The Efficiency of Slavery: Another Interpretation

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By GAVIN WRIGHT*

The managing editor of this *Review* has very kindly invited me to comment on the recent reiteration by Robert Fogel and Stanley Engerman (hereafter, F-E) of their views on the efficiency of American slave agriculture (1977). Based on my reading of the article, my conclusions are that their lines of defense do not adequately respond to the most important objections, and that their new interpretations suffer from many of the shortcomings of the original work. This brief note will discuss two problems: the representativeness of the census year 1860, and the effects of the crop mix on the apparent efficiency of slavery. A concluding section will suggest some alternative lines of analysis for future work.

I. The Census Year 1860 in Perspective

In *Time on the Cross*, F-E argued that southern agriculture was more efficient than northern agriculture in 1860, and that this regional superiority resulted from an efficiency advantage of slave-using enterprises over free farms in both regions. They interpreted this efficiency advantage as a superiority in physical efficiency, which they attributed to economies of scale in plantation operations and to the development of distinctive managerial skills and methods among planters. The same factors were held to be responsible for the enormous expansion of cotton production and the rapid growth of southern per capita income down to 1860. The efficiency calculations on which these conclusions were based (and the subsequent revised estimates presented in their 1977 article) used data from only one crop year, the census year 1859–60. In response to this analysis, along with several other writers, I argued (presenting econometric evidence) that the cotton demand situation was unusually favorable during 1820–60, especially for the census year 1860. I also argued that the cotton yield of 1860 was unusually high, because output was far above the level predicted by a supply curve estimated over the period 1820–60.

In their rebuttal to the critics, F-E reject these results (1977, pp. 280–82). They treat the two arguments as though they were entirely separable, dismissing the importance of demand solely by reference to the relative cotton price, and disposing of the supply argument with a discussion of yield variability. But this is an elementary error: because the world cotton price was heavily influenced by the size of the American crop, price is not a measure of demand.[1] And because *both* supply and demand were abnormally high, the price was not exceptional, but the year in question was exceptional to say the least.

To show how evident these points are, and to indicate that they do not depend on some special econometric formulation, I display as Figure 1 F-E's own index of cotton supply and demand, reproduced from page 92 of *Time on the Cross*. The unique position of the census year 1860 for both supply and demand is apparent. Fogel and Engerman dispute the assertion that per acre cotton yields were unusually high in 1860, but their discussion is largely irrelevant: they offer no alternative reason why one particular year should be so far above the trend, and the precise reason for

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[1]The elasticity of demand for American cotton has been estimated at approximately unity, if not lower. Because the cotton crop was essentially predetermined, the demand curve may be estimated by ordinary least squares with price as the dependent variable. The simplest estimate for the antebellum period is

$$\ln P = 8.21 - 0.94 \ln Q + 0.052$$

which implies an elasticity of demand equal to 1.06. The residual from this curve in the census year 1859–60 is 15.9 percent.

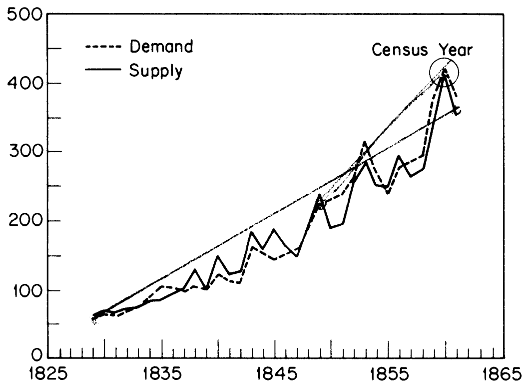

FIGURE 1. A COMPARISON BETWEEN INDEXES OF COTTON DEMANDED AND SUPPLIED, 1829–61

this upward deviation is immaterial. If yields were not unusually high, the alternative interpretation can only be that an unusually large cotton acreage was planted in that year; but such a development, in conjunction with favorable demand, in no way challenges the assertion that the crop year in question was atypical. Fogel and Engerman stress that relative cotton prices were even higher in 1850 than in 1860, but they do not mention the reason, which was that the 1850 crop was a particularly bad one which "encountered a series of disasters from first to last."[2] The claim that more than 91 percent of the increase in cotton production between 1850 and 1860 can be "accounted for" by long-run factors (1977, p. 281) is rather implausible, as readers may see by drawing a line connecting the two census year outputs and comparing its slope with that of a trend line for the whole period.

The significance of the extraordinary character of the census year 1860 is not mainly for the calculation of aggregate regional indices of efficiency.[3] But because the relative importance of cotton was so much greater for large slave plantations than for small southern farms, one would expect an unusually favorable cotton year to affect cross-sectional comparisons within the South. Fogel and Engerman's figures confirm that the average share of cotton in the gross value of farm output varied from 29 percent on slaveless farms to 61 percent on plantations with more than 50 slaves (1977, p. 288). Even if the high cotton output of 1860 were attributable to a large cotton acreage rather than yields, the cross-sectional pattern would still be affected: the precise effect would depend on the distribution of "abnormal" cotton acreage among farm sizes, but since roughly half of the slaveless farms grew no cotton at all, they obviously could not have shared in the distinctive prosperity of that year. And since available evidence indicates that the marginal return to cotton acreage (measured at market prices) was substantially above that of corn acreage in all but the poorest years, the *combination* of heavy cotton planting and strong demand will markedly effect an efficiency calculation which ignores these facts.

Despite its secondary importance, however, it is worth looking more closely at F-E's denial that yields were unusually favorable in 1860. This denial is based on their belief that my estimate of the upward deviation in yields is too high to be believable (1977, p. 281). On its face this is a rather peculiar argument: they assert that the residual for 1860 is so large as to occur only once in a thousand cases, they argue that this is so unacceptably high as to completely invalidate the underlying supply curve estimate, and they apparently conclude that they can safely proceed as though 1860 were just an ordinary year. But this is also an inaccurate argument, because they compare the 1860 residual for a five-state area (in which the high yield was concen-

[2]See U.S. Commissioner of Patents (1849, p. 149). Contemporary accounts of the crop failure are abundant: see Commissioner of Patents (1849, Vol. 2, pp. 144, 170, 307); Commissioner of Patents (1850, Vol. 2, p. 510); James Watkins (pp. 81, 197, 150, 197, 217, 240, 258). Note that the cotton crop for the census year 1850 actually grew in the calendar year 1849.

[3]I omit discussion of these regional calculations because in my opinion the conceptual problems which afflict such comparisons are insuperable. Since in any case the South's superiority is entirely attributed by F-E to the superiority of slave plantations over the free farms of both regions, the entire argument rises or falls with the cross-sectional comparison within the South.

trated) with the 1867–1900 standard deviation for the entire South;[4] my residual estimates for the whole South in 1860 (11.6 percent to 23.6 percent) could hardly be called incredible.[5] It is, finally, an inappropriate argument because the probability calculations assume that yields are normally distributed, when in fact the distribution of cotton yields is known to be positively skewed (see Richard Day).

It is in the nature of cotton yield determination that this should be so. Fogel and Engerman appear to believe that the yields of all crops are highly correlated, and they implicitly scoff at the notion that the "favorable weather that supposedly visited the South in 1859–60" (1977, p. 282) should have affected only cotton; but the rainfall and temperature requirements of cotton are very distinctive and very demanding, and cotton yields are not highly correlated with those of other crops even in the South.[6] Fogel and Engerman's estimates of the "reallocation of corn land to cotton" (1977, p. 282) between 1850 and 1860 is based on the implicit assumption that relative outputs of the two crops were proportional to relative acreage planted in the two years (i.e., that yield fluctuations were identical for cotton and for corn). These estimates are highly misleading, because despite the abundant testimony to the failure of the 1850 cotton crop, the corn crop for that year was quite a good one, and observers noted: "Cotton was hurt by the same rains that helped corn" (p. 64).[7] Cotton passes through several critical phases of development during the growing season, and bad weather at any one of these points can drastically reduce yields below potential. Even if the growth of the stalk and bolls are ideal, it was common in the nineteenth century for the fruit to deteriorate rapidly from inclement weather during the picking season.[8] Periodically, however, a favorable growth capped by an extended period of fair weather for the harvest will produce a

[4]The postbellum figures show that the standard deviation for the five-state area is 20 percent higher than the aggregate standard deviation.

[5]Fogel and Engerman's objections to the supply curve itself (1977, p. 281) are also inappropriate. The most general specification is

$$\ln Q = A + b \ln P_{-1} + ct + d \ln L_{-2}$$

where L_{-2} is the cumulative acreage of public land sold in the cotton states, lagged two years. This equation does not assume that, for example, the "proportion of land in farms that was improved" was "fixed during the decade of the 1850s" (1977, p. 281), nor does it imply such constancy about any of the ratios which they list. None of these sources of output growth were new to the 1850's, and they are all presumably reflected in the coefficients of P_{-1}, L_{-2}, and certainly t. I would be the last to claim that an equation like this one embodies any very deep explanation for the growth of cotton output. The whole point of including land sales is simply to account for more of the year-to-year variation in the growth of cotton acreage and hence to obtain a better estimate of yield fluctuations. Contrary to what F-E imply, these equations track the growth of output very well during the 1850's—except for the census year deviation. Econometrically speaking, the main effect of including lagged land sales is to eliminate the pattern of serial correlation in the residuals which is obtained when the *log* of output is regressed against time. If the land sales variable is considered objectionable, the same effect (and similar residual estimates for 1850 and 1860) can be generated by conventional methods of correction for autocorrelation. Examination of residual plots from any of these regressions (available from the author on request) leaves no doubt as to the distinctiveness of the census year under discussion.

[6]Fogel and Engerman compute a peculiar parameter, "the elasticity of corn yields with respect to cotton yields" (1977, p. 281, fn. 12), but using the same data one finds that only one-quarter of the variation in cotton yields is explained by variation in corn yields. Much better data from twentieth-century experiment station records show no correlation whatsoever (r^2 = .0036). The data may be found in Perrin H. Grissom and W. I. Spurgeon.

[7]See Alfred Smith, pp. 63–65. Referring to South Carolina, Smith writes that the heavy rains "helped the corn considerably, and the crop was a good one, one of the best ever grown" (p. 64). Comparison of the 1850 census crop with the Patent Office estimates for the preceding year shows that cotton output declined while the corn crop (in the seven leading cotton states) rose by 5 to 10 percent.

[8]"Normally the cotton plant produces bolls the entire length of the stalk.... It often happens, however, that unfavorable weather conditions cause a part of the fruitage to fall, and if bad weather prevails for a sufficient period a considerable portion of the stalk may become bare of fruit.... In fact, it seldom happens that a season is so favorable that the plant is fruited from bottom to top, but when such is the case a bumper crop is the result" (see James Covert, p. 93).

phenomenal yield, far above average. This seems to have been what occurred in the Southwest in 1859-60.

Fogel and Engerman assert that "available commentaries" on the 1860 crop "are devoid of references to an extraordinarily high yield" (1977, p. 281), but numerous testimonials are in fact available. Ezekiel Donnell's survey of the New Orleans press, for example, cites the "fine and very favorable picking weather" as the explanation for the cotton receipts "larger than ever before known" in December 1859 (p. 496). The *American Cotton Planter* of Montgomery states that "the picking season has been one of the most favorable ever known, especially in the Southern states" (p. 463). Even the rainfall data attest to the unusually fine weather during the harvest season.[9] And after noting that the fine weather had probably improved the quality of the cotton crop as well, the writer for the *American Cotton Planter* went on to marvel that "the prices have been wonderfully sustained." He concluded: "Taken as a whole, the cotton interest was never in a more prosperous condition" (p. 164). To avoid misunderstanding, there is no claim here that F-E's results would be reversed by data from another crop year. But the choice of year clearly exaggerates the apparent efficiency of slave labor, and it is unreasonable to deny or ignore this fact.

II. The Crop Mix Effect

In my earlier critique, I argued and presented evidence that F-E's efficiency index was closely linked to cotton, and that the share of cotton in output was the main determinant of the value of output per worker (1976, pp. 316–18; 334–36). I suggested that this pattern of crop selection could be explained by the riskiness for small farms of growing cotton at the expense of food crops for home consumption. In their response, F-E reject this interpretation on the grounds that the correlation between cotton and efficiency, using their revised calculations, is less than perfect (1977, p. 289). They go on to express skepticism about the proposition that cotton was a riskier choice than corn, and to argue that the figures imply an implausibly large risk premium in exchange for "some unspecified reduction in the variance of ... income" (1977, p. 290). On this basis they apparently conclude that no account whatever need be taken of the effects of crop mix on efficiency.

Nothing in this discussion would lead me to alter my views. It is true that the new efficiency figures would somewhat reduce the correlation between cotton and efficiency; but these new figures have been incorrectly calculated, as Paul David and Peter Temin show. The key new result is that free farms and small slave farms are now found to have been of equal efficiency when locational rents are removed; but taken at face value, the new estimates do not indicate that small slave farms had any locational advantage over free farms. Their estimates place the average value of unimproved acreage, which they identify as "the locational component of land" (1977, p. 283) at $2.57 per acre for slaveless farms, $2.53 for farms with one to fifteen slaves (1977, p. 284, Table 6, line (4)). Hence, these figures do not suggest that any alteration in the relative efficiency ranking of these two classes is called for. But even if this were not the case, it is difficult to see why we should limit outselves to four size-class aggregates when the micro data them selves are available, and why a failure to find universal constancy in efficiency should call for complete dismissal of the crop mix effect. Direct statistical tests show the importance of the share of cotton in total production as a determinant of productivity when output is valued at market prices. This effect is revealed in the following semilog regression run on the Parker-Gallman sample of farms in the cotton South in 1860:

[9] During the harvest months October through January, 1859–60, rainfall in Mobile was only half of the average for the nineteenth century for these months; only three other years fell below this figure (in the period for which records exist 1840–1900). For New Orleans, rainfall was less than two-thirds of the nineteenth-century average. The data may be found in H. Helm Clayton.

$$(1)\quad ln(V/L) = 4.87 - \underset{(0.4)}{.0004SQ} + \underset{(26.45)}{1.27^{**}CS} - \underset{(1.78)}{.0001IA}$$
$$R^2 = .129; N = 4977$$

where V = value of crop outputs, L = labor, SQ = index of soil quality, CS = share of cotton in total crop output, IA = improved acreage, and t-ratios appear in parenthesis. Experimentation with additional explanatory variables has not uncovered any which eliminate or even seriously weaken the effect of CS on $ln(V/L)$.[10] To be sure, F-E subject their input-output figures to a battery of elaborations and refinements, but nothing in their discussion challenges this basic relationship.

Indeed, F-E seem to recognize such a relationship, but they acknowledge only that the "optimum share of cotton" (p. 289, fn. 26) will differ for each slaveholding size class. What they do not mention is that the same relationship holds when the sample (for the same year and geographical area) is restricted to *slaveless southern farms alone:*

$$(2)\quad ln(V/L) = 5.01 + \underset{(17.97)}{1.27^{**}CS} + \underset{(4.17)}{.010^{**}SQ}$$
$$R^2 = .123; N = 2412$$

Note that the coefficient for the cotton share is precisely the same as in the aggregate regression (1). Thus, even if it were true that the "optimum cotton share" were higher on large plantations than on small farms, the fact remains that the crop mix per se had a marked impact on productivity at all farm sizes, as one would certainly expect in a bumper cotton year. The significance of this effect does not depend on acceptance of any particular formulation of the risk argument: it is not particularly critical whether equations (1) and (2) reflect risk preference, leisure preference, irrationality, or merely the luck of the draw in 1860. The important point is that the crop mix called the tune in that year.[11]

Since the question of risk has been raised, however, F-E's discussion deserves a brief response. It is a disappointment to read the complaint that my meaning is "never clearly defined" (1977, p. 290), when they fail to cite the background article to which readers were referred for explanation (my paper written with Howard Kunreuther),[12] and they do not acknowledge the definition and empirical evidence presented there.[13] In that article it is made clear that we are not discussing the "variability of income," but the risk of falling below self-sufficiency in

[10]For example, adding IA/L to the regression significantly improves the fit but has no effect on the coefficient of CS:

$$ln(V/L) = 3.02 + \underset{(6.33)}{.0057^{**}SQ} + \underset{(30.88)}{1.28^{**}CS} + \underset{(39.31)}{.606^{**}IA/L} - \underset{(9.88)}{.005IA}$$
$$R^2 = .335; N = 4969$$

In fact, we now have a significantly negative relationship between productivity and scale.

[11]The results also show the fallacy in F-E's argument that the regional crop mix problem can be waved away because small southern farms, which had the choice of growing cotton, are found to have efficiency levels equal to the average of northern farms (1977, p. 288). That argument assumes explicitly that small southern farms chose an efficiency-maximizing crop mix in 1860. ("But since there was no climatic obstacle that prevented the free southern farms of the cotton belt from choosing exactly the same mix of products that was selected by large slave plantations, it may be assumed that they chose the product mix that was most efficient for them. Presumably a product mix with a larger cotton share would have decreased, or at least not increased, their efficiency" (1977, p. 288). But regression (2) suggests that small farms would have markedly increased their productivity if they had planted more cotton, even without acquiring slaves.

[12]The referrals appear in my papers: 1975, p. 446; 1976, p. 318.

[13]It is similarly disappointing that they raise the possibility of a cash or liquidity constraint and state: "Wright does not explore this issue, although it is widely suggested in the traditional literature on nineteenth century agriculture" (1977, p. 290, fn. 30). But this is one of the central themes of our uncited article. The notion of a cash constraint is widely suggested in the literature on *post-Civil War* southern agriculture and may certainly help to explain the concentration on cotton by small farmers after 1865. It is not of much help in explaining the very limited amounts of cotton grown by small farmers in 1860, though the effort to avoid being caught in a treadmill of indebtedness is one of the underpinnings of the safety-first model.

basic foodstuffs.[14] We argued that because certain food requirements must be satisfied regardless of a particular year's yields and prices, relevant measures of risk (of this type) should be denominated in units of food, primarily corn in the historical case now under discussion. In this context, we compared two alternative means of obtaining corn: production of corn for on-farm consumption, or production of cotton in exchange for purchased corn. Assuming that corn required no cash inputs, we defined the two choices as (in corn-bushel-equivalents per acre):

$$x = \frac{Y_{cot}P_{cot} - D}{P_{crn}}$$

$$y = Y_{crn}$$

where Y_{cot} = cotton yield, Y_{crn} = corn yield, P_{cot} = cotton price, P_{crn} = corn price, D = additional costs per acre associated with growing cotton instead of corn. Using postbellum data Kunreuther and I showed that the standard deviation of x was four to five times greater than the standard deviation of y (see p. 537). We also showed (pp. 534–535) that crop allocation choices in 1860 could be explained by farm size and by subsistence requirements, as measured by number of family members per improved acre. In fact, farm population per improved acre was twice as great on slaveless farms as on the largest class of slave plantations.[15] Clearly the small farms had to plant a larger relative acreage in food crops, even if slave plantations also sought self-sufficiency at the same per capita consumption levels. And as the safety-first model would predict, per capita corn production remained virtually constant between 1840 and 1860, as the expansion of acreage beyond food requirements went predominantly into cotton.[16]

Thus, when F-E document the high variability of the corn price compared to the cotton price (1977, p. 290 and fn. 28), they are supporting my position and not theirs. The many uncertainties of *buying* corn were precisely the reasons why farmers chose to grow their own. And, when F-E claim that the implied risk premium was implausibly high, they forget completely about the exceptional character of the year in question. The census year 1860 was outstanding for cotton but was reported to have been only poor to medium for corn.[17] In contrast, as previously shown, the census year 1850 was characterized by "excellent crop except cotton."[18] A rerun of regression (1) for a sample of cotton South farms of all sizes from the 1850 Census shows the following result:

$$(3)\quad ln(V/L) = 4.92 + \underset{(1.84)}{.009SQ} + \underset{(4.08)}{0.50^{**}CS} + \underset{(0.89)}{.000081A}$$

$$R^2 = .050;\ N = 668$$

The crop mix effect remains significant, but the coefficient value is less than 40 percent of its 1860 level. Whereas in 1860 the favorable crop mix effect is found in all regions and size classes, in 1850 these subset regression coefficients are frequently insignificant and are sometimes negative. As Kunreuther and I showed for the postbellum years, while cotton was a more profitable choice on the average, there were a number of years for which farmers would have saved more money growing corn than they made

[14]This target has little to do with modern estimates of nutritional requirements (see F-E, 1977, p. 290), but refers instead to the farm household's own assessment of its minimum tolerable standards. Note that because our interpretation hinges on the essentially financial consequences of shortfalls (indebtedness or loss of assets), we are including the feed requirements of working livestock in the determination of minimum requirements.

[15]For the cotton South as a whole, farm population per improved acre was .225, .153, .133, and .119 for the four slaveholding categories, respectively (0, 1–15, 16–50, over 50).

[16]For the five cotton states of the deep South, corn output per capita stood at 29.07, 31.07, and 29.58 in the three census years.

[17]See the citations in Robert Gallman (1970, p. 8).

[18]See W. I. Thorp, p. 125. The Patent Office crop estimates for the 1840's show a marked *rise* in the corn crop between 1848–49 and 1849–50, while the cotton output figures show a decline. See Gallman (1963).

growing cotton (see p. 537). Thus if the risk premium for 1860 appears to be implausibly large, the explanation is simply that the farmers did not know in advance that the year would turn out that way.

III. Some Alternative Lines of Interpretation

Taking a long view of the matter, the special features of the year 1860 are less important than the special features of the whole antebellum era. Cotton demand grew at better than 5 percent per year between 1820 and 1860, a rate of growth never equaled thereafter for such a sustained period. This growth was based on the opening of new markets for British cotton textiles, a phenomenon which could not continue indefinitely at the same pace.[19] Indeed, the first major phase of expansion had largely played out by 1860. When the *U.S.* cotton crop finally recovered to its 1860 level in the crop year 1877–78, the price was exactly the same as it had been before the war: demand had gone nowhere for 18 years, implying at least a 30 percent fall in southern per capita cotton earnings. Since F-E acknowledge that they find an efficiency advantage for slavery only in a handful of commercial crops (1977, p. 292), of which cotton was easily the most important, slavery's dependence on this burgeoning external demand is implied as much by their analysis as by mine. But after the period of postwar recovery, American cotton still dominated world production, and there was no increase in price representing the costs of less efficient production under free labor.[20] The economics of American slavery look very different from this historical perspective.

It seems to me that rather than calculating efficiency indices and then attempting to adjust and refine them to represent special historical and institutional elements of the situation, we would be better off to frankly acknowledge the ambiguities of these measures as applied to slavery, and that by exploring these ambiguities we may come to a better understanding of the economic nature of slavery. For example, all authorities agree that one of the important economic features of slavery was participation of slave women in fieldwork to a much greater extent than free women. But how shall we characterize this phenomenon? Since the returns to female fieldwork seem to have been higher than the alternatives, one might call it an increase in efficiency. Alternatively, one might call it an increase in female "labor force participation" and thus in reality a higher level of input. One might even say, since such activities as cooking and childcare can be centralized, that economies of scale are involved. There is a logic to each of these choices. But it seems to me that the central element is the same as the choice between cotton and corn: slavery involved the involuntary reallocation of family labor from nonmarket economic activity to production of crops for sale. In this alternative conception, it was the *interaction* between the crop mix and female field work which gave slavery its distinctive advantage: per capita food production was roughly similar on farms of all sizes, but this common pattern of self-sufficiency implied an extremely high payoff to the reallocation of "marginal" labor from the household to the fields. When slavery ended, black family labor allocation moved markedly toward a more conventional pattern (see Roger Ransom and Richard Sutch, pp. 22–23). From this point of view, the efficiency of slavery is historically specific to an era in which free households chose to sharply limit their participation in the market economy.[21]

Instead, F-E now interpret the efficiency

[19]See Lars Sandberg's recent history of the Lancashire industry: "This earlier growth had been based principally on the opening-up of new markets.... It is utterly reasonable to have expected such progress would continue up to World War I, especially with regard to the growth of output and exports" pp. 131, 180).

[20]Cotton prices were high during the transition period 1866–77, but there is no trend in real cotton prices over the whole period 1825–1910:

$$\ln P = 2.25 - \underset{(0.50)}{0.0008t}; \qquad R^2 = .003$$

[21]This theme is developed in my book (1978).

of slavery as a matter of the greater intensity of slave labor time per hour (1977, pp. 291–94). This view is not based on direct evidence: it is virtually the only remaining choice, given that F-E have convinced themselves that slaves worked no more hours than free farmers, and that neither crop mix effects nor distinctive features of the census year need be allowed for. The best of our models are no more than metaphors, and perhaps there is a grain of truth in the claim that an hour of cotton cultivation is a more intensive activity than an hour behind the stove. But if this were the best metaphor, it would be difficult to say why the efficiency advantage should be limited to a handful of southern commercial crops. And if an assembly-line analogy were appropriate, one would be hard pressed to say why the factory was not the ideal setting for slavery. The implication that the "assembly line" and the "speedup" were not well adapted to "urban industries" (see F-E, 1977, p. 292) should be hard for any economic historian to swallow. But if the economic essence of slavery was the shift of family labor from nonmarket to market activity, then it comes as no surprise that the "productivity advantage" disappears whenever output is fully monetized.

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