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THE FARM-NONFARM WAGE GAP IN THE ANTEBELLUM UNITED STATES: EVIDENCE FROM THE 1850 AND 1860 CENSUSES OF SOCIAL STATISTICS

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ABSTRACT

Sectoral wage gaps for workers of comparable skill are central to issues in economic development and economic history. This paper presents new archival evidence on the farm-nonfarm wage gap for the United States just prior to the American Civil War. Measured at the level of local labor markets, the wage gaps are small and not very persistent over time. Aggregated to reflect the geographic distribution of farm and nonfarm labor, the gaps are larger than previously thought. I also show that investment in manufacturing capital between 1850 and 1860 responded to labor market inefficiencies indicated by the gaps: counties with relatively low farm wages experienced above-average investment.

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1.0 Introduction

Sectoral gaps in wages figure prominently in economic development and economic history. A gap is usually taken to be evidence of an inefficiency in resource allocation -- too much labor in the low-wage sector. Labor that does migrate between sectors, however, experiences a boost in earnings, which translates into a higher value of per capita income. Rapid growth in per capita incomes experienced by some late industrializing countries has been partly attributed to this process (Maddison 1987). The allegedly slow pace of economic growth during the British Industrial Revolution has been attributed to the absence of sectoral gaps (Crafts 1985), though others disagree (Williamson 1990).

This paper examines the farm-nonfarm wage gap in the United States during the late antebellum period. Past research on the antebellum wage gap has focussed on narrowly defined local labor markets (eg. Adams 1982, on the Brandywine region surrounding Philadelphia) or has used published census data (Williamson and Lindert 1980). My analysis is based on a sample drawn from the 1850 and 1860 manuscript censuses of social statistics, which provide far greater geographic coverage than previously available. I find that, within counties, a small wage gap existed in 1850 and 1860. Aggregate wage gaps, constructed to reflect the geographic patterns of the distribution of farm and nonfarm workers, are considerably larger than previously thought.

Finally, I show that the pattern of investment in manufacturing capital in the 1850s was systematically related to the size of the wage gap at the county level.

2.0 Data

One of the largest extant bodies of wage data for the antebellum period can be found in the 1850 and 1860 federal censuses of social statistics. As part of the enumeration effort in both years, census marshals were instructed to collect information on: the average monthly wage of farm laborers with board; the average daily wage of common nonfarm labor, with board; the average daily wage of common labor without board; the average daily wage of carpenters; the average weekly wage of female domestics, with board; and the average weekly cost of board to "laboring men".[1] At the time the census was compiled the data were aggregated to the state level, and state averages were reported in the published volumes of the 1850 and 1860 censuses. It is these state averages that economic historians have relied on in previous work (see, for example, Lebergott 1964 and Moen 1993).

The original manuscripts of the social statistics survive, however, and microfilm copies for many states can be found at the National Archives. I have supplemented these with microfilm copies from various state archives not included in the National Archives collection.[2]

The instructions to census enumerators specified that social statistics were to be collected for each civil subdivision of every county "as far as practicable" (DeBow 1853, pp. xxiv). Inspection of the microfilms reveals that, while few counties were fully canvassed, data for at least one civil subdivision was collected for the majority of counties in most states (or territories). Sometimes the reported subdivision names correspond in obvious ways to known geographic units (for example, wards in Philadelphia) but in many cases they do not (for example, the marshal may refer to a subdivision as "my subdivision"), or the names are illegible. Because of this problem I have chosen to aggregate all subdivision data to the county level. Further, because appropriate within-county population weights are difficult or impossible to determine for some states, all data analyzed in this paper are unweighted averages of the original subdivision figures.

Table 1 lists states for which archival information has been retrieved, and the number of county-level observations per state. This number is usually less than the number of counties in each state because data might be missing for a particular county in 1850 or 1860, or because the county came into existence in the 1850s. All told, there are 813 counties in 1850 and 1,169 counties in 1860 in the sample that has been retrieved thus far. While it is obvious that coverage across states is far from complete (except, perhaps, in the South), all regions of the country, other than the Mountain or Pacific states, are

represented in both years.

Other than commenting that information was "not to be ascertained entirely by personal inquiry of individuals, but in part from public records and reports, and public offices of towns, counties, states, or other sources of information" the census was silent on just how marshals were to compute an "average" wage, and the records I have examined contain no marginalia on data collection procedures (DeBow 1853, p. xxiv).[3] On apriori grounds it is very doubtful that "public records" provided relevant wage information; state and local governments were not in the habit of surveying the incomes of their citizens before the Civil War. Nor is it likely that marshals visited, for example, every farm laborer in a community -- or even a sample of them -- for the purpose of calculating a literal average. Such a procedure would have been far too costly in time and money to justify to census authorities. Rather, it seems plausible that marshals asked a few knowledgeable individuals what the "going" or typical wage in the area was for a specific occupation.[4]

Despite the lack of detail in the instructions to census enumerators, previous scholars have judged the average wages published in the 1850-70 census volumes to be generally trustworthy. In particular, Lebergott (1964, p. 284) thought the data had "a considerable measure of reliability", enough to use the published figures as benchmarks and, in the case of farm wages, to measure state-level changes in wage dispersion between

1850 and 1860.[5]

A somewhat different problem is whether "farm" and "common" labor were truly different occupations in the eyes of census enumerators (or the respondents). Research on historical wage gaps in Europe (eg. Siscic 1992) has tended to use identifiable nonfarm occupations (for example, urban ditch-diggers) -- rather than generalized "common" labor. Two points can be made in response. First, there is no doubt that "common" labor in the antebellum U.S. commonly performed non-farm tasks, and that the distinction between common and farm labor was a meaningful one (see, for example, the discussion in Lebergott, 1964, pp. 267-271). Second, it is possible to compare average wages of common labor from the census to those derived from payrolls of civilian employees of the army (Margo and Villaflor 1987) for which it is known, a priori, that the data refer to workers performing specific non-farm tasks; when this is done, the two sources invariably coincide.[6]

3.0 Measuring the Farm-Nonfarm Wage Gap

3.1 County-Level Comparisons

To measure the farm-nonfarm wage gap at the county level, I use the following procedure. I compute two gaps, a nominal and a "real" gap, the latter being adjusted for geographic variation in the cost of living.

The first step is to compute the per-day value of board, v, which is defined to be

$$v = w_w - w_b$$

where w_w is the daily wage of common labor without board and w_b is the daily wage of common labor with board. The nominal farm wage is

$$f = w_f + 26*v$$

where w_f is the nominal monthly wage of farm labor with board. I next calculate the full-time monthly earnings of common labor

$$n = 26*w_w$$

Next, I define the variable GAP, the difference between the farm wage and nonfarm earnings

$$GAP = f - n$$

I then scale GAP by a weighted average of the n's; the weights are the number of manufacturing workers in each county (see section 3.2).[7] The purpose of this scaling, as will become clear in section 3.2, is to enable direct comparisons to be made between my county-level estimates of the farm-nonfarm wage ratio,

and aggregate estimates that reflect the geographic distribution of farm and nonfarm production. For ease of interpretation, the figure I report are 1 - $|GAP^*|$, where GAP^* is the scaled value of GAP.

The real gap is computed in a similar manner, except that f and n are first deflated by an estimate of the monthly cost of board, 4.3*b, where b is the weekly cost of board. Note that the scaling factor is still a weighted average of the n's, but the n's themselves are "real" wages, not nominal.

This methodology suffers from several problems. First, and perhaps foremost, it ignores a premium built into the daily wage for unemployment risk (Lebergott 1964; Margo and Villaflor, 1987). The premium compensated for the fact that day labor might not be fully employed on a monthly basis, while a farm labor hired on a monthly basis would be, by definition. Second, farm labor hired on a monthly basis with board may have received additional perquisites (such as housing) not made available to day laborers (Hatton and Williamson, 1991). Third, while deflating by board controls for a significant portion of living costs, it does not control for all of them. To the extent that the geographic variation in the non-board component of the cost of living was larger than in board, my estimates of the real gap may be too large.

Nothing can be done about the third bias (see below on the second bias) with the data at hand. But, to get around the bias due to unemployment risk, I propose two adjustments. First, some

census enumerators apparently misunderstood their instructions, and recorded the monthly wage of common labor with board, or (less frequently) the daily wage of farm labor with board. Using the entire data set for common labor pooled across both years, I estimate a regression which includes a dummy variable for the mis-recorded observations; the adjustment for unemployment risk can be derived from the coefficient of the dummy variable. Second, I make use of earlier estimates of adjustment factors derived from the Margo-Villaflor sample of civilian employees of the U.S. Army (Margo 1992). In the latter case, I have sufficient information to use different adjustment factors for each census region.

The estimates of the sample mean values of 1 - GAP^* for 1850 and 1860 are shown in Panel A of Table 2. The estimates are less than one -- that is, the farm wage was less than the nonfarm wage -- and there is very little difference between the nominal and real figures. Not adjusted for unemployment risk, the gap was about 20 percent. But, adjusted for unemployment risk, the gap was much smaller -- roughly 8 to 10 percent. It is noteworthy that both adjustments for unemployment risk give approximately the same estimates, despite being derived from independent sources of data.

Is the average within-county gap -- 8 to 10 percent-- economically meaningful? As noted above, it is possible that farm labor received additional perquisites so that, in equilibrium, the farm wage equalled the common wage. Unfortunately, there is

no information on perquisites other than board in the social statistics.

To get at the second bias I propose an indirect test based on the following idea: if the gap were a true disequilibrium, it would create rents that might have been capitalized into the value of the fixed factor, namely agricultural land. That is, ceteris paribus, the value of land would fall when the gap gets smaller.[8] To see if it did, I estimate the following regression

$$dVALUE = \alpha + \beta GAP + \Sigma \delta_i X_i \qquad [1]$$

where dVALUE is the change in the per acre value of farm land between 1850 and 1860, dGAP is the change in GAP (nominal, not real) adjusted for unemployment risk using the first method described above, and the X's are other variables (expressed in change form or dummy variables) that would expected to affect dVALUE.

The specification can be motivated by the following econometric argument. Suppose that the difference in the per acre value of improved (V_i) and unimproved (V_u) land is a constant, c

$$V_i - V_u = c$$

The average per acre value is

$$V = V_u + (V_i - V_u) \times IMP = V_u + c \times IMP$$

where IMP = percent of land that is improved. The specific functional form for V_u is

$$V_u = \alpha_{tj} + \beta GAP + f + \epsilon$$

where α_{tj} is a time dummy (t = 1860) that is allowed to vary across census regions or states, f is a fixed county effect, and ϵ is a random error. Because GAP is negative for almost all counties, a reduction in the difference between the farm and nonfarm wage is equivalent to an increase in GAP; hence ß should be negative. When α_{tj} is constrained to vary only by region, I also include a state level variable RAIL, which is the number of miles of rail per square mile in the state; the coefficient of this variable is expected to be positive. Inserting the expression for V_u into the expression for V, and taking differences between 1850 and 1860 gives regression [1]. The results are shown in Panel B of Table 2.

The estimate of ß is, in fact, negative -- increases in GAP were associated with decreases in the value of farm land, other factors held constant -- and statistically significant at the 5 percent level.[9] The other variables in the regression -- the change in the proportion of improved acreage and the change in railroad miles -- have coefficients that conform to prior expectations. Statistical significance, however, is not the same

as economic significance. To evaluate economic significance, I imagine the following thought experiment. Suppose that GAP had been zero in 1850 and 1860: by how much smaller would the mean value of V have been? The answer is between 2.5 to 3.0 percent, depending on the year. Expressed in this manner, the economic importance of the within-county wage gap seems to have been very small.

The conclusion that disequilibria were "small" -- or perhaps, could not have become persistently large -- is also suggested by the following test for market integration.[10] Consider the regression:

$$df = \alpha + \beta dn + \delta GAP_{50} + \epsilon$$

where df = change in the farm wage between 1850 and 1860, dn is the change in the nonfarm wage, and GAP50 is the gap in 1850. If the markets for farm and non-farm labor were integrated we would expect that $\beta > 0$ and $\delta < 0$ -- that is, wage changes for farm and nonfarm labor should be correlated, and economic forces (for example, labor migration; see also section 4.0) should help reduce disequilibria over time. The results of estimating this regression are (the regression includes state dummies):

$$df = \underset{}{-1.332} + \underset{(33.974)}{0.672}\, dc - \underset{(18.780)}{0.717}\, GAP50$$
$$\underset{(4.297)}{}$$

$$N = 672 \quad R^2 = 0.689$$

Absolute values of t-statistics are shown in parentheses. As hypothesized, ß is positive and δ is negative. The elasticities (of df with respect to dc and df with respect to GAP50), however, are less than one in absolute value, suggesting that market integration was less than perfect.[11]

3.2. Aggregate Comparisons

The fact that wage gaps within counties were small does not imply that their aggregate counterparts were small. Demand for nonfarm labor did not grow at equal rates at different locations over the antebellum period. Geographic imbalances in demand, coupled with imperfect short (or medium) run adjustment in labor supply, could produce a larger wage gap at the aggregate level. In particular, if farm labor were concentrated in low-wage counties (relative to non-farm labor), the aggregate gap will exceed the within-county gap.

To measure the aggregate gap it is necessary to weight the county-level data in some manner. Unfortunately, neither the 1850 or 1860 census reported county-level data on, for example, the number of nonfarm workers (or, for that matter, the number of farm workers). As proxies for the true weights, I weight the farm wage by the number of improved acres, and the nonfarm wage (as noted in section 3.1) by the number of manufacturing workers, both of which were reported by the census at the county level. These are not ideal weights, and so my results should be viewed

as provisional.

The results are shown in panel C of Table 2. Again, both a nominal and a real gap are shown. The nominal gaps are much larger at the aggregate than at the county level, although it appears there was some reduction in the aggregate gap between 1850 and 1860. Much of the disparity between the county-level and aggregate gaps disappears once the aggregate gap is adjusted for geographic differences in the cost of board. Evidently farm labor tended to be concentrated where nominal wages were lower, but the effect of this concentration on the aggregate gap was muted once the cost of board is controlled for.

4.0 Discussion

My results suggest that, at the aggregate level, the nominal wage gap in the U.S. in the 1850s was between 30 and 40 percent, and the real wage gap was between 10 and 20 percent. Aggregate gaps of such magnitude are grossly at variance with Williamson and Lindert (1980, p. 71), who claimed that "intra-regional wage differentials" -- by which they mean the farm-nonfarm wage gap-- "were trivial in late antebellum America ... no regional exhibited pronounced ... wage gaps for labor of comparable skill." In fact, if Williamson and Lindert's estimates of farm and nonfarm wages are aggregated to the national level, and the wage gap computed as in the previous section, the estimate for 1850 comparable with Panel A of Table 2 is 1.02, and this is the

nominal, not the real, gap.

To derive their estimates of the wage gap Williamson and Lindert used the published state averages of monthly farm and day wages of common labor, precisely the aggregate counterparts of the manuscript data I use. They converted monthly farm wages to daily wages using an "adjustment factor" derived from Lebergott (1964). The primary source of the discrepancy between my results and theirs is apparently due to this adjustment factor. Lebergott's Table A-30 (1964, p. 546), from which the adjustment factor was taken, gives monthly wage and daily wages (both with board) for 1832. Lebergott's presentation of Table A-30 makes it appear that the wages figures refer to the same type of labor (namely, farm labor). Williamson and Lindert's adjustment factor is simply the monthly wage divided by the day wage, from Lebergott's table. But Lebergott's text (1964, pp. 258, 267-268) and Tables A-23 (p. 539) and A-25 (p. 541) makes it clear that the monthly wage in Table A-30 refers to farm labor and day wage to common labor.[12] Obviously, had Williamson and Lindert computed an "adjustment factor" by dividing the daily wage of common labor in 1850 into the monthly wage of farm labor in 1850, they would have found, by definition, no wage gap. As noted by Lebergott (1964, p. 267), the ratio of the day wage of common labor and the monthly wage of farm labor in 1832 was almost identical to the ratio prevailing in 1850. Thus, in effect, Williamson and Lindert found no wage gap in 1850 because their adjustment factor eliminated it by construction.

How do my estimates of the wage gap compare with those from other countries undergoing industrialization in the nineteenth century? Although differences across countries are difficult to interpret because of differences in methodology and data sources, the U.S. in the 1850s appears to be between France and Great Britain.[13] Wage gaps in France ca. 1850 were smaller than in the U.S; according to Siscic (1992) the nominal gap in France was about 8 percent and essentially negligible (1 percent) in real terms. Nominal gaps in Great Britain in the 1830s, particularly southern England, were very large in the 1830s (about 73 percent; see Williamson 1990, p. 186). Adjusting for cost of living differences, urban "disamenities", and the fact that poor relief was used to supplement farm incomes during slack periods (moreso than urban incomes) reduces the gap to between 18 and 33 percent, still higher than my estimates.

My findings also bear on the controversy over the rate of growth in per capita income before the Civil War. David (1966) observed that the rate of growth of per capita income from 1800 to 1860 could be inferred from growth rates of labor force participation, labor productivity in the farm and nonfarm sectors, and the shift of labor out of agriculture. The "sectoral shift" effect was quantitatively important, because output per worker in agriculture was well below output per worker in non-agriculture.

It has never been very clear how much of the sectoral <u>productivity</u> gap was a true disequilibrium, and how much

represented differences in complementary factors per worker or technology. According to Weiss's recent estimates, the farm-nonfarm ratio of output per worker in 1860 was 0.376 in 1860 (calculated from Weiss 1989, Tables 2 and 3). Based on my estimates of the aggregate real wage gap (using the first adjustment for unemployment risk; see Panel C of Table 2), about 17 percent of the sectoral productivity gap was a disequilibria; using the nominal wage gap instead, about half of the productivity gap was a disequilibrium.

Finally, my estimates bear on relative productivity theories of industrialization. Such theories posit that the relative wages of different types of labor influence the geographic pattern of industrialization. The best known example is Goldin and Sokoloff (1984). In the Goldin and Sokoloff model, the relative price of female labor in agriculture is less than one, but there is no supposition that the labor market is in disequilibrium -- that is, male and female agricultural labor receive the value of their marginal products. Over time, however, technological and organizational innovation create the potential for a narrower gender gap in productivity in manufacturing (relative to agriculture). Goldin and Sokoloff show that, as result, female labor will migrate towards manufacturing, and manufacturing growth will be biased towards locations where the (initial) relative price of female labor is low -- the Northeast compared with the South, in the American case.

Goldin and Sokoloff's argument, in fact, is broader than

their application to gender differences. As long as the manufacturing technology is flexible enough so that agricultural labor is a potential substitute for nonfarm labor, a relatively low agricultural wage (compared with nonfarm labor) may be an incentive for new investment in manufacturing capital. Some evidence that this was so in the 1850s is revealed by the following county-level regression, in which the dependent variable (LGMCAP) is the log of the ratio of manufacturing capital in 1860 to manufacturing capital in 1850, LCF50 is the log of the ratio of the nonfarm and farm wage in 1850, and LMF50 is the log of the ratio of the nonfarm and female domestic wage in 1850. The regression also includes dummy variables for states[14]:

$$\underset{}{\text{LGMCAP} = 0.870} \underset{(2.219)}{} \underset{(-2.106)}{- 0.567\ \text{LCF50}} \underset{(-0.507)}{- 0.102\ \text{LMF50}}$$

$$N = 571 \quad R^2 = 0.127$$

The negative coefficient of LMF50 is consistent with Goldin and Sokoloff (1984), although the coefficient is not statistically significant. The coefficient of LCF50 is negative and statistically significant. Counties where the relative price of agricultural labor was low attracted a disproportionate share of manufacturing capital between 1850 and 1860. The economic importance of this effect, however, should not be overemphasized. If the state dummies are excluded, the R^2 falls to 0.038. If LCF50 and LMF50 are added to a regression with just the state

dummies, the mean squared error of the regression declines by only 0.4 percent. Clearly, many factors other than relative wages influenced the geographic variation in the growth of manufacturing capital between 1850 and 1860.

5.0 Conclusion

This paper has used the 1850 and 1860 manuscript censuses of social statistics to study nominal and real wage gaps between farm and nonfarm labor. Within counties, the gap was about 10 percent, and does appear to capture small disequilibria. Aggregated geographically the gaps were larger, perhaps 30 to 40 percent in nominal terms, and 10 to 20 percent in real terms. Evidently the antebellum U.S. was not immune to labor market imperfections between its farm and nonfarm sectors, just like other nineteenth century industrializing counties such as France or Britain. Finally, the geographic pattern of industrialization was influenced by wage gaps. Counties with a relatively low agricultural wage experienced disproportionate growth in manufacturing capital between 1850 and 1860.

Notes

1. The marshals also collected data on wealth, churches, libraries, pauperism, schools, and perceptions of agricultural yields. For an analysis of the data on agricultural yields for the South, see Schaefer (1983).

2. This supplementation is ongoing, so the results in this paper should be viewed as provisional.

3. Specific instructions for collecting the wage data amounted to nothing more than "the information called for in the six columns is so simple and so plainly set forth in the headings that it is deemed unnecessary to add thereto" (DeBow 1853, pp. xxv).

4. This interpretation of the data is suggested by the considerable heaping, evident in every state, on dollar or sub-dollar amounts, for example, $12.00 per month for farm labor, or $0.75 for common labor.

5. Reliability of the published aggregates, however, is not the same as reliability of the county-level figures. Margo (1994) provides a discussion of measurement error in the county-level wage data; the basic conclusion is that "trimming" the data (removing outliers) may be appropriate in certain circumstances. Except where noted, the conclusions of this paper are unaffected if the data are subjected to a 10 percent trim (that is, the top and bottom 10 percent of wage observations are excluded).

6. See Margo (ch. 3, in progress).

7. Here, and elsewhere in the paper, published county-level data were extracted from the ICPSR computer file, "Historical, Demographic, Economic, and Social Data: The United States, 1790-1970".

8. This is a partial equilibrium argument -- that is, I am assuming that land is truly a fixed factor. In general equilibrium, the supply of land might increase, bidding down it price, thus obscuring any capitalization effect.

9. If the data are subjected to a 10 percent trim, the coefficient on DGAP remains negative, but statistical significance declines to the 10 percent level.

10. This specification is similar to Siscic's (1992) except that Siscic does not include the initial wage gap in his regressions.

11. Measurement error may be biasing the coefficients in the regression. In particular, if the errors of measuring f and n at the county level were positively correlated (within counties), ß and δ may be biased upwards in absolute value (I am grateful to Pierre Siscic for this point). To investigate the size of the bias I re-estimated the regression after subjecting the data to a 10 percent trim (see Margo 1994 for the rationale for trimming the data). The results were as follows:

$$df = -0.801 + 0.483\ dc - 0.506\ GAP50$$

$$(2.838)\quad (18.077)\quad (10.155)$$

$N = 432 \quad R^2 = 0.578$

As hypothesized, trimming the data reinforces the finding that market integration was less than perfect. However, further trimming has little effect on the coefficients. In addition, I also estimated a regression of dGAP on GAP50; if measurement error were the sole reason why the coefficient of dc was positive, the coefficient of GAP50 in this second regression should be close to zero. In fact, the coefficient is -0.85 and is statistically significant. If measurement errors were uncorrelated across occupations within counties, the coefficient of GAP50 in this second regression (-0.85) would be biased towards -1, but ß (see the text) will not be biased. Hence, regardless of how market integration is tested for, it is present in the case of the antebellum farm and nonfarm sectors, albeit imperfectly.

12. The original source for the 1832 figures, the so-called "Livingstone" report (23rd Congress, 1st Sess., Serial Set 252) reported state averages of wages for several states and, for two states (New Hampshire and Georgia), town and county averages respectively. The primary purpose of the survey was to inquire about the burden of taxation and local government expenditures; Livingstone sent surveys to county and town commissioners and then published the results. The wage statistics collected were the following: average daily wages, without board, of labor used in road and bridge construction and maintenance; average monthly

wages of "labor", with board; average daily wages of "labor" with board; average daily wage of harvest labor; and the weekly cost of board to "laboring men". Internal evidence in the report (specifically, the letter to Livingstone accompanying the Missouri documents) verifies -- at least for Missouri -- that the day wage with board (not the harvest wage) refers to common labor, and Lebergott used these figures in constructing state averages for 1832 (his table A-25). Lebergott interpreted the monthly wage with board as pertaining to farm labor (his table A-23); in reading the original report, however, I could find no direct statement in it that the monthly figures refer to farm labor per se. However, the consistency between the Livingstone monthly wage figures, estimates of monthly wages for 1832 made by Senator John Holmes of Maine and published in the Congressional Register of Debates; and scattered farm wage quotations in the McLane report strongly suggest that the Livingstone data do refer to farm labor; see Lebergott (1964, p. 258).

13. For example, Siscic (1992) and Williamson (1990) both use urban unskilled wages to calculate the gap, and the skill level of the urban "unskilled" may have been higher than the skill level of nonfarm workers in my data (Williamson 1990, p. 183 uses laborers in the building trades).

14. Data are weighted by the average number of acres of agricultural land in the county in 1850 and 1860.

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Table 1: Distribution of Wage Observations: By States

State	1850 Sub-divisions	1850 Counties	1860 Sub-divisions	1860 Counties
AL	54	43	63	47
DE	20	3	24	3
FL	23	23	34	32
GA	89	88	130	113
IA	62	35	375	92
IL			540	100
KY	141	97	115	104
KS			63	22
LA	52	43	60	46
MA	311	14	334	14
MI	302	33	289	58
NC	98	76	113	84
NE			24	22
PA	1,137	63	1,465	65
SC	46	29	42	27
TN	157	76	97	74
TX	65	60	117	108
VG	151	130	165	141
Total	2,708	813	4,067	1,169

Sub-division: number of wage observations per state
County: number of wage observations per state, after aggregating sub-division observations to county averages
Source: manuscript census schedules, censuses of social statistics, 1850 and 1860

Table 2: The Farm-Nonfarm Wage Gap in 1850 and 1860

A. Farm/Nonfarm Wage Ratio: County-Level Data

	1850		1860	
	Real	Nominal	Real	Nominal
Unadjusted	0.806	0.835	0.802	0.810
Adjustment #1	0.926	0.929	0.926	0.922
Adjustment #2	0.922	0.925	0.902	0.825

Figures are unweighted means of the difference between the farm and nonfarm wage (see text), scaled by the aggregate weighted average of the nonfarm wage; the weight is the number of manufacturing workers in the county, as reported in the 1850 or 1860 census volumes.
Unadjusted: nonfarm wage is not adjusted for unemployment risk premium
Adjustment #1: adjusted from regression estimate of unemployment risk premium using mis-reported observations (see text)
Adjustment #2: adjustment based on Margo (1992, pp. 199, 200, 202, 203)

B. Regressions of dVALUE

	ß	t-stat	ß	t-stat	ß	t-stat
Constant	1.720	3.137	2.830	1.917	7.481	11.286
dGAP	-0.223	-3.346	-0.195	-3.014	-0.200	-3.132
dIMP	19.460	2.788	13.427	4.708	14.513	4.935
dRAIL	99.047	3.785	97.721	2.523		
Dummies						
Regional	No		Yes			
State	No				Yes	
R^2	0.106		0.172		0.217	
Mean value-dep. var =	4.266					
N = 601 counties						

dValue: change in the per-acre value of farm land between 1850 and 1860 (Note: 1860 figure is in 1850 dollars; deflator is Warren-Pearson price index)
dGAP: change in the farm-nonfarm nominal wage gap (as defined by adjustment #1 in Panel A) between 1850 and 1860 (1860 is in 1850 dollars, deflated as above)
dIMP: change in the percentage of improved acreage between 1850 and 1860
dRAIL: change in rail mileage per square mile in the state between 1850 and 1860

C. Farm-Nonfarm Wage Ratios: Aggregate (Weighted) Estimates

	1850		1860	
	Real	Nominal	Real	Nominal
Unadjusted	0.778	0.591	0.795	0.674

Table 2 (continued)

Adjustment #1	0.896	0.681	0.917	0.777
Adjustment #2	0.865	0.657	0.825	0.698

Aggregate weighted estimates: difference between weighted average of farm real wage and weighted average of nonfarm real wage, divided by weighted average of nonfarm wage. Weight for farm wage is the number of improved acres in the county; weight for nonfarm wage is the number of manufacturing workers in the county. Unadjusted, Adjustment #1, Adjustment #2: see Panel A.

Source: see text