

Cotton, Corn, and Risk in the Nineteenth Century: A Reply*

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We expected that some economists would have difficulty accepting the analysis we presented of the South's abandonment of self-sufficiency between 1860 and 1880. But we admit to being taken by surprise by the argument proposed by Higgs and McGuire that it never happened. The claim that changes in Southern crop-choice behavior between these dates were "of trivial empirical consequence" is crucial to their case. This is so, because despite frequent references to "an alternative interpretation," a "compelling interpretation," and even a claim that these "crop choices were satisfactorily explained long ago," a careful reading of their article will show that Higgs and McGuire advance no explanation for the developments which we examined. It is only the belief that no essential change occurred before and after the war that justifies their complete neglect of the antebellum evidence, a neglect with severe consequences for an understanding of observed behavior in historical context.

THE ABANDONMENT OF SELF-SUFFICIENCY

In 1860 the South was essentially self-sufficient in basic foodstuffs; in 1880 it was not. The research on these matters is now so extensive that these statements brook no denial; certainly Higgs and McGuire offer none.¹ This change, and not the cotton-corn ratio, was the basic motivating *explicandum* for our article and the development on which the

* This paper constitutes a reply to the preceding article ("Cotton, Corn, and Risk: Another View," by Robert McGuire and Robert Higgs). Our original article appeared in *Journal of Economic History* (Vol. 35 (1975), pp. 526–551).

¹ For some reason, Higgs and McGuire object to our statement that "in the leading cotton states, per capita corn production and the per capita stock of hogs were only about half of what

safety-first model focuses. Despite the availability of cotton as a profitable cash crop for more than 50 years, the antebellum South never abandoned self-sufficiency. We argued that this regional self-sufficiency reflected purposeful, planned self-sufficiency at the farm level; we cited the evidence of Gallman to this effect, and we presented evidence of our own that the choice of crops was a function of household consumption requirements and farm size, as one would predict on the basis of a safety-first self-sufficiency model (Wright and Kunreuther, 1975, pp. 528–536).² We supported this interpretation further by citing evidence that self-sufficiency was maintained even though cotton was a much more profitable choice at the margin: farms and plantations differing in no other way but crop mix show large differences in the value of output per worker, with cotton leading the way.³ Higgs and McGuire ignore this entire argument, with fatal results. Attention to historical perspective refutes their statement that “many—perhaps the great majority of—Southern farmers *always* recognized heavy specialization in cotton as the best opportunity open to them” (our italics). Higgs and McGuire believe that we have confused “self-sufficiency in foodstuffs” with “the relative acreage of cotton and corn,” but it is they who have confused them, in attempting to refute a self-sufficiency argument *solely* by reference to relative crop outputs.

Now it is quite true that the cotton–corn ratios which we presented are not the most illuminating indicators of the change, even as (appropriately) corrected by Higgs and McGuire; to this extent we bear partial responsibility for their misdirected discussion. Even if there had been no change in the ratio of cotton to corn output (a proposition with which we do not agree), we would still have to explain why postbellum Southern farmers did not respond to lower productivity and smaller farm size by growing primarily subsistence crops, as small, impoverished farmers did before the

they had been twenty years earlier.” The facts are as follows: The ratio of 1880 to 1860 corn output per capita stood at 0.524 for the four leading cotton states of 1860, 0.527 for the five “Old South” cotton states on which Ransom and Sutch focus, 0.531 for the seven leading cotton states of 1860 (which are the same as in 1880). The corresponding figures for hogs per capita are 0.544, 0.530, and 0.583, respectively. “About half” seems to us a reasonable description of these fractions.

² Because of the absence of antebellum acreage data, these regressions used estimated acreage shares, derived from output data on the assumption of constant relative yields. On the basis of subsequent work, we can now report that the same results hold if instead we use the share of cotton in the total value of crop production.

³ In a footnote, Higgs and McGuire declare this evidence “completely irrelevant,” but in fact it is a basic part of the argument. Obviously we do not have independent evidence on antebellum yield variability, but Higgs and McGuire present no other explanation for this crop differential in profitability. Perhaps it is the unusually favorable characteristics of 1859–1860 as a cotton year which concern them (though they seem unaware of these elsewhere in the article). If so, then it should be noted that the differential is also found (though of smaller magnitude) in the very poor cotton year of 1849–1850.

war.⁴ Why was it “unacceptably costly to do otherwise” (than abandon self-sufficiency) *after* the war but not before? Higgs and McGuire allow only that “transactions costs deterred them from consistently maximizing returns on a year-to-year basis,” and we are left to infer that they believe the South’s failure to abandon self-sufficiency down to 1860 can be satisfactorily understood as a 50-year lag, compelled by unobserved “transactions costs.” They do not say this, but only because they do not face up to the issue. It is difficult to know what point Higgs and McGuire are attempting to make in stressing the superior expected earnings in cotton growing (the very position we defended and documented in the original article). They offer no explanation for the U-shape between farm size and cotton, nor for the correlation between cotton and tenancy, nor in particular for the fact that the majority of medium-sized and larger farms remained self-sufficient in foodstuffs in 1880.⁵

Although cotton–corn ratios are not critical to the argument they do have some significance. What we intended to convey was the fact that by 1880 the ratio had come to exceed *even* the level of the exceptional census year 1860, which is known to have been an unusually good cotton year but is reported to have been a poor-to-medium year for corn. We included the 1850 figure to suggest that the normal antebellum ratio was probably much lower than the 1860 level. A closer look shows, however, that 1850 was also not a typical cotton year, but one for which “the failure . . . was so great, that it is almost impossible to expect a like deficiency again.”⁶ The corn crop of that year, on the other hand, seems to have been normal. This characterization of the two census years is supported by both contemporary testimony and econometric evidence, and it is the main reason for the large increase in the cotton–corn ratio between 1850 and 1860.⁷ The

⁴ Stanley Engerman reminds us that such a shift into food crops was commonly observed after emancipation elsewhere in the Americas, wherever the freed slaves had unhindered access to the land.

⁵ Ransom and Sutch, 1972, p. 663. Higgs and McGuire do raise the possibility that what appear to be “small” sharecropping farms are actually part of much larger managerial units. They may be right, but they do not mention the evidence (which we cited) that *cash* tenancy is even more strongly correlated with cotton growing than sharecropping, and that the U-shaped pattern is present for cash renters and owner-operated farms as well.

⁶ U.S. Commissioner of Patents, *Annual Report for 1850*, Vol. 2, p. 510.

⁷ Residuals from cotton-supply-curve estimates range from +11.6 to +23.6% in 1859–1860, from –5.8 to –19.7% in 1849–1850 (see Wright, 1975, pp. 333–334). Contemporary testimony may be found in the 1849 Annual Report of the Patent Commissioner, pp. 144, 149, 170, 307, and in J. L. Watkins, (1908, pp. 81, 107, 150, 197, 217, 240, 258). Thorp (1926), characterizes 1849–1850 with the phrase “excellent crops except cotton” (p. 125), and a comparison of 1849–1850 output with the Patent Office estimate for 1848–1849 shows that the corn crop rose by more than 5% in the seven leading cotton states (12% if Texas is excluded), while the cotton output figures show a decline. Concerning 1859–1860, Thorp writes “enormous cotton crops” (p. 217); the exceptional picking season is described in *American Cotton Planter* (1860, pp. 163–164). On the corn crop for that year, see the citations in Gallman, 1970, p. 8.

argument is reinforced by reference to the only other comparable antebellum evidence, the state-level estimates of crop production prepared by the Patent Office during the 1840's, which show ratios averaging above the 1849–1850 level and below the 1859–1860 level in the seven leading cotton states of the Deep South.⁸ The key missing information is for the 1850s, but if we were to regard the 1850 and 1860 ratios as having equal and opposite biases, and take their midpoint as an estimate for that decade, we would have the following pattern:

	Cotton/Corn (000 bales/000,000 bushels)		
	5 states	7 states	11 states
1840s (6 obs)	26.62	24.48	10.91
1850s (2 obs)	25.50	23.91	14.59
1870s (10 obs)	30.99	27.38	17.22
1880s (10 obs)	37.71	28.82	19.91
1890s (10 obs)	37.12	32.94	18.34

Thus, what data we have do suggest that in the Deep South the Civil War decade marked a watershed in terms of output proportions as well as self-sufficiency, though we reiterate that this is not an essential precondition for the applicability of our argument. It is of course perfectly possible that the cotton–corn ratio was in fact rising during the 1850s, and we do not mean to deny this. Indeed, while Higgs and McGuire offer no explanation for such a trend, it is precisely what our model would predict, because the ratio of improved acreage to population was rising throughout the South during this decade. As more improved acreage becomes available above subsistence needs, we would expect cotton to get the largest share of the increase. This pattern of expansion, however, would not lead to the abandonment of self-sufficiency, and hence the postbellum experience cannot be viewed as a simple extension of antebellum trends.

THE CONCEPT AND MEASUREMENT OF RISK

Higgs and McGuire raise a number of objections to our contention and demonstration that cotton was a riskier choice than corn. Most of these objections stem from a failure to appreciate the implications of defining the decision variables in the way we did, viewing cotton growing as a means of

⁸ The usefulness of these estimates is examined, with generally favorable conclusions, by Gallman (1963, pp. 185–195). Note however that the corn estimates for Tennessee are especially suspect (p. 194) because the state did not come close to matching these levels until the 1890s. For this reason, and in order to exclude the areas of Tennessee, North Carolina, and Virginia which grew corn and raised swine commercially, we concentrate here on the states of the Deep South, which are the same as the seven leading cotton states of 1860 and 1880.

obtaining corn. Our variables x (cotton) and y (corn) were defined as follows:

$$x = \frac{Y_{\text{cot}} P_{\text{cot}} - D}{P_{\text{crn}}}, \quad (1)$$

$$y = Y_{\text{crn}}, \quad (2)$$

where Y_{cot} = cotton yield; P_{cot} = cotton price; Y_{crn} = corn yield; P_{crn} = corn price; D = cost differential per acre associated with growing cotton instead of corn. (We assumed that corn required no cash inputs.) The main reason for defining the variable x is to relate the variance of yields and prices to the subsistence target. We did not argue that cotton yields and prices happened to be more variable than corn yields and prices during a certain historical period, but that x will *generally* be more variable than y , because x involves a combination of yield risk and two kinds of marketplace risk. Because variances of ratios of random variables are analytically intractable, we chose simply to illustrate by direct calculation the magnitude of the riskiness differentials which were generated by this kind of pyramiding—not as a representation of the actual parameters of year-to-year decision making by farmers, but as a statistical rendition of the *logic* of the safety-first argument. It is the logic as much as the statistical result which prompts our confidence that antebellum relationships were much the same.

With this summary as a background, we can now show the irrelevance of what Higgs and McGuire believe to be “the critical problem”: our neglect of covariance between x and y . The important feature of our argument which they overlook is that the major types of covariance are already absorbed in our definition of the variable x . If cotton yields and prices are inversely correlated, or if cotton and corn prices are positively correlated, these effects will be reflected in the measured variance of x . The remaining covariance between x and y is not large, but it is definitely *positive*.⁹ The incorporation of a positive covariance into the analysis serves to strengthen our position, by providing the rationale for complete specialization in corn by risk-averse farmers. To see this, note that the allocation of acreage which minimizes the overall variance (using the formula in Higgs and McGuire’s footnote 14) is given by

$$Q^* = \frac{S_y^2 - \text{cov}(x,y)}{S_x^2 + S_y^2 - 2 \text{cov}(x,y)}, \quad (3)$$

where Q is the share of acreage in cotton, S denotes standard deviation, and x and y are defined as above. If we ignore the covariance and use the data in

⁹ The average state-level correlation coefficient is 0.35, which is to say that less than 15% of the variance in x is “explained” by the variance in y .

our Table 5, we obtain figures for Q^* ranging from 2.2 to 8.2%. However, if we add a correlation of 0.35, expression (3) becomes negative for every state, which is to say that the variance is in fact minimized by planting all corn. Furthermore, for any Q greater than Q^* (in this case for any positive Q), the variance will be monotonically related to Q .¹⁰ Thus, contrary to Higgs and McGuire's apparent belief that consideration of the covariance will completely wipe out the results, the existence of a positive covariance only augments the proposition that choosing cotton at the expense of self-sufficiency was risky.

The lesser objections may be dealt with more briefly. While the appropriateness of computing the variance for each crop around a time trend could certainly be debated, we will not pursue the matter since detrending makes not the slightest dent in the results.¹¹ As for the coefficient of variation (ratio of standard deviation to the mean), Higgs and McGuire refer to this as the "correct statistical concept," but without discussion or citation. The use of the coefficient of variation is analytically necessary only when the two crops are measured in different units; but since we have defined x and y in terms of the same units (bushels of corn), the standard deviations can be meaningfully compared independently of the means. It is the standard deviation (or variance) which enters into the basic portfolio formulas and which is used in conventional mean-variance analysis.¹² The use of the coefficient of variation would involve an implicit homogeneity restriction on the risk-return preference function.

Despite Higgs and McGuire's persistent efforts to push us back into a portfolio-balance framework, however, the real measures of risk in our model are the probability of falling below the target yield Z^* and the relative

¹⁰ Using the same formula, we have $dS_{\tau^2} dQ = 2QS_x^2 - 2(1-Q)S_y^2 + 2\text{cov}(x,y)(1-2Q)$ which will be greater than zero if $Q > Q^*$ as in Eq. (3).

¹¹ If a linear trend is fitted to variables x and y , the *difference* between raw and detrended standard deviations for each state is as follows:

	Cotton	Corn
Alabama	0.51	0.04
Arkansas	1.30	-0.02
Georgia	0.16	0.05
Louisiana	-0.12	0.04
Mississippi	-0.09	0.04
North Carolina	-0.02	-0.01
South Carolina	-0.07	-0.01
Tennessee	3.26	-0.03
Texas	2.04	-0.06

Comparison with Table 5 in the original article will show that no conclusion is altered. (Note that because of a typographical error, the standard deviations for cotton and corn for South Carolina were reported as being identical to North Carolina's. South Carolina's figures should be 6.31 and 1.41, respectively.)

¹² See, for example, Fama, 1971; Fama and MacBeth, 1973.

costs of such a shortfall. We specifically did *not* argue that postbellum farmers “bore more risk” than antebellum farmers in the sense of facing a higher probability of actual starvation (p. 540). We argued instead that a large fraction of postbellum farmers were more *willing* to bear risk (pp. 528, 539) in the sense of a higher probability of not breaking even on their food requirements (or a higher variance in that portion of their productive activity which went toward satisfying these requirements), because the relative costs and benefits of shortfalls and surpluses had changed, and because the target yields were higher.¹³ We called this “gambling” because these risks were borne in the hopes of attaining “an exceptionally large payoff,” and we were at great pains to caution that this term could encompass several different underlying psychological perspectives on the part of farmers (p. 550).

OFF-FARM EMPLOYMENT OPPORTUNITIES

In our model, the riskiness of specialization in cotton is a function of the options available in case of a failure to meet the subsistence target. Higgs and McGuire argue in essence that these risks were not serious, because the postbellum Southern farmer “possessed ample opportunities for supplementing his income outside his own farm.” It is certainly not true that there were literally no such opportunities, and where part-time employment was available the logic of crop choice would surely be different. But the handful of postbellum citations offered by Higgs and McGuire do not add up to a demonstration that off-farm employment was “ample,” and there is substantial testimony to the contrary. Rural employment opportunities were limited because of the weak development of commerce and manufacturing in the Southern countryside, and because the seasonality of labor requirements in cotton (and the lack of alternative cash crops) meant that agriculture could not generate wage-paying jobs except when the farmer’s labor was needed on his own farm. Occasional odd jobs around the plantation were too uncertain and irregular for farmers to count on them as a means of meeting subsistence requirements and debt payments. Of course able-bodied men could go *somewhere* in the South and expect to find work (with some probability), but these choices were effectively ruled out of the portfolios of most Southern farmers by what we may loosely call transactions costs—a category in which we would include not only information and distance, but the distaste for being separated from one’s family and for doing hard, disagreeable gang labor. Readers who look

¹³ We might have clarified the argument by distinguishing the self-sufficiency target from the *cash* constraint which many small farmers faced for the first time after the war. Most small farms could meet a target yield determined solely by subsistence requirements, but on a limited acreage they could not satisfy *both* of these constraints, and self-sufficiency had to give way, probabilistically speaking.

closely at Higgs and McGuire's suggested "nonfarm employment" will find a list of jobs which is highly specific geographically (and largely peripheral to the cotton belt) and which possesses the aforementioned qualities in abundance.

To support these assertions, one need go no further than Higgs and McGuire's citations. They cite, for example, the testimony of Mr. L. W. Youmans of Fairfax, South Carolina, who ran his 1300-1400-acre plantation with labor hired by the day.¹⁴ Without belaboring the fact that this was a rather atypical arrangement in the Cotton South, should we not also take note of the fact that in the prosperous 20th-century year of 1901, Mr. Youmans reported that "there is a great deal more of it [labor by the day] than I have any need for" (p. 117), and went on to say that his employees "always want work. . . . They want employment. The trouble with me is trying to find work for them" (p. 122). Youmans described his "colored tenants" coming to him in winter, without food, asking for credit or work, and he allowed that "if I can arrange work for them I always do" (p. 122). This is how the tenant farmer "supplemented his income."

Another citation tells about a large wage-paying sugar plantation in Louisiana, to which "small cotton croppers" came during the grinding season, after the cotton harvest.¹⁵ These croppers obviously had to come some distance to take these day-labor jobs, and the writer states that they do so, "*there being no other work for them to do in the cotton section*" (p. 110; our italics). He also observes: "They very much dislike the gang system of labor and roam all over the country seeking job work, when they can work as they please" (p. 119). These were the sorts of "ample opportunities" which Southern tenants "commonly grasped." Higgs and McGuire even cite employers' complaints that sharecroppers abandoned their crops after receiving advances, or after deciding prospects were poor—behavior which led landlords frequently to insist on retaining contractual control over their tenants' labor time.¹⁶

The fact that small farmers could find a way to survive if they had to is in no way inconsistent with our model. But the kind of options which Higgs and McGuire document represents exactly the sort of unpleasant alternative which, in our view, family farmers were trying to avoid by maintaining or working towards farm ownership and self-sufficiency. Much of the economic history of the Southeast in the 20th century has recounted

¹⁴ Report of the U.S. Industrial Commission, 1901, Volume X, *Agriculture*, pp. 117-122.

¹⁵ Laws, 1902, pp. 95-120.

¹⁶ Their citations are to the 1880 census, *Report on Cotton Production*, and to the 1885 Senate Committee, *Report on Relations between Labor and Capital*. Each of these documents, it might be noted, contains substantial testimony on the overproduction of cotton, indicating that these complaints were not limited to the 1870s and 1890s. Two of the best examples are the statements in the U.S. Senate Report by W. H. Gardner of Alabama (pp. 75-76) and John Peabody of Georgia (pp. 555-557). For an example of a sharecropping contract which restricts labor mobility, see Taylor, 1943, pp. 122-123.

the dramatic ways in which agricultural resource allocation changed when relatively modest wage-paying opportunities penetrated rural counties.¹⁷ These studies generally argue that the coming of employment opportunities brought an increase in part-time farming, a reduction in rural under-employment, and little if any decline in farm output. If labor markets were so imperfect so late in the 20th century, can we reasonably believe they were better in 1880?

INTERPRETATIONS OF HISTORY

We conclude with some brief observations on the general methodological and interpretive points raised by Higgs and McGuire. They are skeptical of safety-first and other models which are not rooted in von Neumann–Morgenstern axioms of rational behavior; they object to our characterization of the shift into cotton as “quasi-voluntary” on the general grounds that “actions are no less voluntary simply because they are constrained”; they argue that the “abandonment of self-sufficiency” is quite consistent with the elementary principles of efficiency, specialization, and comparative advantage, and hence these principles provide an interpretation symmetrical with the accepted analysis of developments in the North. For reasons already given, it is not clear exactly what Higgs and McGuire mean by the “orthodox choice-theoretic interpretation,” since they have offered no explanation for most of the phenomena we described. But it seems a fair statement of their position that one should adopt the interpretation given by elementary price theory so long as it suffices, moving into “unorthodox and untried” behavioral explanations only if it is established that “an interpretation based on the straightforward maximization of expected utility is incapable of explaining the data.” This is, at any rate, a frequently heard view with which we disagree.

It is not a crucial point for us whether lexicographically ordered preferences are consistent with the axioms of expected utility theory. We do not shrink from defending the propositions that in the real world individuals do have specific goals and objectives, that these objectives have different degrees of importance and priority, and that economic actors have computational limitations and typically follow simplified strategies, rules-of-thumb, or decision rules in pursuit of their goals. Substantial evidence has now accumulated concerning the difficulty with which individuals absorb and process information about choices and risks, and the extent to which they do rely on simplified principles to guide their behavior.¹⁸ Empirical evidence from the study of behavior with respect to low-probability events indicates that individuals frequently do not have

¹⁷ Tang, 1958, esp. Chap VI; Nicholls, 1961, pp. 319–340; Parker and Davies, 1962, pp. 121–129.

¹⁸ This evidence is summarized in Slovic, Kunreuther, and White, 1974.

enough information to make decisions in the manner described by expected-utility theory, and that those who do possess such information frequently behave in a manner inconsistent with expected-utility theory.¹⁹ We believe that incorporating these behavioral principles explicitly into econometric research is of positive benefit in opening the possibility of fruitful interaction between economists and managers, survey takers, and farmers (etc.), providing more and not less empirical specificity to the analysis.

The methodological issue appears to be a question of the burden of proof between competing explanations, but we believe this to be a misleading description. We chose to focus on the change in self-sufficiency between 1860 and 1880 precisely because there did not seem to be a straightforward explanation in conventional price theory, and our model seemed to fit the evidence. In the considerable amount of comment we have received on this article (and on related work), we have frequently been urged to show how our explanation is superior to an "orthodox" explanation. Higgs and McGuire are unique in claiming that there is nothing to be explained, but neither they nor anyone else has offered an "orthodox" explanation for the empirical phenomena we describe: the end of self-sufficiency between these dates, the change in the shape of the farm size-crop mix relationship, and the U-shaped pattern itself. We begin to feel we are wrestling with a phantom, and that it is not so much a question of burden of proof as a way of looking at the world.

This is not to say that "orthodox" theory is "incapable of explaining the facts." The present case is apparently something of a challenge, but presumably one could find a change in some incentive variable which favored cotton; one could compute the appropriate "response coefficients" as they varied by farm size, if necessary with lags that varied by farm size as well; one could infer that there must have been some technological change in the "optimal crop mix" by farm size; or at the very most, one could allow that there had been some change in the shape of the utility function over time, perhaps a result of the aftermath of the war and emancipation. By devices such as these, economists have convinced themselves that their theoretical presuppositions have been repeatedly confirmed by the evidence, when in fact they have only rationalized the facts in terms of a particular way of viewing the world.²⁰

¹⁹ Kunreuther, 1976.

²⁰ This viewpoint is ably outlined with reference to the economists' treatment of "tastes" by Foley (1975). The objection to our use of terms like "compulsion" is a case in point. Higgs and McGuire argue in essence that no one is ever "forced" to do any economic activity, but this narrow definition can hardly be the "commonly understood sense" of a term so frequently applied. Because they do not examine the process of change over time, they do not even distinguish individuals whose opportunity sets have *contracted* from those whose choices have *expanded*: if an option previously exercised is closed off by a new constraint, a reasonable man will say that he has been "forced" into his new position.

This *weltanschauung* tends to produce an isolated and unhistorical economic history, not well integrated with the broader themes of traditional history nor the subtler perceptions of contemporaries. Higgs and McGuire ask: "Why have economic historians failed to produce a literature bemoaning the failure of Pennsylvania or New York or Massachusetts to remain self-sufficient in foodstuffs?" We are not responsible for what makes economic historians tick, but it is a narrow knowledge of history which is unaware of the parallel doubts, fears, and reluctance with which Northeastern farmers abandoned self-sufficiency. It was not a Southern reformer, but a New England farmer who urged in 1852:²¹

As a general rule, however, it is better that the farmer should produce what he needs for home consumption. . . . He may obtain more money from tobacco, hops or broom corn, than from breadstuffs, but taking all things into consideration, will he be better off?

The critical strategic and class implications of "access to the means of subsistence" are a common theme in history generally, not just in the South.²² The reason why *regional* self-sufficiency was uniquely important in the South was given in our original article (pp. 544–546, 551), namely, that the Southern cotton crop dominated the world market, and for this reason the region as a whole actually lost from the shift. The reasons why the microeconomic issue of self-sufficiency was less prominent in the North are also suggested by the analysis: Northern farmers could choose from a greater variety of cash crops, and most of their choices were also basic food crops, so that they did not face as severe and as stark a choice as did Southerners; and in contrast to cotton, the Northern crops remained in strong and growing demand (with notable exceptions late in the century), thus ratifying and reinforcing the risky choices farmers had taken. Thus, far from producing an undesirably asymmetric analysis, we argue that it is a virtue of our approach that it provides insight into divergent patterns of regional behavior, attitudes, and development.

Finally, we commend these models to the sympathetic attention of historical economists, because they lend themselves to interpretations in which history really matters, in the sense that "initial conditions" do affect outcomes, and the precise historical sequence of events makes a difference. In our view, it was critically important that the 1860s saw a *coincidence* of emancipation, cotton famine, the failure to distribute land to freedmen, and the onset of an era of stagnation in cotton demand. The absence of any of these ingredients would have altered the pattern of

²¹ *Plough, Loom and Anvil* IV (1852), quoted in Danhof, 1969, p. 23. Danhof's footnotes are misnumbered, but the pages of his book are filled with documentation for these attitudes.

²² As surveyed in Tilly, 1975. An argument which stresses the political consequences of abandoning self-sufficiency in Northern agriculture appears in Mayhew, 1972.

subsequent development. In our view, the historical record of Southern agriculture does not so much trace the course of a moving equilibrium, but shows us instead the sequence of outcomes of some risky choices in a game of chance played a relatively small number of times: a game in which each year's choices are constrained by the cumulative legacy of the previous history. Of course, our particular analysis could be wrong, and certainly it deserves to be looked at with the critical energy Higgs and McGuire have mobilized. But if we adopt the methodological position which they recommend, economic history will never have a distinctive contribution to make to the field of economics. We will remain camp followers to the profession, generating numbers of antiquarian interest, and cranking out "orthodox" interpretations which will convince economists, if they bother to look, that they are missing nothing essential by ignoring history.²³

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²³ Two recent contributions which develop these arguments more fully are McCloskey (1976), and still more aptly for the prediscussion, David (1974, Introduction).

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