

# American Incomes 1774-1860\*

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## Abstract

Building what we call social tables, this paper quantifies the level and inequality of American incomes from 1774 to 1860. In 1774 the American colonies had average incomes exceeding those of the Mother Country, even when slave households are included in the aggregate. Between 1774 and 1790, this income advantage over Britain was lost, due to the severe dislocation caused by the fight for Independence. Then between 1790 and 1860 US income per capita grew even faster than previous scholars have estimated. We also find that the South was initially much richer than the North on the eve of Revolution, but then suffered a severe reversal of fortune, so that by 1840 its white population was already poorer than free Northerners.

In terms of inequality, our estimates suggest that American colonists had much more equal incomes than did households in England and Wales around 1774. Indeed, New England and the Middle Colonies appear to have been more egalitarian than anywhere else in the measureable world. Income inequality rose dramatically between 1774 and 1860, especially in the South.

The paper offers an open-source style, since our data processing is posted on <http://gpih.ucdavis.edu> (click on the folder “American incomes 1774-1870”). Detailed defense of the 1774 and 1800 benchmarks can be found in our previous NBER working paper (17211, July 2011), although the estimates reported here are revised. The 1860 benchmark is completely new.

Key words: Inequality, growth, early America. JEL: N11, N31, O47, O51

## Acknowledgements

Our work has been aided greatly by a battalion of scholars and archivists. The list of helpful colleagues includes Jeremy Atack, Paul Clemens, Paul David, Farley Grubb, Herb Klein, Allan Kulikoff, Bob Margo, Paul Rhode, Josh Rosenbloom, Carole Shammas, Billy Gordon Smith, Richard Sylla, Sam Williamson, and especially Tom Weiss. Archival help was supplied by, among others, Jan Kinzer (Pennsylvania State Archives), Diana McCain (Connecticut Historical Society), Clifford C. Parker (Chester County Archives), and Marc Thomas (Maryland Historical Society). We thank them, yet absolve them from any responsibility for the results presented here. We also acknowledge with gratitude the research assistance of Lety Arroyo-Abad, Julianne Deitch, Sun Go, Patricia Levin, David Nystrom, Brock Smith, and especially Nick Zolas. The paper has also benefitted from earlier suggestions by participants at the University of Warwick – Venice, the University of Michigan, the Institute for Research on Poverty at UW-Madison, the NBER’s summer DAE workshop, and the World Economic History Congress at Stellenbosch.

## **I. Growth and Inequality Debates about America's First Century**

Economic historians need fresh information about American income levels and their distribution at the end of the colonial era and the dawn of independence in order to understand growth and evolving social structure over America's first century. The need for more evidence becomes apparent whenever we try to cast back from the late colonial period, to project ahead from the colonial and early federalist years, or to view American incomes in trans-Atlantic perspective.

Modern debate over American growth experience before 1840 starts with Paul David's classic 1967 article offering what he called controlled conjectures. David (1967, 1996), Robert Gallman (1992, 1999), Thomas Weiss (1992, 1993a, 1993b, 1994), and others focused their conjectures on the division of the economy into large output sectors, each with its own labor force and labor productivity growth. All of these competing estimates have been built up from the output side in the spirit of Simon Kuznets. This paper offers something quite different, building up estimates from the income side in the form of what have been called social tables. Not only does our approach offer fresh new estimates of American incomes at three benchmark dates – 1774, 1800, and 1860 – but it also allows us to estimate income distribution within regions and for America as a whole. Until recently, our social tables approach would have been impossible given a paucity of data on the occupations and the sector mix of the labor force before 1840. The early censuses did not help much with these, except to give indicators that should have affected labor force participation, such as sex, race, age, region, and urban/rural. Accordingly, we have long thought that a new attack on the issue of early American growth must feature new information on the American labor force by occupation in the form of social tables. Even though many household heads were simply called artisans, merchants, farmers, planters, farm hands, or slaves, it helps considerably to know what labor force shares they represented, where they lived, their average incomes, and how those incomes changed over time.

Our interest in American incomes around the time of the Revolution is enhanced by viewing them from across the Atlantic. Angus Maddison (2007) estimated that it was not until after the 1870s that the United States caught up with the United Kingdom in real GDP per capita, though active debate has ensued about the uncertainties of his index numbers.

Maddison's estimates need to be reconciled with the fact that America attracted a significant net emigration from the Mother Country and that colonial and republican population growth was much faster there. In the light of current research on the Great Divergence, on the history of European incomes, and on the continued use of the Maddison world income estimates, we think the time is ripe to add data from the American side to compare with the new estimates for Europe.

We can now offer new estimates based on more archival data than were available to earlier researchers. The estimates are offered as an "open-source" presentation of our detailed data and procedures on the internet, for both negative and positive reasons. The negative reason is that many scholars might resist accepting some new estimates based on vulnerable primary data, wishing to offer their own estimates. The positive reason for open sourcing is the dynamism of the database itself. The information explosion that has offered us new data will continue to offer additional data to all scholars in the future. Maximizing the disclosure of our data and procedures accelerates the opportunities for improving the reliability of the estimates. Hence, our work is inextricably tied to a growing downloadable set of spreadsheet and text files at <http://gpih.ucdavis.edu>.

Our findings confirm some conventional wisdom about American growth and inequality between 1774 and 1860, yet contradict others, and also introduce some brand new issues. They certainly offer a much clearer view of colonial American inequality and how the incomes of different classes compared with those of their counterparts in England. Inequality was much lower in 1774 and 1800, especially among free whites, than in England. Inequality was also lower than in the United States today. We find higher colonial incomes in 1774 than have previous scholars, especially higher in the Southern colonies. Indeed, average incomes in 1774 were higher than those in England and Wales, using either exchange rates or purchasing power parity guesses. But they may have been lower in 1800. Thus, America gave up its per capita income leadership to England between 1774 and 1800, as a price paid for independence. Our estimates raise new questions about what happened between 1774 and 1800, during the disastrous war years and the costly post-war struggle with confederation, only partially offset by the 1790s boom. But our findings also suggest that America's growth rate between 1800 and 1860 was quite a bit faster than has been previously thought. The Old South did not share that

dynamic experience, however, since its reversal of fortune,<sup>1</sup> which had started after 1774, continued after 1800.

## **II. Estimating Three Social Tables for America's First Century: 1774, 1800, and 1860**

### **A. The 1774 Colonial Evidence**

Our approach starts by counting people by occupations or social classes, and mustering evidence about their average incomes. That is, we build national income and product accounts (NIPA) from the income side. This departs from all recent scholarship on early American growth, which has built its real income series from the production side, and then used price indexes to report nominal incomes for the price levels of that time. Historians will immediately recognize our approach as that of building *social tables*, in the “political arithmetick” tradition spawned by such Englishmen as Sir William Petty and Gregory King in the 17<sup>th</sup> century. That is indeed our approach here, as it has been in other publications of ours.<sup>2</sup> In fact, at least two early American efforts tried to imitate Petty with their own calculations of what their region was worth – presumably to guess at its ability to pay taxes and fight wars.<sup>3</sup>

Fortunately, the archives continue to accumulate early local returns that recorded people's occupations, including such social labels as “Esquire” or “widow” in the English tradition. Reconstructing society from these sources is no easy task, however, and will continue to be challenging as the primary data accumulate in the future. This challenge requires that the reader take a tour of our data warehouse in which we counted colonials and determined their incomes. The tour will be shorter than the longer one offered in our earlier working paper (Lindert and Williamson 2011: section II).

Any social profile of Americans on the eve of the Revolution must start from local censuses, supplemented by tax lists and occupational directories, and supported indirectly by the earliest national censuses of 1790 and 1800. Fortunately, the recent electronic revolution has made local enumerations from the late 18<sup>th</sup> century much more accessible. While all records before 1790 were local, aggregate regional counts can be developed by assuming that proportions from one documented locality represents those of other localities in the same region and with the same population density, urbanization levels, and qualitative attributes.

Our estimates of early American work status, location, and living arrangement start from basic population counts themselves, and then add early labor force estimates (themselves constructed from labor participation rates by age-sex-slave/free cells), before dividing up that labor force by occupation and by household headship status.

Population census counts. The few local censuses from the colonial period are now collated and referenced in the colonial section of *Historical Statistics of the United States*, both in the Bicentennial Edition (1976) and in the Millennial Edition (2006). These offer detail by sex, race, free/slave status, and rough age distributions for seven colonies; we clone the other six colonies from the seven, matching contiguous colonies.

Labor force participation rates. Next we derived the numbers of persons in the labor force for each demographic group defined by place, sex, race, free/slave, and age. The conventionally defined labor force consists of all persons generating product sold in significant part (or, for slaves, demanded in significant part) outside the household.

To convert population into labor force, we use the detailed labor participation rates for 1800 supplied by Thomas Weiss. It seems reasonable to assume that there were no behavioral changes in the rates defined in the detailed cell-specific Weiss estimates, which give separate rates for such cell categories as urban Pennsylvania's free white females age 10-15, or rural South Carolina's male slaves over the age of 10, or small town Connecticut's free white males aged 16 and older. However, since these categories changed in relative importance over time, the regional and national labor participation rates could and did change between 1774 and 1800.

Recorded occupations. Sketching the social make-up of the labor force requires detailed occupation counts for different localities. We draw on newly accessible counts for years near 1774, though only for a few places, only for parts of the labor force, and only with the help of some comparison of occupational mixes over time and space.

Our effort to reconstruct the American social structure on the eve of the Revolution uses local tax assessment lists and occupational directories. Such lists allow us to create the following occupational groups for the free population: officials, titled, and professionals; big city merchants and shopkeepers; small town and rural merchants and shopkeepers; skilled artisans in manufacturing; skilled in the building trades; farmers (renters, sharecroppers and owner-operators); male menial laborers; and female menial laborers. Our new data re-shape the standard view of the colonial occupational structure. For example, relative to Alice Hanson

Jones (1977), our estimates shift a lot of the Middle Colonies' labor force from middling farmers to less wealthy craftsmen and laborers, and to males with no stated occupation.

For the urban South, we use the 1790 directory for Charleston but scaled back to conform to the estimated total population of Charleston in 1774. One gets the same occupational patterns by starting with Alice Hanson Jones's  $w$  weights for a sample drawn from four Southern states. In either case, one must adjust for the over-representation of landowners and, especially, slaveholders (both absent from their plantations). We adjust the Jones weights, guided by some very useful local censuses from three North Carolina counties in 1779-1782. These enumerated all household heads according to whether they held slaves or real estate or both or neither. We assume that the same adjustment of weights is required in Charleston as in the rest of the South.

For the rural South, we carried out the same adjustment away from slaveholders and landowners, giving instead more weight to ordinary farmers. One could wish for a broader sampling of the rural South than just the Alice Hanson Jones sampling from four states, plus our new sampling from the three North Carolina counties. There are other rural Southern county assessment documents on the internet, but only very few are for dates earlier than 1798, and none of the lists we have seen record the occupations of the household head.

Unrecorded occupations. Persons with occupations recorded by tax assessment lists or urban occupational directories fall short of the persons in the labor force. In most cases the lists even fail to capture all household heads, the exception being those three counties of rural North Carolina between 1779 and 1782, which seem to have captured all free household heads.

Not all members of the 1774 labor force without recorded occupations are equal. Some lack an occupational label despite assessed wealth. Some lack an occupational label, and are listed as tax-exempt because they had zero or near-zero wealth. We distinguish three groups whose occupations aren't recorded: free males with positive wealth listed but without a recorded occupation; free females with positive wealth listed but without a recorded occupation; and free persons listed without a recorded occupation and having zero or near-zero wealth.

Counting Households. One could avoid measuring household headship if we were interested only in measuring aggregate national product, since it depends only on who is in the labor force and their average income. Yet we need the headship rates by occupation to measure inequality.

Households are the income recipient units used here to measure income inequality, for both practical and theoretical reasons. Previous investigators have been forced to confront the simple fact that taxable property, such as real estate, is used by all household members, even if only one is the owner and taxpayer. The prevailing practice is to measure income inequality among households, not among individual income earners. In order to compare apples with apples, we do the same. That is the practical reason. The theory comes from Simon Kuznets (1976), who emphasized the superiority of the household focus. Caring about economic inequality means caring about how unequally people consume resources over their lifetimes. Even if data constraints force us to study annual inequality rather than life-cycle inequality, Kuznets argued that annual income per household was a better measure of income distribution than one using income per earner.

Since the early population censuses usually did not count households, some assumptions must be invoked to decide who were household heads and who lived under the same roof with the head. Fortunately, historians of early America have already grappled with this issue. Following the leads of Billy Gordon Smith (1981, 1984, 1990) and Lucy Simler (1990, 2007; Clemens and Simler 1988) in particular, we estimate the number of household heads from c1774 and c1800 population data from invoking assumptions detailed elsewhere (Lindert and Williamson 2011: Section II). These assumptions have generated total households by place – that is, by region and by urban versus rural.

By subtraction, we derive the number of household heads that are missed by the listed occupation accounts. The shares of heads omitted are often large when the occupational data come from the tax lists and the urban directories. The colonial business directories and the tax lists missed more than 30 percent of all households. Left uncorrected, such counts would underestimate total income and bias inequality downwards, since many of the unregistered were poor. Fortunately, the tax lists nearer 1800 seem to have captured something like the full population, at least according to our samplings from New York State property tax rolls that began around 1799. The same should have been true of the federal direct tax of 1798, which required a household enumeration subject to external audit.

Which groups were most frequently omitted in the colonial era? The literature has advanced the plausible intuition that the omitted consisted mainly of the tax-excused poor, whose names could be safely omitted from tax lists or business directories. Yet, there is also

some evidence that many in the middling and rich groups may also have been omitted, or at least that their wealth was under-assessed. Three questions need to be answered dealing with those who were in the labor force (LF), according to the censuses and the Weiss estimates of labor force participation rates, yet who were not reported as household heads (HHs): First, how many of them were there for each place defined by region and by urban/rural? Second, what kinds of occupations did they have? Third, whose households did they live in? Guided by the censuses, we identify the following groups in the labor force who were not household heads: free white males and females, ages 10-15; free white males and females, ages 16-up, but not household heads; free black males and females, ages 10-15; free black males ages 16-up, minus free black household heads; free black females ages 16-up; white indentured servants in Maryland, the only colony that labeled them separately in a census near 1774; and slaves ages 10-up. Some of these contained laborers who were almost surely paid only unskilled wages, while others were spread over occupations of higher earnings. What we assumed about their occupational distribution, and thus their incomes, is reported elsewhere (Lindert and Williamson 2011: Section II).

For inequality purposes, and following Kuznets, we must also decide in whose households these non-HH head members of the labor force lived. The data are almost non-existent on this issue. We make the following assumptions about the non-head earners “imported” into the households of others: For each region and urban/rural location (e.g. New England big cities or rural South), the non-heads and their individual earnings are absorbed into the same region and place. In other words, earners do not engage in long-distance commuting between regions or between countryside and city. For the free population, we assume that the average earnings of each non-HH head imported into free families is the same for all free persons of that occupation in that place. Slave non-heads are taken into slave households only, leaving household income the same as the retained earnings of all slaves. This same assumption holds for the separately recorded group of Maryland servants, though the assumption is redundant here because these are one-person households. These assumptions certainly can be challenged. We emphasize one point about data sources: For each place defined by region and by urban/rural, the aggregate imports of non-HH heads are driven by the census, the labor participation rates, and by the household headship rates. But the allocation of non-HH heads to



households by place is not yet derived from micro-studies about how households shared earnings, because few such studies exist.

Labor earnings by occupation, circa 1774. We are able to assign annual incomes to the most ubiquitous occupations in each location, thanks to the enormous archival gleanings of Jackson Turner Main's *The Social Structure of Revolutionary America* (1965), Stanley Lebergott's *Manpower in Economic Growth* (1964), the work of Carroll Wright (1885), the BLS (1929), T. M. Adams (1944), Winnifred Rothenberg (1985), several articles of Donald Adams (1968, 1970, 1982, 1986, 1992), and the contributions of a few others. Their time-consuming collection of newspaper quotes and account book entries must be used with care. Some are in the depreciated local colonial currency, whereas others are in (British) pounds sterling. Fortunately, most sources, and Main in particular, are careful to say which was which. Finally, we assume that slave and free labor marginal product was the same for all occupations where they both worked, but, of course, the slave only retained or consumed a fraction of that marginal product.<sup>4</sup>

Some of the earnings are annual, as for white collar professionals and farmers, but many are monthly, weekly, or daily rates of pay, requiring assumptions about how many days or months they spent in gainful employment each year. We believe that for those days or months in which a colonist did not hold his or her main stated job, he or she nonetheless filled in with other productive work, like weaving and farming at home, and some of this output was traded on the market. Thus, our "full-time" estimates assume that daily or monthly full-time equivalent (FTE) workers performed productive work of some kind for 313 days a year (excluding only Sundays). This assumption implies, of course, that we include more non-market work in our income estimates than do other scholars that include only or mainly market work in their output estimates. However, to imitate the labor force behavior that other scholars may have assumed in estimating early American GDP, and as sensitivity analysis, we also calculate "part-time" estimates that use fewer labor days per year.<sup>5</sup>

We enlarged the concept of labor earnings to include farm operators' profits, estimated by Main, plus slaves' and indentured servants' retained share of what they earned (discussed above). As we noted previously, this labor income amalgam we have called "own-labor incomes".

Property income in 1774. Our property income estimates benefit from Alice Hanson Jones's exhaustive and masterly study of America's wealth structure in 1774, based on her 919 probate inventories and supporting documents.<sup>6</sup> A key advantage of her data is that they identify the occupation or social status of most of the people in her colonial sample. We have examined her data and procedures in great detail, and find no flaws.<sup>7</sup> Jones realized that a probate-based sample ran the risk of overstating average wealth, and understating wealth inequality, because probate was more likely for the deceased rich than poor. She went to enormous lengths to adjust for this, ending with what she called  $w*B$  estimates that were meant to capture more of the poor. We have moved in the same direction, using a different procedure. Our greater weighting of the poorer households was achieved by introducing the new data on occupational structure described. As it turns out, our estimates imply an even greater probate-wealth markdown than did her  $w*B$  estimates.

Wealth is not property income or total income. Jones confined her income-measurement efforts to brief conjectures about wealth-income ratios, using aggregate capital-output ratios found in the macroeconomics literature of the 1970s.<sup>8</sup> We have followed a different route, in order to exploit the wage data just described. Our reading of the limited evidence on colonial rates of return suggests that, on average, assets probably earned a net rate of return of about 6 percent per annum.<sup>9</sup> Later we will quantify the sensitivity of our aggregate income estimates to this 6 percent rate of return.

The gross rate of return, which is more appropriate to the calculation of gross national product for comparison with other studies, equals this net 6 percent plus rates of depreciation that differed by asset. Following NIPA accounting standards, we have assumed zero depreciation on financial assets and real estate (positive depreciation offset by rapid capital gains), 5 percent for servants and slaves, 10 percent for livestock and business equipment, and zero for net changes in producers' perishables and crops.<sup>10</sup>

Combining own-labor income and property incomes. Here we reap big returns to having invested so much effort in gathering occupation data. Since own-labor incomes and property incomes are both arranged by occupation, we can combine the two to get their total incomes, by region, by urban/rural, and by socio-economic group.

Households were practically the whole economy. Our calculations offer what NIPA accountants call total private income of the household sector. The colonial government sector's

contribution consisted only of the wages and salaries of government employees and military personnel (already included in our occupations and own-labor earnings). There were no government corporations in 1774. Nor do we need to worry about the retained earnings of private corporations, since there were so few of them by the end of the century. The same assumptions will be made for 1800. The non-household sector will take a larger share of national income for 1860, however.

### **B. The 1798-1800 Early Federalist Evidence**

Our next benchmark for appraising national income is the census year 1800. On the labor-income side, our procedures for 1800 are roughly the same as those we applied to 1774, though the data sources are more copious and of higher quality. What is distinctive about the estimates for 1798-1800 relates not to labor incomes but to property incomes. To fund a possible conflict with France, Congress passed the first direct tax, a one-off tax levied on real estate wealth and on the numbers of slaves.<sup>11</sup>

The 1798 direct tax returns remain the most useful source available for the property income side of 1800 national income estimation. True, one might view these returns with some suspicion: Can we trust the quality of the data extracted by tax collectors from a new nation that had just shed its royal government partly over tax issues? Such suspicion is indeed warranted, especially given some evidence that properties had already been under-assessed in tax returns of the previous two decades.<sup>12</sup> The 1798 direct tax under-assessed market values by something like 15.5 percent in New England and the Middle Colonies, a figure based on a contemporary study of marketed real estate in Connecticut in that same year.<sup>13</sup> We have adjusted upwards our 1800 property income estimates for this 15.5 percent underassessment in New England and the Middle Colonies, and also for the 7 percent rise in average asset values from 1798 to 1800 suggested by the contemporary Samuel Blodget (1806).

The South might have under-assessed rich households' realty, and slaves, by even more than the North. We have three clues about the degree of underassessment there. Two of these relate to slaves and do not affect our income estimates. The third relates to real estate, and it does affect our income estimates.

The first clue arises from slave counts. The tax return of 1798 reported only 86,840 slaves of taxable age 12-50 and 323,905 slaves overall. These numbers are much too low and also imply an

implausibly low working-age share for the slave population (0.268). Indeed, only two years later the 1800 census reported 513,905 slaves aged 10-up and 835,490 slaves overall. Even the 1790 census reported far more slaves than were revealed in the 1798 tax returns. Fortunately, we were able to reject the 1798 tax-return slave counts in favor of the 1800 census, combined with the Fogel-Engerman (1976, updated 2006) sample values for the rental incomes derived by slaveholders.<sup>14</sup>

A second clue supporting underassessment in the South lies with the overall tax valuation of slaves, rather than just their numbers. According to Timothy Pitkin's (1817) summary of the 1798 returns, slave taxes were only 21 percent of all reported realty plus slave taxes in the South Atlantic, while in 1774 slave values were 58.1 percent of all slave plus realty value in that region. Either the market value of slaves relative to the value of real estate dropped spectacularly over the quarter century, or slaveholders gained a considerable tax break relative to real estate owners. It seems clear that the fifty-cent tax per slave aged 12-50 years was based on an undercount of those slaves.<sup>15</sup> Fortunately, our estimates of slaveholder incomes are based on the market value of slaves rather than on the tax assessor's slave values, and our slave counts are based on the 1800 census.

In contrast with the first two clues, the third underassessment clue does have implications for our southern property income estimates. The South Atlantic (here excluding Delaware) paid 38.1 percent of the eastern US realty tax in 1798. This share was tied by law to the South Atlantic share of the free population of the eastern states in 1800 (35 percent). Note that the region's 38.1 percent tax share is much lower than the 57.7 percent share of the thirteen colonies' real estate wealth in the probate valuations for 1774.

There are two possible explanations for the discrepancy. The first possibility is that the South could have under-assessed its 57.7 percent share of the market-based value of all realty in the 13 colonies, reporting only 38.1 percent. This implies that we should mark up greatly South Atlantic real estate values in 1798-1800, from a South/North assessment ratio of  $38.1/61.9 = 0.62$  to a market ratio of  $57.7/42.3 = 1.36$ .<sup>16</sup> We will call this assessment our upper bound assumption. The second possibility is that Southern realty was truly worth only 38.1 percent of total market value, implying that its relative real estate values must have crashed in the Revolutionary war and postwar years. This assessment will be called our lower bound assumption.

Neither assumption seems persuasive. The lower bound assumption would disregard the three clues implying that the South got away with a lower assessment rate. The upper bound assumption suggests something that would have been spotted by Northern members of Congress, and we would

have read their outcry. Its implication that the South Atlantic suffered relative war and postwar losses that enormous also strains belief. The true underassessment differential probably lies between these two extremes. We therefore settle on the middle ground: that is, we assume that the North had the 15.5 percent underassessment of real estate demonstrated in the 1798 Connecticut market study, and that the South had the same 15.5 percent underassessment plus half of the upper bound extra underassessment. Thus, the adjustment for the extra Southern underassessment raises real estate plus slave wealth values by 30.1 percent for the South Atlantic. This combined with the nationwide underassessment of real estate by 15.5 percent raises real estate plus slave wealth values by 40.4 percent for the South Atlantic, or 27.7 percent for the whole Eastern seaboard.

Since the 1798 returns covered only real estate and slaves, we had to use the same ratios of total property/(realty plus slave values) obtained from the 1774 evidence to inflate them to total property. We apply region-specific ratios to each of the three colonial regions.

There is one other important difference between the data sources on the property side between 1774 and 1798-1800. The 1798 tax returns are very handy in that they were aggregated for us at the time. But a serious drawback of the 1798 returns is that it reports no data on occupation. This means that we cannot document the occupational distribution of *total* income for 1800, although we can document the distribution of own-labor incomes and property incomes separately, as well as the aggregate value of total income.

## **B. The 1860 Income Estimates: IPUMS Meets Wage History**

The procedures for building social tables for each region in 1860 are essentially the same as for 1774. Once we again we have a fairly reliable source of data on wealth. Parallel to Alice Hanson Jones's wealth estimates, we now have the IPUMS random ("flat") sample of households in the 1860 census. Again, we convert the wealth data into property incomes using a rate of return. To the real estate wealth we applied a net rate of return of five percent, based on interest rates of that era. For gross rates of return on other assets, we had to reckon the approximate share of "personal estate" that tended to be slaves versus other productive assets and consumer assets. Drawing on the wealth portfolio assumptions of Raymond Goldsmith for the US in 1850 and 1880, we came up with a gross rate of return of 7.8 percent for the South and 7.3 percent for the non-South, the difference explained by slaveholding, for which we used a depreciation rate of 11 percent.

Again, as with 1774, we were able to exploit the occupational detail to link property incomes with own-labor incomes. The occupational weights are given directly in the IPUMS sample. For 1860 we have an even greater abundance of job-specific labor incomes, thanks to a variety of sources – several authors’ gleanings from the Weeks and Aldrich Reports, Stanley Lebergott’s wage series, Robert Margo’s rich data on civilian wage rates at Army Posts around the country, and the American Almanac’s estimates of salaries for a wide variety of professional occupations. Slave retained earnings were estimated from the Fogel-Engerman slave hire rates for 1858-1860, from free wage rates, from the literature on the exploitation ratio for slaves, and from the IPUMS sample of slaves by age, sex, and place. Finally, the pure residual profits of farm households were estimated from Lee Craig’s sophisticated estimates for the North in 1860. For the South, we adjusted his profit estimates in proportion to the wage rates of free farm laborers in different regions. For 1860 we continued to apply our assumptions about the “part-time” work year for certain occupations.<sup>17</sup>

### **III. Income Levels and Growth: 1774-1860**

The estimates speak to growth and inequality performance at the regional and national levels. This section summarizes our findings about aggregate income levels and aggregate growth, while Section IV reports inequality measures for our end-years 1774 and 1860.

#### **A. The Income Level Estimates for 1774**

The aggregate estimates of labor and property incomes shed new light on average income in 1774 and 1800, and the growth of income per capita over a quarter century of war, postwar, and national emergence. The levels and composition of total personal income are shown in Tables 1-4, for the three regions used by Alice Hanson Jones and for a geographically fixed “nation”, defined as the 13 colonies in 1774 and the easternmost 15 states plus the District of Columbia in 1800, and adding states further west for 1860.

Here we stress two key results. First, the colonial South had an average income far above those of New England or the Middle Colonies in 1774, even when one rightly counts slaves as persons. That the South was so much richer in 1774 may not surprise those who have studied the history of colonial wealth, but even average free labor earnings were higher in the South. (Wages were not much higher occupation by occupation, but there were fewer unskilled free laborers in the South,

both farm and non-farm.) Second, these new estimates imply that real income per capita dropped considerably over that quarter century. The 1774-1800 decline of about 20 percent looks almost as serious in per capita terms as the 1929-1933 drop into the Great Depression. If other authors are correct in reporting brisk income gains across the 1790s, then the Revolutionary disaster and Confederation turmoil could have been America's greatest income slump ever, in percentage terms. Let us first scrutinize the levels of income at each date, before searching for explanations of the implied net decline.

As Table 2 shows, our thirteen-colony estimate of 173.2 million dollars is 26 percent greater than the average of the Jones and McCusker estimates (137.7 million), assuming a full-time work year for all occupations, or 19 percent above Jones and McCusker if we assume that certain occupations worked only part of the year. Yet our colonial income estimates differ greatly from those of Alice Hanson Jones for only one region. There is little difference for New England, and for the Middle Colonies we report incomes "only" about 9 percent higher than hers. The main source of the big difference with Jones arises in the South, for which our income estimate (\$98.9 million) is 67 percent above that of Jones (\$59.2 million).

For the 13 colonies as a whole, the large gap is not driven by any higher estimate of wealth per household, since we rely on Alice Hanson Jones' own work. Supplementing her data with our new occupation weights, we get a slightly *lower* net worth per wealth holder than she did. Furthermore, because we find many fewer households with wealth than her estimated number of "potential wealth holders", our aggregate wealth estimate is only about 70 percent of her implied total wealth.<sup>18</sup>

An alternative suspicion might be that our income estimates overestimate the net and gross rates of return on productive wealth. It seems very unlikely that our 6 percent figure for 1774 overstates the net rate of return, the opportunity cost of not having lent at interest. The colonies and the early republic had a legal usury limit of 6 percent that was vigorously supported by law and custom.<sup>19</sup> That is, the usury constraint seems to have checked a strong demand for capital, so that the 6 percent ceiling might very well have been below market. Could the (illegal, market) rate of interest foregone by holders of directly productive assets have been higher, say 8 percent? We agree that this is a distinct possibility, especially for 1800, for which the literature suggests even greater capital scarcity than for 1774.<sup>20</sup> Table 3 shows the impact of assuming 8 rather than 6 percent. Shifting to the higher rate of return would raise our total income estimates relative to those conjectured by other scholars.

So much for property income. What about our own-labor income estimates for 1774, supported as they are by new occupation weights, full-time employment assumptions, and occupation-specific wage rates? Could these have exaggerated labor income for the 13 colonies as a whole, thus raising our aggregate income estimates above that of previous scholars?

What did Jones assume about rates of pay for labor, including the earnings retained by slaves? In fact, she did not make any assumption at all, but took a single leap of faith that we have already noted: By picking up some capital/output ratios quoted in the aggregate growth literature from the 1970s, she jumped from her impressive and reliable wealth estimates to less reliable total income guesswork which stands or falls on her assumed aggregate wealth/income ratio (not necessarily the same as a capital/output ratio). The macro literature offered Jones capital/output ratios ranging from 2.5 to 10 for the nineteenth and twentieth centuries. Within this wide range, she said, “I hazard that ratios of three or three and a half to one may be reasonable”. Yet we find that the 1774 ratio of net worth (wealth) to national income was only 1.89.<sup>21</sup>

The strikingly wide gap between Southern and Northern incomes in 1774 has a simpler explanation. In 1774, unlike 1860 and later, the South had a very different mix of free men's occupations, with a much higher propertied share and fewer poor. On the eve of the Revolution, the South was still a frontier with high productivity in their exportable tobacco, rice, indigo, and cotton. We find this contrast between the regional occupation mixes among free household heads in 1774:

	among free household heads (%)		
	<u>New England</u>	<u>Middle colonies</u>	<u>Southern colonies</u>
Farm operators	43.9	25.8	72.7
Professions, commerce, crafts	11.0	32.5	14.3
No occupation given, some wealth	16.7	28.7	11.0
Menial laborers + those with zero wealth	28.4	13.0	1.9

Southern farmers not only had higher incomes than other farmers on the average, but they constituted a larger share of households, while low-paying occupations took a lower share among free Southerners. Therefore, the advantage of the colonial South should not seem surprising, even without any gap in wage rates for given occupations.

### **B. The Income Level Estimates for 1800**

Unlike those for 1774, our 1800 total income estimates are not above those offered by other scholars. Rather, our estimates are in the middle of several competing estimates for the nation as a



whole (Table 2). Our 1800 estimates for the Lower South match those of Mancall, Rosenbloom, and Weiss (2003), even though we used the income approach and they used the production approach. It might seem comforting that our 1800 estimates are so close to others. However, ours would have been a bit higher than most if we had been able to make all the adjustments that we feel are warranted. We are especially concerned about two such adjustments.

The first potential adjustment is one already mentioned in connection with Table 5: using the interest rate on public debt as a measure of the opportunity cost of assets, it appears that the net rate of return on property was higher in 1800 than in 1774, presumably in response to Revolutionary War and Confederation inflation, financial disruption, and perhaps even productivity advance.<sup>22</sup> As we have noted, if the interest rate tended to be 8 percent in 1800 versus 6 percent in 1774, then the 1774-1800 decline in real per capita income would be a bit less, using the “alternative” estimates for 1800 shown in Table 3.

The second adjustment relates to an omission from the baseline 1800 estimates. We have no 1800 data documenting farm operators’ pure residual profits, as distinct from their asset returns or the implicit value of their own physical labor. For 1774, we were able to use a few testimonies unearthed by Main (1965) to guesstimate that the farm profit residual was 18.9 percent of all farm operators’ income in New England, 21.1 percent in the Middle Atlantic, 34 percent in the South, and 28.8 percent for the 13 colonies as a whole. We cannot apply these ratios to 1800, however, since we lack any delineation between farm operators and free farm laborers in the census or in the Weiss labor force estimates on which we rely.<sup>23</sup> Until evidence on this issue emerges, we can only propose our alternative estimates in Table 3, and note that accordingly the nation still experienced a big net income decline of 20 percent over the quarter century, though the decline may turn out to be a little less than our estimates show if we add farmers’ pure profits to the present 1800 estimates.

### **C. The Income Level Estimates for 1860**

Our income estimate for 1860 is 5,338 million in current dollars, using the part-time assumption about employees’ work years.<sup>24</sup> This is 26.4 percent above Thomas Weiss’s (1993b) “broad” GDP definition.

What might account for the large gap between Weiss (and Gallman) and ourselves? One possibility points to price deflators. Recall that we estimate current-price, or nominal, incomes. Other scholars, like Thomas Weiss (1992, 1993b), build up their estimates from the

production side, using their production indices to project their constant 1840 price estimates from the Gallman 1840 base year. To compare the LW and the Weiss 1860 estimates, either the LW estimates need to be deflated or the Weiss estimates need to be inflated. In either case, we need good price deflators, something the authors have not yet produced on their own.

Getting the price deflator right may be crucial to reconciling with others our direct estimates of nominal income or product and growth of real GDP they imply. We continue to investigate this, challenged by our inability to learn the details of others' layering of index-number calculations of real outputs and price deflators for GDP as a whole. It is entirely possible that deriving a correct detailed set of prices would make other scholars' estimates of real incomes match our estimates of nominal incomes.

#### **D. Long-Run Growth Implications 1774-1860**

Our estimates imply that between 1774 and 1800 America suffered a serious net decline in income. We need to conduct some reality checks on these results, both in terms of their longer-run growth implications and in terms of their implications about the turbulence within that quarter century itself.

We are more confident in using our income estimates to assess America's growth performance up to 1840. Table 4 supplies our real per capita income growth estimates for each of the three regions, and for the three combined (the "nation" consisting of the thirteen original colonies), all the way from 1774 to 1860. For the entire period 1774-1860 real per capita incomes in the three-region "nation" grew 0.80 percent per annum. Over these 86 years, the South Atlantic fell behind: The per annum growth rates for New England, 1.26 percent, and the Middle Atlantic, 1.08 percent, were well above the South's 0.31 percent.

Our implied growth rates may be compatible with those of other scholars for the 1800-1860 period on which they concentrated their attention. At face value, the upper part of Table 5 seems to show that we find higher growth, at 1.53 percent a year, over this period. Yet our growth rates for 1800-1860 will be reduced as soon as we are able to make two adjustments. The first adjustment, already considered in Table 3, would be to raise our assumed rate of return for the highly uncertain time around 1800, thus raising property income and total income for that starting point. The second, discussed but not yet quantified, would be to make a rough guess at the missing pure farm profits form

that 1800 starting year, again reducing our implied growth rate for 1800-1860. Once these adjustments are made, we suspect that the main question about our income-side estimates of nominal national product will center more on their higher *levels* everywhere during the 1774-1860 era, and less on any remaining difference in implied *growth rates*.

In international perspective, the income per capita in our eastern-seaboard “original thirteen” region grew after 1800 at a rate that compared well with European growth rates for 1820-1860. So say the comparisons in the lower part of Table 5 (though including farmers’ residual profits in 1800 would have lowered our 1800-1860 growth rates a bit). Yet over the longer 1774-1860 period that included the Revolutionary War our overall growth rate was slower than in Western Europe. With the help of Table 4’s greater detail, we can identify two events that suppressed American growth in the late eighteenth century: The macro shock of the War of Independence itself, and the start of a Southern “reversal of fortune”.

#### **E. Revolutionary Shocks: War Damage, Diverted Trade and the Crisis at the Top**

What stands out in the longer run perspective is the economic turbulence between our two benchmark years 1774 and 1800, first with the war years themselves and then with the troubled Confederation in the 1780s. The last quarter of the eighteenth century found the economy on a rickety swinging bridge, a metaphor that also describes scholarly attempts to span that gap with numbers from what has been called a statistical dark age. Like late eighteenth century France, early nineteenth century Latin America, early twentieth century Russia, and Africa after World War II, scholars of the early United States have had great difficulty bridging the data gap across their revolutionary upheaval and early nation-building. On the one hand, Thomas Berry (1968, 1988), Louis Johnston and Samuel Williamson (2010), Richard Sylla (2011) and others have emphasized the strong growth experienced across the 1790s, perhaps due to the wisdom of Alexander Hamilton and other founding fathers and/or due to the recovery of foreign markets. Yet, the more we come to accept their sanguine view of the 1790s, the more we must infer a true economic disaster between 1774 and 1790.

Any study attempting to measure incomes for 1774 and 1800 alone cannot quantify the depth of any economic depression in between. Yet, we can help guide the search for the magnitude of the Revolutionary war and post-war depression by posing a question: How deep would the per-capita income loss have been from 1774 to 1790 if the scholars cited above are right about the growth from 1790 to 1800, and our estimates of the net decline from 1774 to 1800 are also right? This question has

eight numerical conjectures, based on our two estimates for 1800 (“baseline” and “alternative”) times the four leading series documenting real income per capita growth from 1790 to 1800. The four series are those by Richard Sutch, Louis Johnston and Samuel Williamson, Thomas Berry, and John McCusker.<sup>25</sup> All eight conjectures imply significant drops in income per capita between 1774 and 1790. Between these two years GDP per capita might have dropped 18 percent, based on Sutch and our alternative estimate for 1800. The largest estimated drop is 30 percent, based on Berry’s series and our baseline estimate. The estimates seem to agree with John McCusker and Russell Menard that the “Colonists paid a high cost for their freedom”, with Allan Kulikoff that the drop in incomes was “equal to the early years of the Great Depression”, and with their consensus that recovery was painfully slow.<sup>26</sup>

What could have caused such sustained income losses? There is good *prima facie* evidence that three related negative shocks could have been large enough to cause the deep depression between 1774 and 1790. The first was the economic destruction of the war itself, as well as the impact of nearly two decades of hyperinflation and a dysfunctional financial system. The second negative shock consisted of the disruptions of overseas trade during the Revolution and, after 1793, the Napoleonic Wars.<sup>27</sup> The shock was concentrated in the South. Available price and trade data show that the colonies, especially in the Lower South, suffered heavy volume and value losses in trade and shipping as the war deepened, and that they recovered only slowly and partially across the 1780s. In real per capita terms, New England’s commodity exports rose by a trivial 1.2 percent between 1768/72 and 1791/92, rose by a modest 9.9 percent in the Middle Atlantic, but fell by a spectacular 39.1 percent in the Upper South, and by an even bigger 49.7 percent in the Lower South (Mancall *et al.* 2003 estimate an even larger 67 percent), yielding a decline of 24.4 percent for the thirteen colonies as a whole.<sup>28</sup> The most painful of these shocks was the loss of well over half of all trade with England between 1771 and 1791. In addition, America lost Imperial bounties like those on the South’s indigo and naval stores, as well as New England’s reversal from colonial bounties to prohibitive duties on its whale oil exports.

While these negative demand shocks to American commodity exports were very large, especially for the Lower South, the initial share of exports in regional income was only about 6-7 percent in the early 1770s, according to the Shepherd-Walton export values per capita in 1768-1772 and our 1774 income estimates for the three main regions. Thus, it is hard to imagine that the huge depression of 1774-1790 was entirely “export-led”: A 24-percent trade fall times a 6-7 percent share of income equals no more than a 2 percent fall in income colony-wide. The numbers are bigger for the

South, where exports fell by perhaps 45 percent and the trade share was 7.1 percent, implying an income loss of more than 3 percent. These calculations only deal with foreign trade losses; the trade losses would be considerably higher if they included the decline in inter-colonial and subsequent inter-state trade between 1774 and 1790. Finally, these negative trade shocks created a move back to subsistence farming, and presumably lower agricultural productivity.

The third major negative shock involved what we call a *crisis at the top*, and it was felt primarily in the coastal cities and smaller river towns. This shock was related to the trade losses, but transcended them and could have caused much greater income losses. America's urban centers were severely damaged by British naval attacks, blockades, occupation, and by the eventual departure of skilled and well-connected loyalists, especially from New York, Charleston, and Savanna. In Richard Hildreth's summary, "one large portion of the wealthy men of colonial times had been expatriated, and another part impoverished".<sup>29</sup>

The damage to urban economic activity was considerable, and potentially enough to bring great declines to per capita incomes, even though population kept growing. To identify the extent of the urban damage, one could start by noting that the combined share of Boston, New York City, Philadelphia, and Charleston in a growing national population shrank hugely from 5.1 percent in 1774 to 2.7 percent in 1790, recovering only partially to 3.4 percent in 1800. To the extent that urbanization is a close correlate of levels of economic development, this big fall in the American city population share certainly confirms what our income estimates document. There is even stronger evidence confirming an urban crisis: the share of the mainly-urban white collar employment fell from 12.7 percent in 1774 to 8 percent in 1800; the ratio of earnings per free worker in urban jobs relative to that of total free workers dropped from 3.4 to 1.5; and the ratio of white collar earnings per worker to that of total free workers fell even more, from 5.2 to 1.7. This evidence offers strong support for an urban crisis, and it also supports the view that America had not yet recovered from the Revolutionary economic disaster even by 1800.

#### **F. Southern Reversal of Fortune**

The absolute decline of South Atlantic per capita income over the last quarter of the 18<sup>th</sup> century and its relative decline over the next four decades stand out as a classic example of what has come to be called *reversal of fortune* (Acemoglu *et al.* 2002). Table 4 underlines the change. According to Richard Easterlin (1960, 1961), the South Atlantic was well behind the Northeast and the

national average by 1840, while it was well ahead of all other regions in 1774. The “Southern reversal of fortunes” draws support from the apparent absence of any large army of poor whites in the colonial South. We note again that what few local colonial censuses and tax records we do have reveal that nearly all white households around 1774 were assessed as having positive wealth. The familiar image of the South as a repository for much of the nation’s poor whites was apparently a later development.

Why the reversal of fortune for the South? A benign part of the story seems to have been that the colonial South Atlantic was still a labor-scarce frontier with high returns to export crops. Its decline after 1774 was echoed in two other cases of relative frontier decline: the relative decline of the West South Central region 1840-1860 (again see Table 4), and the loss of the Pacific region’s super-incomes after 1870. Still, the Southern reversal had multiple causes, and we do not yet know what weights to attach to the decline of frontier super-returns, the exceptionally severe damage incurred in the Revolutionary War, or some deeper institutional failure.

#### **IV. Income Inequality, 1774 - 1860**

While social tables are not an inherently superior way to estimate aggregate national product, they have the clear advantage of being able to reveal the inequality of income in historical settings where occupational labels and class rewards conveyed a great deal of income information. Thanks to the social tables, we can now compare American income inequalities on the eve of the Revolution and on the eve of the Civil War, whereas earlier scholarship could only draw on sketches of wealth inequality and some scattered wage ratios.<sup>30</sup>

Incomes were much more equally distributed in colonial America than in America today, or in other countries in the late eighteenth century. Among all American households, slaves included, Table 6 reports that the richest 1 percent had 7.1 percent of total income, and the Gini coefficient was 0.437. Without the slaves, the top 1 percent of free households had 6.1 percent of total incomes, and the Gini was 0.400. Compare colonial American inequality with that of the United States today, where almost 20 percent of total income accrues to the top 1 percent, and where the Gini coefficient is almost 0.50 (Atkinson, Piketty, and Saez 2011: Table 5, p. 31). The image of an egalitarian colonial America emerges even more clearly at the regional level, in colonial New England (Gini 0.354), in the Middle Atlantic (Gini 0.381), and, surprisingly, among free Southerners (Gini 0.328). That is, within any

American colonial region, free citizens had more equal incomes than the thirteen colonies as a whole, the reason being that wide income gap between the Southern region and the rest.

Free American colonists also had much more equal incomes than did west Europeans at that time. The average Gini for the four northwest European observations reported in Table 6 is 0.57, versus that American colonial Gini of 0.437. Indeed, there was no documented place on the planet that had a more egalitarian distribution in the late 18<sup>th</sup> century (Milanovic *et al.* 2011). For those who feel that a large and strong middle class is essential for establishing institutions friendly to modern economic growth, note that the middle of the distribution (Next 40%) received 41.6% of American incomes in 1774, but only 27.8% in 1802 England and Wales, or 24.4% averaged over the four European observations in Table 6. And note that the loudest revolutionary noise came from New England, where the middle got 52.5 percent of total New England incomes.

If people had far more equal incomes in America than elsewhere, which kinds of colonists were better off than their counterparts in Europe? Figure 1 offers a striking answer, using an Anglo-American comparison. On the horizontal axis each society is ranked from its poorest to its richest, and on the vertical axis their average group incomes are displayed on a log scale. It appears that an American colonist of any percentile rank had a higher income than his or her English counterpart of the same rank until we reach the top one percent. Indeed, it turns out that even American slaves were above the bottom of the Anglo-American income ladder, although such comparisons fail to account for the loss of freedom, longer hours worked, and harsher working conditions. Yet, the top incomes so dominated in England-Wales that its national product per capita was still close to that of America. Of course, average English incomes are likely to have matched or surpassed the American average after our economic disaster of 1775-1790.

What about the impact of relative purchasing power on such income comparisons? As is widely recognized, simple exchange rate conversion does not adequately account for cost of living differences between classes and places. This familiar point has a number of important applications in the colonial American context, and they deserve emphasis and further investigation. One is that the cost of a standard consumption bundle probably was lower in New England than it was either in the Southern colonies or in England and Wales. So say some recent calculations for this era. If true, then these nominal income contrasts might be somewhat misleading. Perhaps New England -- with cheap fish, corn, beans, rum and molasses -- was not so much poorer than the Southern colonies as the nominal figures in Table 5 imply. This might also have been true of the Middle Colonies with its

cheap grains, exported to England and the West Indies where they were expensive (Mancall *et al.* 2008b). In any case, such adjustments should also deal with the relative cost and quality of housing (Shammas 2007). Perhaps New Englanders weren't so much worse off relative to Southerners as our figures suggest, and perhaps workers in the Middle Atlantic were even better off compared with English workers than our figures suggest. In contrast, an upper-class cost of living bundle, including the cost of music, theater, and servants, must have been lower in London than in the Northern colonies. These "real inequality" dimensions need to be explored further, but we do not expect them to overturn the inequality contrasts shown here.<sup>31</sup>

The 1860 distribution of income for the US, for the original 13 colonies, and for each census region is reported in Table 7, which can be compared with the 1774 results in Table 6. The main finding is clear enough: American inequality rose everywhere over this 86-year span of history -- in every region, among free households alone, and among slave and free combined. Furthermore, the rise was steep. For the eastern seaboard, the "original thirteen", the Gini rose from 0.40 (Table 6) to 0.53 (Table 7), and the share of income going to the top one percent of households rose from 7.1 to 10.0. While income gaps widened in all parts of the eastern seaboard, the widening was most pronounced in the South. Among slave and free household combined, the South Atlantic Gini rose from 0.46 in 1774 .60 in 1860 -- a figure much the same for the ESC (.58) and WSC (.60), and a level of inequality hard to find anywhere in Europe or the rest of the world (Milanovic *et al.* 2011). Especially notable about the South Atlantic was the enormous increase in income inequality among free households, where the Gini rose from .33 to .51. The top-one-percent share of income increased from 6.3 to 10.1 percent among free households in the South Atlantic, while the share going to the poorest 40 percent fell from 21.9 to 11.3 percent. Any historian looking for the rise of a poor white underclass in the Old South will find it in this evidence.

## V. Summary and Agenda

The only way to push back the quantitative frontiers of inequality, living standards, and growth history is to adapt to the archival environments of the deep past, just as archeologists have done. Even reaching back to the late 18<sup>th</sup> century requires the use of an eclectic array of incomplete evidence. One of the most underexploited bodies of evidence for the early modern era consists of social and occupational class counts allowing us to construct aggregate incomes and their distribution. Working



on that frontier, we have emerged with a rich harvest of provisional measures sketching early American growth and inequality.

It appears that the colonists had far higher incomes in 1774 than previously thought, on average probably greater than England, the richest country in Europe. Between 1774 and 1800 American incomes declined in real per capita terms, so that any rapid growth after 1790 failed to make up for a very steep wartime decline and the “lost decade” of early independence. The quarter-century decline was sufficiently big that America lost its lead to England in the income per capita rankings. In addition, we find that free American colonists had much more equal incomes than did households in England (and elsewhere in Europe). The colonists also had greater purchasing power than did their English counterparts over all of the income ranks except at the top one percent.

There is abundant evidence confirming reversal of fortunes for the original southern colonies, a region that became the South Atlantic in census terms. Our results suggest that Southern per capita incomes were far above that of New England and the Middle Colonies in 1774, and that poor whites were much less common there than in other colonies. It appears that the colonial South lacked the large numbers of poor free labor that could be counted in Boston, Philadelphia, New York and lesser coastal and river towns in the north. In short, our results suggest that mass poverty did not spread among the Southern white population until the early 19<sup>th</sup> century. In any case, the Old South had lost almost all of its lead in average income by 1800. Why did the Old South (to become the South Atlantic) undergo such a spectacular reversal of fortune between 1774 and 1800? Not only do we need to answer that question, but we also need to know more about the distribution of incomes in late colonial and early national South, research of the sort already accumulated for Chesapeake.<sup>32</sup>

We need more work to reinforce the credibility of the estimates offered here. While we have certainly found strong support for a steep rise in inequality over America’s first century, we do not yet know when it happened. Future research needs to supply an inequality estimate for 1800. Only then will we be able to separate out the role of early industrialization (1800-1860) from the turbulent first quarter century (1774-1800). Adding an estimate of farm profits for 1800 – currently missing -- would certainly lower our estimated 1.53 percent per annum growth rate in per capita income 1800-1860. And new GDP deflators must be fashioned to replace the traditional ones. Finally, we are now working on a 1870 social table, and hope that we can soon sketch the income effects of the Civil War decade.

**Table 1. Estimated American Personal Incomes, 1774 and 1800**

	<b>New England</b>	<b>Middle Atlantic</b>	<b>South Atlantic</b>	<b>All 13 Colonies (15 states + DC)</b>
<i>Gross income, millions of current dollars (\$4.44/£ sterling)</i>				
<b><i>Circa 1774</i></b>				
FTE free own-labor income	31.09	28.85	62.81	122.75
Ditto, part-time (see text)	28.16	27.26	58.27	113.70
Slave retained earnings	0.13	1.06	12.18	13.37
Gross property income	4.84	8.37	23.83	37.04
Gross total income	36.06	38.28	98.81	173.16
Ditto, with part-time	33.13	36.69	94.28	164.11
<b><i>Circa 1800</i></b>				
FTE free own-labor income	73.65	84.20	87.77	245.62
Ditto, with part-time	66.57	76.91	80.88	224.36
Slave retained earnings	0.07	2.10	37.34	39.51
Gross property income	21.39	47.83	89.77	158.99
Gross total income	95.11	134.13	214.88	444.12
Ditto, with part-time	88.03	126.83	208.00	422.86
<i>Relevant denominators</i>				
Free labor force 1774	185,999	156,875	195,938	538,812
Total labor force 1774	188,230	175,655	436,136	800,021
Free population 1774	657,567	582,134	719,875	1,959,577
Total population 1774	661,563	613,685	1,101,151	2,376,399
Free labor force 1800	334,685	380,162	402,504	1,117,351
Total labor force 1800	335,500	404,900	835,590	1,575,990
Free population 1800	1,231,671	1,423,924	1,428,695	4,084,290
Total population 1800	1,233,011	1,464,548	2,222,221	4,919,780

**Notes to Table 1:**

The estimates exclude Native Americans.

The 1800 estimates currently lack any estimate of farm operators' residual incomes beyond the implicit value of their farm labor and their property incomes (see text).

The gross property incomes for 1800 are based on middling assumptions about Southern underassessment in 1798 (see text).

The baseline estimates use the full-time assumptions of 313 days per labor year, in occupations where the primary earnings data are sub-annual (e.g. daily or monthly wage rates). The part-time assumptions retain the explicitly annual income estimates for titled and

professionals, for commercial proprietors, for manufacturing trades, servants, slaves, and household heads of unstated occupations having positive wealth. Part-time work years for explicit pay are assumed to have been only 280 days for construction workers, farm operators, and the rural unskilled. For urban unskilled and for household heads with zero wealth and unstated occupations, we assumed a work year of only 222 days.

Delaware is here included with the Middle Colonies for both years, following Alice Hanson Jones's sample design.

**Table 2. Alternative Estimates of National Income 1774 and 1800, in current \$ and 1840 \$ (millions)**

	1774 current \$m	1774 1840 \$m	1800 current \$m	1800 1840 \$m	Source
<b>US (orig 13)</b>	142.2	152.5			GDP: McCusker (2000)
	131.7	141.6			Gross inc: Jones (1980)
	173.2	185.7	444.1	305.9	Gross income: LW (2012)
	164.1	176.3	422.8	291.3	" , part-time: LW (2012)
<b>US (all)</b>			508.7	350.3	GDP: McCusker (2000)
	150.3	161.6	515.5	355.5	GDP: Mancall & Weiss (1999)
	135-157	145-169			GDP: Gallman (1972)
	134.8	145.0	500.1	344.9	GDP: Goldin & Lewis (1980)
	132.6	142.6			Narrow GDP: Weiss (1992)
			430.9	297.2	Berry (1988)
			446.3	307.8	David (1996)
			510.4	351.5	GDP: Mancall <i>et al.</i> (2003)
			470.7	324.2	Gross income: LW (2012)
			448.1	308.7	" , part-time: LW (2012)
<b>New Eng.</b>	35.5	38.2			Income: Jones (1980)
	34.6	37.1	95.1	65.5	Gross income: LW (2012)
<b>Middle Atl.</b>	36.5	39.3			Income: Jones (1980)
	39.7	42.6	134.1	92.4	Gross income: LW (2012)
<b>South Atl.</b>	59.2	63.6			Income: Jones (1980)
	98.9	106.0	214.9	148.0	Gross income: LW (2012))
<b>Lower</b>	22.0	23.7	93.5	64.4	GDP: Mancall <i>et al.</i> (2003)
<b>South Atl.</b>			94.1	64.8	Gross income: LW (2012)

Notes to Table 2: (1) Gross inc, Net inc = personal income, gross and net of depreciation. (2) This culled set omits very old estimates, and if a modern source offers more than one estimate, this set selects the most recent. It also selects the highest in the Jones range, as recommended by Gallman and Weiss.

(3) We use the McCusker composite price index here, as in Tables 1 and 4.

(4) The LW estimates for 1800 are the "baseline" estimates. For our "alternative" estimates, see Table 1.

(5) The western states included in the LW "US (all)" estimates are KY and TN, plus MS for labor incomes only.

**Table 3. Alternative Property Incomes and Total Incomes, 1774 and 1800**

1774	\$ millions in 1774 (at \$4.44/£)			
	New England	Middle Colonies	South	All 13 Colonies
<b><u>Estimated using 6% net rate of return on all NIPA-type assets and slaves</u></b>				
Gross personal property incomes	4.840	8.372	23.830	37.042
Net personal property incomes	3.662	6.534	15.736	25.932
Total gross personal incomes	36.064	38.281	98.814	173.159
Total net personal incomes	34.886	36.444	90.719	162.049
<b><u>Estimated using 8% net rate of return on all NIPA-type assets and slaves</u></b>				
Gross personal property incomes	6.061	10.550	29.075	45.685
Net personal property incomes	4.883	8.712	20.981	34.575
Total gross personal incomes	37.285	40.459	104.058	181.802
Total net personal incomes	36.106	38.622	95.964	170.692
1800	\$ millions in 1800			
	New England	Middle Atlantic	South	All 15 states and DC
<b><u>Baseline estimate, using 6% net rate of return on all NIPA-type assets and slaves</u></b>				
Gross personal property incomes	21.391	47.829	89.772	158.993
Net personal property incomes	16.787	29.346	46.490	92.624
Total gross personal incomes	95.112	134.128	214.880	444.119
Total net personal incomes	90.508	115.645	171.598	377.750
<b><u>Alternative estimate, using 8% net rate of return on all NIPA-type assets and slaves</u></b>				
Gross personal property incomes	26.987	57.611	105.269	189.867
Net personal property incomes	22.383	39.129	61.987	123.498
Total gross personal incomes	100.707	143.910	230.376	474.994
Total net personal incomes	96.103	125.427	187.094	408.625

**Table 4. Real Product per Capita, 1774 - 1860**

	(in 1840 dollars)			
	<u>1774</u>	<u>1800</u>	<u>1840</u>	<u>1860</u>
New England	61.83	56.66	<i>129.01</i>	181.39
Middle Atlantic	73.81	68.73	<i>119.68</i>	186.65
South Atlantic	105.70	74.29	<i>85.49</i>	137.75
East North Central			<i>71.50</i>	135.78
West North Central			<i>79.27</i>	136.20
East South Central			<i>85.49</i>	132.83
West South Central			<i>161.65</i>	175.30
Mountain				209.07
Pacific				501.81
"Original thirteen"	85.26	68.22	110.93	169.18
All USA			<i>101.03</i>	160.16
<b>Implied real growth rates per annum</b>				
	1774-1800	1800-1840	1840-1860	1744-1860
New England	-0.33	2.08	1.72	1.26
Middle Atlantic	-0.27	1.40	2.25	1.08
South Atlantic	-1.35	0.35	2.41	0.31
East North Central			3.26	
West North Central			2.74	
East South Central			2.23	
West South Central			0.41	
"Original thirteen"	-0.85	1.89	2.13	0.80
All USA			2.33	0.74

**Notes to Table 4:**

(1) The italicized product estimate for the whole United States in 1840 is Thomas Weiss's (1993) "broad" measure. For the 1840 italicized regions, the Weiss total was multiplied by regional relative products per capita implied by Richard Easterlin's (1960, Table A1, variant A) estimates. All other national "products" are our own "part-time" estimates of national personal income. As noted in the text, we use the "part-time" estimates to make our concept conform to the more conventional estimates, even though we believe that for 1774 and 1800 the conventional estimates make too little imputation for non-market home production.

(2) For 1800 we here used our "baseline" estimate, not the higher alternative estimate using an 8 percent rate of return.

(3) The price deflator for 1800-1860 is that of Thomas Weiss (1993, Table 4). For 1774-1800 we spliced the David-Solar (1977) series onto the Weiss series at 1800. Using 1840 = 100, the resulting price index is 81 for 1774, 126 for 1800, and 106 for 1860.

(4) The South Atlantic includes Delaware and Florida for 1840 and 1860, but not for 1774 or 1800.

(5) The “Original thirteen” refers to the territory of the original thirteen colonies in 1774 and 1800, even though the same territory had become fifteen states plus DC by 1800.

(6) The growth rate of 0.74 percent a year for the whole USA, 1774-1860, incorporates the expansion from the thirteen colonies in 1774 to the whole USA in 1860.

**Table 5. Real Growth in Income Per Capita,  
US and other Countries 1774-1860**

(percent per annum)

*United States 1800-1860*

David (1967)	1.29
Weiss (1992), narrow	1.06
Weiss (1992), broad	0.94
Gallman (2000)	1.18
Abramovitz-David (2000)	0.93
Lindert-Williamson (2012)	1.53

*United States 1774-1800*

Lindert-Williamson (2012)	-0.85
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*United States 1774-1860*

Lindert-Williamson (2012)	0.80
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*Western Europe 1820-1860*

Austria	0.95
Belgium	1.39
France	1.29
Germany	1.06
Netherlands	0.65
Switzerland	1.18
United Kingdom	1.27
Unweighted average	1.11

Source: The Lindert-Williamson estimates are those for the eastern seaboard taken from Table 4. The West European Figures are calculated from Maddison (2010).

**Table 6. Inequality in the American Colonies 1774**

Region:	All 13 colonies	All 13 colonies	New England	Middle Colonies	South	South
Households:	All	Free only	All	All	All	Free only
Gini coefficient:	0.437	0.400	0.354	0.381	0.464	0.328
Income shares in % of total income						
Top 1% of HHs:	7.1	6.1	3.8	6.4	7.9	6.3
Top 5%:	22.2	21.6	11.4	19.3	25.6	21.3
Top 10%:	30.8	29.6	20.1	28.3	34.3	30.8
Top 20%:	47.3	43.8	35.7	43.8	49.1	42.3
Next 40%:	40.3	41.6	52.5	40.1	39.4	35.7
Bottom 40%:	12.3	14.6	11.8	16.1	11.4	21.9
Household income levels in \$ (at \$4.44/£ sterling)						
Mean:	345	406	278	289	411	620
Median:	282	377	371	274	322	585
Top 1% of HHs:	2379	2471	1059	1862	3243	3910
Top 5%:	1272	1754	631	1118	2105	2635
Top 10%:	859	1202	559	818	1410	1910
Top 20%:	776	890	496	634	1011	1312
Next 40%:	369	339	365	290	406	694
Bottom 40%:	104	230	82	117	118	199

***Western Europe, as a comparison group***

Region:	England	England			
(All households)	& Wales	& Wales		Holland	Netherlands
Year:	1759	1802		1732	1808
Gini coefficient:	0.522	0.593		0.610	0.563
Income shares in % of total income					
Top 1% of HHs:	17.5	14.6		13.7	17.0
Top 5%:	35.4	39.2		37.0	39.5
Top 10%:	45.1	48.8		50.9	51.3
Top 20%:	57.5	63.2		65.8	64.7
Next 40%:	30.0	27.8		25.6	22.8
Bottom 40%:	12.5	9.0		8.5	12.5
Household income levels					
Mean:	£	43.4	90.6*	fl.	67.8
Median:	£	25.0	55.0	fl.	35.0

(\* or £106.8 if we count government revenue, the King, and certain pensioners.)

Notes to Table 6 and Figure 1: The inequality results in Table 6 are based on the full-time (FTE) measures of incomes at 313 days per year. Inequality would have been raised only slightly by using the part-time work year assumptions described above. For example, using the part-time work years would yield a 13-colony gini coefficient of 0.440 for all households, or 0.408 for free households. Figure 1 is based on the part-time work years, for better comparability with English and Dutch inequality.

**Table 7. The Inequality of American Household Incomes 1860**

		Percent shares of total income						
<u>Region</u>	<u>Gini coeff</u>	<u>Top 1%</u>	<u>Top 5%</u>	<u>Top 10%</u>	<u>Top 20%</u>	<u>Next 40%</u>	<u>Bottom 40%</u>	<u>Mean Inc.</u>
<b><i>All households</i></b>								
New Eng	0.44	6.9	20.0	31.4	48.7	37.7	13.6	989
Midd Atl	0.48	9.3	23.5	35.7	52.8	34.8	12.5	1,083
South Atl	0.60	12.7	31.6	45.6	63.9	28.6	7.6	742
ENC	0.39	7.2	19.0	29.2	44.8	37.8	17.4	806
WNC	0.42	7.4	20.4	31.0	46.7	38.0	15.3	799
ESC	0.58	12.4	31.6	45.0	62.1	29.8	8.1	727
WSC	0.60	15.5	34.5	47.0	63.7	28.4	8.0	931
Mountain	0.51	10.6	26.3	39.0	55.9	32.0	12.1	1,018
Pacific	0.42	6.9	19.7	30.9	47.1	38.0	14.9	2,089
USA	0.51	10.0	25.5	37.7	54.7	34.7	10.6	903
Orig 13	0.53	10.0	25.6	38.4	56.2	34.3	9.4	951
<b><i>Free households</i></b>								
Midd Atl	0.48	9.3	23.5	35.7	52.8	34.8	12.5	1,083
South Atl	0.51	10.1	26.0	38.1	54.5	34.2	11.3	1,130
WNC	0.38	7.0	19.4	29.6	44.7	37.6	17.7	872
ESC	0.48	9.7	26.1	38.1	53.6	32.7	13.7	1,110
WSC	0.50	12.6	29.4	40.5	55.2	32.3	12.5	1,456
USA	0.47	9.3	23.8	35.4	51.6	34.8	13.6	1,035
Orig. 13	0.50	9.3	23.9	36.0	53.1	34.8	12.2	1,083

**Note:** Midd Atl includes DE, and South Atl includes FL.



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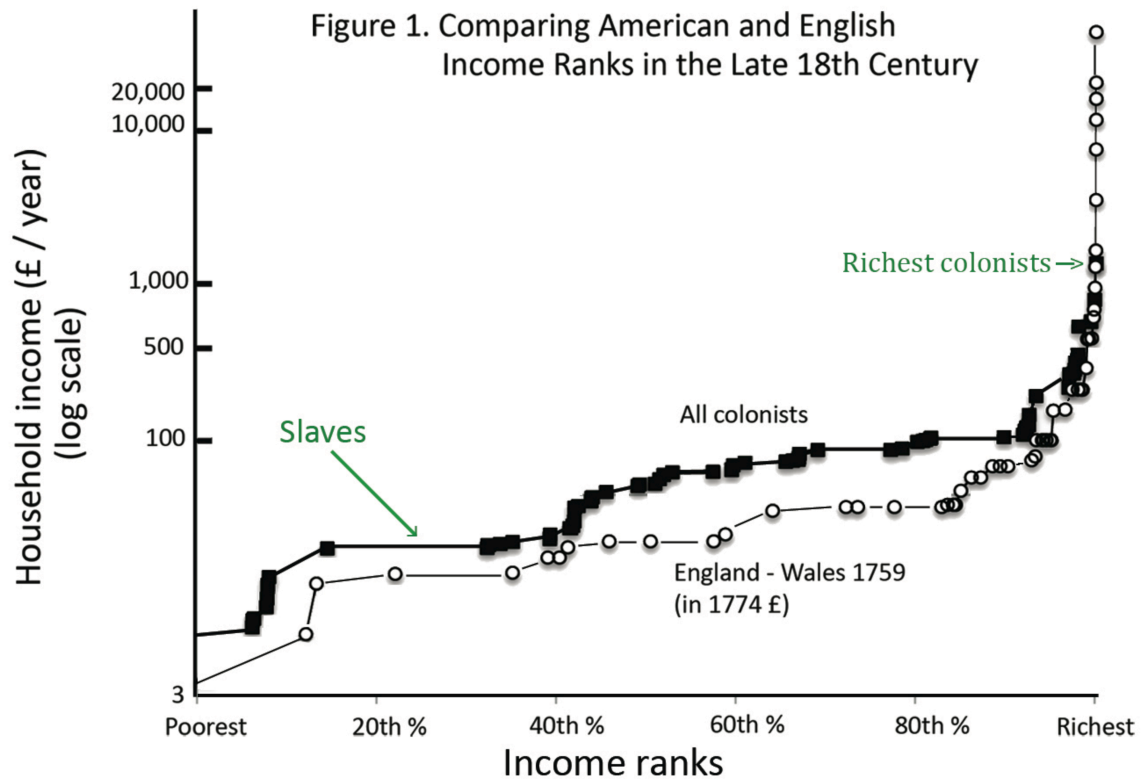
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## Endnotes

<sup>1</sup> The term “reversal of fortune” was made popular by Acemoglu, Johnson, and Robinson (2002).

<sup>2</sup> See Lindert and Williamson (1982, 1983), and Milanovic, Lindert, and Williamson (2011).

<sup>3</sup> One of these imaginative efforts was constructed by colonial Governor James Glen of South Carolina in 1751 (cited in McCusker 2006) and one by Samuel Blodget (1806: p. 99). They appear to have been readers of the English political arithmeticians, whose writings accelerated with the growing needs to finance wars. On the rise of the quantification culture in late-18<sup>th</sup> century England, see Hoppit (1996).

<sup>4</sup> The debate over slave maintenance and its share of the market wage is extensive, especially for 1850 and 1860. See “Slave earnings retention 1774 & 1800.doc” at <http://gpih.ucdavis.edu>.

<sup>5</sup> The shares of the 313-day full year that seem most plausible as a conventional “part time” measure of work for pay outside the home are:

1.00 (100% of 313 days) for those households with the head employed in the professions, commerce, and skilled manufacturing artisanal jobs, and for slave households;

0.89 (280 days) for households with the head employed in construction trades, rural unskilled workers, and (to understate total annual days a bit) farm-operator households; and

0.71 (222 days) for households headed by free urban unskilled laborers and zero-wealth household heads of unknown occupation.

This “sensitivity analysis” range is certainly wide enough to cover such estimates for England: 1760 and 1771 averaged 278 days; and 1800 280 days (Broadberry *et al.* 2012, Table 12). For 1774, these part-time assumptions yield the following ratios of part-time to full-time total incomes (labor plus property):

	New England	4 Middle Colonies	South	Thirteen Colonies
Free h’holds	0.918	0.957	0.948	0.943
All h’holds	0.919	0.958	0.954	0.948

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These ratios imply that our use of “full-time” estimates will not explain away the gap between our income estimates and those of others for 1774.

<sup>6</sup> See Jones (1977, 1980) and her ISPCR data file 7329 at the Inter-University Consortium for Political and Social Research at the University of Michigan.

<sup>7</sup> In a set of side experiments, we tried to replicate Jones’s A\*-weighted estimates using her own data and her own procedures. In no case did we achieve exact replication, and for one regional wealth total, we were off by 4 percent. We cannot find the source of this discrepancy, but suspect that she had to take some shortcuts in the pre-spreadsheet era that we have not understood. Despite the discrepancy, we feel confident of both her estimates and ours. See the “property incomes 1774” files at <http://gpih.ucdavis.edu> for the details.

<sup>8</sup> See in particular Jones (1980, pp. 61ff).

<sup>9</sup> There are only meager data on colonial interest rates, i.e. the net opportunity cost of holding real capital. For estimates near the 1774 benchmark, see Homer and Sylla (1991, pp. 276-279). Near the 1800 benchmark, federal government bonds had a market yield of 6.94 percent per annum, while New England municipals yielded 6.13 percent (Homer and Sylla 1991, p. 286). Winifred Rothenberg (1985, p. 790) notes that 6 percent was the “Lawful Interest” stipulated by colonial law, but that “beginning in 1785, interest rates began to climb to 7, 8, and 9 percent”. In personal communication, Farley Grubb notes late colonial evidence that could argue for either a 5 percent or a 6 percent rate on government borrowing.

<sup>10</sup> The use of a zero rate on changes in producer perishables and crops adheres to a referee’s objection to our initial assumption that all probated stocks of such perishables were used up in a year, thus adding a 94 percent depreciation rate to the 6 percent interest forgone. Our initial assumption added slightly to the gross national income estimates for 1774, though of course it added nothing to the net national income.

<sup>11</sup> The best introduction to the quantitative dimensions of the 1798 direct tax returns is still that of Lee Soltow (1989). For the underlying political history, see Einhorn (2009).

<sup>12</sup> Warned in advance by Gerard Warden’s (1976) investigation of the Massachusetts 1771 tax rolls, we found implausibly low assessments not only on those rolls but also in the Philadelphia 1772 returns supplied to us by Billy Gordon Smith and in the 1786 New York City returns supplied to us by Herbert Klein. We found those tax rolls useful for identifying occupational



coverage, including occupations revealed by the presence or absence of each asset type, but not for the assessed values themselves.

<sup>13</sup> Lee Soltow (1989, pp. 37, 256-257) cites correspondence he found in the Oliver Wolcott papers showing that for 518 Connecticut properties sold in 1798, the average ratio of US-assessed value to market value was 0.845.

<sup>14</sup> See the Excel file “1798-1800 property totals” at <http://gpih.ucdavis.edu>.

<sup>15</sup> The *ad valorem* tax rate as a share of the Fogel-Engerman slave values (1976, updated 2006) resembles the share of slaves that were reported. This again suggests that the undercount of slaves was the main mechanism for understatement of Southern taxable wealth. The slave undercount was common to all states in 1798, though over 60% of the 1800-census slaves were reported in Connecticut, New York, and Pennsylvania, whereas less than 40% were reported in New Jersey, Kentucky, and Tennessee.

<sup>16</sup> The algebra of adjustment is as follows. We observe the ratio of total assessed values, South to North ( $As/An$ ) = 0.381/0.619. Under the upper bound assumption, the regional ratio of true market values ( $Rs/Rn$ ) = 0.577/0.423. Within the regions, the relationships of assessed to market value are  $As = (1-U_s) Rs$ , or  $An = (1-U_n) Rn$ , where the  $U$ 's are the shares of underassessment. The 1799 market value study suggested that  $Un = 0.155$  in the North. These values imply that the market value of Southern real estate  $Rs = 2.6226$  times  $As$ , so that the underassessment rate  $Us = 0.619$ . (Just by coincidence, this matches the Northern share of assessments.)

When Tables 1 and 2 introduce estimates of nominal income based on our middle assumption, one can add \$9.547 million to get the result obtainable from the upper bound assumption for 1800, or subtract the same amount for the lower bound result.

<sup>17</sup> See Lebergott (1964), Margo (1990), the *American Almanacs* for 1856-1861, Craig (1991), and our Excel files for 1860 in the American Incomes folder at <http://gpih.ucdavis.edu>.

<sup>18</sup> In this passage, “wealth” means household net worth. See the <http://gpih.ucdavis.edu> file on total property incomes for 1774.

<sup>19</sup> See Homer and Sylla (1991) p. 271 and *passim*.

<sup>20</sup> See the interest rate literature cited in endnote xvi above.

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<sup>21</sup> Jones's conjectural range was given in her *Wealth of a Nation to Be* (1980), p. 62. Robert Gallman and Thomas Weiss have preferred her top wealth-income ratio, 3.5 to one, and that is used in Table 3's display of her estimates. Decomposing our aggregate 13-colony wealth-income ratio of 1.89 into regional wealth/income ratios for 1774, we estimated the ratio at 0.96 for New England, 1.80 for the Middle Colonies, and 2.25 for the South.

<sup>22</sup> See Homer and Sylla (1991, pp. 274-296).

<sup>23</sup> Lucy Simler's detailed study of Chester County, Pennsylvania found that farm operators' families supplied 60 percent of farm labor in 1799, with the rest being hired farm labor. For 1774, she implied 67 percent (Simler 1990, p. 197, Table 3). The only alternative to extrapolating from Chester County to the whole nation is to assume that the 1860 shares (Atack and Bateman 1987, pp. 43-44) applied to 1800. We are exploring this option so as to estimate farmers' pure profits in 1800.

<sup>24</sup> The FTE estimate, assuming everybody in the labor force worked 313 days a year, would be \$5,712 million. Yet we would not apply such an FTE assumption to the economy of 1860, even though we feel it could be valid for 1774 or 1800, because there is evidence that non-market home production had dropped across the antebellum years.

<sup>25</sup> See Series Ca11, Series Ca16, and Series Ca17 in the *Historical Statistics of the United States* 2006, and McCusker (2000).

<sup>26</sup> See McCusker and Menard (1985, p. 374) and Kulikoff (2005, p. 27).

<sup>27</sup> As Shepherd and Walton (1976) have noted, the loss of trade in the 1780s was domestic as well as overseas, because the loose Confederation that preceded federal union briefly allowed the new states to tax interstate trade. We concentrate here, however, on the larger and longer shocks to trade with Britain and its possessions.

<sup>28</sup> Shepherd and Walton (1976: especially Table 5 and the surrounding text). The Mancall *et al.* (2008, Table 1) estimate for the Lower South refers to the twenty years 1770-1790.

<sup>29</sup> Hildreth's summary (1849, vol. III, pp. 465-466) is cited by McCusker and Menard (1985, p. 365).

An estimated 60,000 free persons (3.1 percent of the free population) and 15,000 slaves (3.6 percent of the slave population) had left as of the early 1790s (Jasanoff 2011, pp. 351-358). The losses to the American economy were presumably much greater than the losses that the

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departing loyalists experienced themselves. A high estimate of loyalist claims presented to His Majesty for losses in American rebellion came to \$1,053,024 at the \$4.44 exchange rate, or about 0.6% of the 1774 income of the 13 colonies (Eardley-Wilmot 1815, reprinted 1972, Appendix VIII). Of course, this ignores the human capital that the new republic lost.

<sup>30</sup> For the vast 1960s-1970s literature on early wealth inequality, see Williamson and Lindert (1980). For the wage ratios, see the same source and the important update by Robert Margo (1990).

<sup>31</sup> For the specific contrast of consumer prices between New England and other regions, see the file “Massachusetts vs. England and WV” at <http://gpih.ucdavis.edu>, and also Allen, Murphy, and Schneider (2011, Table 3). On the more general subtlety about class- and place-specific costs of living, see Williamson (1977) and Hoffman *et al.* (2002). England’s lower interest rates may also have implied a lower user cost of capital than in the colonies.

<sup>32</sup> See, for example, Kulikoff (1986), Carr *et al.* (1991), and Walsh (2010). See also Robert Gallman’s (1982) study of Perquimans County, North Carolina.