
DEBATES

*The New Economic History Revisited**

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I have written this essay neither to praise the new economic history nor to bury it. Exaltation of its accomplishment is a ritual already and better performed by others; and interment is decidedly premature for an activity practised by still increasing numbers of younger scholars. Rather, my intent is to communicate in a non-partisan and pedagogic spirit: to set down what I regard as the differentiating methodological characteristics of the new economic history, to explore their strengths and limitations both, and to draw out some of their implications for the study of history more generally.¹

METHODOLOGY.

No issue surrounding the new economic history has been more contentious than its apparent commitment to pseudo-history. Historians, in particular, have been more concerned with the limits of generalization than with extension of its possibilities. For them, it can hardly be comforting to be told that « every investigation of the consequences factor involves a train of experimental reasoning and an answer to the question of what conditions would have been

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¹ For fuller discussion of substantive contributions, and references to the now extensive bibliography on the new economic history see ALBERT FISHLOW and ROBERT W. FOGEL, *Quantitative Economic History: An Interim Evaluation of Past Trends and Present Tendencies*, « Journal of Economic History », March 1971, pp. 15-42. For a highly favourable view of the new economic history, see ROBERT W. FOGEL and STANLEY L. ENGERMAN (eds.), *The Reinterpretation of American Economic History* (New York: Harper and Row, 1971).

like had the particular factor not been present, or had it taken some different form. Historians who have no use for such conjectural reasoning must therefore refrain from passing judgment on the effects of individual forces in a common development ».² Yet the author of that dictum is no apostle of the present cliometrics movement. He is none other than the highly respected Eli F. Heckscher writing some forty years ago in the conclusion to Volume 1 of his classic work on mercantilism.

The question of the role of the conjectural mode therefore is hardly novel. Unfortunately, one has the sense that the discussion of the last decade, shrill and prescriptive as it has sometimes become, has clouded rather than clarified the legitimate uses of the counter-factual in historical research. For there are in fact two quite different types of conjectural history that have been practised and defended by new economic historians. The distinction between them is central to an understanding of the lack of unanimity within the cult, and more importantly, goes to the heart of the methodological issues involved.

One application of counterfactual logic is that to which Heckscher refers, as an indispensable instrument for determining the contributions of diverse factors to an observed result. How important was the Jacksonian repudiation of the Second United States Bank to the economic crisis that followed in the 1830's? What was the burden upon the colonists of the Navigation Acts? What was the role of migration in agricultural productivity growth in the nineteenth century?³ These are but a few of the questions to which new economic historians have profitably addressed themselves in recent years. To answer them requires appeal to economic theory to isolate the consequences of changing the economic variables central to each case. That theory, armed with the historical values of the parameters, traces out in principle the observed results: the money supply, the extent of colonial production, the level of agricultural productivity. Just as readily it can sweep out alternative magnitudes that correspond to states that were never realized: renewal of the Bank charter, independent status, restrictions on factor mobility. Then the difference in realizations enables one to measure the role of particular, historical events in changing the flow of outcomes.

² ELI F. HECKSCHER, *Mercantilism*, v. 1 (London: George Allen and Unwin, 1931), pp. 436-437.

³ The specific works addressed to these problems include PETER TEMIN, *The Jacksonian Economy* (New York: Norton, 1969); ROBERT P. THOMAS, *A Quantitative Approach to the Study of the Effects of British Imperial Policy upon Colonial Welfare: Some Preliminary Findings*, « Journal of Economic History », December, 1965, pp. 615-638, but cf. also PETER D. MCCLELLAND, *The Cost to America of British Imperial Policy*, « American Economic Review », May 1969, pp. 370-381; WILLIAM N. PARKER and JUDITH L. V. KLEIN, *Productivity Growth in Grain Production in the United States, 1840-60 and 1900-10*, Conference on Research in Income and Wealth, vol. 30, « Output, Employment and Productivity » (New York: Columbia University Press, 1966).

How *well* it measures is a question I return to shortly. Here I wish only to establish the intent to do so. Such an exercise is dubiously labelled pseudo-history. A more apt and descriptive analogy, familiar to modern man through the ubiquity of the television camera, would be the instant replay of some event. Just as the additional camera provides a slow-motion opportunity for the viewer to reconstruct what happened, and thereby to ascertain how the outcome depended upon individual actions, so the theoretical model does exactly the same for the researcher. It isolates the central independent variables in question and traces out how their changes contribute to the new situation. An onlooker can tell the story of what happened more perceptively because he can see it unfold more clearly.

Another way to fix ideas is to illustrate how the counterfactual is embedded in the theory. The functional relationships making up any theoretical structure are inherently in the conditional mode. They are the rules by which the values of the independent variables determine that of the dependent. All theories must possess such rules that are valid for more than a single observed outcome; for, if not, they would merely be tautological. At any moment of time, only one result corresponding to the set of extant independent variables is observed; other potential realizations are latent. The actual historical sequence consists of the latent values progressively activated by historical changes in the independent variables. The comparison sequence consists of the latent values activated by altering the independent variables to conform to a different formulation and alternative situation.

It is apparent from such a formulation that the accuracy of the measurement of the effect of specific factors depends crucially upon the validity of the initial theory in specifying a causal relationship among the variables in question, upon the stability and ascertainability of that functional relationship over the range of variation admitted, *and* upon the accuracy with which the change in the independent variables reflects the presumed historical factors of interest. Let me illustrate more specifically. Suppose what is at issue is the contribution of the common school revival to ante-bellum economic growth in the United States. Then what is necessary is a specification of how education affects output, an estimate of the parameters corresponding to that function, and an estimate of how much more education the common school revival was responsible for, so as to characterize accurately its impact. Error can creep in at all three points: the theory can be wrong, but irrelevant statistical correlation may yield stable values for the incorrect parameters; or the theory may be right but require parameters that cannot be estimated; or the additional education credited to the common school revival may be misstated.

This quest for measurement, for assignment of importance, is thus no trivial matter. Theory is clearly at center stage in tracing out the latently factual alternative required to provide the basis for comparison. But the

theory, and its implicit parameters, are intimately associated with observed historical experience. Education is not related to output universally, but quite specifically dependent upon the type of economic and political system; the particular circumstances appropriate to 1855 cannot be estimated from the marginal consequences in 1955. In this application the structure of the observed historical sequence is taken as the most important source of the information needed to construct the analytic story of what occurred.

Contrast such an application with the counterfactual objective of describing the characteristics of a world that explicitly did not occur: no railroads in America, no slavery in the South, no discovery of bullion in the New World, or even, no common school revival. Now the interest no longer centers upon measuring what the particular historical event meant to some general evolution, but rather upon the structure of the pseudo-historical alternative itself. That structure is ill-defined even by a theory that might be adequate to the earlier task of measurement. For the functional relationships must now assume a further degree of generality in order to incorporate new features that were historically irrelevant. In other words, we need a meta-theory to describe how the functional relationships would be structured in such a non-realized state. Thus in the absence of railroads, would the same inventive talent that led to steam locomotion have led to another transport innovation; in the absence of slavery, would consumption tastes and social values have continued unchanged in the South; in the absence of bullion, would monetary substitutes have increased in variety and volume to facilitate increased trade, etc. These exercises *are* hypothetical history. Their richness and usefulness depend upon a theoretical structure so fully elaborated, and so stable, as to enable meaningful predictions for situations far different than those historically realized.

Such a claim is considerable. In the first place there is a rapidly increasing confidence interval imposed statistically upon predictions lying well outside the observed range for an estimated and knowable functional relationship;⁴ but secondly there is the further task of inferring the unknown and unobserved relationship from inadequate information. Such generality can of course formally be embodied in the theory. But can it be implemented? The record of prediction of large, general equilibrium-like economic models, for short periods and proximate economic variables alone, does not inspire confidence. Even recent departures in favour of larger scale, interdependent simulation models in history do not do much to make this kind of hypothetical history operational, for they are explicitly conditioned upon the permanence of an existing, historically observed structure.

⁴ Statistical theory incorporates an ever wider confidence interval for conditional estimates of y , given x , as one moves farther and farther away from the mean of the observations from which the x, y relationship was estimated.

This distinction between measuring the effects of actual historical events on the one hand, and characterizing a hypothetical and historically unrealized alternative on the other, corresponds to the logical distinction between sufficient and necessary conditions. A is a sufficient antecedent for B if the presence of A always leads to the emergence of B. A is a necessary antecedent for B if the absence of A always means the absence of B. Hypothetical history arises from an interest in establishing how necessary a given historical event was to an outcome. What I should term latently factual history is directed toward sufficiency.

I would maintain that pursuit of necessity is low on the priority list for historians, economic or otherwise, new or old. Theory has simply not progressed to the point that it can effectively guide us in this pursuit. Indeed, so far as economic history is concerned, the underlying neo-classical economic theory conventionally employed in fashioning the pseudo-historical narrative precludes ever finding a significant necessary condition. That theory presumes continuity and substitution as essential elements, both of which clearly negate indispensability. More generally it would be surprising if even reasonable men could not differ considerably about the course of hypothetical worlds: that is why it is best left to the literary skills of writers of fiction rather than history.

Since as a general rule one cannot establish necessity it is otiose to pose the hypothesis in the first place. Here is where the uniqueness and complexity of history make themselves especially felt. There are simply too few observations to enable empirical evidence to respond meaningfully to the question of necessity. Neither temporal nor comparative experience is very informative about the vast range of potential alternatives, thereby placing the entire burden of reconstruction upon untestable theoretical models.

Dismissal of necessity does not, however, preclude appeal to causal historical analysis. One takes over directly from the natural sciences a definition of cause that requires satisfaction of both necessary *and* sufficient conditions. But physical laws in fact satisfy a much more limited, and experimentally controlled, concept of necessity. The law of gravity expresses the rate of acceleration of falling bodies as a function of gravitational attraction. It does not exclude the possibility that such acceleration be created by an alternative agent other than gravitational force. That event would not constitute a valid counter-example to the law. What is required is that such acceleration not occur when, all else unchanged, gravitational force is cancelled experimentally: then we say that gravitational attraction causes acceleration. But when we speak of the causes of historical experiences, economic growth say, sufficiency alone will do quite nicely because the experimental context is no longer applicable. Under some circumstances economic expansion may occur in the absence of one or more of the identified antecedents, because some other conditioning variables may

also have changed. But if virtually every time a given set of factors is present they yield the same consequence, that regularity will provide a rigorous basis for defining how the process *in fact* works. It yields a valid set of predictions and retrodictions both. Ultimately our appeal to the requirements of causality is subsidiary to concern for adequate historical description. We want to tell a convincing, and verifiable, story. For too long, arguments about analytic history have been plagued by the spectre of the impossibility of universal laws. The burden of my case is that such a debate is misspecified, and no pun intended, unnecessary.

This distinction between necessity and sufficiency is apparent enough when large displacements are involved. While less important when small deviations are at issue, the difference persists. The measurement, or assignment, problem is a *ceteris paribus* one; the quest for necessity allows, and requires, a wider freedom for compensating variation. As the hypothetical history approximates ever more closely to the actual, in the limit it no longer matters. But then we have returned to evaluating the world as it was.⁵

The thrust of this section is therefore to reassure that the new economic history is seriously engaged with the past, and preoccupied with understanding and interpreting what actually happened. It is perhaps unfortunate that the most widely known advocate of the new economic history, Robert W. Fogel, has surrounded himself with controversy owing to his concern with the indispensability of railroads, a question of necessity. His hypothetical world of never constructed canals is really incidental to the principal merits of his research. It is unfortunate that he, and others, should have placed such stress upon that particular methodological innovation as the distinguishing characteristic of the new economic history as a whole.

Having thus dwelt, perhaps excessively, upon what the new economic history is not, let me seek to describe what it is. Its distinctive, and distinguishing, quality is the appeal to formal, neo-classical economic theory to specify, and to quantitative techniques and evidence to test, hypotheses relating to the evolution of production over time. Those hypotheses focus especially upon the economic growth of the United States, and perhaps most particularly in the nineteenth century. Their intent is to separate out the factors in the economic environment contributing to, or impeding, progress. This research is motivated, moreover, by a genuine historical interest — whether the subject be regional development, or technological change, or the institution of slavery, etc. — rather than by a desire to test the validity of the theory by reference to additional data which perchance are historical. No

⁵ In mathematical terms, as dx approaches zero, the approximation $\frac{\partial x}{-\partial x} \approx \frac{dy}{-dx}$ is likely to hold; but conceptually, the partial derivative which holds all else constant, and the total derivative, which incorporates all the secondary changes on other variables, are not the same.

small part of the energies of the new economic history has been dedicated to the awesome task of both extending the statistical frontier and organizing the mass of extant data in more modern conceptual categories. Without such careful and constructive enterprise there would have been no cliometrics.

More glamorous by far, and perhaps too overshadowing as well, has been the destructive bent to which the data and received theory have been put to use. The appetite for revision, and zeal for change, is aptly conveyed by Douglass North's assessment some years ago that « even a cursory examination of accepted 'truths' of U.S. economic history suggests that many of them are inconsistent with economic analysis and have never been subjected to — and would not survive — testing with statistical data ».⁶ This attitude further reveals a significant difference between the historian's and the economist's economic history, which is what the new economic history really is. For the historian what matters primarily are the premises and consequences of particular decisions — why the farmer believed his prices were falling in the late nineteenth century, and how he organized to defend interests. For the economist the underlying « real » economic structure occupies a more central place, and he delights in explaining why apparently beneficial policies have second order effects that negate their efficacy.

In short, the new economic historians have come to history as economists of the post-Keynesian and post-computer generation. They have come committed to quantitative technique as a method and to economics as a discipline. Not by chance, their confidence in their historical models parallels almost exactly the economics profession's faith in the New Economics in the early 1960's as a means of assuring full employment, growth and price stability. Their conviction in the feasibility of scientific economic history manifests nothing else but the same strong endorsement of logical positivism that has pervaded other social science disciplines in recent years. Their occasional intolerance of, and impatience with, traditional scholarship is the modest vice produced by the combination of intense belief and youth. It is their hour, and alas, they sometimes know it.

A BALANCE SHEET.

A significant element and clear strength of the new economic history is its explicit and intimate relationship with economic theory. There is available to the economist a considerable number of tested propositions relating cause and effect. Price is dependent upon demand and supply; the

⁶ DOUGLASS C. NORTH, *Quantitative Research in American Economic History*, as reprinted in RALPH ANDREANO (ed.), « New Views on American Economic Development (Cambridge: Schenkman, 1965), p. 10.

price level is related to monetary expansion; the growth rate is associated with capital accumulation and technological change, etc. Around these a persuasive and structured story can be told of prosperity and depression, incomes and growth. The theory both selects out the essential components of the tale and specifies their articulation. It thereby gives guidance to the marshalling of historical evidence to test its own applicability.

Even when events deviate from expectations, theory endows the resourceful scholar with considerable explanatory potential. For the short-coming can more often than not be repaired by reference to changed circumstances in some other part of the model. If this unfortunately avoids a critical test of the basic relevance of the theory, it also enhances its application to persuasive historical description. Note that it makes little difference whether the near or long distant past is at issue. To those who question why consumption in the United States did not decline in 1968 as a result of the tax surcharge, as economic theory might predict, the economist now answers that the temporary character of the increased burden led to continued durable purchases of durables. The structure of the underlying theory is never proved wrong; it is amended and adapted to circumstance. The new economic historian must therefore be concerned with history. Were his interest mere hypothesis testing with historical data for its own sake, he would have a limited audience indeed.

Theory is incessantly demanding and impatient with the indiscriminate lists of implicit causal factors in which we all occasionally indulge in order to conceal our ignorance. Fleshed out by numbers it provides the basis for ascertaining the relative importance among the multitude of forces making for a given result. This is indeed the constructive side of the new economic history I am most inclined to stress. It is not so much that the traditional 'truths' are all wrong; it is that many are truisms without much content. Refer to any standard catalogue of diverse factors responsible for the Industrial Revolution for example.

Finally, and not of trivial significance, the economic theory required to accomplish most of these tasks is *not* inaccessible. To more traditional scholars frustrated by the proliferation of symbols and jargon in the new economic history this may seem an especially unkind cut. But alas, it is true. It is the simple concepts that on the whole illuminate historical economic behaviour, and not the sophisticated frontier developments in theory. Like all professions the new economic history creates its own mystery rites to enhance its status and frighten potential entrants. Despite the aura of impenetrability, even the most conventional historians will be surprised at how readily they too can achieve the inner sanctum.

These benefits have translated themselves concretely into a continuing flow of substantive and significant scholarship over the past decade which I need not detail. Sometimes, however, the product has been more dubious

when the limits of theory have been transgressed. It is the essence of economic theory that it is valid for small rather than large displacements, even when our concern is with sufficiency alone. The wider the scope of the inquiry, the greater the interdependence among the functional relationships describing the economic process, and the more complex and potentially unstable the required economic model. It is possible to project with considerable accuracy the impact of lower railroad freight rates for a small geographic area, but much more difficult to determine the corresponding national consequences. It is possible to assess the effect of modestly lower charges in a single year, but much more difficult to ascertain the implications of a discontinuity in rates for development during a decade.

This limitation gives rise to a basic tension inherent in the new economic history: the interesting questions are large and involve stretching the applicability of theory to its extreme, while the inquiries to which the theory is most suited are frequently hardly worth pursuing. Each practitioner must wrestle with this dilemma in his own distinctive way; each reader must judge the individual product. No less than the old economic history, there is no uniform prescription for success.

There is another and more fundamental characteristic of the economic theory underlying the new economic history that requires comment as well. The theory being applied almost exclusively is the garden variety conventional paradigm with its basis in uncoerced, individual maximizing decisions. There is no tinge of Marxist class structure and non-market power hierarchies; there is no expropriated surplus and exploitation. Non-economic motivations enter only trivially. Rarely enough indeed, are there even flesh and blood people whether they be entrepreneurs or workers.

Not surprisingly then, there has been much less concern in the new economic history with the distributive than the productive side of the economic process. The allocation of resources is what neo-classical economic theory does best. There is none of the fervour or sense of justice of the Beards or the Webbs that marked an important surge of scholarship in social history around the turn of the century and thereafter. In fact the new economic history is viscerally conservative: the invisible hand has created the *only* world compatible with individual preferences, for if not, there would be a different one. Slavery was not so inefficient after all; the profits of railway promoters was not really excessive once uncertainty is taken into account; sharecropping did not penalize the Black in the Reconstruction South, etc. As William Parker has summed it up, perhaps more kindly, « this is in good part what the new economic history is about: it is a gigantic test of the hypothesis of economic rationality of a system and of the behavior of individuals within it. It turns out that after all, the market has really worked very well... Even in the question of possibly irrational behavior, where profits are not maximized or labor flows do not move except with

long lag toward higher returns, our interest has been not so much in asking why, as simply in asking what society has paid for this perversity».⁷

The « new institutional history » does not alter the focus.⁸ Indeed its novel thrust is to extend the domain of maximization from the market place to the entire institutional structure surrounding it. The new institutionalists posit changes in the institutional arrangements motivated by individual benefit calculations involving the economic gains from changing laws, organizing cartels or unions, creating new business forms, establishing public education, etc. The form of the new arrangement — whether private or public — is based on comparative discounted profits; the lag in its activation likewise depends inversely on the magnitude and certainty of the profit stream.

The theory is patterned self-consciously on that referring to investment in physical capital; just as a prior extension to human capital has occurred in the economic literature, now we have, in effect, institutional capital. But while human capital is presumed to contribute unequivocally to output by means of improved labour quality, institutional capital need not be so benign. Such capital derives its value from the profit occasioned by the deviations of the market from pure competition. Sometimes institutional action is corrective — transport subsidies justified by externalities, say; but sometimes also distorting — cartels to exploit economies of scale.

Another way to interpret this new institutionalism is to think of particular institutional arrangements as enforcing a certain set of property rights. Corresponding to enforcement of such claims and their exchange are a series of policing and transaction costs that necessarily give rise to externalities and inefficient allocation of resources. As the economy changes, new opportunities for profit are constantly presented to alter the institutional arrangements, and hence the structure of property rights, within narrowly defined limits. The response of individuals provides the engine for institutional change.

Framed in this way we see clearly the anti-Marxist thrust of the theory. For Marx and his followers property rights are more central still. But their initial allocation and subsequent evolution are tied to the historically specific mode of production and exchange and the class structure entailed. Social relations among men are disguised under capitalism by the presumed impartiality of commodity production and exchange. In earlier modes they are more obvious. Any economic structure gives rise to a distribution of power, therefore, and its constant exercise influences the remaining institutional super-

⁷ WILLIAM N. PARKER, *From Old to New to Old in Economic History*, « Journal of Economic History », March 1971, pp. 6-7.

⁸ For the fullest historical applications in this now burgeoning literature on the new institutionalism see LANCE E. DAVIS and DOUGLASS C. NORTH, *Institutional Change and American Economic Growth* (Cambridge: Cambridge University Press, 1971) and DOUGLASS C. NORTH and ROBERT P. THOMAS, *The Rise of the Western World* (Cambridge: Cambridge University Press, 1973).

structure, « the economic structure of society is the real basis on which the juridical and political superstructure is raised, and to which definite social forms of thought correspond; ... the mode of production determines the character of the social, political, and intellectual life generally... ».⁹

In contrast to the new institutionalists, and their Benthamite individualism, Marxism poses class interests and materialist determination. Yet despite their fundamental difference in motivational premises they each partake of more than a little tautological circularity: the institutional structure that is observed can always be rationalized by reference to the theory; what is *not* observed is ignored. But what of the institutions that might have been profitable to individuals, or those that might have enabled expropriation to proceed more efficiently and effectively, why were they not innovated? Nor can one rely upon politics and sociology for the answer: neither exist in either formulation. Institutions are appendages that lack a life and dynamic of their own, and do not influence either the mode of production or individual utility functions.

This long digression suffices to emphasize how the new institutionalism does not really respond either to the necessity for a wider scope of historical analysis or to the potentialities of alternative paradigms. It makes clear how the values implicit in the present neo-classical theoretical apparatus colours the questions to be asked and the range of answers admitted. This is all the more critical since the theory is rarely tested against alternative explanations but more commonly coopts them. Conventional theory then becomes as much an article of faith as Marxist theory.

Ironically, moreover, the implications of neo-classical theory parallel too closely for comfort the presumed rules of orthodox, non-generalizing history. As Daniel Boorstin has perceptively noted, in introducing a volume of collected writings of the new economic history, « one of their recurrent themes is a caution *against* the historian's traditional temptation to give cataclysmic or monistic significance to certain simple, measurable aspects of this historical story ».¹⁰ While this reserve is undoubtedly more frequently merited than not, the method leads to an undue emphasis upon analysis and criticism rather than synthesis. Where would the new economic history be without their benighted forbears? It is not surprising that no integrated text summarizing the American past has emerged from the accumulation of findings. Neo-classical economic theory has up to now provided little guidance for grand organizing themes in instances of historical success.

Economic theory is only one of the pinions of the new economic history. Its quantitative emphasis is the other. Among historians this concern with numbers has not always been well received. Many continue to be intensely

⁹ KARL MARX, *Capital* (Charles Kerr, 1906), pp. 93-94, fn. 2.

¹⁰ DANIEL J. BOORSTIN, *Enlarging the Historian's Vocabulary*, in FOGEL and ENGERMAN (eds.), « Reinterpretation », p. xiii.

suspicious, if not overtly hostile. Carl Bridenbaugh's impassioned plea a decade ago not to « worship at the shrine of that Bitch-goddess, quantification » is perhaps not representative of the profession today;¹¹ it probably still strikes a responsive chord, however. There seem to be two principal fears: that qualitative evidence will be ignored, and thereby historical judgment slighted; and that the only legitimate questions will reduce to those to which numbers can respond.

Both concerns are exaggerated as regards the new economic history. Counting has not done away with judgment; it has heightened it. Those engaged in reconstructing the numerical record of the past continuously rely upon historical judgment not merely in the straight forward evaluation of one series versus another, but also in filling in the many gaps in micro-series, and aggregating to broader conceptual categories. Extrapolation and interpolation are no longer acceptable techniques. The care and self-criticism typical of this effort are a credit to the scholarship of the new economic historians.

More generally, the emphasis on quantification provides an efficient and convenient way both for specifying and evaluating diverse historical judgments; so long as it is a case of my citations against yours, the argument and the means for resolving it remain diffuse. Means and standard deviations are not as exciting to read, but if it is accuracy rather than style that is at stake, they do have an edge. The range of possibilities for quantification remains impressive; many and highly varied aspects of economic behaviour do quantify. I do not exclude here even categories of beliefs or values: for example, we can count how many New Englanders moved to the Mid-west and measure how their special attitudes toward public education manifested itself in state legislation. Much argumentation in literary economic history is in fact implicitly numerical without the evidence. For questions and answers to be framed more explicitly is gain rather than loss.

That practice does not mean loss of subtlety or historical sophistication. Qualitative evidence can and is brought to bear in the new economic history as well. Opinions and reactions of contemporaries inform, and even define, the topics for research. Much of the discussion about comparative American and British technology in the nineteenth century originates in the observations of contemporary travellers. Similarly, a decisive element underlying the scepticism with which Walt Rostow's take-off mechanism has been greeted by the new economic history has been the inability to demonstrate convincing attitudinal changes from the late eighteenth to the mid-nineteenth centuries that could correspond to an economic discontinuity.

If I thus staunchly defend this emphasis on quantity, I nevertheless retain misgivings about some aspects of the subsequent manipulation of the

¹¹ CARL BRIDENBAUGH, *The Great Mutation*, « American Historical Review », January 1963, p. 326.

data. Statistical, like economic, theory is both powerful and demanding. Inference from a sample is greatly facilitated by presuming the characteristics of the universe from which it has been drawn. The common, and not particularly defensible, assumptions are those of an underlying normal distribution and random selection; they are made because the principal, and most powerful, statistical tests require them. These properties are highly dubious not only because of the special kinds of quantities being measured, but also because of the inevitable practice of specifying one's model on the basis of the available, rather than the desired, data. Thereafter, in the process of estimation, the very canons of statistical inference are violated in the extreme by continuing experimentation with the data set in an effort to obtain « good » results. Conventional measures of statistical significance, derivative from repetitive experimental contexts, presume a single test only. Yet we choose to ignore that restriction, and understandably. Whatever the merits of pseudo-history, there is only one non-replicable experiment history has in fact provided, and we must make do with that.

These problems are not unique to historical applications; they plague empirical research in the social sciences more generally. They are coming to be more fully appreciated and more adequately dealt with. I emphasize them here because they confirm the historian's intuition that historical uniqueness is not without its special difficulties. I wish also to point out another implication of statistical models that has special significance in this context. These models are inherently designed to *minimize* data requirements; the merit of the strong assumptions is that they permit one to make do with little information. The historian, on the other hand, typically starts from a data *minimizing* strategy that sets out to exploit all known sources; original archival research is highly regarded and rewarded. The historian thus evaluates information as unique and essential whereas the new economic historian may treat it as repetitive and potentially redundant. The specific circumstances will determine whether one or the other is correct. But this initial difference in attitude is probably an important source of some of the mutual misunderstanding and misinterpretation that have reduced effective communication.

Note that I attach less weight than has frequently been given in other surveys to such particular statistical problems as mutual dependence and multicollinearity. Although these difficulties do indeed exist, their mischief is on the whole most virulent in aggregate time series analysis where the underlying economic model has been ignored in favour of direct statistical manipulation. It makes little sense to correlate steel output and national income in order to ascertain how one influenced the other. Regardless of the high degree of association, which could be replicated by substituting any other variable growing over time, the results are uninteresting. It is by refining the specification, through the intermediation of theory, and thereby separating

out demand and supply effects, and introducing information on technological change, that we make the problem both relevant and soluble.

It is on such a positive note that I should like to conclude this section. The balance sheet of the new economic history over the past 15 years, despite the limitations I have raised, is emphatically positive. My criticisms and concerns are those of a committed adherent and practitioner. They do not lend comfort to those who dismissed analytic methods long ago, firm in the belief that such an approach is « dehumanizing » and inappropriate to history. Quite to the contrary. I intend my comments as frankly proselytizing. Many of the problems now facing the new economic history derive from its too narrow basis in professional economics. What was an essential and vigorous impulse a decade ago can no longer serve as an exclusive source of continuing guidance. That does not mean retreat from the novelty of formal models and lesser reliance upon statistical techniques. In both dimensions, greater participation by historians trained in and sympathetic to analytic methods would be a great boon.

CONCLUDING OBSERVATIONS.

Thus far my remarks have been confined to the new economic history, I cannot resist the temptation to dwell briefly on some of their implications for the study of history more broadly.

Foremost is the firm rejection of as-if history as a basis for more « scientific » analysis of the past. If hypothetical — but not latently factual — economic history opens Pandora's box, one need not be imaginative to project the consequences for other historical pursuits. No one need read the message of the new economic history as an endorsement for such exercises.

The correct lesson, rather, is the potential enrichment historical study can receive from closer involvement with its corresponding substantive discipline, whatever it may be. The latter are the source of the theory required to measure causal influence and sharpen historical inquiry. It is no accident that economic history — new *and* old — affords a longer record of analytic emphasis than other branches of historical study. Among the social sciences, economics developed an earlier and more coherent theoretical structure that economic historians could and did invoke, sometimes with assurance and explicitly, sometimes more diffidently and implicitly. Methodological discussion among economic historians has almost always agreed upon the relevance of theory and statistics to economic history; the question has been the kind of theory and quantitative techniques and the limits of their validity.

(If I am right in assessing the constraining effect of current economic paradigms, perhaps more discussion along this line is now in order. I do not mean sterile debate over the labour theory of value or definitions of exploitation, but rather serious consideration of the appropriateness of individualistic,

purely rationalist assumptions for the historical phenomena we wish to explain).

Be that as it may, it is not an argument for ignoring the potential advantages of closer links to social science theory by historians. In recent years, many of the social sciences have seen far-reaching changes in the direction of formalization. Behavioural hypotheses, based for the most part upon contemporary evidence now abound. Their application to history could prove an extremely fruitful pursuit: already there have been abundant indicators of a new political and new social history. But unlike economics, in which history has had a special place as a recognized sub-discipline since before the turn of the century, neither political science nor sociology have a similar tradition. The changes will have to come within the conventionally defined discipline of history itself.

That means, perforce, in these instances and others as well, more specialization in substantive subject matters among historians. Arguing from the experience in economics I can unequivocally counsel an exposure in depth. For it is only thereby that one can acquire the expertise and confidence to manipulate rather than merely comprehend the special concepts and techniques that are employed. Let me also stress a greater investment in the acquisition of statistical skill than has been common in the past. Nothing is worse than sufficient knowledge to replicate statistical formulae without firm grasp of their relevance. These capabilities must be internalized if consequential results are to be achieved: the new social historian cannot rely on a consulting sociologist and a consulting statistician to guide his research.

What I am suggesting as an opportune course is already occurring. One sign is the unhappiness voiced by some historians, a reaction not unrelated to their lack of enthusiasm for the new economic history. I too would lament the passing of those rare and valued scholars whose insightful and holistic reconstructions of the past have brightened the study of history — *if* I believed they were threatened by such developments. But they are not, of course; their talents will not so readily be dispossessed. It seems to demand too much, yet also too little, to limit all historical inquiry thus. The study of the past poses special and interesting problems that can be informed and illuminated by appeal to disciplinary research. The record of the new economic history over the past decade, and hopefully into the future, vouchsafes for that.

