in complex ways, produced the observed dynamics:

> If black people had been better educated earlier in the century, more would have been able to enter blue- and white-collar occupations in the South, more would have migrated to the North, and relative black status would have been higher. But the shift of black labor out of southern agriculture would have been much smaller than it actually was had it not been for "shocks" like the world wars. (p. 131)

Margo's large samples and skillful econometric analyses are impressive, but his most remarkable attribute as a scholar is the painstaking effort he makes to master the data before shoving them into the computer. I know of no one who possesses equal mastery of the schooling data for Southern blacks. By virtue of the care taken to know the strengths and weaknesses of the data, Margo has been able to avoid pitfalls into which other analysts—even some highly regarded economists—have fallen. After analyzing the data, he is judicious in his interpretations. The argument is well presented: data are adeptly displayed; theoretical and econometric analyses are explained in comprehensible English.

Even in capable hands, however, econometrics remains a blunt instrument, and some of the details may be questioned. In analyzing racial differences in occupational attainment, for example, Margo is too quick to ascribe to "race per se" the differences remaining after adjustment for the effects of the usual suspects—age, education, region, urbanization, marital status, and an index of geographic mobility. No one would base labor-market decisions on knowledge of these variables alone; they are surrogates at best, selected by the analyst because they are measurable. In consequence, the race dummy variable soaks up explanatory force that may actually belong to variables left out of the analysis. In addition, Margo's index of mobility, defined at the state level, is too crude to justify his rejection of the hypothesis of "spatial mismatch" between black workers and job vacancies in the actual labor markets. Also, some of the counterfactual exercises generate results that, although plausible in general, are questionable in detail because of problems of variable definition or measurement or crude specification of the underlying model. Applied econometrics is always messy work.

But these objections are minor matters. Margo has produced an excellent study, which can serve as a model for aspiring cliometricians. To describe it as "required reading" would fail to indicate just how important, indeed indispensable, the book will be to scholars interested in racial economic differences, past or present.

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Institutions, institutional change and economic performance. By DOUGLASS C. NORTH. The Political Economy of Institutions and Decisions series. Cambridge; New York and Melbourne: Cambridge University Press, 1990. Pp. viii, 152. $32.50, cloth; $10.95, paper. ISBN 0–521–39416–3. *JEL 91–0764*

Perhaps no challenge facing economists or economic historians is more important than that of understanding the sources of persistent differences betweeen rich and poor societies. There have been chronic differences in economic performance throughout history, and they exist across societies today. While these issues have provided puzzles for historians interested in explaining the past, contemporary economic development policies aimed at narrowing the gap between rich and poor countries have involved enormous transfers of capital and uncounted teams of consultants. In large part these efforts to explain or promote economic growth have failed.

In the search for reasons why, Douglass North has been one of the foremost economists insisting that more attention be directed to the institutional structure of a society, in general, and to its property rights arrangements, in particular. The need to understand sources of growth has become more urgent with efforts to promote the successful transition of Eastern European economies to market systems. This important book, which is part of the Cambridge Series in the Political Economy of Institutions and Decisions, represents the development of North's thinking on these problems over the past 20 years and provides new insights into the development of institutions and the ways they affect economic performance.

In Part I North outlines a number of basic behavioral postulates that depart from traditional neoclassical theory. According to North, the sources of contrasting economic performance lie within the institutional structure of the society that defines the incentives for saving, investment, production, and trade. Institutions are defined as humanly-devised constraints on political, economic, and social behavior, and they range from informal arrangements, such as customs, to formal statutes and judicial rulings. As such, institutions influence the array of benefits and costs, including both production and transactions costs, associated with the various alternatives available to individuals in economic decision making. Hence, they shape the direction of economic growth or decline. The central problem of economic history and development, then, is to account for the evolution of political and economic institutions that create an environment for growth. Because it assumes zero transactions costs or static institutions, standard neoclassical theory is of little help in this task. Indeed, it predicts that over time inefficient institutions will be replaced, and economies will gradually converge. Of course they have not, and North offers extensions that enrich, but also complicate the theory.

Institutions are created to reduce uncertainty and the costs of production and exchange. In the face of incomplete and asymmetric information regarding economic choices and a limited ability of individuals to process that information, they provide a structure for interpreting and responding to economic events. Once created, institutions may be changed, but their legacy is a powerful one, since present and future choices are necessarily shaped by institutions created in the past. Indeed, economic history is the consequence of a dynamic, though not smooth, process of institutional change. The kinds of institutions appropriate for subsistence, rural societies characterized by personal exchange and repeat contracting, are very different from those necessary for modern economies characterized by complex production and exchange. The key point is that there is nothing in the process of institutional change to insure that efficient institutions will evolve.

The intricacies involved in institutional change are the focus of Part II. The three chapters in this section provide the major extensions from North's previous work, but they also show how much more remains to be done to understand institutional change. The agents of change are organizations, and entrepreneurs within them, which attempt to maximize the achievement of particular objectives within the institutional environment when prices, technology, or perceptions change. Where this is not possible within the current structure, they seek to alter it. But their actions are molded by incomplete information, the subjective processes by which they analyze it, and the influence of existing institutions, in which they likely have a stake. Accordingly, the process of institutional change generally is one of incremental alteration, characterized by path dependence. The organizational skills and resources necessary to use effectively or modify a particular institution, however, may not result in greater social efficiency.

This is such an important point that one wishes that North had spelled out more of the details, especially in the politics of institutional change. This is where path dependence and many of the potential conflicts and inefficiencies of institutional change can be seen most clearly. Institutional change in one way or another occurs in the political arena, and it necessarily involves a redistribution of wealth and political power. Both winners and losers are created, and this brings risks for politicians. Politicians forge links with influential interest groups to protect their positions. The coercive power of the state and the political influence of various interest groups allow for opportunism in statutes, court opinions, and administrative rulings in redistributing wealth and property rights to those who are politically advantaged. There is no reason for this process to lead to a distribution of property rights that is consistent with social wealth maximization. Only under very specific (and history suggests, rare) conditions, will side payments in the political arena be complete enough to compensate the opposition, facilitate socially-beneficial exchanges, and create efficient institutions. Further, if the distribution of political power in this process is unstable, the resulting property rights will not be secure and uncertainty will dampen incentives for productive behavior.

In the final part of the book, North returns to some of the behavioral postulates discussed earlier to examine the effects of institutions on economic performance. He points to divergent examples drawn from U.S., English, and Latin American economic history. To understand these puzzles of economic growth and to devise useful policy options, North challenges the economics profession to adopt more elaborate and complex views of economic and political behavior. The elusive concepts of ideas, ideologies, and institutions play such a major role in economic development that they can no longer be ignored by the profession. Indeed, a true political economy is needed that integrates institutional analysis into neoclassical theory. New research must examine the details of the political framework in which negotiations for institutional change occur. This involves identifying competing interest groups, their links to politicians and bureaucrats, and how the maneuvering of all the parties affects the institutions that result. Only through micro analyses can we see exactly how beneficial institutional arrangements are promoted or blocked.

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Mickey Mouse numbers in world history: The short view. By D. C. M. PLATT. London: Macmillan Press; distributed by International Specialized Book Services, Portland, Oreg., 1989. Pp. ix, 91. $47.00. ISBN 0–333–46787–6. *JEL 91–0918*

The argument of this short book is that widely used estimates for world trade, income, and population prior to the Second World War are completely untrustworthy. Platt strongly criticizes the continual reusing, as if authoritative, of statistics prepared by the League of Nations or by pioneer investigators such as Mulhall, Tysynski, Woytinsky, etc. The users of these data include at some time or another virtually the complete set of writers on economic development and a good proportion of all empirically oriented economists. Platt's view is a generalization of his earlier strictures on estimates of nineteenth century British balance of payments and foreign investment estimates.

Platt structures his book as a short history of the collection and use of statistics of international economic relations. The author was a professional historian and his account is a lament at the amateurish approach to the source materials of economic history by economists and statisticians. Practitioners are divided into "the sowers" and "the reapers." This latter group is peopled by some very famous names covering a wide spectrum of the economics profession including Kravis, Kuznets, Lewis, Maddison, North, and Wallerstein.

There is something to be said for Platt's position. Economic historians generally spend rather more time worrying about the accuracy and representativeness of statistical sources and trying to improve and to construct new data sets than do most economists. In the economics profession as a whole there probably is too much uncritical acceptance of official statistics. In the literature reviewed by Platt, he establishes a strong tendency for reliability to be assumed through constant citation by many authors rather than to be seriously checked out. There is a need for a scholarly effort at reassessment of commonly used materials to provide good users' guides as to what various figures can and cannot reasonably be used for and to assess possible margins of error. Had Platt in this book undertaken something of this task in a spirit of constructive criticism then his contribution might have been a valuable one.

As it stands, however, this short essay is a bucolic and indiscriminate attack which will deservedly get short shrift from the well informed. The tone is set by the following remarks:

> A Nobel Prize can preclude further thought; the conference circuit becomes a kind of Monte Carlo Grand Prix. Kuznets was a great figure in international economics, but it is clear that he never felt the need to re-examine numbers sacred since the Nobel celebrations of 1971 . . . He was indeed hypnotized by figures, particularly "official" figures . . . (p. 51).

Quite apart from the question of the author's unattractively aggressive style, there are important flaws in his critique. In particular, innocent of formal statistical training and uninterested in any calculation of the welfare effects of his position, he does not confront the question "just how accurate do data need to be to be useful?" and indeed appears to be advocating a purist approach to the use of statistical evidence which would entail high costs for few, if any, benefits. Although the data Platt criticizes contain many imperfections, for many of the purposes for which they have been used it is surely the case that this will not matter.