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**ABSTRACT**

Building social tables in the tradition of Gregory King, we quantify the level and inequality of American incomes before and after the Revolutionary War. Our tentative estimates suggest that between 1774 and 1800 American incomes fell in real per capita terms. The colonial South was richer, and then suffered a greater Revolutionary decline, than suggested by previous estimates. Any rapid growth after 1790 seems to have just partially offset part of a very steep wartime decline. We also find that free American colonists had much more equal incomes than did households in England and Wales. Indeed, New England and the Middle Colonies appear to have been more egalitarian than anywhere else in the measurable world. The colonists also had greater purchasing power than their English counterparts over all of the income ranks except in the top few percent.

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## I. Early American Growth and Inequality Debates

American economic historians need fresh information on the income levels that prevailed at the end of the colonial era and the dawn of independence in order to understand this country's growth process and its evolving social structure. This need keeps arising whenever we try to cast back from today to the late colonial period, to project ahead from early colonial years, or to view American incomes in trans-Atlantic perspective.

The debate that tries to back-cast American growth from 1840 has centered around Paul David's classic 1967 article on "New Estimates, Controlled Conjectures", a descriptive label that should apply to this paper as well. David, Robert Gallman, Thomas Weiss, and others centered their plausible conjectures on the division of the economy into large sectors, each with its own growth of labor force and labor productivity. The debate has been hampered by the paucity of data on the occupations and the output-sector mix of the labor force before 1840. The early censuses did not help much with these, except for giving 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 occupations that Americans held. Even though many household heads were simply called farmers, planters, farm hands, or slaves, it helps considerably to know what labor force shares they represented, where they lived, how their occupation's average incomes ranked, and how those incomes changed over time.<sup>i</sup>

Economic growth, or its absence, across the colonial era defines more contested territory. Some have seen stagnant productivity, with only Smithian growth of population and land area, and without gains in average living standards. Others have seen evidence of considerable productivity growth, some emphasizing seventeenth-century emergence from initial hardship and mortality, and others emphasizing gains across the middle of the eighteenth century. This debate has also been hampered by lack of knowledge about labor inputs and occupational structure, and by the roughness of any estimates of productivity growth within such sectors as agriculture or shipping.<sup>ii</sup>

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 and others' index numbers. One wonders how far colonial income could have lagged behind in the eighteenth and early nineteenth centuries, given the significant net migration from the Mother Country and the much faster colonial population growth. In the light of current research on the Great Divergence, on the history of European incomes, and on the continued use of the Angus Maddison world income estimates, we think the time is ripe for adding data to the American side for comparisons with the new estimates for Europe.

We can now offer a bumper crop of new estimates based on more archival data than were available to earlier researchers. The harvest is 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. The positive reason for open sourcing is the dynamism of the database itself. The same information explosion that has offered us new data will continue to offer further new data to all scholars. Maximizing the disclosure of our data and procedures accelerates the opportunities for improving the reliability of the estimates. Hence our paper is inextricably tied to a growing downloadable set of spreadsheet and text files at <http://gpih.ucdavis.edu>. It also makes it unnecessary to attach a ponderous, multi-page appendix here.

Our "controlled conjectures" confirm some popular hunches about growth and inequality in early America, yet contradict others, and introduce still other possibilities for the first time. As of this writing, we have a clearer view of colonial American inequality and how the incomes of different classes compared with their counterparts in the Mother Country. The inequality was clearly lower, especially among free whites, than in England or Wales at the time – and clearly lower than in the United States today. The levels of average income seem to have been close to those prevailing in England and Wales, using either exchange rates or some hints about purchasing power parities. In terms of economic growth, we find higher colonial incomes in 1774 than did past scholars, especially higher in the Southern colonies. Given that our estimates for national income in 1800 are also higher than those of other

scholars who derived them in completely different ways, our estimates raise new questions about what happened between 1774 and 1800, during the Revolutionary War era and then at the dawn of the Republic.

## **II. How the Sausage Is Made**

### **A. The 1774 Colonial Recipe**

Our approach starts from a renewed emphasis on counting people by occupations or social classes, and mustering evidence about the average incomes in those classes. Economists will recognize our approach as one that builds 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 offered price indexes to anybody who wanted to know what those incomes might have looked like in the price levels of another 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 seventeenth century. That is indeed our approach here, as in some earlier publications.<sup>iii</sup> In fact, at least two early American efforts tried to imitate Petty with their own calculations of what their region was worth economically – presumably to guess at its ability to pay taxes and fight wars.<sup>iv</sup> Table 1 shows these two imaginative efforts, one by the colonial Governor of South Carolina in 1751 and one by Samuel Blodget in 1806. Neither cited their sources in any detail.

Fortunately, the archives continue to accumulate early 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 necessitates a tour of the sausage factory in which we counted colonials and determined their incomes.

Any social profile of Americans on the eve of the Revolution must start from local censuses, supplemented by tax lists and occupational directories, gaining indirect support from the earliest national returns of 1790 and 1800. Fortunately, the recent electronic revolution has made local enumerations from the late eighteenth century more accessible.

While all records before 1790 were local, we can develop aggregate regional counts by assuming that proportions from one documented locality can represent the patterns of other localities in the same economic region.

Our path to a rough count of early Americans by work status and living arrangement starts from basic population counts themselves, and then adds early US labor force estimates, before dividing up that labor force by occupation and by household headship status.

(1) Population census counts. There were a few local censuses from the colonial period, which 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 very little detail other than sex, race, free/slave status, and occasional rough age distributions. Those over and under the age of 16 are recorded for seven of the colonies, with extra detail on Vermont and Maine. For the other six colonies, we estimated the age distribution by combining the 1774 total populations from the Bicentennial Edition with age distributions from 1790 and 1800.

(2) 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. When our tabulations include property incomes, we implicitly stretch the definition slightly to the small number of recipients of property income who did not rent out their labor.

To convert demographic groups into numbers in the labor force, we use the labor participation rates for 1800 supplied by Thomas Weiss. It seems reasonable to assume there were no behavioral changes in the rates defined in the detail of the Weiss estimates, which give separate rates for such 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.

(3) Recorded occupations. Sketching the social make-up of the labor force requires detailed occupation counts for different kinds of 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. The comparisons involve some cloning of one context in the image of another. Yet they also interpolate and extrapolate some ratios without assuming strict equality between contexts.

Our fresh start on the social structure of America on the eve of the Revolution uses local tax assessment lists and occupational directories reported in Table 2. Such lists allow us to create the following occupational groups for the free population:

- Group 1 = Officials, titled, professionals
- Group 2 = Merchants & shopkeepers, big city only
- Group 3 = Merchants & shopkeepers, outside big city
- Group 4A = Skilled artisans in manufacturing
- Group 4B = Skilled building trades
- Group 5 = Farm operators
- Group 6A = Male menial laborers
- Group 6B = Female menial laborers

The new data re-shape the occupational structures of the colonies. For example, relative to Alice Hanson Jones (1977), our estimates shift a lot of the Middle Colonies' population weight from middling farmers to less wealthy craftsmen, laborers, and males with no given occupation.

In the urban South, the directory for Charleston 1790 seems usable here, when scaled back to the estimated total population of Charleston in 1774, although 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 by either source. We adjust the Jones weights, guided by a useful set of local censuses in three North Carolina counties in 1779-1782. These enumerate the whole population of 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. One could arguably add Baltimore's population to that of Charleston, absent any separate sampling of occupations from Baltimore before 1799. This would give greater weight to the big city occupational and property patterns at the expense of

the countryside in the South. Such an adjustment would give slightly higher average income, and higher inequality, for the South as a whole, but only slightly.

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

(4) Unrecorded occupations. The counts of persons with occupations recorded by tax assessment lists or urban occupational directories fall short of the numbers of persons in the labor force. In most cases they even fail to capture all household heads, the exception being those three counties of rural North Carolina between 1779 and 1782, for which the listings seemed to have captured all household heads.

Not all unlabeled labor force members are equal. Some simply lack an occupational label, despite a positive amount of assessed wealth. Some lack an occupational label, and are also listed as tax-exempt because they had near-zero wealth. Some are men, and some are women. In the social tables, we end up distinguishing between

- Group 7 consisting of free men with positive wealth but no recorded occupation,
- Group 8 consisting of the corresponding free females, and
- Group 9 consisting of free persons recorded as having near-zero wealth and no stated occupation, and others who are in the labor force but completely unlisted in local records.

(5) Counting Households. The other population count we need to quantify is the number of household heads in each occupational category. We 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. But we need the headship rates to measure inequality.

We need to focus on households as income recipient units in order to measure income inequality, both for practical and theoretical reasons. Empirically, we need to conform where

possible to the practice followed by other countries, in order to compare apples with apples. The prevailing practice is to measure income inequality among households, not among individual income earners. For centuries other 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.

Simon Kuznets (1976) emphasized the superiority of the household focus on theoretical grounds. 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 pleaded for measuring annual income per consumer in the household. The numerator must capture the incomes of all economically active household members, and the denominator should capture the number of adult-equivalent consumers. He warned against measuring inequality among individual earners.

Since the early population censuses usually did not count households, some assumptions must be invoked to decide which demographic parts of the population are in fact household heads. Fortunately, historians of early America have already grappled with this issue. Following the leads of Billy Gordon Smith (1981, 1984, 1990) and the late Lucy Simler (1990, 2007) in particular, we estimate the number of household heads from population data from c1774 and c1800 invoking the following assumptions:

- (1) All free white males, 21-up, were household heads, subject to (4) below.
- (2) All free white widows with any indication of property ownership or of occupations were household heads.
- (3) One-sixth of the free black population consisted of household heads.<sup>v</sup>
- (4) The number of free white males, 21-up, who were not household heads is matched by the number of free white females, 18-up, who were household heads, despite not being included in (2) above. That is, we assume that two errors offset each other when using the white males 21-up as household heads. Hopefully, later studies can make more headway on the headship rates of these two demographic groups.

These assumptions have generated the total numbers of households by place – that is, by region and by urban versus rural.

Next, by subtraction, we find the numbers 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. While we leave the actual counts to the posted files that develop our estimates, the business directories and the tax lists that originated in the colonial era missed more than 30 percent of all households, threatening underestimation of total income and also, if they mostly came from poor classes, threatening to bias inequality measures downwards. Less ominously, the more recent tax lists may have captured something like the full population. So it appears in our early tax samplings from New York State's property taxes that began around 1799. The numbers of assessments approximate the roughly measured numbers of households. The same might have been true of the federal direct tax of 1798. There are also hints that the newer post-Revolutionary taxes also gave near-market value assessments.<sup>vi</sup>

Still, we have found significant omissions in tax lists and business directories, especially those from the colonial era. Which groups were most frequently omitted? The literature has advanced the plausible intuition that the main omitted groups consisted 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 have been omitted, or at least that their wealth was under-assessed.

Aided by clues from the tax assessments, we divide the household heads of no recorded occupation into three groups:

- Group 7 = Male household heads assessed with positive assets but no stated occupation,
- Group 8 = Female household heads assessed with positive assets but no stated occupation (including many widows), and
- Group 9 = Household heads of either sex identified as having zero assessable wealth.

There are three tough questions still left 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):

- How many of them were there for each place defined by region and by urban/rural?
- What kinds of occupations and earnings rates did they have?
- Whose households did they live in, and share resources with?

Guided by the censuses, we identify the following groups in the labor force who were not household heads:

- Group 10 = Free white males ages 10-15
- Group 11 = Free white females ages 10-15
- Group 12 = Free white males ages 16-up, but not household heads
- Group 13 = Free white females ages 16-up, but not household heads = LF for this group, minus Group 8
- Group 14 = Free black males ages 10-15
- Group 15 = Free black females ages 10-15
- Group 16 = Free black males ages 16-up, minus free black household heads
- Group 17 = Free black females ages 16-up
- Group 18 = White indentured servants in Maryland, the only colony that labeled them separately in a census near 1774, and
- Group 19 = Slaves ages 10-up (only some censuses reported males and females separately).

Some of these groups contained laborers who were almost surely paid only unskilled wage rates, while others could have been spread over occupations of higher earnings. Our income estimations make the following assumptions within each location:

- Groups 10, 11, 14, and 15 (free non-HH heads ages 10-15) are allocated to Groups 6A and 6B, menial or unskilled, by sex.
- Groups 12, 13, 16, and 17 (free non-HH heads 16-up) are allocated location-specifically across Groups 2-6 in the same proportion as are Groups 2-6.

- Group 18 Maryland servants are allocated across occupations following Galenson (1981) and Grubb (1985), occupations which were relatively urban and skilled (see “Indentured LF earnings/own-labor incomes 1774” at <http://gpih.ucdavis.edu>).
- For Group 19, slaves ages 10-up, we use other scholars’ estimates of the amount of their earnings they were allowed to retain for consumption, specific to region and occupation. In the South, the rate varied between 41.4 percent for field hands to 52.7 percent for Charleston labor (see “Slave earnings divided/own-labor incomes 1774” at <http://gpih.ucdavis.edu>). Following Claudia Goldin (1976), Richard Wade (1964), Richard Sutch (1975) and others, slaves doing non-farm work are assumed to have been artisans, construction workers, or unskilled (including servants). The non-farm share unskilled is based in the Charleston 1848 census, while the residual is divided equally between artisans and construction workers (see “Slave LF 1800” at <http://gpih.ucdavis.edu>).

Using indirect census clues, we further assume that 35 percent of slaves over the age of 10 were household heads.

So far, the calculations affect both our estimation of aggregate national income and income inequality. For inequality purposes, following Kuznets we must further 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:

(1) For each region and urban/rural (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.

(2) For the free population, within each group defined by region and urban/rural, we assume that the average earning power of each non-HH head imported into free families is the same for all free families in that place. We do not imagine any correlation between earning power of the household head and the earning power of the non-head LF participants.

(3) Slave non-heads are taken into slave households only, leaving household income the same as the retained earnings of all slaves.

(4) The same assumption holds for the separately recorded group of Maryland servants, though the assumption is redundant here because these are one-person households.

The resulting allocations can certainly 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. The allocation of non-HH heads to households by place is not yet derived from micro-studies about how households shared earnings, because there are too few such studies. Nor are the allocations simply assumed, except for the key middle-of-the-road assumption (2) above.

(6) Labor earnings by occupation, circa 1774. We are able to assign incomes per year 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), the many articles of Donald Adams (1968, 1970, 1982, 1986, 1992) and the contributions of a few others.<sup>vii</sup> 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 we used, and Main in particular, were careful to say which was which. Some of the earnings are annual, as we would wish, but others are monthly or daily rates of pay, requiring us to make assumptions about how many days or months one spent in gainful employment each year. We believe that for those days or months in which a person 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 much of this output was traded on the market. Thus, we assume that our daily or monthly full-time equivalent (FTE) workers performed productive work of some kind for 313 days a year (excluding only Sundays). This might overstate formal-sector incomes somewhat, but hopefully not total income.<sup>viii</sup> To repeat, many of the wage data are already in the appropriate annual form, especially for merchants and white-collar professionals.

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".<sup>ix</sup>

(7) Property income in 1774. On the property income side, we have the benefit of 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>x</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, finding no flaws.<sup>xi</sup> Jones realized that a probate-based sample ran the risk of overstating the wealth, and understating its inequality, because probate was a process that was more likely for the rich. She went to enormous labors to adjust for this, ending up with a set of  $w*B$  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 earlier in this section. As it turns out, we come up with an even greater wealth markdown than did her  $w*B$  estimates.

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

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 type. For the kinds of producer assets that would have entered into NIPA accounts, 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 94 percent (on top of the 6 percent) for producers' perishables and crops. For these last two perishables, the wealth at a moment in time equals the gross income that such an inventory would yield within a year.<sup>xiv</sup>

(8) Combining own-labor income with property incomes. Here we reap a main advantage of our having invested so much effort in gathering occupation data. Since own-labor incomes and property incomes are both arranged by occupations, we can combine the two to get total incomes. For farmers, the largest occupational group, we can even exploit some of the regional size distribution of property income data, dividing it into the top 2 percent of farmers, the next 18 percent of farmers, a middle 40 percent, and a bottom 40 percent. This disaggregation helps us judge the degree of income inequality within each region.

(9) Households were practically the whole economy. Our calculations offer what national income and product (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 only eight of them by the end of the century. The same assumptions will be made for 1800, except that we must reckon government proceeds from substantial land sales. When a future paper compares 1774 and 1800 with similar accounts for 1860 and 1870, the non-household sector will take a significant share of national income.

## **B. The 1798-1800 Post-Revolutionary Recipe**

After 1774, the next data-useful benchmark for appraising national income is 1800, a census year preceded by America's first direct tax, the one-off tax levied in 1798 on real estate wealth and on the numbers of slaves, to fund a possible conflict with France.<sup>xv</sup> On the labor-income side, our procedures for 1800 are roughly the same as those we applied to 1774, though the data sources change. What is distinctive about the estimates for 1798-1800 relates not to labor incomes but to property incomes.

On the property side of 1800 national income estimation, the 1798 direct tax returns remain the most useful source available. 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>xvi</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>xvii</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 Blodget (1806).

The South might have under-assessed realty, along with slaves, by even more than the North. We have three clues about the degree of underassessment in the South. 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 ages 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 sample values for the rental incomes derived by slaveholders.<sup>xviii</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 21.0 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, or slaveholders gained a considerable tax break relative to other owners of real estate. It seems clear to us that the fifty-cent tax per slave reported to be 12-50 years old was based on an undercount of those slaves.<sup>xix</sup> Fortunately, our estimates of slaveholder incomes are based on market values rather than on any assessment values for slaves.

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, a share that is close to the South Atlantic share of the free population of the eastern states in 1800 (35.0 percent), and also close to the South Atlantic's share of the free population over age 10 (34.2 percent). More ominous is the comparison with its

share of real estate wealth in the non-fiscal probate valuations for 1774. At that time the South Atlantic's share of the thirteen colonies' realty income and wealth had been much higher, at 57.7 percent.

The realty clue offers two extreme possibilities, and we will choose a midpoint between them. At one extreme the South could have avoided paying taxes on its 57.7 percent share of all true market-based value of realty in the 13 colonies, getting away with paying taxes on only 38.1 percent. This implies that South Atlantic real estate in 1798-1800 should be marked up greatly, from a South/North assessment ratio of  $38.1/61.9 = 0.616$  to a market ratio of  $57.7/42.3 = 1.364$ .<sup>xx</sup> Borrowing from Henry David Thoreau's famous saying that "Some circumstantial evidence is very strong, as when you find a trout in the milk", we will call this extreme assessment hike the "full trout" assumption.

At the other extreme, if the South paid its fair 38.1 percent share of true market value, then its incomes and real estate values must have crashed in the Revolutionary War years, but kept up with the rest of the country between 1800 and 1840.<sup>xxi</sup> This extreme will be called the "no trout" assumption.

Neither extreme seems persuasive. The no-trout assumption would have to disregard the three clues that the South got a lower tax assessment rate. The full-trout 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 greatly after 1800 also strains belief. The true underassessment differential favoring Southern realty probably lies between these two extremes. We therefore provisionally settle on the "half-trout" assumption. That is, we assume that the North had the 15.5 percent underassessment of real estate demonstrated in the 1799 market study, and that the South had the same 15.5 percent underassessment plus half of the full-trout 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, or 13.0 percent for the whole Eastern seaboard, in 1798-1800. 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.

Next, 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 a different ratio for 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 already aggregated at the time, saving us the labor of working up aggregates from below. A serious drawback of the 1798 returns, however, is that they report no data on occupation. This means that we cannot discuss the occupational distribution of *total* income for 1800, although we can document the distributions of own-labor incomes and of property incomes separately, and the aggregate value of total incomes. To survey the resulting estimates, we turn first to the levels and growth of aggregate income over the quarter-century 1774-1800, and then to the income inequality of 1774 alone.

### **III. Provisional Conclusions about Income Levels and Growth, 1774-1800**

The new data on 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 3-6, 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. Table 3 can be used to calculate any of several important ratios, using the denominators in the lower half of the table and the price deflators in the notes to the table. Here we stress two key results. First, in 1774 the colonial South had about twice the income per capita of New England, even when one rightly counts slaves as persons. That the South was so much richer may not surprise those who have studied the history of colonial wealth, but even average free labor earnings were higher in the South, despite the competition from slaves. Second, these new estimates imply that real income per capita dropped seriously over that quarter century. The 1774-1800 decline of 27 percent looks as serious as the Great Slump of 1929-1933 in per capita terms (though total income fell much less). If other authors are correct in reporting brisk income gains across the 1790s, then the Revolutionary War period could have been the country’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.

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

The new estimates suggest that in 1774 the Southern colonies were richer and more productive than other estimates have implied. The left half of Table 4 underlines the contrast, focusing on the more recent and more prominent set of scholarly estimates. Our thirteen-colony current price estimate of 189.7 million dollars is 38 percent greater than the average of the Jones and McCusker estimates (137.7 million). Yet our thirteen-colony income estimate differs greatly from that 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 19 percent higher than hers. The main source of the big difference with Jones arises in the South, for which our income estimate (\$110.7 million) is almost twice that of Jones (\$59.15 million)!

We have examined this difference at length. Thus far, we cannot find an error in our new estimates for 1774. The gap is not driven by a higher estimate of wealth per household, estimates that continue to rely on Alice Hanson Jones’ own impressive 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 a lot fewer households with wealth than her estimated number of “potential wealth holders”, our aggregate wealth estimates are actually only about 70 percent of her implied total wealth (as shown elsewhere).<sup>xxii</sup>

The striking discrepancy relates to two steps we have taken, one relating to property income and one to labor income. Neither of these steps was taken by Alice Hanson Jones, nor by John McCusker, nor by any other scholar that pioneered in estimating early American incomes. Both steps are important, and need elaboration here.

Our conversion of wealth into property income, as described earlier, involved multiplying different asset holdings by net and gross rates of return. A reader feeling that our income estimates seem high might want to challenge both our net and our gross rates of return as being somewhat high. As for the net rate of return, corresponding to the opportunity cost of earning interest instead of the asset in question, it seems unlikely that our 6 percent figure 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>xxiii</sup> Yet, the demand for capital was strong, 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>xxiv</sup> Table 5 shows the impact of assuming an 8

rather than 6 percent rate. Not surprisingly, shifting to the higher rate would raise our total income estimates even further above those conjectured by other scholars: it would raise the total gross personal income estimate by 4.4 percent in 1774, and by 7.0 percent in 1800. If instead the 8 percent interest rate was relevant in 1800 only, then our 1774-1800 decline in real per capita income (implied by Table 3, shown in Table 6) would be 22 percent rather than 27 percent.

One might also challenge our depreciation assumptions in deriving gross property incomes. Indeed, some might argue that depreciation should not have been included in the income estimate at all. If the reader prefers net property income estimates, ones that only include that 6 percent rate of return on wealth, then she can refer to the 1774 net household income estimate of \$162.6 million shown in Tables 4 and 5.<sup>xxv</sup> This net household income estimate would eliminate about half the gap between our gross income estimate of \$189.7 million and the \$137.7 million average estimate offered by other scholars. That partial convergence might seem comforting, but it should not. The debate over early American economic growth has consistently used gross national (or domestic) product, not net national product. We should conform to the same convention for purposes of comparing apples with apples. Thus our favored 1774 aggregate income estimate remains the gross income figure of \$189.7 million shown in Tables 3 through 5.

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, thus raising our aggregate income estimates above that of previous scholars? The source of the difference cannot lie with our new occupation shares, which give greater weight to less affluent and less probated whites, since this would serve, once again, to make our estimates *lower* than Jones's, not higher. We also do not think the full-time employment assumption of 313 working days per year for those hiring out is inappropriate, given the widespread prevalence of home production and direct non-market consumption in the eighteenth and early nineteenth century.<sup>xxvi</sup> This brings us to the 1774 wage rates. What did Jones assume about rates of pay for labor, especially Southern 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 estimates that should stand or fall on her assumed aggregate capital/output ratio (not necessarily the same as a wealth/income ratio). Noting that the literature offered

capital/output ratios ranging from 2.5 up to 10 for the nineteenth and twentieth centuries. Within this range, she said, “I hazard that ratios of three or three and a half to one may be reasonable”.<sup>xxvii</sup> While our income estimates for New England and the Middle Colonies are close to those of Jones, we think she may have overstated the wealth/income ratio for the South, where abundant slaves could have substituted for capital, and lowered the capital-output ratio relative to the macro literature from which Jones took her capital-output ratio of 3.5. For the present, we retain our estimates for the South, which are based partly on her own occupation weights, adjusted by some new data we have found for three North Carolina counties, plus our new exploitation of wage data.

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

For 1800, unlike for 1774, our estimates of total income are not above those offered by other scholars. As shown on the right-hand side of Table 4, ours are in fact in the middle of the several estimates for the nation as a whole. For the Lower South in 1800, our estimates match those of Mancall, Rosenbloom, and Weiss (2003), even though we used the income approach and they used the production approach. Having our estimates so close to others for 1800 might seem comforting.

However, our estimates would have been a bit higher than most other income estimates for 1800 if we could have made all the adjustments that we feel are warranted. We are especially concerned about two such adjustments. One of these can be quantified but one cannot.

The first potential adjustment is one already mentioned in connection with Table 5: There is evidence that the net rate of return on property, using the interest rate on public debt as a measure of the opportunity cost of assets, was higher in 1800 than in 1774, in response to Revolutionary inflation and the disruption of finance.<sup>xxviii</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, 22.4 percent, using the “alternative” estimates for 1800, shown in Tables 5 and 6, rather than the bigger decline implied by the baseline estimates, 27.4 percent.

The second adjustment relates to an omission from the baseline 1800 estimates. We have no 1800 data, or even guesses, about 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 Jack Main 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, 34.0 percent in the South, and 28.8 percent for the 13 colonies as a whole. We cannot apply these ratios to 1800,

however. The main barrier is the lack of any delineation between farm operators and free farm laborers in the census or in the Weiss labor force estimates on which we rely.<sup>xxix</sup> In addition, the likely jump in rates of return on farm property from something like 6 to 8 percent should have meant a drop in farmers' pure-profit share in their total income. For now we can only propose our alternative estimates in Table 6, and note that accordingly the nation still experienced a big net income decline of 22.4 percent over the quarter century, though the decline may have been a little less than our estimates show.

### **C. Long-Run Growth Implications**

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.

How do the estimates fit into accepted narratives about the longer run development of early America and its regions? Our new higher income estimates for 1774 will, of course, re-open the debate over growth during the long colonial period. It seems wisest to refrain from commenting on this part of the debate until our social tables technique has been applied to some benchmark date earlier in the eighteenth century. Here we venture only a conditional statement: *If* others' estimates for earlier colonial dates are unchanged, our higher income per capita figures for 1774 would imply more support for the optimists arguing for some colonial per capita income growth (0.3-0.5, according to McCusker and Menard 1985) and less support for the pessimists (zero, according to Mancall and Weiss 1999).

We are more confident in using our income estimates to assess America's growth performance up to 1840. Table 6 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), and it does so for 1774-1800, 1800-1840, and 1774-1840. Real per capita incomes in the three-region "nation" grew very slowly over the entire period, 0.38 percent per annum, a rate that would have taken almost two centuries to double income.<sup>xxx</sup> But Table 6 suggests that this pre-modern growth performance<sup>xxxi</sup> was driven by two special events: the economic disaster associated with the Revolutionary War, and the lagging South. Between 1800 and 1840, per capita income in the North grew at very fast pace, 2.10-2.25 percent per annum in New England, and 1.45-1.63 percent per

annum in the Middle Atlantic, rates that are consistent with the 5 percent per annum industrial output growth centered in the Northeast (Davis 2004). These rates are far in excess of western Europe, the alleged leader of the pack: for example, between 1781 and 1801, per capita income in Britain grew at only 0.35 percent per annum and at 0.52 percent per annum 1801 to 1831 (Crafts 1987). In contrast to fast growth in the Northeast, the figure for the South Atlantic was only 0.43-0.61 percent per annum, which pulls down the “national” average to a still impressive 1.26-1.43 percent per annum, well above the one percent per annum criteria needed to make the modern growth club. All of these impressive growth rates fall significantly when the period is extended backwards to 1774. Over these seven decades, the “nation” grew at only 0.38 percent per annum, as we have seen. But the per annum rates for New England, 1.24, and the Middle Atlantic, 0.69, were almost two to four times that of the “nation” since the South recorded a secular fall, -0.38 percent per annum.

How do our estimated per capita income growth rates 1800-1840 compare with others? Our per annum figure for the South Atlantic is 0.43-0.61 while Mancall, Rosenbloom, and Weiss (2003) estimate something similar for the Lower South, 0.53-0.79. For the US as a whole, Weiss (1992: Table 1.2, p. 27) offers three estimates, ranging between 0.56 and 0.80, far below David’s (1967, Table 8) estimate of 1.13. Our estimates are even higher than David’s, 1.26-1.43 percent per annum,<sup>xxxii</sup> though including farmers’ residual profits in 1800 would have lowered our 1800-1840 rates a bit.

The absolute decline of the South Atlantic over the last quarter of the eighteenth 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.<sup>xxxiii</sup> By 1840 the South Atlantic was already well behind the Northeast and the national average, having been well ahead of all other regions in 1774. We can find no evidence that the colonial South had any large army of poor whites in 1774. To support this assertion, 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. Furthermore, Southern free labor had some of the highest wages anywhere in the colonies. Thus, it appears that the concentration of America’s poor whites in the South was strictly a nineteenth- and early-twentieth-century phenomenon, associated, presumably, with post-1774 decades of very poor growth.

#### **D. Revolutionary Shocks: Diverted Trade and the Crisis at the Top**

What stands out in the longer run perspective is again the turbulence of the years between our two historical benchmark years 1774 and 1800. 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, and early twentieth century Russia, scholars of the early United States have great difficulty in bridging the data gap across their revolutionary upheaval. 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 partial recovery of foreign markets). Yet, the more we come to accept their sanguine view of the 1790s, the more we must infer an 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 depression by posing a joint hypothesis: How deep would the per-capita income loss have been from 1774 to 1790 if (a) other scholars are right about the growth from 1790 to 1800, and (b) our estimates are right about the net decline from 1774 to 1800? This question has six numerical answers, based on our two estimates for 1800 (“baseline” and “alternative”) times the three leading series for real income per capita from 1790 to 1800. The three series are those by Richard Sutch, Louis Johnston and Samuel Williamson, and Thomas Berry (alias Series Ca11, Series Ca16, and Series Ca177 in the *Historical Statistics of the United States* 2006). All six combinations imply a serious drop in real per capita income from 1774 to 1790, one that was longer lasting, if not deeper, than the 1929-1933 fall into the Great Depression. The smallest estimated drop from 1774 to 1790 is 28 percent, based on the Sutch series and our alternative estimate for 1800. The largest estimated drop is 39 percent, based on Berry’s series and our baseline estimate. Thus far, our reading of the evidence supports Allan Kulikoff’s emphasis on the destruction and economic depression attending the Revolutionary War, as well as others’ belief in a strong partial recovery across the 1790s.<sup>xxxiv</sup>

What could have caused such sustained income losses? There is good *prima facie* evidence for two related shocks to the American economy that could have been large enough to cause a deep depression between 1774 and 1790, and one that was concentrated in the South and in every coastal city and smaller river town.

The first shock consisted of the disruptions of overseas trade from the Revolution and, after 1793, the Napoleonic Wars.<sup>xxxv</sup> The historical literature and some available price and trade data show that the colonies, especially in the Lower South, suffered heavily in trade volumes and values as the war deepened. James Shepherd and Gary Walton have studied in detail the changes in American exports of commodities and shipping services. In real per capita terms, regional commodity exports changed as follows from 1768/72 to 1791/92: they rose by a modest 1.2 percent in New England, rose by 9.9 percent in the four Middle Atlantic colonies/states, but declined by 39.1 percent in the Upper South, and by 49.7 percent in the Lower South, yielding a decline of 24.4 percent for the thirteen colonies as a whole.<sup>xxxvi</sup>

Most of the Southern, and national, trade shocks were demand-side in origin. The most obvious, of course, was the loss of well over half of all trade between the mainland colonies and England between 1771 and 1791. In addition, the loss of Imperial bounties decimated the Lower South's indigo revenues and, to a lesser extent, that of naval stores. New England suffered a similar loss of bounty on its whale oil. Demand shocks were less evident for products where a decline in American supply raised price. Thus, the losses in tobacco and rice revenues were smaller, since the fall in volume was partially offset by the rise in relative price. In addition, the Revolutionary cloud had another silver lining for the Chesapeake which shed the Navigation Acts and thus could export directly to Continental European markets.

While these negative demand shocks to American commodity export markets 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" even though trade volumes plummeted in the South; that is, a 6-7 percent share times a 24-percent trade fall equals no more than a two-percent fall in income.

The second major negative shock we call a "crisis at the top". It was related to the trade losses, but transcended them and could have caused much greater income losses. America's urban centers were damaged by British occupation, the ebb and flow of war, and the eventual departure of skilled and well-connected loyalists. In particular, New York, Charleston, and Savanna were not free of Loyalists and waves of recriminations between the warring sides until 1783. We believe that the

damage to urban economic activity was considerable, and potentially enough to bring great declines to per capita incomes, even though the population kept growing.

Very little of the urban damage can be revealed just by the direct losses from the exodus of loyalists. For all their suffering, only an estimated 60,000 free persons (3.1 percent of the free population) and 15,000 slaves (3.6 of the slave population) had left as of the early 1790s. A high estimate of their claims presented to His Majesty for losses in America came to \$1,053,024 at the \$4.44 exchange rate, or less than 0.6% of the 1774 income of the 13 colonies.<sup>xxxvii</sup>

To identify the extent of the urban damage, one could start by noting that the population of the major urban centers grew very little over a quarter century in which total population actually doubled. Accordingly, the combined population share of Boston, New York City, Philadelphia, and Charleston 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 share certainly confirms what our income estimates are documenting.

Even stronger evidence is offered, however, by our data on the employment numbers and rates of pay by occupation and location in 1774 versus 1800. These reveal what one could call an urban-oriented “crisis at the top”, that is, a sharp decline in both the labor force share and the relative earnings of top white collar groups in cities and small towns, groups consisting of officials, the titled, highly-paid professionals, teachers, merchants, and shopkeepers. The share of the free labor force in white collar positions had been 12.7 percent in 1774, but fell to 8 percent in 1800. The ratio of earnings per free worker in urban jobs relative to that of total free workers dropped from 3.41 in 1774 to 1.53 in 1800. Similarly, the ratio of white collar earnings per worker to that of total free workers fell even more, from 5.21 in 1774 to 1.65 in 1800. This evidence offers strong support for the urban crisis at the top thesis. It also supports the view that America had not yet recovered from the Revolutionary War economic disaster even by 1800.

### **E. Implications for Property Income and Tax Shares**

The income compression implied by the crisis at the top is but one of several ways in which the Revolutionary Era affected the distribution of income. Let us note two other income distribution effects implied by our estimates, before turning to an international perspective on American income inequality on the eve of Revolution.

One aggregate shift should have widened inequalities among Americans, offsetting the income-compressing effect of the urban crisis at the top. Table 3 reveals a striking rise in the property income share from 28 percent in 1774 to 36 percent in 1800. Furthermore, that rise took place in each of the three regions, by 10 percentage points in both New England and the South Atlantic, and by 5 percentage points in the Middle Atlantic. Note also the striking regional differences in the levels of that property share. While the property income shares were roughly the same in the Middle colonies and the South, they were considerably lower in New England: in 1774, for example, the figures are 31.4 and 32.3 percent in the Middle Colonies and the South, respectively, and only 11.9 percent in New England. The aggregate rise from 28 to 36 percent indirectly supports the possibility, already discussed above regarding the interest rate evidence from Homer-Sylla and from Rothenberg, that the annual net rate of return on property assets might well have risen from around 6 percent in 1774 to 8 percent in 1800. After all, a rise from 6 to 8 percent implies by itself a rise in net property incomes by a third (Table 5). Future research should tell us more about the forces that produced these trends. In the meantime, we note that this shift toward property incomes would, by itself, have implied a rise in inequality between the colonial sunset and the early federal dawn.

Another dimension of income distribution that can now be roughly quantified is the share of taxation in national income. Here we can draw on Alvin Rabushka's (2008) monumental study of colonial taxation and some tax-related contributions by H. James Henderson and John McCusker, which yield average tax shares when combined with our regional incomes for 1774 and 1800. The new figures confirm what some British observers resented at the time of the Revolution: The colonists paid very little in the way of taxes, relative to residents of the Mother Country or to the citizens of the new United States. This fundamental point can now be quantified in the form of tax shares of income, as long as one accepts some slight mis-matches of place and year. As of 1775, Rabushka finds per capita tax rates in New England that were less than one percent of the region's average income per capita, though McCusker's addition of some imperial taxes to the provincial incomes raises the Massachusetts rate over one percent. The rates in New York and New Jersey were even less than 0.1 percent of regional average income. Virginians paid only about one percent as of 1765-1773. Only for South Carolina do we find evidence of a colonial tax burden over two percent of the average regional income. By the 1790s, representation brought more taxation with it, since those rates in Massachusetts and Virginia rose above two percent. Even with freedom and

representation, however, the Americans paid far lower taxes than did their counterparts in Western Europe (as they do today).<sup>xxxviii</sup>

No doubt these tax rates rose during the Revolutionary War, especially when one adds the much greater implicit taxation by sudden inflation. Much of that tax burden would have been in kind by serving in the Continental Army (getting paid only irregularly and in depreciated currency), by supplying both armies at less than market prices (sometimes looted), by moving the army in transport hired at less than market rates, and so on.

#### IV. Colonial Inequality in World Perspective

We now turn to the other main object of our early American income estimates, a comparative assessment of colonial income inequality. Thanks to the data and procedures described above, we can explore the distribution of incomes, just as Alice Hanson Jones did for wealth. Also, like her study, ours is able to divide American incomes into three regions.

Table 7 displays the income inequality results. One way to put these results into historical perspective is to compare them with income in the United States today, where almost 20 percent of all household income accrues to the top 1 percent of households, and where the Gini coefficient is about 0.50.<sup>xxxix</sup> In 1774, Americans (free and slave) had similar inequality for the thirteen colonies as a whole, as did all Southern households (free and slave). Yet, much of that inequality was driven by the unpleasant societal fact that slave households had much less income than did the average free household. Within any American colonial region, free citizens had more equal incomes than do today's Americans, especially within New England or, surprisingly, the South.

Free American colonists also had much more equal income distributions than did west Europeans at that time. So suggests Table 7's comparison with England and the Netherlands in the eighteenth and early nineteenth centuries. The average Gini for the four northwest European observations reported there is 0.572, 0.116 higher than the American colonies, and 0.224 higher than New England. A future paper will elaborate on early American income distribution, and confirm that there was hardly any other place on the planet that had a more egalitarian income distribution.<sup>xl</sup>

The finding of far greater American equality than elsewhere (especially among free citizens) brings us to a question pondered by European visitors at the time: If so many colonists seem to have been relatively well off, just which ones were better off than their counterparts in Europe – and how

would we define their counterparts? Figure 1 offers a way to make that assessment. The figure is limited to two countries and two dates, and it compares incomes using the silver exchange rate. 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 in logarithms – in logs, because the top incomes soar so high in absolute values that the diagram delivers more information by curbing that top-income dominance. It would appear that an American colonist of any income rank had a higher income than his or her English counterpart of the same rank until we reach the top 2 percent.<sup>xli</sup> At that point, the English advantage appears over even the Charleston elite. The top group incomes so dominated England that its national product per capita was still near that of America, and average English incomes probably surpassed the American average in the American depression of 1775-1790. For 1774 itself, however, if one shares the literature’s conclusion that Southern slave field hands retained and consumed about 41 percent of their product, and a little more in non-farm activities, it turns out that even American slaves were not at the bottom of the Anglo-American consumption ladder, although such comparisons fail to deduct for their loss of freedom, their longer hours worked, and their harsher working conditions.

Future research also needs to pursue the impact of relative purchasing power on such 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 this 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 7 imply, and this might also have been true of the Middle Colonies with its cheap grains (exported to England 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 England wasn’t so much worse off relative to Southerners as our present figures suggest, and perhaps workers in the Middle Atlantic were even better off compared with English workers than our present figures suggest. These “real inequality” dimensions need to be explored further,<sup>xlii</sup> but we certainly do not expect them to overturn the sharp inequality contrasts shown here.

## V. Summary and Agenda

The only way to push back the quantitative frontiers of the history of inequality and living standards is to adapt to the data environments of the deep past. In the archeological extreme, that means accepting skeletal remains and DNA as our sole form of evidence. Even a journey back to the eighteenth century must accept an eclectic array of incomplete evidence. One of the most underexploited frontiers for the early modern era is occupation counting, an approach which allows us to assemble aggregate incomes and their distribution among the social classes. Working on that frontier, we have uncovered suggestive findings about early American growth and inequality. It appears that the colonists had far higher incomes in 1774 than previously thought, and almost as much higher in 1800. Between 1774 and 1800 American incomes rose only very modestly in real per capita terms, so that any rapid growth after 1790 appears to have barely made up for a very steep wartime decline. In addition, we find that free American colonists had much more equal incomes than did households in England and Wales. The colonists also had greater purchasing power than their English counterparts over all of the income ranks except at the top 2 percent.

Inter-regional inequality also demands further scrutiny. Our results suggest that Southern per capita incomes were far above other colonies in 1774, and that poor whites were much less common than in other colonies. It appears that the colonial South lacked the large numbers of poor whites that could be counted in Boston, Philadelphia, New York and lesser coastal towns. In short, our results suggest that mass poverty did not spread among the Southern white population until the nineteenth century. Surely the late colonial income distribution in the South cries out most loudly for further research of the sort already done for its Chesapeake sub-region.<sup>xliii</sup>

The research agenda for the future seems clear enough: more data, better ways of using the data, and new interpretations. Fortunately, the archives and the internet keep adding to the supply of usable primary data on occupations, earnings, and property income. We hope to have blazed the trail for additional work on early American incomes and by leaving behind an open-source data description and some provocative initial results.



**Table 1. Two Early American Attempts at Social Tables****(A.) A 1751 Social Table by Governor James Glen of South Carolina**

<b>Class</b>	<b>Class Numbers</b>	<b>Class Expenditure (£)</b>	<b>Average £ / year</b>	<b>Comments</b>
Top	5,000	182,500	36.50	(a.)
Upper Middle	5,000	91,250	18.21	(b.)
Lower Middle	10,000	91,250	9.13	(c.)
Bottom	5,000	35,000	7.00	(d.)
Total	25,000	400,000	16.00	

(a.) Those "who have plenty of the good things in Life, and spend at the rate of two Shillings per day."

(b.) Those "who have some of the Conveniencys of Life, and spend at the rate of one Shilling per day."

(c.) Those "who have the Necessarys of Life, and spend at the rate of Six pence per day."

(d.) Those "who have a bare subsistence and spend about a Groat [4 pennies] per day."

**(B.) Samuel Blodget's Conjectural Social Table for 1805**

" A Table improved on the plan of Sir William Petty, and other statistical writers, for a classing, and a valuation or tarif of exchange for the white people of the United States, for 1805".

<b>Class</b>	<b>Active persons, male and female (1000s)</b>	<b>Total Persons U. State (1000s)</b>	<b>Exchange value of each person, in \$</b>	<b>Total \$, millions [He rounded to millions]</b>
Slaves to planters	300	800	200	160
Ditto, variously employed	100	200	300	60
Free planters and agriculturalists	1200	4800	400	1920
Mechanical artisans	100	500	500	250
Fishermen	6	30	900	27
Seamen, &c.	110	400	700	280
Professionals and others not enumerated	50	250	500	125
	1866	6180		2822

These appear to be imagined asset values, including a capitalization of human earning power. See his extravagant comment on fishermen and seamen, on the same page. Source: Blodget (1806, p. 89).

Note on the Glen estimates in Table 1:

The population of the Lower South (North Carolina, South Carolina, Georgia) was 148,665 in 1751 (Mancall, Rosenbloom, Weiss (2008a: Table A2, p. 41).

It appears that Glen was talking about free citizens, excluding slaves. With the first census in 1790, South Carolina had only 29% of the total of the three states, but the figure would have been somewhat bigger forty years earlier, perhaps 40 or 50%. If it were the lower number, then there would have been something like 60,000 in South Carolina. Thus, Glen's 25,000 cannot include slaves. Finally, Glen is talking about a relatively small share of the Lower South, and it was much more unequal than the small holder areas in North Carolina and Georgia. In any case, Jamaica was *much* more unequal than South Carolina at that time (Burnard 2001).

Glen's comments can be found in his "An attempt towards an Estimate of the Value of So. Carolina," March 1750/51, enclosure in Governor James Glen, at Charleston, to Board of Trade, June 24, 1761. Cited by John McCusker (2006: 5-633).

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**Table 2. 1774 Occupation Lists**

<u>Places</u>	<u>Data sources and adjustments for occupational shares</u>
<b>New England</b> (CT, MA, ME, NH, RI, VT)	
• Big city = Boston	• Boston 1780 shares from J.T. Main, backed by Boston 1790 shares from Price (1974) and the downloaded Boston 1800 occupational directory
• 19 lesser cities & rural	• Use the 1771 Massachusetts-Maine tax returns to estimate the shares of land-owning farmers, non-land-owning farmers, and others with positive vs. zero realty. Then for the towns, apply the non-farm, non-big city occupation mix from Lancaster PA 1800 to lesser cities in 1774 and the Chester County PA rural occupation mix of non-farmers in 1800 to the rest of New England 1774. (Later add the estimated number of households missed by the 1771 tax lists altogether.)
<b>Middle Colonies</b> (NY, NJ, PA, DE)	
• Big-city = Phila., NYC	• Philadelphia 1772 occupations from assessment lists supplied by Billie Gordon Smith*
• 3 NJ lesser cities**	• Lancaster Borough 1773
• Rural	• Chester County PA 1800, 9 rural townships
<b>South</b> (GA, MD, NC, SC, VA)	
• Big City = Charleston***	• Charleston 1790 directory, downloaded. Re-weighted away from slave holders and land owners, based on assessments for 3 North Carolina counties, 1779-1782
• Rural	• Start with Alice Hanson Jones's rural w weights from 4 colonies (MD, VA, NC, SC), and apply the same adjustment as for Charleston based on 3 NC counties

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Notes to Table 2:

For further details on the derivation of these occupational shares, see the worksheets on LW weighting in the "Aggreg Property 1774" files.

\* See Smith (1984, 1990), supported by Price (1974) on Philadelphia 1780-1783. For New York City, we could have drawn on the 1789 tax lists with occupations, as supplied by Herbert Klein. We have not yet done so, and have instead cloned New York 1774 from Philadelphia 1772.

\*\* To the three New Jersey lesser cities could be added several from New York, extrapolating back from the 1790 census. This laborious task has not yet been done, since the 1790 New York returns did not provide total populations, but only sub-group details that cannot be scanned electronically. Pennsylvania and Delaware lacked lesser cities with populations that would have exceeded 2,500 in 1774, to judge from more aggregate growth rates.

\*\*\* The earliest Baltimore occupational directory available is for 1799, and the earliest for Norfolk VA is for 1801. Both are posted on the [gpih.ucdavis.edu](http://gpih.ucdavis.edu) site.

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**Table 3. 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>				
Free own-labor incomes	31.09	29.13	62.81	123.03
Slave retained earnings	0.13	0.78	12.18	13.09
Gross property incomes	4.21	13.67	35.74	53.62
Gross total income	35.43	43.58	110.72	189.74
<b><i>Circa 1800</i></b>				
Free own-labor incomes	73.65	84.20	87.77	245.62
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
<i>Relevant denominators</i>				
Free labor force 1774	185,999	156,875	207,438	550,312
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 3:**

The estimates exclude Native Americans.

John McCusker's (2001) price deflators = 97 for 1774, 151 for 1800 if 1860 = 100, or 93.3 for 1774 and 145.2 for 1800 if 1840 = 100.

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

The gross property incomes for 1800 are based on the "half-trout" assumptions about Southern underassessment in 1798 (see text).

**Table 4. A Culled Set of Income Estimates 1774 and 1800,  
in current \$ and 1840 \$ (millions)**

	<b>1774</b>	<b>1774</b>	<b>1800</b>	<b>1800</b>	
	current	1840	current	1840	
	\$m	\$m	\$m	\$m	<b>Source</b>
<b>US (orig 13)</b>	142.200	152.460			GDP: McCusker (2000)
	131.653	141.561			Gross inc: Jones (1980)
	<b>189.737</b>	<b>203.362</b>	<b>444.119</b>	<b>305.867</b>	Gross inc: LW (2011)
	<b>161.222</b>	<b>172.799</b>	<b>377.750</b>	<b>260.159</b>	Net inc: LW (2011)
<b>US (all)</b>			508.650	350.310	GDP: McCusker (2000)
	150.283	161.593	515.524	355.536	GDP: Mancall & Weiss (1999)
	135-157	145-169			GDP: Gallman (1972)
	134.813	144.959	500.138	344.925	GDP: Goldin & Lewis (1980)
	132.603	142.583			Narrow GDP: Weiss (1992)
			430.888	297.166	Berry (1988)
			446.277	307.779	David (1996)
			472.000	325.520	Trescott (1960)
			510.410	351.522	GDP: Mancall <i>et al.</i> (2003)
			<b>470.706</b>	<b>324.178</b>	Gross inc: LW (2011)
		<b>399.757</b>	<b>275.315</b>	Net inc: LW (2011)	
<b>New Eng.</b>	35.539	38.214			Income: Jones (1980)
	<b>35.434</b>	<b>37.979</b>	<b>95.112</b>	<b>65.504</b>	Gross inc.: LW (2011)
<b>Middle Atl.</b>	36.514	39.262			Income: Jones (1980)
	<b>43.579</b>	<b>46.709</b>	<b>134.128</b>	<b>92.374</b>	Gross inc.: LW (2011)
<b>South Atl.</b>	59.154	63.606			Income: Jones (1980)
	<b>110.723</b>	<b>118.675</b>	<b>214.880</b>	<b>147.989</b>	Gross inc.: LW (2011)
<b>Lower</b>	22.002	23.658	93.454	64.362	GDP: Mancall <i>et al.</i> (2003)
<b>South Atl.</b>			<b>94.124</b>	<b>64.824</b>	Gross inc: LW (2011)

Notes: (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 3 and 6.

(4) The LW estimates for 1800 are the "baseline" estimates. For our "alternative" estimates, see Table 5.

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

**Table 5. 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	4.210	13.670	35.740	53.620
Net personal property incomes	2.595	6.723	15.787	25.105
Total gross personal incomes	35.434	43.579	110.723	189.737
Total net personal incomes	33.819	36.632	90.770	161.222
<b><u>Estimated using 8% net rate of return on all NIPA-type assets and slaves</u></b>				
Gross personal property incomes	5.075	15.911	41.003	61.989
Net personal property incomes	3.460	8.963	21.049	33.473
Total gross personal incomes	36.299	45.820	115.986	198.105
Total net personal incomes	34.684	38.873	96.032	169.590
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

Source: The "Aggreg Property ... 1774", "1798-1800 property totals", and "own labor incomes 1800" files, <http://gpih.ucdavis.edu>.

**Table 6. Real Income per Capita 1774-1840**

	<b>New England</b>		<b>Middle Atlantic</b>		<b>South Atlantic</b>		<b>All three regions</b>
<i>Gross personal income per capita (in 1840 prices)</i>							
1774	57.41	(67)	76.11	(89)	107.77	(126)	85.68
baseline 1800	53.13	(85)	63.07	(101)	66.59	(107)	62.17
alternative 1800	56.25	(85)	67.67	(102)	71.40	(107)	66.49
Weiss-Easterlin 1840	129.28	(118)	120.19	(109)	84.84	(77)	109.89
<i>Per annum growth 1774-1800 (%)</i>							
baseline	-0.30		-0.72		-1.83		-1.23
alternative	-0.08		-0.45		-1.57		-0.97
<i>Per annum growth 1800-1840 (%)</i>							
baseline	2.25		1.63		0.61		1.43
alternative	2.10		1.45		0.43		1.26
<i>Per annum growth 1774-1840 (%)</i>							
	1.24		0.69		-0.36		0.38

**Notes:** The figures in parentheses are percentages of the all-three-regions average. The baseline estimates for 1774 and 1800 are the ones using a 6% net rate of return on assets, as for 1774, whereas the alternative estimates for 1800 use 8%. As noted in the text, both 1800 estimates omit farm operators' pure profits for 1800. The 1840 estimates start with Weiss's (1992, Table 1.2, page 27) national estimates, and derive regional relatives from the state-level relatives in Easterlin (1960, pp. 87-98). The three-region totals are derived from the regional averages. The South Atlantic excludes DE and FL; the Middle Atlantic includes DE, MD, and DC in 1800. The price deflator is, once again, the McCusker composition price index.

**Table 7. 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
Year:	1774	1774	1774	1774	1774	1774
Gini coeff	0.471	0.436	0.348	0.415	0.508	0.383
<i>Income shares in % of total income--</i>						
Top 1% of HHs:	9.3	8.4	3.9	5.8	9.4	7.0
Top 5%:	25.6	24.3	11.5	20.3	29.3	23.1
Top 10%:	36.1	35.4	19.8	30.4	40.7	35.0
Top 20%:	50.2	47.5	35.2	46.5	54.4	49.9
Next 40%:	38.5	38.6	52.8	39.2	35.4	29.7
Bottom 40%:	11.3	13.9	12.0	14.3	10.2	20.4
<i>Household income levels In \$ (at \$4.44/£ sterling) --</i>						
Mean:	378	448	273	322	461	705
Median:	282	383	368	308	330	535
Top 1% of HHs:	3064	3782	1063	1903	4348	4948
Top 5%:	1668	2177	626	1343	2698	3264
Top 10%:	1011	1586	541	924	1876	2467
Top 20%:	918	1064	452	767	1254	1760
Next 40%:	380	432	375	540	407	632
Bottom 40%:	105	155	82	118	118	251
<i>Western Europe, as a comparison group --</i>						
Region:	England & Wales	England & Wales		Holland	Netherlands	
(All households)						
Year:	1759	1802		1732	1808	
Gini coeff	0.522	0.593		0.610	0.563	
<i>Income shares in % of total income--</i>						
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	
<i>Household income levels --</i>						
Mean:	£	43.4	90.6*	fl.	67.8	319.3
Median:	£	25.0	55.0	fl.	35.0	150.0

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

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(Note: Includes some references used in the Excel files posted at <http://gpih.ucdavis.edu>, though not cited in the text and tables here.)

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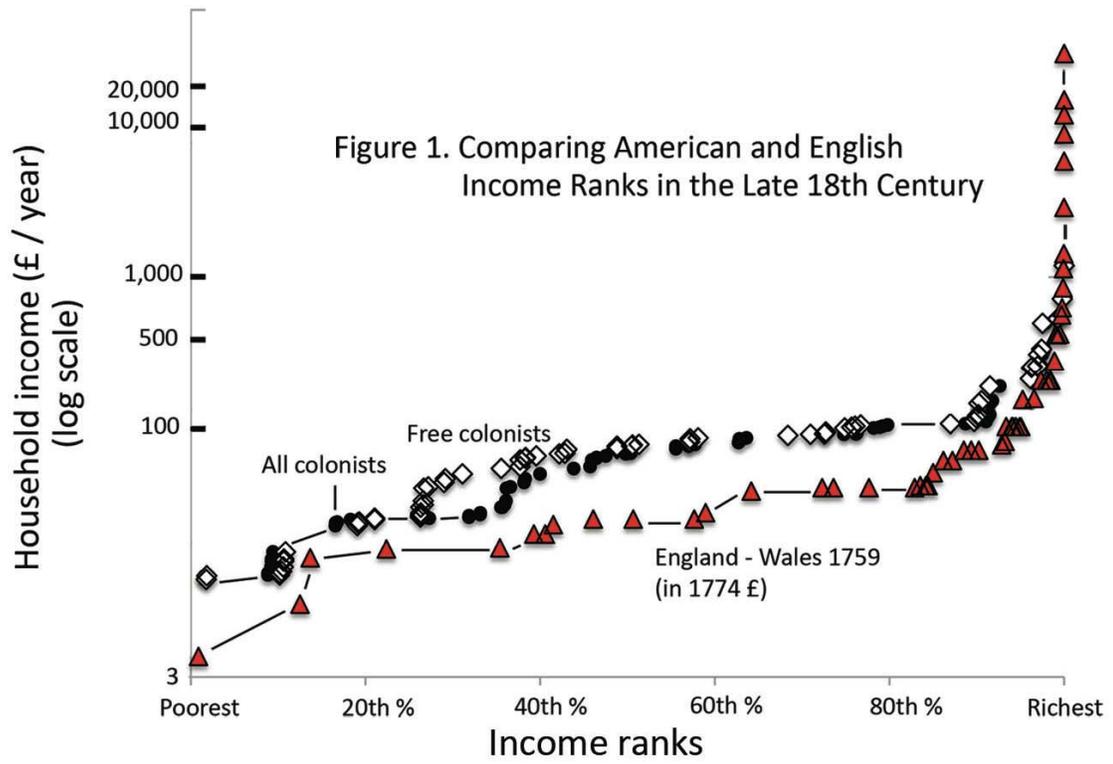
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## Endnotes

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<sup>i</sup> The debate over growth rates from 1790 or 1800 to 1840 is well represented by David (1967, 1996), Gallman (1992, 1999), and Weiss (1992, 1993, 1994).

<sup>ii</sup> See, for example, Egnal (1975), Kulikoff (1986), McCusker and Menard (1985), Carr *et al.* (1991), Mancall and Weiss (1999), and McCusker (2000), and the sources cited there.

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

<sup>iv</sup> 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-eighteenth-century England, see Hoppit (1996).

<sup>v</sup> The one-sixth assumption is supported by the somewhat distant 1820 census, the earliest census to give an age distribution for free blacks. As of 1820, 24.3 percent of free blacks consisted of likely household heads, using the same assumptions as for free whites. We believe that the headship rate was probably lower in 1774, both because children were a higher population share of whites and slaves and because fewer free black adults would have been able to establish separate households then. Hence, we choose 16.7 percent, or 1/6, as the headship rate for 1774. For an elaboration, see the file “1774 occupations by region” at <http://gpih.ucdavis.edu>.

<sup>vi</sup> Lee Soltow (1989) cites Oliver Wolcott’s 1798 survey team as finding that the assessed valuations tends to be 84.5 percent of true market value of real estate in a large special study of Connecticut. We accordingly will multiply our estimates of property income in 1798-1800 by (1/0.845) for New England and the Middle Colonies.

<sup>vii</sup> See the downloadable file “wage data c1774” in the early America folder at <http://gpih.ucdavis.edu>.

<sup>viii</sup> It might be argued that we should perform some sensitivity analysis in a later draft by scaling down their workdays from 313 to 227, as Main (1965) suggested for some New England farm laborers, to see how much difference it would make to our income estimates. But such an experiment would only measure income from hired labor, not unpaid farm labor on their small plot or spinning, weaving and other manufactures work at home, some of which was sold (Tryon 1917).

<sup>ix</sup> See the file “Own-labor incomes 1774” in the early America folder at <http://gpih.ucdavis.edu>.

<sup>x</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>xi</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 [gpih.ucdavis.edu](http://gpih.ucdavis.edu) for the details.

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

<sup>xiii</sup> There are only meager data on 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>xiv</sup> We have also considered an alternative measure of gross income that omits the depreciation rates on producer perishables and crops, to allow for the possibility that the literature’s estimates of profits may already have imputed these two kinds of depreciation as part of profits. Omitting income from perishables and unsold crops would, of course, yield an aggregate personal income that is somewhere between the gross and net income figures displayed in Table 5.

<sup>xv</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>xvi</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>xvii</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>xviii</sup> See the Excel file “1798-1800 property totals” at the same internet site.

<sup>xix</sup> The *ad valorem* tax rate as a share of the Fogel-Engerman slave values 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 New Jersey, Kentucky, and Tennessee.

<sup>xx</sup> The algebra of adjustment to reported South Atlantic realty is as follow. We observe the ratio of total assessed values, South to North ( $A_s/A_n$ ) = 0.381/0.619. Under the “full trout” assumption, the regional ratio of true market values ( $R_s/R_n$ ) = 0.577/0.423. Within the regions, the relationships of assessed to market value are  $A_s = (1-U_s) R_s$ , or  $A_n = (1-U_n) R_n$ , where the  $U$ ’s are the shares of underassessment. The 1799 market value study suggested that  $U_n = 0.155$  in the North. These values imply that the market value of Southern real estate  $R_s = 2.6226$  times  $A_s$ , so that the underassessment rate  $U_s = 0.619$ . (Just by coincidence, this matches the Northern share of assessments, given above.)

When Tables 3 and 4 introduce estimates of nominal income based on the “half-trout” assumption, one can add \$9.547 million to get the result obtainable from the “full-trout” assumption for 1800, or subtract the same amount for the “no-trout” result.

<sup>xxi</sup> The inferences about growth to 1840 are based on our calculation of regional incomes for 1840. Starting from Tom Weiss’s broad-based measure of GDP in 1840, the measure used in

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*Historical Statistics of the United States*, we derived regional GDPs by applying each region's ratio of output per capita to the three-region output per capita in 1840, where the "output" is that calculated by Richard Easterlin (1960).

<sup>xxii</sup> In this passage, "wealth" means household net worth. See the [gpih.ucdavis.edu](http://gpih.ucdavis.edu) file on total property incomes for 1774.

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

<sup>xxiv</sup> See the interest rate literature cited in footnote 12 above.

<sup>xxv</sup> The markdown from gross to net personal income is not so different in these estimates from the comparable markdowns in the US economy today. There are various accounting ratios available for comparison with the 84.6% share of net personal income in gross personal income in 1774 (Table 5). As of 2009, that same concept would be 78.3% of gross personal income if one omitted personal transfer receipts, or 87.3% if the net transfers were included in the numerator.

<sup>xxvi</sup> In the terminology of Thomas Weiss and other scholars estimating aggregate American incomes before 1840, our gross income estimates therefore conform more closely to the "broad", rather than "narrow", definition of GDP.

<sup>xxvii</sup> Jones, *Wealth of a Nation to Be* (1980), p. 62. Robert Gallman and Tom Weiss have preferred her top ratio, 3.5 to one, and that is used in Table 4's display of her estimates.

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

<sup>xxix</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 variations on hired farm labor. For 1774, she implied 67 percent. See Simler (1990, p. 197, Table 3). Yet we cannot extrapolate from Chester County to the whole nation, and doing so would still leave this paragraph's other reasons for not trying to estimate farmers' pure profit share.

<sup>xxx</sup> Labor productivity growth was a bit faster, perhaps 0.58 percent per annum. The difference between this, and the per capita rate of 0.38, is explained by demography. The "relevant denominators" panel of Table 3 confirms that the labor (free and slave combined) participation rate fell from 0.337 in 1774 to 0.320 in 1800. The change was caused by a slight decline in the share of free males over 16 years of age in the North, and a slight decline in the population share of slaves, who were compelled to have much higher labor force participation rates.

<sup>xxxi</sup> As Douglas Irwin and Richard Sylla remind us in their introduction to their *Founding Choices: American Economic Policy in the 1790s*, growth is considered modern if per capita income growth reaches 1 percent per annum or more for long stretches of time (2011: p. 4).

<sup>xxxii</sup> See the excellent survey on growth estimates for early America by Sylla (2011: pp. 81-83).

<sup>xxxiii</sup> The recent empirical growth literature has come to call this a reversal of fortune, as in Acemoglu *et al.* (2002).

<sup>xxxiv</sup> See Kulikoff (2005).

<sup>xxxv</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>xxxvi</sup> See Shepherd and Walton (1976, especially Table 5 and the surrounding text). Mancall, Rosenbloom, and Weiss (2008, Table 1) estimate that the Lower South suffered an even

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greater reduction in its domestically produced exports, a drop of fully 67 percent, between 1770 and 1790.

<sup>xxxvii</sup> The estimates of gross population loss are from Jasanoff (2011, pp. 351-358), and the high-side estimate of the claims presented to the King is cited in Eardley-Wilmot (1815, reprinted 1972, Appendix VIII).

<sup>xxxviii</sup> See Rabushka (2008, pp. 796-824), and *Historical Statistics* (2006), Series Eg427, Eg428 contributed by John J. McCusker.

<sup>xxxix</sup> Atkinson *et al.* (2011), Table 5, p. 31 offer three Gini coefficients for the United States in 2006: 0.470, 0.493, and 0.519. We favor the 0.493 figure in the comparison with 1774, since it is with “good” data, but without capital gains.

<sup>xl</sup> This comparison will rely heavily on Milanovic *et al.* (2011).

<sup>xli</sup> Figure 1’s contrast may be slightly overstate the Americans’ income advantage, for at least two reasons. First, a true comparison for 1774 would have found the English better off than we could show with English social tables from 1759 (Massie, revised by Lindert and Williamson 1982) or 1801-1803 (Colquhoun, revised by Lindert and Williamson 1982). Second, personal income data miss the indirect taxes and current surpluses of government or para-statal enterprises. These would have been a higher share of NNP or GNP for the Mother Country than for the colonies. Thus one may read “top 2 percent” more loosely as “top few percent” here.

Our conclusion that the American working class was considerably better off than the English working class in 1774 is consistent with Lois Carr’s (2005) similar conclusion regarding Maryland’s ex-servant immigrants in the late eighteenth century.

<sup>xlii</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>. On the more general subtlety about class- and place-specific costs of living, see Hoffman *et al.* (2002).

<sup>xliii</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.