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The Strategic Role of Theory: A Commentary

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The Strategic Role of Theory: *A Commentary*

I

For the economist-historian there are three abiding critical issues:

What is the question for which an answer is sought?

What is the appropriate theoretical framework?

Where can the data be found to fill the empty boxes?

I shall focus first on the strategic role of the theoretical framework. It is the appropriateness of the theoretical framework to the question posed which determines the ultimate relevance of the answer; and it is also the character of the theoretical framework which determines the kind of data sought and mobilized. In the end, I shall have something to say about further questions that might usefully be addressed as we move forward in the development of our craft.

II

To be an economist-historian is to accept from the beginning an inescapable tension. An economist strives to build or use a model of maximum simplicity, with a minimum number of variables which his model relates to each other in a straightforward way. An historian strives to deal with things that happened at a particular place over a given period of time in their full complexity.

D. H. Robertson was correct in his response to me in early 1950 when I told him I was writing a book on *The Process of Economic Growth*. He wanted me to be sure I knew what I was getting into; and he urged me to go back and read Marshall's Appendix J in *Money, Credit and Commerce*. Along with Appendix H in *The Principles*, Marshall demonstrated clearly the inelegant world of interaction and endless disequilibrium which is the long period; and all modern economic history is embedded in the Marshallian long period.¹

This tension between the model chosen and the disarray of reality is both our glory—as compared to the pure theorist or institutional historian—and it is our basic limitation. In citing now some examples of limitations imposed by this tension, I do so not to score points—for I believe the problem is inevitable—but to illustrate some directions in which the linkage of theory to major historical problems might be improved. For that, I take it, is a mission shared among us.

*Tsiang on Stages of Growth.*² At one end of the spectrum, Swanson

¹ See *The Process of Economic Growth*, second edition (enlarged) (Oxford: The Clarendon Press, 1960), pp. 4-9, for an elaboration of this theme.

² S. C. Tsiang, "A Model of Economic Growth in Rostovian Stages," *Econometrica*, 32, No. 4 (October 1964), pp. 619-648. Since the version of Tsiang's paper was first delivered in September 1962 and *The Stages* was published in 1960, the translation

and Williamson illustrate what the problem is about for the pure theorist wrestling with a model built by an economist-historian. They note: “. . . it took a theoretician like Tsiang a number of years before he was able to fully appreciate the implications of Professor Rostow’s interpretation of the growth process” (p. 4). I have an exceedingly high regard for Tsiang’s elegant paper; but, as he lucidly points out, he constructs “a simple aggregate economic model . . . abstracting all socio-political factors and intersectoral changes.” Tsiang’s model does illustrate certain aspects of *The Stages of Economic Growth*, but it does so at the expense of eliminating a good deal of the dynamic sectoral theory of production, elaborated in *The Process of Economic Growth* on which *The Stages* is based. Neither the succession of leading sector complexes—each subject to deceleration—nor the role of the income elasticity of demand nor the changing quality of entrepreneurship come clearly through in his model.

We can, indeed, manipulate in imaginative ways the assumptions built into aggregative models covering “technical progress” by introducing deviations from “neutrality”; and we can, for certain purposes, even accept Tsiang’s simplification, which would treat a favorable shift in the quality of entrepreneurship as a form of technical progress. But we are still some distance from a formal, mathematical growth model that relates the movement of the aggregates to the minimum range of technical, sectoral, and entrepreneurial variables that an economic historian requires, if he is to answer his minimum questions about growth.

David on U.S. Growth Before 1840. A related but distinct problem is raised by David’s analysis of American growth rates before 1840,³ referred to in Fishlow’s paper. David’s model is less aggregated than Tsiang’s; in fact, the heart of his proposition is a two-sector industry-agriculture model. For pre-1860 United States such a model is, I believe, well worth elaborating in quite formal terms as a framework for dealing with the alternating succession of surges in agricultural and industrial expansion.

The limitation in David’s argument—insofar as it bears on *The Stages of Economic Growth*—is that he fails, in the end, sharply to distinguish increases in output per capita induced by the coming in of new rich land (plus agricultural innovations) from those brought about by new production functions in industry.

In the original take-off article,⁴ I discussed the case of increasing output per capita in nations with a highly favorable balance between population and rich land:

time-lag does not seem excessive. Tsiang notes (p. 620n.) that he was in fact working along the lines of his model from 1958. It is the kind of problem posed by Tsiang’s model that Solow and I discuss in general in the Konstanz volume [W. W. Rostow, editor, *The Economics of Take-off into Sustained Growth* (New York: St. Martin’s Press, 1963), pp. 468-74. (Solow); pp. xxiv-xxvi (the author)].

³ Paul David, “The Growth of Real Product in the United States before 1840: New Evidence, Controlled Conjectures,” *THE JOURNAL OF ECONOMIC HISTORY*, XXVII (June 1967), pp. 151-197.

⁴ W. W. Rostow, “The Take-off into Self-Sustained Growth,” *The Economic Journal*, LXVI (March 1956), pp. 38-9.

Theoretically, such fortunate societies could continue to grow in *per capita* output until diminishing returns damped down their progress. Theoretically, they might even go on as growing nonindustrial societies, absorbing agricultural innovations which successfully countered diminishing returns. Something like this process might describe, for example, the rich agricultural regions of the United States. But, in general, it seems to be the case that the conditions required to sustain a progressive increase in agricultural productivity will also lead on to self-reinforcing industrial growth.

My general point is this: We all start with modern growth as rooted in the generation and diffusion of new technologies to particular sectors and sub-sectors; but we move over easily to the world of highly aggregated analyses of output and income; and we do not yet have satisfactory tools for linking these related but not identical concepts, although Austrian capital theory, with its distinction between capital widening and capital deepening, offers a fruitful clue.⁵

Temin on the Cotton Price in the 1830's. I turn now to an observation on Temin's analysis of the 1830's, referred to at several points in the papers for this meeting. I sympathize wholly with his view that Jackson's policy was a marginal factor in the rather majestic transatlantic boom and bust of that decade. And he is right in focusing on the critical role of the international cotton price in general, the British demand for raw cotton in particular; although I suspect there was more lumpiness in the bringing in of new cotton acreage than his analysis would allow, and there is quite a lot in North's analysis of the discontinuity in the supply factors at work.

Temin's critical demand equation, relating the British harvest to the cotton price, is not satisfactory for two reasons.

First, the link between good and bad harvests and the British demand for raw cotton is not simple.⁶ The British harvest bears in this period a systematic and rather complex relation to real wages, the course of the business cycle, the level of income, and the effective demand for textiles.⁷ Year-by-year movements in the level of employment and real wages are

⁵ François Crouzet makes essentially this distinction in comparing the relative expansion in Britain and France of the eighteenth century ["England and France in the Eighteenth Century: A Comparative Analysis of Two Economic Growths," Chapter 7 in R. M. Hartwell, editor, *The Causes of the Industrial Revolution in England* (London: Methuen, 1967)]

⁶ Peter Temin, "The Causes of Cotton-Price Fluctuations in the 1830's," *The Review of Economics and Statistics*, XLIX, No. 4 (Nov. 1967); also, *The Jacksonian Economy* (New York: W. W. Norton, 1969).

⁷ See, notably, A. D. Gayer, *et al.*, *The Growth and Fluctuation of the British Economy, 1790-1850*, II (Oxford: Clarendon Press, 1953), pp. 563-65 and pp. 792-94 on the behavior of wheat imports, whose fluctuations, with all their consequences for the state of the money market, represented one route for the impact of the British harvest on the state of the British economy as a whole. For relative rates of increase in British cotton exports and the American cotton crop, in relation to the raw cotton price, see Vol. II, pp. 838-841. For an account of the British economy as a whole in the 1830's, see Vol. I, Chapter V, pp. 242 ff.

required to "explain" the cotton demand, not merely the British harvest. The rate of growth of British cotton exports is relevant; for example, the expansion in cotton yarn and twist produced between 1831 and 1836 was 44 percent for the export market, 14 percent worked up for the British market. The absolute increases were similar for the two markets (27 million pounds and 30 million pounds, respectively). And, given the pace of productivity increase in the cotton industry of this period, the movement of supply as well as demand curves is also relevant to the industry's raw material requirements.

Second, there is the question of cotton stocks. Their behavior raises a more basic question about Temin's hypothesis. Cotton stocks move downward from 1828 to 1834 (with a slight rise in 1830) at a period when British cotton consumption was generally rising;⁸ that is, they decline before the good harvests bring down the wheat price in the period 1833-35. Stocks increase in 1835 and 1836 (as annual averages), although pressure on stocks emerged again at the close of 1835 which helped move the cotton price to its peak in April 1836—incidentally, some three months before Jackson's specie circular. There were, in fact, speculative peaks in the cotton price in 1833, 1835, and 1836—the latter at almost exactly the level of the previous year.

The proportion of cotton stocks to imports falls from an 1828 peak of 64 percent to 45 percent in 1830 to a trough of 25 percent in 1834-35, rising to 29 percent in 1836-37. The raw cotton price picks up at least from 1831; that is, there is a rise during the business cycle contraction of 1831-32. And one could even date the end of the long price decline from 1818 as early as 1828.

⁸ *Ibid.*, Vol. II, p. 688, and Vol. I, pp. 263-65. For the critical years of the 1830's here are the relevant annual data:

	Quantity of raw cotton imports (in million lbs.)	Stocks of raw cotton	Proportion of cotton stocks to quantity of imports	Price of raw cotton including duty (pence per lb.)	Price of cotton piece goods exports (d. per piece)
1827	272	164	60%	6.8	7.52
1828	228	147	64	6.3	7.12
1829	223	116	52	6.5	6.31
1830	264	119	45	6.8	6.43
1831	289	114	39	6.7	6.09
1832	287	104	36	7.2	5.42
1833	304	94	31	8.9	5.41
1834	327	82	25	9.1	5.51
1835	364	90	25	10.5	5.97
1836	407	116	29	10.2	5.91
1837	407	116	29	7.6	5.10

The price of slaves in Virginia and New Orleans began to rise (after a protracted decline) in 1830; in Georgia, 1831; in Charleston, 1832; Ulrich B. Phillips, *Life and Labor in the Old South* (New York: Little, Brown & Co., 1929), p. 177.

We are clearly confronted here by pressures of demand on supply which antedated the good harvests and general business expansion of the 1830's. My general points, then, are: (1) the analysis of short-period changes in demand must be put into their full cyclical context, including fluctuations in export as well as domestic demand and including the effects on supply curves of current cost-reducing investment, with their impact on the price of manufactured goods as opposed to related raw materials: harvests and the cotton demand simply do not suffice. (2) The analysis of individual price movements requires a sensitive linking of demand and supply which may well transcend cyclical fluctuations.⁹ We need a much longer look than Temin's to understand the transatlantic dynamics of the boom and slump of the 1830's. And (as Thomas Tooke's knowledgeable market analyses suggest) the position of stocks deserves the attention of the historian or econometrician concerned with price movements over short time periods in Britain of this period.

Fishlow on the Railroads. Now two illustrations of my theme drawn from Fishlow's remarkable and tenacious effort to track out the full consequences of railroad building in pre-1860 United States. The first concerns a variable dropped from his equation; that is, the lateral effects spreading out from leading sectors.¹⁰ On this concept Fishlow has the following to say:¹¹

The 'lateral' effects of Rostow, by which he means the induced 'set of changes which tend to reinforce the industrialization process on a wider front,' might

⁹ My essay on "The Terms of Trade in Practice" (*The Process of Economic Growth*, Chapter ix) deals at some length with the determinants of the cotton price in this period, pp. 201-205. And it seeks, in general, to specify the variables required for an econometric "explanation" of price movements.

¹⁰ In *The Economics of Take-off into Sustained Growth* (pp. 5-6), I defined lateral effects as follows:

. . . the leading sector will induce around it a whole set of changes which tend to reinforce the industrialization process on a wider front. Modern industrial activity surrounded itself with urban men, services, and institutions whose existence strengthened the foundations for industrialization as an ongoing process: a disciplined working force organized around the hierarchies decreed by technique; professional men to handle the problems of law and relations to the various markets for input and products; urban overhead capital; institutions of banking and commerce; and the construction and service industries required to meet the needs of those who manned the new industrial structure. The coming in of a new leading sector thus often transformed the whole region where it took hold; as, for example, the cotton textile revolution transformed Manchester and Boston and the automobile industry transformed Detroit. Wherever they went, the railroads induced the transformation of old urban centers or the creation of new ones, not merely for railroad maintenance but also to handle the marketing and commercial traffic that the railroads made possible and profitable. These lateral effects—symbolized by the acceleration in urbanization during take-off—expanded the proportion of modern folk in the total population and strengthened modern attitudes towards the production process far beyond the narrow impact of the new activity itself and the inputs it directly induced.

¹¹ Albert Fishlow, *American Railroads and the Transformation of the Ante-Bellum Economy* (Cambridge, Mass.: Harvard University Press, 1965), p. 16n.

seem to be another possibility. But these are so general as to constitute the very process of industrialization and are a *consequence* of the other effects rather than an additional route of influence.

I do not believe this comment quite disposes of the matter. The proportion of the urban population about doubled between 1840 and 1860, having taken the previous half-century to double.¹² In all conscience, the lateral effects symbolized by this acceleration are extremely difficult to quantify as distinct from other forces at work; and the direct and indirect role of the railroads in this process (which I would guess is substantial) may be impossible to isolate statistically. But difficulties of measurement do not justify dropping factors from the equation. The acceleration of urbanization in the period 1840-60 was, evidently, the result of other factors; but growth is an interacting process, and rapidly enlarging cities, in turn, played back on the economy, reinforcing the industrialization of the economy in many directions. I do not believe lateral spreading effects can be dealt with in quite the cavalier way Fishlow's footnote suggests.

Second, Fishlow's interpretation of the 1840's in general leaves me uneasy and feeling we all have more work to do. He is correct in observing that, after the boom and bust of the 1830's (based on the expansion of cotton and other acreage, plus infrastructure expansion in the mid-West), capital in London and the eastern United States was inclined to stay closer to home in the 1840's. The British domestic railroad boom and the intensive railroadization of the American northeast were concurrent phenomena. But neither the availability of capital for eastern investment nor his observations on the limited role of transport development in relation to textiles, boots and shoes, coal and iron, quite support his conclusion:¹³ "One could write an independent history of manufactures and railroads in the 1840's; one could not do the same for western expansion and railroads in the 1850's. This contrast is the important point."

There is no doubt that the economics of linking up new agricultural lands with railroads differs from the economics of linking up existing commercial and industrial centers. But an increase of railway mileage

¹² The proportion of urban to total population in the United States moved as follows from 1790 to 1860:

1790	5.1%
1800	6.1
1810	7.3
1820	7.2
1830	8.8
1840	10.8
1850	15.3
1860	19.8

Data for 1790-1810 from W. S. Woytinsky and E. S. Woytinsky, *World Population and Production: Trends and Outlook* (New York: The Twentieth Century Fund, 1953), p. 124; for 1820-1860 from Abram Bergson and Simon Kuznets, editors, *Economic Trends in the Soviet Union* (Cambridge, Mass.: Harvard University Press, 1963), p. 72.

¹³ A. Fishlow, *American Railroads*, p. 261.

of 211 percent in a decade for the nation (a quadrupling in the major industrial region, New England) is both too massive and too interwoven with the whole life of the economy to be separated from all the rest that happened in the 1840's. I suspect that the heart of his argument is that industrial growth in the 1840's would not have been "stymied"¹⁴ if the railroad boom had not occurred. I would guess that this is true; but that is not quite the same as arguing that the industrial and railroad development, which actually did occur, were substantially "independent."

The difficulty here for the analyst is real; and, understanding the labor that went into Fishlow's Chapter VI, I am not inclined to be over-critical. The difficulty is that the effects of a railroad boom of the kind experienced in the 1840's are much more diffuse than, say, the throwing of railroads into new wheat lands. The expansion of investment and income is general, and hard to track out except in aggregative terms. The effects of transport cost reductions for freight and passengers are, again, diffuse, but they could be powerful, even if they did not break a dramatic bottleneck in growth. In any case, having, as it were, lived through the British railway boom of the 1840's,¹⁵ I am skeptical that the railway development of the American northeast of the 1840's was as divorced from industrial development in that decade as Fishlow suggests.

Friedman and Temin on the Great Depression of the 1870's. Now the decline in prices from 1873 to the mid-1890's. Temin, on this point, merely refers to the counterrevolution of the monetarists and poses some questions about an analysis of the price decline which would link an increased savings rate, the equilibrium capital-output ratio, the interest rate, and the time required for the requisite expansion of the capital stock. I wish him well in this line of enquiry; although, as you would suspect, I am sceptical of an analysis that does not deal with the composition of investment as well as its scale.

I would, however, underline briefly two aspects of my view of the matter to which Temin refers, because they bear on the subject of this paper. First, I did not take the fall in the interest rate as *prima facie* evidence that the monetarist view was incorrect. I considered carefully the proposition that the falling rate of interest might, in some sense, still have been too high.¹⁶ And I examined the cyclical patterns, year by year, to see what role, if any, monetary stringency might have played in producing premature downturns and prolonged slumps. I concluded as a matter of fact that monetary stringency did not play a significant role. Long-term

¹⁴ *Ibid.*, p. 250.

¹⁵ A. D. Gayer, *et al.*, *Growth and Fluctuation*, Vol. I, Chapter VI.

¹⁶ W. W. Rostow, *British Economy of the Nineteenth Century* (Oxford: Clarendon Press, 1948), Chapters III, IV, and VII, especially pp. 60-61, 83-88, and 148-59. A more detailed examination of the issue is contained in my unpublished doctoral dissertation, *British Trade Fluctuations, 1868-1896: A Chronicle and a Commentary*, Yale University, 1939, especially pp. 471-487. For the same issues considered for the period 1815-1850, see A. D. Gayer, *et al.*, *Growth and Fluctuation*, Vol. II, Chapters IV and V.

trends in history are not neat, straight lines. They emerge from the averaging out of short period behavior. I am not inclined to credit a long-period force as operative unless it can be identified as operating over short periods of time, in concrete circumstances.

Second, I offered a quite substantive alternative explanation of the price decline, which I shall not take the time to repeat here. It was rooted in a dynamic theoretical structure which aimed to relate systematically monetary and non-monetary factors in price analysis. Right or wrong, my explanation of the price decline was positive and dynamic, not merely a static and negative verdict on the role of money supply and interest rates.

No issue in economic history has been debated with more sophistication by a more distinguished cast of characters than the price decline of these years: Fisher and Cassel; Giffen, Marshall, and the London *Economist*; Wicksell and Keynes of *The Treatise on Money*; Layton and Crowther. In the light of this debate, I found Friedman's bland reassertion of the Cross of Gold doctrine somewhat surprising. He has every right to revive that doctrine; but I would have thought the quite substantial lions in the path deserve his attention.

As I read Friedman's observations on this point, I recalled the conclusion of my review of the British debate:¹⁷

Perhaps the most useful lesson in this rehearsal of explanations is not for economists, but for historians. Men observing honestly the same set of data emerged with quite different explanations. Each explanation depended directly on theoretical presuppositions. But more than that, the data selected as relevant depended on those presuppositions. The royal commission reports contain pages of statistics on price movements, practically no statistics of investment. Fisher and Cassel were content with prices and gold stocks. Keynes of the *Treatise* was content with prices and interest rates. None of the theorists (with the possible exception of Wicksell in his reference to rising real wages) could talk about interest rates and costs at the same time, or, at least, not in the same chapter.

In many historical terrains convention has marked out useful frameworks of organization, containing implicit assumptions of relevance and cause, which leave the writer free to exercise his talents for the collection of fact. The economic historian, if he is to go beyond the great institutional studies of the line which runs from Thorold Rogers to Clapham, must concern himself consciously with the problem of adapting such a framework from the corpus of current economic theory.

I do not believe now—as I did not believe then—that the quantity theory of money—no matter how sophisticated its formulation—is a sufficient framework for the economist-historian in dealing with price movements.

The clue to Friedman's analysis is embedded in a footnote where he asserts:¹⁸ “. . . a more rapid increase in the monetary stock . . . would

¹⁷ W. W. Rostow, *British Economy of the Nineteenth Century*, pp. 159-60.

¹⁸ Milton Friedman and Anna Jacobson Schwartz, *A Monetary History of the United States, 1867-1900* (Princeton: Princeton University Press, 1963).

have meant a more rapid decrease in interest rates, and hence . . . would have meant an expansion in investment and so in money income. . . . the declining interest rates may very plausibly be interpreted as in part a reflection of the declining prices and as in turn increasing the rate of growth of the money stock required to keep the price decline at any given level." Like the monetary crusaders of 1815-48 and 1873-96 he is simply arguing that a different monetary policy would have produced a different outcome for the level of income, employment, and prices. And so one could argue also for a backward projection of modern fiscal policy, as Keynes, by implication, once did.¹⁹ But that is a quite different matter than explaining what did happen to prices in monetary terms.

Temin on the Great Depression of the 1930's. As you will have perceived, my comments on Friedman are similar to Temin's perceptive observations on the debate over the depth of the American depression of the 1930's. I wholly agree with his dictum: ". . . there is no single correct counterfactual statement to be made about the depression. Both monetary expansion and fiscal expansion undoubtedly would have acted to moderate or even avoid the depression, and reasonable men may disagree on the relative efficiency of these tools." And I agree also (as I would for the late nineteenth century and the period 1815-48) that such counterfactual exercises must face up to the political as well as technical context of their arguments.

I would, on this subject, only underline a point I have made elsewhere, which, I believe, ought to be taken into account as we fashion a theoretical model appropriate to the problem:²⁰ the leading sectors of the boom of the 1920's were the automobile-durable consumers goods complex and the related movement to suburbia, with all its implications for housing and road construction. The momentum of these sectors depended on a continued expansion of consumer's income. Once depression occurred—and lacking built-in income stabilizers—an exogenous rise in consumer's

¹⁹ In a letter to me of February 2, 1940, Keynes wrote (in connection with the initial publication of "Explanations of the Great Depression" in *Economic History*, 1940):

Reading about my own opinions as set forth in the *Treatise* does indeed feel like an historical exercise! One of the many elements which I entirely omitted then and certainly should stress now is the greatly increased volume of investment required to produce full employment, as the years went by after 1870, owing to the great growth in wealth. The opportunities for profitable investment have not merely to keep pace with what they had been formerly, but to increase far beyond that, if they are to match a greatly increased volume of savings, corresponding to full employment in a much richer community.

The British data on unemployment and output per capita do not justify Keynes' post-*General Theory* implication that the period 1873-1900 was depressed relative to the pre-1914 years (W. W. Rostow, *British Economy of the Nineteenth Century*, pp. 45-50 and 8). Therefore, I take his analysis to mean, like Friedman's, that a different public policy might have yielded a different result.

²⁰ W. W. Rostow, *The Stages of Economic Growth* (Cambridge, England: Cambridge University Press, 1960), pp. 76-80.

income was required to set these leading sectors into motion again. This was a quite different situation than that typical of the nineteenth century where a fall in interest rates, raw material prices, and money wages could lead to resumed investment in new technologies on the supply side; or where population and urbanization—expanding at relatively steady rates—could make profitable the opening up of new agricultural territories, in response to a fairly regular expansion in the rather inelastic demand for grain. Keynesian income analysis was, therefore, a peculiarly appropriate doctrine to emerge in the stage of high mass-consumption, when a high income elasticity of demand controlled the fate of the leading sectors.

III

Now some general conclusions and a few hopefully constructive suggestions. The body of theory we need to grip the problems that interest us is sometimes more complex than the models ready at hand. This is the burden of my observations on Tsiang on capital widening versus capital deepening as it may relate to David's analysis, on Temin's cotton price analysis, and Fishlow's treatment of spreading effects and the role of the railroads in the 1840's. Here we can—and I suspect we shall—gradually do better; although the inherent tension between the imperatives of elegant theory and historical circumstance will remain with us.

Much more serious is the fact that the body of contemporary economic theory is badly fragmented, as Friedman has freshly dramatized for us and as the puzzlements defined in Temin's paper as a whole make clear. We do not have yet an agreed theory of production and growth that relates systematically the paths of total output to the sectors and sectoral complexes; an agreed structure for relating monetary phenomena to such a theory of production; an agreed structure for relating cycles and trend movements to such a theory of production.

In absorbing the body of work called *The New Economic History*, I have been much struck by the results already achieved; by the élan of its practitioners; by its promise for the future. But I have also been impressed by its fragmentation. This fragmentation accurately reflects the fragmentation of contemporary theory; and that may explain also why so much of the effort has been "revisionist." A "revisionist" implicitly or explicitly takes his frame of reference from someone else's definition of the problem; and he is sometimes thereby led to set up exaggerated targets, if not exactly straw men, to maximize product differentiation. "Revisionism" should, of course, go on. It is the engine that drives forward much of scholarly life. But my respect for the current generation of American economic historians is such as to lead me to propose two large, positive missions.

First, I believe we need and would greatly benefit from a short period (or cyclical) history of the United States from 1790 to 1860—and, indeed, for the period beyond. In this post-Keynesian, inflationary world, cyclical history is somewhat out of fashion, I know; but cycles were more than cyclical: they were the way growth actually happened. And the

structure and timing of growth cannot be fully understood without knowing the way the pattern of investment unfolded through time.

Thus, reflecting on the powerful but partial insights of Fishlow and Fogel, David and Temin, Gallman and Lebergott, North and Parker, and all the others who have deepened our knowledge of American economic history in recent years, I have come to feel that the body of material before us cries out for synthesis. Such an effort, supplanting the pioneering work of Smith and Cole, weaving together all the new data, insights, and tools, would force us to contemplate the interconnections among the sectors of the economy. These interconnections are often not difficult to perceive if one embeds oneself in what actually happened year by year, cycle by cycle. The interconnections somehow get lost when one moves to the world of overlapping or decadal averages to measure trends and regression calculations.²¹ Of course, we need such measurements; but I can attest that when, on the Gayer study, I sat day after day beside Anna Jacobson Schwartz, interpreting with her the National Bureau measurements of average cyclical behavior and trend for Britain from 1790 to 1850, we were greatly strengthened because we knew each cycle—indeed, each year—like an old friend.

Second, I believe that it is up to us to contribute actively to the unification of the body of contemporary economic theory. The pure theorist can, if he chooses, elaborate the models that happen to interest him, for whatever reason. He is under less compulsion than we are to relate the sectors to the aggregates, money to production, cycles and trends to the structure of growth.

As I read the flow of work from our lively centers of economist-historians, I believe the talent and imagination and dedication is there to make such a contribution to the unification of theory. And we badly need it to do our job.

And so my agenda for *The New Economic History* goes beyond that set out in Robert Fogel's paper. I would reverse the apocryphal non-monetary dictum of Andy Jackson and urge that we "elevate them sights a little higher."

W. W. ROSTOW, *The University of Texas at Austin*

²¹ This point is vividly illustrated by comparing the image of French industrial growth in the 1840's emerging from Crouzet's annual index with Marczewski's decadal averages (François Crouzet, "Essai de construction d'un indice annuel de la production industrielle française au XIX^e siècle," *Annales, Economies, Sociétés, Civilisations*, no. 1 (Jan.-Feb. 1970), especially pp. 76 and 87-8; J. Marczewski, "The Take-off Hypothesis and French Experience," Chapter 7 in *The Economics of Take-off into Sustained Growth*, especially pp. 128, 133, and 135). The revolution of 1848 (compounding the cyclical recession) produces a brief but very sharp decline of industrial production toward the end of the decade. This damps the average industrial growth rate for the decade. Therefore, in Marczewski's work the 1840's emerge as a period of deceleration sandwiched between two more glamorous decades of economic progress. But it is clear, on an annual basis, that the 1840's was, along with the 1850's, the decade of maximum industrial growth rate (in excess of trend) for France in the nineteenth century. If we confine ourselves to decadal averages (or similar devices), we can, quite literally, be in the position of not knowing what we are talking about.