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THREE CENTURIES OF INEQUALITY IN BRITAIN AND AMERICA

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Arthur Burns was delighted with what he read in Simon Kuznets’s massive new book in 1953. Kuznets found that incomes were getting more equal. For Burns, this finding re-wrote all the rules for the perennial debate over inequality and redistribution through government:

“Few Americans and fewer Europeans are aware of the transformation in the distribution of our national income that has occurred within the past twenty years - a transformation that has been carried out peacefully and gradually, but which may already be counted as one of the great social revolutions of history....

“Considerable income inequalities still exist in our midst, but they require careful interpretation the upper stratum is dominated by the most productive age, sex, and educational groups in the population

“These conclusions of Kuznets’ investigation have great significance for the American people. If we are to look forward constructively to a material reduction of income inequalities in the future, we must seek to attain it principally by raising the productivity of those at the bottom of the income scale rather than by transferring income from the rich to the poor Substantial further government redistribution of income may ... affect adversely the size of the national income, while it cannot improve appreciably the living conditions of the great masses.” (Burns 1954, p. 137).

Burns was neither the first nor the last to base a sermon about inequality on some historical data. His enthusiasm stands out in retrospect, however, because it came at a time when an epochal equalization of incomes seemed tangible to many. Seeing him in that dawn of discovery, and marveling at his breath-taking leaps of logic, we naturally wonder about the longer and deeper history. Was he right? How long had that egalitarian trend been going on before the 1950s? Was the non-meritocratic part of inequality really stripped away in those past twenty years? Would the change be permanent? And what would Burns have written about inequality movements “within the past twenty years” if he were writing at the end of the twentieth century?

We can now take stock of past inequality movements in Britain and the United States with the help of recent progress on three fronts: (1) New experiences since the 1970s; (2) archeological progress, yielding better retrospective data on the more distant past; and (3) a highly-developed algebra that decomposes inequality movements into their proximate causes, in order to trace more fingerprints of the underlying causal forces than simple inequality aggregates can reveal.

A number of **conclusions** about inequality movements stand out, despite all the data flaws and the nuances we have learned to expect from movements in the distribution of incomes among fluctuating human populations:

(1) Income and wealth inequality definitely rose over the first 150 years of U.S. history. Britain may also have had an early period of rising inequality, but the most likely period of rising inequality (1740 - 1810) was earlier than most writers have imagined.

(2) Britain and America, and indeed most high-income countries, did indeed experience a shift toward more equal pre-fisc incomes in the first half of the twentieth century, as Kuznets believed. The leveling was brief and sharp for

America, but proceeded more gradually for Britain. Most or all of the leveling took the form of a narrowing of the gaps between the top and middle ranks.

(3) From the 1970s to the 1990s income inequality clearly rose in these two countries. This widening reversed most or all of the previous equalization of pre-fisc incomes. There was probably still a net equalization of post-fisc (disposable) incomes over the whole three centuries, however.

Exploring these movements has deepened our knowledge of their underlying causes:

(4) Even “pre-fisc” income inequality moves partly in response to redistribution through government. The rise of tax-transfer progressivity equalized the ownership of human and non-human capital, and its later stasis played a permissive role in the recent return of rising inequality.

(5) Government redistribution cannot explain all of the epochal reversals in inequality trends, however. Factor-market forces and economic growth would have produced a similar chronology of rises and falls in income inequality even without shifts in the progressivity of redistribution through government. The dominant causal forces here are demographic change, unbalanced technological advance, and income effects.

(6) These underlying forces change overall inequality both through movements in relative factor prices and through compositional shifts in group weights.

(7) The key to future improvements in our understanding of the forces driving income inequality lies in simultaneously explaining the pre-fisc inequality, the inequality of political voice, and government redistribution between rich and poor. Only with such a three-sided simultaneous system will we have a satisfactory explanation of the Robin Hood paradox, which notes that redistribution toward the

poor tends to happen least in those times and polities where it would seem most justified by the usual goals of welfare policy.

I. CHOOSING ISSUES, MEASURES, AND METHODS.

Our conventions for addressing, measuring, and explaining inequality movements have governed what we are prepared to see, for better and for worse. Before turning to the long history that can now be mapped, and surveying the usual approaches, we should note where the literature has placed its lampposts, illuminating some aspects of inequality but leaving others in the dark.

A. Redistributable Income or Living Standards?

Much follows from one's choice of a social issue for research and policy debate. Our whole view of inequality hinges on whether we care more about (1) the inequality of economic resources that economic policies might redistribute or (2) the overall inequality of living standards. The division is sharp here. Pursuing redistributable incomes, as in the rest of this chapter and this book, reveals a history of episodic swings in this kind of inequality. Pursuing the inequality of living standards among individual lifetimes reveals more of an equalizing trend.

Economists' exploration of inequality movements has seldom strayed far from the issue that dominates most of economics: What is the proper role of government in our lives? Income inequality is of interest primarily as an exhibit in the debate over how, or whether, government should redistribute income and wealth. The (valid) pre-occupation with this perennial debate shapes all choices of inequality measurement. In the choice of independent variables (influences on inequality trends), considerable attention is spent on allocating the credit or blame

for inequality trends between government redistribution, market movements, and the distribution of human capital. It matters to most writers whether the credit for a reduction in inequality should be given to government and labor unions, or to the normal workings of the marketplace, or to equalization of individuals' human capital.

Similarly, the dependent variable of interest is typically one directly responsive to government manipulation and to market forces, such as taxable market income or full-time annual earnings. When the subject turns to the health and longevity side of inequality, our usual instinct is to view health and death as things experienced by families at different positions in the income ranks, or by families headed by persons in different socio-occupational classes. Thus infant mortality is something suffered differentially by poor and rich parents, and we measure its impact at the household level (e.g. Titmuss 1943). The implicit policy question is how much mortality could be reduced and equalized by redistributing economic resources (income, health care, etc.) across households.

Yet one might care primarily about that other inequality concept, the inequality of overall living standards themselves, not just the income part of them most manipulable by changing government policy or other economic institutions. Such a broader concern for inequality of human living standards would give far more attention to inequalities in individuals' health and length of life in particular.¹ Even if we valued whole lifetimes only according to people's total lifetime consumption, the literature on economic inequality would look much different from the literature to be surveyed below. Robert Summers (1956) noted this, and Lee Lillard (1977) offered indirect measures of the inequality of lifetime income and consumption. Such measures, however, stay close to the annual income idea by positing a fixed economic lifetime. A bigger second step is to follow the inequality of lifetime consumption among birth cohorts of individuals, taking account of the inequality in the length of life. The inequality of living standards, as proxied by lifetime consumption, is governed more by

movements in infant mortality than by movements in the inequality of annual income. Improved infant survival, even if evenly spread across economic classes, has probably converted an upward drift in income inequality into a clear trend toward more equal lifetime consumption across individuals (Lindert 1991, pp. 213-4, R.V. Jackson 1994).

The usual economic treatment of inequality resists giving such heavy weight to newborns as citizens, preferring to concentrate on infant death as something experienced differentially by parents in different social classes. The literature says much about mortality gaps by income or social class, little about how the greatest reduction in individual-lifetime inequality may have been achieved by advances in medicine and health care that did not favor any class. Since this chapter's task is to share the literature's pre-occupation with the debate over income inequality, differentials in life expectancy will be noted only *en passant*, as extra twists on inequalities between income ranks.²

B. The Pre-fisc Focus.

Much of the literature on income inequality movements chooses to follow measures of the inequality of pre-fisc, or original, incomes, rather than the post-fisc disposable incomes people actually receive. This frequent choice has a rationale and a major implication.

The rationale is to concentrate on the larger intellectual challenge. The directly redistributive component of post-fisc inequality is transparently attributable to government, at least in the accounting sense. The task of explaining movements in pre-fisc or original income is more challenging. Many economic forces compete for explanatory roles. Indeed pre-fisc is not even pre-fisc, inasmuch as prior fiscal interventions, such as estate tax, affect the inequality of this year's original incomes.

The implication to bear in mind is that the literature focusing on movements in pre-fisc inequality, even when it recognizes feedbacks from past taxes and transfers to current original income, hides much of the role of government in shaping the inequality of current disposable income.

C. Causal Methods.

Different analytical techniques compete for our energies and attention, so that using one more fully can crowd out other insights. The treatment of income inequality has passed from simple factor-price and factor-share tales, to a more sophisticated decompositional accounting based on identities, to the use of regressions and large-model simulations to weigh exogenous causal forces. Time spent at each step is time not spent at the next.

Before the mid-twentieth century the usual instinct was to imagine fixed shares of the population for different economic classes, each rewarded by a different factor price, and to assume that movements in rents and profit rates and wage rates summarized the movements in inequality. While this simple equation of factors and quantile ranks had some validity back when the classical economists wrote (Lindert 1986), it was obsolete long before it was abandoned.

Simon Kuznets (1955) ushered in the current era of decompositional inequality accounting with his often-cited example of how shifting group weights could generate inequality trends without any movement at all in factor prices. The algebra has grown in sophistication, as evidenced by other chapters in this volume. Identifying the behavior of the different components makes it possible to test numerous side-implications of each hypothesis about the sources of inequality.

While decomposing inequality into its parts sharpens our sense of how inequality changes, it leaves open the question of why. Each of the classes into which decompositions divide an inequality change can be affected by several

underlying forces in unknown proportions, and each of those forces typically shapes inequality through more than one component. Decompositional analysis must share the stage with statistical and simulation-model (e.g. computable general equilibrium) techniques for weighing the contributions of underlying forces.

D. The Kuznets Conjecture.

Finally, one other lamppost has illuminated a corner of the subject rather well, a corner from which it is time to move. This is the literature testing whether or not inequality follows an inverted-U curve, a Kuznets curve, as per capita income rises.

Despite its name, Kuznets never drew such a curve. He was content to offer a verbal conjecture about how income inequality might move, and to use a tale of compositional shifts and some common sense to suggest explanations. He was rightly modest about the international data base he had at his disposal, and described his conjectures about trends as "... perhaps 5 percent empirical information and 95 per cent speculation, some of it possibly tainted by wishful thinking" (Kuznets 1955, p. 26).

Kuznets did not feel the same about the rise as he did about the fall of inequality. That inequality tended to decline at some advanced stage of development, he seemed quite confident. He barely asserted -- rather, wondered about -- the possibility of an earlier rise. His confidence in his explanations for it all were similarly mixed: He emphasized the role of sectoral shifts as an engine of inequality, and mused more vaguely about the possible importance of the demographic transition (Kuznets 1955).

The Kuznets curve has to some extent tyrannized the literature on inequality trends. Energies that could have moved earlier into exploring the underlying causes of inequality were diverted into a debate over whether there

was or wasn't an inverted U curve, either in history or in postwar international cross-sections. Like other writings, the rest of this chapter will show both theoretical and empirical reasons to doubt that countries must follow such a rise and fall in inequality. It is time to move onto explorations that proceed directly to the task of explaining any episodic movement, without bothering to relate it to the Kuznets Curve.

II. WAS THERE A RISE IN INEQUALITY SOMETIME BEFORE 1914?

“[As a] conjectural conclusion I would place the early phase in which income inequality might have been widening, from about 1780 to 1850 in England; from about 1840 to 1890, and particularly from 1870 on in the United States; and, from the 1840's to the 1890's in Germany.” (Kuznets 1955, p. 19)

The top candidates for rising inequality, in Kuznets's view, were those epochs that the debates of the 1960s would call “industrialization” or “take-off,” including the classic dating of Britain's Industrial Revolution.

Was it true? Our interest has remained strong since 1955, and our views have changed. Pioneering work by Lee Soltow has amassed an impressive array of primary data. Soltow doubts that there was any period of sustained and serious widening of inequalities in either Britain or America. Rather, he emphasizes that inequalities were traditionally stark before they narrowed dramatically across the twentieth century. Jeffrey Williamson and I, by contrast, see early widening and later narrowing of inequality in both countries, though not with the timing conjectured by Kuznets. Jan Luiten van Zanden has posited an early rise in inequality by arguing that most economies of Western Europe ascended a “super-Kuznets curve” before industrialization, sometime between

the sixteenth century and the late eighteenth century. The evidence, and the additional patterns of interest, need to be viewed for Britain and America separately.³

A. Britain, 1688 - 1914.

For Britain before 1914, our best guesses are necessarily eclectic. There is little choice but to weave an archival quilt of indirect clues on income inequality. The main pieces of primary material are (1) the social tables used by the "political arithmeticians" from Gregory King through A.L. Bowley, (2) measures of personal wealth based on probate records and occasional tax assessments, (3) the paths followed by a few dozen wage series, (4) land-rent series, and (5) early partial tax returns.

Britain's early income distributions start from educated works of fiction, those social tables drawn up by Gregory King, Joseph Massie, Patrick Colquhoun, R. Dudley Baxter, A.L. Bowley and others. Each of these experts had access to the best miscellany of data available in London at the time. The first three of them, at least, had axes to grind. King seemed intent on warning that the nation had only a limited capacity to raise tax revenues for wars against France. Massie railed against the sugar monopoly. Colquhoun highlighted the nation's achievements and its ability to afford more poor relief. Such dangerous estimates need to be cross-checked and revised with the help of all the records unearthed by subsequent scholarship. Weighing them carefully has yielded useful tentative revised estimates of the whole distribution of class-average incomes per household (Lindert and Williamson 1982, 1983).⁴

Table 1 and Figure 1 summarize the income distribution estimates for England-Wales and the United Kingdom since 1688. We focus on top-rank income shares because the underlying data aggregated the poorest ranks of

society into a few large classes, blurring our view of inequalities below the median household before 1914. The estimates imply that Britain's inequality was higher between 1688 and 1911 than anytime since, though the gaps in the 1990s approach those of 1911. There is no clear early widening of the income gaps, though the period 1759-1802 (or, probably, from the 1740s to the 1810s) gives signals of a rise in the share received by the richest. There is also the suggestion that income inequality declined gently in the last five decades before World War I, though the 1911 figures are based on highly aggregated distributions.

[Table 1, Figure 1 and their notes about here.]

The suggestion of overall stability in the income gaps, with a slight rise in 1759-1802 and a possible slight decline in 1867-1911, should not be accepted on the basis of the revised social tables alone. We need to see what other evidence says about the suggested long-run stability, the apparent net rise of 1759-1802 (or similar dates), and the possible decline between 1867 and 1911.

We have three main kinds of additional clues available: (1) Movements in factor-price ratios, (2) estimates of movements in the inequality of wealth or property income, and (3) estimates of movements in the inequality of human earnings.

The first set of clues uses a crude factor-price ratio, the ratio of land rents to wage rates. For an early era in which land still commanded a significant share of national product, land rents alone can represent much of what was happening to the average reward for the use of property. And for England and Wales as late as 1867, land was almost exclusively an upper-class asset. Land rents accruing to the top decile of households were 13 percent of their income versus only 1 percent of the income of the other 90 percent of households. Stated differently, about 89 percent of land rents were earned by that top decile (Lindert 1986, p. 1155). In such a society, any rise in the ratio of land rents to the wages of common labor would imply a rise in the top decile's income share, other things

equal. As it happens, the only period between 1688 and 1914 in which the rent/wage ratio clearly rose was circa 1750 - 1810, roughly the period in which the social tables show their only rise in the top-decile and top-quintile income shares.⁵

By contrast, the separate estimates of wealthholding inequality and of earnings inequality do not follow the same chronology. The next set of clues consists of wealth distributions worked up from large samples of probate inventories.⁶ Wealth is not income, of course, but it sheds indirect light in two ways: by showing the assets on which current property income is based, and by reflecting the wealth accumulated from earlier total incomes.

When one follows the average levels of estimated net worth by social classes -- landed gentry, merchants, yeomen, craftsmen, and so forth -- one finds a striking widening of the wealth gaps between 1810 and 1875. The top landed groups and merchants accumulated at a prodigious rate, it would seem, with their wealth growing far faster than that of professionals, shopkeepers, yeomen, or craftsmen. Marx might have been pleased with such estimates, were it not for the fact that even the middling groups gained in absolute real wealth and held their share of the population, instead of slipping down into the proletariat.⁷

Yet the rise in wealth inequality vanishes when the personal wealth figures are weighted and combined into a size distribution for England and Wales. As Table 2 makes clear, the wealth share held by the top five percent of adults (approximately the top 10-11 percent of household heads) was high, but not clearly changing any time before this century. The lack of trend is consistent with the dramatic widening of class wealth gaps between 1810 and 1875, simply because the very richest groups (landed aristocrats and merchants) were a declining share of the adult population, and land was a declining share of national wealth and national income. The evidence on non-human wealth thus shows wide inequality gaps before 1914, but no clear trend.⁸ Combining this

trendless property distribution with the available estimates of human earnings or human capital still leaves an apparent net rise in income inequality between mid-18th century (1740-1759 benchmarks) and the French War era (1801-1810), whether one sticks with an income measure or uses a total-wealth measure (Lindert 1986).

[Table 2, Figure 2 about here]

The other main quantitative data base for judging movements in British income inequality before 1914 consists of series on the mean and dispersion of labor earnings by occupation. Jeffrey Williamson (1985, Ch. 3) has ambitiously pieced together the average male pay rates, intra-occupational earnings distributions, and employment weights for dozens of occupations for benchmark years from the late eighteenth century to the early twentieth. He finds that earnings inequality rose over the first half of the nineteenth century, peaking at the 1851 benchmark. After an apparent plateau, 1851-1881, earnings inequality began to drop, both within and between broad occupational classes. On these estimates, the rise and fall of earnings inequality look more dramatic for the economy as a whole than within non-agriculture, since the nonfarm / farm ratio of wage rates for common labor also peaked in 1851. The rise and fall of earnings within the nineteenth century contrasts with the lack of trend for overall income inequality shown in Table 1.⁹

While the occupational earnings data have thus become abundant for Britain in the nineteenth century, their use as a clue to overall income inequality trends is compromised by three drawbacks. The first, of course, is their omission of property incomes. Second, the occupations tend to slide around the income ranks, denying us a view of pay ratios between fixed percentile positions. Williamson has documented such rank-switching (Williamson 1980; 1985, p. 11), but it remains a problem. Third, Jackson (1987) and Feinstein (1988) have

pointed out defects in some of the pay series Williamson collected and presented, particularly those for the higher-paid services. When the most suspect series are removed, the nineteenth-century rise and fall are muted. It is hard to say there was any rise-fall pattern in pay gaps within the nonfarm sector across the nineteenth century. The revisions suggested by Williamson's critics do show a slight rise-and-fall pattern in the economy-wide ratio for skilled / unskilled pay from 1827 to 1851 to 1911 (Jackson 1987, p. 567; Feinstein 1988, p. 712). But the economy-wide rise and fall in earnings inequality now hinges almost solely on the nonfarm/farm ratio, and caveats abound.

To supplement these traditional inequality measures, we should briefly note the likely changes in five other sources of inequality before 1914: (1) difference in the cost-of-living trends for rich and poor, (2) unequal mortality, (3) difference in household composition, (4) male/female pay gaps, and (5) regional inequalities.

(1) Real inequality trends differ from nominal inequality trends whenever the cost of living moves differently for rich and for poor. Cost-of-living trends can indeed differ by income class even when everybody faces the same prices for individual commodities. In most settings this point does not matter much (e.g. for the United States up to the 1970s, as shown in Williamson and Lindert 1980, Ch. 5, and for the UK since 1978 in Crawford 1996). Yet it matters greatly in our judgment of English inequality trends in the 18th and 19th centuries, as argued elsewhere (Lindert 1998). In that setting the rich spent a much lower share of their incomes on food than did the poor, and the rich also paid out a smaller share of their income in housing rents.¹⁰ The relative price of food rose something like 25 percent 1760-1800, then fell back after 1815. Real housing rents quadrupled between 1760 and 1835, again relative to the overall cost-of-living index. The consumer goods that declined in real price were fuel and textiles-clothing. Thus the cost of living rose more, or fell less, for the bottom 80 percent

of the income ranks than for the top 20 percent or top 5 percent, as sketched in the “real” inequality series of Table 1 and Figure 1. Paying attention to this point re-introduces a noticeable rise in inequality, especially between the mid-eighteenth century and the early nineteenth.

(2) Mortality trends could change our perceptions of inequality trends in ways already introduced. If one chooses to view deaths in the family as deductions from the well-being of survivors in the same family and same income strata, then again inequality may have risen more sharply between the mid-eighteenth century and the mid-nineteenth than Table 1 has shown. The reason is that the chances of survival improved markedly for the upper classes, to judge from peers’ family records, but only slightly for the nation as a whole. On the other hand, if we follow the inequality of lifetime consumption among individuals, then even the modest gains in life expectancy between 1688 and 1867 were enough to bring a net equalization. Again, as noted, the choice depends on the question asked.¹¹

(3) Adjusting for the changing social gradient of household composition would give an upward tilt to the British inequality trend between 1688 and 1867. So far we have discussed only the distribution of household incomes. A popular alternative is to rank households by their income per capita, or per adult-consumption-equivalent, on the ground that larger household size dilutes consumption standards. While no such adjustment is presented here, we know the direction in which it would change the trend between, say, 1688 and 1867. Over these 179 years, household size fell more rapidly among high-income households than among low-income households. In 1688 household size had a slight positive correlation with household income, with the richest households including servants and with unrelated individuals making up a large share of the pauper host at the bottom of the ranks. Thus for 1688 the ratios of top to bottom incomes would be lower on a per-capita basis than on the per-household basis

shown in Table 1. Two centuries later the correlation between household income and household size was less positive, and possibly negative. For 1867, the ratios of top to bottom incomes might have been higher on a per-capita basis than for total household income. There would be more of a trend toward inequality in income per capita (or per adult consumption equivalent) than Table 1 has revealed.

(4) Our view of early trends in Britain's male-female income differences is still obscured by the paucity of data on women's wage and salary rates.¹² The few quantitative studies available tend to focus on the classic 1780-1850 era (Horrell and Humphries 1992, 1995; Lindert 1994a; Feinstein 1996b; and the literature cited there). For this era, there is a range of possibilities. It seems unlikely that women's real wage rates advanced faster than those of unskilled males and there are hints that they advanced slower than those male rates, or not at all, across the early nineteenth century. An overall income distribution featuring good data on women's wages might thus show a bit more trend toward inequality between 1780 and 1850 than is now evident.

(5) As for regional income inequalities, British history reveals two sharp turning points, though their implications for overall inequality are unclear. Before the late eighteenth century, the poorer regions tended to be in Northern England, Wales, and highland Scotland (Schofield 1965, Hunt 1986). By 1800, however, poverty had become a feature of the rural South and West. Northern England retained an income advantage over the rest of Britain (bar London) for over a century. World War I brought the other great turning point, and prosperity has been a southeastern specialty ever since. Famous as these two turning points are, they carry no obvious implications for a quantitative measure of national inequality trends.

B. When Did American Inequality Rise?

By 1929, and probably by 1914, income and wealth and earnings were as unequally distributed in America as in Britain. Had it been that way ever since Jamestown?

Lee Soltow has implied as much, consistently doubting any early rise in inequality (Soltow 1971, 1984, 1989, 1992). If that is true, then the colonists' incomes were at least as unequal as the incomes back in Britain. Such inequalities may fit preconceptions about the colonial South, but they clash with most preconceptions about the middle or New England colonies. Were past observers wrong in thinking that migrants to these colonies set up a more egalitarian property system, free of the latifundistas that controlled the English and Irish countryside? A host of scholars have worked on this issue since the 1970s.

Most evidence fits our usual preconceptions, not Soltow's hypothesis, showing a relatively egalitarian America, outside the South, up to at least 1800. That evidence comes in indirect forms: wealth distributions, suggestive wage gaps, mortality trends, and other odds and ends. There are many studies to draw on, but none of them has the kinds of income distributions that were conjured up by Britains' early social tables and partial income-tax returns, since America did not have an income tax that reached below the top one percent until this century.

The best starting point is Alice Hanson Jones's pioneering estimation of the 13-colony distribution of net worth in 1774 from 939 probate records and supporting materials. Using an elaboration of estate-multiplier methods, Jones developed a distribution of wealth among the living from the wealth of the deceased, with results shown at the top of Table 3. While the sample is small, no clear defects in her estimates have been revealed.

To compare colonial inequality with English wealth inequality at similar dates, one can roughly equate the top 10 percent of household heads with the top 5 percent of all adults. Equating these two shares shows an unmistakable

contrast between the mother country and Jones's portrait of the 13 colonies. The richest 5 percent of adults held 85-87 percent of net worth in England and Wales (1740 and 1810 in Table 2) but only 59 percent of net worth in the 13 colonies, even when America's slaves are counted both as holders of zero wealth and as other people's property.¹³

While Jones's study is the only one to pull together estimates over all the 13 colonies, it is buttressed and extended by a host of local studies following the distributions of probated or assessed wealth across a century or more of colonial experience.¹⁴ The flavor of the local-wealth results is shown by Figure 3's trends from Boston and nearby Hingham, Massachusetts. The general trend seems to be upward in most cases, often dramatically so, suggesting that wealth might have been held even more equally in the seventeenth century than in the eighteenth. The appearance deceives, however. Most of the studies follow wealth trends in fixed places, usually near the seaboard of the New England and middle colonies. The inland frontier, however, was both more egalitarian and an ever-rising share of the total population. The westward drift of people was, in fact, so great that there appears to have been no trend at all in the wealth inequality of the New England and Middle colonies (Williamson and Lindert, 1980 and 1981). For these colonies, wealth inequality back in the seventeenth century was probably not much different from that shown in Alice Hanson Jones's 1774 benchmark.

Yet there was one region where wealth inequality probably did rise across the colonial era -- the region omitted from most of the studies of colonial wealth trends. In the South, the share of slaves in the overall population rose from near zero in 1630 to 40 percent in 1770. So great a rise in zero-wealth population, and in people who represented wealth for others, must have raised wealth inequality within the South over the century and a half ending in the Revolution, even

though we lack earlier figures to compare with Jones's small Southern sample in 1774.¹⁵

[Table 3, Figure 3 -- US wealth shares]

But if colonial life outside the South was much more egalitarian than life in the early twentieth century, we have a nineteenth-century American puzzle: When did the Americans become so unequal? Did it happen before, during, or after the Civil War?

The wealth-inequality studies imply rising inequality over most decades of the century and a half from Alice Hanson Jones's 1774 benchmark to 1929. There was no sudden jump in wealth inequality, as far as we can tell. Still, there were episodes. The most likely short-run troughs in wealth inequality came near wars: the 1810s-1820s, the 1860s, and World War I. All other periods of a decade or longer probably brought rising inequalities.¹⁶

Table 3 and Figure 3 sketch the net trend in nineteenth-century wealth inequality, using a few relatively reliable benchmark studies. The main pillar supporting both the antebellum and postbellum spans of the bridge is Soltow's pathbreaking (1975) study of census returns on the real and personal estates of males living in 1860 and 1870, plus returns on real estate alone for 1850.

For the period between 1774 and 1860, most local studies show the same kind of rise that Table 3 and Figure 3 imply with their contrast between Alice Jones's 1774 and Lee Soltow's 1860. The changes across the Revolutionary and early federal years are hard to judge. One might have expected that top-rank shares of all wealth would have been raised by the confiscation of large properties from Loyalists whose primary resident was outside the colonies, but we lack good number on this.¹⁷ Soltow has made a valiant attempt to plot the contours of early federal wealth by sampling 1798 census values of real estate. The data, however,

are not up to the standard of his wealth samples from the 1850-1870 censuses. The 1798 census asked people to estimate “dwelling houses ... lands, lots, buildings, wharves, owned, possessed, or occupied” with no reporting of holdings under \$100 or vacant lots over 2 acres (Soltow 1989, p. 286 and *passim*). The data omit all non-land property and all human earnings. They also cast a fog by mixing tenancy with elements of ownership. If the data had been gathered only from households in their role as occupants, their consumption of housing could be used to conjecture about the income distribution. That was not done. On the other hand, the ownership data are incomplete, in that the holdings of the same person in different areas are not collated. Soltow struggled to interpret the ostensible rise in inequality from Alice Jones’s 1774 to his 1798, saying it was true but probably smaller than he himself had estimated (1989, pp. 170-174). The best resolution seems to be to agree that inequality might have risen a bit between 1774 and 1798, but not as much as his 1798 figures imply.

As a corollary, the widening of wealth gaps appears to have continued beyond 1798 all the way to the Civil War, aside from an 1810s-1820s dip suggested by a few local studies. The ante-bellum widening apparently owed little to compositional shifts in the population. True, there was a rise in immigration, an urbanization trend, and a continuing frontier settlement. Yet several accounting exercises show no major role for shifts in the age distribution, the urban share, or the share foreign born (Williamson and Lindert 1981).

Beyond 1860, the wealth gaps remained wide, aside from temporary narrowing during the Civil War decade and during World War I. In either 1913 or 1929, American wealth inequality matched that in the United Kingdom.

Still, non-human wealth relates to only part of the income distribution, and one strains to find other indicators of relative income movements across the nineteenth century. One promising path is to collect occupational pay series, to suggest possible movements in the Lorenz curves for earnings and for total income, as several scholars have done (Williamson and Lindert 1980; Margo and

Villaflor 1988; Goldin and Margo 1992a; Margo 1992). Jeffrey Williamson and I saw an ante-bellum surge in wage inequality between the 1820s and the mid-1850s, a timing that would suggest parallelism between wealth-widening and wage-widening. Margo and his co-authors challenged this view by introducing new data on civilian workers hired by the army in each of the major settled regions. In their data wage widening proved elusive between 1821 and 1856. It showed up for some regions but not others, under some summary measures but not others. This does indeed clash with the series used earlier, and casts some doubts on an ante-bellum surge in wage inequality. The doubts serve to repeat the question already posed in this section. If there was no pronounced widening of pay gaps before the Civil War, when did income inequality, like wealth inequality, reach the heights we can document for 1929? Nothing we know about the colonial economy suggests that income should already have been so highly unequal outside of the South, given that wealth was not nearly so unequal as it was to become in 1929. If the income gaps didn't widen between the 1820s and the 1850s, then when?

The inequality of average regional incomes offers a better-data haven from the larger uncertainties about the income distribution.¹⁸ We do know that the regional inequality in commodity product per capita rose across the nineteenth century. In this case, however, the shift was a single discrete event. The Civil War and emancipation cut Southern incomes relative to the rest of the nation between 1860 and 1880. The main reason for this widening was not wartime destruction, but a change with an unusual welfare twist: Slave emancipation cut black labor supply by 28-37 percent, as they used their freedom to reduce the work hours of children, women, and the elderly down to white norms (Ransom and Sutch 1977). While it may have raised the inequality of conventional incomes across regions, emancipation is a change that lacks the welfare cost usually associated with a widening of regional income gaps, since people near the bottom of the income ranks were choosing to cut their incomes when given

control over their own time. After the 1880 benchmark, anyway, the wide gaps between the non-South and the South remained up through 1940.

The nineteenth-century movement of male / female wage gaps in the United States was quite different from the widening trends that show up for the inequality of wealth and of regional incomes. Thanks to Claudia Goldin's (1990) pioneering work, we have a better quantitative history of the gender pay gap for America than for Britain. Goldin finds considerable narrowing of the male /female pay gap (i.e. a rise in women's relative pay) between the 1820s and the 1850s, further blurring the picture of this era as one of rising inequality. After the 1850s, the trends in the male /female pay-ratio were flatter until the late twentieth century.

To raise further the stakes in figuring out just when Americans became more unequal across the nineteenth century, consider a health-trend puzzle that hints at a widening of gaps in overall life expectancy up to about 1870. Several authors have found that stature and life expectancy both shortened from about 1790 to about 1870 (Kunze 1979, Fogel 1986, Steckel 1995), even though real wage rates surely rose between these two dates, both for common laborers and for artisans. The worsening of health appears to have happened all across the country, north and south, rural and urban. By itself, the worsening mortality lowered average living standards, in the sense described in Part I. In addition, if worsening health and earlier death visited the poor in particular, as Steckel's work implies (1992, 1995), then we have another way in which the inequality of living standards widened before 1870. One should be cautious about the related belief that the rising inequality of life expectancy shows us a rise in the inequality of annual incomes. Other studies cast doubt on any reliable link between annual-income inequality and the level and inequality of mortality. The puzzle remains, however: What caused that long gradual worsening -- and the presumably increasing inequality -- of mortality?

In sum, we know that income inequality must have risen sometime between 1774 and any of these three competing peak-inequality dates: 1860, 1913, and 1929. The inequality of health and life expectancy also worsened between 1790 and 1870, and improved thereafter. Beyond this, the evidence on the rise of unequal America is only suggestive and incomplete.

III. WHEN INCOMES LEVELED

The early twentieth century brought three related changes to Britain, to the United States, and to other high-income countries: (1) Governments redistributed more, (2) governments collected and published more income data, and (3) incomes became more equal even “before” taxes. Let us follow the third of these developments, carefully using the second and wondering about the role of the first. While the role of redistribution is automatically reduced by our following the convention of looking at the distribution “pre-fisc” income, it is still a significant force in shaping that distribution.

The timing of the equalization of incomes differed greatly between these two countries. Let us turn first to Britain, whose leveling era lasted longer and achieved more.

A. Britain.

When did the leveling of British incomes start? There is strong reason to wonder, and there are some shaky data to satisfy our curiosity on events before 1938. We wonder primarily because we seek to know whether the leveling of market incomes antedated the confiscation of top property holdings by progressive taxation. Taken at face value, the rough estimates shown in Table 1 and Figure 1 say that the equalization of fixed incomes did indeed antedate

Lloyd George, since inequality was less pronounced in the revised-Bowley estimates for 1911 than for the revised-Baxter estimates for 1867. Intriguing as this possibility is, it cannot be considered a “finding” until far better data are available for the late nineteenth century and early twentieth.

Starting in 1938, and continuing through 1974, the Central Statistical Office produced its Survey of Personal Incomes (SPI) estimates of the distribution of before-tax income among tax units. From 1949 through 1984/5, it offered the alternative “Blue Book” series drawing on results of the Family Expenditure Survey, still sticking with the tax unit as the population base. Then, with data running from 1977, the CSO (now the Office of National Statistics) transformed the population unit to the consumption-equivalised household. This current series, however, presents shares only for quintiles, hiding our view of movements within each quintile. Subject to the much-discussed limitations of the various series (Royal Commission 1977, Chs. 2, 3, 5; Atkinson and Micklewright 1992; Atkinson 1995, Ch. 1), Figure 4 and Table 1 present Gini’s and top-quantile shares to summarize the history they offer.

[Figure 4 about here -- 20th-century UK income shares]

The gap between top-income groups and other Britons continued to narrow across the first three quarters of the twentieth century. There were important limitations to this movement, however. The top 5 percent definitely lost greatly in their income share, but there the leveling may have stopped. The very next group, the 80-95% group, did not suffer any erosion of income relative to those below them. Table 1’s SPI estimates for taxpayer units imply that the average pre-fisc income of the 80-95% group kept the same ratio to that of the bottom 80 percent of taxpayers all the way through to the end of the leveling era around 1974:

<u>Year</u>	<u>Mean-income ratio (80-95%/0-80%)</u>
1867	2.03

1911 (SPI)	1.96
1938 (“)	2.34
1949 (“)	2.16
1964 (“)	2.04
<u>1974</u> (“)	<u>2.08</u>
1975 (Blue Book)	2.39
1984 (Blue Book)	2.76

This contrasts with the reverse movement from 1975 one, when the 80-95% group definitely shared in the top-rank gains. Furthermore, as far as we can tell from the especially poor figures on income in the bottom income ranks, the bottom forty percent did not gain relative to the middle quintile after 1938 (no guesses should be ventured about movements below the median between the 1911 and 1938 benchmarks). Britain’s leveling in pre-fisc income, then, may have conformed to a simple formula: The top 5 percent lost ground, and (at least after 1938) the gaining ranks were the next 55 percent, not the bottom 40 percent.

Trends in the inequality of disposable income, after taxes and transfers, probably had similar turning points, but with a greater net change and a different locus of equalization between income ranks. Fiscal redistribution brought more equalization after World War II than any time before. The fiscal redistribution, unlike the trends in pre-fisc inequality, clearly raised the share received by the bottom forty percent of households.

The same three-quarters of a century saw a drop in the concentration of personal wealth into the hands of the top 5 percent of adults, as Table 2 and Figure 2 have shown. To be more precise, that dramatic decline in wealth concentration came between the 1911/13 benchmark and about 1980 -- and then stopped. While the wealth figures require, and have received, very careful handling (Atkinson and Harrison 1978; Economic Trends, November 1991,

Feinstein 1996a), the existence of a decline can withstand even large errors in the estimates.

Britain's pay ratios, too, have shown some compression since the start of the twentieth century (Routh 1965, Lydall 1968, Phelps Brown 1977). Despite the usual caveat about the trickiness of the link between pay ratios and inter-quantile earnings (or income) ratios, the twentieth-century pay data are rich enough -- and the pattern of compression consistent enough across broad occupational groups -- to establish that there was a net change, at least over the whole sweep of 75 years. So both wealth inequality and pay ratios (and presumably labor-earnings inequality) moved in harmony with the overall pre-fisc income distribution.

Probably very little of Britain's twentieth-century leveling took the form of a drop in regional inequality. There was, to be sure, that historic shift of relative prosperity from northern England to the southeast, particularly to the home counties, across World War I. This may not have implied a great reduction in income inequality, however. Rather, the regional inequalities seem to have moved only in sympathy with the aggregate unemployment rate. Given that Britain's unemployment has been highly regionalized in this century, a period of high unemployment tends to become a period of high regional inequality. Thus World War II brought a lasting drop in Britain's regional income inequalities (Williamson 1965, p. 25), and the rising unemployment since the late 1970s has raised them.

Like nineteenth-century America, twentieth-century Britain poses a puzzle about trends in unequal mortality. The British mortality puzzle is this: Why, over three-quarters of a century of income leveling, didn't mortality, even infant mortality, become more equal across the five main socio-occupational classes? In fact, the opposite happened, to judge from standardized mortality measures: Of the five census occupational classes, the highest (professional and managerial) had the greatest improvement in life expectancy, and the lowest

(manual labor) had the least from the start of the century to the 1970s (Titmuss 1943; Hollingsworth 1979; Preston, Haines and Pamuk 1981; Townsend et al. 1988; Hollingsworth et al. 1990; Lee 1991; Wilkinson 1996, Chs. 3-5).

There are ways to discount the puzzle, but it resists vanishing. Mere shifts in group sizes and inclusiveness do not seem to explain the puzzle, though there could have been some selectivity effect related to the rise in the top-class group's share of the population and the decline in the bottom group's share. It is also true that the absolute mortality rates, per 1,000 per year, have converged, even though the inter-class ratios among them have diverged. Finally, one can switch to a focus on the inequality of lifetime consumption among individuals, as described in Part I of this chapter. Doing so makes the trend in life-expectancy egalitarian, simply by reducing absolute infant mortality.

Nonetheless, the puzzle remains: Why didn't the inter-class mortality ratios also decline? While the debate continues, we need only to grant that something in twentieth-century health experience did not conform to movements in income inequality as one might have expected.

B. America.

For the United States, the shift to more equal pre-fisc incomes lasted only a quarter century, from 1929 to 1953, the year when Burns read Kuznets's book. Over that quarter century, it kept pace with the changes in Britain's pre-fisc inequality. Then it stopped altogether. Thus over the entire sweep from 1867 to 1974 Britain's leveling was greater. Britons were less equal than Americans around the 1870s. A century later the two countries' inequalities may have been similar before taxes and transfers, but the disposable incomes people could consume or save were probably less unequal in Britain.

The American change was nonetheless pronounced. Table 4 and Figure 5 plot what we know about American income inequality since the income tax was

introduced in 1913. The fuller Lorenz curves show that the decline at the top was shared by the whole top 20 percent, and there is no clear shift of relative incomes within the remaining 80 percent. America's wide lower income gaps -- for example, between the middle quintile and the bottom -- have stood out in international perspective throughout this century.

[Table 4, Figure 5 about here -- U.S. income shares]

The income leveling of 1929-1953 was not a statistical lie, even though the main data set comes from income tax returns. To explain away the apparent decline in the top income shares, the pattern of hiding or mis-reporting income would have had to have twisted implausibly, and production-based data confirm that the aggregate underreporting of income is not peculiar to interest and profit incomes (Williamson and Lindert 1980, pp. 86-88). Less direct confirmation of the change can be seen in shifts in America's occupations and living arrangements, particularly across the 1940s. Domestic servants, barbers, and beauticians declined as a share of the labor force, probably because higher-income customers found them less affordable (Stigler 1956). Boarding and lodging stopped being a common practice, and people moved to their own homes with fewer persons per housing unit. While some of these changes were responses to the absolute growth of average incomes, the equalization of incomes probably brought more people over those occupational or home-ownership thresholds.

As with Britain, the compression in America's income distribution was paralleled by compression in its wealth distribution. For the same era studied by Kuznets, Robert Lampman (1962) found a reduction in top wealth shares. Since then both the estimates for those years and the experience of more recent years have changed. Edward Wolff and Marcia Marley (1989) have adjusted the estimates, and have presented variants with and without a valuation of pension

entitlements. As shown in Table 3, the net wealth leveling from the 1929 peak to the 1950s still stands. Since the 1950s, there have been further gyrations in the top wealth share, with a trough in the late 1970s and a rise across the 1980s.

Another parallelism is that U.S. occupational pay ratios and earnings inequality also declined between 1929 and 1953, mainly across World War II (Ober 1948, Phelps Brown 1977, Williamson and Lindert 1980, Goldin and Margo 1992b). While skilled / unskilled pay ratios, the main form of evidence here, are subject to the same caveats mentioned earlier in this chapter, their behavior over the leveling era is clear enough to withstand some roughness on the income positions each occupational average wage defines. The drop in those ratios also guides our search for underlying causes of the change in income inequality: Any explanation should incorporate changes in the market returns to different kinds of labor.

The parallelism also extends to America's inequalities among regions and between races, and perhaps to the gender gap in wage rates, though these three conformities are not equally close. Regional inequalities shrank across the 1940s in particular, coinciding with at least part of the equalization of incomes nationwide (Smolensky 1963, Williamson 1965, Amos 1989, Fan and Cassetti 1994). So did the gap between white and black average incomes, though this particular egalitarian trend continued at least through 1975 (Donaghue and Heckman 1991, Bound and Freeman 1992; Maloney 1994). The male-female pay gap may also have improved sometime between 1930 and 1970, though the change looks small, especially in comparison with what followed in the 1980s (Goldin 1990).

IV. RISING INEQUALITY SINCE THE 1970s.

The main creative contribution of the last two decades to the study of inequality trends has been to serve notice that we should spend at least as much time asking why there are episodic reversals between decades as we have spent on the long-run sweep across the centuries. If the Kuznets curve meant graduation from Marxian-classical linearity to a quadratic trend, then one should hope that the British and American experience of the last two decades leads modelers to take more than just the next step. Instead of just predicting a long-run cubic inequality curve, they should invest in an eclectic approach that finds different causes for movements in different epochs, as Atkinson (1997) has stressed. The obsolescence of the Kuznets curve, in any case, stands out clearly enough in these two countries' recent experience.

A. Britain.

Britain's era of gradual leveling reversed around 1977, according to the various income and earnings series reported in Table 1 and Figure 4. Since 1977 the top quintile of households gained at the expense of the bottom 40 percent. The turning point and the new trend are robust to choices of inequality measure, and are also not the result of shifts in age, household composition, fiscal policy, or industrial structure. By most measures, Britain's inequality rise was as great as that experienced by any industrialized country after 1977.¹⁹

Movements in Britain's overall wage-salary gaps paralleled those in household income (Atkinson and Micklewright 1992; Katz, Blanchflower, and Loveman 1993). The top-wealthholder shares of all wealth, however, did not widen until an upturn from 1984 to 1991/2 (Table 2 and Banks et al. 1996).

There were important cross-currents related to gender. As far as rates of pay were concerned, women experienced a slight fall-back between 1978 and 1985, though it was not serious enough to erase their relative progress from 1973 to 1978 (Blau and Kahn 1993, p. 106). On the other hand, the rise in married

women's rates of participation and work hours was so great that it played a key role in restraining the overall widening in household income gaps shown by those income estimates in Table 1 and Figure 4 (Borooah et al. 1995, 1996; Harkness et al. 1996).

B. America.

America's gaps in household income, already wide by international standards, have also been widening. The turning point came sometime between 1974 and 1980, depending on the specific measure chosen. As a general rule, it is the top 5 percent of households that have gained, and the bottom 60 percent that have lost, in relative shares.²⁰ Even within that favored top 5 percent, the biggest gains may have come at the very top. Studies of the compensation given to corporate Chief Executive Officers show that America's CEOs have extended their already substantial lead, both relative to CEOs in other industrialized countries and relative to US production workers (Crystal 1993; Abowd and Bognanno 1995). Measures of inequality in individual earnings, as distinct from household income, show that the widening extended all the way down the spectrum. Thus, for example, the pay ratio of the 90th percentile to the median and the ratio of the median to the 10th percentile both widened, both among men and among women (Blackburn and Bloom 1987; Karoly 1993, pp. 57-65; Freeman and Katz 1995; Karoly and Burtless 1995). Wealth inequality also jumped after 1980.

In fact, the rise in American inequality since the early postwar years may have advanced further, and may have started earlier, than implied by the top-group income shares and Gini's of Table 4 and Figure 5. There is mounting evidence that the official figures shown there underestimate the incomes of the top 3-5 percent of households.

The official U.S. Census figures miss two key developments in the top tail of the income distribution. First, they omit capital gains and stock options, which became a large share of top incomes in the 1990s. Second, they are subject to a serious “top coding” problem. As others have begun to point out (U.S. Congress 1992, 1993; Ryscavage 1995; Mishel et al. 1997, pp. 417-421), the Census estimates value all household incomes in the top class at the floor of that top class. That floor was only \$50,000 for 1967-1976, then \$100,000 for 1977-1984, \$300,000 for 1985-1992, and \$1 million since 1993. The official CPS estimates imply that between 1980 and 1997 Bill Gates of Microsoft earned less than \$8 million -- from which he somehow accumulated a personal net worth valued over \$36 billion in 1997 (Newsweek, Aug. 4, 1997, 49-50). Worse yet, the published official CPS figures display even lower top-class cutoffs, frustrating any attempt to view what has happened within the top 5 percent of households.

Better clues about the true postwar movements in U.S. income inequality are afforded by abandoning the top-income shares and Gini's in favor of inter-quantile income ratios that only dare measure incomes up to the 95th percentile, just below that top-5-percent darkness. Table 5 and Figure 6 do so, showing a quite different view of the net change in inequality since 1929. At face value, it appears that households at the 95th- and 80th-percentile positions in 1995 could be as far above the median household, in ratio terms, as their counterparts back in 1929, thus erasing all the leveling of the 1929-1953 era. While changes in the basis of measurement pose dangers for such long-run comparisons, there is a case for re-examining the whole basis of the income inequality measurements to see what share of the earlier income equalization has now been reversed.²¹

[Table 5 and Figure 6 about here.]

The overall rise of inequality since the 1970s has cast different moving shadows when viewed from a regional, racial, or gender standpoint. Among

regions, it took the form of a 1978-1988 reversal in the continuing convergence of regional incomes-per-capita in the United States. After that decade of widening, some narrowing of regional gaps resumed (Amos 1989, Fan and Cassetti 1994, Husted 1991; Ram 1992; Nissan and Carter 1993; Sherwood-Call 1996). On the racial front, the relative income position of blacks failed to make progress after 1975, especially for black males, though it did not retreat on the average (Oliver and Shapiro 1995; Donoghue and Heckman 1991, Freeman and Bound 1992).

America's gender pay gap has been particularly wide because the whole pay structure is more spread out in America. That is, gender pay gaps tend to be correlated with overall occupational gaps across industrialized countries, the main exception being the high relative pay for Australian women. Still, the 1980s and early 1990s brought a peculiar cross-current. American women swam upstream against the general rise in inequality, posting their best relative gains in pay per hour of any decade since the mid-nineteenth century (Goldin 1990; O'Niell and Polachek 1993; Blau and Kahn 1995 pp. 106-7; Blackburn and Bloom 1987; Cancian et al. 1993). This dramatic improvement in women's relative position came later than in other countries, and appears to have owed much to the rise of anti-discrimination enforcement across the 1980s.

V. THE MAIN SOURCES OF EPISODIC INEQUALITY MOVEMENTS.

Economists' attempts to explain such movements in income inequality generally pass the data through a group-decomposition filter, and then settle on choices of more exogenous underlying causes. The decomposition phase is of great value in channeling the search for underlying causes, because it multiplies the number of separate movements -- changes in between-group inequalities, versus changes in within-group inequalities, versus inequality changes due to

shifts in group weights -- that any underlying theory must explain. When the decompositions are done, however, six kinds of causal forces usually are chosen for the task of explanation:

- (1) population growth (demographic transition, migration);
- (2) the rate of skills growth per member of the labor force;
- (3) biases in technological change;
- (4) product-demand shifts (either domestic or global);
- (5) labor-market institutions, including unions; and
- (6) government fiscal redistribution.

The first four forces have been featured in most explanations of America's inequality movements. They have been emphasized over labor-market institutions and government redistribution, for the most part, because these fifth and sixth categories were smaller shares of American economic life, especially before 1933.

For example, Williamson and Lindert (1980) featured the first three forces in their interpretation of movements in U.S. earnings gaps from 1839 through 1973. The rates of population growth and skills growth were negatively correlated and worked in combination. In particular, one reason why the leveling came in the period 1929- 1948 was the combination of slower population growth and faster skills growth. Conversely, across the nineteenth century population grew faster, skills per worker grew slower, and the skilled / unskilled pay ratio widened. Imbalances in the factor-demand implications of technological (or total-factor-productivity) change played an important complementary role in explaining trend reversals in pay ratios.²²

The recent debate on the causes of the wage widening in Britain and America since the 1970s is another case study, one that has featured demand and supply forces equivalent to (1) - (5) above. The competing views differ in the

relative roles to be assigned to (a) immigration (a part of (1) above), (b) slowdown in skills growth, (c) labor-saving technological bias, (d) shifts in domestic product demand (part of (4) above), (e) increasing import competition and out-sourcing of supply sectors (also a part of (4) above), versus (f) the decline of labor-union power ((5) above).

On the heavily-studied American experience since the 1970s, there seems to be an emerging consensus that the international parts of the story -- immigration, out-sourcing, and trade competition -- will explain part, but less than half, of the observed widening. Biased technological progress and the deceleration of skills growth across the 1980s combine to explain a large part of the recent widening.²³ Labor-market institutions, our force (5) above, do play a role in twentieth-century income movements, even in the United States. Several writings by Richard Freeman (e.g. 1980, 1993) have shown that unionization trends shaped both the U.S. wage compression of 1929-1953 and the more recent U.S. wage widening. Blau and Kahn (1996) confirm that de-unionization and decentralized wage bargaining account for most of the peculiarity of the American income distribution relative to Europe.

The sixth force, government fiscal redistribution as an influence on the inequality of pre-fisc incomes, remains a singular challenge. It is always hard to trace effects of tax-transfer progressivity or regressivity back onto the pre-fisc distribution. We can test the premise, however, that the movements in pre-fisc inequality (equalization) seemed to follow trends toward regressivity (progressivity) of the fiscal structure. Crude tests of this sort can be sketched for Britain's pre-fisc income leveling up to the 1970s and for both countries' return to greater inequalities thereafter.

Could all of Britain's income leveling up to the 1970s have been the result of government fiscal redistribution?²⁴ That is possible, even though we are following measures of pre-fisc income here. Perhaps government took such a

large confiscatory tax bite from the richest in society, year after year, as to reduce their share of non-human wealth and therefore of property income, bringing about the overall leveling we observe.

There are at least three reasons why fiscal redistribution probably does not explain all of the observed leveling of Britain's pre-fisc incomes since the late nineteenth or early twentieth century:

(a) The compression of occupational pay ratios could not have come from fiscal redistribution as such, and it was large enough that it must have accounted for a noticeable share of the income leveling.

(b) The income leveling occurred in many countries, some with more progressivity than Britain and some with less (Lydall 1968, Lindert and Williamson 1985, Phelps Brown 1988).

(c) The historic decline in the income share of the top 5 percent seems to have started before the tax-transfer system took a particularly large bite from that top 5 percent. The early estimates by Lord Samuel (1919) imply that the top 5% paid only something like 10 percent average tax on unearned income in 1903/4 and only 5-7 percent on earned income, versus 5-9 percent for everybody else in the taxpaying ranks. By 1913/4 Lloyd George and others had raised the top-5-percent tax bite to 15 percent on unearned income and 7-8 percent on earned income, versus 5-7 percent on all other taxpayers. These differences would not seem large enough to have caused the declines in the top 5 -percent share we observe. Granted, Barna (1945) has estimated that by 1937 the average tax take from the top 5 percent had risen greatly, to numbers like 40-60%, versus 20 percent for all other taxpayers. The CSO estimates for 1953 say something similar. These 1937 and 1953 snapshots do indeed imply a system that could radically re-shape the holding of property income. But if further study of interwar tax incidence confirms that the progressivity did not single out the top 5 percent much until the 1930s, the point will remain that much of the leveling had

taken place before the linkage from differential tax rates to differential property accumulation could have taken effect.

Could Britain's widening pre-fisc inequalities since the 1970s have been the result of a prior regressive shift toward lighter taxation of the top income ranks? The recent history is difficult to read. There was indeed a long uneven decline in the progressivity of tax-transfer effects from 1949 to 1980, to judge from the usual kind of incidence calculations published in Economic Trends. One might imagine that this set the stage for the reversion toward higher property-income growth in the top ranks since the 1970s. Yet from 1980 to 1984, over the first half of the Thatcher government, the figures show a pronounced rise in progressive redistribution through government, placing the mid-Thatcher years alongside the Attlee years as the most progressive spells of the whole postwar era. The underlying reason, of course, is that the early-1980s return to progressivity was unintentional: Unemployment soared so much that fixed entitlement formulas raised the transfers toward the poor. It is only after 1984 that one sees a simultaneous combination of increasing regressivity and increasing pre-fisc inequality (Atkinson 1996, 1997). If there is a longer-run feedback from regressivity to pre-fisc inequality in recent decades, only a more detailed calculation can quantify it. ²⁵

The 1980s US income widening might have been slightly augmented by a retreat from progressivity. While we again lack a detailed tracing of the feedback from regressivity trends to subsequent pre-fisc inequality, studies of the determinants of post-fisc inequality do show that regressivity and pre-fisc inequality marched together in America since the late 1970s. Gramlich, Kasten, and Sammartino (1993, pp. 233-243) find that of the 6.8 percentage point rise in the post-fisc Gini for US family incomes between 1980 and 1990, a pre-fisc rise accounted for 5.0 points and a shift away from tax-transfer progressivity accounted for the remaining 1.8 points.

Thus in three cases -- Britain's pre-fisc leveling, plus the widening of pre-fisc inequalities in both countries since the 1970s -- the trends in fiscal progressivity (regressivity) were more or less followed by trends toward pre-fisc income leveling (widening). The timing is imperfect, however, and the underlying link awaits more detailed studies covering decades of data.

VI. LESSONS ABOUT LONG-RUN CHANGES.

In addition to spotlighting the six forces that shape most of the episodic swings in income inequality, the accumulated history of British and America also offers generalizations that span the sweep of the last three centuries. These generalizations yield predictions about the future experiences of the world's least developed countries. They also light the way to the next phase of research on what drives inequality in the long run.

A. The Kuznets Curve as a Milky Way.

First, it is evident that the Kuznets curve flickers. It cannot steadily illuminate all inequality history, any more than the Phillips Curve reliably links unemployment to wage-price inflation. Best seen dimly in the distance without the distraction of competing light sources, the Kuznets Curve is still visible as a convenient tendency related to the development process. It blurs into the background where Kuznets admitted he had the greatest doubts, namely in the early-modern settings where he thought inequality might have risen. As noted earlier, that is as we should have expected, since countries begin sustained development from radically different initial distributions, especially land distributions. The downslope of the inverted U stands out more clearly and predictably. So does the end of the downslope.

B. The Robin Hood Paradox.

A final pattern that emerges over the centuries points toward a different path for future research on the determinants of inequality trends.

The pattern is this: Across time and across jurisdictions, redistribution toward the poor is least achieved where it is most warranted by the usual principles of welfare policy, such as cushioning the lowest absolute incomes most, redressing inequalities where they are the greatest, and encouraging labor-force re-entry. Elsewhere I have called this the Robin-Hood Paradox, since the paradox suggests that Robin Hood's redistributive army is missing when and where it is most needed (Lindert 1991, pp. 226-231). There is an immediate corollary for trends in redistribution and inequality: A rise in pre-fisc inequality will be accompanied by a shift toward fiscal regressivity, and an era of leveling will be an era of increasing fiscal progressivity.

The earlier and poorer the setting, and the greater the inequality, the stronger the case for taxing property to aid the poor. With a large share of the population near subsistence and in poor health, there is a good chance that giving aid will raise labor supply: The aid can improve workers' health and survival enough to outweigh any incentive to take more subsidized leisure. Yet the earlier and poorer the setting, the less that support was given.²⁷ With the advance of average incomes, and especially in the eras when pre-fisc inequality was also being reduced, aid to the poor became more generous. As we have seen, recent experience hints that the correlation might even hold when the trend is away from, not toward, pre-fisc equality. That appears to be the case in the United States, though in Britain the temporal correlation was weakened by the temporary rise of redistributive spending from 1979 to 1984. Over space, the paradox also holds more often than not. Certainly in today's global international cross-section, progressive redistribution toward the poor correlates

strongly with both average incomes and pre-fisc equality of incomes. Across sub-national jurisdictions, the same is often true. In twentieth-century America, particularly before the late 1960s, the poor have received more aid, even as a share of average incomes, in those states where poverty has been less severe. There are spatial exceptions to the paradox, however.²⁸

How could pre-fisc inequality be correlated with a regressivity in taxes and transfers? Here we are triply challenged. First, there is that difficult task of quantifying the feedback from the tax-transfer system to the pre-fisc Lorenz curve. A second challenge added here is to determine how pre-fisc inequality in turn affects society's willingness to redistribute between income ranks. Having received hints about a simultaneous relationship between redistribution and pre-fisc inequality, we must solve the problem of estimating them simultaneously. Correct appreciation of the influence of fiscal redistribution on pre-fisc inequality waits upon the simultaneous identification of the determinants of the redistribution itself.

Before sending the task off to the econometric laboratory, however, one should formulate a strategy for dealing with a third research challenge, one related to political voice. Our usual hunches about the effect of income distribution on redistributive policies are in danger of colliding with the overall empirical pattern summarized by the Robin Hood paradox. The quickest way to see the third research challenge is to think of an unequal and underdeveloped society, like Britain before the 1830s or a Latin American country today. In such societies, incomes and socio-economic mobility are highly skewed. There is a wealthy elite far above the rest of the ranks, and the mean income far exceeds the median. Our usual theoretical priors are that such a skewed society is ripe for taxing the rich, with the median voter preferring a high rate of progressive taxation. So say most recent models of the redistributive process (e.g. Peltzman 1980, Meltzer and Richard 1981, Kristov et al. 1992, Alesina and Rodrik 1994, Persson and Tabellini 1994). If so, then why do we observe the opposite, with

such less-developed and highly skewed societies yielding the least redistribution from rich to poor?

The answer must lie in the relationship of the income distribution to political voice. In fact, highly skewed societies are ones in which the wealthy elite retains a high share of political power as well as of wealth and income. The usual pressure-group models, such as median-voter models, should not be applied until they are cast in terms of the self-interests of those who actually have political voice. In the highly skewed societies, the median voter is often someone up in the top quintile of the income ranks. Thus, for Britain, the task is to re-examine how the self-interests of well-to-do swing voters were transformed by the Reform Acts of 1832, 1867, 1883-4, and beyond. For the task of understanding what is so different about America, it is essential to incorporate the peculiarly low rate of political participation of America's poor.

Here, surely, is a key to resolving the mysteries of how redistribution through government relates to overall inequality. Only when we have a tested working theory of the three-way relationship between income inequality, inequality of political voice, and redistribution through government, will we have a clear view of any of these three sides to the inequality issue.²⁹

FOOTNOTES

¹ For a review of alternative concepts of the standard of living, with some discussion of inequality movements, see Steckel (1994). There is a large literature on the economic valuation of gains in life expectancy (e.g. Usher 1973, Williamson 1984), but without quantification of its impact on the inequality of living standards, a task left to Jackson (1994).

² To emphasize that either view to the inequality of life expectancy seems valid, depending on the question being pursued, I should note that I have viewed it both ways. In (Lindert 1991, p. 214) I suggested a focus on individual lifetime consumption patterns, so that, for example, infant deaths in any social class would raise the inequality of living standards among individuals. This view is developed and quantified in Jackson (1994). A comment on Britain on the same page, and in (Lindert 1994), I reverted to the implicit convention of viewing infant mortality as a subtraction from the well-being of households in the affected ranks of the income distribution.

³ See, in particular, Soltow (1968, 1969, 1971, 1975, 1984, 1989, 1990, 1992); Williamson and Lindert (1980); Lindert and Williamson (1983, 1985); Williamson (1985, 1991); Lindert (1986, 1991, 1994); criticisms of Williamson (1985) by Jackson (1987) and Feinstein (1988); criticisms of Lindert (1991) by Jackson (1994); Phelps Brown (1988); and van Zanden (1994).

⁴ The choice of population units is driven by data availability. In this case it is expedient to compare estimates for households. The early social tables sometimes called them “families” but apparently included servants in wealthy households and unrelated adult individuals at the bottom of the distribution. The rest of this chapter alternates between households, earners, and adults, depending on which units are offered in the available series.

⁵ This view of rent/wage trends rests on a miscellany of sources. The wage series are the Phelps Brown - Hopkins wage for building laborers and John's (1989) farm wage rates. The rents are those reported in John (1989), in my gleanings of several rent series in (Lindert 1983 working paper), and in an updated version of Gregory Clark's (1991) rack-rent series. Clark's current estimates of the rental/wage ratio in English agriculture show a large sustained rise from 1740 all the way to 1840.

Another crude hint also points to the era ending in the French Wars as the top candidate for rising inequality in Britain. Between 1780 and 1801 the current-consensus estimates of national product per employed person grew substantially, whereas the real wages received by broad groups of workers stagnated or even declined (Feinstein 1996b, and the sources cited there). Growth rates between the 1801 and 1831 benchmarks again suggest faster growth in average national product than in real wages, though the hint looks stronger for 1780-1801 than for 1801-1831.

⁶ The wealth distributions for England and Wales 1670-1875 are detailed and interpreted in Lindert (1985, 1986, 1987). The financial and social position of the very top wealth-holders was described at length by Rubinstein (1981, 1986). See also Soltow (1990) on Scottish landed wealth in the eighteenth century.

Future research could narrow the wide confidence intervals on wealth inequality for the mid-nineteenth century reported in Lindert (1986), by using the death duty returns in the Public Record Office, which were unavailable at the time of my research. These returns attach real estate to personal estate more closely than I could do by collating materials from separate sources.

⁷ Though he refused to make a will, "Karl Marx, Gentleman, a Widower" left almost £300 in personal estate in 1883, according to the Principal Probate Registry. Frederick Engels, again a "Gentleman," left £25,265 a dozen years later.

Other personal-estate probate entries (excluding real estate in each case) include £31,821 for Sir Isaac Newton in 1727, almost £10,000 for Sir Frederick Morton Eden in 1810, almost £300,000 for David Ricardo in 1823, and £129,542 for Charles Dickens in 1870 (Public Record Office, PROB3/26/66 and IR59).

⁸ Relative to other countries, mid-Victorian Britain (1867-1875) stood out as a nation of extreme inequality in landownership, personal net worth, and pre-tax incomes (Lindert 1987). We lack sufficient data to say definitively, however, whether Britain occupied the absolute top inequality position among major nations at the time.

⁹ While carefully noting that earnings inequality and overall income inequality need not follow the same trends, Williamson felt that they just happened to rise and fall together in nineteenth-century Britain. This coincidence no longer holds, however, now that the present Table 1 (like Feinstein 1988, Jackson 1994, and Lindert 1994a) has adjusted the key 1867 income distributions to a household basis more comparable with earlier and later income distributions.

An earlier movement noted by Williamson (1985, pp. 47-49) also differs from a trend in overall income inequality implied by the social tables. He found that pay gaps narrowed from 1781 to 1805, before rising again. The narrowing of employee pay rates during the French Wars is a plausible counter-current in the presumably turbulent income movements of that era. Sudden wartime inflations often compress the pay ratio between higher- and lower-paid employee groups, because higher salaries tend to advance more steadily, less cyclically, than the wage rates of lower-paid groups.

The unusual compression of employee pay gaps around 1805 is consistent with the conclusion that overall inequality had widened considerably (that rise from 1759 to 1801/03 in Table 1). Those in the skilled manual trades and lower-paid professions, whose nominal pay failed to keep pace during the wartime

inflation, were probably dropping down the quantile ranks as well, while farmers, yeomen, and farm laborers were probably rising. Even tenant farmers and yeomen on long-term leases must have shared some of the wartime jump in the residuals generated by farming. (The relative income of handloom weavers, a non-wage 3-4 percent of the labor force, also peaked briefly in the French War era, on the eve of the weavers' infamous demise).

My overall impression of the changes from the mid-eighteenth century to the French War era is that top groups gained relatively to all others, while many occupations reshuffled their relative positions in the lower income ranks. The identity of the fastest-gaining top groups is an uncertain mix of landed aristocracy and top merchants. The top-end gainers in the income distribution were the top 5 percent of households (Table 1), but the top 1 percent did not gain in income share, unlike the gain shown for the top 1 percent in the wealth distribution (Table 2). For much richer detail on the social and occupational identities of the richest individuals, see Rubinstein (1981, 1986).

¹⁰ A technical point of considerable importance here is that much of the top income households' housing was owner-occupied. The available data apparently do not impute income from owner-occupied housing as part of nominal income. Accordingly, it should also not be counted as part of the consumer bundle purchased by home-owning households. Thus, rent was a lower share of household income for the rich than for the poor, and the rapid rise in rents hurt lower-income purchasing power more than the purchasing power of the rich. This difference in housing weights and the difference in food weights explain why real inequality probably rose more between 1759 and 1801/03 than did nominal inequality. For a fuller discussion, see (Lindert 1998).

This point seems to have been missed by the otherwise excellent coverage of recent UK class differentials in housing costs by Crawford (1996, 89-90), who views the opportunity cost of wealth tied up in owner-occupied housing as a

user-cost part of the cost-of-living deflator for income measures that failed to include the full value of that housing. Yet Crawford does usefully capture the capital-gain effects in his user-cost measure.

¹¹ On mortality trends by class and age group, see Hollingsworth (1977); R.V. Jackson (1994); Wrigley and Schofield (1981); Wrigley et al. (1997); Woods (1988-89, 1993); Williamson (1984); Lindert (1994a); Floud and Harris (1996).

¹² The phrase “wage and salary rates” is chosen over “wages and salaries” or “earnings” in order to set aside the changes in male / female income inequality that reflect differing trends in the annual labor hours and labor-force participation of women. As argued elsewhere (Lindert and Williamson 1983, pp. 17-19; Lindert 1991, p. 374), it seems wiser to focus on the wage-price of a unit of a woman’s time as a measure of her earning potential. This approach strikes a compromise between the extremes of valuing women’s unpaid time at zero and valuing it above the wage rate (as would be valid for women who actually choose to work zero hours for pay). Most of the literature still adheres to the former extreme view, interpreting non-participation in the labor force, or any reduction of hours worked, as a shift toward a use of women’s time that is worth zero.

¹³ Recently Lars Osberg and Fazley Siddiq (1988) have argued that slaves should be counted as having had negative net worth, equal to (minus) £155 per slave household in 1774, because their freedom was denied them. On this basis they conclude that colonial wealth inequality was much greater than today’s wealth inequality. The assumption and interpretation do not seem valid. They offer no defense of the large absolute value of £155 per slave household, which nearly equals the mean wealth of all households at the time. Why not £1 or £10,000, and what is such a valuation (of freedom?) doing in a distribution of capital excluding free people’s ownership of their own human capital? And why

choose a value so large that this arbitrary valuation of negative Southern wealth drives the whole conclusion about all 13 colonies? The conventional procedure followed here at least lends itself to familiar interpretations. In addition, their interpretation should have included the point that the time-trend would still follow a great rise from colonial egalit  toward greater inequalities, starting from the relatively non-slave 1630 and rising for over a century, possibly rising all the way to 1860 (depending on what negative values they would put on the net worth of slaves who had a higher real price in 1860 than back in 1774).

¹⁴ For a list of relevant colonial wealth studies by Bruce Daniels, Allan Kulikoff, James Lemon, Gloria Main, Jack Main, Gary Nash, Daniel Scott Smith, and Gerard Warden, and others, see Williamson and Lindert (1981).

¹⁵ The inequality trend implied by the rise in the slave share of the population across the colonial era was pointed out by Robert Gallman (1981, p. 133).

¹⁶ For an extensive survey, see Williamson and Lindert (1981). A more recent contribution, one that follows individuals over time, is Steckel (1994).

¹⁷ The values of non-resident Loyalist estates available for confiscation as of the 1770s are sketched by Shammas (1993). We still need better post-Revolutionary numbers, however, on who acquired these assets.

¹⁸ The underlying data here are Richard Easterlin's estimates of state and regional income, as reproduced in Fogel and Engerman (1971) and in the Historical Statistics of the United States, and as transformed into an inequality measure in the earlier article by Williamson (1965). The measurement of real, as opposed to nominal, regional income gaps is pursued with spatial cost-of-living indices in Coelho and Shepherd (1976) and Williamson and Lindert (1980, Ch. 5). The real gaps move like the nominal ones, albeit at lower levels of inequality.

For a recent overview of the regional inequality motif, see Nissan and Carter (1993).

¹⁹ See Jenkins (1995) on both the alternative trend series and the decompositions by population group, and also Atkinson and Micklewright (1992, 269-278 and Tables BE1-BI4), Johnson and Webb (1993), Smeeding and Coder (1995), Atkinson (1996, 1997), and Goodman et al. (1997) on the trends.

²⁰ In addition to the series shown in Table 4 and Figure 5, see Blackburn and Bloom (1987, 1994); Danziger and Gottschalk (1993, 1995); and Raj and Slottje (1994).

²¹ The author thanks Claudia Goldin, Lawrence Katz, Lawrence Mishel, and the U.S. Census Bureau for guidance on the mis-measurement of top U.S. incomes. My attempts to produce better estimates of incomes above the 95th percentile with the help of tax-return data have been unsuccessful, leaving Table 5 and Figure 6 as the best set of indirect clues.

²² The computable-general-equilibrium (CGE) exercises performed by Williamson and Lindert should be extended in a number of directions. First, the model should be complicated to include more than four factors of production and more than three output sectors, including input-output ratios between the output sectors. Second, it could incorporate forces that shift product demand, such as tariff policy and transportation costs, as Williamson (1974) did when analyzing growth rather than inequality. Third, it could be used to explain movements in the relative returns to non-human property, as O'Rourke et al. (1996) have done for international patterns of movements in land rents.

²³ See Lawrence and Slaughter (1993); Murphy and Welch (1993); Berman, Bound, and Griliches (1994); Wood (1994, 1995); Katz, Blanchflower, and Loveman (1995); Burtless (1995); Feenstra and Hanson (1995); Richardson (1995);

and the whole January 1995 issue of the New York Federal Reserve Bank's Economic Policy Review.

²⁴ Bear in mind that the only government interventions being considered here are taxes and transfers, with no attention to industrial relations laws, incomes policies, and other less-budgetary tools of government.

Note also that the text here is considering the effect of taxation on income equalization, not its effect on wealth equalization. The fisc's share of the credit for wealth equalization might be different from its share of the income equalization. In particular, it could be that a greater share of the wealth equalization achieved by 1938 was due to taxation of high unearned incomes, and less to other forces, than for the income movements featured here.

²⁵ In the absence of detailed calculations about feedbacks from tax-transfer regressivity to pre-fisc income inequality, all we have are the kinds of studies that document the co-existence of the two movements, by decomposing the sources of change in post-fisc inequality. Thus for the United Kingdom between 1979 and 1988, Johnson and Webb (1993) estimate that the changes in the tax-benefit system account for 43 percent of the shift in post-tax-and-transfer income inequality, versus only 23 percent for the widening of earnings, 29 percent for the rise in unemployment, and 5 percent residual noise. As the text makes clear, that effect of the tax-transfer system must have come after 1984.

²⁶ "Land" here should include mineral and forest rights. Bourguignon and Morrisson (1990) have rightly stressed the importance of mineral rights in explaining international differences in inequality and skewness.

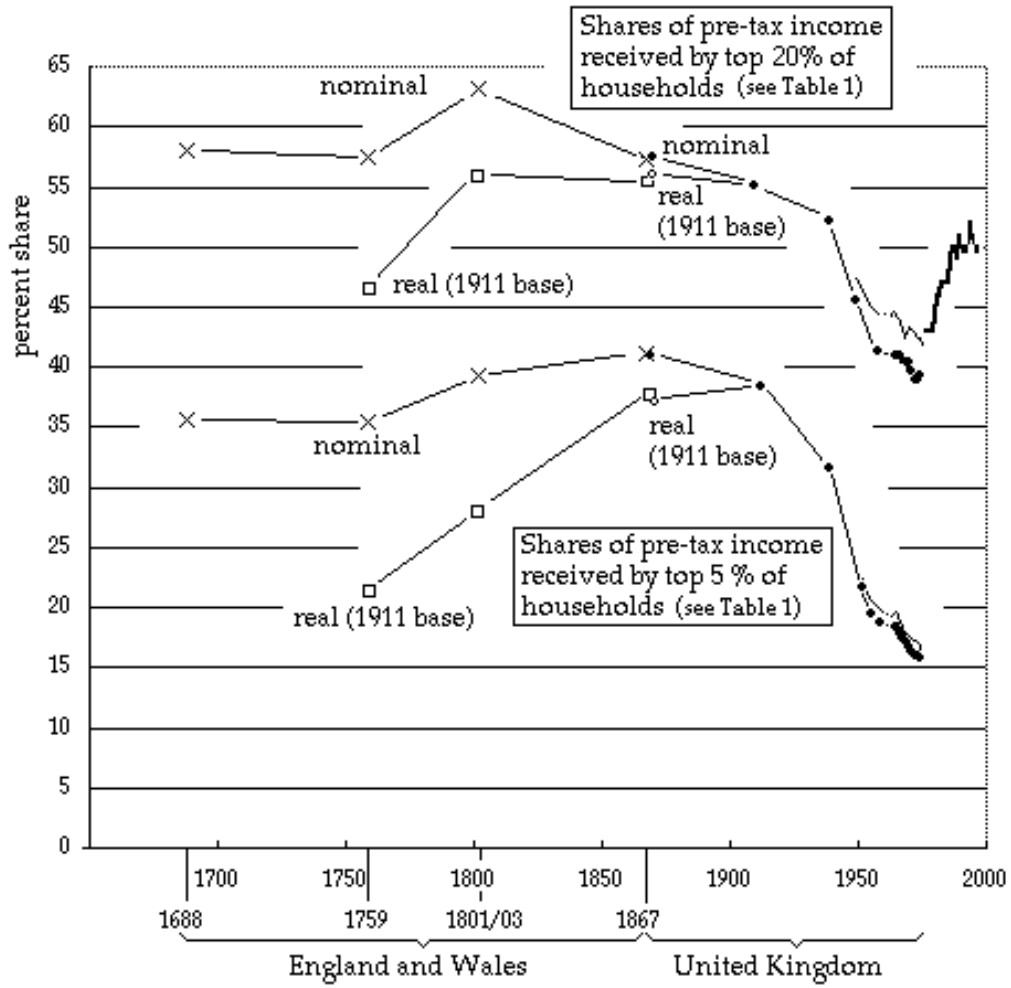
²⁷ Here the text concentrates on trends in Britain and other European settings, where the earlier settings remained highly unequal and average incomes grew across the nineteenth century. In such settings the paradox predicts a drift toward poor relief. For early America, the trend predictions of

the paradox are mixed: Per-capita income growth across the nineteenth century would favor giving more to the poor, but the rise in inequality would cause less to be given.

²⁸ One exception relates to the distribution of poor relief across the parishes of England in the Old Poor Law era 1780-1834. In that case, tax-based poor relief was indeed most generous where poverty was greatest, namely in the rural Southeast. This pattern has been well explained by George Boyer (1985) as a reflection of differences in the lobbying power of labor-hiring landlords. In the southeast such landlords had disproportionate power in local government, and outvoted the non-hiring family farmers, raising local poor rates so as to keep the poor around during the winter.

²⁹ Some initial headway into the three-way relationship between income inequality, redistribution, and political voice has been made empirically by Lindert (1994b, 1996) and Barro (1996), and theoretically in a new model by Acemoglu and Robinson (1996).

Figure 1. Income Inequality Trends in the United Kingdom since 1688



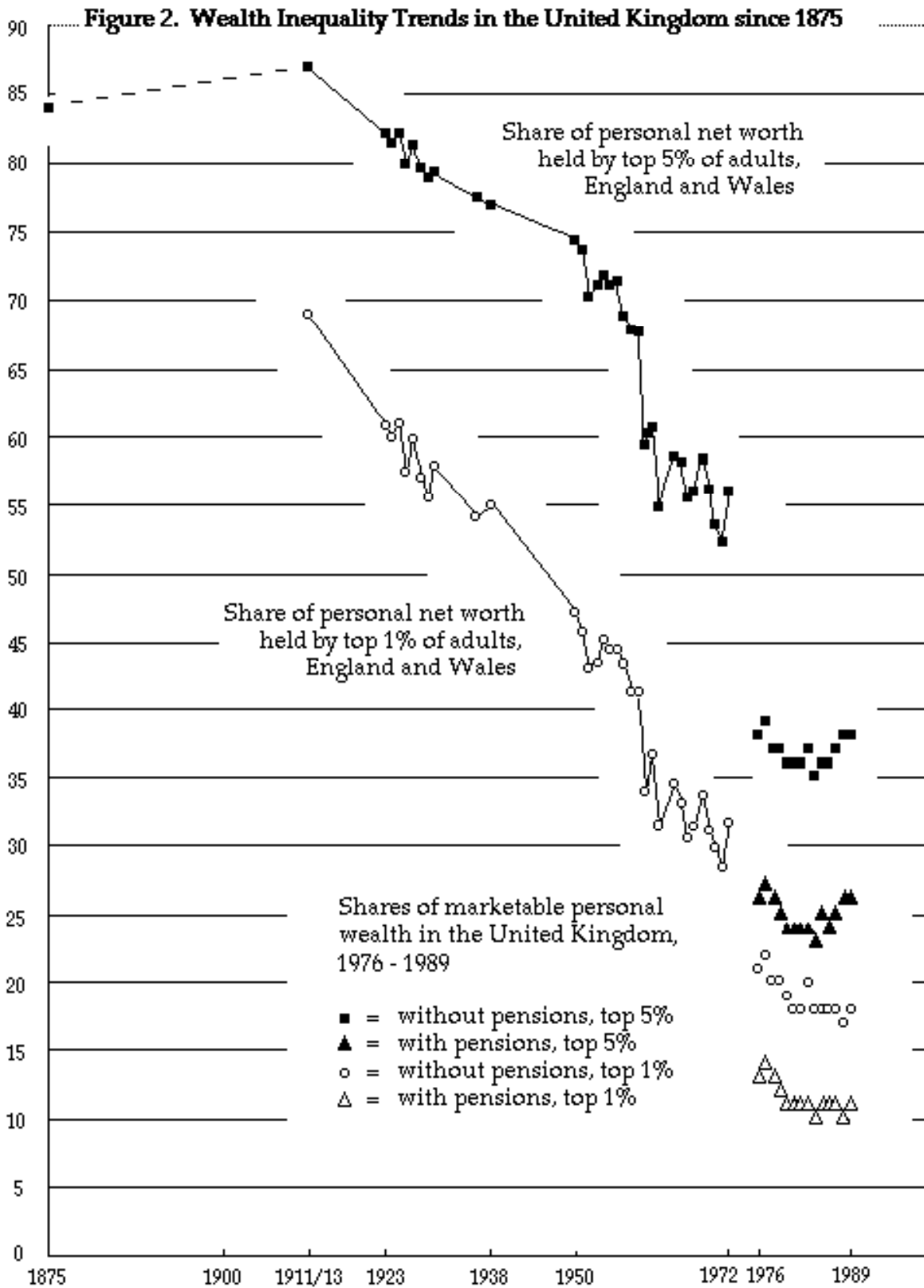


Figure 3. Wealth Inequality Trends in the United States since Colonial Times

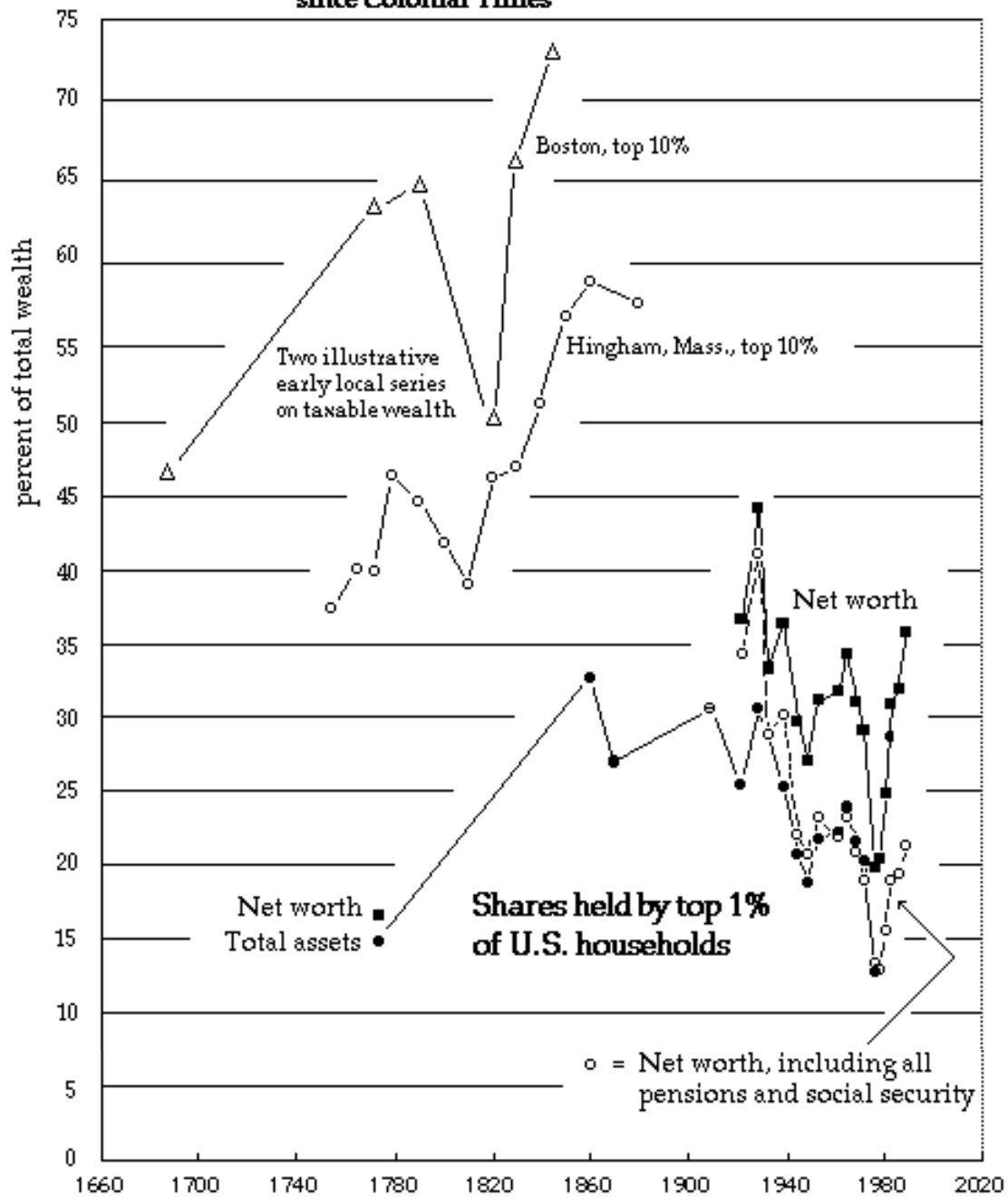


Figure 4. Income Inequality Trends in the United Kingdom since 1911

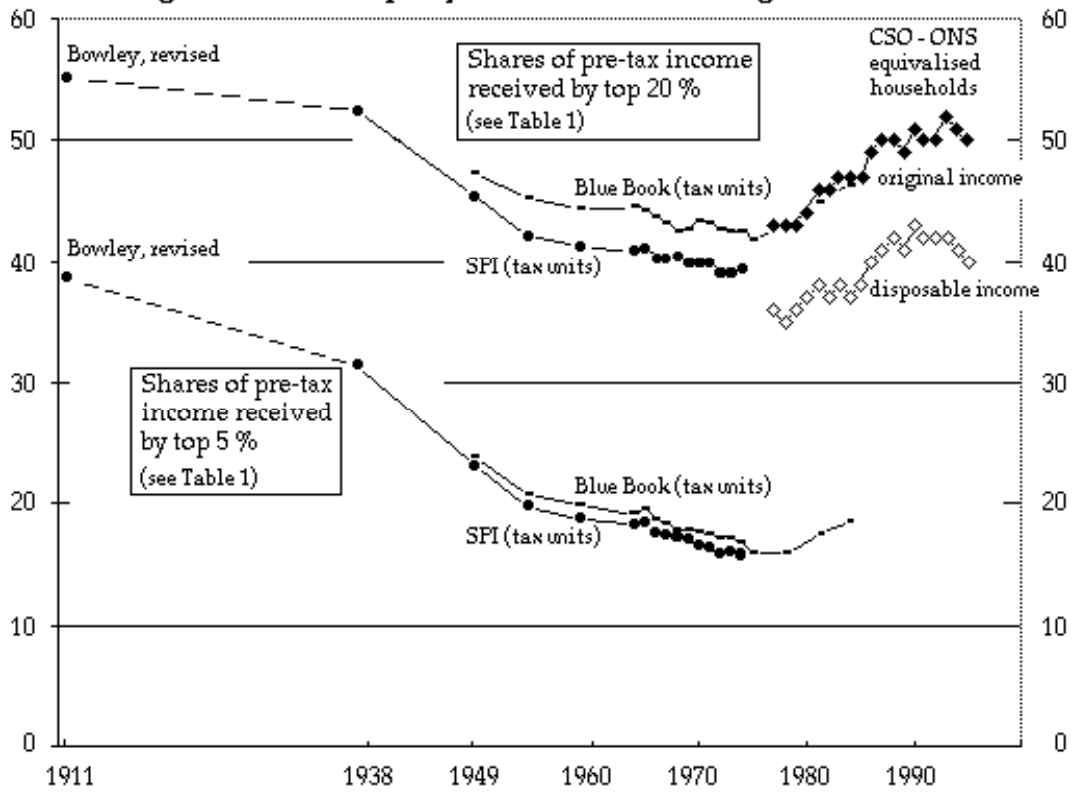
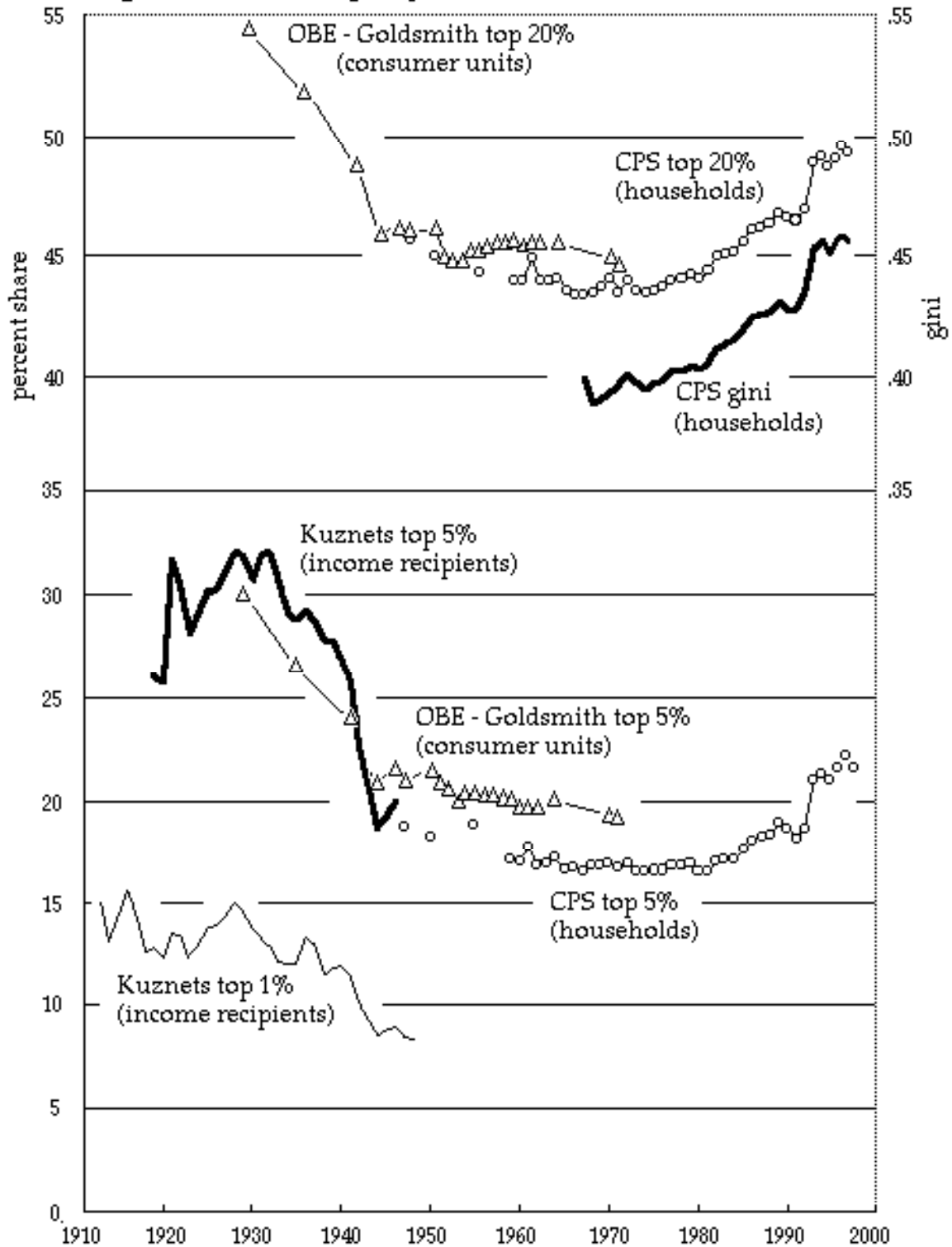


Figure 5. Income Inequality Trends in the United States since 1913



**Figure 6. An Alternative View of U.S. Inequality since 1929:
High-Percentile Incomes versus the Median Income**

