole of the increase in population during the late eighteenth and early nineteenth centuries. For the population as a whole inoculation only became popular after about 1765, when the Suttons perfected their much safer technique. Jenner himself recognized this. He wrote "that the common people were rarely inoculated for the smallpox, till that practice was rendered general by the improved method of the Suttons ..."[16] Howlett in 1782 collected statistics from 225 parishes for the two approximate periods 1734–53 and 1754–73; the balance of baptisms over burials in the first period was negligible, and was only slightly greater in the second, suggesting that the great increase in population occurred after 1770,[17] which fits in very well with the chronology of the spread of inoculation.

Other medical and environmental 'improvements' were largely associated with towns, yet in 1801 only about a fifth of the total population lived in towns with a population greater than 10,000.[18] Even as late as the 1840s

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[15] That inoculation was responsible for the elimination of smallpox, rather than vaccination, is supported by the negligible rise in life-expectancy for the aristocracy between 1800 and 1824.

[16] *The Medical Repository*, Vol. 5 (1803), p. 239. Chambers draws attention to payment by a Nottinghamshire parish to one of the Suttons for inoculating some poor children in 1767, *op. cit.*, p. 32, n. 4. He also notes a relatively slight smallpox epidemic occurring in Nottingham in 1801, which is not incompatible with the slow spread of inoculation in towns. The same is to some extent true of Boston, Lincs. (mentioned by Chambers), where the decline of registered smallpox deaths was from 14.1 smallpox burials per 100 baptisms during 1749–75 to 5.25 per 100 during 1776–1802.

[17] The exact figures are: 1734–53 – baptisms 109,478, burials 104,750, marriages 34,110; 1754–73 – baptisms 123,715, burials 109,758, marriages 40,285. See J. Howlett, *Observations On The Increased Population, Healthiness ... Of Maidstone* (1782), p. 14. This pamphlet was published anonymously and a copy of it is to be found in Maidstone Museum. [1994]: New work on parish register reliability suggests that any conclusions based on parish registers should be treated with caution.

[18] See B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), pp. 8, 24–27.

mortality in the large towns was very high: for example, about 48.5 per cent of all males born in the Liverpool district died before the age of 5 during 1838–44.[19] Any improvements in the large towns would have been more than outweighed by the consequence of a smaller proportion of the total population living in the relatively healthy rural areas. Furthermore, the medical historians T. McKeown and R. G. Brown have pointed out that most of the medical 'improvements' during the eighteenth and nineteenth centuries, for example, fever hospitals and midwifery services, were probably ineffective.[20] Even if they were effective it is doubtful whether they affected more than a very small minority of the total population.[21]

In the country as a whole smallpox was the most significant epidemic disease so far as mortality was concerned in the eighteenth century. For example, Charles Deering the historian of Nottingham, wrote in 1751 that "there mostly happens once in five Years some Distemperature in the Air, which either brings along with it some Epidemical Fever, (tho' seldom very Mortal) or renders the Small-Pox more dangerous than at other Times; of this last, the Year 1736, was a fatal Instance ... the Burials exceeded that Year the Births by above 380 ..."[22] Deering's description of five-year cycles for smallpox echoes that known from bills of mortality and parish registers in other towns such as Northampton and Maidstone. His belief that the outbreak of smallpox in 1736 was the most severe epidemic since the plague, is borne out by the evidence. Smallpox was increasing in virulence throughout the seventeenth and eighteenth centuries, an increase which was particularly marked during the 1720s and afterwards. For example, the total number of smallpox deaths in Godalming, Surrey, was

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[19] *Registrar-General's 8th Report*, 1846, p. 206.

[20] T. McKeown and R. G. Brown, "Medical evidence related to English population changes in the eighteenth century", *Population Studies*, Vol. 9 (1955–6).

[21] With reference to improvements in midwifery, the figures produced by Dr Eversley for the Worcestershire area do not suggest any significant fall in infant mortality during the eighteenth and early nineteenth centuries; this finding is compatible with the high infant mortality rate (about 15 per cent) for England and Wales at the beginning of civil registration. See D. E. C. Eversley, "A survey of population in an area of Worcestershire from 1660–1850 on the basis of parish records", *Population Studies*, Vol. 10 (1956–7), pp. 269–71. [1994]: But this conclusion is now open to question, because of new evidence on burial register unreliability.

[22] C. Deering, *Nottinghamshire Vetus Et Nova* (1751), p. 82.

as follows: 1686, 50; 1701, 24; 1710–11, 39; 1722–3, 94.[23] The increasing virulence of smallpox probably explains the check to population which occurred in the 1720s.

Creighton, the medical historian, mentions influenza as an important disease during this period, but it rarely appears in bills of mortality and parish registers as accounting for large numbers of deaths. Also, Creighton could be misleading in his reading of evidence. For example, he reported a rumour that the high mortality in Exeter in 1729 was due to influenza, yet he overlooked the testimony of a local diarist, who recorded that not only was smallpox in the town that year, but was a particularly virulent variety.[24]

An improved standard of life may have diminished mortality amongst the general population, but such an explanation does not fit easily with the known chronology of population growth and per capita incomes. Any rise in real incomes of the labouring population probably took place during the first half of the eighteenth century, rather than the second[25], yet population increased much more rapidly at the end of the century. Also, growing real incomes cannot explain the sharp fall in mortality amongst the gentry and aristocracy. And finally, there was surprisingly little variation in adult male mortality between different occupational groups, due to income differentials, during the middle of the nineteenth century,[26] suggesting that income factors were not important in determining rates of mortality.

II

The most recent comprehensive work on the history of Irish population during the eighteenth and early nineteenth centuries is that by Professor K. H. Connell, who concluded that the great acceleration in population growth at the end of the eighteenth century was due "very likely to the increase of fertility that followed earlier marriage."[27] Connell argued that

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[23] *Surrey Archaeological Collections*, Vol. XXVII, pp. 16–20.

[24] "The Small Pox was very fatall to some. Mr. Vivian lost all his children, being four sons." See R. Pickard, *Population And Epidemics Of Exeter* (1947), pp 65, 66.

[25] For example, see Deane and Cole, *op. cit.*, pp. 19, 91.

[26] See the *Registrar General's 14th Report*, (1851), pp xviii, xxii.

[27] K. H. Connell, *The Population Of Ireland, 1750–1845* (1950), p. 248.

before the Great Famine of 1846, the age at marriage had been low, and that the famine itself had been responsible for dramatically increasing the average age at marriage. Connell's work was criticized by Dr Michael Drake, on the ground that the statistics for the 1830s do not, in fact, indicate a low age at marriage.[28] This can be seen in the Irish Census returns for 1841:

Table 2. Proportion Unmarried of 100 of the Population of the Respective Ages, Ireland, 1841[29]

		Under 17	*17–25*	*26–35*	*36–45*	*46–55*	*55+*
Males	Rural	100	93	44	16	16	8
	Civic	100	87	36	17	12	10
Females	Rural	100	81	28	15	12	12
	Civic	100	79	33	20	15	15

The distribution of the unmarried amongst various age groups was very similar to that in England at about the same time:[30] if allowance is made for the overstatement of early marriages in the statistics for the 1830s (as outlined by Drake), the mean age of marriage of spinsters and bachelors was nearly the same for both Ireland and England, i.e. about 24.5 for spinsters and 25.5 for bachelors.[31] Both the crude birth rate and age-specific fertility were similar for the two countries for the period around 1840.[32] It might be argued, of course, that the relatively late age of marriage in Ireland was not typical of the period before 1841. Drake has examined the statistics for the 1830s and has concluded that there was no trend towards a lower age at marriage in the 1830s.[33] Possibly at an even earlier period marriage

[28] M. Drake, "Marriage and population growth in Ireland, 1750–1845", *Economic History Review*, Vol. XVI (1963–4).

[29] *Population Census Ireland, 1841*, (Parl. Pap., 1843, XXIV), pp. 41, 42. Indeed Ireland appears to have had one of the highest mean ages at marriage and lowest marriage rates in Europe in the pre-Famine period.

[30] See Mitchell and Deane, *op. cit.*, pp. 15, 16.

[31] For English ages at marriage during 1839–41 see the *Registrar General's Fourth Annual Report* (1842) p. 10.

[32] The proportion of women between 15 and 44 as a percentage of the total female population and the crude birth-rate were about the same for both countries during this period. See Connell, *op. cit.*, pp. 30, 37.

[33] Drake, *loc. cit.*, p. 311.

took place at a lower age, but then the age at marriage would be rising throughout the early nineteenth century when population was increasing very rapidly.

The only evidence for early marriage is literary rather than statistical, but if the evidence for the 1830s is typical we are unable to rely upon the estimates of casual observers. Connell has written that "according to an official summary of the immense mass of evidence presented to the Poor Inquiry Commission of 1836, men in Galway usually married when they were between 14 and 21; in Leitrim between 16 and 22; in Mayo and Sligo usually under 20, and in King's County between 17 and 20",[34] yet according to the 1841 Irish Census there were only fifty-three married men and 480 married women under the age of 17 in the whole of Ireland.[35] It is probable that the Commission's informants had a vested interest in castigating the moral 'laxity' of agricultural labourers and small cultivators: they had to find an explanation for the poverty of the majority of the population, and what more convenient explanation than the Malthusian one?

Drake has argued that alternative explanations exist for the rapid expansion of the Irish population: (i) "that a highly nutritious and regular diet of potatoes so improved the health of Irish women that their fecundity increased markedly";[36] and (ii) "that the universal acceptance of the potato as the staple food would lead to a once-and-for-all drop in the general level of mortality".[37] There are two major difficulties with this interpretation. First, that population increased rapidly only after 1772, whereas potatoes had been used widely in Ireland since at least the beginning of the eighteenth century. Second, that diets in the earlier period were probably much more nutritious than the exclusive reliance on potatoes at a later date. Petty wrote in about 1671–2 that "The Diet of these people [the Irish] is Milk, sweet and sower, thick and thin, which is also their Drink in Summertime, in Winter, Small-Beer or Water. ... Their Food is Bread or Cakes, whereof a Penny serves a Week for each; Potatoes from August till May, Mussels, Cockles and Oysters, near the Sea; Eggs and Butter, made

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[34] K. H. Connell,"Peasant marriage in Ireland: its structure and development since the Famine", *Economic History Review*, Vol. XIV (1961–2), p. 520.

[35] *Population Census Ireland , 1841* (Parl. Pap., 1843, XXIV), p. 439.

[36] Drake, *op. cit.*, p. 311.

[37] *Ibid*, p. 312.

very rancid, by keeping in Bags. As for Flesh ... tis easier for them to have a Hen or Rabbit, than a piece of Beef of equal substance".[38] Several contemporaries thought that the Irish poor could no longer afford milk and other 'extras' during the late eighteenth and early nineteenth centuries.[39] It seems inconceivable that the slightly more luxurious earlier diet was less nutritious than potatoes by alone. And if potatoes were associated with higher fecundity, why were not Irish women – with their more exclusive reliance on potatoes – more fertile than English women?

If increasing fertility cannot explain rising population, what is the evidence that falling mortality was responsible for growth? Mortality in Ireland appears to have been lower during the 1830s than it was in England. According to the retrospective statistics collected for the Irish census of 1841, the crude death rate was 16.8 per 1,000 for the late 1830s,[40] whereas in England and Wales for the same period it was 22.2 per 1,000.[41] That this finding is not an artefact of the method of collecting statistics or due to differences in the age composition of the two populations is demonstrated by comparing age-specific death rates for the year 1840/41.[42] Below the age of about 35 the Irish mortality rates were all lower than the English, with the greatest disparity occurred amongst young children: Ireland suffered about 40 deaths per 1,000 children living under the age of five, whereas the equivalent English rate was about 67 per 1,000.[43] The explanation for this marked difference in child-mortality rates is probably that a much higher proportion of the Irish population lived in rural areas. Within Ireland, the urban civic districts had a child mortality rate (about

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[38] Other writers during the late seventeenth century emphasize potatoes and milk in the diets of the Irish poor. See G. O'Brien, *The Economic History Of Ireland In The 17th Century* (1919), pp. 137–42.

[39] G. O'Brien, *The Economic History of Ireland From The Union To The Famine* (1921), p. 21.

[40] *Ibid*, p. 189.

[41] *The Registrar-General's Fifth Annual Report* (1843), p. 379.

[42] As the number of deaths in 1840 was ascertained from a house to house survey made in the following year (1841), the figures likely to be reliable, especially for young children's deaths.

[43] For the Irish age-specific mortality rates see Connell, *Population Of Ireland* p. 193; for English mortality rates for roughly similar age groups, see Mitchell and Deane, *op. cit.*, pp. 38, 40; for the exact figures under the age of 5, *The Registrar-General's Fourth Annual Report* (1842), p. 128.

78 per 1,000) well over twice that in the rural districts (about 35 per 1,000). The conclusion to be drawn from these comparisons is that like the age at marriage, and the age-specific birth and marriage rates, the age-specific death rate in Ireland was similar in about 1841 to that in England and Wales when allowance is made for distribution effects of population in urban and rural areas. This essentially implies that demographic factors were independent of economic differences, a conclusion similar to that reached from a study of the age at marriage and age-specific birth and marriage rates within England during the eighteenth and nineteenth centuries.

If the increase in Irish population before the pre-1841 period is not to explained in terms of a high birth rate associated with a low age at marriage, but in terms of a falling death rate, what possible cause or series of causes could explain any reduction in mortality during the late eighteenth and early nineteenth centuries? We have already rejected the hypothesis that there was an improvement in the Irish diet during the eighteenth century. Professor Connell, after reviewing possible causes for a reduction in mortality, concluded that his "examination of the social habits and the housing of the Irish, the dissemination of hospitals and dispensaries, the spread of vaccination and the incidence of fever does not support the proposition that in Ireland, as is said to have been the case in England, greater cleanliness and medical advances led to a substantial lowering of mortality".[44] Professor Connell also reviewed the history of smallpox and inoculation, but unfortunately did not treat the subject at any length; here it is only possible to elucidate some hypotheses and briefly illustrate them with relevant statistics.

Smallpox appears to have been present in Ireland at least from the Middle Ages onwards and had become endemic before the eighteenth century.[45] The disease seems to have occurred almost every year in Dublin during the period 1661–1746, when bills of mortality were kept.[46] Accord-

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[44] Connell, *op. cit.*, p. 239.

[45] As Rogers wrote in 1743: "though of foreign Growth, and by Transplantation brought in amongst us, it is now become a Weed of our own Soil, and a Native of our Country." Joseph Rogers, *Essay On Epidemic Diseases* (1734), p. 82.

[46] For a description of the content of the bills and relevant statistics, see J. Fleetwood, *History Of Medicine In Ireland* (1951), p. 65, and Dr J. Rutty, *A Chronological History Of The Prevailing Diseases In Dublin* (Dublin, 1770).

ing to statistics derived from these bills, smallpox accounted for about 20 per cent of the total deaths during the two periods 1661–90 and 1715–46.[47] Smallpox killed about 33 per cent of all children born during 1715–46, according to the Dublin bills of mortality. No other reliable statistics of smallpox mortality are available for Ireland before the 1830s.

Several observers described smallpox epidemics the 1760s. Dr. James Sims recorded a smallpox epidemic in 1766–7: smallpox broke out with unheard

> havock, desolated the close of this year [1766], and the succeeding spring of 1767. They had appeared above a year before along the eastern coast of the kingdom, and proceeded slowly westward with so even a pace, that a curious person might with ease have computed the rate of their progress. ... As they had not visited the country for some years, numerous subjects were grown up for them to exercise their fury upon, and many blooming infants were just opening to the sun, in vain, since they were so soon to be cropt by this unfeeling spoiler. Of thousands who caught the infection in this [Tyrone] and the neighbouring counties, scarcely one-half escaped, and even of these, some with the loss of one or both eyes, and several with faces so altered, as to be known with difficulty by their most intimate acquaintances.[48]

A later epidemic in 1770 was less mortal but this was attributed to "the want of subjects for them to exercise their fury upon, the preceding disorder having left few who had not undergone the malady, than to any abatement in their malignancy".[49] These descriptions of smallpox epidemics in the countryside are very similar to those found in England before the advent of inoculation, and smallpox was always more virulent in isolated country areas owing to a lack of a pool of antibodies.

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[47] The actual figures are as follows: Dublin, 1661–90: smallpox deaths (annual average) – 472, total deaths (annual average) – 2,236; 1715–46 (excluding 1739): smallpox deaths – 13,759, total deaths – 74,585; total births – 42,566.

[48] J. Sims, *Observations On Epidemic Disorders* (1773), pp. 36–38.

[49] *Ibid*, pp. 134–5.

III

The Rev. H. Townsend wrote in 1810 that the increase of Irish population was partly due to "the universal custom of inoculating children for the smallpox, a disorder, which was once a little less injurious in its ravages than the plague".[50] In this essay I will briefly examine the evidence for this claim.

Inoculation was introduced into Ireland in 1725, and it seems to have spread very slowly amongst the general population. The watershed of the practice of inoculation in Ireland, like that in England, was probably the perfection of a safe technique by the Suttons during the 1760s. The Suttons appointed several partners in Ireland: "Messrs. Houlton, Blake and Sparrow in Dublin; John Hailey, MD. in Cork; John Morgan, MD. in Straborne, Tyrone; and Messrs. Vachell, Ward, Shields & Arnold soon [1768] to be appointed to particular districts in Ireland".[51] Inoculation does not appear to have been used much during the 1766 epidemic as described by Sims, although he refers to the existence of 'inoculators' at that time.[52] Houlton observed in 1768 that several itinerant inoculators were claiming that they practised the safe Suttonian technique,[53] and as this was probably the beginning of popular inoculation in Ireland.

In 1769 "a special infirmary was set apart in the Foundling Hospital of Dublin, for Experimenting with inoculation upon the inmates".[54] In April 1777 "agreeable to the humane resolutions of the King's County Infirmary, 461 persons were, in the course of last month, inoculated".[55]

By the beginning of the nineteenth century inoculation was practised almost universally. The Dublin College of Physicians, when asked their opinion in 1807 of vaccination, replied that "Variolous Inoculation had

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[50] Rev. H. Townsend, *Statistical Survey Of The County Of Cork* (1810), p. 90.

[51] R. Houlton, *Indisputable Facts Relative To The Suttonian Art Of Inoculation* (1768), p. 10.

[52] Sims, *op. cit.*, p. 42.

[53] Houlton, *op. cit.*, p. 25: "Some, I am informed since my arrival in Ireland, are now travelling over several parts of the kingdom ..."

[54] *Population Census 1851* (Parl. Pap., 1856, XXIX), p. 146.

[55] *Ibid*, p. 422.

been long, almost exclusively in the hands of a particular branch of the profession ('irregular practitioners') ... being the usual medical attendants in families, and especially employed in the diseases of children. ... Smallpox is rendered a much less formidable disease in Ireland by the frequency of inoculation for it ... hence parents, not unnaturally, objected to the introduction of a new disease [vaccination] rather than not recur to that with the mildness and safety of which they are well acquainted".[56] The difficulty of tracing the history of inoculation in Ireland is that most of it was carried out by "individuals [who] proceed from village to village several times during the year for the purpose of inoculating the infantile population",[57] a practice made necessary through the lack of doctors in Ireland. The activities of the itinerant inoculators were noted in Derry in 1812[58] and in Co. Waterford, Cork, Kerry, and Clare at later dates.[59] Sir William Wilde noticed the activities of the inoculators as late as 1851.[60]

Professor Connell seems to have accepted that inoculation was practised very extensively, but accepted the conventional medical view about the dangers of inoculation. According to Sir William Wilde, vaccination was practised in Irish towns much more than in country areas, as a result of a preference for inoculation amongst country people.[61] Smallpox mortality was much lower in the country areas than in the towns:

Table 3. Irish Smallpox Mortality in Town and Country[62]

Period	*Population 1841*	*Smallpox Deaths 1831–40*	*Annual Average Smallpox Deaths Per Million Living*
Civic Districts	1,135,465	12,418	1,093
Rural Districts	7,039,659	45,459	647

This difference cannot be explained by the different age structures of the

[56] *Report Of The Royal College Of Physicians Of London On Vaccination* (1807).
[57] *Population Census Ireland 1841,* (Parl. Pap. 1843, XXIV), p. xii.
[58] W. S. Mason, *Statistical Account, A Parochial Survey Ireland,* I (1814), p. 313.
[59] *First Report Of The General Board Of Health In The City Of Dublin,* pp. 94–97.
[60] *Population Census Ireland, 1851* (Parl. Pap. 1856, XXIX), p. 422.
[61] *The Epidemiological Society Report* (1852–53), p. 29.
[62] *Royal Commission On Vaccination, 1st Report* (1889).

town and countryside population (they were approximately similar), or by the greater extent of smallpox in the towns: everywhere in Ireland during the 1830s smallpox was a young child's disease: 49,000 of the 58,000 total smallpox deaths during 1831–40 were of children under 5 years of age. Most children caught it (unless they were inoculated or vaccinated) by their fifth birthday. In such a situation inoculation could not spread smallpox, as it was already a universal disease. Smallpox mortality was higher in urban areas because inoculation and vaccination were less practised there. Rural areas had lower smallpox mortality rates because of the protection given by inoculation. The total smallpox mortality rate of Ireland was about 710 annual deaths per million living. Although this figure may appear at first sight to be high, it is, in fact, remarkably low if compared with earlier mortality rates. In Dublin during 1661–90, for instance, the smallpox mortality rate had been about 8,600 per million.[63] Expressed as a proportion of total deaths, smallpox had accounted for about 20 per cent of deaths in the 1661–1745 period in Dublin, whereas in that city during 1831–40 it accounted for under 3 per cent of them.[64]

The rate of 710 per million is also low by what might be expected if neither inoculation nor vaccination had been utilized on a wide scale. The case fatality rate of natural smallpox amongst infants was about 40 deaths per 100 cases during the 1830s.[65] Had all children under the age of 5 caught smallpox without inoculation or vaccination, the smallpox mortality rate would have been 400,000 deaths per 1,000,000 living rather than the 39,300 per 1,000,000 which was the actual rate for children under 5,[66] i.e. it would have been about ten times the actual rate. The point of these

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[63] This is using Petty's population figure of 55,000 for Dublin; undoubtedly this is an underestimate, but so many deaths were not registered that the two underestimations appear to cancel each other out. The overall crude death-rate using Petty's population figure is about 40 per 1,000, a not unreasonable figure for a city the size of Dublin during this period.

[64] *Report ... By The ... Vaccination Committee 1853*, (Parl. Pap. 1852–53, CI), p. 80. The smallpox mortality statistics in this essay should be taken only as approximations, as smallpox deaths were probably under-registered.

[65] See the *Royal Commission On Vaccination, 1st Report* (1889), pp. 74, 215; *Ibid, 6th Report*, pp. 717–20; E. G. Edwards, *A Concise History Of Smallpox And Vaccination* (1902), p. 55.

[66] Connell, *The Population Of Ireland*, p. 219

hypothetical comparisons is to indicate the scale of saving of life by inoculation and vaccination.

Because of a lack of reliable statistical data for Ireland in the eighteenth century, it is not possible to trace the precise effects of inoculation. However, informed observers attest to a significant decline in smallpox mortality in the last quarter of the eighteenth century. Sir William Wilde's survey of smallpox epidemics in Ireland shows that the half-century between 1776 and 1827 had no major outbreaks of the disease.[67] The apparent success of inoculation and vaccination could account for a substantial part of the increase in population after about 1770.[68]

IV

I have suggested that much of the population growth in both England and Ireland during the latter half of the eighteenth and first half of the nineteenth centuries may be explained by the gradual elimination of smallpox, and therefore may be considered independent of contemporary economic changes. But since it appears that the demographic experience of the two countries was similar, why was it that economic effects were so different? Any answer to this question would be complex, involving consideration of a wide range of economic, social, political and other factors. Here, I will suggest some points of possible relevance.

The cloth industry was England's chief commercial manufacture during the eighteenth century, but it accounted only for about 5 per cent of the total national income,[69] and its domestic market appears to have changed

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[67] *Population Census Ireland, 1851* (Parl. Pap. 1856, XXIX), p. 422.

[68] Certainly if the 1767 epidemic was typical of pre-inoculation experience, the disappearance of smallpox in Ireland would explain a significant increase in population. Generally, smallpox mortality appears to have been heavier in Ireland than in England; nevertheless population expansion in Ireland before 1770 was probably due to earlier long-term changes such as the disappearance of the plague. In this sense, the gradual elimination of smallpox would only explain the great acceleration of Irish population after 1770.

[69] Deane "The output of the British woollen industry in the eighteenth century", *Journal Of Economic History,* Vol XVII (1957), p. 221.

little between 1695 and 1772.[70] As most of the expansion in the cloth industry before 1772 can be explained as a consequence of increasing exports, we must ask how much other economic growth during this period was due to domestic expansion. Deane and Cole have argued that a general economic expansion took place from the 1740s onwards. This conclusion is based, however, on an index of real output which is virtually an index of estimated population growth, as agriculture (43%) and rent and services (20%) are both based on questionable estimates of the size of population.

An analysis of the production series which are available throws considerable doubt on the 1740s as a turning-point. As one writer has pointed out: "Of the dozen or so commodities for which output figures are available there are several in which the levels reached in 1741–45 and 1746–50 were lower than those achieved earlier in the century. This is true of strong beer, starch, hides and skins, coal imports, raw silk and thrown silk. Indeed, for some of these commodities the 1740s is a low point. In other commodities, such as printed goods and soap, the acceleration of output was clearly later in the century".[71] This criticism appears valid, since, if one takes Deane and Cole's own home industries index (beer, leather, candles, and soap), the uninterrupted and main increase in production certainly occurs after 1770.[72]

One hypothesis which would explain differences in the chronology of increased consumption of different commodities is that the consumption of quality goods increased much sooner and in greater quantities than that of cheaper goods. The output of tallow candles, used by poorer people, doubled between 1715 and the end of the century, whereas that of wax candles, used by the wealthier classes, increased nearly tenfold.[73] The

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[70] According to Deane's estimate, domestic consumption of manufactured cloth was about £3 million in 1695. If one accepts the proportion of Yorkshire woollens and worsteds exported in 1772 as being typical of the country as a whole (at this time Yorkshire output accounted for about 60 per cent of the total) domestic consumption of manufactured woollen cloth was also about £3 million in 1772. See Deane, *op. cit.*, pp. 220, 221.

[71] D. Whitehead, "History to scale? The British economy in the eighteenth century", *Business Archives And History, Vol. 4*, No. 1 (1964), p. 83.

[72] The index numbers were as follows (beginning at 1700 and continuing at every tenth year until 1800): 100, 98, 108, 105, 105, 107, 114, 114, 123, 137, 152. Deane and Cole, *op. cit.*, p. 78.

[73] T. S. Ashton, *An Economic History Of England: The Eighteenth Century* (1955) p. 60.

production of high-quality white glass nearly quadrupled between 1747 and 1801, whereas that of common bottles only began to increase during the 1790s.[74] The best comparison between the output of quality and cheap goods is between silk and beer. The consumption of silk increased rapidly after 1755, whereas that of beer only really began to increase after 1775.

Table 4. Output of Quality and Cheap Goods, 1695–1804[75]

	Imports of Silk (1695–1704 = 100)	*Strong Beer Production (1695–1704 = 100)*
1695–1704	100	100
1705–1714	92	99
1715–1724	110	112
1725–1734	130	104
1735–1744	107	102
1745–1754	116	108
1755–1764	153	113
1765–1774	182	112
1775–1784	203	123
1785–1794	225	136
1795–1804	217	163

It may be suggested that the earlier expansion of the market for quality products was a result of the rapidly increasing population of the aristocracy, gentry, and other wealthy groups. During the eighteenth century about a quarter of the national income went to 3.5 per cent of all families, that is to say, the aristocracy, gentry, and merchant class.[76] Due to decreased mortality their numbers probably quadrupled between 1750 and 1850,[77] and they were the social classes most able to translate their increased numbers into effective demand. This could have occurred in several ways: by a switch from savings to consumption; by increased borrowing, including mortgaging of land; improvements of their assets, through the enclosure of land and a more intensive use of their capital in

[74] Mitchell and Deane, *op. cit.*, p. 267.

[75] Deane & Cole, *op. cit.*, p. 51. The index figures are only approximations.

[76] Mathias, "The social structure in the eighteenth century: a calculation by Joseph Massie", *Economic History Review*, Vol. X (1957–8), pp. 42–45.

[77] See T. H. Hollingsworth, "A demographic study of the British ducal families", *Population Studies*, Vol. XI (1957).

business.[78] The scale of possible profit from enclosures was estimated by Gregory King in 1685: only about half of the total land surface of England was farmed, of which three-fifths was cultivated under the common-field system;[79] and by a general exploitation of patronage through increased participation in Parliament, particularly with reference to finding places in the very rapidly expanding army.[80] The main problem would have been to find positions and capital for their now surviving younger sons and provide their daughters with portions; possibly this was one of the reasons for the frequent failure of many of the poorer gentry and yeomanry during this period.

Beginning probably during the 1770s, there was a considerable expansion of the home market for cheap woollens and cottons, due almost certainly to an increase in population rather than a growth in per capita incomes. It would appear that the domestic consumption of woollen cloth increased rapidly from about 1772: after this date the total output of woollen cloth rose, while the proportion exported fell from about 70 per cent in 1772 to 35 per cent in 1805, and 20 per cent after the 1820s.[81]

It is not necessary to describe the effects of the great upsurge in population after 1770 which affected every branch of economic and social life – the growth of canals, the improvement of roads, enclosure of land, development of the factory system – in short, the Industrial Revolution. Although increasing exports and the raised demand of the wealthy led to a expanded production, they were not the foundation of the fundamental change in the economy.[82]

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[78] Both the number of patents taken out and the number of bankruptcies increased sharply from the 1760s onwards: Mitchell and Deane, *op. cit.*, p. 268; Ashton, *op. cit.*, p. 254.

[79] See J. L. and S. Hammond, *The Village Labourer* (1919), p. 26 n. 1.

[80] The proportion of the old aristocracy in the House of Commons rose significantly during the eighteenth century, and younger sons of the aristocracy increased their numbers in the church, navy, and 'civil service', as well as in the army. The colonial army and mercantile 'administration' provided outlets particularly for younger sons of the gentry.

[81] Although this reduction in woollen exports was partly due to the substitution of cottons for woollens in the export market, only about 30 per cent of all cottons were exported during the second half of the eighteenth century. See Deane and Cole, *op. cit.*, pp. 185, 196.

[82] The growth in the export market depended partly upon emigration, and thus on population increase at home; inoculation was also widely used in America and the West Indies, and so was contributory to population growth in these markets.

They helped to maintain the real incomes of the mass of the population, and therefore helped to translate increased needs (from an enlarged population) into effective demand, which raised prices and stimulated economic growth.[83] Only a radical expansion of mass markets could provide the sufficient condition necessary for the fundamental transformation of the economy, that is to say, the growth of the new factory capitalism. It is no accident that this capitalism did emerge ultimately in Lancashire, after its earlier forms had developed elsewhere. Lancashire had been the centre of production of the very cheapest cloth in the early eighteenth century, and untrammelled by traditional constraints it was the natural place for the emergence of the factory system producing for a mass market.

V

In Ireland the result of the population explosion was the growth of a deepening subsistence economy rather than an industrial revolution. Although the Irish census of 1841 recorded that about 30 per cent of the total occupied population as employed in industry, two-thirds of these were women, most of whom worked at home in domestic industry, providing goods for local consumption.[84] The only province with a sizeable male population employed in industry was Ulster, the centre of the linen manufacture.[85] This industry had been encouraged since the beginning of the eighteenth century as a compensation for the destruction of the Irish woollen industry in 1699. Although the manufacture of woollen cloth was very small in Ireland at the end of the seventeenth century, it was growing very rapidly during the last decade. It was suppressed at the instigation of English clothiers, who were afraid it might eventually provide overwhelming competition.[86] The export of linen cloth and yarn trebled between

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[83] According to figures computed by Arthur Young, the price of wheat began to rise in about 1764; the price of wheat (statute measure) at the Windsor Market was as follows: 1714–38 – £1 15s. 5d. per qtr.; 1739–63 – £1 14s. 2d.; 1764–88 – £2 6s. 6d. See A. Young, *Annals Of Agriculture*, Vol. XIV (1790), pp. 228–30.

[84] T. W. Freeman, *Pre-Famine Ireland* (1957), pp. 76–77.

[85] *Ibid.*

[86] See G. O'Brien, *The Economic History of Ireland In The Seventeenth Century* (1919), pp. 227–9.

1718–47 and 1748–77, about 90 per cent finding its way into the English market.[87] In 1771 it was estimated that the manufacture of linen was worth £2,200,106, 70 per cent of the export output.[88] Linen was estimated to be worth about half the total value of all exports during 1771–7,[89] but its export importance declined during the late eighteenth and early nineteenth centuries, while home consumption appears to have expanded sharply during the same period.[90] Cotton, however, began to displace linen, for, as one observer noted in 1840:

> men cannot live for what they get for [linen] weaving now. There is a great difference in respect of the appearance of weavers who come to market now and formerly; they are not so well dressed, nor near so comfortable looking: the fine sturdy young men, who once came to the market, have now gone out of the trade, and many have emigrated to America. I remember when it was the best trade in Ireland; now it has gone to nothing. The cotton trade has ruined the linen; formerly everybody wore linen, and now everybody wears cotton.[91]

The change was probably due to the abandonment of protection of Irish industry in 1825, as even the domestic cotton industry began to wilt under the competition from England.[92] The first cotton mill driven by water power in Ireland was established near Belfast in 1784.[93] By the 1830s and 1840s "the deserted factory with its silent water wheel was already a familiar aspect of the Irish scene".[94] One of the main reasons for the eclipse of Irish industry was the lack of indigenous coal, although presumably the cheapness of labour might have more than offset the cost of importing coal from England.

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[87] A. W. Hutton, *Young's Tour Of Ireland*, Vol. 2 (1892), pp. 200, 202.

[88] *Ibid*, p. 201.

[89] *Ibid*, p. 255.

[90] The following are contemporary estimates: linen manufacture 1771: export – £1,541,200; home consumption – £658,906; value of linen manufacture 1817 – £3,151,752; exports of linen 1822 – £861,944. See Hutton, *op. cit.*, p. 201, and O'Brien, *op. cit.* (1921), p. 302.

[91] Freeman, *op. cit.*, p. 85.

[92] See O'Brien, *Economic History Of Ireland* ... (1921), p. 311.

[93] Freeman, *op. cit.*, p. 85.

[94] *Ibid*, p. 6.

The failure of industry in Ireland was probably rooted in the nature of the country's social structure. In 1779 Arthur Young had noted:

> [the] only considerable manufacture in Ireland, which carries in all its parts the appearance of industry, is the linen; and it ought never to be forgotten that this is solely confined to the Protestant parts of the kingdom; yet we may see from the example of France and other countries that there is nothing in the Roman Catholic religion itself that is incompatible with manufacturing industry. The poor Catholics in the south of Ireland spin wool very generally, but the purchasers of their labour, and the whole worsted trade, is in the hands of the Quakers of Clonmell, Carrick, Bandon, etc. The fact is, the professors of that religion are under such discouragements that they cannot engage in any trade which requires both industry and capital. If they succeed and make a fortune, what are they to do with it? They can neither buy land, nor take a mortgage, nor even fine down the rent of a lease. Where is there a people in the world to be found industrious under such a circumstance?[95]

Young was undoubtedly correct in emphasizing the lack of financial incentives for Catholics to engage in industry, and another factor probably as important was their lack of capital. Very little land was owned by Catholics, and as early as the late seventeenth century most of the Irish population were peasants relying on subsistence farming. According to one observer writing in 1691, "their food is mostly milk and potatoes, their cloathing coarse bandrel cloth and linen, both of their own make; a pot of gruel; a griddle whereon to bake their bread, a little salt, snuff, and iron for their ploughs being almost all they troubled their shopkeeper or merchant for. A little hut or cabin to live in is all that the poverty of this sort hope or have ambition for".[96] Petty had estimated that out of a total of 200,000 houses, 160,000 were without any chimney, suggesting that they "live in a brutish nasty condition as in cabins with neither chimney, door, stair nor window".[97] With this degree of poverty it must have been impossible for Catholic peasants to acquire capital sufficient to establish manufacturing industry, quite apart from the lack of a home market

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[95] Hutton, *op. cit.*, p. 65.
[96] O'Brien, *op. cit.* (1919), p. 141.
[97] *Ibid*, pp. 137–8.

suitable for the absorption of such manufactures. Any capital available was owned by the Protestant landlords, many of whom were absentees; and as the population grew it became increasingly lucrative for them to invest their money in land, from which it was possible to obtain very high rents.[98] The derivation of these rents was described by Arthur Young:

> The poverty, common among the small occupying tenantry, may be pretty well ascertained from their general conduct in hiring a farm ... they provide labour, which in England is so considerable an article by assigning portions of land to cottars for their potato gardens, and keeping one or two cows for each of them, and by means of living themselves in the very poorest manner, and converting every pig, fowl, and even eggs into cash, they will make up their rent ...[99]

In 1841 Ireland had a subsistence economy based on small peasant cultivation, widely scattered throughout the whole country: only about 20 per cent of the population lived in villages and towns, the rest in isolated cabins.[100] Pressure of population drove cultivation of potatoes "towards the summits of the hills"[101] and meant that "every possible spot of land is laboured".[102] Subdivision of land and an almost exclusive potato diet enabled population to grow, inasmuch as the survivors of diminished mortality did not starve – until the subsistence economy collapsed in 1846 and there occurred the great famine. The causes and consequences can best be seen in the following table:

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[98] O'Brien, *op. cit.* (1921), pp. 12, 89, 97, 98.
[99] Young, *op. cit.*, pp. 31, 32.
[100] Freeman, *op. cit.*, p. 27.
[101] Connell. *op. cit.*, p. 96.
[102] *Ibid,* p. 118.

Table 5: Size of Land Holdings in Ireland, 1841 and 1851[103]

Size of holdings	*Number of holdings*	
	1841	*1851*
Not exceeding 1 acre	134,314	37,728
Exceeding 1 but not exceeding 5 acres	310,436	88,083
Exceeding 5 but not exceeding 15 acres	252,799	191,854
Exceeding 15 but not exceeding 30 acres	79,342	141,311
Exceeding 30 acres	48,625	149,090

The very poor peasants and casual labourers were virtually eliminated within a decade: these were the inhabitants of the 'growth class' housing – one-room cabins – which declined in number by 355,689 between 1841 and 1851, a decline of about 70 per cent.[104] The majority of the people leaving these cabins probably emigrated, although their reliance on potato subsistence meant that many of them starved to death or died from fevers associated with the famine.

VI

Unable to industrialize, and with a rapidly expanding population increasingly dependent on the potato, famine in Ireland was inevitable. In England, on the other hand, all the conditions for industrial growth had been present before the population explosion: a relatively high standard of living and a social structure encouraging enterprise and providing a potential mass market; a thriving textiles industry; the existence of provincial capital markets and a great and growing commercial centre in London; relative political stability; a progressive agriculture; sufficient technical innovation; abundant market outlets and sources of supply in overseas markets – including colonial Ireland – to mention only the best known of the much-discussed influences on growth. Although in both countries population increased rapidly during the second half of the eighteenth century through the use of inoculation against smallpox, England was fortunate in being able to industralize and thus avoid the mass starvation that was the disastrous fate of Ireland.

[103] O'Brien, *op. cit.* (1921), p. 59.
[104] *Ibid*, p. 59.

Chapter 4

The Evaluation of Baptism as a form of Birth Registration through Cross-matching Census and Parish Register Data: a Study in Methodology[1]

This essay was written to address the problem of parish register accuracy. The paper discusses research involving the comparison of census and parish register data, and covers a number of topics relevant to the debate about eighteenth-century population growth. The arguments are technical and detailed and perhaps it is useful to summarise the main conclusions of the work. First, about a third of all births in the 45-parish sample were not registered through baptism, and this did vary greatly over time. Second, the quality of baptism registration varied greatly from parish to parish. Third, the 1851 Census appears to have been very accurate, both with respect to statements about birthplace and age.

The accuracy of age statements is an important issue as it is fundamental to estimates of population and demographic changes calculated in the Cambridge Group's back projection programme.[2] Recent research by Audrey Perkins involving the checking of the 1851–1881 censuses with the parish registers of six Kent parishes, confirms the findings of the present study and the next essay of this book: census birthplace and age statements were of a high order of reliability.[3]

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[1] Originally published in *Population Studies*, Vol. 26, No. 1 (1972). The research on which this paper was based, was conducted with the aid of a grant from the Social Science Research Council.

[2] See Chapter 7.

[3] Audrey Perkins, "Age checkability and accuracy in the censuses of six Kentish parishes 1851–81", *Local Population Studies*, No. 50, Spring 1993.

In England all new work on the historical demography of the pre-civil registration period has used information provided by parish registers. Most of the available registers end in 1837 with the beginning of civil registration. For a period of 300 years prior to that date, parish registers provide the only systematically collected data available for demographic analysis. The reliability of register information is crucial to English historical demography. Yet the only comprehensive scholarly study of parish registration accuracy – the work of J. T. Krause – has cast serious doubts upon its reliability.[4] Krause concluded that there were very significant changes in the accuracy of registration over time:

> ... it seems that parochial registration was relatively accurate in the early eighteenth century, became somewhat less so in the 1780s, virtually collapsed between roughly 1795 and 1820, and then improved somewhat between 1821 and 1837.[5]

Krause reached this conclusion from a general survey of all the factors which might have affected registration accuracy, not from evidence based on direct statistical measurement. The only such evidence is for London and Southampton at the end of the seventeenth century, compiled by David Glass by comparing parish register returns of baptisms and burials against information on births and deaths derived from tax returns.[6] Glass's work is, however, only the beginning of an attempt to measure directly the completeness of registration, and the major aim of this paper is to outline a new method of measurement developed through substantive application to a limited number of parishes.

One of the fortunate accidents of English demographic history is that the core unit of registration of baptisms, burials and marriages on the one hand, and nineteenth-century census returns on the other, has been the

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[4] J. T. Krause, "The changing adequacy of English registration, 1690–1837", in D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965).

[5] *Ibid*, p. 393.

[6] D. V. Glass, "Notes on the demography of London at the end of the seventeenth century", *Daedalus* (1968). Information on Southampton privately communicated by Professor Glass.

parish, the boundaries of which have remained unchanged for centuries. It is thus possible to compare census information with that in the parish register. The logic behind this method may be summarized in highly simplified form, as follows: If the 1851 Census (the earliest with the relevant information) was perfectly accurate with respect to information on name, age and birthplace, it would be possible to check the completeness of Anglican baptism for people surviving to the census by comparing the census entries with those in the baptism register.[7] For example, the 1851 Census for Horringer, Suffolk, records that Peter Day was aged 44 years and was born in that parish; if baptism were a perfect form of birth registration, Peter Day would appear in the register in about 1807. In practice, of course, it cannot be assumed that the 1851 Census is perfectly accurate, but is it generally agreed by historical demographers that it was very much more reliable than the parish registers.[8]

It is possible to test the accuracy of the 1851 Census by comparing it in detail with that for 1861, on the assumption that a comparison of independent censuses will yield relevant information on errors (this will be discussed below). Anderson has undertaken such a comparison for a part of Preston, and found that for 475 people traced in both the 1851 and the 1861 censuses there was agreement on stated age to within two years in about 96% of cases and agreement on birthplace in 87.7%.[9] Allowing for the compounding of errors between the two censuses, these figures suggest that the 1851 Census was of a high degree of accuracy. The 1851 Census need not be perfectly accurate for the census/parish register comparison method to be of value; it is only necessary that the relative levels of census accuracy did not vary significantly between different age groups and parishes, which are key variables for checking variations in register reliability over time and between different parishes.

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[7] Assuming that baptism was performed in the parish of birth. This is one of the assumptions examined in the next chapter.

[8] See A. J. Taylor, "The taking of the census, 1801–1951", *British Medical Journal* (1951), pp. 715–720; P. M. Tillott, "Inaccuracies of census statistics resulting from the method of collection in 1851 and 1861", in E. A. Wrigley (ed.), *Nineteenth-Century Society: Essays In The Use Of Quantitative Methods For The Study Of Social Data* (1972).

[9] M. Anderson, "The Structure of the Family", in E. A. Wrigley (ed.), *op. cit.*

There is one other major problem. It cannot be assumed that the individuals enumerated in 1851 were typical of all those born before this date, as the characteristics of those dying before the census may have been different from those of the survivors. There is no direct way of dealing with this problem. It is only possible to mention the factors which might make the census population unrepresentative and to attempt to measure their effect on the reliability of baptism registration. Two factors might influence the representativeness of the census population: (i) variations in the delay between birth and baptism and effects of changes in infant mortality on the proportion of babies baptized; (ii) a correlation between social class and pre-census mortality and registration reliability. The first factor arises as a result of children dying before baptism: changes in birth/baptism delays and infant mortality rates would affect the proportion of births unregistered through baptism. The second factor is more-or-less self-explanatory – variations in class mortality could affect the representativeness of individuals who survived to the census.

The delay between birth and baptism can be measured where information on both dates is given in the register. Variations in infant mortality cannot be precisely measured because of the absence of exact information on the deaths of such infants, although it might be possible to use all the information in those unusual parish registers which list deaths as well as burials. The effects of social class on census representativeness may be directly assessed by measuring the relation between occupation/social status and registration accuracy; if no significant correlation exists, the distorting influence of social class can be ruled out.[10]

As an initial way of evaluating the accuracy of the 1851 Census, the following parishes were selected: Maidstone, Kent, Bethnal Green, Middlesex, Bramfield, Suffolk, Wylye, Wiltshire and Bretforton, Worcestershire. Individuals were then traced in both the 1851 and 1861 censuses wherever possible. The criteria for establishing identity between censuses were as follows:

(1) The same name. Name is such an important criterion for establishing the correct identity of an individual that it is basically impossible to

[10] Even if there were a significant correlation, it would only be important if there were variations in the relationship between social class and pre-census mortality, for we are primarily interested in relative registration accuracy, over time and between different parishes.

check variations in name spelling from one census to another or from census to parish register. There were occasions, however, when it was obvious that there was a genuine variation in spelling with phonetic equivalence; such variations were discovered through searching the baptismal registers, although the indexes to these registers usually provide relevant information on surname spelling variations. However, if there was any doubt about the name being the same, it was always assumed that the names referred to different individuals.[11]

(2) Residence in the same household of at least one other person of the same name in 1851 and 1861; this might be a relative, apprentice, friend, or servant. In many cases the individual was found to be living in the same house or street, particularly in Maidstone and Bethnal Green where it was difficult to locate individuals in both censuses except through a common address.

1,282 cases were located in both the 1851 and 1861 censuses for the five parishes. In 127 – 9.9% – there was disagreement about stated birthplace, and in 108 – 8.4% – a difference of three years or more in stated age. These figures conceal some important differences between the individual parishes, which may be initially summarized by grouping the three village parishes of Bramfield, Wylye and Bretforton together and comparing them with the urban areas, Maidstone and Bethnal Green, individually. The results are given in Table 1.

[11] That this is not a major problem has been confirmed by work done by Richard Wall of the Cambridge Group. He has kindly checked a proportion of Colyton census cases which could not be found in the baptism register (N.I.R. cases) against his own records – and there are no disagreements on the identification of individuals by name.

Table 1. 1851 and 1861 Census Comparison

	Maidstone	*Bethnal Green*	*Bramfield, Wylye and Bretforton*
Birthplace Statement Disagreements (B.S.D.s)	15 (5.9%)	75 (17.0%)	37 (6.3%)
Age statements differing by three years or more	15 (5.9%)	41 (9.3%)	52 (8.9%)
Total Cases	255	440	587

The figure which stands out in Table 1 is the relatively high proportion of birthplace statement disagreements in Bethnal Green, the other variations being relatively insignificant. Inaccuracies in birthplace statements are much more important for the comparison method than are discrepancies in age. The latter can be allowed for in the register itself to a large extent, by searching through a time period around the expected date of baptism, whereas the former cannot be checked through the register. The relatively large proportion of birthplace statement errors can probably be explained by the fact that Bethnal Green was a large urban parish with indistinct boundaries; many discrepancies in birthplace statement attributed the birth to contiguous parishes. Bethnal Green may also have had a larger proportion of non-natives who were likely to have been less precise about their parish of birth than natives. In order to test this hypothesis, the consistency of birthplace statements was measured for those stated in the 1851 Census to be born in the parish, as against non-natives.

Table 2. 1851 and 1861 Census Comparison of Birthplace Statements
(B.S.D. = Birthplace Statement Disagreement)

	Natives in 1851 Census			
	Maidstone	*Bethnal Green*	*Bramfield, Wylye and Bretforton*	*Total*
B.S.D.s	6	31	4	41
Total Cases	150	273	349	772
% B.S.D.s	4.0	11.4	1.1	5.3

Non-Natives in 1851 Census				
	Maidstone	*Bethnal Green*	*Bramfield, Wylye and Bretforton*	*Total*
B.S.D.s	9	44	33	86
Total Cases	105	168	238	511
% B.S.D.s	8.6	26.2	13.9	16.8

Table 2 suggests that it was not only in Bethnal Green that migration was important in causing birthplace statement errors. The most striking influence of migration and nativity was found in the three rural parishes, where there were nearly thirteen times as many birthplace statement disagreements amongst non-natives in the 1851 Census as amongst natives. The overall difference accords with what would be expected on commonsense grounds and the remarkably low number of birthplace statement disagreements for 1851 natives in the rural and small town parishes suggests that birthplace information for them was highly accurate. Those who had lived in such parishes all their lives are likely to have been quite certain about their birthplace; the probable source of error is the census itself, for example the faulty copying of information by the enumerator.[12]

The main conclusion to be drawn from this evidence is that the key information on birthplace is significantly more reliable for those stated as being native to a particular parish in 1851 than for those stated to be non-native. Since the census comparison method duplicates errors – an error in either of the 1851 and 1861 census will give rise to a birthplace statement disagreement – it could be argued that even the census data for 1851 non-natives were of high quality. It is, however, much easier to use the data for natives in the census/parish register comparison method, because of the concentration of cases in the one (native) register – non-natives would have to be traced in a large number of other registers. It is for this reason, and because of the more accurate census information for natives, that the census/register comparison has been restricted to natives only, with one exception which will be discussed below.

Individuals were included in the sample provided; (a) they were stated as native to the parish in the 1851 Census; and (b) aged 17 or over in the 1851 Census – persons under 17 would be too young to be found in the

[12] This has been discussed at some length by Tillott, whose figures suggest that this was very rare indeed in the rural and small town parishes. See Tillot, *op. cit.*

registers which ended in 1837.[13] Married women were excluded because of the extra work required in tracing their maiden names.

Some difficulty was anticipated at the outset in relation to the allowance to be made for age errors in the 1851 Census when deciding whether an individual was to be identified with an entry in the baptism register or not. The comparison of censuses suggested that ages in the census were likely, in most cases, to be accurate to within two years. The appropriate allowance for age error was decided on the basis of additional information collected to check the validity of names, birthplaces and ages used to identify an individual in a register. This additional information consisted of the names of the parents of those individuals enumerated with their parents in 1851, checked against the names of the parents listed in the parish register at baptism.[14] The relation between correct identity and discrepancies in age between the census and the baptismal registers is set out in Table 3.

Table 3. Correctness of Identity Established Through Parental Names

		Identity Correct	*Identity Incorrect*	*% Incorrect*
Discrepancies in Age Between Census Statement and Baptismal Date in Register. (45 parishes)	2 years	2,391	67	2.7
	3 years	54	6	10.0
	4 years	18	6	25.0
	5 years	7	4	36.4
	6–15 years	7	11	61.1
	Total	2,477	94	3.8

A number of conclusions follow from this table. The vast majority of cases located in the baptismal register were correct identifications, as independently measured by information on parents' names: only 3.8% of the

[13] The age of 17 was originally selected in order to allow for an error of three years for those born in 1834; eventually the appropriate age error decided upon was five years, but this change does not materially affect the results as these errors were of insignificant proportions.

[14] Discrepancies in the mother's name compatible with the father having re-married were accepted, but the minimum criteria for establishing identity were simply that at least one of the parents' names was known to be correct, and that neither of their names was incorrect.

total cases were discrepant. The proportion of discrepant cases, however, rises dramatically with increase in age discrepancies between census and register, and, although the number of cases in most of these groups is too small for precise generalization, the evidence does suggest that the majority of cases with an age discrepancy of five years or less are correct identifications; greater discrepancies yield a majority of incorrect cases. Table 3 also demonstrates the overall accuracy of birthplace statements in the 1851 Census, for these acted as criteria and pointers to the 96.2% of correctly identified cases.

It was therefore decided to include all individuals found in the baptismal register within five years on either side of the expected date of baptism (derived from stated age in the 1851 Census); cases with age differences of 6–15 years were excluded as not being in the register (N.I.R. = Not In The Register).[15]

Table 4. Distribution of Discrepancies in Age Between Census Statement and Baptism Date in Parish Register (45 parishes)

Years	*0*	*1*	*2*	*3*	*4*	*5*	*6–15*	*Total*
Number	1,792	1,226	406	195	93	56	85	3,853
Per Cent	46.5	31.8	10.5	5.1	2.4	1.5	2.2	100

By excluding the group with differences in ages between 6–15 years, we are only excluding 2.2% of all 'matched' cases – the vast majority of these cases are either perfect fits or show discrepancies of two years or less. But there are some special categories in the sample which are over-represented in the group with discrepancies between 6–15 years. The two main factors to be considered are variations in the proportions in N.I.R.s over time and between different parishes. Changes in the proportion of N.I.R.s over time may be measured by arranging the figures by age group as classified in the

[15] It was assumed that age errors greater than 15 years could not occur in the census, and so names outside this range were excluded. It should be noted, however, that baptisms of "persons of riper years" constituted between 2 and 3 per cent of all baptisms in England during the period 1885 to 1958 (see *Facts And Figures About The Church of England*, Church Information Office, 1962, p. 57), and so N.I.R. proportions are likely to overstate birth omissions from baptisms, although not to the full extent of this proportion as some children aged under 15 were included in these figures.

1851 Census, assuming that variations in birthdates are exact reflections of stated age, so that, for example, a person aged 51 in 1851 would have been born in approximately 1800. There was no significant correlation between census/register age discrepancies and different age groups, with one exception. The very small group of those aged between 81 and 90 in the sample of matched cases (51 cases) had a significantly higher proportion of age discrepancies.

Table 5. Distribution of Discrepancies in Age Between Census Statement and Baptism Date in Parish Register for 81–90 Age Group (45 Parishes)

Years	*0*	*1*	*2*	*3*	*4*	*5*	*6–15*	*Total*
Number	20	11	7	3	3	2	5	51
Per Cent	39.2	21.6	13.7	5.9	5.9	3.9	9.8	100

If Table 5 is compared with Table 4, it will be seen that the proportion showing differences of four or five years among the 81–90-year-olds is about two and a half times the figure in the total sample, and that with differences of six to fifteen years about four and a half times as great. The marked difference for the 6–15 years discrepancy suggests that some of the 81–90-year-olds were stating their ages in the 1851 Census inaccurately, although this must not be exaggerated – 90.2 per cent were correct to within 5 years.

Variations in age discrepancies between different parishes are generally insignificant; only 1.0% of the total sample (including matches and N.I.R.s) had age discrepancies of six years or more, and most parishes had discrepancies of this magnitude. Only seven parishes had 2% or more of cases with discrepancies of 6–15 years, which were as follows: Acomb 2 0%; Horton 2.6%; Hemyock 2.3%; Halberton 3.6%; Wheathampstead 2.3%; Hartland 4.6%; Speldhurst 2.1%. The only significant proportions are those for Hartland and Halberton, which also have higher proportions of age discrepancies of 4–5 years than the total sample. The reasons for the higher proportions in these two parishes are unclear.[16] But these age

[16] The proportion for Hartland with discrepancies of two years (9.9%) was significantly higher than average. Although a search for all cases within three years of expected date was made in all registers (in addition to a search of the index) a double search was only made in a limited number of parishes (including Hartland and

discrepancies should not worry us too greatly, for we will see that variations in proportions of N.I.R.s between different parishes are not greatly affected by variations in age discrepancies.[17]

We are now in a position to present the main findings of the census/parish register comparison. The sample of parishes for comparison was selected primarily on the basis of the availability of parish registers in the library of the Society of Genealogists. Parish registers were first selected on account of being typed or printed (and therefore available on loan) and indexed for the period covering at least the years 1760–1837, although there were one or two special exceptions to this. From among these, registers were selected haphazardly depending on their availability. The final sample consisted of 45 parishes, a full list of which is given in Table 6. This Table shows the variation between parishes of the proportion of census cases not found in the baptism register.

Table 6. Distribution of N.I.R.s by Parish, Total Sample (N.I.R. = Not In Register)

Parish	*Population*	*Total Cases*	*N.I.R. Cases*	*% N.I.R.s*
Bramfield, Suffolk	343	116	28	24.2
Aston Abbotts, Bucks.	340	91	10	11.0
Hunsdon, Herts.	481	32	7	21.9
Burtonwood, Lancs.	831	118	33	28.0
Cocking, Sussex	482	68	12	17.6
Acomb, Yorks.	979	101	6	6.0
Lapford, Devon	766	78	12	15.4

Halberton) due to shortage of time. Of nine parishes intensively searched within a two-year period, 66 alternative cases were found out of 1,126 (5.9%). This percentage does not differ greatly from that found through one search in all parishes: 4.0% for the three-year period. These low proportions confirm the general reliability of the census/register comparison method inasmuch as there is no significant problem in selecting a case (and therefore establishing a correct identity) from a number of alternative cases. Most of the evidence – in particular the name of parents – suggests that the vast majority of alternative cases were not related to each other in any way.

[17] Even if all cases in the seven parishes with discrepancies of 6–15 years were defined as being in the register and subtracted from the relevant percentages of N.I.R.s, the relative position of these seven parishes would not be materially affected.

Parish	*Population*	*Total Cases*	*N.I.R. Cases*	*% N.I.R.s*
Chipstable, Somerset	395	60	9	15.0
Wylye, Wilts.	510	89	9	10.1
Bretforton, Worcs.	575	91	5	5.5
Preston-Bisset, Bucks.	554	123	16	13.0
Medmenham, Bucks.	401	62	19	4.8
Horton, Bucks.	—	38	15	39.4
Chetwode, Bucks.	217	33	5	15.2
Barton-Hartshorn, Bucks.	137	29	4	13.8
Rushton, Northants.	429	88	13	14.9
Black-Torrington, Devon	1,115	198	87	44.0
Hemyock, Devon	1,185	222	26	11.8
Chardstock, Dorset	1,387	302	112	37.1
Great Clacton, Essex	1,281	231	43	18.6
Purleigh, Essex	1,184	127	29	22.8
Danbury, Essex	1,221	197	42	21.3
Chiddingstone, Kent	1,260	212	55	25.9
Tooting-Graveney, Surrey	1,089	152	51	33.6
Muker, Yorks.	1,321	362	111	28.9
Hambledon, Bucks.	1,840	177	38	21.5
Iver, Bucks.	1,985	214	53	24.8
Langley-Marish, Bucks.	1,874	222	81	36.5
Fowey, Cornwall	1,606	208	47	22.6
Halberton, Devon	1,745	252	71	28.2
Chigwell, Essex	1,965	168	72	42.9
Wheathampstead, Herts.	1,908	349	138	39.5
Benenden, Kent	1,608	391	130	33.3
Old Malton, Yorks.	1,505	151	38	25.1
Lanercost, Cumb.	1,574	251	68	27.1
Eton, Bucks.	3,666	354	85	24.0
Colyton, Devon	2,504	460	95	20.6
Hartland, Devon	2,183	575	115	20.1
Fordingbridge, Hants.	3,096	729	209	28.7
Ringwood, Hants.	3,928	834	362	43.4
Hadlow, Kent	2,395	430	99	22.8
Speldhurst, Kent	2,839	326	108	32.1
Hackney, Middlesex	30,372	595	345	58.0
Kingston, Surrey	10,622	952	475	49.9
Putney, Surrey	2,845	96	15	15.6

The most important conclusion to emerge from Table 6 is that for some parishes baptism was a rather unreliable method of birth registration – for example, those with over 40% of N.I.R. cases: Black-Torrington, Chigwell, Ringwood, Hackney and Kingston; whereas for others it is strikingly good – for example, those with under 10% of N.I.R.s: Acomb, Bretforton and Medmenham. Most parishes cluster around the overall average of 31.0%. However, there are some significant differences between parishes of different sizes, as will be seen in Table 7.

Table 7. Distribution of N.I.R.s by Size of Parish (45 parishes). Population Sizes of Parishes, 1851

	Under 500 (9 parishes)	*500–999 (7 parishes)*	*1,000–1,499 (9 parishes)*	*1,500–1,999 (10 parishes)*	*2,000 + (10 parishes)*	*Total Sample*
N.I.R. Cases	107	96	556	736	1,908	3,403
Total Cases	579	638	2,003	2,383	5,351	10,954
% N.I.R.s	18.5	15.0	27.7	30.9	35.7	31.0

The larger the parish, the larger the proportion of N.I.R.cases – this is no great surprise and would be expected on commonsense grounds. It is possible that the association between larger parishes and religious nonconformity accounts for part of the correlation between parish size and poor baptism registration. To check the influence of religious dissent on the adequacy of Anglican baptism as a form of birth registration, it is possible to analyse the list of non-parochial registers which were collected by the government in 1838 and 1857.[18]

Nine of the 45 parishes in the main comparison sample were also in the list of parishes possessing non-parochial registers: Hackney, Fordingbridge, Hambledon, Fowey, Colyton, Hartland, Chigwell, Ringwood and Kingston. The proportion of N.I.R.s in these nine parishes is 35.3% compared to the 27.6% of N.I.R.s in the rest of the sample for which no non-parochial registers have survived. The relative similarity of

[18] An attempt was made by the government in 1838 to collect all surviving non-parochial registers for legal purposes and a second attempt in 1857 added virtually no additional registers, which suggests that most of the surviving registers had been collected in 1838. See *Report On Non-Parochial Registers* (Parliamentary Papers, 1838, XXVIII); *Report On Non-Parochial Registers*, (Parl. Pap., 1857, XXIII).

N.I.R. proportions in the two types of parish, suggests that religious nonconformity played a minor role in the non-registration of births through baptism.[19] We will see later, that the registration process itself was probably responsible for most under-registration, and this was probably linked to size of parish.

The most interesting variation in registration accuracy is that occurring over time, as this will throw light on the difficult question of changes in the adequacy of baptism as a form of birth registration.. Table 8 summarizes changes in proportions of N.I.R.s in time periods calculated from age in the 1851 Census.

Table 8. Distribution of N.I.R.s Over Time (45 parishes)

	1761–70	*1771–80*	*1781–90*	*1791-1800*	*1801-10*	*1811–20*	*1821–30*	*1831–34*
N.I.R.s	22	97	208	379	486	656	936	619
Total Cases	68	347	637	1,053	1,517	1,989	3,092	2,251
% N.I.R.s	32.4	27.9	32.6	36.0	32.0	33.0	30.0	27.4

Leaving aside the doubtful figure for the period 1761–70,[20] the overall trend would appear to be one of deteriorating accuracy between 1771 and 1800 and a trend towards improving reliability from 1801 onwards. The changes in the proportion of N.I.R.s from any one decade to another are not great and at no point suggest a collapse in registration reliability as suggested by Krause for the period 1795 to 1820. The trend of deterioration followed by improvement is in line with his analysis, although the degree of change is certainly not of the order suggested by him.[21] This relative lack of deterioration may partly be a function of the rural nature of the sample of 45 parishes. In order to explore this further, a special analysis of the larger parishes in the sample was carried out.

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[19] A comparison between degree of under-registration estimated on proportions of N.I.R.s and the number of entries in the non-parochial registers for Hambledon, Fowey and Chigwell suggests that only 11.5% of all unregistered cases (estimated from N.I.R. figures) were due to non-parochial registration.

[20] The earlier discussion of the accuracy of the census statements of age of this group would suggest that the correct figure for the percentage of N.I.R.s is about 25%.

[21] See Krause, *op. cit.*

Table 9. Distribution of N.I.R.s Over Time

		1761–80	*1781–90*	*1791-1800*	*1801-10*	*18II–20*	*1821–30*	*1831–34*
	N.I.R.s	8	12	11	9	13	28	14
Colyton	*Total Cases*	25	35	41	51	75	141	92
	% N.I.R.s	32.0	34.3	26.8	17.6	17.3	19.9	15.2
	N.I.R.s	5	10	15	14	9	30	32
Hartland	*Total Cases*	35	39	54	87	84	154	122
	% N.I.R.s	14.3	25.6	27.8	16.1	10.7	19.5	26.2
	N.I.R.s	13	18	24	25	33	44	52
Fordingbridge	*Total Cases*	39	47	66	96	129	187	165
	% N.I.R.s	33.3	38.3	36.4	26.0	25.6	23.5	31.5
	N.I.R.s	12	27	42	38	60	115	68
Ringwood	*Total Cases*	36	59	80	110	135	245	169
	% N.I.R.s	33.3	45.8	52.5	34.5	44.4	46.9	40.2
	N.I.R.s	13	31	76	92	133	—	—
Hackney	*Total Cases*	17	44	106	183	245	—	—
	% N.I.R.s	76.5	70.5	71.7	50.3	54.3	—	—
	N.I.R.s	8	22	44	44	67	189	101
Kingston	*Total Cases*	18	41	73	97	130	359	234
	% N.I.R.s	44.4	53.7	60.3	45.4	51.5	52.6	43.2
Colyton, Hartland, Fordingbridge, Ringwood, Hackney, Kingston	*N.I.R.s*	59	120	212	222	315	406	267
	Total Cases	170	265	420	624	798	1,086	782
	% N.I.R.s	34.7	45.5	50.5	35.7	39.5	37.4	34.3
16 Parishes With Population Less Than 1,000	*N.I.R.s*	5	11	14	32	46	55	24
	Total cases	34	74	103	151	238	346	262
	% N.I.R.s	11.7	14.9	13.6	21.2	19.3	15.9	9.2

Table 9 does not suggest that the pattern for larger or for very small parishes was very different from that for the overall sample, and the relatively unchanging reliability of the baptismal registers raises a number of important problems. Krause assumed that the rapid growth of religious nonconformity at the end of the eighteenth century resulted in a major deterioration in the value of Anglican parish records as a source of registration. An examination of the non-parochial registers lodged in the

Public Record Office suggests, however, that only a minority of nonconformist congregations registered their own births and baptisms – by the end of the period 1839 non-parochial entries represented only about 9% of the equivalent Anglican baptism entries.[22] There are several other factors which may have influenced the reliability of Anglican registration, but these need not concern us here; the important point is that there is no known reason to doubt the findings about variations in registration accuracy based on the figures for N.I.R. proportions. In fact, as we shall see later, some independent evidence exists to suggest that the N.I.R. proportions are good indicators even of the absolute level of under-registration.

The above tentative conclusion about the relatively unchanging reliability of parish registers as a source of demographic information applies, of course, only to the aggregate figures for groups of parishes together. For the historical demographer interested in a particular parish it is more important to know whether the register for that parish is reliable as a form of registration over time. Only parishes with relatively large populations are suitable for this kind of detailed analysis and Table 9 summarizes the relevant information for all such parishes. There is a considerable amount of variation between the parishes levels of registration accuracy over time. Colyton and Hackney show a more or less progressive and consistent improvement, Fordingbridge a tendency to improvement, and Hartland, Ringwood and Kingston a fluctuating pattern not unlike that of the total sample of 45 parishes. Both Colyton and Hartland were included in the sample because they were the first two parish registers to be used for the family reconstitution method by E. A. Wrigley.

In Table 9, the number of cases in some of the decennial groupings is too small for firm generalization, although an increase in numbers could probability be achieved in a special study of a particular parish by including married women in the sample. The figures do, however, hold serious implications for family reconstitution work; for example, the significant increase in fertility which Wrigley believed he had found in Colyton at the end of the eighteenth and the beginning of the nineteenth centuries could

[22] See P. E. Razzell, *The Role Of Smallpox Inoculation In The Growth Of Population In Eighteenth-Century Britain* (Oxford University D.Phil. Thesis, 1965), pp. 11–15, for a general discussion of the non-parochial evidence and the basis of the estimated 9% figure. There is evidence to suggest that some nonconformist baptisms were not registered in any way.

be an artefact of the improvement in registration during this period in the parish. [(1994): The same-name technique (considered in Chapter 7), suggests a very similar pattern of changing parish registration accuracy in Colyton. The proportion of N.I.R.s in Colyton was 31.0% in 1761–1800, falling to 17.9% in 1801–34; the proportion of burials unregistered according to the same-name technique was 36.2% in 1751–1800 and 16.5% in 1801–1837.]

The improvement in registration would have less effect on the accuracy of the results of family reconstitution if all unregistered cases tended to occur in particular families; these families could then be excluded, in extreme cases, from the reconstitution sample, and thus the final results would not be materially affected. Evidence from the census/parish register comparison suggests, however, that N.I.R.s were distributed more or less randomly amongst all families: of a sample of families of which at least one member was a N.I.R. (a total of 3,600 cases) 27.8% were not found in the baptismal register,[23] a proportion of N.I.R.s which is lower than that for the total sample, 31.0%.

Table 9 also indicates that a London parish register such as that for Hackney for example, would be quite unsuitable for family reconstitution, or, indeed, for any demographic work. The same might be said of Kingston-on-Thames and Ringwood, since there were decades during which the majority of cases were not being registered through baptism. The findings for Hackney are of particular interest for some of the historical conclusions which have been reached on the basis of parish register data about London's demographic experience during the eighteenth century. Dorothy George, in her work on the eighteenth-century demographic history of London, quotes extensive statistics derived from parish register returns, perhaps the best-known example being that "for the twenty years from 1730 to 1749 the burials of children under five were ... 74.5% of all the children christened".[24] This, of course, was the famous 'gin-period', but the evidence from the census/ parish register comparison for Hackney (which was a part of the London Bills of Mortality area) suggests that such extreme

[23] This sample of families was selected about one-third way through the project and included 30 parishes; only families with at least two members qualifying for the census/register comparison were included in the sample.

[24] M. Dorothy George, *London Life In The Eighteenth Century* (1966), pp. 39, 399. See also her article in *The Economic Journal*, Vol. 32 (1922), pp. 325–352.

levels of child mortality were a function of under-registration of births – the proportion of N.I.R.s for the eighteenth-century period was over 70%. The mortality ratio quoted by George depends on the assumption that baptisms and burials were an equally reliable method of registering births and deaths respectively, for the ratio is simply the number of baptisms at a particular time divided by the number of burials during the same period. If baptisms and burials were equally defective as forms of registration, errors would cancel out, but it is unlikely that some 70% of all deaths were not registered through burial statistics.[25] It would be very premature to regard Hackney as typical of all London, but the census/register comparison for this parish does raise fundamental questions about statistics which have been widely quoted by historians and others as indicating the demographic and social conditions of the period.

Enough has been said to illustrate the value of the census/parish register comparison method in assessing the reliability of register information for particular places; we must now return to the weaknesses and problems associated with the method. It was anticipated that the main problem would lie in the accuracy of the 1851 Census for the larger parishes, and so an 1851/1861 census comparison check was made for individuals in the census/register sample who were living in parishes with populations over 1,000.[26] Table 10 gives the proportion of birthplace statement disagreements for sample individuals in the larger parishes, as well as the N.I.R.s for purposes of comparison.

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[25] A special survey of unregistered burials in London was carried out by the Collector of the Tax on Burials in the last six months of 1794; this survey showed that 3,148 persons were interred without being registered by the Anglican Church. As there were 20,537 Anglican burials in the year 1794, unregistered burials formed about one-quarter of the total for that year. See the *1811 Parish Register Abstract*, p. 200, and George, *op. cit.*, p. 398.

[26] Parishes with smaller populations were excluded because of lack of time.

Table 10. 1851/1861 Census Comparisons
(B.S.D. = Birthplace Statement Disagreement; N.I.R. = Not In Register.)

	B.S.D.s	*Total Cases*	*Percentage Of Birthplace Statement Disagreements*	*% N.I.R.s*
Black Torrington	0	43	0.0	44.0
Hemyock	1	31	3.2	11.8
Chardstock	4	52	7.7	37.1
Great Clacton	0	48	0.0	18.6
Purleigh	2	16	12.5	22.8
Danbury	0	50	0.0	21.3
Chiddingstone	1	36	2.8	25.9
Tooting-Graveney	0	22	0.0	33.6
Muker	0	89	0.0	28 9
Hambledon	1	37	2.7	21.5
Iver	3	42	7.1	24.8
Langley-Marish	3	46	6.5	36.5
Fowey	1	41	2.4	22.6
Halberton	3	44	6.8	28.2
Chigwell	2	39	5.1	42.9
Wheathampstead	2	80	2.5	39 5
Benenden	4	69	5.8	33.3
Old Malton	0	31	0.0	25.1
Lanercost	0	52	0.0	27.1
Eton	1	37	2.7	24.0
Colyton	0	63	0.0	20.6
Hartland	0	71	0.0	20.1
Fordingbridge	1	133	0.8	28.7
Ringwood	4	157	2.5	43.4
Hadlow	2	92	2.2	22.8
Speldhurst	4	55	7.3	32.1
Hackney	8	26	30.8	58.0
Kingston	1	71	1.4	49.9
Putney	0	12	0.0	15.6
Total	48	1,585	3.0	

The table shows that there is little correlation between the proportions of N.I.R.s and B.S.D.s – the 14 parishes with the highest percentages of N.I.Rs (28.2% and above) had an average of 3.9% of B.S.D.s compared with 3.0% B.S.D.s for the whole sample. This latter figure is even smaller

than the proportion of birthplace disagreements for natives in the initial sample of five parishes given in Table 2 (5.3%), and suggests that the 1851 Census was highly accurate in respect of birthplace statements for natives. The superiority of information for natives was also confirmed by analysing data on those individuals who acted as criterion cases for the main sample (women and children in the 'native' group, men being included in the sample). B.S.D.s amounted to 6.5% of 476 native criterion cases as against 14.3% of 357 non-native ones.

Although the high figure for Hackney (30.8% B.S.D.s) is based on the very small sample of 26 cases, it does suggest the possibility of serious census inaccuracy, and in this case, it was impossible to extend the sample in the time available. Census comparison work on large urban areas is extremely laborious. However, information on criterion cases and others proved amenable to analysis.[27] Of 46 of these extra 'native' cases, only 2 were B.S.D.s; of 31 non-native cases, 13 were B.S.D.s. There is little doubt that Hackney was rather like Bethnal Green in its high proportion of birthplace statement disagreements, and the percentage of cases in the latter parish, where the sample was much larger was 11.4% for 'natives.' This figure is very similar to the 13.9% of a total of 72 for all the 'natives' in Hackney who were checked through census comparison. Although this is a relatively high figure, it would only account for a fraction of the very high proportion of N.I.R.s in Hackney. It is likely that the Hackney N.I.R. ratios would be similar to those of Kingston if allowance were made for the higher level of B.S.D.s and the main conclusions drawn from these N.I.R. ratios would not have to be altered.

The fact that only 8.8% of all N.I.R. cases in the census comparison sample were B.S.D.s (36 out of a total of 409) suggests that the overall effect of census inaccuracies on proportions of N.I.R.s is small. Although this proportion is significantly higher than that for matched cases found in the baptism register – 1.0% (12 out of 1,176) – it is still a very small minority of all N.I.R. cases and therefore can only explain a fraction of them.

There is some evidence of a slight correlation between age and the proportion of B.S.D.s, which could distort the accuracy of the frequency

[27] Many individuals who could normally have been included in the main sample but for the fact that the Hackney parish register finished in 1820.

of N.I.R.s over time. However, as will be seen from Table 11, only the 17–20 age group is affected to any appreciable extent.

Table 11. Age as Given in the 1851 Census, Compared to Birthplace Statement Disagreements

	17–20	*21–30*	*31–40*	*41–50*	*51–60*	*61–90*	*Total*
B.S.D.s	11	13	17	9	6	2	48
Total Cases	128	420	400	338	214	90	1,585
% B.S.D.s	0.8	3.1	4.2	2.7	2.8	2.3	3.0

Table 11 should be read in conjunction with Table 8: the proportion of N.I.R.s for the 1831–34 group (this is the same group as the 17–20 age group in the table above) would be about 30% rather than the 27.4% in that table, if it were assumed that the whole of the lower-than-average B.S.D. percentage (2.2%) was due to inaccuracy of the 1851 Census. Other than this slight modification – which is based on an extreme assumption – Table 11 does not suggest any other significant changes to Table 8.

I have assumed that the census comparison method is an adequate check on the reliability of census information, on the general grounds that two independent censuses will yield information on errors in either of the two censuses. However, two fundamental objections may be made against this assumption: (i) that the enumerators were the same in the 1851 and 1861 censuses; (ii) that the individuals enumerated repeated erroneous information in the two censuses. It is possible to check the first factor directly by examining the relationship between enumerator continuity and the percentage of B.S.D.s in the different parishes, as well as by calculating the percentage of all enumerators who acted in both censuses. When this is done no correlation is found between enumerator continuity and the proportion of birthplace statement disagreements, a conclusion further borne out by the fact that only 23.1% of the total number of enumerators (113) acted in both censuses. The problem of erroneous information being given in both censuses is rather more difficult to deal with. The only way of evaluating the scale of this problem is by checking the 1851 Census/parish register age discrepancies against age statement disagreements of the 1851/61 census comparison. If the census comparison method is a reliable method for measuring census errors, it should be possible to trace errors for individuals to

discrepancies between the 1851 Census and the parish register. Table 12 summarizes the relevant data for this checking process.

Table 12. Distribution of 1851 Census/Baptism Register Age Discrepancies and 1851/61 Census Age Disagreements for Individuals in Parishes With Populations Greater Than 1,000

		1851/61 Census/Age Disagreements							
		0	*1*	*2*	*3*	*4*	*5*	*6–15*	*Total*
1851 Census/ Register Age Discrepancies	*0*	420	161	36	13	6	3	3	642
	1	165	137	40	9	5	4	9	369
	2	26	36	17	9	5	3	1	97
	3	12	11	2	9	2	3	1	40
	4	5	2	1	2	0	0	1	11
	5	2	2	1	1	1	1	1	9
	6–15	4	1	1	0	0	0	2	8
	Total	634	350	98	43	19	14	16	1,176

This table needs careful interpretation before it can yield the information we need. The number of cases with large age disagreements is very small, but it is possible to reach certain very tentative conclusions. We want to know from the table the proportion of genuine age errors which are picked up by the census comparison method. Not all the larger census/register age discrepancies can be assumed to be genuine errors, as most of the group with differences of 6–15 years, for example, are probably incorrectly identified individuals (see Table 3). The group with a discrepancy of three years or less between the census and the register provides the most useful test of the value of the census comparison in discovering age errors, as Table 3 indicates that only about 10% of these cases are incorrect identifications. Only 12 of the 40 cases in this group were perfect fits in the census comparison (30.0%) as against 420 of the 642 cases in the census/register fit category (65.4%). Some of the perfect fits in this group may be due to a possible delay of about three years between birth and baptism, so that in these cases the census fit would still be correct in spite of the census /register discrepancy. It is impossible to determine the exact proportion of age discrepancies that can be accounted for by the census comparison method; however, Table 12 does indicate that the method is capable of detecting a very significant proportion.

At the beginning of this paper it was suggested that two main factors might influence the representativeness of the census population. First, variations in the delay between birth and baptism,[28] and the effect of shifts in infant mortality on the proportion of babies baptized. Second, possible correlations between social class and both pre-census mortality and registration reliability. Reliable data on infant mortality in the pre-civil registration period does not exist,[29] but infant mortality would only affect variations in registration accuracy if it accompanied variations in the delay between birth and baptism (the effect of infant mortality on absolute levels of registration is discussed below). A number of registers used in the present study do give some information on dates of both birth and baptism, allowing a tentative analysis in changes in the interval between birth and baptism over time.

Table 13. Interval Between Birth and Baptism Over Time (45 parishes)

	Birth Date (From Age Statements in 1851 Census)						
	1761–80	*1781–90*	*1791–1800*	*1801–10*	*1811–20*	*1821–30*	*1831–34*
Total Cases	38	82	148	268	215	307	234
Approximate Medians (in weeks)	3.5	3	4	4	3	6	6
Means (in months)	6.4	2.3	3.8	3.9	2.4	2.6	3.9

These figures show no discernable trend, although the median interval between birth and baptism shows a tendency to lengthen slightly at the end of the period. These figures do not suggest that we should seriously question the representativeness of the census on the basis of these data.

The other major factor which might disturb the representativeness of the census – social class – can also be assessed. The relationship between social status (as measured by the presence of servants in the household) and the proportion of N.I.R.s can be examined. Of the servant-keeping

[28] The information in Table 13 indicates that there was no significant overall change in interval between birth and baptism during 1761–1834. This conclusion is confirmed for 'median' parishes in the period 1771–1812 by Berry and Schofield. Their evidence suggests, however, significant variations between individual parishes. See B. Midi Berry and R. Schofield, "Age at baptism in pre-industrial England", *Population Studies*, Vol. 25, (1971), pp. 453–464.

[29] (1994): See a discussion of this issue in Chapter 7

class, 32% were N.I.R.s (382 out of 1,194), as against 30.8% of the rest of the sample without servants (3,004 out of 9,760). This absence of a significant correlation between social status and registration reliability is confirmed by the evidence for occupational class and proportions of N.I.R.s. Table 14 indicates that there was some significant variation between the different occupational groups and registration accuracy, but there is no linear relationship between the social status of the occupational group and the proportion of N.I.R.s.

Table 14. Distribution of N.I.R.s by Occupational Group (35 parishes)[30]

	Occupational Group						
	Professional	*Farmers*	*Shopkeepers*	*Pensioners*	*Servants*	*Labourers*	*Paupers*
N.I.R.s	49	194	789	16	88	1,282	25
Total Cases	118	724	2,454	67	244	4,207	61
% N.I.R.s	42.2	26.8	32.2	23.9	36.1	30.5	41.0

There is no obvious pattern in this table: a high-status group like the professionals has the highest N.I.R. ratio, but other high-status groups like farmers and shopkeepers have relatively low ratios. The lack of a correlation between occupational status and N.I.R. ratios suggests that social class did not distort the census/parish register comparison.

For much demographic work it is necessary to estimate the absolute levels of under-registration. Because of the exclusion of young infants who died before baptism, N.I.R. proportions are not a complete measure of non-registered births. It is possible to estimate the scale of this problem by examining the interval between birth and baptism in the light of the Registrar-General's evidence about infant mortality during the early period of civil registration. Table 15 gives the distribution of intervals between birth and baptism for all 1,292 cases in the sample for which the relevant information was available.

[30] The parishes of Bramfield, Cocking, Acomb, Lapford, Chipstable, Wylye and Bretforton are not included in this sample since the appropriate information was not collected at the early stage of the research. The names of the occupational groups indicate the occupations included in each group (where there was some indication of a pauper's previous occupation he was classified under that heading).

Table 15: Distribution of Intervals Between Birth and Baptism for the Total Census/Baptism Register Sample

Delay	*0–6 days*	*7–13 days*	*14–20 days*	*21–27 days*	*28 days to 2 months*	*2–3 months*	*3–6 months*	*6 months to 1 year*	*1 year +*
Number of Cases	112	54	159	313	341	95	99	52	67

The table shows that the median interval between birth and baptism was about one month and about 75% of the sample had been baptized within two months. According to the Registrar-General's figures for 1839–1844, just after the introduction of civil registration, approximately 4.3% of all children born in England and Wales died within the first month of life, which was the median interval between birth and baptism for the whole census/register sample.[31] It cannot be assumed that this percentage was typical of similar infant mortality during the whole period 1760–1834, and there is some evidence that a figure of about 5% would be a low estimate of infant mortality for the median interval of one month.[32]

It cannot be assumed that all infants dying in the first month after birth were not registered through baptism, since the Church of England made special provision in cases of "great cause and necessity" for the private baptism of sickly infants. However, private baptisms were often not entered in the baptismal register;[33] although Rickman claimed that the

[31] For the numbers of infants dying at various times within the first year of life, see the *Registrar-General's Eighth Annual Report* (1847/48), p. 282. For the number of births during the period 1839–44, see B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), p. 29. I have inflated Mitchell and Deane's number of births by 8%, which is the approximate inflation ratio suggested by Glass on the basis of comparing census and civil registration data. See D. V. Glass, "A note on the under-registration of births in Britain in the nineteenth century", *Population Studies*, Vol. 5, (1951), pp. 70–88.

[32] (1994): See the discussion of infant mortality in Chapter 7.

[33] For example, in the period before 1811, see *B.M. Add. MSS 6896*, folios 7, 8, 11, 29 and 80. Rickman writing in the report on the 1801 Census observed that those "who are privately baptised are not always registered. The practice of the Clergy is not uniform on this point ..." ("Observations on the results of the Population Act, 41 Geo. 3", *1801 Census, Parish Registers*). Examples of registered private baptisms are to be found in the Kelsale, Suffolk register for the period 1801–12, and the Wylye, Wilts., register for 1813–20. See Krause, in Glass and Eversley, *op. cit.*, p. 391.

1812 Parish Register Act had virtually ensured the registration of all private baptisms by making this compulsory by law.[34] This might mean that there was a relative increase in registration accuracy after 1812, not measured by the N.I.R. statistics. Some infant private baptisms are known to have been registered before 1812 and not all neo-natal deaths would have been registered through private baptism after 1812. Given that the N.I.R. figures suggest that about one-third of all births were not registered through baptism during 1760–1834, the unmeasured element of neo-natal mortality would not greatly affect the N.I.R. figures as indices of absolute levels of registration accuracy, even before 1812 (one-third of the 5% would in any case be accounted for by 'normal' under-registration).

The validity of the N.I.R. figures as an index of absolute registration inaccuracy in the country as a whole can be checked roughly for the 1830s, since alternative means of estimating registration accuracy became available. Glass has estimated that at least 24.8% of all births in England and Wales during 1831–37 went unregistered through Anglican baptism. He arrived at this estimate by comparing national baptism and civil registration data with census/civil registration information.[35] This independent estimate of 24.8% is close to the N.I.R. proportion of 27.4% for the 1831–34 age group in the census/registration sample. The fact that the N.I.R. sample used in this paper consists of only 45 parishes selected according to the availability of registers means that generalizations based on this sample must be treated as hypotheses to be tested by an application of the comparison method to a larger and more randomly selected sample. Nevertheless, the similarity between Glass's estimate of registration inadequacy and that derived from the N.I.R. proportions does suggest that the latter can be used tentatively as an index of absolute registration inaccuracy in the country as a whole.

One obvious drawback of the census/register comparison procedure is that because it takes its starting point from the 1851 Census, it is only applicable to people born in the 1760s at the earliest. Much of the controversy about population change during the period of the industrial revolution concerns changes which occurred from the early eighteenth century onwards. It is, however, possible to apply the method outlined in

[34] See "Preliminary Observations", *1821 Census*, p. xxvii, and *Parish Register Abstract, 1831 Census*, p. xiv.

[35] Glass and Eversley, *op. cit.*, p. 234.

this paper to data for this earlier period. The marriage licence information for the Archdeaconry of Chichester area of Sussex contains detailed information on the period of residence in the parish of those marrying after 1760,[36] and I have carried out an exploratory study of a number of cases where the parties were stated as having resided in a particular parish 'all their lifetime'.

Thirteen parishes with an average population of about 1,200 in 1851 were selected for analysis,[37] and 60 people marrying in the decade 1758–1768 were compared with 60 others marrying in the period 1795–1800. The mean age at marriage of the first sample was about 25 years and that of the second 24 years, indicating from the stated age of marriage on the marriage licence, that the first group of births centred around 1738 and the second around the years 1773–74. In both groups it was possible to locate 48 of the 60 cases in the appropriate baptism register within five years either side of the expected birth date (this taken from the stated age of marriage on the marriage licence). Thus, exactly 20% in both groups may be considered as being the equivalent of N.I.R.s, a figure which, in spite of the small numbers and the different source of information about name, age and birthplace, is very similar to that of comparable groups in the census/register sample.The most important point about the similar proportions of married people being found in the baptism registers, is that it does not suggest any significant change in registration accuracy for Sussex parishes from the 1730s to the 1770s. It is evidently necessary to extend this type of work before any firm conclusions can be reached. Information similar to that found in these Sussex marriage licences may be available for other areas and possibly for even earlier periods.

Why were there so many missing baptisms in the period covering the 1730s to the 1830s? One of the main factors was the separation of the acts of baptism and burial from the actual registration process itself. The law stipulated that "the parson, vicar, or curat ... shall every Sonday take furthe [the register], and in the presence of the said [church] wardens, or one of

[36] See D. Macleod (ed.), *Calendar Of Sussex Marriage Licences* (Sussex Record Society, XXXII, 1926, and XXXV, 1929).

[37] The parishes with their 1851 populations are as follows: Petworth, 3,439; Pulborough, 1,825; Yapton, 609; Boxgrove, 755; Mid-Lavant, 284; East Dean, 419; Midhurst, 1,481; Eastbourne, 1,076; Funtington, 1,079; Bosham, 1,126; Westbourne, 2,178; Walberton, 578; Aldingbourn, 744.

them, write and recorde in the same all the weddings, christenyns, and burialles, made the hole weeke before ..."[38] Although Rose's Act of 1812 modified this provision it did so only to the extent of requiring registration to take place within seven days of the event itself. Needless to say, the law was frequently ignored. The week-to-week business of registration tended to be left in the hands of the parish clerks, some of whom kept their own records which were copied by the incumbent into the parish register sometimes as infrequently as once a year.[39] The effect of handing over registration to parish clerks was discussed by Burn in his study of parish registers, first published in 1829:

> The custody of Parish Registers having been frequently committed to ignorant parish clerks, who had no idea of their utility beyond their being occasionally the means of putting a shilling into their pockets for furnishing extracts, and at other times being under the superintendence of an incumbent, either forgetful, careless or negligent, the result has necessarily been, that many Registers are miserably defective, some having the appearance of being kept from month to month, and year to year, yet being deficient of a great many entries ...[40]

A correspondent of the *Gentleman's Magazine* in 1805 had reached the same conclusion:

> Our Parochial Registers are in many instances now kept by Parish-Clerks, and as these Record-keepers derive no profit from the employment, except a casual shilling now and then for a search, it may be imagined what sort of Record is kept, where ignorance and negligence are united.[41]

Burn quoted numerous examples of the negligence of parish clerks, the one most illuminating for our present purposes being the

> parish clerk [who] said it was usual for him, and not the clergyman, to take an account of Burials, and he entered them in a little sixpenny memorandum book, thus: 'A.B. 1s.' If the fee was paid at the time,

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[38] J. S. Burn, *The History Of Parish Registers In England* (1862), p. 18.
[39] *Ibid*, p. 261.
[40] *Ibid*, p.40.
[41] *The Gentleman's Magazine*, Vol. 75, No. 22 (1805), p. 1117.

> no name would be put into his book, he only booked what was due to him and the clergyman entered in the Register at the end of the year from his Memorandum Book.[42]

Much under-registration was clearly directly due to negligence by the incumbent. The *Gentleman's Magazine* remarked in 1811 that "the clergyman (in many country places) has entered the names at his leisure, whenever he had nothing better to do, and perhaps has never entered them at all."[43] Examples of such negligence can be found from the beginning of parish registration: in 1567 the incumbent of Tunstall, Kent, appeared to have tired of registering the Pottman family because of its concentration in the parish and simply stated in the register: "From henceforwd I omitt the Pottmans".[44] That this kind of negligence continued thereafter is indicated by the fact that in 1702–03 "a committee of Convocation drew up a list of ecclesiastical offences notoriously requiring remedy, in which irregularity in keeping registers is prominent in the list of gravamina".[45] Rickman gave one reason for under-registration of both births and deaths: "negligence may be supposed to cause some omissions in the Registers, especially in those of small Benefices, where the Officiating Minister is not resident".[46] Perhaps an extreme example of such irregularity in the period of greatest relevance to this paper is provided by the attempt by Charles Reynolds to 'prove' that William Fogden was baptized in the parish of Appledram, Sussex, for marriage licence purposes. At the end of 1822 Reynolds stated:

> that he hath this Day applied to the Clerk of the Parish of Appledram in the County of Sussex in which Parish this Deponent was informed the said William Fogden was born for the Register of Baptisms within that Parish when the Clerk said he did not know where it was that he then went to the House in the City of Chichester where the Reverend Mr Broadwood the Minister of the said Parish of Appledram resided to ask him for the said Register where he was informed

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[42] Burn, *op. cit.*, p. 44. This paragraph is quoted at greater length in Chapter 8 of the present book.

[43] *Ibid*, p. 42

[44] *Ibid*, p. 41.

[45] W. E. Tate, *The Parish Chest* (1969) p. 49.

[46] *Preliminary Observations, Population Returns 1811* (Parl. Pap. 1812, XI), p. xx.

the said Mr Broadwood was absent from Chichester And this Deponent saith that he hath been informed that the Churchwarden of Appledram, if there is any, is gone away from the said Parish.[47]

It was not only negligence that lead to under-registration. One Northamptonshire incumbent indirectly indicated that the clergy were not above the practice of refusing to register cases for which fees were not paid: "The very true reason why this [Brington] register, is found as imperfect in some years as from 1669 to 1695 is because the parishioners could never be persuaded to take to see it done, nor the church-wardens as ye canon did require, and because they refuse to pay such dues to ye curate as they ought by custome to have payed."[48] Enough has been said on this subject to indicate to the reader probable reasons for the large-scale under-registration of both births and deaths.[49]

What are the implications of the tentative conclusions reached in this paper about the adequacy of Anglican registration ? If the statistics of national baptisms collected in connection with the 1801–1841 Censuses are inflated by the appropriate N.I.R. ratios, approximate birth rates may be computed for decennial periods between the first five population censuses (Table 16).

Table 16. Estimated Birth Rates for England and Wales, 1801–1841[50]

	1801–10	*1811–20*	*1821–30*	*1831–40*
Birth Rate Per 1,000 Population	41.4	42.0	40.1	35.9

From a high birth rate in the 1800s there appears to have been a fall to a rate in the 1830s which was very similar to that computed by Glass

[47] "Marriage licences and affidavits", *Document EP. 1 19* (West Sussex Record Office).

[48] J. C. Cox, *The Parish Registers Of England* (1910), pp. 20, 21.

[49] For a very general source of further evidence on this subject, see the evidence taken before the Select Committee in 1833 to he found in *Parl. Pap. 1833, XIV.*

[50] I have used the adjusted population figures computed by Krause from official returns, by making certain assumptions about early census under-enumeration. See J. T. Krause, "Changes in English fertility and mortality, 1781–1850", *Economic History Review,* Vol. 11 (1958–59), p. 60. For numbers of baptisms, see Mitchell and Deane, *op. cit.*, p. 28. The population figures for each period were arrived at by taking an average of the beginning and end of a decade.

independently for the same decade – and, according to further estimates by Glass, the birth rate remained at this level for a further three decades.[51]

Unfortunately there is no known similar method for analysing changes in the adequacy of Anglican burial as a form of death registration,[52] although it should be possible to extend Glass's work on registration accuracy in London at the end of the seventeenth century to many parts of the country. Where reliable population figures are available and there is insignificant net migration (inward – outwards) it is possible, however, to calculate burial/death ratios when firm estimates of the total number of births are at hand (where migration is zero, net population increase = births – deaths). Using the number of births calculated for Table 16, we arrive at the following figures:

Table 17. Estimated Proportions of Deaths Omitted From Anglican Burial Registers, England and Wales, 1801–1841[53]

	Percentage of Omitted Deaths	
Period	*On Official Population Returns*	*On Krause's Population Estimates*
1801–10	34.1	37.5
1811–20	38.8	42.4
1821–30	28.3	27.9
1831–40	15.7	15.2

Two sets of population figures have been used to prepare this Table: the uncorrected official figure derived directly from the early nineteenth-century censuses, and 'corrected' estimates made by Krause, which assume that the 1801 Census was deficient by 5%, the 1811 Census by 2% and the remaining three censuses (1821, 1831 and 1841) by 1%. In fact, these assumed corrections do not materially affect the overall trend of burial registration accuracy, which suggests a high level of

[51] Glass and Eversley, *op. cit.*, p. 240.

[52] (1994): But see the new work on checking the accuracy of burial registration discussed in Chapters 7 and 8.

[53] The evidence on net migration is scanty but does not suggest that it would significantly affect the figures in this table. See Krause, "Changes in English fertility ...", *op. cit.*, p. 60; and for numbers of burials, see Mitchell and Deane, *op. cit.*, p. 28.

omissions for 1801–1820, followed by a dramatic improvement in the 1820s and 1830s. It is possible to check the estimate for the 1830s by comparing it with that made by Glass – who compared Anglican burial returns against civil registered deaths for the late 1830s[54] – which was 18.7% as against 15.7% and 15.2% in Table 17.[55] The methods of estimating burial deficiency in Table 17 was entirely independent Glass's, using different techniques and sources of information. The similarity of the results for the 1830s is noticeable and certainly lends support to the value of the procedure that I used in preparing Table 17. The percentages for the first two decades of the nineteenth century are surprisingly high, although Krause has consistently argued that Anglican registration improved after 1821.[56] The difference between the earlier and later periods is partly due to the fact that the published returns of burials for the 1800s and 1810s do not appear to have included any significant number of non-parochial returns, whereas the 1820s included at least 126,000 non-parochial burials of religious dissenters and others, and the 1830s a minimum of 156,000.[57] These figures only represent, however, about 3% and 4.5% respectively of the estimated total number of deaths during these decades, and so would not alter significantly the trend of burial/death registration from the beginning of the century.

The high level of under-registration of deaths during the first two decades of the nineteenth century raises the problem of how such an objectively observable event as death, which by all accounts invariably resulted in Anglican burial, escaped registration through the parish burial registers. There is, of course, evidence of the existence of large-scale non-parochial burial grounds in the bigger towns, and Krause has drawn attention to some absurdly low official burial rates for the first two or three decades of the nineteenth century in these same places.[58]

Large towns, however, only contained a small minority of the total population in the early nineteenth century, and London, which was by far the largest urban area, does not appear to have had a burial/death omission

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[54] With a slight correction factor for deficient civil registration.

[55] For Glass's estimates see Glass and Eversley, *op. cit.*, p. 234.

[56] For example, Krause in Glass and Eversley, *op. cit.*, p. 392.

[57] *Ibid*, pp. 390 and 392. Krause estimates that there were only 1,300 non-parochial burials included in the figures for the 1810s.

[58] Krause, "Changes in English fertility ...", *op. cit.*, p. 56.

rate very different from that quoted from the first two decades in Table 18. The main reason the under-registration of burials is probably the same as that for the under-registration of baptisms – but the discussion of this is dealt with in Chapters 7 and 8.

Using the burial omission proportions outlined in Table 17 it is possible to calculate death rates for the period 1801–1841.

Table 18. Estimated Death Rates per 1,000 Population for England and Wales, 1801–1841[59]

Period	*On Official Population Returns*	*On Krause's Population Estimates*
1801–10	30.1	30.5
1811–20	27.7	28.9
1821–30	26.1	25.9
1831–40	23.1	22 8

Both sets of estimates suggest a similar pattern of a significant decline in mortality from 1800 onwards – they differ only in the chronology of this decline. Crude death rates, of course, are only rough indices of mortality as they can conceal genuine changes in mortality disguised by shifts in the age structure of the population. The comparison of the age structure of the population in 1821 and 1841 does not reveal any such significant shifts,[60] so that the figures in Table 18 suggest that there was a real fall in mortality.

The published statistics of age structure of the enumerated 1821 and 1841 populations provide us with an appropriate source of information which, with the first life table of 1838–44, based on civil registration, permits an independent check of the plausibility of the figures in Table 18. The expected number of survivors of a particular age group at a specific moment can be calculated by applying age-specific mortality rates to the estimated numbers of children born into that group. The likely number of

[59] Populations for each period are averages of the censuses at the beginning and end of each decade. For the populations used in computing these rates, see Krause, "Changes in English fertility ...", *op. cit.*, p. 56; for burials, see Mitchell and Deane, *op. cit.*, p. 28.

[60] Glass and Eversley, *op. cit.*, p. 215. The slight decrease in the proportion of young children revealed by this comparison is an indirect confirmation of the fall in fertility outlined in Table 16.

births in the period 1801–1841 has already been estimated above through the use of the N.I.R. ratios.

There is no direct evidence on age-specific mortality for this period, but there is good reason to believe that the 1838–44 life-table gives a good approximation of the relative rates of mortality between the different age groups, from about 1813 onwards. The returns of Anglican burials, during the period 1813–40, contain information on age at death, and a comparison of the data for 1813–30 and 1831–40 indicates that there was no significant change in the distribution of ages at death between the two periods. The age structure of the 1821 and 1841 populations was very similar, which suggests that relative age-specific mortality was approximately the same in both periods. The distribution of ages at burial and the equivalent age structure of the population (England and Wales) in younger age groups was as follows:

	Distribution of burial ages, 1813–40	*Distribution of burial ages, 1831–40*
	%	%
0–4	34.5	35.8
5–9	4.2	5.0
10–14	2.7	2.7
15–19	3.4	3.5
	Age structure of population, 1821	*Age structure of population, 1841*
	%	%
0–4	14.9	13.2
5–9	12.6	12.0
10–14	11.1	10.9
15–19	10.0	10.0

The conclusion that relative age-specific mortality was probably the same in both periods depends on the assumption that the relative omission of deaths from burial registration by age group did not vary over time. As only very large variations of this kind could affect relative age-specific mortality significantly, this assumption is not a particularly critical one.[61]

[61] For the evidence on burial ages, see *Parish Register Abstract 1831* (Parl. Pap. 1833, XXXVIII), p. 488, and *Parish Register Abstract 1841* (Parl. Pap. 1845, XXV), p. xxi. For comparative age structure see *Registrar-General's Sixth Annual Report* (1844), p.xxvi.

The period 1831–40 is sufficiently close in time to the years for which the first life table was constructed (1838–44), to permit the assumption that mortality was about the same in both periods – this assumption is, moreover confirmed by the fact that the estimated death rate for the 1830s in Table 18 (about 23 per 1,000) is almost identical to that during the years 1838–44 calculated from civil registration returns. Of course, the absolute levels of mortality at different ages cannot be assumed to have been the same during the whole period 1801–1840, and, in fact, the estimated death rates in Table 18 suggest otherwise. If we use these death rates as indices of absolute mortality levels, we can weight relative age-specific mortality taken from the first life table by multiplying by the proportionate excess of a decade's death rate over that for the 1830s.[62] This means that the estimated burial omission rate is under evaluation, as this was used to calculate the death rates during 1801–40. The final result of the relevant calculations is set out in Table 19.

Table 19. Expected and Actual Numbers of People Living at Various Ages, England and Wales (in thousands)[63]

Age Group	*Expected 1821 Census*	*Actual 1821 Census*	*Discrepancy (percentage)*
0–9	3,127	3,347	–6.3
10–19	2,539	2,552	+0.7
Age Group	*Expected 1841 Census*	*Actual 1841 Census*	*Discrepancy (percentage)*
0–9	3,931	3,997	–1.7
10–19	3,503	3,307	+5.9

These figures suggest that the estimates of under-registration of births (the

[62] I have calculated proportions of children dying by a particular age, according to the 1838–44 civil registration returns from the detailed age-specific mortality figures for males and females given in Mitchell and Deane, *op. cit.*, pp. 38, 40, and weighting them for sex differences in age structure with figures given in the *Registrar-General's Sixth Annual Report* (1844), p. xxvi. For the absolute indices of mortality I have used the death rates calculated from Krause's estimated population figures.

[63] For the actual numbers living in each age group, see Glass and Eversley, *op. cit.*, p. 230.

N.I.R. proportions) and deaths (through comparison of enumerated populations statistics) are of a high order of plausibility. The discrepancies for the 0–9 age group in 1821 and the 10–19 age group in 1841 are fairly significant, but they are small compared to the correction rates applied to the original raw figures (the expected figures of Table 19 depend mainly on the correction rate to baptisms and this falls within the range of 27.4% to 33.0%).[64]

CONCLUSION

The comparison of census and baptism register data for 45 parishes indicates that the level of registration adequacy did not change significantly during 1760–1834 . About one-third of all births were omitted from baptism registers – and there is some evidence to suggest that this level of accuracy was typical of both earlier and later periods. There were, however, some marked variations between individual parishes, and significant variations over time in individual parishes. A number of sources of error in the census/register comparison method emerged, although no substantial evidence has emerged from this study to question its basic validity. It is therefore suggested that all family reconstitution projects using parish registers include a comparison of the 1851 Census with the baptismal register so as to evaluate register reliability. To economize on time it would be sufficient to collect information on the census ages of individuals (in order to analyse changes over time) and to use the following criteria for establishing a person as being in the register: (i) name the same; (ii) age within five years of that expected from the 1851 Census. If this information were collected for a sufficient number of parishes it would be possible to make firm generalisations for the country as a whole, particularly if cities such as Nottingham which have complete sets of parish registers could be included (in these cases it would be a question of comparing city of birth with all the registers for that city). The method might also be capable of being extended to data for much earlier periods than that dealt with in this

[64] The actual figures in Table 19 would also probably require correction because of under-enumeration in the 0–4 age group; Glass has estimated this to be about 5% in 1841; Glass and Eversley, *op. cit.*, p. 234.

paper. The census/register method already points to certain tentative conclusions about the pattern of fertility and mortality during the period of the Industrial Revolution; further studies in conjunction with family reconstitution projects could help finally to answer the fundamental problem of the relationship between population and economic growth in the eighteenth and nineteenth centuries.

Chapter 5

Further Evaluation of Anglican Baptism as a Form of Birth Registration Through Cross-matching Census, Parish and Civil Register Data[1]

This paper was written in response to various criticisms of the census/baptism register comparison method outlined in the previous chapter. There were three main criticisms of the method: (i) the 1851 Census significantly mis-stated the birthplace of individuals enumerated; (ii) many children were baptised outside their parish of baptism; (iii) the 1851 Census occasionally mis-stated names and ages.

An evaluation of these criticisms was carried out by cross-matching census, baptism and civil register data. The conclusions from this research for the parishes studied are: (i) about 3.2 per cent of all birthplace statements were in error; (ii) approximately 4.0 per cent of children were baptised outside of their parish of birth in the period 1813–52; (iii) about 2.1 per cent of all census entries were in error on name and ages statements sufficient to create a false match. In total, it is estimated that 9.3 per cent of all census cases not found in the baptism register (N.I.R.s) were due to under-matching as a result of these three categories of error.

However, this is counter-balanced by the over-matching of cases due to: (i) over-strict criteria employed in ruling out correct matches; (ii) missed births resulting from infant death before baptism. These two sources of over-matching are estimated as forming about 10.2 per cent of all cases, in effect counter-balancing the 9.3 per cent of under-matched cases. It is

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[1] Not previously published. The research on which this paper is based was funded by the Social Science Research Council.

concluded that the census/baptism register comparison method is overall a simple and reliable method of estimating baptism register accuracy.

The bulk of historical work on demography of England before the introduction of civil registration is dependent on Anglican parish registers for its source material. The development of new techniques of analysis, such as family reconstitution, does not overcome the problem of unreliable raw data, and the application of these new techniques alongside the proliferation of work using older methods like aggregative analysis, makes it essential to evaluate the quality of Anglican register information as a form of demographic data. Previous work by the present author[2] involving the comparison of census and parish register information, suggested large deficiencies in the reliability of Anglican baptism as a form of birth registration, with marked variability in the quality of data between different parishes.

The cross-matching of census and baptism register data works on the assumption that the 1851 Census (the first to give full details of birthplace) accurately states name, age and the birthplace of individuals enumerated. It also assumes that a person's birthplace is the same as his or her parish of baptism, allowing a comparison of the expected parish of birth (from the census) and the actual parish of birth as indicated through baptism. The validity of this cross-matching methodology has been criticised on three grounds: (i) the 1851 census significantly mis-states the birthplace of individuals enumerated; (ii) many parents had their children baptised in neighbouring parishes to that in which they were born, exaggerating the short-fall between births and baptisms; and (iii) "to a lesser degree", the 1851 Census sometimes mis-states names and ages.[3]

Tony Wrigley and his colleagues at the Cambridge Group have developed the census/ baptism register comparison method by applying it in great detail to Colyton and a number of surrounding parishes, the results of which have been published in *Population Studies*.[4] Obviously a single

[2] See Chapter 4.

[3] E. A. Wrigley, "Baptism coverage in nineteenth century England: the Colyton area", *Population Studies*, Vol. XXIX, No. 1 (1975).

[4] *Ibid*, p. 178.

parish will not necessarily be representative of the population as a whole; in the original cross-matching study of census and baptism registers, Colyton had a N.I.R. (= Not Found in the Baptism Register) ratio of 20.6 per cent, which was significantly below that of the average of the whole sample of 45 parishes: 31.0 per cent. Wrigley has quoted the low N.I.R. ratio of 6.3 per cent for Bottesford[5] in support of his general conclusions, and he equally could have referred to the even lower ratios for Acomb, Yorks (6.0 per cent), Bretforton, Worcs. (5.5 per cent) and Medmenham, Bucks. (4.8 per cent) from my original study, but this would be misleading as several parishes had N.I.R. ratios in the 40–50 per cent range, and, as we have seen, the average was above 30 per cent. This latter conclusion must be borne in mind when we are assessing the significance of findings about baptism registration in the Colyton area.

The major method used by Wrigley and his colleagues in assessing the quality of the 1851 Colyton census is the comparison of its contents with data taken from the 1841 and 1861 censuses for the same parish. This is an extension of the method employed in my first paper,[6] the difference being that an analysis of all birthplace statements for individuals listed in the censuses has been undertaken, instead of confining it to people enumerated as Colyton-born in the 1851 Census. This has meant that not only "loss errors" (individuals listed in 1851 as being born in Colyton, but elsewhere in 1861) could be counted, but also "gain errors" (elsewhere in 1851, Colyton in 1861).The names that Wrigley has given to these inconsistencies in census birthplace statements, indicate the nature of his analysis. It is assumed that most of the inconsistencies in the first category are due to individuals having mistakenly claimed Colyton birth in the 1851 Census, and then corrected their birthplace statement in the subsequent one; similarly, the people in the second category, are defined as having "gained" an error by changing a correct birthplace statement in 1851 to an incorrect Colyton one in 1861. Some evidence is produced in support of these assumptions: of 16 "loss error" cases traced in a baptism register, 10 were baptised outside of Colyton, while 6 were baptised in the parish.[7] Similarly, of the 35 "gain errors", only one could be traced in the Colyton baptism register, although it is not known whether any of the remaining

[5] *Ibid*, p. 316, fn. 27.
[6] See Chapter 4.
[7] *Ibid*, p. 316.

34 cases were baptised in the birthplace mentioned in the 1851 Census, as no search of these registers has been carried out.[8] At first sight the evidence on the baptism patterns of the "loss errors" and "gains errors" appears to confirm the assumptions about the nature of the errors. In one respect this is crucial to the argument, because it is on the basis of these "errors" that the later estimates of the number of birthplace statement errors in the 1851 Colyton census are made.

The assumptions about "gain errors" were further tested by dividing the sample between those present in Colyton in 1841 and 1851, as against those only present in 1851. The argument is that if there is a tendency for people born outside of the parish to cumulatively claim it as their birthplace, more "gain errors" would occur among those who were in Colyton in 1841 and 1851, than among those enumerated for the first time in 1851. Unfortunately this evidence suffers, as does that above about the baptism patterns of those in the two "error" categories, from a central defect: 62.5 per cent of cases baptised in a neighbouring parish, had parents resident in Colyton at the time, indicating that nearly two-thirds of the cases baptised outside of Colyton were in fact born there, suggesting that most of the birthplace statements were correct.[9] This has a very basic significance for the discussion about the reliability of census birthplace statements which I will return to later, but for the moment it is sufficient to point out that it brings into question the small amount of direct evidence supporting the assumptions about loss and gain "errors".

The second criticism of the census/baptism cross-matching methodology is the unreliability of name and age statements in the census. In practice this criticism has been seen as very minor compared to the postulated deficiencies of the census with respect to birthplace statements. A great deal of evidence from my first paper showed how reliable age statements were according to both 1851/1861 census comparisons, and the checking of age statements with date of baptism. This is a conclusion that Wrigley is in agreement with: he notes that 10 cases of a match between census and baptism register out of a total of 1,133 (0.9 per cent) would have been missed by assuming that age was accurate to within five years,

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[8] *Ibid*, Private communication from Tony Wrigley.

[9] This finding applies to the 80 (of the 107) F cases with information on residence. See Table 11, *Ibid*, p. 313. For Wrigley's conclusions about the birthplace of these cases see pp. 313, 314.

and the vast majority of cases were accurate to within one year.[10] The problem of name inaccuracies has been found to occur on a similar scale: it is estimated that about 17 links out of a total of 1,471 (1.2 per cent) would have been missed by the census/baptism register comparison method on account of name recording inaccuracies.[11] Taken together, age and name inaccuracies would only account for 2.1 per cent of unmatched cases.

The third ground of criticism of the cross-matching methodology is that it assumes all children were baptised in their parish of birth. The Cambridge Group through its contact with local amateur historians has been able to arrange a search of 23 registers of parishes in the neighbouring area of Colyton, so as to check the parish of baptism of those claiming Colyton birth. Of a total of 1,433 people claiming birth in Colyton (in the 1851 Census), 107 (7.5 per cent) were found to be baptised in neighbouring parishes, and for the pre-civil registration group who were ten years and over, the proportion was even higher – 89 out of a total of 910 (9.8 per cent).[12] There are two possible reasons for these people being baptised outside of their claimed parish of birth: (i) the 1851 census was incorrect on birthplace statement, and these cases were actually born in their parish of baptism; (ii) the census was correct on birthplace, but parents were simply baptising their children in neighbouring parishes. There is enough additional evidence available to settle this question fairly definitively. Wrigley has analysed the relationship between the area within Colyton and the location of the neighbouring parish in terms of geographical proximity and found a very close correlation, that is to say people baptised outside Colyton tended to use the parish church adjoining the Colyton localities in which they were living in 1851. He points out that:

> the parish church of Colyton is far less accessible from the periphery of the parish than from any part of the town of Colyton itself. Many people lived nearer to other parish churches than to their own.[13]

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[10] *Ibid*, p. 303. As my original sample excluded married and widowed women, the proportion of missed matches on account of age would have been even smaller, i.e. 7 out of 1,000 (0.7 per cent), instead of 10 out of 1,133 (0.9 per cent).

[11] *Ibid*, pp. 302, 304.

[12] *Ibid*, p. 309.

[13] *Ibid*, pp. 311, 312.

Even more direct evidence comes from the study of the stated residence in the baptism registers of 80 of the 107 children baptised outside of Colyton (most of these are people who were under forty in 1851, because place of residence only appeared regularly in the baptism register after the introduction of Rose's Act in 1812). As previously noted, fifty of the eighty were resident in Colyton at the time of their baptism, and taken along with all the other evidence about geographical residence and baptism patterns, the conclusion is escapable:

> It is evident that Colyton residents frequently preferred to use the services of incumbents of neighbouring parishes for baptisms and that therefore many respondents were giving accurate information at census time in claiming Colyton birth even though they had been baptised elsewhere.[14]

The practice of baptising outside of the parish of birth seems to have taken place with some frequency in the Colyton area: for the total number of people studied, 4.7 per cent (50/80 x 7.5 per cent) were baptised outside Colyton even though they were born there.[15]

Wrigley has estimated that about 140 of the 1,4471 individuals who gave Colyton as their birthplace in 1851, were in actual fact born outside of the parish. This conclusion is reached in two ways: (i) the "gain error" ratio is applied to all those who claimed extra-Colyton birth in 1851 (1,032 cases) – and various complex estimated adjustments are made for "loss errors", migrations and deaths between census periods;[16] (ii) the number of people claiming Colyton birth yet baptised and residing elsewhere – estimated as numbering 41 people – is added to the 103 individuals claiming Colyton birth in 1851, who could not be found in either the Colyton baptism register or those of surrounding parishes.[17] We have already seen that the concepts of "gain" and "loss errors" are highly suspect, and the small amount of direct evidence that we have about them leads us to question their validity. This mode of calculating the number of census birthplace mis-statements is of a highly hypothetical character involving a number of speculative assumptions, leading to an inflation of

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[14] *Ibid*, p. 313.

[15] *Ibid*. See Table 8 on p. 309 and Table 11 on p. 313 for the source of these figures.

[16] *Ibid*, pp. 305, 306.

[17] *Ibid*, p. 314.

known inconsistencies from the order of 20 ("loss errors") and 35 ("gain errors") to a number four to seven times that magnitude (140).

The second method of calculating census errors is much more satisfactory than the first because it is based on direct empirical observation and involves a minimum of hypothetical assumption. This involves comparing census statements about birthplace with baptism register entry. There are two components to the estimate: the F cases (*F Case = Found in a baptism register outside Colyton*) and NF cases (*NF Case = Not found in any baptism register.*) I estimate that 41 F cases were census statement errors – a figure arrived at by multiplying the total number of F cases (107) by the proportion known to have been residing in Colyton, and subtracting this from the total. The figure can be checked by using civil registration data published by Wrigley at the end of his paper: 23 of 32 F cases of an age to be found in civil registers, were found to have been born in Colyton.[18] If we apply this ratio – 23/32 – to all the 107 F cases, we reach a total of only 30 born outside of Colyton. The conclusion must be that between 30 and 41 of these 107 cases were census birthplace statement errors.

The second component of the estimate is the NF cases – it was estimated by Wrigley that 103 of these cases were birthplace statement errors, but this figure is almost certainly incorrect. As a part of the civil registration cross-matching, the 103 NF cases were checked where appropriate in the local Colyton civil register; 57 of the 103 cases were under the age of nine in 1851, and 42 of them were found to have been born in Colyton itself.[19] In other words, 73.7 per cent (42 out of 57) of these NF cases were definitely born in Colyton, and as we shall see later, there is some reason to believe that even the remaining 26.3 per cent might not have been born outside Colyton. It is clearly invalid to add the 103 NF cases to the 30–41 F ones in arriving at an estimate of the number of 1851 Census birthplace mis-statements, as the majority of the former were almost certainly correct in claiming Colyton as their birthplace.

We may summarise the position on the actual birthplaces of the 1,471 people who claimed Colyton as their birthplace in the 1851 Census, as follows: (i) 79.1 per cent of them were found in the Colyton Anglican baptism register and can therefore be assumed to have also been born in

[18] *Ibid.*

[19] *Ibid.*

the parish;[20] (ii) 6.3 per cent were baptised in non-conformist Colyton registers and similarly can be assumed to have been Colyton-born; (iii) of the 7.5 per cent in the F category, between 4.7 per cent (calculated on a residence basis) and 5.4 per cent (calculated on a civil registration basis) were born in Colyton; (iv) the remaining 7.2 per cent not found in any baptism register can on conservative assumptions – taking the evidence from the civil register entries on the 57 NF cases in the 0–9 age category – be divided between 5.5 per cent (7.2 per cent × 42/57) born in Colyton and 1.7 per cent born elsewhere. If we total these figures, we find that between 95.6 and 96.3 per cent of people claiming Colyton birth in 1851, were actually born there. This is a conservative figure because it only counts someone as being Colyton-born where there is tangible evidence in support of an estimate; many of the NF cases not found in the Colyton civil register may have been born in Colyton, but have been excluded in calculating the appropriate percentage for this category.

It is possible to extend this mode of analysis by directly comparing census statements of birthplace with civil registration information for those born after the introduction of civil registration. Wrigley and his colleagues were unable to do this for Colyton in a complete and systematic fashion because of restricted access to civil registration returns. I have however been able to check the birth entries of 117 children listed in the 1851 Census as born in Colyton and aged twelve and under, the check being made in both the local area register for the district of Axminster and in the national index kept at St Catherine's House. Of these 117 children, 94 were traced in the civil register, 92 of them to the actual parish of Colyton, i.e. of those traced, 97.9 per cent (92 out of 94) were returned as being born in Colyton. At first sight it seems surprising that only 80.3 per cent (94 out of 117) could be traced to a civil register, but we will see later that there is large amount of evidence to suggest that civil registration was often deficient to this order during the early period of

[20] It is possible of course that some of these cases were born in neighbouring parishes and baptised in Colyton. This is unlikely given that they were returned in the 1851 Census as having been born in Colyton and were baptised in that parish. The assumption that they were born in Colyton could however be checked by analysing their parish of residence at the time of their baptism as stated in the baptism register. The same considerations apply to the 6.3 per cent of people found in the Colyton non-conformist registers.

civil registration.[21] Age is not an important factor in explaining the results (through, for example, people being born before the introduction of civil registration), indicated by the fact that the proportion traced for the 0–9 age category – 77.9 per cent (60 out of 77) – is even lower than that for the 10–12 age group, which was born according to the census nearer to the introduction of civil registration: 85.0 per cent (34 out of 40). It is possible of course that some matches were missed on account of name and age mis-statements in the census, but as we have already seen this is likely to have been only on a minor scale, not even allowing for the fact that a search of the Axminster district register was made with this in mind.

If these figures are at all representative of the Colyton population as a whole, birthplace mis-statements occurred only very rarely. There is some small discrepancy between the proportion of such errors calculated from Wrigley's data (3.7 – 4.4 per cent) and that from the direct census/civil register comparison (2.1 per cent). This can be explained by the inclusion of 1.7 per cent of the total population in the NF category in the born outside of Colyton group; evidence from work on civil registration would suggest that individuals are not always found in the civil register for the area in which they were born. Also the 117 cases in the census/ civil register cross match were all children and it is possible that older people may have had a higher percentage of birthplace errors.[22]

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[21] In the case of Colyton, an especially thorough search was made because of the importance of the parish in the debate about parish register reliability. The first 117 cases with information on parents and aged twelve and under were selected from available photocopies of the 1851 Census. These cases were then checked in the civil register for the Superintendent-Registrar's District of Axminster (which covers about half of the neighbouring parishes in which Wrigley and his colleagues found F cases), allowing for both name and age errors in the searching. Cases which could not be found were then checked in the national index kept at St Catherine's House, and traced back to original entries where appropriate.

[22] There is little to suggest this conclusion from Wrigley's paper: roughly the same proportion of people in the 0–9 and 10+ age groups were baptised in Colyton itself. See Table 8, *Ibid*, p. 309. However, there were more of such errors among the younger age group in the census/civil register sample, although the samples were rather small: there were 2 out of 32 (5.9 per cent) birthplace errors amongst the 10–12-year-olds, and none out of the 60 traced among the 0–9-year-olds. Also, see the later discussion in the text of this quotation of age and birthplace errors.

It is now possible to summarise the position on the criticisms of the original cross-matching methodology mentioned at the beginning of the paper. As far as Colyton is concerned, we have arrived at the following estimates of the proportion of errors in each category: (i) age and name errors would account for about 2.0 per cent of missed matched cases in the census/baptism register comparison; (ii) 4.7 per cent of the people claiming Colyton birth in 1851 were probably born in Colyton but baptised elsewhere; (iii) the percentage of census birthplace mis-statements ranges from 2.1 per cent through to 3.7–4.4 per cent, with the probability that the lower figure is more correct than the higher. The total proportion of errors from these three categories ranges from 8.9 per cent to 11.2 per cent, approximately a half of the percentage of N.I.R. cases found in Colyton. This obviously is a very significant proportion of the total. Two points however must be made about these conclusions on the quality of the Colyton data, before we consider evidence for a number of other parishes elsewhere: Colyton's N.I.R. ratio – 20.6 per cent – is significantly smaller than that of the average for the total 45 parishes in the original sample (31.0 per cent), and therefore a 10 per cent error ratio would only constitute about a third of the N.I.R. ratio for all parishes; the exaggeration of the number of N.I.R. cases through errors in the census must be counter balanced to some extent by N.I.R. cases missed on account of young infants dying before baptism. I estimated from data on the time interval between birth and baptism for 1,292 cases in the original census/baptism sample that about 5 per cent of young infants died before they had time to be baptised. Wrigley suggests an even higher figure and believes that it might be "necessary to inflate any baptism total by just over eight per cent"[23] on this account. This suggested correction would almost completely restore the validity of the N.I.R. ratio for Colyton on the lower estimate of error proportions (a total of 8.9 per cent), and only leave a gap of about 3 per cent on the higher one (11.2 per cent).

We can now turn to the results of the work done on cross-matching census, parish and civil registers for areas other than Colyton. This research was designed primarily to measure the extent of errors introduced into census/baptism register matching on account of: (i) census birthplace mis-statements; and (ii) the practice of baptising outside of the parish of

[23] *Ibid*, p. 315.

birth. Some work was done on census errors in age and name (particularly the latter), but this was viewed as secondary because of the existing evidence on the high reliability of this kind of information. Two sub-samples of parishes were selected from the list of 45 used in the census/baptism register work: the five parishes with the highest N.I.R. ratios; 19 parishes for which parish registers were available at the Genealogical Society's Library for the post-1837 period. The logic of the two stages of the research was as follows. If census mis-statement of birthplace was responsible for a large proportion of N.I.R. cases, those parishes with the highest proportions of N.I.R.s will provide the harshest test of census reliability with respect to birthplace; therefore the census documents of the five parishes with the greatest N.I.R. ratios (with an average of 47.6 per cent of cases not found in the baptism register) were cross-matched with civil register returns for children in the appropriate age range. Secondly in order to measure the extent of the practice of baptism outside of the parish of birth, as many parishes in the original sample with baptism registers comparable with civil register returns should be covered – the comparison yielding information on the child's parish of birth (from the civil register) and place of baptism (from the parish register). Obviously, results from this work on children and young people of an age to be found in civil registers, can not necessarily be generalised to other age groups without great care and further analysis. This and other problems with the census/parish/civil register match will be discussed in an appropriate context.

The five parishes with the highest N.I.R. ratios were Black-Torrington, Devon (44.0 per cent), Chigwell, Essex (42.9 per cent), Ringwood, Hants. (43.4 per cent), Hackney, Middlesex (58.0 per cent), and Kingston-on-Thames, Surrey (49.9 per cent). For each of these parishes, the first fifty cases were selected from both the 1851 and 1871 censuses, meeting the following criteria: (a) that they should be fully legible; (b) all should be stated as being born in the parish in question; (c) they should be of an age to be traceable in the civil registers – in the case of the 1851 Census from 0–8 years old, and the 1871 Census from 20–28 years old. The first criteria is self-explanatory. The second was adopted so that the information on birthplace used in the census/baptism register match (that on "native" parishes) was being directly evaluated. The third was designed to allow a search in the civil register within the range of at least plus or minus three years of the expected age from the census, the constraint being on the upper age limit so as to allow for three years after the introduction of civil

registration.[24] The 1871 Census was included along with that from 1851, so as to enable a check on birthplace statements of people in an older age range. It is accepted that findings from the 1871 Census cannot necessarily be generalised to the 1851 one, but it does provide an additional source of information with which to evaluate the latter. A search was made in both the civil register for the whole of the local Superintendent-Registrar's District area in which the parish was located, and for cases which could not be found there, in the national index at St Catherine's House.[25]

Table 1. The Cross-Matching of Census and Civil Register Information on Children Listed as Natives in the 1851 and 1871 Censuses

Parish	*Date of Census*	*Number in Sample*	*Number Traced in Civil Register*	*Percentage of Sample Traced*	*Number Born in Parish*	*Percentage Born in Parish*
Kingston	1851	50	38	76.0%	38	100%
Kingston	1871	50	42	84.0%	42	100%
Ringwood	1851	50	47	94.0%	47	100%
Ringwood	1871	50	46	92.0%	46	100%
Chigwell	1851	50	49	98.0%	49	100%
Chigwell	1871	50	45	90.0%	43	95.6%
Hackney	1851	50	40	80.0%	40	100%
Hackney	1871	50	42	84.0%	38	90.5%
Black-Torrington	1851	50	47	94.0%	47	100%
Black-Torrington	1871	50	42	84.0%	41	97.6%
Total		500	438	87.6%	431	98.4%

The most striking finding from Table 1 is that 98.4 per cent of cases traced in the civil registers were born in the parish stated to be their birthplace in the 1851 and 1871 censuses. In seven of the ten samples, the proportion born in

[24] Ideally, a search should have been made for an even earlier period, but this kind of cross-matching of data is extremely laborious and the evidence anyway is that about 98.0 per cent of all census statements were accurate to within plus or minus three years. (See Wrigley, *op. cit*, p. 303 and Razzell, *op. cit.*, p. 126).

[25] Wrigley found 58 of the 107 F cases baptised outside of Colyton in parishes within the Superintendent-Registrar's District of Axminster and a direct search of these local civil registers allows cases with name and other errors to be picked up, where they might be missed in the national register via an index.

the stated birthplace was 100 per cent and it was in only three of the samples – Chigwell 1871, Hackney 1871 and Black-Torrington 1871 – that there were any cases found in the civil register outside of their stated birthplace. In all, there were only seven cases in this category, and four of those were in the Hackney 1871 sample. If we compare the samples taken from the 1851 census with those taken from 1871, the overall proportions of cases found outside their expected birthplace are none for the former, and 3.2 per cent (7 out of 216) for the latter. It is possible that the 1871 Census was more deficient than the 1851 one, but there is no reason or evidence to suggest such a conclusion. More likely is the effect of age on accuracy of birthplace statement: the children selected from the 1851 Census were all aged eight or under, whereas the people chosen from the 1871 Census were aged between 20 and 28. Yet this effect of age on census accuracy appears to be limited; both Wrigley and I found that age had little effect on the accuracy of census age statements.[26]

This finding that census birthplace statements were of a high level of accuracy applies to people under the age of 28. What is the evidence that it also applies to older age groups? In my original paper, I gave evidence on the relationship between stated age in the 1851 Census and birthplace statement discrepancies between the 1851 and 1861 censuses. Although birthplace comparisons were only made for people listed as being born in "native" parishes in 1851, the results are surprisingly compatible with those derived from census/civil register matching.

Table 2. 1851 Census Age and Birthplace Statement Disagreements Between the 1851 and 1861 Census. (45 Parishes)

	Age in the 1851 Census						
	17–20	*21–30*	*31–40*	*41–50*	*51–60*	*61–90*	*Total*
Birthplace Statement Disagreements (B.S.D.s)	1	13	17	9	6	2	48
Total Cases	128	420	400	338	214	90	1,585
% B.S.D.s	0.8	3.1	4.2	2.7	2.8	2.2	3.0

The similarity between the findings in the above Table for the age groups under thirty and the results from the census/civil register comparison quoted in Table 1 is partly fortuitous, as the two Tables refer to different

[26] Wrigley, *op. cit.*, pp. 303, 304; Razzell, *op. cit.*, p. 12.

samples and the second Table compounds errors between the two censuses. However, the similarity in findings gives some confirmation to the validity of Table 2, and the results of that Table indicate no correlation between census age and mis-statement of birthplace.[27]

The overall findings of Table 1 reveal a very high level of census accuracy on birthplace statement. Only 1.6 per cent of all cases traced were born outside of expected birthplace, which can be contrasted with the 47.6 per cent of cases not found in the baptism registers of the five parishes in the sample. If we confine the comparison to the age groups involved in the census/civil register match we come to an identical conclusion: the proportion of 20–29 year olds enumerated in the 1871 Census born outside of expected birthplace was 3.2 per cent, which can be set against the 30.0 per cent of N.I.R. cases among the same age group enumerated in 1851.[28] If we compare individual parishes, the contrast becomes even starker: the N.I.R. proportions for Ringwood and Kingston in the 1851 20–29 age group (these were the only two parishes with large enough samples for analysis) were 46.9 per cent and 52.6 per cent; the equivalent proportion of people born outside the two parishes for the same age group checked from the 1871 Census to the civil birth register, were zero per cent in both cases. Of course these comparisons are making the assumption that the 1871 20–29 age group is equivalent to the same age group in 1851; although this assumption cannot be tested, there is no reason to believe that the two censuses were greatly different in their overall level of accuracy, and any such difference would have to be massive to affect the conclusions drawn from the above data, i.e. that the very high proportion of N.I.R. cases found for the original sample cannot be a function of census inaccuracy of birthplace statement.

It probably has not escaped the reader's attention that there is one feature of Table 1 which could undermine the conclusions drawn from it. Of the 500 cases selected from the 1851 and 1871 censuses, only 438 (87.6 per cent) could be traced in the civil register for the appropriate period.

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[27] Although there are difficulties with Wrigley's evidence on birthplace, the information that he does give on the characteristics of various age groups also supports this conclusion. See Wrigley, *op. cit.*, p. 309.

[28] There is the problem with phonetical name variations, but as a very careful search was made in the local civil register, as well as a one-year search in the national index, this is not likely to result in many missed cases.

There are a number of possible explanations of these missing cases, all of which can be evaluated through further work: (i) census birthplace statement inaccuracies could make a detailed search of relevant civil register difficult; (ii) problems of name variation could lead to missing cases; (iii) census age inaccuracies might lead to people being missed through an insufficient time search; (iv) there could be deficiencies in civil registration during the 1840s. I will discuss these four topics in the above sequence, and in the process of analysing the relevant data, will attempt to deal with substantive points which also have a bearing on the census/parish register comparison.

The procedure adopted for checking census birthplace statements in the civil register was as follows. An entry was initially searched for in the local Superintendent-Registrar District's register for a seven-year period, centring on the expected year of birth as listed in the census. An inspection of all entries in the civil register for this seven-year period was made, until a satisfactory match could be established. Most of the cases taken from the 1851 census had information on parents available, which facilitated the matching process and ensured that the quality of the match was very high: 96.8 per cent of the 217 cases traced had matching information on a least one parent. Even with the 1871 census, a majority of cases had parental information – 56.5 per cent (122 out of 216) – and the over-all figure for the whole sample was 76.7 per cent (332 out of 433). The minimal criteria for establishing a match was that a person of the correct name had to be found in the civil register for the seven-year period (plus or minus 3 years from expected year of birth).

In practice nearly all the cases found were traced in this way, as the Superintendent-Registrar's district included many of the neighbouring parishes where one would expect to find cases born outside of the parish. When a person could not be found in this local civil register, a very detailed search was then made in the National Index kept at St. Catherine's House, and this was undertaken for the 46 cases with parental information and confined to expected year of birth. The search was restricted in this way for three reasons: (i) it is impossible to make an adequate match for cases without parental information outside expected area of birth; (ii) a search of the national index is extremely laborious, as it contains many individuals of the same name, particularly where the person in question has a common surname; (iii) evidence to be discussed later in the paper indicates that 85.6 per cent of matched 1851 census cases, and 69.0 per cent of 1871 ones, were found in their expected year of birth. Given these restrictions on the searching process, it is obviously necessary to inflate those cases

found in the national index by two ratios in order to correct for the restrictions: (a) multiply the cases found by 63/43 in order to allow for only 43 of the 63 people not found in the local civil register having parental information; (b) and by 215/184 to allow for the restricted one-year search for the 1851 cases, and 216/149 for the 1871 ones. In fact, these complicated adjustments are more-or-less irrelevant as only one case out of a total sample of 500 was found outside of the local registration district in the national index, in their expected year of birth. Applying the above adjustments to this figure, gives approximately one case which should be added to the number of matches, i.e. a figure of 0.2 per cent. This proportion reduces the 12.4 per cent of 1851 and 1871 Census cases not found in the civil register to 12.2 per cent.

We have already discussed the problem of census name inaccuracies in the analysis of the Colyton data. The census/civil register comparison material does not lend itself to this kind of work, as necessarily cases with significant name variations might be excluded from the sample. We must anticipate research to be discussed later in the paper to find data relevant to this topic. In order to evaluate the extent of people baptising their children outside of the parish of birth, a sample of 2,042 entries in 19 baptism registers was compared with equivalent civil register entries in the period 1838–53. Three of these baptism registers give date of birth as well as date of baptism, which along with information on names of parents and father's occupation, make it possible to make links even in cases of extreme name variation. Four hundred and thirty-three cases were studied in these three parishes (Iver, Mednenham and Kingston-on-Thames) and the following is a list of name variations which probably would cause matching difficulties:

Baptism Register Entry	*Civil Register Entry*
John Ottoway	Charles Otway
George James Beard	George James Thompson
Charlotte Winterham	Charlotte Winterbourne
Joseph Grimsann	Joseph Gainshire
William Burryn	William Berry
Lydia Bance	Lydia Barnes
Phoebe Bance	Rhoda Barnes
Edward Moore	Edwin Moore
Richard Bance	Richard Barnes
Emily Bance	Emily Barnes

In all, there were ten name variations that probably would have led to a missed match, i.e. 2.3 per cent of the cases in the sample. Five of these

ten cases however, referred to one family (Bance/Barnes), and are almost certainly the result of a census copying error. Such an error which affects a whole family can be relatively easily spotted because of the repetition of relevant information. An example of this is to be found in the census/civil register comparison for the 1851 census Hackney sample: after searching the local civil register, it was discovered that the three members of the Hebbin family were in fact registered under the name of Stebbing. If we exclude the Bance/Barnes family from the list of name variations, we are left with five cases out of a total of 438 –1.2 per cent – exactly the same proportion as found by Wrigley in Colyton. There were quite a few additional cases of name variation in the sample, but they were all of the phonetical kind that are easily linkable, e.g. Truby/Trubee, Sherwell/Shurvell, Perce/Pearce and Caywood/Keywood.[29] Taking the figure of 1.2 per cent as the most accurate proportion of cases likely to be unmatched on account of name variation, we must reduce the percentage of census cases not found in the civil register from 12.2 to 11.0 per cent.

The question of accuracy of census age statements can be examined within certain limits by a direct analysis of census/civil register matching information. as all but one of the 436 cases found in the civil register were discovered through the seven-year (0 plus or minus 3) local research, the degree of age accuracy can be revealed within those limits.

Table 3. Distribution of Expected Age (Census), Minus Actual Age (Civil Registration), 5 Parishes

	Years							
	–3	*–2*	*–1*	*0*	*+1*	*+2*	*+3*	*Total*
1851 Census	1	1	3	188	24	2	0	219
	0.5%	0.5%	1.4%	85.8%	11.0%	0.9%	0%	100.1%
1871 Census	6	7	26	150	24	3	1	217
	2.8%	3.2%	12.0%	69.0%	11.1%	1.4%	0.5%	100%
Total (1851 and	7	8	29	338	48	5	1	436
1871 Censuses)	1.6%	1.8%	6.7%	77.5%	11.0%	1.1%	0.2%	99.9%

The sample of 0–8 year-olds selected from the 1851 Census matched

[29] The only problem with phonetical name variations is when the search is made by index rather than by direct register examination. Every attempt was made to allow for phonetical variation during the one-year search of the national index.

extremely well with respect to their stated age and date of birth. The only substantial error category was the overstatement of age by one year and this appears to have been a function of parents rounding up an age of a child whose next birthday was soon after the date of the census. The sample of 20–28 year-olds taken from the 1871 census was less satisfactory, although even there the majority (92.1%) of cases was to be found in the narrow band of plus or minus one year of expected birth. The question arises as to what proportion of cases could have been found outside of the band of plus or minus three years of expected year of birth if a sufficiently wide search had have been conducted? One way of arriving at an estimate of this figure is to compare the data from the above Table with that from Wrigley's paper on Colyton.[30]

Table 4. Distribution of Age Matches and Errors, 5 Parishes and Colyton

	Years						
	0	*± 1*	*± 2*	*± 3*	*± 4*	*± 5*	*± 6*
5 Parishes	338	77	13	8	—	—	—
(1851 & 1871)	77.5%	17.7%	2.9%	1.8%			
Colyton	594	173	30	6	4	2	4
	73.1%	21.3%	3.7%	0.7%	0.5%	0.2%	0.5%

The two distributions up to the plus or minus three-year category are similar, particularly when it is remembered that the Colyton figures are based on a comparison of census ages with baptism and not birth dates. If we combine the 0 and plus or minus one-year categories so as to allow for the inaccuracy of Colyton baptism dates, we find that 95.2 per cent of the cases in the five parishes were accurate to within plus or minus one year, compared to 94.4 per cent of the Colyton ones. We would expect the Colyton figure to be smaller as 1.2 per cent of its cases were found outside of the plus or minus three year band. These figures are sufficiently close, to warrant taking the Colyton data on the larger age errors as being representative of the five parishes as well. Thus we should further reduce

[30] I have taken the figures for single men and single women from the Colyton data, as these were most comparable from an age point of view with the information on the five parishes. See Wrigley, *op. cit.*, p. 303.

the 11.0 per cent of census cases not found in the civil register on account of age errors in the census by 1.2 per cent, giving a figure of 9.8 per cent.

We have now reached a final figure for the percentage of census cases not found in the civil register. In order for the census/civil register method to be valid, it is necessary to accept that 9.8 per cent of the cases selected from the 1851 and 1871 censuses for the five parishes of Kingston, Hackney, Black-Torrington, Chigwell and Ringwood, were not registered under civil registration. D. V. Glass published estimates of birth under-registration during this period for England and Wales, derived from a comparison of 1851 Census age returns with national civil register data on births and deaths.[31] He estimates that 8.6 per cent of all births were omitted from civil registration during the period 1841–45, and 6.0 per cent during the following five years, 1846–50.[32] Glass wrote on the subject that "the estimates very probably understate the deficiencies of birth registration",[33] so it is clear that the figure of 9.8 per cent of census cases not found in the civil register is compatible with this independent estimate of under-registration.

In addition to this independent confirmation of the scale of civil under-registration, there is direct evidence from the research involving the comparison of baptism entries with civil registers that leads to the same conclusion. Of the 2,042 cases taken from the 19 baptism registers, 281 could not be found in the civil registers – representing a proportion of 14.3 per cent. A search was made in both the local Superintendent-Registrar's district register, and where cases could not be found locally, in the national index. A check was always made of the yearly quarter in which the baptism took place, and the preceding quarter. A complication arises with this data because of the known delay between birth and baptism, but in three of the parishes, the baptism register gives the date of birth – enabling a very precise search of the civil birth register.

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[31] D. V. Glass, "A note on the under-registration of births in Britain in the nineteenth Century", *Population Studies* (1951); D. V. Glass, "Population movements in England and Wales, 1700 to 1850", Glass and Eversley (eds.) *Population In History* (1965); and "Vital registration in Britain during the nineteenth century" (Appendix 4), D. V. Glass, *Numbering The People* (1973), pp. 181–205.

[32] D. V. Glass, *Numbering The People*, p. 182.

[33] *Ibid*, p. 181.

Table 5. Comparison of Baptism Register Entries With Civil Birth Register Information (3 Parishes)[34]

Parish	*Total Cases in Sample*	*Cases Found in Civil Register*	*Percentage Found in Civil Register*
Iver, Bucks.	100	74	74%
Medmenham, Bucks.	200	192	96%
Kingston, Surrey	200	160	80%
Total	500	426	85.2%

The proportion of cases found varied between 74 and 96 percent, with an average for the total 500 cases of 85.2 per cent. The 14.8 per cent of cases not found could not be reduced, even though the yearly quarter in which the births took place were searched twice in both the local civil registers and in the national index, as were the preceding and following quarters. Also, in the case of the parish of Iver, information on place of residence at time of baptism was available in the parish register, and in every one of the 26 cases not found in the civil register,the parish of residence was Iver itself.

Enough has been said to indicate that the proportion of cases not found in the census/civil register match is not a function of the cross-matching methodology, but is a genuine finding about civil under-registration. We have been forced to diverge widely from the main discussion of the census/baptism register matching process, and in the analysis of the reasons for civil under-registration, a number of points relevant to the evaluation of the census/baptism comparison method were made but not related to the mainstream of the discussion. I will therefore summarise the main relevant findings from this point of view which have now emerged. (1) Of 438 cases selected from 1851 and 1871 Census documents for five parishes, 431 of them (98.4 per cent) were found through civil registration to have been born in their expected parish of birth as defined by the census. (2) An estimate has been made on the basis of baptism/civil register data that 1.2 per cent of names varied sufficiently to lead to missed matches in

[34] The cases were selected as follows: from 1 January 1838, the first hundred baptisms from the Iver Register and first one hundred cases from Medmenham and Kingston; from 1 January 1845, the first one hundred baptisms from the Medmenham and Kingston registers.

the census/baptism comparison process. (3) On the basis of census/civil register data for five parishes and the information on Colyton on census age statement accuracy, it was estimated that 1.2 per cent of age statements were inaccurate beyond the plus or minus 3 year level (the proportion of age errors relevant to the census/baptism comparison would be smaller as the criteria allowed for a plus or minus 5 year error).

The above covers two of the three main criticisms made of the census/baptism register cross-matching methodology, leaving the argument that it failed to take into account the practice of parents baptising their children outside of the parish of birth. In order to evaluate this difficulty, 19 of the original 45 parish registers in the census/baptism matching sample were selected on the basis of having available registers with post–1837 information on baptism, allowing cross-matching with civil registration. Data yielded from this research gives a precise measure of the extent of baptising outside the parish of birth during the late 1830s and 1840s. Table 6 outlines the main body of findings from this work

Table 6. The Evaluation of the Extent of Baptism Outside the Parish of Birth, Through the Cross-Matching of Baptism and Civil Registration Data

Parish	*Period*	*Total Cases*	*Cases Found*	*Born in Parish of Baptism*	*Percentage of Cases Born in Parish of Baptism*
Muker, Yorks.	1838–40	100	100	97	97.0%
Purleigh, Essex	1845–48	100	88	86	97.7%
Barton-Hartshorn, Bucks	1838–40	13	12	12	100.0%
Preston-Bisset, Bucks.	1838–40	67	63	61	96.8%
Horton, Bucks.	1845–52	100	80	76	95.0%
Eton, Bucks.	1845–46	100	77	75	97.4%
Bramfield, Suffolk	1838–43	100	91	91	100.0%
Chetwode, Bucks.	1838–40	19	19	17	89.5%
Lapf'ord, Devon	1845–50	100	91	89	97.8%
Benenden, Kent	1838–40	100	94	93	98.9%
Old Malton, Yorks.	1845–48	100	95	94	98.9%
Chardstock, Dorset	1845–48	100	67	66	98.5%
Chigwell, Essex	1838–40	100	79	77	97.5%
Old Malton, Yorks.	1837–38	43	42	42	100.0%
Medmenham, Bucks.	1838–46	100	95	93	97.9%
Ringwood, Hants.	1838–39	100	86	85	98.8%
Bramfield, Suffolk	1845–5C	100	74	74	100.0%

Parish	*Period*	*Total Cases*	*Cases Found*	*Born in Parish of Baptism*	*Percentage of Cases Born in Parish of Baptism*
Langley-Marish, Bucks.	1838–40	100	75	68	90.6%
Iver, Bucks.	1838–39	100	74	70	94.6%
Kingston, Surrey	1838	100	77	76	98.7%
Medmenham, Bucks.	1845–53	100	97	95	97.9%
Kingston, Surrey	1845	100	83	80	96.4%
Fordingbridge, Hants.	1838–39	100	94	91	96.8%
Total		2,042	1,751	1,708	97.5%

The proportion of cases baptised in their parish of birth varied from 90.6 to 100 per cent, with a total average for the whole sample of 97.5 per cent. Nearly all of the 2.5 per cent of baptisms which occurred outside the parish of birth were in areas immediately adjoining the birthplace, 37 of the 43 cases falling into this category. Even the six exceptions tended to be found in the same regional area: Muker/Middlesborough, Medenham/Reading, Iver/ St. John's London, Kingston/Guildford, Kingston/Hillingdon, Kingston/Gt Portland St London).

Again the question is raised whether the very high proportion of cases born in the parish of baptism is an artefact of the problem of locating cases born outside of the parish in question. I have dealt with this possibility in the discussion of civil registration, where it was concluded that the failure to trace cases was a genuine function of under-registration. In the case of the baptism/civil registration match, additional information is available on the not found cases which confirms this conclusion. For three of the parishes in the sample – Old Malton, Chigwell and Chardstock – 27 cases were traced to the 1851 Census and their stated birth-place was examined: 26 of them were listed as having been born in their parish of baptism, a proportion of 96.3 per cent. For six parishes – Fordingbridge, Old Malton, Lapford, Iver, Horton and Ringwood – systematic evidence on the parish of residence at time of baptism was available in the parish register; 69 out of 75 not found cases were resident in their parish of baptism (92.0 per cent) at the time of baptism. Although this proportion is 5.5 per cent below that for the found cases, the size and nature of the samples is greatly different, and the important point is that the vast majority of these not found cases were resident in their parish of baptism. (If we allocated the not found cases to the categories of "born in parish of baptism" and "born outside parish of baptism" on the ratio of these residence findings, the

proportion of cases born outside the parish of baptism would rise by only 0.2 per cent).

This data on baptism/civil register patterns is limited to the post-civil registration period, whereas the main interest of the census/baptism register comparison lies in the several decades immediately previous to this. There is evidence to suggest however, that a majority of children were baptised outside their parish of birth because as a result of moving from one parish to another, and information on residence is systematically given in many baptism registers after 1812 with the introduction of Rose's Act. Of the 43 cases born outside the parish of baptism, 23 (53.5 per cent) were listed as residing outside of the parish of baptism. The parish register does not always give information on residence, so it is possible that more cases were in this position than is indicated by these figures, but if we assume that listed residence gives us over a half of the cases born outside of the parish of baptism, we can analyse birth/baptism patterns as far back as at least 1813.

Table 7. Patterns of Residence in Relation to Parish of Baptism, 1813–52

Period	*1813–22*	*1823–32*	*1833–42*	*1843–52*	*Total*
Total Cases	3,261	3,721	3,413	1,908	12,303
Cases Residing Outside of Parish at the Time of Baptism	92 (2.8%)	105 (2.8%)	51 (1.5%)	25 (1.3%)	273 (2.2%)

In order to translate the percentages of cases residing outside the parish of baptism into proportions of children born outside the parish of baptism, it is necessary to inflate them by the ratio of 43/23. This gives the following proportions: 1813–22 – 5.2 per cent; 1823–32 – 5.2 per cent; 1833–42 – 2.8 per cent; 1843–52 – 2.4 per cent. The latter two percentages confirm the validity of this inflation procedure as they refer to the period of the baptism/civil register comparison (1838–53) when 2.5 per cent of children were born outside their parish of baptism. The figures derived from Table 7 reveal a distinct trend, with the proportion of children born outside their parish of baptism approximately halving between the 1810s and the 1840s: from 5.2 per cent to 2.4 per cent.

However, the above figures conceal significant variations between the eleven parishes included in the residence sample, and as we shall see, one of them contains over half of the cases of children born outside their parish of baptism.

Table 8. Patterns of Residence in Relation to Parish of Baptism

Parish	*Total Cases*	*Cases Residing Outside Parish at Time of Baptism*
Old Malton, 1813–1850	1,238	13 (1.6%)
Kingston, 1813–1847	3,387	139 (4.1%)
Benenden, 1813–40	1,380	3 (0.2%)
Lapford, 1813–49	755	33 (4.4%)
Eton, 1813–49	2,574	8 (0.3%)
Medmenham, 1813–50	501	16 (3.2%)
Langley-Marish, 1813–40	1,049	33 (3.1%)
Preston-Bisset, 1813–40	522	20 (3.8%)
Chetwode, 1813–40	128	2 (1.8%)
Harton-Hartshorn, 1813–40	110	2 (1.8%)
Bramfield, 1813–40	659	4 (0.6%)
Total	12,303	273 (2.2%)

The percentage of cases residing outside the parish of baptism varies from a low of 0.2 per cent, to a high of 4.4 per cent. The second highest proportion is for Kingston, and although it contains 27.5 per cent of the total sample, it accounts for 50.9 per cent of the "outside parish birth" cases. The total samples were selected on the availability of information on residence (although the really large parishes like Kingston were reduced by half so as to not completely overbalance the sample). If we analyse changes in birth/baptism residence patterns over time for Kingston as against the rest of the sample, the results are as follows:

Table 9. Patterns of Residence in Relation to Parish of Baptism, Kingston Compared to Rest of Sample

	1813–22	*1823–32*	*1833–42*	*1843–52*	*Total*
Kingston					
Total Cases	824	902	897	764	3,387
Cases Residing Outside of Parish at Time of Baptism	45 (5.5%)	64 (7.1%)	13 (1.4%)	17 (2.2%)	139 (4.1%)
Rest of Sample (10 Parishes)					
Total Cases	2,436	2,818	2,515	1,147	8,916
Cases Residing Outside of Parish at Time of Baptism	46 (1.9%)	41 (1.5%)	39 (1.5%)	8 (0.7%)	134 (1.5%)

Not only is the trend much less distinct in these figures, but the absolute level of cases born outside the parish of baptism is lower. If we apply the inflation ratio to the sample excluding Kingston, we arrive at the following estimate of proportions of children born outside of their parish of baptism: 1813–22 – 3.6 per cent; 1823–32 – 2.8 per cent; 1833–42 – 3.0 per cent 1843–52 – 1.3 per cent. The latter estimates still appear to be soundly based: the average of the residence figures for 1833–52 – 47 cases out of 3,662 (1.3 per cent) – inflates to 2.5 per cent when multiplied by 43/23, exactly the same figure of cases born outside of the parish of baptism in the baptism/civil register sample during the same period. Applying the same inflation ratio to the total of the rest of the sample we arrive at a figure of 2.9 per cent of cases born outside of their parish of baptism. These estimates suggest that up to the 1840s, there was an even plateau in the proportion of children born outside of their parish of baptism, with something of a slump after 1842. The results of the Kingston data indicate a different pattern: a slight increase during the 1820s, followed by a very sharp fall in the 1830s and 1840s.

We are now in a position to summarise the findings of this paper with reference to an evaluation of the validity of the census/baptism matching methodology, and will compare the results of the discussion of the Colyton data with that of the census/civil register and baptism/ civil register research material. The summary can be grouped under the three critical headings outlined at the beginning of this paper:

1.a. The percentage of 1851 Census birthplace statement errors for Colyton ranges from 2.1 per cent to 3.7–4.4 per cent, with the probability that the lower figure is more correct than the higher one.

1.b. Census birthplace errors as measured by the census/ civil register comparison method for the five parishes of Kingston, Ringwood, Chigwell, Hackney and Black-Torrington (the parishes with the highest N.I.R. ratios), were zero for the 1851 Census and 3.2 per cent for the 1871 one. There is evidence to suggest that the latter figure is more representative than the former of most age groups in the 1851 Census.

2.a. The Colyton data suggests that 0.9 per cent of census age statements were inaccurate by more than plus or minus 5 years; it has been estimated that name variations would lead to 1.2 per cent of correct matches being missed.

2.b. The census/civil register comparison indicates (along with the Colyton data) that 1.2 per cent of census age statements for the five parishes were inaccurate by more than plus or minus 3 years; from the study of 433 cases in three baptism registers which give date of birth, it was estimated that 1.2 per cent of correct matches would have been missed through name variation.

3.a. Of the people claiming Colyton birth in the 1851 Census, 4.7 per cent were probably born in Colyton but baptised elsewhere.

3.b. Of 2,042 baptism entries from 19 parish registers cross-matched with civil register information, 2.5 per cent were born outside their parish of baptism. Information on residence patterns suggests that for a sub-sample of ten parishes, this proportion of 2.5 per cent for the post-civil registration period ought to be increased to 2.9 per cent to cover the forty-year period 1813–1852. The parish of Kingston appears to be exceptional in its very high proportion of children born outside the parish of baptism as indicated by information on residence: a total of 4.1 per cent of cases residing outside the parish of baptism in the period 1813–1852 indicating a proportion of 7.7 per cent of children born outside of the parish. If we aggregate Kingston with the other ten parishes in the residence sample, we arrive at a total figure of 2.2 per cent of "outside residence" cases, suggesting a figure of 4.1 per cent of children born outside of their parish of baptism during the period 1813–1852.

It should be clear to the reader that the above summary indicates a general agreement between the Colyton findings as analysed in this paper, and the results of the census/civil register and baptism/civil register comparisons. The average of the three percentages of birthplace statement

errors mentioned for Colyton is 3.4 per cent and because such an average tends to overweight the higher figures, the 3.2 per cent figure emerging from the census/civil register work can be seen as a high point of convergence. There is precise agreement on the proportion of missed matches on account of name variance – 1.2 per cent – and the number of age statement errors: 1.2 per cent of statements out by more than three years, and following the Colyton data, 0.9 per cent out by more than five years (which was the age boundary of a match in the census/baptism comparison method). There is somewhat less agreement in the figures on children baptised outside of the parish of birth: the 4.7 per cent Colyton figure is significantly higher than the 2.5 per cent figure for the 19 parishes in the post-civil registration period, and also higher than the estimate of 2.9 per cent for the ten parishes during 1813–52, or even than the 4.0 per cent for the whole of the eleven parishes including Kingston. The latter figure does however, represent the best estimate for the whole sample studied, and therefore again represents a point (although a high one) of convergence.

What are the implications of the above findings for the census/baptism register comparison method? The proportion in the linked baptism register was in the original study 31.0 per cent. If we take the above figures which bring together the conclusions of the Colyton and census/ civil register/baptism studies, we would have to subtract 9.3 per cent (3.2 + 1.2 + 0.9 + 4.0) from the 31.0 in order to correct for deficiencies in the census and other problems. However, as has already been mentioned in discussing the Colyton results, this makes no allowance for under-registration on account of infant mortality before baptism. The three parish registers with birthdate information – Kingston, Iver and Medmenham – can yield data on birth/baptism delays during the post-civil registration period when detailed information on infant mortality is available.

Table 10. Birth/Baptism Delay in Kingston, Iver and Medmenham, 1838–1855

	0–6 days	*7–13 days*	*14–22 days*	*23–29 days*	*30 days*	*2–3 months*	*3–6 months*	*6–12 months*	*1 Year +*
Number	9	5	40	123	209	57	45	8	20
Percentage	1.75%	1.0%	7.8%	23.8%	40.5%	11.0%	8.7%	1.6%	3.9%

The *Registrar-General's Eight Annual Report* (1845), gives a monthly breakdown of deaths of infants under the age of one for the period 1839–44, but this is insufficient for our purposes as much of infant

mortality is known to be concentrated in the first days of life. The earliest information giving this level of detail for England and Wales is for the year 1905. If we compare the distribution of infant mortality between the two periods, we obtain the following Table:

Table 11. Monthly Percentage Distributions of Deaths of Infants Under 1 Year of Age[35]

	Under 1 month	*1–2 months*	*2–3 months*	*3–6 months*	*6–9 months*	*9–12 months*
England, 1839–44	30.9%	11.5%	8.1%	19.1%	15.7%	14.7%
England & Wales, 1905	32.6%	10.9%	8.5%	19.3%	15.6%	13.1%

The two distributions are very similar, and even the absolute levels of infant mortality in the two periods are not greatly different: 150.1 per 1,000 in 1839–44, and 128.2 per 1,000 in 1905. In order to use the detailed information in the 1905 data, I have taken its distributions but inflated them by the ratio 150.1/128.2 in order to correct for these differences in absolute levels. The results of these calculations are contained in Table 12.

Table 12. Estimated Mortality Rates for Infants Under 1 Year in England, 1839–1844

	Under 1 week	*1–2 weeks*	*2–3 weeks*	*3–4 weeks*	*1–2 months*	*2–3 months*	*3–6 months*	*6–12 months*
Mortality Rate Per 1,000 Births	29.5	7.1	6.9	5.4	16.3	12.8	29.0	21.6

If we apply these rates to the birth/baptism data contained in Table 10, and make the conservative assumption that none of the percentage groups in the various time periods were exposed to the mortality rates within the periods,[36] we arrive at the following Table of the numbers dying:

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[35] For the source of the data in this Table see the *Registrar-General Eighth Annual Report* (1845), pp. 187, 268; the *Registrar-General's Sixty-Eighth Annual Report* (1905).

[36] Other assumptions about an even distribution of mortality within time periods would only have marginal effects on the estimates, as mortality tends to be concentrated in the first few weeks when the vast majority of children remain unbaptised.

Table 13. Estimated Mortality Rates of Infants Dying Before Baptism, 1839–44

	Under 1 wk	*1–2 wks*	*2–3 wks*	*3–4 wks*	*1–2 mths*	*2–3 mths*	*3–6 mths*	*6–12 mths*	*1 Yr +*	*Total*
Proportion of Total Born Exposed to Mortality before Baptism	98.3%	97.3%	89.5%	65.7%	25.2%	14.2%	5.5%	3.9%	2.9%	
Mortality Rate per 1,000 Born	29.0	6.9	6.2	3.5	4.1	1.8	1.6	0.8	0.6	54.5

The total estimated mortality of infants dying before baptism in the period 1839–44 is 54.5 per 1,000 born, i.e. 5.45 per cent. Can we assume that the same proportion of under-registered births on account of infant mortality before baptism in pre-civil registration periods? In my first paper on the census/baptism comparison method, I published distributions of birth/baptism delays on 311 cases in the sample of 45 parishes where this was available (covering the period 1760–1834). If we apply the estimated mortality rates contained in Table 13 to this distribution, the total estimated mortality of infants dying before baptism drops to 51.4 per 1,000 (5.14 per cent). This slight drop occurs in spite of a significantly smaller birth/baptism delay in the pre-civil registration era, and this is because much of infant mortality is concentrated in the first few days after birth which was not affected by the shortening of the interval between birth and baptism. A higher overall level of infant mortality would of course have a significant effect on the proportion of under-registered infants, but this should not be exaggerated. Even if the infant mortality rate was as high as 250 per 1,000 in the earlier period, the proportion of infants dying before baptism would only be of the order of 8.6 per cent, i.e. an increase of 3.5 over the estimated 5.1 per cent unregistered during 1760–1834.

We can therefore estimate that between 5.1 per cent and 8.6 per cent of unregistered births omitted because of infant mortality before baptism, ought to be added onto the general N.I.R. ratio, but this is not the only positive correction that ought to be made to the N.I.R. rate: D. C. Levine pointed out in a paper on the census/baptism register comparison method that as formulated in the first paper it risked "crediting a successful cross-match when the baptism ... used referred to someone who had

subsequently died".[37] This problem of identifying a correct match was dealt with in the first paper by using independent information on parents names, although the actual data derived on parents was not used to make corrections to the N.I.R. ratio, because of a desire to standardise the cross-matching procedure. If we make these corrections, it becomes necessary to add a net figure of 3.4 per cent to the overall N.I.R. rate. The additions on account of infant mortality before baptism (5.1–8.6 per cent) and over-matching of incorrectly identified cases (3.4 per cent), in effect balance out the 9.3 per cent of cases not found in baptism registers on account of census and other errors.

The conclusions of this paper are therefore as follows: (i) the 1851 Census is very reliable, with birthplace errors for Colyton and Kingston, Black-Torrington, Chigwell, Hackney and Ringwood no higher than 3.2 per cent of the total, and significant name variations and age errors greater than 5 years (the N.I.R. criteria) of the order of 1.2 per cent and 0.9 per cent respectively; (ii) the practice of baptising outside of the parish of birth in 19 parishes selected from the original sample of 45 was very limited: only 2.5 per cent of all children baptised in the parish were born outside of them during the 1840s, and it is estimated that no more than 4.0 per cent of children were baptised outside of their parish of birth during the period 1813–52; (iii) applying the national infant mortality figures from the civil register and data on birth/baptism delays in the parishes of Kingston, Iver and Medmenham, we find that 5.45 per cent of infants died before the time of baptism and this figure is not likely to have been higher than 8.6 per cent for the pre-civil registration period, even assuming a much higher rate of infant mortality; (iv) making allowances for the 3.4 per cent of cases which were over-matched in the original N.I.R. sample, the proportion of N.I.R. cases due to census errors of birthplace, name and age, and the practice of baptising outside the parish of birth, appear to be of the same order of magnitude as missed cases not registered through baptism on account of infant mortality before baptism and the over-matching of cases because of the standardisation of procedure.

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[37] D. C. Levine, "A re-evaluation of baptism as a form of birth registration through cross-matching parish register and census data; together with some proposals for remedying gross deficiencies", *Cambridge Colloquium On Historical Demography And Social Structure* (1973), fn. 17.

This conclusion cannot of course be final, as a great deal more work must be done on a more representative sample of parishes.Also, only the most laborious research which compared census/baptism and civil register data for a systematically covered number of neighbouring parishes could hope to come up with final conclusions. These will probably involve other sources of data as well, so that the cross-comparison methodology will eventually be applied to all relevant surviving historical sources of information. For the present it can be concluded that although there are a number of deficiencies and problems associated with the census/baptism comparison method, because of the counter-balancing effects of factors which led to an under-estimation of the N.I.R. ratio, it provides a very simple standardised way of evaluating the adequacy of particular Anglican baptism registers, as well as forming the basis of a more general assessment of changes in registration accuracy over time.[38]

[38] There has been some misunderstanding on the conclusions reached from the overall N.I.R. rates; it has been assumed, for example, by Wrigley and Levine that the census/baptism method should take account of additional factors such as non-conformist baptism registration. This misses the fundamental point about the cross-matching methodology: it was designed as an economical method by which Anglican baptism could be evaluated as a form of birth registration. The logic behind this was that so much historical research has depended, and inevitably will depend in the future, on Anglican records.

Chapter 6

A Critique of "An Interpretation of the Modern Rise of Population"[1]

This essay is a critique of the work of McKeown, Brown and Record, who argued that an increase in the per capita consumption of food was the key variable in explaining the fall in mortality in the eighteenth and nineteenth centuries. In this paper I present evidence to show there was little or no increase in food consumption during the first four decades of the nineteenth century, when mortality appears to have been declining rapidly.[2]

The finding of a fall in mortality during the first half of the nineteenth century led me to revise my thinking on the role of inoculation. The latter was generally practised at the end of the eighteenth century, and therefore cannot have accounted for nineteenth-century mortality decline. In the present essay I examine the evidence for improving personal hygiene, and how this may have been a factor in improved health and reduced mortality during the early nineteenth century.

In their article "An interpretation of the modern rise of population in Europe",[3] McKeown, Brown and Record have attempted to generalise conclusions drawn from a study of post-registration data for England and Wales to a number of other European countries – Sweden, France, Ireland and Hungary. Their mode of argument takes the form of a hypothetical discussion of the plausibility of various medical and non-medical explanations of population growth since the eighteenth century, based not on detailed

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1 First published in *Population Studies*, Vol. 28 (1974).

2 See Chapter 4.

3 Published in the November 1972 edition of *Population Studies.*

historical or statistical evidence for the relevant period, but derived from conclusions reached from the study of later national civil registration returns. This type of analysis is grounded on the apparent belief that demographic and medical evidence before civil registration is valueless. The recent proliferation of detailed demographic studies using new techniques of analysis and sources of data is dismissed with the bald statement that for "the pre-registration period ... national data are not available. It is hardly possible to read the literature of the past two decades without being acutely aware of the deficiencies."[4] In defence of McKeown *et al.*, it might be argued that very little of this new work – for example, family reconstitution – has yet been published. However, I want to argue in this paper that sufficient new material has been made available to cast very serious doubt on their thesis; I will confine my discussion mainly to the evidence for England and Wales, which will allow an examination of the key point of origin of their analysis. Their thesis has been summarized as follows:

> population growth was not influenced by improved sanitation before about 1870 or by specific medical measures before the introduction of the sulphonamides in 1935 ... the rise of population in the eighteenth and early nineteenth centuries (was due) to a decline of mortality which resulted from improvement of diet ... [through] a large increase of food production.[5]

The thesis assumes that it was a decline in mortality rather than an increase in fertility that was responsible for the increase in population. Wrigley has recently published family reconstitution work on the parish registers of Colyton and Hartland which indicates that increasing fertility was important as a source of increasing population; two parishes, however, are obviously not adequate as a sample for national generalizations and it will be necessary to wait for the completion of the Cambridge Group's work before generalization can be made from this material, and even then findings will have to be evaluated in the context of a number of technical demographic considerations. In my recently published paper on the reliability of parish register data, I have calculated national birth and death rates from parish register and census returns

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[4] T. McKeown, R. G. Brown and R. G. Record, "An interpretation of the modern rise of population in Europe", *Population Studies*, Vol. 26, (1972), p. 345.
[5] *Ibid*, p. 341.

for the period 1801–40;[6] these rates have not only been derived through a direct analysis of the reliability of register data, but have been evaluated through detailed census information. Although further work will be required before they can be accepted as being definitive, there are substantial grounds for taking them as a basis of discussion. The birth and death rates for England and Wales calculated on the basis of corrected population returns are as follows:[7]

Period	*Birth Rate (per 1,000 population)*	*Death Rate (per 1,000 population)*
1800–10	41.4	30.5
1811–20	42.0	29 8
1821–30	40.1	25.9
1831–40	35.9	22.8

As there was little change in the age structure of the population during this period according to the 1821 and 1841 age censuses, it is safe to conclude that there was a significant fall in mortality during this period.

The data so far considered support the argument developed by McKeown *et al.*, in their paper about the importance of diminished mortality. Further evidence, however, fundamentally contradicts their thesis. We would expect, if food supply was the crucial variable, mortality reductions to be concentrated almost exclusively amongst the poorer sections of the community. Wealthy groups such as the aristocracy should be unaffected if the food supply hypothesis were true, yet one of the most consistent conclusions of recent historical demographic work is that there were marked reductions in mortality in just such groups as the aristocracy. For England and Wales the most reliable evidence is to be found in Hollingworth's study of the British peerage: he found that there were significant falls in mortality throughout the eighteenth and nineteenth centuries. This conclusion can be summarized by the following figures of changing expectation of life at birth for aristocratic females:[8]

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[6] See Chapter 4 of this book.

[7] See Chapter 4.

[8] T. H. Hollingsworth, "The demography of the British peerage", *Supplement to Population Studies*, Vol. 18, (1964) p. 57.

Cohort born	*Expectation of Life at Birth (Years)*
1700–24	36.3
1725–49	36.7
1750–74	45.7
1775–99	49.0
1800–24	51.7
1825–49	58.4

Reductions in mortality occurred across the whole range of age groups, although there was a particularly significant fall amongst young children under five.[9] Similar results have been found in the study of the mortality of gentry families during the same period[10] and for the "middling and higher classes" who were nominated for the government life annuities and tontines during the eighteenth century.[11] Peller in his large-scale study of Europe's ruling families came to almost identical conclusions: expectation of life of females in these families rose from 33.7 years in the seventeenth century to 38.2 years in the eighteenth and 48.1 years in the first half of the nineteenth.[12]

It is surprising in a way that these findings have not received more attention from historical demographers and medical historians than they have. Clearly, from a methodological point of view, no general explanation of population growth during the eighteenth and nineteenth centuries which cannot explain the falls in mortality among the wealthy aristocratic and upper-class families can be considered adequate. This does not mean that one can rule out explanations like increases in food supply, for it is possible that these were important for some groups in the population and not others. But the scale of the falls in mortality amongst upper-class groups which could account for more or less the whole population increase in England and Wales if occurring amongst the general population suggest that the food supply hypothesis should be looked at very critically.

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[9] *Ibid*, p. 55.

[10] See Chapter 1.

[11] P. E. Razzell, *The Role Of Smallpox Inoculation In The Growth Of Population In Eighteenth-Century Britain* (Oxford University D.Phil. Thesis), p. 54. See also the discussion of tontines in Chapter 7.

[12] S. Peller, "Births and deaths among Europe's ruling families since 1500", D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (London, 1965), p. 95.

McKeown *et al.* unfortunately do not discuss the evidence for the food supply hypothesis in any detail; with respect to England and Wales they merely state that "in relation to interpretation of population growth, it is unnecessary to consider in detail the timing and character of the changes in agriculture which led to the increase in food supplies".[13] In their general review of the evidence, they note that "British agriculture was not only feeding many more people; it was, at least until 1767, feeding them better".[14] As an elaboration to this conclusion, they note in a footnote that Deane and Cole in their *British Economic Growth 1688–1959* "suggest that production of cereals and meat *kept pace* (my italics) with population growth until the last decade of the century".[15] Yet it was during the latter half of the eighteenth century when food consumption was at best static (it may well have been decreasing) that population began to accelerate, whereas during the first half of the century when food consumption per head increased, population was more or less stationary.[16] It is not sufficient for food supply to keep pace with population to bring about a reduction of mortality – it is necessary for consumption per head to increase so as to improve health. The evidence available for the eighteenth century is much more consistent with a reversed hypothesis – that standard of diet was a function of population change.

The evidence on food consumption per head tends to be much more reliable for the nineteenth century than for the eighteenth, and this is partly a function of the fact that accurate population figures are available from 1801 onwards as a result of the introduction of the national census. This is of particular relevance for the present paper which has traced a sharp decline in mortality during the first 40 years of the nineteenth century. Mitchell and Deane have published a series of figures which come nearest to a set of

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[13] S McKeown, Brown and Record, *loc. cit.*, p. 353 fn. 1.

[14] *Ibid*, p. 352.

[15] *Ibid*, footnote 26.

[16] There is little direct evidence on food consumption per head in the eighteenth century. The series of real wage indices for London and Lancashire building labourers published by Mitchell and Deane suggest the conclusion reached in the text: 1703–07 – 227; 1743–47 – 280; 1783–87 – 250. See B. R. Mitchell and P. Deane, *Abstract Of British Historical Statistics* (1962), pp. 346, 347. (1994): Since the above was written, new evidence suggests that population did begin to increase significantly at the beginning of the eighteenth century. See Chapter 7.

reliable national indices of United Kingdom consumption per head for this period:[17]

	Coffee (Pounds)	*Tea (Pounds)*	*Sugar (Pounds)*
1801	0.07	1.49	22.53
1811	0.53	1.24	23.15
1821	0.36	1.27	18.19
1831	0.94	1.24	18.92
1841	1.06	1.37	16.99

There was a very sharp rise in coffee consumption, but a fall in both tea and sugar consumption during this 40-year period. The rise in coffee consumption is unlikely to have had much effect on the standard of diet of most of the poorer section of the population, as coffee drinking during this period was more a middle-class than a working-class habit.[18] The decline in tea and sugar consumption per head is matched by a similar decline in the consumption of beer: 1800–4 – 33.9 gallons; 1810–14 – 30.2; 1820–24 – 29.0; 1831 – 21.3; 1841 – 19.6.[19] This was during a period when the custom of home brewing was waning and so the decline in beer drinking would be even greater than indicated by these figures.

To some extent, of course, figures for the commodities considered so far are secondary to the more basic ingredients of diet such as bread and meat – although as G. R. Porter pointed out, consumption of commodities such as sugar, tea and coffee formed a useful index of the ability of the ordinary population to consume above the subsistence level.[20]

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[17] *Ibid*, pp. 355, 356.

[18] See John Burnett, *Plenty And Want* (1966), p. 10. Perhaps the best single piece of evidence on this is the survey of diet among agricultural labourers and unskilled town workers conducted by Dr Edward Smith in 1863. Only 41 per cent of agricultural labourers consumed any coffee whatsoever and the figures for town workers were at the same relatively low level. The average amount of coffee consumed by these workers' families was about 1 ounce a week, compared to a consumption of about 2.5 ounces of tea. See Dr Edward Smith, *Report On The Food Of The Poorer Labouring Classes In England; Sixth Report Of The Medical Officer Of The Privy Council,* (Parliamentary Papers, 1864, XXVIII.)

[19] Burnett, *op. cit.*, in footnote 17, p. 12.

[20] *Ibid*, p. 10.

Unfortunately, there are no exact figures of the national consumption of basic commodities such as bread and meat during the period under discussion, although none of the estimates made of consumption per head of these commodities has suggested a rise. R. N. Salaman in his study of the social history of the potato estimated consumption per head of wheat and potatoes as follows:[21]

Wheat *United Kingdom*		*Potatoes* *England & Wales*	
1770s	1.36 pounds	1795	0.40 pounds
1830s	0.90 pounds	1838	0.62 pounds

Although on these figures there was an increase in the consumption of potatoes, this was more than matched by a decline in wheat consumption. The agricultural historian L. Drescher has estimated "that wheat cultivation in England and Wales increased from 3 to 3.8 million acres between 1798 and 1846, and that yields increased from 20–24 bushels per acre to 32–34 bushels ... wheat production just failed to keep pace with population".[22] Since Drescher made these estimates of wheat production, the agricultural historians Healy and Jones have confirmed his estimate of the scale of change in wheat yields during the period relevant to the present analysis. They have published a series of wheat yield figures which were derived from direct observations in a number of different parts of the country using a standard box measure of yield. According to these statistics there was an increase from 31.2 bushels per acre in 1815–19 to 36.6 bushels per acre in 1837–41 (this increase from the first to the last five years is representative of the trend over the whole period).[23] This increase represents a proportionate change of 17.3 per cent, which must be set against a population increase of 36 per cent during the same period.[24] All this evidence suggests a decline rather than an increase in bread consumption during the first 40 years of the nineteenth century.

This conclusion is also generally applicable to the consumption of meat. Deane and Cole have published estimates of sheep and cattle production

[21] Mitchell and Deane, *op. cit.*, in footnote 15. p. 358.

[22] R. M. Hartwell, "The rising standard of living in England, 1800–1850", *Economic History Review*, Vol. 13, (1960–61), p. 408.

[23] M. J. R. Healy and E. L. Jones, "Wheat yields in England, 1815–59", *Journal Of The Royal Statistical Society,* Series A, 125 (1962), p. 578.

[24] Mitchell and Deane, *op. cit.*, in footnote 15, p. 8.

which suggest a decline in consumption per head of both: 26.7 million sheep in England and Wales in 1800, declining to 24.0 million in 1841; under 32 million cattle in England and Wales in 1779 which had only increased to 5.2 million for the whole of the United Kingdom by 1832 – and the evidence which does exist suggests no increase in the weight of cattle.[25] The only direct statistical evidence available for this period confirms this conclusion about declining meat consumption per head. The following figures are for the cattle and sheep slaughtered at Smithfield market:[26]

	Cattle ('00s)	*Sheep ('000s)*	*London's Population ('000s)*
1799–1803	125	793	1,117
1839–1843	177	1,443	2,239

The slaughter of sheep kept pace with the growth of London's population but there was a marked decline in the proportion of cattle slaughtered per head of population. The overall conclusion from this review is that with the exception of coffee (which was mainly a middle-class drink) and potatoes, there is no evidence to support McKeown's thesis that there was an increase in average food supply in Great Britain during the latter half of the eighteenth and first half of the nineteenth century; on the contrary, the evidence that does exist suggests a decrease in food consumption per head.

In the light of the above considerations, it is all the more important to consider carefully all possible medical explanations of falling mortality. McKeown and colleagues have not examined the medical and historical evidence in detail, but rather have come to overall conclusions on the basis of a priori general considerations. A major example of this is their brief comment on the smallpox inoculation hypothesis; they write that in their view there are three objections to the idea that smallpox inoculation had a significant effect in reducing mortality:

> it assumes a substantial and prolonged decline of the disease which cannot be confirmed by national data; it postulates an effectiveness of inoculation which is not accepted by virologists who know smallpox; and it attributes to this crude and dangerous measure an influence on

[25] Deane and W. A. Cole, *British Economic Growth* (1969), pp. 69, 195.
[26] Mitchell and Deane, *op. cit.*, in footnote 15, pp. 19, 354.

total mortality which would not be expected from any modern immunization procedure, supported by the full resources of the laboratory and health education.[27]

It is impossible to comment in any detail on points that raise so many complex issues; I have dealt with the subject at length in my book on the history of smallpox inoculation[28] and I will briefly comment here on some of the points made by McKeown and his colleagues:

(1) Although there are no national data for England and Wales during the eighteenth and early nineteenth centuries, there is an abundance of local and literary evidence which demonstrates a radical reduction in smallpox mortality. The best single piece of evidence for the long-term decline in smallpox mortality is the series of smallpox censuses sponsored by Jurin and others during the 1720s; the survey covered towns and six villages in different parts of the country and although not randomly selected, included a very careful study of 13,192 cases of smallpox. Out of this total number of cases of people who caught smallpox, 2,167 are recorded as having died – a case fatality rate of 16.5 per cent.[29] Given the fact that smallpox was a universal disease – reflected in its mainly childhood incidence – a very significant proportion of the population died from the disease. The above figures are likely to minimize mortality for a number of reasons discussed elsewhere.[30] As smallpox deaths formed only 1.5 per cent of deaths per 100 children born during the period 1838–44,[31] (the first available national statistics after the introduction of civil registration) it is clear that there was a very significant decline of mortality. The national statistics for Sweden during the latter half of the eighteenth century confirm this conclusion about the severity of smallpox before the effective introduction of inoculation and vaccination;[32] similarly for other countries considered

[27] *Ibid*, p. 142.

[28] Peter Razzell, *The Conquest Of Smallpox* (1977).

[29] *Ibid*, p. 130.

[30] *Ibid.*

[31] *Ibid*, p. 157.

[32] In the ten year period 1757–66 when national statistics were first compiled, there were 64,956 smallpox deaths out of a total of 543,212 cases in Sweden (the number of smallpox deaths at this time, however, included deaths from measles). See *Royal Commission on Vaccination, First Report*, (1889), p. 112.

by McKeown there is evidence to support the same conclusion, e.g. 21 per cent of all deaths in Dublin during the period 1660–90 were from smallpox.[33]

(2) On the question of the effectiveness of inoculation, no medical historian has ever questioned the effectiveness of inoculation in preventing further attacks of smallpox during the lifetime of the person inoculated – unlike vaccination which gave a limited period of protection against further attacks – and this is a function of the degree of attenuation of the virus injected and the number of antibodies produced in reaction to the injection. Occasionally eighteenth-century writers noted that somebody inoculated had a much later attack of smallpox, but this was obviously a very rare event, because of the amount of comment it provoked.

(3) The notion that inoculation was a 'crude and dangerous measure' is also not supported by the historical evidence. It is true that early inoculation did in a very small proportion of cases lead to the death of the person inoculated, and that it did occasionally spread the disease to those not protected against it. It is the latter point which has led most medical historians to reject the argument that inoculation reduced smallpox mortality; in fact there is evidence that inoculation only very rarely spread the disease and even where it did, it was irrelevant from a demographic point of view because smallpox was already a universal illness affecting the total population. In fact, ironically these isolated cases of inoculation spreading smallpox provoked 'general inoculations' of whole communities based on the fear that the uninoculated would be particularly vulnerable – and this fear even provoked some parish authorities to resort to compulsory inoculation.[34] Also, as the technique of inoculation developed throughout the eighteenth century, it became safer both with respect to the dangers of dying from the operation and in connection with the risk of spreading the natural disease. There is evidence to suggest that early vaccination was in fact only a more attenuated form of inoculation, both in the nature of the injection and in the effects produced.[35] When McKeown *et al.* question

[33] *Ibid*, p. 137.

[34] *Ibid*, pp. 55, 68.

[35] Peter Razzell, *Edward Jenner: The History Of A Medical Myth* (1978).

> the effectiveness of inoculation on the grounds that it was not "supported by the full resources of the laboratory and health education", they also question the effectiveness of vaccination, which has been universally accepted as having combatted smallpox in a highly effective way.

Although I have emphasized the role of smallpox inoculation and vaccination in reducing mortality (inasmuch as there was a difference in these two injections during the early period, the former was more important in Great Britain and the latter in the rest of Europe), it is clear from the demographic evidence quoted earlier that this particular explanation cannot explain the total fall of mortality during both the eighteenth and nineteenth centuries. Inasmuch as inoculation eliminated smallpox during the late eighteenth century, it, or vaccination, could not have been responsible for the very significant fall in mortality as measured by the national crude death rates for England and Wales during the period 1801–41 or the sharp falls in mortality amongst the aristocracy during the same period. Even on the inoculation hypothesis, however, some of the reduction in smallpox mortality would have occurred during the early nineteenth century, but only to an insignificant extent. It is necessary in the light of these considerations to re-examine the literature on improvements in health during the century 1750–1850 to see whether it is possible to put forward additional hypotheses to explain the unresolved significant decline in mortality. Such hypotheses must by their very nature be tentative and act only as pointers to further research.

II

One obvious factor in the improvement of health which could account for reduced mortality amongst all sections of the community is a qualitative change in the environment. During the late eighteenth and early nineteenth centuries contemporaries were virtually unanimous in agreeing that the drainage of marshland through the reclamation of land associated with agricultural improvements led to an improvement in health. It would appear from the description of the illness eliminated through drainage works that in many cases it must have been a mild form of malaria – most accounts emphasize the debilitating effects of the disease on the agricultural working population rather than direct mortality from the

illness.[36] However, there is some statistical evidence that mortality was significantly higher in the marshy areas of the fenlands of East Anglia than in dry areas elsewhere, and Wrigley has suggested that in low-lying badly drained areas it might have been difficult to avoid diseases like tuberculosis and typhoid, as well as malaria.[37] This is obviously a hypothesis which must be explored through further detailed work on particular parishes, but it is difficult to see how it could apply to more than a very small area of the whole country.

Another explanation in the same general category is the improvement of the environment through deliberate measures, such as better drainage of towns, widening and cleansing of streets. There are a number of difficulties against accepting this particular explanation as being important in accounting for reduced mortality. First, only about 20 per cent of the total population lived in towns with a population of more than 10,000 in 1801, and therefore town improvements could hardly have been decisive. Second, it is difficult to believe that the public health of the Victorian town was much improved over that in the eighteenth century: the average death rate of Birmingham, Leeds, Bristol, Manchester and Liverpool in 1840 was 30.8 per 1,000,[38] compared with a national average of 22.9.[39] Even if there had have been an improvement in the public health of towns, this might have been more than outweighed by the fact that a larger proportion of the total population were living in the relatively unhealthy towns, compared to country areas – by 1851 about half the population were living in towns of 10,000 and above. Third, there is some evidence that town improvements were confined largely to the areas mainly inhabited by the wealthy rather than the poor; for example, Dr Southwood Smith stated in his report of 1838:

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[36] For a good discussion of malaria in England see M. C. Buer, *Health, Wealth And Population* (1926), pp. 210, 222. *The Statistical Account Of Scotland* which was published in the 1790s and ran to 21 volumes, contains a number of references to the disappearance of malaria due to land drainage and reclamation. Usually the accounts emphasise the loss of working time through the disease rather than any mortality resulting from it.

[37] E. A. Wrigley, *Population And History* (1969), p. 100.

[38] G. Talbot Griffith, *Population Problems Of The Age Of Malthus* (1967), p. 186.

[39] Mitchell and Deane, *op. cit.*, in footnote 15, p. 36.

> While systematic efforts on a large scale have been made to widen streets, to remove obstruction to the circulation of free currents of air, to extend and perfect the drainage and sewerage, and to prevent the accumulation of putrefying animal and vegetable substances in the places in which the wealthier classes reside, nothing whatever has been done to improve the conditions of the districts inhabited by the poor.[40]

Of course, this improvement in the environment of the wealthy might go some way in explaining the diminishing mortality amongst the aristocracy during the first half of the nineteenth century, but the limitations of such an explanation as far as the general population are concerned are self-evident.

The relative ineffectiveness of public health measures before the latter half of the nineteenth century is readily understandable in the context of the British social structure – weak central and local authorities within a tradition of laissez-faire individualism. The emphasis on public health measures in previous attempts to explain reductions in mortality are not based on empirical evidence – a part of a tradition in social history which lists a number of theoretically possible explanations, particularly those couched in terms of intentionally designed improvements. McKeown et al. have quite rightly criticized this type of explanation with respect to developments of medical knowledge. In fact, it could be argued that most medical advances have occurred through empirical trial-and-error, rather than through sophisticated theoretical advances. Ironically, McKeown, Brown and Record themselves fall victim to this over-emphasis on sophisticated scientific medicine; for example, they insist that modern laboratory methods are necessary to reduce mortality significantly, when it is clear that empirically evolved methods such as inoculation and vaccination were highly effective against smallpox without being based on a sophisticated scientific technology.

There were a number of empirical medical discoveries developed during the eighteenth century which are now known to be scientifically valid. The discovery that citrus fruit was effective in eliminating scurvy and that cod liver oil prevented the development of rickets both fall in this category. There is little evidence, however, that these diseases were ever

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[40] S. and V. Leff, *From Witchcraft To World Health* (1958), p. 170.

particularly fatal for the population at large, or that the remedies were ever applied on a large scale in the period in which we are interested.[41] It is possible that the increased consumption of potatoes could have diminished the effects of rickets through increasing intakes of vitamin D, but there is evidence that rickets persisted amongst the population at large into the twentieth century.[42]

There remains one empirical discovery made in the eighteenth century which I shall argue in this paper could have had a very marked effect on health and made a substantial contribution to reducing mortality – although it is likely that improvement in health came about not so much through the deliberate application of the discovery but rather through its utilization on social and economic grounds. From about the middle of the eighteenth century onwards, a number of medical pioneers began to teach, through their writings and practice, the importance of hygiene and general cleanliness: Sir John Pringle discovered the importance of hygiene for preventing dysentery in army camps, James Lind demonstrated how it was possible to prevent typhus in navy hospitals and ships through rules of hygiene, and Sir Gilbert Blane reduced the incidence of hospital fever through an insistence on scrupulous cleanliness.[43] All these innovations were limited to institutions where some degree of centralized authority made it possible to impose rules of hygiene from above, and so necessarily only influenced a small proportion of the total population. However, the emergence of the dispensary and Lying-In movements at the end of the eighteenth century led to a wider diffusion of this principle of hygiene. Lettsom for example claimed that the influence of the General Dispensary in London had brought about improvements in the way many ordinary people treated their sick relatives, by encouraging cleanliness and better personal hygiene.[44] Lettsom also believed in that "in the nurture and management of infants, as well as in the treatment of lying-in women, the reformation hath equalled that of the smallpox [through inoculation]".[45] A

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[41] This conclusion is based on a reading of Burnett, *op. cit.,* footnote 17, and J. C. Drummond & Anne Wilbraham, *The Englishman's Food* (1957).

[42] Burnett, *op. cit.*, in footnote 17, p. 272; Drummond and Wilbraham, *op. cit.*, in footnote 40, p. 439.

[43] Buer, *op. cit.,* in footnote 35, pp. 119, 120.

[44] Griffith, *op. cit.*, in footnote 37, p. 224.

[45] Buer, *op. cit.*, in footnote 35, p. 150.

part of this reformation had consisted of an insistence on cleanliness, which undoubtedly would have helped to reduce mortality.

In order for this new attitude towards hygiene to affect more than an institutional minority it had to be widely diffused at the individual level; yet Willan writing in 1801 noted that "most men resident in London and many ladies though accustomed to wash their hands and face daily, neglect washing their bodies from year to year".[46] After this date, however, the situation changed radically and I shall argue in this paper that it was an improvement in personal hygiene rather than a change in public health that was responsible for the reduction in mortality between 1801 and 1841. I should emphasize that this argument will be presented very much in the form of a hypothesis, partly because there has been no serious scholarly study of the social history of personal hygiene. The subject has traditionally been treated as a source of amusement and has been presented in the context of social history as entertainment. I will initially outline in summary form current medical opinion on effects of personal hygiene on health and subsequently present fragments of evidence supporting the notion that there was a marked change in personal hygiene during the first half of the nineteenth century.

There are basically two classes of disease which are affected by personal hygiene: (i) diseases of the intestinal tract, and (ii) diseases transmitted from person to person by body lice. In the first category the most important diseases relevant to the period under discussion were probably (a) gastro-enteritis, (b) typhoid fever, and (c) dysentery; and in the second category (d) typhus, (e) relapsing fever, and (f) trench fever. Personal hygiene basically affects the former through the transmission of the pathogenic organisms in the faeces via the hands or through flies; it affects the latter through the cleanliness of the body and clothing which determines whether the body louse can survive or not. In addition, personal hygiene can prevent secondary bacterial infection which can influence the outcome of other diseases not mentioned above. Personal hygiene prevents both classes of disease: for example, in the case intestinal diseases, "all pathogenic organisms will be removed if they are washed in soap and water";[47] in the case of lice-borne infections hygiene is crucial because "lice are not usually found on the bodies of human beings who possess the

[46] *Ibid*, p. 62.

[47] Ronald Hare, *Bacteriology And Immunity For Nurses* (1967), p. 79.

necessary facilities for keeping themselves clean. But when it becomes impossible to obtain soap, hot water and a change of clothing, they almost always become infested with lice".[48] It is not necessary here to discuss in any greater detail the actual processes through which the diseases are transmitted from person to person; it is sufficient for the present purposes to indicate the consensus of medical opinion on the importance of personal hygiene for preventing the diseases under discussion. The earlier quotation from Willan's writings in 1801 indicated that even amongst the genteel population of London personal hygiene was of a very low standard; the women, unlike the men, at least appeared to have washed their hands and faces daily. According to Lawrence Wright, who has written a general social history of personal hygiene, this was typical of the state of cleanliness amongst all social classes before the beginning of the nineteenth century. Wright points out that Pepys only once mentioned his wife having a bath in the nine years he kept his diary:

> My wife busy in going with her woman to the hot house to bathe herself, after her long being within doors in the dirt, so that she now pretends to a resolution of being hereafter very clean. How long it will hold I can guess.[49]

Amongst the advice that Lord Chesterfield gave to his son was the following on personal hygiene:

> Washing yourself, and rubbing your body and limbs frequently with a flesh-brush will conduce as much to health as to cleanliness. A particular attention to the cleanliness of your mouth, teeth, hands and nails, is but common decency, in order not to offend people's eyes and noses.[50]

The fact that Lord Chesterfield felt it necessary to advise his son to wash his hands and mouth although he did not recommend washing his face or body – suggests the lack of the actual practice in this respect. A manual of etiquette dated 1782 merely advised wiping the face every morning with a white linen and warned against washing it in water as that made the face too sensitive to cold and sunburn.[51] Perhaps Dr

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[48] *Ibid*, p. 62.

[49] Lawrence Wright, *Clean And Decent* (1960), p. 76.

[50] Ronald Fletcher, *The Parkers At Saltram 1769–89* (1970), p. 116.

[51] Wright, *op. cit.*, in footnote 48, p. 138.

Johnson was speaking for his age: "Smell, Madam?" he is reputed to have said, "I positively stink".[52]

There is evidence of a somewhat more objective nature for the lack of personal cleanliness amongst the upper classes in the eighteenth century. Not only is there no evidence of the presence of bathrooms in the ground-plans of Georgian houses but "inventories which chronicle the most trivial utensils found in the kitchen, fail to recognize or describe the common bath."[53] It is probable that when people did bathe they, like Mrs Pepys, went to a public bath-house. The history of the wash-basin tends to confirm the literary evidence about the lack of personal cleanliness amongst the "respectable" classes: not until about 1770 did a wash-stand appear that was capable of holding soap and even then the wash-basin itself was of minute proportions.[54] This, however, was a hint of the new cleanliness to come, along with the introduction of Bramah's water-closet in 1778.[55] Before the use of the water-closet, sanitary arrangements even in genteel circles were surprisingly unrefined: one foreign visitor noted that in good society "the sideboard, too, is furnished with a number of chamber pots and it is a common practice to relieve oneself whilst the rest are drinking; one has no kind of concealment ..."[56]

The above evidence, of course, only applies to the wealthier social classes; unfortunately there is little readily available information on hygiene among ordinary people. This kind of evidence is crucial of course not only for explaining changes in overall mortality but even for accounting for decreases in mortality amongst groups such as the aristocracy: most food would be prepared and served by domestic servants – and their personal hygiene would have a marked effect on the health of their employers. It is, of course, very unlikely that the personal hygiene of the ordinary man was any better than that of wealthier groups. Francis Place, who was fascinated by the transformation of manners and morals at the beginning of the nineteenth century, had much first-hand experience of the English working classes and succinctly summarized in 1822 the change that had taken place amongst them with respect to personal cleanliness:

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[52] Fletcher, *op, cit.*, p. 117, fn. 49.

[53] A. S. Turberville (ed.), *Johnson's England, Vol. 2* (1952), p. 130.

[54] Wright, *op. cit.*, p. 112, footnote 48.

[55] *Ibid.*, p. 107.

[56] Fletcher, *op. cit.*, p. 53, footnote 49.

> the change ... has taken place, not only in London, but all over the country, in the habits of the working classes, who are infinitely more moral, more sober, more cleanly in their persons and their dwellings, than they were formerly, particularly the women; partly from the success of the cotton manufactures, which has enabled them to discard the woollen clothes which were universally worn by them, which lasted for years, and were seldom, if ever washed; partly from increased knowledge in domestic concerns, and the nursing and general management of children. Notwithstanding the vice, the misery, and the disease which still abounds in London, its general prevalence has been greatly diminished.[57]

It might be thought that dirty clothing was fairly closely correlated with social class, yet again Dr Johnson in good-humoured fashion suggested otherwise:

> I have often thought that if I kept a seraglio, the ladies should wear linen gowns – or cotton; I mean stuffs made of vegetable substances. I would have no silk; you cannot tell when it is clean; it will be very nasty before it is perceived to be so. Linen detects its own dirtiness.[58]

It is clear from the above descriptions that all sections of the community would have been prone to both intestinal and louse-borne diseases; perhaps the wealthier classes would have been somewhat less affected by the latter because of the greater frequency of changing clothes, but this is an empirical question which can only be settled by more research. Creighton in his study of the history of epidemics in Britain presented evidence to suggest that typhus was more prevalent amongst the poor than the rich during the eighteenth century, but this must be qualified by the fact that fatality of the disease appeared to have been much greater amongst the wealthy.[59] There is also some evidence to suggest that the rich were particularly prone to fevers of various sorts[60] most of which could probably

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[57] Francis Place, *Illustrations And Proofs Of The Principle Of Population* (1930 ed.), p. 253.

[58] Turberville, *op. cit.*, p. 278, footnote 52.

[59] Charles Creighton, *History Of Epidemics In Britain*, Vol. 2 (1965), pp. 102, 134, 141, 266, 290.

[60] C. S. Peel, "Homes and habits", G. M. Young (ed.), *Early Victorian England*, Vol. 1 (1934), p. 84.

be linked to inadequate personal hygiene. Fever was generally regarded by contemporaries as being responsible for a significant proportion of total mortality and the Carlisle Table of Mortality for the years 1779–87 indicates that fever caused at least 10 per cent of all deaths[61] – although the inadequate classification of these diseases makes it difficult to say precisely what form they took. Gastro-enteritis was also responsible for killing large numbers of infants, although again the classification of the cause of death is so imprecise as to make it very difficult to know what proportion of total mortality was due to this disorder.

The London Bills of Mortality show dramatic declines in mortality from fevers and the variously designated infantile complaints – particularly diarrhoea – after the end of the eighteenth century.[62] The Bills of Mortality are, however, unreliable as a source of evidence.[63] Contemporaries certainly believed that greater cleanliness had reduced mortality from these diseases; for example, Blane writing in 1813 referred to the "counteraction of typhus by means of cleanliness and ventilation".[64] According to the Registrar-General's statistics, 'typhus' only accounted for 4.8 per cent of all deaths in the period 1838–42, in spite of the fact that typhus proper was being confused with typhoid and relapsing fever.[65]

The question must, of course, be raised about the actual evidence in favour of the idea that there was a marked improvement in personal hygiene in the first half of the nineteenth century. We have already seen that Place believed that this was the case amongst the working classes and that this was linked to the availability of cheap cottons. Certainly, the price of cotton goods fell dramatically during this period – G. R. Porter quoted the following statistics to illustrate this point: the price paid for weaving 24 yards of Cambric at Stockport fell from 25 shillings in 1802 to ten shillings in 1812, while the selling price of 728 Calicoes fell from £1 8s. in 1814 to seven shillings in 1841;[66] combining these figures suggests that

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[61] Buer, *op. cit.*, p. 269, fn. 35.

[62] *Ibid*, p. 270. Joshua Milne, *A Treatise on the Valuation of Annuities*, 2 vol. (1815).

[63] See the discussion of the unreliability of Hackney parish register – which formed part of the Bills of Mortality – in Chapter 4.

[64] Buer, *op. cit.*, p. 238, fn. 35. See also Griffith, *op. cit.* pp. 227, 228, fn. 37.

[65] Creighton, *op. cit.*, pp. 183, 198, fn. 58. Mitchell and Deane, *op. cit.*, p. 34, fn. 15.

[66] F. W. Hirst (ed.), *G. R. Porter's The Progress Of The Nation* (1912), p. 298.

the 1841 price would only have been about a tenth of that charged in 1802. This dramatic reduction in price led to a marked increase in the consumption of cotton goods: in 1801 52 million pounds of raw cotton imports were retained for United Kingdom production; by 1841 this figure had reached 438 million pounds – an increase of over eight times.[67] As the proportion of cotton goods exported remained more or less constant during this period,[68] domestic consumption of cotton goods per head would have increased by over four times. This did not take place at the expense of other types of cloth: wool, silk and linen all appear to have at least kept pace with population expansion[69] This, of course, is not inconsistent with Place's statement about cotton displacing wool, for whereas twice as much wool as cotton was consumed at the beginning of the nineteenth century, by the end of the 1830s the ratio was more than reversed, with about two-and-a-half times as much cotton being used as wool.[70]

There is also good statistical evidence that personal hygiene improved through the increasing use of soap. Total soap consumption approximately doubled between 1713, when figures are first available, and 1801, the year of the first census: from 24.4 million pounds to 47.6 million pounds.[71] If Gregory King's estimate of population in the 1690s is anything to go by, the population of England and Wales was of the order of 5.5 million at the beginning of the eighteenth century; as population in 1801 was over 9 million, there probably was a modest increase in consumption per head during the century. Most of this increase was concentrated at the end of the century, although without firm population figures it is difficult to be absolutely certain of this. After 1801, it is possible to be very precise about average consumption figures; according to figures published by Porter, soap consumption per head of population nearly doubled between 1801 and 1841, from 4.84 pounds to 9.20 pounds.[72]

The figures published by Mitchell and Deane indicate a somewhat smaller increase – from 6.1 pounds in 1799–1803 to 9.7 pounds in 1841 –

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[67] Deane and Cole, *op. cit.*, p. 179, fn. 24.

[68] *Ibid*, p. 187

[69] *Ibid*, pp. 196, 210; Mitchell and Deane, *op. cit.*, p. 184, fn. 15.

[70] *Ibid*, p. 179; Deane and Cole, *op. cit.*, p. 196, fn. 24.

[71] Mitchell and Deane, *op. cit.*, p. 265, fn. 15.

[72] Hirst, *op. cit.*, in footnote 64, p. 422.

an increase still of the order of 60 per cent.[73] These figures must be treated with some caution; not only was soap produced illegally to escape the excise duty – and this varied during the 40-year period – but soap was used in manufacturing processes as well as for domestic consumption. Nevertheless, the statistics are consistent with the argument that there was a marked improvement in personal hygiene during the period under discussion.

There is some literary evidence to support this conclusion, particularly with respect to the wealthier social classes. Professor Wilson has summarized the transformation in personal hygiene as follows:

> It was the Duke of Wellington who probably did most to spread the fashion for the daily bath among the upper and middle classes. ... By the sixties, a daily bath was usual among those who afford the coal for heating the water and the labour to carry the great jugs from which the hot water was poured into the movable tub. A little lower in the social scale, the bath was a weekly ritual but washing took place daily, and everywhere the wash-hand stand, with its basin, jug, and soap dish, was making its appearance in the Victorian bedroom.[74]

This improvement of hygiene was reflected in the proliferation of various makes of portable bath during the Victorian period,[75] the fixed bath as a part of a bathroom not really existing until the end of the nineteenth century. The absence of a proper bathroom did not stop the Victorians from bathing every day; for example a parson's daughter referring to the period after 1847 wrote that "there were no bathrooms then, and all hot

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[73] Mitchell and Deane, *op. cit.*, p. 265, fn. 15. Professor Charles Wilson in his history of Unilever has given yet a third figure per head for 1801: 3.6 pounds – which is nearer Porter's figure than Mitchell and Deane's. One of the reasons for the increased consumption of soap during this period might have been a decrease in price; the duty on hard soap was reduced from 2s. 3d. per pound in 1801 to ls. 5d. per pound in 1841, but more importantly there is some evidence to suggest that the overall price of soap fell from about 9d. per pound in the 1790s to about 5d. a pound in the 1830s. See Charles Wilson, *The History of Unilever*, Vol. 1 (1954), p. 9; Hirst, *op. cit.*, p. 422, fn. 64; F. M. Eden, *The State Of The Poor* (1928), p. 242; Burnett, *op. cit.*, p. 53, fn. 17.

[74] Wilson, *op. cit.*, p. 6, fn. 71.

[75] Wright, *op. cit.*, p. 165, fn 48.

and cold water had to be carried from the kitchen and scullery. But we all had baths every day in spite of that".[76] Obviously, the mass of the population did not bath every day, partly because in some cases they had to buy water and also because of the expense of heating water. According to one social historian who has made a scholarly study of the Victorian home:

> Servants washed each day and bathed once a week, as did the respectable better-off poor. Those who were neither respectable nor better-off washed when and how they could, or did not wash.[77]

The latter conclusion is later qualified by the statement that the poor "went on trying to be clean, washing in the costly water clothes which when hung out to dry (in industrial areas) were quickly covered in smuts".[78] This is clearly an area which requires careful research for clarification, perhaps through a study of working-class budgets of the period to see how much was spent on soap and cleaning materials; for example, it would appear from budgets of cotton workers living in Manchester and Dunkinfield in 1841 that some families consumed about 2 pounds of soap a week[79] suggesting that personal hygiene was of a very high standard.

The improvement of personal cleanliness through more frequent washing and bathing was accompanied by much more effective sanitation, at least in the case of the wealthier classes. According to one contemporary, water-closets had come into general use by the year 1814[80] although the social historian C. S. Peel believed that this was not the case until about 1830.[81] Inasmuch as the water-closet replaced unhygienic methods of sanitation, health would have been significantly improved, particularly through eliminating fly-borne diseases. However, it is doubtful whether the water-closet was used by more than a small minority of the general population by the end of the 1830s, it obviously being expensive to buy and instal.

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[76] Peel, *op. cit.*, p. 90, fn. 59.
[77] *Ibid.*, p. 87.
[78] *Ibid*, p. 142.
[79] Burnett, *op. cit.*, pp. 53, 70, 77, fn. 17.
[80] Wright, *op. cit.*, p. 108, 78, fn. 48.
[81] Peel *op. cit.* p. 85, fn. 59.

In conclusion, the question must be raised as to whether the argument developed in this paper is relevant to countries other than England and Wales. It is likely that the historical evidence on food supplies is just as problematic for these other countries; for example McKeown *et al.* emphasize the importance of the potato in Irish population growth, yet the potato was generally used about 100 years before the period of most rapid population expansion – and the evidence is that earlier diet was nutritionally much more adequate than that during the first half of the nineteenth century.[82] In practice, it is highly unlikely that any one explanation will be adequate for all European countries, which had significantly different social structures during this period. Only detailed historical research will begin to resolve the issues raised in this paper, but such research must be guided by hypotheses which are both theoretically sound and consistent with the known empirical evidence.

[82] See Chapter 4.

Chapter 7

The Growth of Population In Eighteenth-Century England: A Critical Reappraisal[1]

The growth of the English population in the eighteenth century has long interested economic historians and, since the time of Thomas Malthus, has provoked much debate about the relationship between population change and economic growth. In our own time, scholars have focused on the nature and chronology of change: whether economic development preceded and prompted population growth or vice versa. The structure of demographic change has, however, yet to be resolved. Prior to the nineteenth century, English demographic data are incomplete: there were no national censuses before 1801, and civil registration of births, marriages, and deaths did not begin until 1837. Demographic research on the pre-nineteenth-

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[1] First published in *The Journal of Economic History*, Vol. 53, No. 4 (Dec. 1993). Although there were several people who commented on various drafts of this paper, the person who I owe most to is Dr Ruth Richardson, who made a number of suggestions for re-writing sections of the paper, as well as providing invaluable encouragement and support. Peter Lindert made several useful critical comments on the structure of the paper, as did Pamela Evans and the referees who encouraged me to re-write an earlier draft version. Jim Oeppen helped with a number of technical calculations, particularly the expectation of life figures in Table 10. Ros Davies of the Cambridge Group provided a print-out of the Colyton reconstitution schedules which form the basis of Table 4. Frank Leeson made available information on the tontines, as did Anthony Camp in various genealogical issues. I appreciate the comments on different drafts of the paper by Christopher Hill, John Habakkuk, Tony Wrigley, Keith Snell, Richard Wall, Richard Smith, Michael Anderson and my brother, Edward Razzell.

century period has relied mainly on parish registers, which list baptisms, marriages, and burials. The accuracy and coverage of these materials is uncertain, and their survival is uneven.

Despite these difficulties, all demographers have discerned a rise in the rate of English population increase in the second half of the eighteenth century, and many have emphasized fertility as the key mechanism of population growth. These ideas have received added weight from the ambitious programme of research undertaken since the 1960s by the Cambridge Group for the History of Population and Social Structure. The Cambridge Group's demographic findings were presented in *The Population History Of England*, written by two of the group's leading members, Tony Wrigley and Roger Schofield.[2] The authors argued that English population grew in the latter half of the eighteenth century mainly because of a rise in fertility. This rise, they hypothesized, was due to a reduction in the age at marriage, itself a consequence of rising real incomes caused by economic development. This article questions the validity of their conclusion and develops an alternative chronology and explanation of the demographic transition in England.

NUPTIALITY AND MARITAL FERTILITY

The Cambridge Group have used two methodologies in its demographic work: 'back projection' and 'family reconstitution'. I will evaluate each in turn and offer evidence suggesting that the reliability of both methods as applied to the English data is open to question.

Back Projection

Back projection was a technique used by Wrigley and Schofield to estimate earlier population levels by retrospectively adding the number of deaths and net emigrants to the various age groups enumerated in the nineteenth century censuses, extending their process back into the sixteenth century. They used records of baptisms, marriages, and burials from a sample of 404 parish registers, which in theory allowed them to reconstruct the

[2] E. A. Wrigley and R. S. Schofield, *The Population History Of England* (1991).

numbers of people living at all periods, as well as to compute marriage, birth, and death rates. The method involves using a number of assumptions of unknown reliability, with scope for the compounding of errors and assumptions over long periods of time. Although the technique was developed using a very sophisticated computer programme, the unknown reliability of the raw data and the uncertain assumptions used in the programme led Schofield himself to compare it with looking "through a glass darkly".[3]

Ronald Lee, an active associate of The Cambridge Group, expressed his own reservations about the method in the following terms: "Back projection attempts an impossible task, and can only arbitrarily select one demographic past from among an infinite set of equally plausible and acceptable ones, which are consistent with the input data".[4] Recognition of the method's problems led other scholars to propose adjustments to the technique. Lee advocated its replacement with what he termed "inverse projection"; he claimed to have validated Wrigley and Schofield's findings by applying this new method to their basic data. More recently, Wrigley and Schofield have themselves advocated a variant of a method pioneered by Jim Oeppen, "generalized inverse projection".[5]

However, such methods require reliable data on births, deaths, migration, age structure, and mortality by age for the appropriate period – though they differ in their exact demands for reliability. Lacking accurate source material, the advocates of these methods have had to adjust their back-projected data in various ways.

For example, to correct for the under-registration of births, Wrigley and Schofield have inflated the number of baptisms by various ratios, derived from a comparison of expected births with actual records of baptisms. The estimates of expected births were calculated by taking the various census age groups and adding the estimated number of those born into the groups, who died or migrated in the period before the census. A crucial factor in this computation is the magnitude of the various age groups, because it is

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[3] R. S. Schofield, "Through a glass darkly: the population history of England as an experiment in history", *Journal Of Interdisciplinary History* (1985).

[4] R. D. Lee, "Inverse projection and back projection: a critical appraisal, and comparative results for England, 1539 to 1871", *Population Studies*, Vol. 39, No. 2 (July 1985), p. 190.

[5] Wrigley & Schofield, *op.cit.*, p. xvii.

the starting point for the process of estimating expected births. A poor estimate of the number of people in each age group would affect the inflation ratios used to correct the figures for baptisms, and hence would affect back-projected estimates of birth rates.

Peter Lindert argued that the Wrigley–Schofield findings were distorted by the changes they made to census age figures. He concluded that "life tables and nineteenth-century censuses suggest that birth registration was worse before 1780 than after. Yet Wrigley and Schofield turn the suggestion upside down, arbitrarily revising the censuses instead".[6] Lindert has calculated the inflations they made to the birth rate in a tabular form, reproduced here in Table 1.

Table 1. Birth Rates per 1,000 Population, England and Wales[7]

	1749–1753	*1814–1818*
Birth Rate Before Inflation	29.70	27.99
Penultimate Estimates (after Inflating for Non-Conformity and Delayed Baptisms)	32.14	32.69
Final Estimates after "Residual" Inflations	33.76	41.92

Lindert's disquiet at the transformation of the pattern of fertility through the use of these inflation ratios seems justified. The inflations adopted by Wrigley and Schofield progressively increase the birth rate, though the critical inflation is for "residual" non-registration. This residual inflation increases the birth rate for the period of 1814–1818 from 32.69 to 41.92 per 1,000, transforming the pattern of fertility in the period. Before this residual adjustment Wrigley and Schofield's original data suggested a constant birth rate during the latter half of the eighteenth century; after it, a very significant increase was apparent. That increase was due entirely to the inflation ratios derived from their assumptions about the age structure of the population applied to the original data.

My own research also throws doubt upon those inflation ratios. I have compared census statements directly with the expected baptism register entries for individuals living in 45 parishes selected from all parts of England. Table 2 displays the two sets of figures.

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[6] Peter H. Lindert, "English living standards, population growth, and Wrigley–Schofield", *Explorations In Economic History*, Vol. 20 (1983), p. 136.

[7] For the source of this table see Lindert, *op. cit.*, p. 138.

Table 2. Individuals Listed in the 1851 Census but Not Found in the Baptism Register Versus The Cambridge Group's Inflation Ratios[8]

Period	*Percentage Not Found in Register (Razzell)* (1)	*Period*	*Wrigley & Schofield's Inflation Ratios (%)* (2)
1761–1770	32.4	1760–1769	8.4
1771–1780	27.9	1770–1779	9.3
1781–1790	32.6	1780–1789	13.1
1791–1800	36.0	1790–1799	20.9
1801–1810	32.0	1800–1809	28.8
1811–1820	33.0	1810–1819	38.0
1821–1830	30.0	1820–1829	34.1
1831–1834	27.4	1830–1839	26.0

The figures in column 1 are based on direct empirical evidence; those in column 2 are derived from theoretical reconstruction.[9] The two series are radically different in their trends over time; the census-baptism register data show little or no change over the period, whereas Wrigley and Schofield's figures show a sharp deterioration in registration accuracy from 1781 onward.

The critical ingredient in their inflation ratios Wrigley and Schofield used was their adjustment of age structure data derived from the nineteenth century censuses. They themselves pointed out that one of their major

[8] See Peter Razzell, "The evaluation of baptism as a form of birth registration through cross-matching census and parish register data: a study in methodology", *Population Studies*, Vol. 26 (1972), p. 129 (published as Chapter 4); and Wrigley and Schofield, *The Population History*, p. 561.

[9] For column 2, I calculated the percentages that Wrigley and Schofield used to inflate baptisms in order to produce the number of births (excluding non-registration due to delayed baptism.) The census-parish comparison method has attracted criticism on three grounds: (1) the 1851 census mis-stated the birthplaces of individuals enumerated; (2) many parents had their children baptised in neighbouring parishes; and (3) the 1851 census mis-stated names and ages. From research linking census, parish and civil registration data (see Chapter 5), it has been established that the "false negatives" arising from these three factors amounted to about 10 per cent for the whole sample of 45 parishes. The "false negatives" were counter-balanced by "false positives" due to using over-strict criteria for successful matches and to infants dying before baptism.

assumptions was "that the age data for the older age groups became progressively less trustworthy with rising age, until above the age of 70 very substantial corrections to the published totals are necessary".[10] This is not a minor step in their calculations. It is not only central to the question of baptism registration adequacy, but it can be crucial for estimates of population size using back projection. Older age groups in the nineteenth-century censuses form the starting point of back projection, and any change in their numbers makes a critical difference to estimates of population size because of the compounding of errors with each "pass" through the computer programme. For example, Wrigley and Schofield reduced the size of the group aged 90 to 94 in 1871 by 44 per cent; if they had chosen instead to reduce that age group by 40 per cent, their estimate of the English population in 1541 would have been about 9 per cent greater.[11]

How reasonable are Wrigley and Schofield's assumptions? When we examine age statements by comparing the census with baptism register entries, a very different picture emerges from that assumed for the back-projection programme. For the census-parish register sample of 45 parishes, 88.8 per cent of all adult ages in the 1851 census were accurate to within two years, 97.8 per cent to within five years. Contrary to Wrigley and Schofield's assumptions, there was no deterioration in the accuracy of age statements above the age of 70; the reliability of age statements in the 70 to 80 age group was the same as for the total sample. Only in the 80 to 90 age group was there any decrease in accuracy. But even there, 74.5 per cent of the ages were accurate to within two years, and 90.2 per cent to within five years.[12] This conclusion is confirmed by Wrigley himself from his detailed work on the 1851 Colyton Census: "The generally high standards of statements of age is clear. Only a tiny percentage of ages were out by more than two years ... Even at advanced ages this holds true in general ... Only one of the 26 [cases aged over 70] mis-stated his age by more than three years".[13]

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[10] Wrigley and Schofield, *Population History*, p. xiv.

[11] R. D. Lee and D. Lam, "Age distribution adjustments for English censuses, 1821 to 1931", *Population Studies*, Vol. 37 (1983), pp. 445–464.

[12] Razzell, "The evaluation of baptism", *op. cit.*, pp. 126, 127.

[13] E. A. Wrigley, "Baptism coverage in early nineteenth century England: the Colyton area", *Population Studies*, Vol. 29 (1975), p. 304.

On the substantive issue of the increase of the eighteenth-century population, the evidence suggests no increase in the birth rate during the latter half of the century. Wrigley and Schofield, however, supported their argument about the central role of a rise in fertility by quoting data from their research on family reconstitution, which purports to show that a rise in fertility associated with a reduction in the age at marriage (rather than a fall in mortality), was responsible for eighteenth-century population growth. Although they expressed a caveat about the reliance on a very small number of parishes in their reconstitution work – about 13 from a total of about 10,000 have formed the basis of the sample to date – these scholars used their family reconstitution findings to underpin the conclusions they reached from back projection. Yet there are also grounds for disquiet about the accuracy of their use of the reconstitution method. This is a theme of such importance as to deserve detailed examination.

Family Reconstitution

Family reconstitution involves the study of individual families at the parish level. Individuals are traced in the baptism, marriage, and burial registers, and certain assumptions are made to establish family links among the individuals traced. From those links data are generated on a range of demographic variables, including age at marriages, fertility, and mortality rates. Family reconstitution is only applicable to individuals who remained in their parish of origin, as those who left disappeared from local records. For example, in the case of marriage, those who migrated after baptism invariably married elsewhere and would be excluded from the age-at-marriage calculations. Wrigley and Schofield worked on the assumption that those who remained in a parish were representative of the whole population, including migrants.

Ever since Peter Laslett's well-known 1960s' study of Clayworth and Cogenhoe, social historians have increasingly come to recognize just how mobile the English population was. A general study of migration in early modern England by Peter Clark and David Souden found that up to 80 per cent of the population was mobile, the percentage varying by place and over time, with increased mobility during periods of population growth.[14]

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[14] Peter Clark and David Souden, *Migration And Society In Early Modern England* (1987), pp. 32, 122, 123, 222.

As migrants are excluded from reconstitution studies, these very high levels of migration mean that reconstitution charts include only minorities of the population.

Evidence suggests that, because of the association between migration and social status, these minorities were atypical. Clark and Souden found that "more respectable members of local society tended to be less mobile than small craftsmen, servants and labourers" – though this may have varied over time.[15] Most evidence on geographical mobility and social status shows that they were very strongly correlated. From his work with the Cambridge Group, Souden noted "the high mobility of labourers in the reconstitution material" and commented on the "high mobility of labourers and many craftworkers and the relative immobility of farmers and food retailers." He concluded that "the marked lifetime immobility of farmers – of yeomen and husbandmen – contrasted with labourers ... would show the degree to which landholding, or its prospect, would condition movement".[16] Those included in the reconstitution cohorts – the stayers – were much more likely to be farmers and other property owners, whereas the migrants were invariably labourers, servants, and other propertyless groups. Labourers, servants, and other impoverished groups formed a significant proportion of the population at this time – perhaps up to half the total. Their relative exclusion would raise major questions about the validity of reconstitution methodology.

Migration also serves to distort reconstitution calculations in a more technical way, that can most easily be illustrated with respect to calculations of the average age at marriage. Wrigley's study of Colyton indicated that the proportion of women born and married in the parish fell from 43 per cent in the 1560–1646 period to 25 per cent in 1720–1769 before rising to 31 per cent between 1770 and 1837.[17] Such a significant shift in the amount of migration would affect calculations of age at marriage, if migration was not evenly distributed among the various age groups. For example, if for some reason a larger proportion of women in their late twenties migrated out of a parish, this would have the apparent effect of

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[15] *Ibid*, pp. 122, 123.

[16] David Souden, *Pre-Industrial English Migration Fields* (D.Phil., University of Cambridge, 1981), pp. 250, 254, 310.

[17] R. S. Schofield, "Age-specific mobility in the eighteenth-century rural English parish", *Annales De Demographie Historique* (1970), p. 262.

lowering the age at marriage: women marrying at older ages would have left the sample before they could be included in the reconstitution age-at-marriage calculations, and only the younger ones would be recorded. Thus, even where there were no real changes in the age at marriage, variations in migration patterns could create the illusion of change, because of the calculation method used in reconstitution work. Without a detailed knowledge of migration, it is impossible to say precisely what effect it would have on age-at-marriage calculations. Clearly, the effect could be significant.

Various sources suggest that the number of widow and widower remarriages as a proportion of the total number of marriages fell from approximately 30 per cent at the beginning of the eighteenth century to about 10 per cent at the end.[18] Whether this reduction occurred as a result of falling mortality or of changes in the propensity to remarry is an open question, but the fall itself could influence the accuracy of reconstitution by reducing the number of older men and women marrying in a parish. Most parish registers do not give information on the marital status of the marrying parties; for men, this could lead to a systematic over-statement of first-marriage ages in the earlier period by accidentally including marriage ages of widowers. Large numbers of women of unknown marital status listed in the marriage registers could also distort reconstitution findings because of the greater likelihood of confused identity.

The problem of identity confusion also arises when parish register information is inadequate. The linking of baptism and marriage dates in reconstitution work is essentially speculative, based on the assumption that a similar name within a certain time period confers a common identity. Yet there are grounds for believing that this assumption is unjustified. As we will see, it was a widespread practice in England to give the name of a dead child to a subsequent sibling of the same sex, and many parish registers were defective in registering the baptism and burial of those

[18] Wrigley and Schofield, *Population History*, pp. 258, 259. The parish registers of Stoke Poges, Eton, and Farnham Royal in Buckinghamshire; of St Margaret's Rochester in Kent; and of Barnstable in Yorkshire give information on previous marital status during the civil registration period of 1653 to 1658. Total marriages of widows ranged between 25.7 and 37.0 per cent. The marriage licences of East Kent and West Sussex show a fall in the proportion of widows, from over 30 per cent in the first half of the seventeenth century to approximately 10 per cent in the early nineteenth century.

subsequent siblings. The registration of burials – and possibly baptisms – improved in at least some of the reconstitution parishes during the eighteenth and early nineteenth centuries, which might have affected calculations of the changing mean age at marriage. The Cambridge Group uses identical names in the baptism and marriage registers as the basis for calculating marriage ages. The non-registration of subsequent same-name siblings would inflate marriage ages by incorrectly linking the first dead sibling with the sibling of the same name listed in the marriage register. This would have been more significant in the earlier period of course, because of the less adequate registration of same-name individuals.

There are therefore four serious grounds for questioning the validity of reconstitution methodology as it has been applied to English marriage data: (1) The sociologically unrepresentative nature of reconstituted cohorts due to the exclusion of migrants; (2) the technical distortion effects of migration upon the calculation of reconstitution statistics; (3) the unknown effect of changes in the proportion of widows and widowers in the marriage registers; and (4) the effect of changing patterns of same-name sibling registration on the calculation of marriage ages.

Given the uncertain reliability of back projection and family reconstitution as they have been applied to English historical data, it is necessary to carefully examine other forms of demographic evidence for the seventeenth- and eighteenth-century period to see what they reveal.

AGE AT MARRIAGE DURING THE SEVENTEENTH AND EIGHTEENTH CENTURIES

The mean age at first marriage for women in the Cambridge Group's reconstitution sample was at its highest for the period from 1650 to 1699 – 26.2 years.[19] In historical terms this is a high figure, and its magnitude is largely responsible for the subsequent fall in the age at marriage found by the group. It is therefore important to evaluate that mean carefully, as it represents the key element in the pattern of marriage ages generated by reconstitution.

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[19] E. A. Wrigley and R. S. Schofield, "English population history and family reconstitution: summary results 1600–1799", *Population Studies*, Vol. 37 (1983), p. 164.

Two forms of marriage were legal in England in the seventeenth and eighteenth centuries: marriage by licence and marriage by banns. Although both types were included in parish registers, marriage licences were recorded separately by the ecclesiastical authorities and often contain a great deal more information – such as age at marriage – not found in parish registers. Marriage by licence was marginally more expensive than marriage by banns, and therefore was more socially exclusive. In particular, labourers tended to marry by banns, though all other occupational groups appear to have been well-represented by licences.[20] However, the flexibility of marriage by licence – it allowed marriage in any parish without having to call banns on three successive Sundays – meant that this type of marriage became very popular in the seventeenth and eighteenth centuries. For example, over 50 per cent of all marriages in the Diocese of Canterbury were by licence between 1677 and 1725.[21] Indeed, in some parishes in the Diocese of London at that time, the proportion rose to over 80 per cent.[22] For demographers licences have the advantage of giving information on migrants as well as non-migrants, and of covering large groups of parishes; they therefore help overcome the problem of concentrating on individual, and possibly atypical, parishes.

The accuracy of age statements in marriage licences seems to have been high. Vivian Elliott evaluated marriage ages in a sample of 69 cases of London licences at the beginning of the seventeenth century: the averages were 23.47 years in the licenses and 23.50 years by reconstitution – that is, by comparing baptism and marriage dates in the parish register. A similar exercise for 50 Leicestershire marriages at the end of the same century yielded averages of 24.8 and 23.8 years respectively, indicating a difference of about one year.[23] This may be due to inaccuracies in marriage

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[20] D. J. Steel, *General Sources Of Births, Marriages And Deaths Before 1837* (National Index Of Parish Registers, Vol. 1, 1976), p. 227.

[21] The number of licence marriages is listed in J. M. Cowper (ed.), *Canterbury Marriage Licences* (1894) and (1898). The total number of marriages in Kent is given in *Enumeration Abstract, 1841 Census*. The proportion marrying by licence was 50.74 per cent for the period between 1677 and 1725.

[22] See, for example, the St. Michael Cornhill, St. Mary Aldermary, and St. Helen's Bishopsgate marriage registers for this period.

[23] Vivian B. Elliott, *Mobility And Marriage In Pre-Industrial England* (D.Phil. thesis, Cambridge University, 1978), pp. 291, 325.

licence age statements or to a confusion of identities in the parish register as a consequence of same-name registration problems.

In the late seventeenth century, high-quality information is available from licences taken over 1,000 parishes in five counties in different regions of England: Kent, London, Nottinghamshire, Suffolk, and Yorkshire.

Table 3. Age at First Marriage of Women Listed in Licences, 1660–1714[24]

Period	*Region*	*N*	*Mean Age at Marriage*	*Reconstitution Mean Age at Marriage, 1650–1699*
1662–1714	Yorkshire	7,242	23.76	
1660–1702	London	500	21.93	
1661–1700	Kent	1,000	24.06	26.2
1670–1709	Nottinghamshire	3,284	24.44	
1690–1709	Suffolk	356	23.60	

Table 3 shows that the mean age at marriage in the four counties other than London lies within a narrow band of 23.60 to 24.44 years. The overall average age at first marriage for the five counties is 23.56 years. significantly lower than the mean age found in the reconstitution sample for the same period: 26.2 years. In the 1840s, the earliest years of civil registration, the mean age at first marriage of women was about 25.[25] The data in Table 3 suggest, therefore, no fall in the mean age at first marriage, but on the contrary, a long-term rise of about 1.5 years.

[24] Sources: For Yorkshire: M. Drake, "An elementary exercise in parish register demography", *Economic History Review*, Vol. 14 (1962), p. 444; for London: George Armytage, *Allegations For Marriage Licences ... At London* (Harleian Society, Vol. 24, 1886) – selecting the first 100 cases from the beginning of each decade; for Kent: Cowper, *Canterbury Marriage Licences*, (1876, 1898) – selecting the first 500 cases from each volume; for Nottinghamshire: T. M. Blagg and F. A. Wadsworth (eds.), *Abstracts Of Nottinghamshire Marriage Licences* (British Record Society Index Library, Vol. 58, 1930) – selecting all cases listed; for Suffolk: W. B. Bannerman (ed.), *Allegations For Marriage Licences In The Archdeaconry Of Sudbury* (Harleian Society, Vol. 69, 1918) – selecting all cases listed.

[25] *Registrar-General's Fifty-Eighth Annual Report* (1897), p. ix; and *Registrar-General's Twenty-First Annual Report* (1860), p. iii.

THE HISTORY OF MORTALITY

Because the evidence considered in the previous section offers no support for a decline in age at marriage – nor for a rise in fertility – it is necessary to look elsewhere to explain English eighteenth-century population growth. In this section I will argue that the key demographic change was a decline in mortality, that was particularly marked in the first half of the eighteenth century.

Population studies covering the centuries prior to reliable civil registration largely depend on data derived from parish registers. These registers invariably include information on baptisms (not births), marriages, and burials (not deaths). The reliability of the burial registers is obviously crucial to the study of mortality. For their calculation of reconstitution mortality rates, Wrigley and Schofield assumed burial registration accuracy of 100 per cent. Yet evidence suggests that in certain respects burial registration was significantly more defective in the seventeenth and eighteenth centuries than at a later period.

I have developed a method for measuring the adequacy of burial registration that may be termed the "same-name evaluation technique". It is based on child-naming customs prevalent in early modern England. It was extremely rare to give two living children identical Christian names; for example, of 2,221 children named in sixteenth century Essex Wills, only 0.5 per cent of living siblings shared the same name. An examination of seventeenth century census returns from different parts of the country revealed no clear cases of living brothers and sisters with the same name.[26] On the other hand, it was widely customary to pass a dead child's name on to the next-born sibling of the same sex.

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[26] F. G. Emmison (ed.), *Essex Wills, 1558–1565* (1982). The censuses searched were the Ealing 1599 census, the Clayworth censuses for 1676 and 1688, and the 1695 Marriage Duty Act censuses for London, Lyme Regis, Swindon, and Wanborough. The London census was published in D. V. Glass (ed.), *London Inhabitants Within The Walls* (London Record Society, Vol. 2, 1966). The Bristol census is in Elizabeth Ralph and Mary Williams (eds.), *The Inhabitants Of Bristol In 1696* (Bristol Record Society, Vol. 25, 1968). Copies of the other censuses can be found in the library of the Cambridge Group.

A look at two parishes used intensively in reconstitution work, Hartland and Colyton, enables us to estimate the frequency with which this same-naming custom was observed. In Hartland in the period from 1725 to 1743, a sample was chosen from the parish register of 50 dead children whose parents bore subsequent children of the same sex. Thirty of the subsequent children – 60 per cent – were given the same name as their pre-deceased sibling.[27] In Colyton, a similar examination of the data has proved possible over a much longer period by means of a re-analysis of the reconstitution schedules from 1538 to 1851.[28] There was a total of 789 families in the parish in which a child was baptised after the death of another of the same sex. Of those families, 508 – 64.4 per cent – gave the name of a previously baptised dead child to a subsequent child. The changes over time in the proportion of same-named children were as follows: 1538–1600: 54.9 per cent; 1601–1650: 55.5 per cent; 1651–1700: 76.9 per cent; 1701–1750: 70.0 per cent; 1751–1800: 73.5 per cent; 1801–1837: 63.4 per cent; and 1837–1851: 62.2 per cent. These are sufficiently large proportions of the total number of families to form the basis of an evaluation of burial registration during the whole 400-year period covered by the reconstitution schedules.

The importance of same-naming to the study of burial register accuracy can be illustrated as follows. During the middle part of the eighteenth century, Thomas Turner, a Sussex shopkeeper, kept a detailed diary and compiled notes on his family's history.[29] He listed his children's births and deaths as follows:

Peter (born August 19, 1754, died January 16, 1755)
Margaret (born March 20, 1766)
Peter (born June 1, 1768)
Philip (born November 9, 1769)
Frederick (born December 8, 1771, died November 7, 1774)
Michael (born April 29, 1773)
Frederick (born May 3, 1775, died June 13, 1775)
Frederick (born December 17, 1776)

[27] See the *Hartland Parish Register*.

[28] A computer print-out of the reconstitution schedules of Colyton was kindly provided by Ros Davies of the Cambridge Group. The grouping of the families is specified in the print-out. Families with just interpolated baptisms were not included in the analysis because doing so would introduce bias into the analysis.

[29] G. H. Jennings (ed.), *The Diary Of A Georgian Shopkeeper* (1979), pp. 79–84.

Turner's first wife died after the birth of his eldest son Peter, and he subsequently remarried. The list of his children reveals the pattern of same-naming: the first Peter and the first two Fredericks each died, and the next child of the same sex was given the dead child's name. Thomas Turner had lived all his married life in the parish of East Hoathly, and it is instructive to compare this list of births and deaths with the record of baptisms and burials of his children in the East Hoathly parish register:[30]

Peter baptised August 31, 1754.
Margaret Turner baptised April 23, 1766.
Peter baptised June 28, 1768.
Philip baptised November 5, 1769.
Frederick baptised December 30, 1771.
Michael baptised May 19, 1773.
Frederick baptised May 14, 1775, buried June 13, 1775.
Frederick baptised January 10, 177.

All of Turner's children were baptised and registered in the parish, but only one of the three dead children was recorded in the burial register – the second Frederick, who died in 1775. Turner's diary reveals that Peter and the first Frederick were in fact buried in the neighbouring parish of Framfield, where their grandparents had died and been interred.

The Cambridge Group's reconstitution rules work on the assumption that all family events occur within the parish of residence. Given this, the demographic history of the Turner family, in which two children were buried outside the parish, would be misrepresented. The group's reconstitution rules would generate a calculated child mortality rate of 12.5 per cent (one out of eight children), whereas in fact the true mortality rate was 37.5 per cent (three out of eight children).

The practice of same-naming, however, allows us to assess the adequacy of parish registers in registering the deaths of children. For example, though we would not know from the East Hoathly burial register what had happened to Peter and the first Frederick, the repetition of their names in the baptism register would tell us that they had died, even though no record of their burial was available. We can thus assess the reliability of burial

[30] I am grateful to the East Sussex Record Office for conducting a search of the East Hoathly parish register.

registration of a particular parish register by measuring the proportion of same-name baptisms against registered same-name burials.

Application of this technique to a sample of cases selected from the Hartland parish register reveals that the accuracy of burial registration varied over time. Two hundred children baptised with the same name as a subsequent sibling were selected in alphabetical sequence from the register index for the period of 1558 to 1837.[31] Sixty-three of them – 31.5 per cent – were missing from the burial register. The first 100 cases, in the period from 1558 to 1724, had an omission rate of 39 per cent, whereas the second hundred cases, from 1725 to 1837, had a rate of only 24 per cent. These provisional results suggest a significant improvement in burial registration in Hartland during the eighteenth century.

A similar analysis of the 508 families in the Colyton reconstitution schedules who gave two or more of their children the same name yields the results shown in Table 4.

Table 4. Analysis of Burials Registration of Same-Name Siblings in Colyton, 1538–1837[32]

Period	*N*	*Number Found in Burial Register*	*Percentage of Cases Unregistered*
1538–1600	95	62	34.7
1601–1650	121	71	41.3
1651–1700	114	86	24.6
1701–1750	84	54	35.7
1751–1800	94	60	36.2
1801–1837	77	64	16.5
1837–1851	38	34	10.5
Total	623	431	30.8

The omission rate for the whole Colyton sample – 30.8 per cent – is similar

[31] The initial identification of names was provided by the Hartland parish register index. In the earlier period only the father's name was available for establishing a correct identity, but when two or more families had the same name, place-names were used as an additional criterion.

[32] All calculations were made from Colyton reconstitution schedules supplied by Ros Davies of The Cambridge Group. The identity of same names is specified in the schedules, and in every case those names were selected for analysis.

to that found in Hartland, and registration accuracy there also seems to have varied over time. The Colyton registers reveal a sharp improvement at the beginning of the nineteenth century, which is consistent with what is known generally about the relative accuracy of Anglican burial registration at the time of the introduction of civil registration.[33]

I have made a special study of the Colyton Anglican burial register between 1837 and 1851, the period immediately following the introduction of civil registration. The civil registration records there list the deaths of 199 children under the age of ten during this period. Of that number, 170 were registered in the Anglican burial register, giving an omission rate of 14.6 per cent – slightly higher than the 10.5 per cent figure found using the same-name technique for the same period. However the civil registers included young infants who died before baptism and were therefore often denied full burial status by the church. If we exclude infants who died in less than 24 days – the approximate mean age of baptism in Colyton at the time – the burial omission rate declines to 10.8 per cent.[34] We must not make too much of the almost identical findings of the same-name technique and the civil-Anglican burial register comparison method, as the sample in the former study is small. Nevertheless, the similarity in the results of these two methods indicates a degree of reliability.

There were a number of reasons why Anglican burial registration was so deficient before the nineteenth century. The major factor was probably the negligence of clerks and clergymen in registering burials that had taken place in their parish.[35] Of all same-name cases in Colyton during the period from 1538 to 1851, 30.8 per cent were missing from the burial register. We can evaluate this figure by comparing it with the proportion of people dying in Colyton who left wills, but who were not registered in the burial register. Information is available on 124 people living in Colyton or who

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[33] Glass estimated that about 20 per cent of all deaths were omitted from Anglican burial registration in the early period of civil registration, but this figure was lower in rural parishes like Colyton. See D. V. Glass, "Population and population movements in England and Wales, 1700 to 1850", in D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965), pp. 221–246.

[34] This analysis is based on a list of Anglican burials and civil registration deaths that took place in Colyton between 1837 and 1851. The list was kindly provided by Richard Wall of the Cambridge Group.

[35] See the discussion of this topic in Chapter 4.

specified burial in the parish churchyard and made wills between 1554 and 1797; of this number 35 – 28.1 per cent – were not registered in the burial register.[36] The similarity between this and the same-name figure suggests that there was a general under-registration of burials – both of adults and children – during the period.

We have seen in the case of the Turner family another reason for unrecorded burials was the interment of children in neighbouring parishes – a practice described by Schofield as a "traffic in corpses".[37] This probably accounts for some of the missing burials in a parish like Colyton. In the reconstitution schedules, information is sometimes given on the residence of a family, and there is a correlation between place of residence and registration reliability between 1538 and 1837, the period covered by the schedules. Of 65 same-name cases in which the father was listed as living in the town of Colyton, 48 were found in the burial register, an omission rate of 26.2 per cent. When families lived outside the town, in hamlets and outlying farms, the omission rate was as high as 43.9 per cent, only 83 out of 148 same-name cases being found in the burial register. Some of these missing cases were probably buried in neighbouring parish churchyards, that were closer to the outlying areas than was the Colyton parish churchyard. Children baptised in Colyton but buried in surrounding parishes would not appear in the reconstitution statistics of infant and child mortality, and their omission would lead to an under-statement of mortality.

Wrigley and Schofield's assumption of absolute accuracy of the parish registers used in their reconstitution work was based partly on their having carefully selected high-quality parish registers, eliminating those with obvious defects. In the case of baptism registration, their assumption may be justified – particularly as missing baptisms can be interpolated from information on child burials, and registers can be selected on the basis of having the right pattern of birth intervals (that is, baptisms of children in a particular family occurring approximately every two years).

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[36] S. A. Smith, *Extracts From (302) Wills Proved In P.C.C. Relating To The Parishes Of Shute And Colyton* (1901); Edward A. Fry, *Calendars Of Wills ... Relating To ... Devon And Cornwall*, Vol. 1 (1908) and Vol. 2 (1914); and the *Colyton Parish Register*. Information is usually given on the dates of the making and proving of wills, which allows a precise check against the burial register.

[37] R. S. Schofield, "Through a glass darkly: the population history of England as an experiment in history", *Journal Of Interdisciplinary History*, Vol. 15 (1985).

No such interpolation or selection is possible with burial registers, however, and the evidence derived from the same-name technique as applied to Hartland and Colyton suggests that death registration was unreliable throughout the sixteenth- to eighteenth-century period. The deficiency was probably greater than that measured by Table 4. The same-name technique can only be applied to cases in which baptisms were accurately registered, and it is likely that children whose baptism registration was defective, also had more deficient burial registration. As we have seen, neither does the technique allow for children who died before baptism, and many of them would not have appeared in the burial register.

The Cambridge Group's estimates of infant and child mortality rates for Hartland and Colyton in the seventeenth and eighteenth centuries are low by historical standards: in the range of 83 to 106 per 1,000 between 1600 and 1749, falling to 57 to 97 per 1,000 between 1750 and 1799.[38] The results of the same-name technique indicate higher rates for all periods. If we allow for the various factors just discussed, which would further inflate registration unreliability, it is likely that infant mortality in Hartland and Colyton in the seventeenth and eighteenth centuries has been under-estimated by between 35 and 50 per cent.

According to the group's figures, the average infant mortality rate for the 13-parish reconstitution sample for 1600 to 1749 lay in the range of 161 to 169 per 1,000.[39] If we inflate this rate as indicated earlier, it would increase infant mortality to between 250 and 340 per 1,000. Given that national infant mortality was about 150 per 1,000 under early civil registration in the late 1830s, infant mortality probably dropped significantly during the eighteenth and early nineteenth centuries. However, it is too early to reach firm conclusions about the overall direction of this type of mortality; further research is needed on the registration reliability of other reconstitution parish registers.

The uncertain reliability of parish registers increases the value of other forms of evidence on mortality during the seventeenth and eighteenth centuries. Nearly all of these data concern adult mortality. In a 1974 article on parental loss, Peter Laslett commented on an apparent decline in the number of orphans in the seventeenth and eighteenth centuries. Community surveys of eleven localities taken between 1500 and 1706 revealed a

[38] Wrigley and Schofield, "English population history", *op. cit.*, p. 179.
[39] *Ibid*, p. 177.

median of 25 per cent (with a mean of 22.5 per cent), whereas eight surveyed between 1724 and 1811 had a median of 16.5 per cent (with a mean of 15.9 per cent). Laslett concluded that the decline in the number of orphans probably "arose because of shifts in demographic rates, particularly in mortality".[40]

Of the communities Laslett studied, perhaps the most famous is Clayworth, in Nottinghamshire. The disappearance of large numbers of people in this community between 1676 and 1688 was used to illustrate the high level of mobility at that time. What has not been sufficiently realized is that in the case of adult heads of household, most of them disappeared through death rather than migration. Of 95 heads of household living in Clayworth in 1676, 44 were no longer living in the parish in 1688 – 10 may have left through migration, but the remaining 34 died between the two censuses.[41] Allowing for the effects of migration, those 34 deaths represent a mortality rate of 3.05 per cent per annum – over twice the 1.39 per cent adult mortality rate found in England under civil registration 150 years later.[42]

In his discussion of orphans, Laslett quoted the civil marriage returns for the Manchester area in the 1650s, which recorded the father's name, parish of residence, and father's mortality status. Using these data, it is possible to calculate the mortality rate of fathers. Of 380 spinsters married in the Manchester area between 1654 and 1600, the fathers of 226 were dead at the time of their marriage. That is, the fathers of 59.5 per cent of these women were dead.[43] Assuming an average age at first marriage for women of about 23, this represents an annual mortality rate of fathers of 2.59 per cent per annum, well above the figure found in early civil registration. The fathers of these women marrying in Manchester came from all parts of Lancashire as well as from other northern counties. There appears to have been little variation in mortality between different areas

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[40] Peter Laslett, "Parental deprivation in the past: a note on the history of orphans in England", *Local Population Studies*, No. 13 (Autumn, 1974), p. 15.

[41] Peter Laslett and John Harrison, "Clayworth and Cogenhoe", in H. E. Bell and R. L. Ollard (eds.), *Historical Essays 1600–1750 Presented To David Ogg* (1963), pp. 157–184, p. 15.

[42] *Registrar-General's Ninth Annual Report* (1849), Appendix.

[43] These figures were calculated from all marriages listed in the marriage register between 1654 and 1660. See the *Manchester Cathedral Parish Register*.

within Lancashire. Of the 49 fathers who came from Manchester itself, 61.2 per cent were dead at the time of their daughter's marriage – a proportion close to that for the whole sample covering all areas. (Evidence from tontines, marriage licences and other material suggests that the urban–rural gradient post-dates the seventeenth century.)

This evidence suggests a radical long-term decline in mortality between the seventeenth and nineteenth centuries. It also fits traditional ideas of a high mortality rate in the pre-industrial era. However, it is at variance with the Cambridge Group's reconstitution work on adult mortality, which found only a very modest rise of about three years in life expectancy for men at age 30 during the 250 years between 1550 and 1799.[44] Most of the problems associated with the reconstitution of marriage ages – unreliable parish registers, sociologically unrepresentative samples, and the technically distorting effects of migration – are also applicable to the study of adult mortality. With the adult mortality cohorts there is the additional problem of very small sample sizes. For example, approximately 21.5 per cent of all females born in Colyton between 1560 and 1646 were included in the adult mortality cohort, with equivalent figures for 1720 to 1769 and 1770 to 1837 of 12.5 per cent and 15.5. per cent.[45] In other words, in some instances the Cambridge Group's mortality cohort was derived from only an eighth of the total population. Reliable conclusions about mortality cannot safely be based on such small samples.

There is, however, another source of information that allows a provisional assessment of adult mortality over the 300-year period between the sixteenth and eighteenth centuries: marriage licences. The licences issued in the Diocese of Canterbury are of particularly good quality and run continuously (except for the interregnum period of 1646 to 1660) from 1568 through to 1809 and beyond. The diocese covers the East Kent region and includes 289 parishes. Seventeenth-century marriage licences record information on the parents of bachelors and spinsters at all ages, particularly

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[44] Wrigley and Schofield, *The Population History*, p. 250.

[45] Insufficient evidence has been published to calculate exact figures, but for Colyton approximately half of the initial cohort of married women was included in the mortality sample: applying that ratio to the proportion of females included in the marriage sample yields the figures quoted in the text. See E. A. Wrigley, "Mortality in pre-industrial England: the example of Colyton, Devon, over three centuries", *Daedalus*, Vol. 97 (1968).

for young women. By canon law, the consent of parents or guardians was required before a marriage licence could be granted: those marrying under 21 had to provide it in writing or in the form of a sworn affidavit.[46]

The allegations attached to the licences issued from 1619 to 1646 and from 1661 to 1676 nearly always refer to parental consent, particularly in the former period: over 96 per cent of licences give information on parental consent between 1619 and 1646. The richness of this information allows us to examine whether a father or parents were alive or dead for virtually all those marrying by license in that period: 42.4 per cent of the total population. The licences give information on age and occupation, which allows a study of both of those variables. Table 5 summarises an analysis of parental mortality by age for a sample of 1,000 individuals.

Table 5. Parental Mortality by Age of Daughter in East Kent, 1619–1646[47]

Age of Daughter	*Number in Sample*	*Father Alive, Consenting (%)*	*Father Dead, Mother Consenting (%)*	*Both Parents Dead (%)*
16–20	280	58.2	23.2	18.6
21–25	484	42.1	23.1	34.7
26–30+	236	26.7	25.0	48.3
Total	1000	43.0	23.6	33.4

This table reveals a high level of parental mortality: a third of women had lost both parents by the time of their marriage, a figure that increased to

[46] Steel, *op. cit.*, pp. 226–268.

[47] In preparing Table 5, I adopted the following rules: (i) if a father was listed as giving his consent, he was assumed to be alive; (ii) if a father was not mentioned, and a mother was stated as giving her consent, the father was assumed to be dead and the mother alive; and (iii) if a guardian was listed as giving consent, both parents were assumed to be dead. In the majority of cases, particularly during the earlier periods, information is given directly on mortality status of parents – for example, a mother giving consent is recorded as a widow of a lately deceased husband, or both parents are recorded as being dead. For the source material for this table see: J. M. Cowper (ed.), *Canterbury Marriage Licences* (1892, 1894, 1896, 1905 and 1906); and Arthur J. Willis (ed.), *Canterbury Marriage Licences* (1967, 1969 and 1971).

48.3 per cent for women aged 26 and above. So nearly one-half of women had lost both parents by their late twenties. In seventeenth-century Kent, only a minority of women – 43 per cent – had two living parents at the time of their marriage. These figures speak for themselves: adult mortality was very high in this period.

We can calculate the adult mortality rate of fathers by dividing the numbers dead by the average age of their daughters. Fully 57 per cent of all fathers were dead at the time of their daughter's marriage, and they had died during a 23-year period (the average age at marriage of their daughters). This yields an annual mortality rate of 2.48 per cent per annum, almost identical to that found from the Manchester marriage register for the period of 1654 to 1660. These fathers probably died over a fairly even period between the birth and marriage of their daughters: a small sample of 35 cases in which the date of death was given indicates that on average fathers died 10.6 years before the date of their daughter's marriage.

The long-term change in mortality can be measured by comparing these figures with those compiled under civil registration 200 years later. Among men living in Kent of roughly the equivalent age group – between 30 and 55 – mortality was virtually halved between the early seventeenth and early nineteenth centuries: from 2.48 per cent in 1619 to 1646 down to 1.31 in 1838 to 1844.[48]

The chronology of change in the pattern of mortality among the marriage licence population can be traced through an analysis of the marriages of all women marrying under the age of 21. This information is available in the Diocese of Canterbury for all periods except between 1701 and 1750. Table 6 depicts the exact chronology of decline in mortality.

[48] See *Registrar-General's Ninth Annual Report* (1849), Appendix, pp. 17–20.

Table 6. Mortality Amongst Parents of Spinsters Marrying Under Twenty-One by License in East Kent[49]

Period	*Father Alive, Consenting (%)*	*Father Dead, Mother Consenting (%)*	*Both Parents Dead (%)*	*Total Number in Cohort*
1619–1646	53.33	27.06	19.61	1,275
1661–1676	55.70	25.23	19.07	515
1677–1700	58.86	19.82	21.32	333
1751–1779	74.29	21.00	4.29	700
1780–1809	76.89	17.68	5.43	1,233

This table suggests a marked reduction in adult mortality from the mid-seventeenth to the mid-eighteenth century. The proportion of cases in which both parents were dead dropped particularly sharply: from 21.32 per cent in 1677–1700 to 4.29 per cent in 1751–1779. This was matched by the fall in the percentage of fathers dead – from 46.67 per cent to 25.71 per cent – representing a fall in mortality, all else being equal of 44.9 per cent. The reduction in mortality appears to have commenced after the 1660s, though the changes in the late seventeenth century appear to have been relatively slight. The main fall in mortality seems to have occurred between the end of the seventeenth and the middle of the eighteenth century.

For the earlier periods, information is invariably given in the Kent licences on the occupation of both husbands and living fathers, though not usually for fathers who had died. This allows an occupational analysis of mortality, and Table 7 illustrates what is possible in this respect.

[49] For the source material for this table, see Cowper, *Canterbury Marriage Licences* (1892, 1894, 1896, 1905 and 1906); and Willis, *Canterbury Marriage Licences* (1967, 1969 and 1971).

Table 7. Mortality Among Parents of Spinsters Marrying Under 21 by Occupation of Husband in East Kent, 1619–1809[50]

Occupation by Period	*Father Alive, Consenting (%)*	*Father Dead, Mother Consenting (%)*	*Both Parents Dead (%)*	*Number in Cohort*
Gentlemen and professionals				
1619–1646	60.49	16.10	23.41	205
1661–1700	61.83	19.85	18.31	131
1751–1809	72.33	20.12	7.55	159
Total	64.65	18.38	16.97	495
Yeomen and farmers				
1619–1646	58.76	25.18	16.06	274
1661–1700	57.99	15.98	26.03	169
1751–1809	84.54	12.08	3.08	207
Total	66.77	18.62	14.62	650
Husbandmen				
1619–1646	49.77	29.58	20.66	213
1661–1700	60.66	22.95	16.30	122
1751–1809	80.56	16.67	2.78	108
Total	60.27	24.60	15.12	443
Artisans and tradesmen				
1619–1646	54.18	28.48	17.92	491
1661–1700	50.61	29.45	19.94	326
1751–1809	74.31	20.40	5.29	397
Total	59.80	25.86	14.33	1214
Mariners and fishermen				
1619–1646	58.33	25.69	15.97	144
1661–1700	55.34	29.13	15.53	103
1751–1809	75.95	22.15	1.90	158
Total	64.44	25.19	10.37	405

Overall, there is little correlation between husbands' occupations and parental mortality – except in the earlier period, which shows a lower rate for gentlemen and a higher one for husbandmen, with a slightly higher mortality for gentlemen in the later period.

[50] For sources, see *Ibid.*

Although labourers and the unemployed are not covered by Table 7, groups such as husbandmen and fishermen were characterised by similar level of income and were certainly very much poorer than gentlemen and yeomen farmers.[51] The higher mortality among husbandmen indicates that economic forces may have been a factor in shaping mortality patterns in the earlier period. However, the fact that there were very substantial increases in life expectancy among all occupational groups during the eighteenth century suggests that economic factors were not primarily responsible for the reduction in mortality. For the later period we have information on a number of labouring families: of 91 women under the age of 21 marrying labourers in East Kent from 1751 to 1809, 83.52 per cent had fathers living at the time of their marriage – a figure second only to that of yeomen in the proportion of fathers still living. This finding is consistent with those on occupational mortality in the nineteenth century: labourers in agricultural counties in the post-1860 period had one of the lowest mortality rates recorded.[52]

Although no other reliable evidence covering the general population exists, a variety of information is available on special groups, which allows a supplemental assessment of changing mortality. One of the most accurate forms of data available is on Members of Parliament. Biographical information on MPs exists for the period from 1660 to 1820, except for 1691 to 1714. Date of birth, entry, and death to the nearest year is known for 94.58 per cent of the 5,995 MPs who first entered Parliament in 1660–90 and 1715–1820 – an unrivalled level of demographic accuracy for the period.[53]

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[51] Gregory King estimated that the average income of "common seamen" was £20 per annum, not significantly greater than that of "labouring people and out servants" (£15 per annum). See Gregory King, *Natural And Political Observations*, pp. 48, 49.

[52] See Michael Haines, "Conditions of work and mortality decline", in R. S. Schofield *et. al.* (eds.) *The Decline Of Mortality In Europe* (1991), p. 183. According to the East Kent licence date, all rural occupational groups – yeomen, husbandmen and labourers – had a lower parental mortality than the more urban ones in the late eighteenth century.

[53] See Basil Duke Henning (ed.), *House Of Commons, 1660–1690* (1983); Romney Sedgwick, *House Of Commons, 1715–1754* (1970); Lewis Namier and John Brooke, *House Of Commons, 1754–90* (1964); and R. G. Thorne, *House Of Commons, 1790–1820* (1986). The proportion of total cases with information on birth, entry and death by period are as follows: for 1660–1789, 95.7 per cent; for 1715–1754, 89.4 per cent; for 1755–1789, 95.8 per cent; and for 1790–1820, 98.2 per cent. A special study of these data is in process, but the preliminary findings are presented in Table 8.

Table 8. Average Number of Years Lived, Members of Parliament, 1660–1820, by Age at First Entry[54]

	Average Number of Years Lived After Entry into Parliament		
Date of First Entry	*Aged Under 29*	*30–39*	*40+*
1660–1690	25.71 (429)	22.58 (458)	17.87 (633)
1715–1754	30.83 (541)	28.17 (422)	18.52 (347)
1755–1789	37.13 (480)	29.86 (354)	21.16 (431)
1790–1820	38.06 (571)	32.04 (432)	22.40 (572)

There were sharp gains in life expectancy between 1660 to 1690 and 1715 to 1754, particularly for the younger age groups (under the age of 39). Mortality continued to fall during the later period, but was confined to MPs under the age of 29, and mainly for the period between 1715–1754 and 1755–1789.

The finding of a significant fall in mortality during the first half of the eighteenth century is supported by a number of existing studies. Perhaps the most important – and most neglected – is a study of government appointments made by John Finlaison, the actuary to the National Debt Office, which was published in 1829. Finlaison's data derived from four tontines run by the British government in the eighteenth century. A tontine was a device to raise revenue; it involved the payment of annuities to subscribers based on the survival of their nominees. Subscribers buying tontine shares were allowed to nominate whomever they wished. Most of them nominated themselves or, more frequently, their children. The annuity paid out by the government depended on the survival of individual nominees – survivors shared a fixed annuity sum among themselves – and their deaths were monitored by the Exchequer until the last nominee died, in very old age. For example, the last survivor of the 1693 tontine died in 1783.

Although a self-selected group, the subscribers came from all parts of the country and there is evidence that they were demographically representative of the social groups from which they originated.[55] The

[54] Calculations are to the nearest year and include only cases with full information on date of birth, first entry, and death. Figures in parentheses indicate number of cases.

[55] In the 1789 tontine, the government nominated over half of the nominees by lot, and their mortality rates were similar to that of the nominees of the subscribers. See John Finlaison, *Report On Life Annuities* (Parliamentary Papers, 1829, 3), pp. 7, 66, 67.

subscribers to the tontines were a mixture of aristocracy, gentry, merchants, and professional people, and though this was a limited social range, the precision and accuracy of the data helps counter-balance that limitation.[56] The smallest number of nominees was for the 1693 tontine (just over one thousand), but the numbers grew progressively throughout the eighteenth century. Table 9 summarises the mortality experience of the four tontines.

Table 9. Mortality Rates per 1,000 of all Nominees to British Tontines, 1693–1789[57]

	Date of Tontine			
Age Group	*1693*	*1745+*	*1773*	*1789*
5–15	9.12	5.65	5.75	6.75
16–30	18.44	9.27	10.32	10.14
31–45	20.21	12.61	11.88	11.05
46–60	31.57	22.93	17.09	18.57
61–75	66.09	66.81	51.89	77.39

There were marked falls in mortality among all age groups under the age of 60, most of which occurred between the first two tontines. For example, mortality among the 16–30 age group almost exactly halved between the 1693 and 1745 tontines. A majority of the nominees entered the tontines as children, though the survivors went on to be included in mortality calculations for the later age groups. The pattern of mortality revealed by the tontine data indicates that most of the reduction in mortality occurred in the first half of the eighteenth century.

A number of more recent studies confirm the above conclusion. Table 10 brings together all the available evidence, expressed in the form of male life expectancy at 25 years of age. The data are arranged in the sequence in which they were published.

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[56] In 1693 the proportion of subscribers listed as gentlemen (including aristocrats) was 59.1 per cent; professionals, 11.2 per cent; and merchants and others 29.7 per cent. The equivalent proportions in 1745 were 56.8, 10.5 and 32.7 per cent respectively. See *The British State Tontine of 1693*; and F. Leeson, *Guide to the British State Tontines* (1964), p. 7

[57] See John Finlaison, *Report on Life Annuities* (Parliamentary Papers, 1829, 3), pp. 66, 67.

Table 10. Expectation of Life (Years) for Males Aged 25 Years[58]

	Approximate Period				
	1600–1649	*1650–1699*	*1700–1749*	*1750–1800*	*1800–1824*
Social Group					
Tontine Nominees	—	28.0	34.5	36.4	—
Aristocracy	25.4	26.9	31.8	36.4	37.2
Reconstitution Sample	32.9	31.4	33.6	35.4	—
South of England Quakers	26.1	27.6	31.7	31.5	—
Scottish Advocates	28.8	31.1	38.0	38.1	—
Fathers Listed in Marriage Licences	26.9	28.6	—	37.9	—
Members of Parliament	—	25.7	30.8	37.1	38.0

The overall finding is that with the exception of the reconstitution sample and South of England Quakers, there was an increase in adult life

[58] These figures were prepared with the help of Jim Oeppen. In the case of the marriage licences, it was assumed that: (i) the average newborn child had a mother aged 32 and a father aged 35; (ii) the average child was aged 20 at marriage; (iii) Model North in Coale and Demeny was used for translating survivorship between the ages of 32 and 52 for women (35 and 55 for men) into expectation of life at age 25. For the reconstitution sample and the Quakers, conversion was made to expectation of life at age 25 by using the relationship between expectation of life at ages 25 and 30 in Coale and Demeny Model North life tables. More details can be obtained from Jim Oeppen at The Cambridge Group. Sources: The figures for: tontines, Finlaison, *Report On Life Annuities*; for the aristocracy T. H. Hollingsworth, "The demography of the English Peerage", *Population Studies*, Vol. 18, No. 2 (Supplement, 1964), p. 56; for the reconstitution sample (men aged 30) Wrigley and Schofield, *The Population History*, p. 250; for the southern Quakers (men aged 25–30) Richard Vann and David Eversley, *Friends In Life And Death* (1992), p. 229; for Scottish advocates Rab Houston, "Mortality in early modern Scotland", *Continuity And Change*, Vol. 7 (1992), p. 51; for fathers in marriage licences, data in this paper. For Members of Parliament the figures used are those listed in Table 8 of this article; they include MPs aged under 29 when entering Parliament.

expectancy between the seventeenth and eighteenth centuries of about ten years. Table 10 shows that the increase occurred throughout the whole eighteenth century, though the earlier and more detailed analysis revealed particularly sharp gains at its beginning. Whether this fall in mortality was sufficient to account for the whole of population growth is a question that can only be answered by further research.[59]

EXPLANATIONS OF THE FALL IN MORTALITY

What were the reasons for this radical decline in adult mortality? I have previously argued that smallpox inoculation made a significant impact on mortality in the late eighteenth century. In rural areas, where the majority of the population lived, this would have led to a reduction in adult mortality as well as child mortality, in spite of a significant increase in the virulence of the disease.[60] The data for Members of Parliament, the aristocracy, and the Quakers indicate a strong increase in life expectancy after 1750, which could be accounted for by the practice of inoculation during that time. However, smallpox inoculation was not practised on any scale in the first half of the eighteenth century and therefore cannot account for the marked fall in mortality found then. It is therefore necessary to consider other explanations for that period.

Real incomes probably rose for most of the population during the first half of the eighteenth century,[61] so it is possible that this improvement played a part in reducing mortality. Certainly the evidence of higher mortality among husbandmen in the early seventeenth century would suggest that economic factors were important during this early period, but

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[59] A ten-year increase in expectation of life at birth would be more than adequate to explain population growth between 1695 and 1841, assuming that fertility was high during the eighteenth century. Given that the marriage licences indicate a low age at first marriage of women in the last seventeenth century, this assumption is not unrealistic. The development of a model of population change reflecting the mortality changes discussed in this paper, is one of the priorities of future research. I am grateful to Jim Oeppen for commenting on the implication of the changes in mortality for population growth.

[60] Peter Razzell, *The Conquest Of Smallpox* (1977).

[61] Wrigley and Schofield, *The Population History*, p. 643.

the weight of evidence suggests that they were not central in bringing about the overall fall in mortality. The substantial mortality gains among all the socio-economic groups discussed in this article indicate that non-economic forces were of primary importance, but only further research will definitively settle this issue.

It is possible that there was a spontaneous decline in the severity of various diseases at the end of the seventeenth century. However, there is no evidence for this; smallpox, for example, was increasing in virulence throughout the eighteenth century. Certain changes in the environment associated with economic development may have played a role in reducing mortality; there is good evidence that malaria was present in the marshlands of south-eastern England, and the draining and enclosure of those areas may have reducing mortality.[62] However, the disease was probably confined to restricted areas of the country.

We can provisionally explore one hypothesis that fits all the known evidence: that the main fall in mortality during the early eighteenth century occurred because of the marked improvement in domestic hygiene associated with the rebuilding of English housing at that time. It was linked with a move away from older building materials – in particular, earthen floors. Such floors had been commonplace since medieval times in the houses of rich and poor alike. In the seventeenth century, according to M. W. Barley, even among the clergy, "Earth floors were almost universal; even if suitable stone was available locally for flagging the hall, the service room still had earth floors throughout this period ... The use of brick for paving, as for infilling, belongs to the period after 1660."[63] In their history of English housing Bill Breckon and Jeffrey Parker drew his attention to a neglected, if colourful, area of social history:

> Up to the 18th century ... the ground floor of the house was simply beaten earth ... dusty and strewn with straw, rushes or grasses ... [with] some nastiness seeping into the floors, not only from dog and cat excrement but with human urine as well, for our ancestors were not too bothered about sanitation. Whatever its source, the result was

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[62] Mary Dobson, "The last hiccup of the old demographic regime: population stagnation and decline in late seventeenth- and early eighteenth-century south-east England". *Continuity And Change*, Vol. 4, No. 3 (1989), p. 413.

[63] M. W. Barley, "Rural housing in England", in Joan Thirsk (ed.) *The Agrarian History Of England And Wales* (1967), Vol. 4, p. 727.

> that the floors soaked up material rich in nitre – the 'saltpetre' used in making gunpowder. Since this was scarce, the Crown turned to floors as a rich source of much-needed war material, and empowered 'saltpetre men' to enter people's homes, dig up and take away their floors.[64]

The demand for saltpetre for the manufacture of gunpowder was of such critical importance, that these men were allowed to dig up the floors of bedrooms, halls, butteries and other rooms in the house, as well as the floors of churches, town halls, pigeon lofts, and stables.[65] This activity created passionate opposition, particularly when it involved the digging up of earth under the beds of invalids, pregnant women, and old people.[66] Some householders managed to avoid having their houses disturbed by bribing the government's men. However, the importance of the extraction of saltpetre from houses from our point of view is that it indicates the highly unhygienic state of the floors of many English houses in the seventeenth century. The "powers of seisin" of the saltpetre men were revoked in 1656, although the practice of using house floors as a source of saltpetre seems to have continued until the end of the seventeenth century, when its importation by the East India Company made the practice redundant.[67]

Barley gives a detailed account of the history of farmhouses and cottages, in which earthen floors persisted until the early eighteenth century. Church records for Lincolnshire and Bedfordshire reveal that in parsons' houses during Queen Anne's reign

> Earthen floors were still very much the rule rather than the exception ... some houses could be found with nothing else ... The next best thing was brick, and about half of the Lincolnshire houses had one room so paved ... usually the hall. In Bedfordshire the majority of halls were paved, and so were about half the kitchens.[68]

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[64] Bill Breckon and Jeffrey Parker, *Tracing The History Of Houses* (1991), pp. 135–36.

[65] E. A. B. Hodgetts, *The Rise And Progress Of The British Explosives Industry* (1909), pp. 12–28, 213–300.

[66] *Ibid.*

[67] See William Clarke, *The Natural History Of Nitre* (1670), p. 21, for a reference to the continuation of the practice after the 1656 legislation.

[68] M. W. Barley, *The English Farmhouse And Cottage* (1961), p. 258.

The persistence of earthen floors into the late seventeenth century perhaps explains some unsanitary practices of the aristocracy during this period. When Charles II and his court spent the summer of 1665 in Oxford to escape the plague, they were castigated by the diarist Anthony Wood: "Though they were neat and gay in their apparell, yet they were very nasty and beastly, leaving at their departure all their excrements in every corner, in chimneys, studies, colehouses, cellars".[69] That such unhygienic practices were commonplace is suggested by Pepys's diary; he himself used a chimney for not dissimilar purposes.[70] This behaviour was probably due to the absence of toilets in some houses, even those of the rich, until the eighteenth century.[71]

Barley's work suggests that earthen floors were gradually replaced as brick was widely introduced for domestic house building, a process triggered by the great town fires that swept through England during the late seventeenth and early eighteenth centuries. The timing of the process of rebuilding in brick and tile coincides with the early eighteenth-century decline of adult mortality previously discussed.[72] This rebuilding of houses appears to have enabled a revolution in domestic hygiene to take place. As early as 1727 De Saussure could write:

> The amount of water English people employ is inconceivable, especially for the cleansing of their houses. Though they are not slaves to cleanliness, like the Dutch, still they are very remarkable for this virtue. Not a week passes by but well-kept houses are washed twice in every seven days, and that from top to bottom; and every morning most kitchens, staircase, and entrance are scrubbed. All furniture, and especially all kitchen utensils, are kept with the greatest cleanliness.[73]

Whether this account was true of just London or the whole country is open to question, but certainly the eighteenth-century English acquired a repu-

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[69] Quoted in Lawrence Wright, *Clean And Decent* (1960), p. 76.

[70] Christopher Hibbert, *The English: A Social History* (1987), p. 335.

[71] *Ibid*, pp. 196, 335.

[72] E. L. Jones and M. E. Falkus, "Urban improvement and the English economy", in Peter Borsay (ed.) *The Eighteenth-Century Town, 1688–1820* (1990), pp. 120, 145, 146.

[73] Caesar De Saussure, *A Foreign View Of England* (1902), p. 157.

tation for domestic cleanliness that was reflected in the writings of other foreign visitors.[74]

CONCLUSION

The growth of population in eighteenth-century England was primarily due to a fall in mortality, which was particularly marked during the first half of the century. The fall appears to have affected all socio-economic groups and does not seem to be explained by economic improvements. The introduction of smallpox inoculation contributed to the phenomenon, but the major hypothesis considered here is that there was a very significant improvement in domestic hygiene linked with the rebuilding of housing in brick and stone, triggered by the great town fires that swept England in the late seventeenth and early eighteenth centuries.

The population growth that resulted from falling mortality had profound consequences for both the economy and the social structure of England. It both stimulated demand through increasing the number of consumers and transformed the organisation of production because of the impact of surplus labour. These economic changes culminated in the industrial revolution, which was accompanied by a polarization of wealth. But even this polarization was strongly influenced by population growth, since the impoverished surplus labour force was largely created by the rapid increase in population.

This article poses major questions about population, economy and society. More research is required before authoritative conclusions can be reached, particularly about the causes of population growth. Research using local censuses, parish registers, and marriage licences will allow an analysis of variations in mortality by town and region and of changes over time. Additionally, detailed work will have to be undertaken on the history of hygiene and its impact on health and illness. Only when this research has been undertaken – which is likely to constitute a major project over a number of years – will be possible definitively to explain population growth in eighteenth-century England.

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[74] Francesca Wilson, *Strange Island* (1955), pp. 119, 125, 129.

Chapter 8

Recent Research on Eighteenth-Century Population History

This essay summarises my latest thinking and research findings on eighteenth-century English population history. It includes a review of both the work of other scholars and some of my own recent unpublished research findings. Two subjects are covered: (1) The discussion of the reliability of parish burial registers. (2) The explanation of the decline in eighteenth-century mortality. The conclusion of this review is that there is still great uncertainty about the nature and explanation of eighteenth-century population growth, and that the successful resolution of this conundrum is likely to involve a range of different explanations of mortality decline.

In this concluding essay, I wish to address two major topics which have a direct bearing on the themes dealt with in this book: the issue of parish register reliability, and the explanation of eighteenth-century population growth. The main reason for the uncertainty about the increase of population is the unknown quality of the basic source material, parish registers. It was on the basis of parish register evidence, that historians came to believe that population growth accelerated in the latter half of the eighteenth century. My own recent research suggests that this chronology is incorrect. Evidence from the study of groups with reliable non-parish register information – the aristocracy, members of parliament, subscribers to tontines, fathers of brides marrying by marriage licence, and Scottish lawyers – shows that mortality began to fall sharply at the *beginning* of the eighteenth century, and probably decreased throughout the rest of the century. This evidence contradicts that generated by the Cambridge Group, based on back projection and family reconstitution, which showed only an insignificant fall in mortality in the eighteenth century. The detailed reasons for this

discrepancy have been discussed earlier in the book, but in this concluding essay I will focus on the central issue of burial register reliability.

Wrigley and Schofield have assumed in their reconstitution work that parish registers were 100 per cent accurate during the period 1538–1837. They have made this assumption partly on the basis of selecting what they believed to be reliable parish registers. For their aggregative work, they have generated burial inflation ratios through various statistical calculations.[1] On the basis of these calculations, Wrigley and Scholfield have assumed that about 10 per cent of burials were missing from parish registers up to the end of the eighteenth century, increasing to 26 per cent by the early nineteenth century.[2] These assumptions are clearly critical, because any change in them would have an effect on the pattern of mortality.

In order to evaluate these assumptions, it is necessary to analyse all sources of information: parish registers, bishops' transcripts, monumental inscriptions, wills, poor law returns, local censuses, apprenticeship documents, and manorial records. It is only through the "triangulation" of different sources that reliable results can be achieved. I will illustrate what is possible in this respect by quoting provisional results of a comparison of wills with burial registers.

There are two aspects to burial registration reliability: the recording of burials generally, and the registration of individuals in a particular parish. The first affects both back projection and family reconstitution, whereas the second only affects family reconstitution. This is because back projection relies on aggregative data which covers groups of individuals not necessarily linked in any way. With family reconstitution, links are made between events associated with the same individual – for example a person being traced in both the baptism and burial register. It is assumed in family

[1] It is impossible to summarise the nature of the calculations here, the reader is advised to read E. A. Wrigley and R. S. Schofield, *The Population History Of England* (1981), for the source of these calculations. I have given a critical discussion of the inflations adopted for baptisms in the previous chapter, and similar considerations would apply to burial inflations.

[2] The exact figures are: 1538–99: 9.8%; 1600–49: 8.7%; 1650–99: 10.6%; 1700–49: 5.7%; 1750–99: 10.3%; 1800–38: 26.0%. I have calculated these percentages by multiplying initial and secondary sources of burial under registration listed in Wrigley and Schofield, *op. cit.*

reconstitution that all events take place in the parish of residence, so if an individual was baptised in his family's parish of residence, but buried outside it, he would not be registered in reconstituted mortality statistics. We therefore need to check both the general unreliability of the recording process, and the extent of the practice of burying individuals outside their parish of residence – the "traffic in corpses".

We are fortunate in having a source of data which allows a check on both forms of registration unreliability. The Birmingham and Midland Society for Genealogy and Heraldry have recently completed an index of burials in all Staffordshire parish registers for the period 1538 to 1837 – about one million entries. Ten parishes located centrally in the county were chosen for study, each having surviving wills and burial registers for the period covered. 200 wills were selected in sequence from the probate lists – 20 from each parish – and were chosen to cover the three-hundred period involved. A search was made for the burials of this will-leaving population, both in the Staffordshire burial index and the individual parish registers.[3] Table 1 presents the results of this search, giving the proportions of burials traced in each parish.[4]

[3] The search was made for a ten-year period prior to the date of probate in the burial index, and five years in the parish registers. The index search was kindly conducted by Tony Bowers of the B. M. S. G. H., while I carried out the parish register search.

[4] The criteria for a traced case was that a burial had taken place in the parish of residence less than ten years previous to the date of probate. A conservative procedure was adopted of assuming that all similar names in wills and burial registers during this period were correct matches — this would tend to over-estimate the proportion of traced cases. Most cases were found in the previous year to the probate date – 88.1 per cent – and 69.8 per cent were found less than six months previous to that date. Given uncertainty about correct identification, it was decided to define a traced case occurring in a non-residential parish when a burial took place six months before the date of probate in that parish. The figures for Bushbury had to be adjusted to take account of large gaps in the register before 1662.

Table 1. Numbers of People Leaving Wills Traced in Burial Registers in Staffordshire, 1538–1837

Parish	*Traced*	*Not Traced*
Barlaston	14	6
Caverswell	13	7
Cheadle	15	5
Dilhorne	18	2
Stone	10	10
Bushbury	14	6
Darleston	17	7
Penkridge	13	7
Walsall	15	5
Wednesbury	14	6

There were significant variations in the numbers of burials traced, from a high proportion of 90 per cent in Dilhorne, to a low figure of 50 per cent in Stone. The overall average of burials not traced is 33.5 per cent, suggesting that about a third of all adult burials were not registered in parish registers. Only a small minority of traced burials – 9 out of 143 (6.3 per cent) – were found in neighbouring parishes, indicating that the "traffic in corpses" was not of major significance. (This should not surprise us, as virtually all wills stipulate burial in the parish of residence.)

The proportion of burials traced also varied over time. The following table summarises the changing proportion of untraced burials for the whole sample during the three-hundred year period.

Table 2. Proportions of People Leaving Wills Untraced in Burials Registers, Staffordshire 1538–1837

Period	*Traced*	*Not Traced*	*Total Cases*	*Proportion Not Traced*
1538–1649	31	23	54	42.6%
1650–1749	48	18	66	27.3%
1750–1837	66	14	80	17.5%

This table suggests that burial registration improved significantly during the three-hundred year period, and that it was particularly poor in the late sixteenth and early seventeenth century. However, it is not possible to reach firm conclusions on a sample of 200 cases. What is required is a large-scale study of will/burial registers for a randomly representative

sample of parishes, including information on the dates of making and probating of wills.

In one respect the tracing of the burials of people leaving wills is a mild test of burial registration accuracy. Those who made wills were adults – usually males – who owned property and were of high social status. We would expect families of such people to ensure the registration of their burials, particularly because of the legal implications of property transfers. One way of comparing the burial registration accuracy of the rich and the poor is to check the burials of will-leavers against that of paupers. Many parishes paid for the burial of the poor, leaving detailed records of burial expenses, including the provision of coffins and the carrying of the dead to the grave. Lyn Boothman has made such a comparison for the parish of Long Malford in Suffolk. Of 97 people who left wills in 1559–1610, 20 (20.6%) could not be located in the burial register, compared to 34 of 52 (65.4%) paupers who were buried in a similar period.[5] Lyn Boothman has suggested that the very high burial omission rates amongst the Long Melford poor may have been due to the non-payment of burial fees by poor law authorities.[6]

However, the proportion of unregistered pauper burials was not necessarily disproportionate. The pauper burials in Whitchurch, Oxfordshire and Folkestone, Kent, were checked in the appropriate parish registers, with the following results:

[5] I am grateful to Lyn Boothman for sending me details of the burials of the will-leaving population and the paupers of Long Melford.

[6] See Lyn Boothman, "Letter on Long Melford parish registers", *Local Population Studies,* No. 50, Spring 1993, pp 80, 81.

Table 3: Comparison of Poor Law Records and Parish Registers[7]

Period	*Burials Not Found*	*Total Burials*	*Proportion Burials Not Found*
Whitchurch			
1651–1750	19	93	20.4%
1751–1800	15	68	22.1%
Folkestone			
1732–51	10	57	17.5%
1752–87	6	57	10.5%

The percentages of missing pauper burials in Whitchurch and Folkestone are smaller than that found in Long Melford, although they were still significant – about a fifth in the pre-1751 period. They are in the range of untraced burials found amongst the will-leaving population in Staffordshire during the same period (27.3% for 1650–1749 and 17.5% for 1750–1837). These early finding on the registration of pauper burials suggest an improvement of burial registration over time, although the samples are too fragmentary to allow firm conclusions.

The probable explanation of poor burial registration was given to the Select Committee on Parochial Registration in 1833. One of the witnesses, Mr William Durrant Cooper, a solicitor, had extensive experience of tracing individuals in parish registers for property cases, and concluded that parish registration was "exceedingly defective ... [with] a very large number of marriages, deaths and baptisms ... not entered at all...especially deaths".[8] To illustrate this, he gave the following example:

> On the sale of some property [in 1819] from Mr Cott to Lord Gage, it was necessary to procure evidence of the death of three individuals, Mrs Pace, Mr Tuchnott and Mrs Gouldsmith. They were at different places, all in Sussex; Mrs Pace was regularly entered; Mr Tuchnott was buried at Rodmell, about five miles from Lewes, and on searching for the register of burial we found no entry whatever. On making an inquiry in the churchyard of the sexton, he stated he

[7] I am grateful to Richard Adair for providing the figures for Whitchurch.

[8] *Report Of The Select Committee On Parochial Registration*(Parliamentary Papers, 1833, XIV), p. 24.

> recollected digging the grave, and the ceremony being performed; Mr Gwynne, the rector, whose neglect in that and other parishes is well known, had omitted to enter it ... Mrs Gouldsmith, who was buried at Waldron, in the same county, was not entered, but on going to the parish clerk, who was a blacksmith, he stated he recollected the circumstance, and accounted for her burial not being entered in this way: he said it was usual for him, and not the clergyman, to take an account of those who were buried, and he entered them in a little sixpenny memorandum book ... If it so happened that the fee [of one shilling] was paid at the time, as was the case with affluent persons, no entry would appear in his book, he only booked what was due to him, and as the clergyman entered the parish register at the end of the year from his book, and not at the time of the ceremony, all burials that were not entered in his book would not find their way into the register.[9]

The above account cautions us against the assumption of a necessary correlation between socio-economic status and burial registration. (In Waldron, the rich who paid their burial fees were ironically excluded from the burial register, whereas those parishioners who were lax in payment, were usually registered.) English parish registration was a haphazard process and much more evidence is needed before it is possible to reach general conclusions. However, we can provisionally conclude from existing evidence that many burials were omitted from the parish register because of clerical negligence, a topic further discussed in Chapter 4.

Although the Cambridge Group have attempted to select reliable parish registers for its reconstitution work, in practice problems remain. As we have seen in the last chapter, 28.1 per cent of people leaving wills in Colyton could not be traced in the burial register, and the evidence from the same-name technique also suggested similar problems with the burial registration of infants and children – over 40 per cent of same-name burials could not be traced in the Colyton burial register in the first half of the seventeenth century. Some of this may have been due to the practice of giving same-names to two living siblings, but the evidence is that this only happened on a very limited scale.[10] The evidence from Colyton also

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[9] *Ibid*, p. 25.

[10] I estimate from a provisional study of wills – which give information on living same-name children – that about 15 per cent of all same-name cases at the end of the sixteenth century were two living siblings.

suggested that burial registration amongst children improved over time, particularly during the early nineteenth century.

How typical was Colyton of other parishes in its same-name practices? We have seen that about two-thirds of eligible families (a family was eligible when at least one of its children was baptised after the burial of a sibling of the same sex) gave same-names to their children in Colyton. I have analyzed the reconstitution schedules for the parishes of Dawlish, Eccleshall, Bridford, Austey, March and Aldenham. The following table summarises the proportion of eligible families who gave same-names to their children in these parishes.

Table 4. Number of Families with Same-Names as a Proportion of Eligible Families

Parish	*Number of Families with Same Names*	*Number of Eligible Families*	*Proportion of Families with Same Names*
Dawlish	206	307	67.1%
Eccleshall	268	443	60.5%
Bridford	84	139	60.4%
Austey	102	155	65.8%
March	482	678	71.1%
Aldenham	296	524	56.6%
Colyton	472	733	64.4%
Total	1910	2948	64.8%

Most parishes had at least 60 per cent of its families giving same-names to their children, and the overall average – 64.8% – is almost identical to the Colyton proportion (64.4%). Given that about two-thirds of all families practised same-naming, we can provisionally conclude that the families practising it were representative of the general population.

An analysis of same-name cases in nine reconstitution parishes, suggests that burial registration for infants and children improved over time.

Table 5. The Analysis of Burial Registration of Same-Name Siblings in Colyton, Hartland, Aldenham, Dawlish, Ansty, Bridford, Eccleshall, March, Shepshed, 1538–1837

Period	*Total Same Name Cases*	*Burials Not Found*	*Proportion of Burials Not Found*
1538–99	358	122	34.1%
1600–49	465	144	31.0%
1650–99	617	167	27.1%
1700–49	858	191	22.3%
1750–99	594	160	27.0%
1800–37	451	104	23.1%
1838–50	72	7	9.7%

A number of qualifications need to be made about these figures, and they do not represent a reliable measure of trends in burial registration accuracy. The parishes included in the sample varied from period to period, and there were strong variations from parish to parish which would affect the overall results. Only two parishes – Colyton and Shepshed – were included in the 1838–50, and the small numbers in that period mean that its proportion of missing burials is not a reliable measure of overall burial registration. Also the above figures make no allowance for burial non-registration on account of the delay between baptism and burial (children often not being baptised) – and this will inflate the figures in Table 5. (Although this might be counter-balanced to some extent by the allowance to be made for living same-name children.)

Only detailed research on a substantial number of registers – and using a variety of sources for comparison purposes – will reveal the overall pattern of burial registration reliability. However it is clear from the evidence on wills, pauper records and same-name patterns that burial registration was much more defective in the sixteenth, seventeenth, and early eighteenth centuries, than has been previously thought.

It is likely there were similar problems with baptism as there was with burial registration. Assumptions about proportions of births omitted from baptism registers affect calculation of the number of births and measures of fertility, and this is discussed in Chapter 7. The question of baptism registration adequacy is dealt with in the fourth and fifth essays of this book. The main conclusion of this work – that about a third of all births were omitted from baptism registers from at least the 1760s onwards – is consistent with findings on burial registration. It will be possible to further

evaluate the accuracy of baptism registers by means of the application of the same-name technique to different parish registers, which involves tracing baptisms of dead siblings bearing the same name.

One important topic not covered fully in this book is marriage – a key issue which is central to the Malthusian interpretation of population change. In Chapters 1 and 7, I discuss the question of age at first marriage, but there is virtually no discussion of the propensity to marry. In my first essay I briefly touched on the issue, and concluded that the marriage rate was more-or-less constant during the eighteenth century. There is now some evidence to cast doubt on this conclusion. It is likely that the clerical negligence which occurred with the registration of births and deaths, also applied to the recording of marriages. Lyn Boothman found that many marriages were missing from the Long Melford parish register in the late sixteenth century: 35 marriages were found in the bishops' transcripts but not in the marriage register – about a quarter of the total. Other than the bishops' transcripts, there are few alternative sources of information to parish registers to evaluate the reliability of marriage registers. It is likely that Hardwicke's Act of 1753 improved marriage registration, with more marriages accurately registered after this date. As a result, there were probably more marriages in the early eighteenth century than recorded in the parish registers. This means that statistics of marriage rates based on parish register evidence probably conceal a decline in the propensity to marry which took place in the eighteenth century.

There is virtually no direct evidence on the propensity to marry in the eighteenth century. There is however some data on the propensity to remarry based on widow and widower remarriages. Wrigley and Schofield note in their *Population History of England* that the proportion of widow marriages to all marriages fell from about 30 per cent at the end of the seventeenth century to approximately 10 per cent at the end of the eighteenth.[11] This conclusion was based on fragmentary parish register returns – most registers do not give the marital status of marrying parties – but information in marriage licences from a number of counties points in the same direction. Perhaps the most reliable evidence to illustrate this is that for the Diocese of Canterbury, involving 289 East Kent parishes.

[11] Wrigley and Schofield, *op. cit.*, pp 258, 259.

Table 6. Proportion of Widows and Widowers Married by Licence in the Diocese of Canterbury, 1568–1809[12]

Period	*Total Number Of Marriages*	*Number of Marriages Of Widows*	*Percentage Of Widow Marriages*	*Number of Marriages Of Widowers*	*Percentage Of Widower Marriages*
1568–1618	1,000	323	32.3%	—	—
1619–1646	1,000	318	31.8%	318	31.8%
1661–1676	1,000	283	28.3%	316	31.6%
1677–1700	1,000	261	26.1%	326	32.6%
1701–1725	1,000	197	19.7%	221	22.1%
1726–1750	1,000	216	21.6%	208	20.8%
1751–1780	1,000	152	15.2%	179	17.9%
1781–1809	1,000	120	12.0%	181	18.1%

These figures confirm the very major decline in the proportions of widow and widower remarriages in the eighteenth century – by nearly two-thirds for widows, and a half for widowers. What could be the explanation of this decline?

Evidence exists which suggests that the propensity to remarry was changing sharply during the period. The East Kent licences give information on whether or not a widowed mother had remarried by the time of her daughter's marriage, allowing a calculation of the proportions of widows remarrying. The following table is for mothers of women marrying under the age of 21.

Table 7. Proportion of Widowed Mothers Remarrying in East Kent[13]

Period	*N*	*Numbers Remarried*	*Proportion Remarried*
1619–1646	100	49	49%
1661–1676	72	37	51%
1751–1780	100	10	10%
1781–1810	100	9	9%

This table reveals that the proportion of remarried widowed mothers at the

[12] These figures are derived from J. M. Cowper (ed.), *Canterbury Marriage Licences* (1876), (1892), (1894), (1896), (1898), (1905), (1906); and Arthur Willis (ed.), *Canterbury Marriage Licences* (1967), (1969) and (1971).

[13] *Ibid.*

end of the eighteenth century was a *fifth* of what it had been at the end of the seventeenth. It therefore indicates a radical decline in the propensity to remarry. This finding is consistent with the evidence on the declining proportion of widow marriages in Table 6. The explanation of this change is beyond the scope of this chapter; it may be that there were fewer opportunities for remarriage as a result of declining mortality and a reduction in the number of widowers, and that the declining proportions of widow and widower marriages were a function of the fall in mortality depicted elsewhere in the book.

Clearly, some very fundamental changes in the structure of marriage were occurring in the eighteenth century. The factors involved are likely to be complex. There is some evidence that the declining propensity to remarry was associated with a general decline in the tendency to marry. The following table offers a comparison of marital status by age in the 1695 and 1851 Lichfield censuses.

Table 8. Age and Marital Status in Lichfield, 1695 and 1851

(N = Total Number In Age Group,
P = Total Proportion Married Or Widowed)[14]

Age Group	*Period*			
	1695		*1851*	
	N	*P*	*N*	*P,*
15–19	171	0.6%	199	1.0%
20–24	147	15.0%	146	21.2%
25–29	144	50.0%	147	53.7%
30–34	111	77.5%	115	60.9%
35–39	138	84.1%	101	77.2%
40–44	62	95.2%	113	77.9%
45+	274	98.2%	432	81.5%

These figures indicate that there was a fall in the total proportion of women marrying between the end of the seventeenth and middle of the nineteenth centuries. In Lichfield in the 1690s it is clear that almost all women had been married at least once during their lifetime: only 2 per cent remained

[14] The 1695 proportions are derived from figures given by D. V. Glass and D. E. C. Eversley (eds.), *Population In History* (1965), p. 181; the ones for 1851 are based on a 1 in 2 sample of the 1851 census schedules for Lichfield.

unmarried after the age of 45. By comparison, in 1851 the figure was very much higher, rising to nearly 20 per cent.

It is of course not possible to make generalizations on the basis of a single community (although Gregory King believed Lichfield to be demographically typical.) There is some evidence that Lichfield was not unrepresentative of other communities at the end of the seventeenth century: for example, there were no known spinsters among the 69 women over the age of forty-five living in Chilvers Coton, Warwickshire in 1684.[15] However, as we might expect, there were local variations, so that for example 15 out of a total of 161 women over the age of forty-five – 9.2 per cent – living in Stoke-on-Trent, Staffordshire were unmarried in 1701.[16] Also evidence from wills indicates that most people married in the early seventeenth century: of 204 men who left wills in Essex during 1597–1603, only 19 (9.3%) were unmarried at the time of death, even though some of them were clearly young men. When linked with parish registers, information in wills should enable an analysis of the changing proportions of will-leaving men who had married by certain ages during the period 1538–1837.

Additionally, a comprehensive survey of all late seventeenth-century censuses with relevant information on age and marital status should allow a comparison of the marriage/age distributions with the equivalent data for 1851, as in Table 8.

Although fragmentary, the evidence considered above suggests that the propensity to marry and remarry declined in the eighteenth century. This strengthens the argument that it was falling mortality, rather than rising fertility, which was the key factor in eighteenth-century population growth.

II

My work suggests that mortality declined over the whole of the eighteenth and early nineteenth centuries, although the magnitude of the fall and its exact chronology had yet to be determined. Six major factors emerged

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[15] This information was kindly provided by Peter Laslett of the Cambridge Group.
[16] *Ibid.*

from the essays in this book for the explanation for the decline in mortality, which can be listed in approximate chronological order as follows:

1. The improvement of domestic hygiene associated with the rebuilding of houses in brick and tile – in particular the replacement of earth floors with brick, tile and timber flooring during the eighteenth century.
2. The growth in real incomes during the first half of the eighteenth century.
3. The progressive elimination of malaria with the drainage of marshlands associated with agricultural improvements.
4. The practice of smallpox inoculation and vaccination after the middle of the eighteenth century.
5. The replacement of woollen clothing with linens and cottons, allowing more frequent and thorough washing at the end of the eighteenth century.
6. The improvement in personal hygiene associate with the introduction of the water-closet and the bath at the beginning of the nineteenth century.

I will briefly comment on recent research on these various topics, and point to ways in which these hypotheses can be further elaborated. Little or no work has been done on the history of domestic hygiene, and I will return to this subject at the end of this essay.

The history of the standard of living in the eighteenth century is still a matter of controversy. Most evidence suggests that average real incomes were rising during the first half of the eighteenth century[17], and it is possible that this contributed to falling mortality. However, recent work by Nicholas, Steckel and Komlos on the average heights of the working population suggests that there was an overall decline of the standard of living from about 1730 to 1860[18] (although this is a matter

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[17] Wrigley and Schofield, *op. cit.*, p. 643.

[18] S. Nicholas and R. H. Steckel. "Height and health of English workers during the early years of industrialization", *Journal of Economic History*, Vol 51 (1991); John Komlos, "The secular trends in the biological standard of living in the United Kingdom, 1730–1860", *Economic History Review*, Vol 46, No. 1 (1993); John Komlos, "A Malthusian episode revisited: the height of British and Irish servants on colonial America", *Economic History Review*, Vol 46, No. 4 (1993).

of controversy[19]), and it is therefore unlikely that economic factors played a role in improving the expectation of life in this later period. Also, mortality appears to have fallen amongst all socio-economic groups – rich and poor alike – and therefore economic factors, including the per capita consumption of food, are not likely to have been of major importance. Professor Livi-Bacci has recently questioned whether nutrition was a significant factor in historical mortality patterns.[20] It therefore seems doubtful whether improving standards of life and nutrition made more than a very minor contribution to the reduction of mortality.

The history of malaria has been investigated in detail by Dr Mary Dobson, and although her full results have yet to be published, she has presented sufficient of her work to come to certain provisional conclusions. She has demonstrated that there were marked variations in mortality in different south-eastern parishes, with much higher mortality rates in marshland areas.[21] This conclusion can only be tentative at this stage, as Dobson has yet to present any evaluation of burial registration reliability in the different types of parish. It is unclear whether environmental improvements significantly reduced the amount of malaria in England, as the disease was still to be found in marshland areas in the nineteenth century. Also, it is not clear what contribution the elimination of malaria made to the total reduction of mortality, as the disease was confined to specific and limited areas of the country.

In my original article on the impact of smallpox inoculation on population growth – reproduced in Chapter 1 – I claimed that it largely accounted for the increase of population in the eighteenth century. In the light of new evidence considered in this book, this claim was clearly overstated. However, that the impact of inoculation was very significant, is confirmed by J. R. Smith in his book *The Speckled Monster*.[22] Smith concludes that inoculation was very widely practised after 1765, and rightly stresses the importance of general inoculations – the inoculation of

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[19] See R. Floud, K. Wachter and A. Gregory, *Height, Health And History; Nutritional Status In The United Kingdom 1750–1980* (1990)

[20] Massimo Livi-Bacci, *Population And Nutrition* (1991), p. xiii.

[21] Mary Dobson, "The last hiccup of the old demographic regime: population stagnation and decline in late seventeenth and early eighteenth-century south-east England", *Continuity And Change*, Vol. 4, No. 3 (1989).

[22] J. R. Smith, *The Speckled Monster* (1987).

whole parishes – which usually arose when there was a threat of an epidemic. As contemporaries believed inoculation spread smallpox to the unprotected, nearly everyone living in a parish was inoculated. It was in the interest of property owners to pay for the inoculation of the poor, as the cost of nursing and burying smallpox victims was so high. Additionally, in market towns trade was ruined for long periods – sometimes for over a year – on account of smallpox epidemics, and it was in the interest of merchants and traders to ensure the eradication of the disease as soon as possible.

Smith concludes that inoculation probably reduced childhood mortality in the eighteenth century by between 10 and 20 per cent, but cautions against excessive claims on its demographic impact.[23] In the light of the new evidence considered in this book, that conclusion may not be entirely unwarranted, although this issue can only be settled through further research. There are two points which Smith does not fully address: 1. The secondary mortality arising from smallpox. 2. The increasing virulence o the disease. It should be possible to settle the first issue by comparing the subsequent mortality of those catching natural smallpox, as against those who had been inoculated or vaccinated. (This would involve a research strategy very similar to that used by Rutten in his work on a Dutch municipality, to be discussed later.) On the question of the increasing virulence of the disease, we know from a number of surveys that the case-fatality rate of smallpox was about 16.5 per cent in the 1720s but had increased to over forty per cent by the mid-nineteenth century.[24] As smallpox was a universal disease, inoculation and vaccination may have prevented the deaths of up to a third of the population in the nineteenth century.

Smith raises the important point about the practice of inoculation outside of the South of England, suggesting that the technique may have been adopted ten years or so later in the North. We know from the writings of the Chester physician, Haygarth, that it was practised on a large scale in the North, but we have less information on its exact chronology and extent. Recently Deborah Brunton has reviewed the history of inoculation and its demographic impact in Scotland.[25] She presents evidence to show that its practice was more limited than it was in England, and that this was

[23] *Ibid*, p. 67.

[24] Peter Razzell, *The Conquest Of Smallpox* (1977), pp. 126–134.

[25] Deborah Brunton, "Smallpox inoculation and demographic trends in eighteenth-century Scotland", *Medical History*, Vol. 36 (1992).

partly the consequence of the periodicity of smallpox, as well as the persistence of Calvinist religious opposition. In the lowland areas, the disease was more-or-less endemic, spreading continuously through scattered and thinly populated areas, and affecting mainly infants and young children. This engendered a fatalistic attitude similar to that encountered in large towns in England. In the highlands and other areas of Scotland, smallpox tended to strike much more as an epidemic disease affecting all age groups, creating panic and provoking mass inoculations, particularly when the disease took a virulent form. This again was very similar to the experience of the English countryside, where the majority of the population lived.

On the basis of clinical evidence, it had been previously suggested by myself and others that the elimination of smallpox may have increased the overall level of fertility. Willibrod Rutten has established that in at least one nineteenth century Dutch municipality, there was little or no reduction in fertility amongst those men who had suffered a smallpox attack.[26] Given Rutten's new evidence, we can provisionally conclude that the gradual elimination of smallpox did not increase fertility, which is compatible with the findings on fertility discussed in Chapter 7.

Little or no research had been carried out on the question of clothing and personal hygiene since I wrote on those topics in the essay which forms Chapter 6 of this book. Contemporaries believed that the move away from woollens had improved hygiene and health. Gilbert White in his history of the rural parish of Selborne in Hampshire, commented on the abandonment of woollen clothing, improving health as a result. He wrote in 1778: "The use of linen changes, shirts or shifts, in the room of sordid and filthy woollen, long worn next to the skin, is a matter of neatness comparatively modern; but must prove a great means of preventing cutaneous ails".[27] Forty-four years later, Francis Place noted that "the success of the cotton manufactures" had enabled the working classes to "discard the woollen clothes which were universally worn by them, which lasted for years, and were seldom, if ever washed".[28]

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[26] Willibrod Rutten, "Smallpox, sub-fecundity, and sterility: a case study from a nineteenth-century Dutch municipality", *Social History of Medicine*, Vol. 6, No. 1 (1993).

[27] Gilbert White, *The Natural History And Antiquities Of Selborne*, (1789), p. 222.

[28] Francis Place, *Illustrations And Proofs Of The Principle Of Population* (1930), p. 253.

Whether this substitution of linen and cotton for woollen clothing had a significant impact on health is an open question; it is possible that the more frequent washing of clothes reduced body lice, fleas and other parasites, which in turn helped to eliminate dirt diseases such as typhus. These improvements would have been reinforced by the introduction of the bath and water-closet, subjects which also warrant more research attention. This leads naturally to a discussion of domestic hygiene, associated with the rebuilding of housing which began at the end of the seventeenth century. The topic is covered briefly in Chapter 7, and I will attempt now to give a fuller historical account of the subject.

The link between poor domestic hygiene and ill health was made as early as the sixteenth century. In 1517 Erasmus gave his well-known description of English housing and its effect on health.

> I am often surprised and distressed by the question how it can be that England has for so many years now been beset by continual pestilence, and in particular by the sweating sickness, which almost seems to be its speciality. We read somewhere of a city set free from a pestilence of long standing by rearranging the buildings on the advice of a philosopher. Unless I am much mistaken, a similar policy might set England free ... [In the buildings] the floors are generally spread with clay and rushes from some marsh, which are renewed from time to time but so as to leave a basic layer, sometimes for twenty years, under which fester spittle, vomit, dogs' urine and men's too, dregs of beer and cast-off bits of fish, and other unspeakable kinds of filth. As the weather changes, this exhales a sort of miasma which in my opinion is far from conductive to bodily health ... I should be confident that the island would be far healthier if the use of rushes were abolished, if rooms were constructed as to be open to the air of heaven on two or three sides, and all the glass windows were so made that they could be opened fully.[29]

These conditions persisted throughout the sixteenth century, and when the German traveller Paul Hentzner visited England in 1598, he noted that the floor of the "presence chamber" in Greenwich Palace was "strewn with

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[29] R. A. B. Mynors and Alexander Dalzell (eds.), *The Correspondence Of Erasmus* (1992), pp. 471, 472.

hay after the English fashion".[30] Similarly in the following year, Thomas Platter found the flooring in the "queen's quarters" at Hampton Court "strewn with rushes", and went on to describe his visit to the royal palace of Nonesuch:

> We were led very soon into the presence chamber ... [and this] apartment like all the others leading into this one was hung with fine tapestries, and the floor was strewn with straw or hay; only where the queen was to come out and up to her seat were carpets laid down worked in Turkish knot.[31]

Fragments of evidence suggest that conditions were even worse in the houses of the poor. For example, in 1591, the mayor and jurats of Maidstone issued a warning to the people living in the almshouses in Stone Street, Pudding Lane, and on the bridge, that if they continued to "keep hogs or swine in the rooms or houses where they lived", they would each be fined.[32]

Earth floors appear to have been more-or-less universal until the end of the seventeenth century, and we know many of them were in a highly unsanitary condition because they were the source of saltpetre (potassium nitrite), which arose as a result of the exposure of earth to urine and animal manure. Saltpetre was used for the manufacture of gunpowder, and the demand for gunpowder led the crown to license saltpetre men to dig up the floors of bedrooms, halls, butteries and other rooms in houses. These "powers of seisin" were eventually revoked by the Commonwealth Government of 1656, although the house floors continued to be a source of saltpetre until the end of the century. Covered in straw and in a highly unhygienic state, house floors were breeding grounds for fleas, rats and lice, all carriers of human disease. The building of covered floors reduced this population of parasites and was therefore probably a major factor in the improvement of health. It is possible that the elimination of earth floors in basements and lower ground floors in London, resulting from the rebuilding of the city after the great fire of 1666, was partly responsible for the disappearance of the plague.

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[30] Paul Hentzner, *Travels In England In The Year 1598* (1797), p. 33.

[31] Clare Williams (ed), *Thomas Platter's Travels In England in 1599* (1937), pp. 192–195, 202.

[32] J. M. Russel, *History Of Maidstone* (1881), pp. 223, 225.

The only statistical source of information on earth floors is that derived from glebe terriers (surveys of church property), compiled from the seventeenth century onwards. Some of this evidence has been presented by Maurice Barley in his work on English vernacular architecture, and is discussed in Chapter 7. Since Barley's work was published, the glebe terriers for Cornwall have appeared in print, which allows a statistical analysis of flooring in parsonages at the end of the seventeenth and beginning of the eighteenth centuries. In 1679, 50 per cent of the 62 parsonages surveyed had all floors made of earth, whereas by 1727 this proportion had fallen to 14 per cent (12 out of 41). Even more significant was the reduction of the proportion of kitchens with earth floors: 71 per cent (25 out of 35) in 1679, falling to 23 per cent (11 out of 48) by 1727.[33] These changes in the internal conditions of houses co-incided with the decline in mortality at the beginning of the eighteenth century and discussed earlier in the book.

There is insufficient evidence to say whether improvements in the housing of the ordinary population improved in the same way. There were undoubtedly great improvements in urban houses during this period, starting with the rebuilding of London after the great fire of 1666. Jones and Falkus have argued that the rebuilding of houses in brick and tile in early eighteenth-century towns "transmitted near-metropolitan models of a way of life and standards of consumption to almost the whole rural population".[34] We know that this conclusion applies to the housing of clergymen and other elite groups, but little in detail is known about the housing of the poor.

It is known however that earth floors survived into the nineteenth century. A number of the reports published in Edwin Chadwick's survey of sanitary conditions published in 1842 mentioned earth floors. Most references described them to be in a highly unsanitary condition, for as the report from Bedfordshire stated, "the bare ground ... cannot possible be cleaned".[35] The correspondent from Tranent in Scotland found the floors

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[33] Richard Potts (ed.), *A Calendar Of Cornish Glebe Terriers, 1673–1735* (1974).

[34] E. L. Jones and M. E. Falkus, "Urban improvements and the English economy in the seventeenth and eighteenth centuries", in Peter Borsay (ed.), *The Eighteenth-Century Town, 1688–1820* (1990), pp. 120, 145, 146.

[35] Edwin Chadwick, *On An Inquiry Into The Sanitary Condition Of The Labouring Population* (Local Reports, House Of Lords 1842, XXVII), p. 131.

of the colliers' houses to be in an indescribable condition: "These [earth] floors are very dirty, and so uneven as to make a stranger fall ... The odour of these apartments is most offensive and sickening, from the long-continued presence of human impurities. Persons not familiar with such situations will be unable to form the most remote idea of the disgusting nature of the atmosphere; but delicacy forbids a more detailed account".[36]

A report on the cottages of labourers living on the Scottish border explicitly drew a parallel with Erasmus's account more than three hundred years earlier:

> This earth floor ... is one of the causes to which Erasmus ascribed the frequent recurrence of epidemic sickness ... It is not only cold and wet, but contains the aggregate filth of years, from the time of its first being used. The refuse and dropping of meals, decayed animal and vegetable matter of all kinds, which had been cast upon it from mouth and stomach, these all mix together and exude from it.[37]

A similar account was given for parts of Buckinghamshire:

> The cottages of Waddesdon, and some of the surrounding parishes in the Vale of Aylesbury, are constructed of mud, with earth floors and thatched roofs ... The earth of the floor is full of vegetable matter, and from there being nothing to cut off its contact with the surrounding mould, it is peculiarly liable to damp. The floor is frequently charged with animal matter thrown upon it by the inmates, and this rapidly decomposes by the alternative action of heat and moisture ... Fever of every type and diarrhoea are endemic diseases in the parish and neighbourhood.[38]

Earth floors were also reported in Bedfordshire, Devon, Dorset, Lancashire, Monmouthshire, Northumberland, Rutlandshire and Scotland.[39] They were to be found in the basements of the houses of the poor in cities like Liverpool, although "generally ... [the floors were] flagged, and in a few cases, boarded".[40] In some areas, the correspondent gave a contemporary

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[36] *Ibid*, XXVIII, p. 86.
[37] *Ibid*, XXVI, p. 22.
[38] *Ibid*, p. 268.
[39] *Ibid*, XXVI, pp. 6, 8; XXVII, pp. 12, 100, 101, 113, 132, 159, 284, 306, 419.
[40] *Ibid*, XXVII, p. 284

history of housing, including the type of flooring. Mr Chrisp, a farmer from Northumberland wrote:

> The older cottages in the country, such as were generally built by the farmers for his servants, consist of a rough wall of lime and stone, covered with thatch, and with nothing but an earthen floor, except a flag-stone or two near the hearth. Cottages of a superior description have been latterly erected by the landlord when he agrees to build, being covered with blue slate, the side walls plastered, and the floor of stone flagging.[41]

But in some ways the reports in Chadwick's survey are misleading. He had invited medical officers and others to submit details of sanitary problems, rather than objective accounts of typical conditions. This can be illustrated by the report made on Berkshire housing, where the medical officer wished to draw attention to the problem of dampness in cottages:

> In Berkshire the floors of the cottages are laid with red tiles, called 'flats', or with bricks of a remarkable porous quality ... The cleanly housewife, who prides herself on the neat and fresh appearance of her cottage, pours several pails upon the floor, and when she has completed her task with the besom, she proceeds to remove with a mop or flannel so much of the water as the bricks have not absorbed. After having cleansed the cottage, the fire is usually made up to prepare the evening meal, and vapour is created by the action of the heat upon the saturated floor".[42]

Often the condition and cleanliness of cottages were only revealed in asides, as when a medical officer gave an account of the life of the Sussex labourer: "I will first describe the cottage and mode of living of a Sussex labourer as to make him one of the most distressed of his class ... On entering, the cottage displays a room about 20 feet long by 15, paved with brick ... The cottage ... is clean and well-drained".[43] Sometimes correspondents did explicitly state that unhygienic conditions were not a health problem in their area:

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[41] *Ibid*, p. 419.
[42] *Ibid*, XXVI, p. 269.
[43] *Ibid*, XXVII, p. 38.

> I have the more pleasing duty to state, that throughout the greater part of these counties [Kent and Sussex] comparatively few diseases are found to arise from the want of sanitary precautions. Mr Evans, the medical officer of part of the Ticehurst union, says – "The situation of this district being high and dry, and the cottages of the labourers in general well-ventilated and clean, and no accumulation of filth about them, we have no cause of complaint".[44]

The high standard of domestic hygiene was probably one of the reasons why agricultural labourers had a very low adult mortality rate in East Kent at the end of the eighteenth century.[45] A number of correspondents contrasted the hygienic conditions of the interior of cottages and houses, with that outside. The medical officer of Spalding in Lincolnshire believed that the accumulation of filth around cottages was due to ignorance, as "cleanliness prevails generally *within* the cottages".[46] And similarly, a Mr Hassell writing of the Penrith area of Cumberland stated that "the country cottages in this district are ... kept very clean by the labourers' wives".[47]

But this conclusion about the high standard of domestic hygiene in labourers' families can only be tentative, given the paucity of work on the subject. A great deal more research needs to be done not only on this topic, but all aspects of disease and mortality in the eighteenth and nineteenth centuries. Special focus must be placed on housing conditions and their effects on health. Additionally, detailed work on occupational mortality will enable an assessment of the role of socio-economic factors in mortality. It is hoped that the present book will re-open the debate about the role of mortality in population change, as well as providing an introduction to some of the unresolved problems of early modern English demography.

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[44] *Ibid*, p. 43.

[45] According to data in the East Kent marriage licences, the parental mortality rate of women marrying labourers was one of the lowest of all occupational groups at the end of the eighteenth century. This echoes the low adult mortality rate of labourers in mid-Victorian England. See Michael Haines, "Conditions of work and mortality decline", in R. S. Schofield *et. al.* (eds.), *The Decline Of Mortality In Europe* (1991), p. 183.

[46] Chadwick, *op. cit.*, XXVII, p. 156.

[47] *Ibid*, XXVIII p. 428.