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“Social Tables for British North America 1774 and 1800”

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#### ABSTRACT

Building social tables in the tradition of Gregory King we tentatively quantify the level and inequality of American incomes before and after the Revolutionary War. Our tentative and revisable estimates suggest that the colonists had somewhat higher incomes in 1774 than previously thought. Between 1774 and 1800 American incomes failed to rise in real per capita terms, and may have declined slightly. Any rapid growth after 1790 may have just made up for a steep wartime decline. In terms of inequality, we find that free American colonists had much more equal incomes than did households in England and Wales, unlike today’s contrast. The colonists also had greater purchasing power than their English counterparts over all of the income ranks except in the top 2 percent of society.

#### CAVEATS

This draft is a preliminary and incomplete report on an open-source research project. Given the uncertainties of any inferences from still fragmentary historical data, we invite others to add additional data, procedural revisions, and alternative interpretations. As much as possible, the data are being posted on <http://gpih.ucdavis.edu> (click on the folder “America’s early social tables (in progress)”). Every sentence describing results is implicitly prefaced by the clause “As best we can infer from limited and fragile data, and until later revisions suggest otherwise, we suggest that ...” Later versions of this paper will add further material, especially on the 1798-1800 benchmark, and a later paper will add corresponding estimates for 1860 and 1870.

#### I. CONTINUING QUESTIONS ABOUT EARLY AMERICAN GROWTH AND INEQUALITY

American economic historians continue to 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. The need keeps arising whenever we try to cast back to this setting from later centuries, to project ahead to it from early colonial experience, or to view it 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 description that will apply to this paper as well as to his. David, Robert Gallman, Thomas Weiss, and others rightly centered their plausible conjectures on the division of the economy into large sectors, each with its own growth of labor force and of labor productivity. The debaters have been hampered by the lack of data on the occupational and output-sector mix of the labor force before 1840. The census did not help us 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 or planters or farm hands, it helps considerably to know what shares they represented, where they lived, and how fast their occupations changed.<sup>1</sup>

Another debate has raged over growth within the colonial era. Some have seen stagnant productivity, and Smithian growth of population and land area, without gains in average living standards. Others have posited considerable growth, some emphasizing seventh-century emergence from initial hardship and mortality, and others emphasizing gains across the middle of the eighteenth century. This debate, too, has 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>2</sup>

Our curiosity about American incomes around the time of the Revolution is enhanced by viewing American from across the Atlantic. Angus Maddison estimated that it was not until the 1870s that the United States caught up with the United Kingdom in real GDP per capita, though a considerable debate has revolved around the uncertainties of his and others' index numbers. It is natural to wonder how far behind the colonists could have been in income, given the strong net migration from the Mother Country in the eighteenth and early nineteenth centuries. Given the present resurgence of projects on The Great Divergence and on the history of European incomes, and the birth of the Angus Maddison Project, the time is especially ripe for adding data to the American side for comparisons with the new estimates for Europe.

We can now offer an initial harvest of new estimates based on more archival data than were available to earlier researchers in this area. Such an offering has to be based on 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 would rightly resist accepting at face value some new numbers that they know to be 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>.

Our tentative and revisable “controlled conjectures” confirm some popular hunches about growth and inequality in Early America, yet contradict others, and introduce still others for the first time. As of this writing, we have a clearer view of colonial American inequality and how different classes compared in income 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 a subtler comparison of purchasing power parities. In terms of economic growth, we tentatively 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 near those that other scholars derived in completely different ways, the new numbers raise questions about what happened in between 1774 and 1800, first in the Revolutionary War era and then at the dawn of the Republic.

## II. HOW THE SAUSAGE IS MADE

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

Our approach starts from a renewed emphasis on counting people by occupations or social classes, and mustering clues 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 cost in current, or “nominal”, prices.

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>3</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. 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 data 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 American 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, starting two centuries later, the electronic revolution has made local enumerations from the late eighteenth century more accessible. While all records before 1774 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 U.S. 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 and in the Millennial Edition. 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 sell no labor.

To convert demographic groups into numbers in the labor force, we use the labor force participation rates for 1800 supplied by Thomas Weiss. While one would not glibly assume that the national labor force participation rate was unchanged between 1774 and 1800, 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.

(3.) Recorded occupations. Sketching the social make-up of the labor force requires detailed occupations 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.

A fresh start on the social structure of America on the eve of the Revolution is a set of local assessment lists and occupational directories, listed in Table 2. We have used these lists to create the following occupational groups for the free population:

- Group 1 = Officials, titled, professions
- Group 2 = Merchant & shopkeepers, big city only
- Group 3 = Merchant & 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

Relative to Alice Hanson Jones, 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, though within Charleston one gets the same occupational patterns by starting with Alice Hanson Jones's weights for a sample drawn from four 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 weight to that of Charleston, absent any separate sampling of occupations from Baltimore before 1799. This would give greater weight to the occupational and property patterns Charleston at the expense of the countryside. Such an adjustment would give a slightly higher average income, and higher inequality, for the South as a whole.

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 starting with 1798. One possibility for the rural South might be to use the 1800 rural occupational structure as a bound for the 1774 structure, aided by the shift in the urban population share. Yet even for 1798 and beyond, we have only the 1798 Wolcott wealth tax summaries for the upper South (DE, MD, VA, KY, TN), some good 1798 returns for rural Maryland supplied by Carole Shammas, and a separate 1798 return for Burke County Georgia.

(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 have 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: Needed for Measuring Inequality, though not for Aggregate Income. The other population count to be quantified is the numbers of household heads in each occupational category. We could avoid measuring household headship for the purposes of measuring aggregate national product, which depends only on who is in the labor force and their average incomes.

To measure inequality, however, we need to focus on households as income recipient units, both for practical empirical reasons and for theoretical reasons. Empirically, we should conform if possible to the practice followed by other countries, in order to 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) has emphasized the superiority of the household focus on theoretical grounds. Caring about economic inequality means caring about how unequally people consume resources over their lives. Even if data constraints force us to study annual inequality rather than life-cycle inequality, Kuznets pleaded for measuring annual household 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 measure inequality among individual earners.

Combining theoretical objectives with practical empirical constraints, we pursue two kinds of measures of income inequality, both of them household-focused. The first is simply that conventional measurement of the inequality of total household incomes, for more fruitful

comparison with existing estimates from other times and places. The second is a measure of the inequality of household income per labor force member in the household. This italicized denominator represents a retreat from the desired measure of overall household size. We lack data on overall household size that are separated by income class or occupational size for any large region. Accordingly, this second inequality measure carries the implicit assumption that all occupations had similar numbers of non-labor-force dependents per labor force participant.

There is a clear principle to be followed when counting the total numbers of households, or of household heads: Selectivity is to be avoided. We must start from a comprehensive count of all households based on censuses, in preference over other sources that can be seriously selective, such as probate populations, tax lists, or directories of occupied individuals.

Since comprehensive population censuses usually do not count households, some assumptions must be used to decide which demographic parts of the population are in fact household heads. Fortunately, historians of early America have grappled with this issue. Following the lead of Billy Gordon Smith and by the late Lucy Simler in particular, we estimate the number of household heads from population data from c1774 and c1800 using the following assumptions:

- (1) All free white male adults, 21-up, were household heads, subject to (4) below.
- (2) 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.
- (4) The number of free white male adults who were not in fact household heads is matched by the number of free white female adults, 18-up, who were indeed household heads despite not being included in (2) above. That is, we assume that two errors offset each other when using the white adult 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 number of household heads that are missed by the listed occupation accounts. Large numbers of household heads were omitted from the tax lists



and the directories. While we leave the actual counts to the posted files that develop our estimates, The business directories and the taxes that originated in the colonial era missed more than 30 percent of households, threatening underestimation of total income and also threatening to bias its inequality. Less ominously, the newer taxes 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 assessments.<sup>4</sup>

Still, for the older taxes and for the business directories, especially those from the colonial era, we have found significant omissions.

Which groups were more heavily omitted than others? The literature has advanced the plausible intuition that the main omitted groups were the tax-excused poor, whose names could be safely omitted from tax lists or business lists. Yet we also have strands of evidence that many of the middling and rich groups may have been omitted, or at least that their wealth was under-assessed.

Helped by partial clues from the tax assessments, we divide the household heads of no recorded occupation into three groups. Picking up after those with listed occupations are grouped into Groups 1-6 (see the files for definitions), we have

- 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 daunting tasks regarding those who were in the labor force (LF) according to the combination of the censuses and the Weiss estimates of labor force participation rates, yet who were not 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 = LF for this group, minus free white HHs other than the widows and other females in Group 8
- 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
- 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 of labor force members were almost surely paid only unskilled wage rates, while others could have been spread over a broader set of occupational classes. Our income estimations make these assumptions within each place:

- Groups 10, 11, 14, and 15 (free non-HHs ages 10-15) are allocated to Groups 6A and 6B, menial or unskilled, by sex.
- Groups 12, 13, 16, and 17 (free non-HHs 16-up) are allocated across Groups 2-6 in the same region-specific percentages as Groups 2-6.
- Group 18's Maryland servants are allocated to the menial labor Groups 6A for males and 6B for females.
- For Group 19, slaves ages 10-up, we use other scholars' rough estimates of the amount of their earnings they were allowed to retain. Using indirect census clues, we further assume that 35 percent of slaves over the age of 10 were household heads.

The calculations up to this point affect both our estimation of aggregate national income and our estimation of income inequality. For inequality purposes, following Kuznets, we must take the further difficult step of deciding whose households these non-HH members of the labor force lived with. On this issue the data are almost non-existent. We make the following assumptions about the non-head earners "imported" or "taken into" the households of others

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

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

(3) 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.

(4) Within each group defined by region and urban/rural, we assume that the average earning power of each non-household-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.

The resulting allocations can certainly be challenged. We emphasize one point about data sources, before turning to our provisional solution to the difficulty of deciding who shared household incomes with whom. The point about data sources is this: For each place defined by region and by urban/rural, the aggregate imports of non-household-heads are driven by the census, the labor force participation rates, and by the household headship rates. The allocation of non-household-heads to others' households by place is not yet derived from micro-studies about households' sharing of earnings, because there are too few such studies. Nor are the allocations simply assumed, except for the key middle-of-the-road assumption listed as number (4) here.

(6.) Labor earnings by occupation, circa 1774. We are able to place rough incomes per year on several occupations in each regions, thanks to the enormous archival gleanings of Jackson Turner Main's *The Social Structure of Revolutionary America* (1965) and a few other sources.<sup>5</sup> The eclectic collection of numbers from newspapers and account books have to be used with care. Some are in the depreciated local colonial currency, whereas others are in pounds sterling. Fortunately, Main in particular was 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 state job he or she nonetheless filled in with other productive work, typically at home. On this hunch we have calculated as though our daily or monthly workers performed productive work of some kind for 313 days a year. This might overstate formal-sector incomes somewhat. Yet in a later draft we will perform the exercise of scaling their

work days down to about 220, as Main suggested for some New England farm workers, to see how much difference that would make. To repeat, many of the wage data are already in the appropriate annual form, especially for white-collar professions.

We enlarged the concept of labor earnings to include farm operators' profits, estimated by J.T. Main, plus slaves' retained share of what they earned. This labor income amalgam we have called "own-labor incomes".<sup>6</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>7</sup> A key advantage of her data set is that it identifies 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>8</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 great weighting of the poorer households was achieved by introducing the new data on occupational structure describe earlier in this section. As it turns out, we come up with an even greater wealth markdown than did her w\*B estimates.

Wealth is not property income or total income, however. How could one adjust to get property incomes? Jones confined her income-measuring efforts to conjecturing roughly about aggregate capital-income ratios.<sup>9</sup> We have followed a different route. Our reading of the few clues on rates of return suggests that assets may have earned a net rate of return of 6 percent per annum. 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. 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 six percent) for producers' perishables and crops. For these last two perishables, in other words, the wealth at a moment in time equals the gross income that such an inventory would yield within a year.

(8.) Combining own-labor income with property incomes. Here we reap a main advantage of our having invested such much effort in gathering occupations. Since own-labor incomes and property incomes are both arranged by occupations, we can combine the two to get total incomes. For the largest occupational group, namely farmers, we can even exploit some of the size distribution of property income for each region, 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 the whole economy. Our calculations are for what NIPA accountants would call the total private income of the household sector. The government sector's contribution consisted only of the wages and salaries of government employees and military personnel, with no government corporations. Nor do we include the retained earnings of private corporations, since there were only eight corporations by the end of the century. The same assumptions will be made for 1800, except that we must reckon in government proceeds from substantial land sales. When we turn in a later paper to similar accounts for 1860 and 1870, the non-household sector will take a significant share of national income.

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

After 1774, the next realistic benchmark date for appraising national income is 1800, a census year preceded by America's first direct tax, the one-off tax on real estate and slaves in 1798. One might immediately fear data extracted by the tax collectors from a new nation that had shed its royal government. To that fear one might add the strong evidence that properties were vastly under-assessed in the late colonial era.<sup>10</sup> Yet all indications are that the repeated assessment process under-assessed market values by only 15.5 percent, a figure based on a side study done in Connecticut in that same year.<sup>11</sup> We have adjusted our 1800 property estimates for this degree of underassessment, and for the 7 percent rise in asset returns from 1798 to 1800 suggested by Blodget's fragmentary data. A greater leap of faith was that we had to use the same regional ratios of (total property/ realty plus slave values) obtained from back in 1774, because the giant 1798 tax covered only real estate and slaves.

Generally, our procedures for assembling total income in 1800 are the same ones we applied to 1774. There is one other important difference, however, on the property side. 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 disadvantage of the 1798 returns, however, is that they gave no data on occupation. This means that we cannot discuss the distribution of total income, though we still know the separate distributions of own-labor incomes and of property incomes, and the aggregate value of total incomes for 1800.

### III. PROVISIONAL CLUES ABOUT GROWTH BETWEEN 1774 AND 1800

This draft will not address inequality change over the last quarter of the eighteenth century, due to the just-mentioned lack of a link of property to occupations for our 1798-1800 benchmark era. Thus in the draft our findings about 1798-1800 are limited to the levels of household income and not to its distribution.

We can already shed new light, however, on the rate of growth in aggregate incomes between 1774 and 1800, thanks to separate data on labor incomes and property incomes. Armed with these estimates from the income side, we can compare our results with those of earlier scholars. Table 3 does so in some detail, even though it features only a “culled” part of the larger set of studies we have examined.

For the year 1800, our estimates happen to be in the middle of the set of existing estimates, even though our procedure is entirely different. A further advantage of our set of estimates is its regional detail for 1800, a feature not provided by other studies. In a later draft we may raise our estimates slightly, because for the 1800 benchmark, unlike 1774, we do not yet have estimates of farm operators’ pure residual profits, as distinct from their asset returns or the implicit value of their own physical labor.

For 1774, however, our estimate of about \$185 million in national (i.e. 13-colony) income is well above the estimates of Jones and McCusker, which hover around \$138 million. The difference arises in the South, for which our incomes are distinctly above those of Alice Hanson Jones. We have examined and debated this difference at length. Thus far we cannot find an error in the new estimates. We did not come up with a higher estimate on the property side – on the contrary, we used her data with different occupational weights and got 4 percent lower net worth than she did.

The difference must lie on the labor side, due to differences in occupational weights or wage rates. Our occupational weights start from hers and raise the weights given to less affluent and less probated whites. Again, that is a difference that should have made our estimates lower than hers, not higher. This brings us to the wage rates. What did Jones assume about labor income rates in the South? Actually, she did not make any assumption at all, but took a single leap of faith that we have already noted: By picking up some broad capital/output ratios from the aggregate growth literature she jumped from her impressive and reliable wealth estimates to a total income estimate that should stand or fall on an aggregate capital/output ratio. For the moment, we stand with our alternative estimates, based as they are on her own occupational weights, adjusted by some new data we have for three North Carolina counties.

Our working hypothesis about the colonial South is this: as of 1774 the South still had no large army of poor whites. We can see from a few local censuses and tax records that nearly all whites were assessed as having positive wealth. Could it be that the South simply lacked the poor whites that were showing up in such large numbers in Philadelphia and New York? Perhaps mass poverty really did not spread among the Southern white population until the nineteenth century. Surely colonial Southern income distribution is an area that cries out most loudly for further research of the sort already done for its Chesapeake sub-region.<sup>12</sup>

Our estimates imply that there was no net growth in real per capita income between 1774 and 1800. What looks like a great rise in real national product was in fact matched by population growth. What happened in between? The last quarter of the eighteenth century found the economy on a rickety swinging bridge, a sensation that also describes scholarly attempts to span that gap with numbers. Like the French and Russians and others, the Americans have great difficulty in piecing data series together between the two historical regimes. On the one hand, Richard Sylla and others have emphasized the strong growth experienced across the 1790s (due to the wisdom of Alexander Hamilton and other founding fathers?).<sup>13</sup> The more we come to accept their sanguine view of the 1790s, the more we may infer a disaster between 1774 and 1790. Perhaps, as Allan Kulikoff argues there was a greater depression attending the Revolutionary War than earlier scholars have appreciated.<sup>14</sup>

#### IV. COLONIAL INEQUALITY IN PERSPECTIVE

Finally, we turn to the other main object of the early income calculations: The judgment of colonial income inequality. Thanks to the data sets and procedures described above, we can view for incomes what Alice Hanson Jones could only view for wealth. Like her study, ours is also able to divide the income structure into three regions.

Table 4 displays the income inequality results. The quickest way to put the results into historical perspective is to compare them with income in the United States today, with its distribution that delivers about 20 percent of all household income to the top 1 percent of households, and has a gini coefficient of about 0.450. Americans seem to have had similar inequality for the thirteen colonies as a whole, and for the population of all Southern households. Yet much of that inequality clearly depended on the fact that slave households had much less income than the average white. Within any one region, colonial American whites were equal in their income than today's Americans, especially within New England or the South.

The American white colonists also appear to have been more equal in their incomes than Western Europeans. So suggests Table 4's comparison with England and the Netherlands in the eighteenth century and the early nineteenth.

The combination of greater inequality (at least among whites) and our previous glance at the issue of comparative growth brings us back to a question pondered by travelers at the time: If some of the colonists seem well off, just which ones are better off than their counterparts in Europe – and how would we define their counterparts? Figure 1 offers a way of channeling one's natural curiosity about such comparisons. By itself, it is a display limited to two countries and two dates. It also compares their incomes using the silver exchange rate. On the horizontal axis we view each society from its poorest to its richest, and on the vertical we see income levels 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. At face value, 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 top 2 percent of society. At that point the English advantage over even the Charleston elite that national product per capita was still as high in England as in America, a point that is hard to reveal because of the need to distort the aggregate income with a logarithmic display. Even slaves, if we believe the literature's data on the share of their productivity (about 40% of it) that they were allowed to consume, were not at the bottom of the consumption scales, though such calculations fail to deduct for the loss of freedom.



One needs to pursue the issue of relative purchasing power, however. As is widely recognized, the cost of living differences between classes and places depart from the comparisons implied by simple exchange rate conversion. This familiar point has a number of embodiments in this context that deserve notice and further investigation. One is that the cost of a standard consumer bundle may have been 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 so, then the nominal income contrasts might be blurred a bit. Perhaps New England was not as much poorer than the Southern colonies as Table 4's nominal figures imply. And perhaps New England was even better off than person in the same economic ranks back in the Mother Country. These "real inequality" dimensions need to be explored further.<sup>15</sup>

## V. SUMMARY AND AGENDA

As economic historians have long known, the only way to push back the quantitative frontiers of the history of living standards is to adapt to the data environments of the deeper and deeper past. In the archeological extreme, that means accepting skeletal remains and DNA as the gold standard of evidence. Even a journey back to the eighteenth century must accept an extremely eclectic array of clues. One of the frontiers most underexploited for the early modern era is the frontier of counting occupations, which sets the stage for measuring aggregate incomes and their distribution among the social classes. Working on that frontier we find suggestive results about early American growth and inequality. It appears that the colonists had somewhat higher incomes in 1774 than previously thought. Between 1774 and 1800 American incomes failed to rise in real per capita terms, since any rapid growth after 1790 may have just made up for a steep wartime decline. In terms of inequality, we find that free American colonists had much more equal incomes than did households in England and Wales, unlike today's contrast. The colonists also had greater purchasing power than their English counterparts over all of the income ranks except in the top 2 percent of society.

The research agenda from here seem clear enough: More data, alternative procedures, and new interpretations. We hope to have set the stage with an open-source data provision and some provocative initial results.

**Table 1. Two Early American Attempts at Social Tables**

<b>Table 1. Two Early American Attempts at Social Tables</b>				
(A.) The Social Table by Governor James Glen of South Carolina 1751				
	<b>Class</b>	<b>Class Expen-</b>	<b>Average</b>	
<b>Class</b>	<b>Numbers</b>	<b>diture (£)</b>	<b>£ / year</b>	<b>Comments</b>
<b>Top</b>	5,000	182,500	36.50	(a.)
<b>Upper Middle</b>	5,000	91,250	18.21	(b.)
<b>Lower Middle</b>	10,000	91,250	9.13	(c.)
<b>Bottom</b>	5,000	35,000	7.00	(d.)
<b>Total</b>	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 valuatiuon or tarif of exchange for the white people of the United States, for 1805".				
	Active per- sons, male and female	Total Persons U. State	Exchange value of each per- son, in \$	Total \$, millions [He rounded to millions]
<b>CLASSES</b>	(1000s)	(1000s)		
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 emunerated	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 to 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 (2008: 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, but the figure would have been somewhat bigger forty years earlier, perhaps 40 or 50%. If it were the lower number, then we are talking about some 60,000. 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 NC and Ga. Yet, Jamaica was much more unequal.

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).

**Table 2. 1774 Occupation Lists**

<u>Places</u>	<u>Data sources and adjustments for occupational shares</u>
<b>New England</b>	
• 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 MA 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 (NY, NJ, PA, DE)</b>	
• 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>	
• 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

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 has not yet been done, since the 1790 New York returns did not provide total populations, but only laborious 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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<b>Table 3. The Culled Set of Income Estimates 1774 and 1800, current and constant 1840 \$ (000)</b>						
	<b>1774</b>		<b>Source</b>	<b>1800</b>		<b>Source</b>
	<b>current</b>	<b>constant</b>		<b>current</b>	<b>constant</b>	
<b>Lower South</b>	22,002	23,658	GDP: Mancall et al. (forthcoming)	155,658	107,351	GDP: Mancall et al. (forthcoming)
<b>South (ex DE)</b>	59,154	63,606	Income: Jones (1980)			
	105,638	113,588	Income: Lindert-Williamson (2011)	213,972	147,568	Income: Lindert-Williamson (2011)
<b>Middle (incl DE)</b>	36,514	39,262	Income: Jones (1980)			
	43,580	46,860	Income: Lindert-Williamson (2011)	200,723	138,431	Income: Lindert-Williamson (2011)
<b>New England</b>	35,539	38,214	Income: Jones (1980)			
	35,446	38,114	Income: Lindert-Williamson (2011)	96,013	66,217	Income: Lindert-Williamson (2011)
<b>US (13 cols, 15 states)</b>	139,233	149,712	GDP: McCusker (2000), new	507,832	350,231	GDP: McCusker (2000), new
	142,200	152,902	GDP: McCusker (2000)			
	131,653	141,561	Income: Jones (1980)			
	137,695	148,058	Average above	507,832	350,231	Average above
	184,665	198,563	Income: Lindert and Williamson (2011)	510,708	352,215	Income: Lindert-Williamson (2011)
<b>US (all)</b>	150,283	161,593	GDP: Mancall & Weiss (1999)	515,524	355,536	GDP: Mancall & Weiss (1999)
	134,813-156,913	144,959-168,722	GDP: Gallman (1972)			
	134,813	144,959	GDP: Goldin & Lewis (1980)	500,138	344,925	GDP: Goldin & Lewis (1980)
	132,603	142,583	Narrow GDP: Weiss (1992)			
				430,888	297,166	GDP: Berry (1988)
				446,277	307,779	David (1996)
				472,000	325,520	Trescott (1960)
				509,679	351,505	GDP: Mancall et al. (forthcoming)
	143,653	154,464	Average above	479,084	330,405	Average above

**Note:** The 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 Alice Hanson Jones range, as recommended by Gallman and Weiss, and accepted by others.

**Sources:** for fuller citations, see reference list.

<b>Table 4. Inequality in the American Colonies 1774</b>						
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	0.456	0.437	0.348	0.415	0.466	0.383
<b><i>Income shares in % of total income--</i></b>						
Top 1% of HHs:	8.9	8.5	3.9	5.8	8.3	7.0
Top 5%:	24.9	24.3	11.5	20.3	28.0	23.1
Top 10%:	35.7	35.4	19.8	30.4	37.9	35.0
Top 20%:	48.9	47.5	35.2	46.5	52.2	49.9
Next 40%:	39.1	38.6	52.8	39.2	39.4	29.7
Bottom 40%:	12.0	13.8	12.0	14.3	12.4	20.4
<b><i>Income levels In \$ (at \$4.44/£ sterling) --</i></b>						
Mean:	407	447	273	322	549	705
Median:	343	383	368	308	449	535
Top 1% of HHs:	3625	3781	1063	1903	4556	4948
Top 5%:	2027	2175	626	1343	3081	3264
Top 10%:	1450	1585	541	924	2081	2467
Top 20%:	994	1063	452	767	1434	1760
Next 40%:	397	432	375	540	821	632
Bottom 40%:	122	155	82	118	171	251
<b><i>Western Europe, as a comparison group --</i></b>						
Region:		England & Wales	England & Wales		Holland	Netherlands
(All households)						
Year:		1759	1802		1732	1808
Gini coeff		0.522	0.593		0.610	0.563
<b><i>Income shares in % of total income--</i></b>						
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
<b><i>Income levels --</i></b>						
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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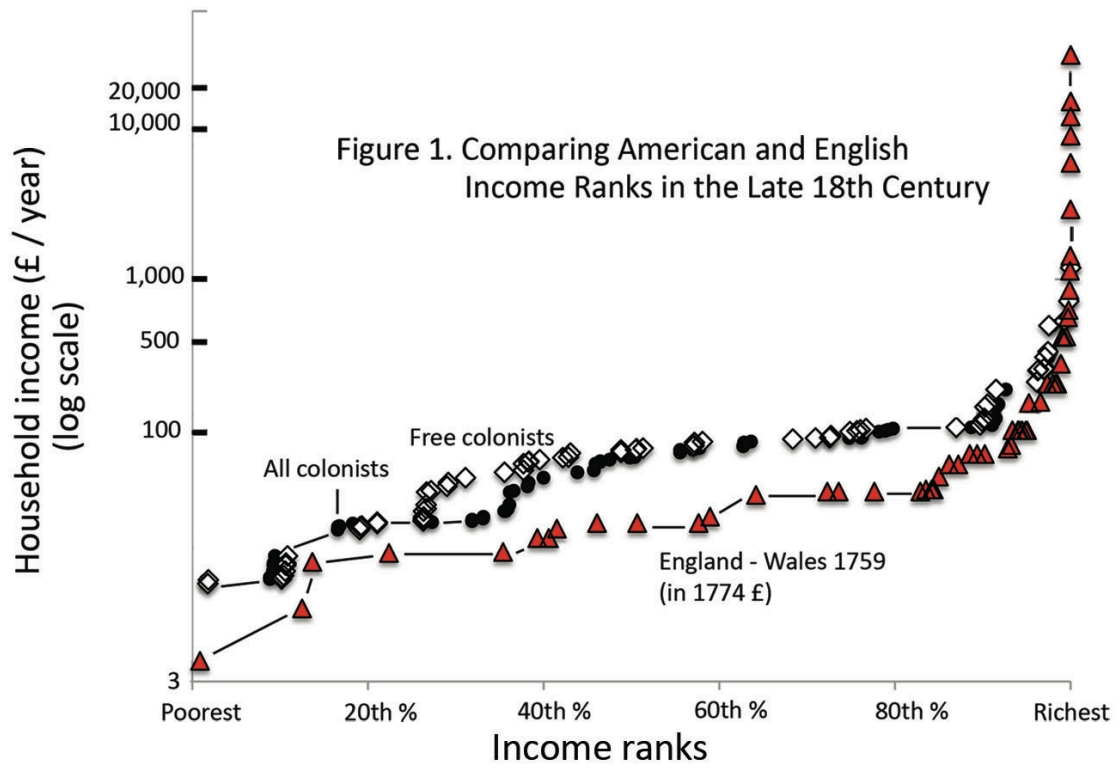
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## ENDNOTES

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<sup>1</sup> The debate over growth rates from 1790 or 1800 to 1840 is well represented by David (1967, 1996), Gallman (1992, 1999), Weiss (1992, 1993, 1994).

<sup>2</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>3</sup> See Lindert and Williamson (1982), and Milanovic, Lindert and Williamson (2011).

<sup>4</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).

<sup>5</sup> In addition to re-reading Main (1965), Carroll Wright (1885), U.S. Bureau of Labor Statistics (1928), and the cited articles by Donald Adams, one can download the file "wage data c1774" in the early America folder at <http://gpih.ucdavis.edu>.

<sup>6</sup> See the file "Own-labor incomes 1774" in the early America folder at <http://gpih.ucdavis.edu>.

<sup>7</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>8</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>9</sup> See in particular Jones (1980, pp. 61ff).

<sup>10</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 asset ownership, but not for the assessed values themselves.

<sup>11</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>12</sup> See, for example, Kulikoff (1986), Carr *et al.* (1991), and Walsh (2010). See also Robert Gallman's studies of Plaquemines County, North Carolina.

<sup>13</sup> See Sylla (2011).

<sup>14</sup> See Kulikoff (2005).

<sup>15</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).