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## PROBLEMS

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### *The Historical Context of the «New» Economic History*<sup>1</sup>

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#### I

It is just over twenty years since A.H. Conrad and J.R. Meyer presented their pathbreaking papers on "Economic Theory, Statistical Inference and Economic History" and "The Economics of Slavery in the Ante-bellum South" at the Economic History Association meetings at Williamstown, Massachusetts, in September 1957.<sup>2</sup> This occasion — so it is usually said — marked the birth of the "new economic history" — or "cliometrics", as it was soon lightheartedly dubbed in its infancy, and since that time there have been innumerable commentaries and critiques of the nature, methods, limitations, achievements, and long-run potentialities of the movement. As yet, however, there has been no systematic effort to account for its genesis, timing, and subsequent development in relation to the general characteristics and evolution of economics and historical scholarship in America, the two disciplines of which it con-

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<sup>1</sup> This paper was originally delivered as a lecture at Queen's University, Belfast, in February 1977, and at the University of Durham, in March 1978. In preparing this revised version I have benefited immensely from correspondence with the following persons, several of whom have supplied unpublished papers or reminiscences:

S. Engerman, P. Fearon, R.W. Fogel, L.E. Davis, J.R.T. Hughes, D.N. McCloskey, J.R. Meyer, W.N. Parker, B.E. Supple.

<sup>2</sup> Both papers are reprinted in their volume of essays, *The Economics of Slavery and Other Studies in Econometric History* (Chicago, 1964). They originally appeared in the *Journal of Economic History* (December, 1957) and *Journal of Political Economy* (April, 1958) respectively.

stitutes such a spectacular offspring. There have, of course, been numerous brief comments and passing references to these matters, and some valuable personal reminiscences of those heady early days, of which it has aptly been said:

Bliss was it in that dawn to be alive.  
But to be young [and numerate] was very heaven!<sup>3</sup>

This paper is designed to provide both a supplement and a corrective to the available literature in the new economic history by employing a broader and longer-run perspective than that adopted by previous commentators, most of whom have been primarily concerned to stake the claims, scrutinize the credentials, emphasize the novelty, and demonstrate the success of their intellectual product. The limitations of perspective in previous accounts frequently stem from a combination of personal involvement and commitment, and a lack of interest in intellectual history and the sociology of academic life.<sup>4</sup> The adoption of a detached transatlantic viewpoint<sup>5</sup> emphasizing the American cultural background and the academic context of the new economic history may at least provoke further reminiscences and rejoinders from the early participants and their rivals. No attempt will be made here to provide a complete account of the intellectual background to the new economic history or to re-examine those aspects of the cliometric campaign which have already been fully and exhaustingly discussed by previous authors, most of whom are cited in the footnotes to this paper.

## II

It is seldom easy to date the birth of a new intellectual movement precisely. There are invariably some antecedents; there is often a lengthy period of gestation; and the historian usually has several degrees of freedom in selecting his starting point. In the present instance, however, it seems appropriate to follow the usual convention of regarding the Conrad and Meyer papers as a watershed - indeed, a kind of intellectual flashpoint. Yet, curiously enough,

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<sup>3</sup> Adapted from Wordsworth by Barry Supple, in "Can the new economic history become an import substitute?", in DONALD N. McCLOSKEY, *Essay on a Mature Economy: Britain after 1841* (London, 1971) p. 423. (This volume is subsequently referred to as McCLOSKEY, *Essays*).

<sup>4</sup> One exception is J.R.T. HUGHES. See, for example, his "Is the New Economic History an Export Product?", in McCLOSKEY, *Essays*, pp. 401-412.

<sup>5</sup> For an earlier example see MAX HARTWELL's, "Comment on J.R.T. Hughes", in McCLOSKEY, *Essays*, pp. 413-22, where the emphasis on institutional factors is in general harmony with the argument of this paper. Supple's contribution, cited above in footnote 3, reflects the views of a Cambridge University postgraduate who spent several years at Harvard during the late 1950's and early 1960's.

this was not because the two leading *dramatis personae* intended it that way. Unlike some of the more aggressive young cliometricians who followed they were not ardent proselytizers. As John R. Meyer has recalled:

the two most shocked people in Williamstown those two days were Conrad and myself as we beheld the storm or "reaction" that we generated. Being naive in these matters, perhaps because we lacked historical perspective due to our training in economics, we really did not consider our papers to be particularly revolutionary or unconventional. Rather, we looked upon them as simple extensions of the teachings to which we had been exposed in our graduate student years, particularly of Schumpeter, Gerschenkron and Kuznets. We saw our papers as essentially an affirmation of their basic views on how historical quantification should be done in economics. In short, we hardly looked upon our presentation as revolutionary, but rather as an attempt to reassert much of what was best in the empirical traditions of American quantitative economics.<sup>6</sup>

The Williamstown gathering was evidently one of those historical occasions when, to use an overworked cliché, the time was ripe, owing to a favourable conjuncture of "slowly generating forces" and "trigger mechanisms".<sup>7</sup> One noteworthy feature was the presence of two distinct, if partially overlapping groups of scholars representing, respectively, the Economic History Association and the Conference on Research in Income and Wealth. While the former was the country's leading (and as yet, unchallenged) body of economic historians, the latter was an offshoot of the most prestigious empirical tradition of American economics initiated (at least in an organizational form) by Wesley C. Mitchell, at the National Bureau of Economic Research.<sup>8</sup> The invitation to hold a joint meeting had come two years earlier from Solomon Fabricant, Mitchell's long-time collaborator at the Bureau, and it was motivated by the desire to put American economic history on a sound quantitative foundation, an aim which has subsequently proved to be one of the twin pillars of the new economic history. Yet there was nothing inherently startling either in the initiative or in the particular combination of forces assembled. American economic history had long been heavily empirical in substance — excessively so, some of its critics would argue — and a formal collaboration with quantitative and statistical economists was therefore much less novel than a similar relationship would have been in Britain, for example, where quantitative economics was comparatively weak,<sup>9</sup> and where economists and economic historians

<sup>6</sup> Quoted, by permission, from a private letter of 4 October, 1977.

<sup>7</sup> The terms are borrowed from PETER MATHIAS, *The First Industrial Nation* (London, 1969), p. 10.

<sup>8</sup> It is worth noting that one of the co-founders and influential supporters of the National Bureau was the Harvard economic historian, Edwin F. Gay.

<sup>9</sup> For a stimulating discussion of the reasons for the lag of quantitative economics in Britain see DON PATINKIN, "Keynes and Econometrics: On the Interaction between

have usually been segregated in their respective university compartments and scholarly societies.

In his recollections Meyer emphasizes quantification and the empirical tradition in American economics as the relevant background, rather than the application of economic *theory* to historical problems, which has usually been regarded as the second main pillar supporting the new economic history. For the present purpose it is unnecessary to assess the respective roles of these two elements in the Williamstown story. It is sufficient to acknowledge that while some members of the audience were more responsive to one than the other,<sup>10</sup> Conrad and Meyer deliberately stressed the *combination* of theory, statistical inference, and economic history, and the combined effect was doubtless greater than the sum of its parts. In terms of the immediate antecedents there is some justification for the contention that quantification predominated in the pioneering Purdue papers of the mid-1950s whereas intellectual subtlety was more

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the Macroeconomic Revolutions of the Interwar Period", *Econometrica*, Vol. XLIV (November, 1976), pp. 1091-1123. Patinkin's views are likely to prove controversial.

<sup>10</sup> In a balanced summary of the Williamstown reactions to the Conrad and Meyer papers Simon Kuznets observed:

it did seem as if almost all economic theorists participating in the discussion were doubtful of the value of theory in work on economic history, while at least some economic historians felt that it is needed. And perhaps there is a simple explanation for this somewhat paradoxical situation. Scholars working in the field of economic analysis are all too aware of the limitations of their tools, those working in the field of economic history in which some of these tools have not been widely used, tend to appraise highly the possible returns from such use.

"Summary of Discussion and Postscript", *Journal of Economic History*, Vol. 17 (December, 1957), p. 550. He cited North and Gerschenkron as advocates of the greater use of economic theory in economic history. Like several of the young cliometric enthusiasts of the early 1960's, North unhesitatingly emphasized the crucial role of economic theory in economic history.

It should be noted that on the same occasion W.W. Rostow presented a persuasive paper on "The Interrelation of Theory and Economic History", *ibid.*, pp. 509-23. Rostow had co-authored (together with A.D. Gayer and A.J. Schwartz) a major National Bureau study of *The Growth and Fluctuation of the British Economy 1790-1850* (Oxford, 1953) which, together with his other books, *The British Economy of the 19th Century* (Oxford, 1948) and *The Process of Economic Growth* (Oxford, 1953 and 1960), had exerted a considerable influence on the first postwar generation of economic historians in Britain, a significant proportion of whom had had some training in economics. During the 1950's and 1960's the gap between political historians and economic historians widened as the latter gained in confidence, academic strength and status, but by comparison with American developments of the 1960's the process of change was gradual, sedate and only mildly disharmonious.

While Rostow's emphasis on the value of economic theory in economic history was probably familiar to most members of the Williamstown audience, it nevertheless reinforced the impact of Conrad and Meyer's papers.

evident in the Harvard contributions.<sup>11</sup> Yet it is also significant that the National Bureau's work had often been dismissed by economists as "measurement without theory", and there was some warrant for this charge, notwithstanding Milton Friedman's ingenious attempt to present Mitchell as an economic theorist.<sup>12</sup> Admittedly, by the late 1950's the Bureau's image was changing, and the dominant intellectual figure behind the Conference on Research in Income and Wealth was no longer Mitchell, but Simon Kuznets. As William Parker observed in his editorial introduction to a volume including some of the Williamstown papers, that meeting was conceived in terms of Kuznets's national income framework, and designed as a "testing of the statistical ground to see what sort of analysis it may be made to support".<sup>13</sup> Thus the object was to arrange a true and lasting marriage between statistics and theory, measurement and analysis, in order to provide a solid foundation for research which could account for the achievements of the American economy in the nineteenth century. And while some of the young enthusiasts may have overvalued techniques and explanatory models as against detailed grubbing in historical archives, it is worth noting that Robert Fogel, the most boldly imaginative theoretician of the new movement, subsequently emphasized the paramount influence of Kuznets (his teacher at Johns Hopkins).<sup>14</sup>

In retrospect there are several historical reasons, apart from their sheer intellectual power and brilliance, why the Conrad and Meyer papers so delighted some members of the Williamstown audience and disturbed others. By itself, the concerted application of the National Bureau approach to historical problems was neither novel nor threatening to members of the older generation of economic historians. After all, a decade or more had passed since Kuznets had pushed some of his statistical estimates back to 1869, and there was every reason to welcome and encourage further efforts to provide a firm statistical foundation for historical research. But Conrad and Meyer represented something else: the propagation of seemingly advanced and esoteric theore-

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<sup>11</sup> McCLOSKEY, "The Achievements of the Cliometric School", *Journal of Economic History*, Vol. XXXVIII (March, 1978), p. 18. It is noteworthy that Conrad and Meyer's work had initially been presented some years earlier at Gerschenkron's Economic History Workshop at Harvard. Gerschenkron was a co-organizer of the Williamstown meetings. For comments on cliometrics at Purdue see below, pp. 192-193, 197-198.

<sup>12</sup> Cf. T.C. KOOPMANS, "Measurement Without Theory", *Review of Economics and Statistics*, Vol. XXIX (August, 1947), pp. 161-172; and MILTON FRIEDMAN, "Wesley C. Mitchell as an Economic Theorist", *Journal of Political Economy*, Vol. LVIII (December, 1950), pp. 465-93; reprinted in A.F. BURNS ed., *Wesley Clair Mitchell: The Economic Scientist* (New York, 1952).

<sup>13</sup> Conference on Research in Income and Wealth, *Trends in the American Economy in the Nineteenth Century*, Studies in Income and Wealth, Vol. 24 (Princeton, 1960) p. 4.

<sup>14</sup> ROBERT W. FOGEL, "The Reunification of Economic History with Economic Theory", *American Economic Review, Papers and Proceedings*, Vol. LV (May, 1965), p. 96

tical and statistical techniques and a demonstration of their effectiveness in disposing of one of the perennially disputed questions of American history — the profitability of slavery.<sup>15</sup> To the younger economic historians this demonstration was both dramatic and conclusive. The scales fell from their eyes as they perceived the immense new possibilities for future research. In work of this kind the new recruits trained in economics obviously had a significant differential advantage over their elders, who in this respect at least were clearly not their betters. Their enthusiasm was infectious; and as economic history was manifestly inferior to economics in respect of manpower, resources, and academic prestige,<sup>16</sup> they could easily envisage a long run shift in the content and balance of power within the discipline. It meant not only new and enlarged research opportunities but also the promise of rapid professional advancement within *economics* departments. How many of those present at Williamstown fully appreciated the prospects opening before them it is impossible to say. But for reasons to be considered later, once the spark had caught, the flame continued to burn brightly, especially during the next few years.

To some members of the older generation the situation appeared in a very different light. The enthusiasm at Williamstown and after seemed exaggerated as no doubt it was; and they viewed the new approaches with a mixture of scepticism, apprehension and even dismay. Apart from a prospective invasion, and possibly even a take-over, by alien forces from economics, a discipline which had long overshadowed economic history, they deplored the tendency to denigrate well-tried procedures and discard previously established conclusions. The young dwelt on the dullness, lack of imagination, and theoretical deficiencies of the existing literature, and were the new movement to succeed most of the established leaders would be left behind, as "handloom weavers of a new intellectual revolution"<sup>17</sup> — if only because of their ignorance

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<sup>15</sup> The following brief characterisation of the two papers by an observer who has described them as "the manifesto of a new movement" is worth quoting:

the one paper, on methodology, explained to the assembled historians and empirical statisticians what scientific method was all about: the other added enormous force to the methodological prescriptions by claiming to follow them in an analysis of the profitability of negro slavery on the eve of the Civil War. The method and data employed, the economic and accounting framework displayed, and above all the choice of a subject so near to the nerve center of American political and social history were to have vast consequences for research and controversy over the following two decades.

WILLIAM R. PARKER, "Historiography", prepared for the *Encyclopedia of American Economic History* (forthcoming).

<sup>16</sup> The pre-1957 state of American economic history is briefly considered below, pp. 199-200.

<sup>17</sup> This expression was coined by a sympathetic 'traditional' economic historian, Carter Goodrich. Cf. "Economic History: One field or Two?" *Journal of Economic History*, Vol. XX (December, 1960), p. 553. For caustic early criticism of the "old" literature see, DOUGLASS NORTH, Review of Historical Statistics of the United States,

of the new techniques and consequent inability to challenge the supposedly dramatic new findings. Generation conflicts are, of course, a familiar feature of periods of rapid intellectual transformation or "paradigm-change", to use T.S. Kuhn's well-known terminology. Not only are the young often more energetic, flexible and innovative; they have much to gain and little to lose from new departures. By contrast, their elders are not only threatened by the obsolescence of their accumulated mental and professional capital; they also have a greater appreciation of the merits of past achievements and procedures, and therefore attach greater weight to the costs of change.

It would, of course, be wrong to imply that there was a clear-cut division of opinion on generational lines.<sup>18</sup> However, the belief that there was such a division was reinforced by a specific event - namely, the rejection of a cliometric article by the editors of the *Journal of Economic History*, apparently on the grounds that it was too technical and statistical. This incident quickly became a *cause célèbre* and a rallying-point for the young innovators.<sup>19</sup> After an organised protest the article was published intact in that *Journal* in 1960, and by 1961 the editorship had passed into new management, both younger in years and more actively sympathetic to the new approaches.<sup>20</sup> This change may

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*American Economic Review*, LVIII (March, 1963), pp. 128-30. Also his "The State of Economic History", *American Economic Review, Papers and Proceedings*, Vol. LV (May, 1965), pp. 86-91.

<sup>18</sup> Some of the older economic historians (eg. Geschenkron and Harold F. Williamson) both sympathized with and actively encouraged the new movement, while reserving judgment about its long term significance. Indeed, Hughes has remarked that: "The guardians of the 'traditional view' did not in most cases attempt to 'legislate' against us". See McCLOSKEY, *Essays op. cit.*, p. 408.

It is difficult to document the reactions of the older generation because, as David Landes has noted, "most of them were too intimidated by the new economic history to challenge it to open combat, "preferring instead to withdraw "into a kind of internal exile within the profession." Cf. "On Avoiding Babel," *Journal of Economic History*, Vol. XXXVIII (March, 1978), p. 5. He notes that the young "proclaimed explicitly or implicitly the obsolescence of their teachers and elders" often without "those formulas of courtesy and euphemism that makes even gall moderately palatable." However the differences between this episode and the British developments referred to in footnote 10, *supra.*, are of course more than merely matters of form.

<sup>19</sup> The article in question, by LANCE DAVIS, "The New England Textile Mills and the Capital Markets: A Study of Industrial Borrowing, 1840-60" was submitted in the fall of 1957.

Cf. J.R.T. HUGHES, "Is the new economic history an export product?", in McCLOSKEY, *Essays, op. cit.*, pp. 411-14.

<sup>20</sup> Up to and including 1960 the *Journal* was edited by George Rogers Taylor, with Associate Editors Vincent P. Carosso, Charles Gilbert, Muriel F. Hidy, and David S. Landes; Assistant Editors Norton Garfinkle and Julius Rubin. From 1961 the co-editors were Douglass C. North and William N. Parker; with Associate Editors Morris

have conveyed the impression that a new era was dawning, arousing alarm and resentment among some members of the older generation.

While Williamstown represented a breakthrough, the new movement really began to gather momentum about 1960. At the annual meeting of the Economic History Association that year the programme included a paper by three young scholars from Purdue who had been invited by the programme chairman, Carter Goodrich, to explain what he termed "La Loi Lafayette".<sup>21</sup> This calm and measured statement of the scope for quantitative methods in economic history evidently caused further disquiet and hostility in some quarters. 1960 also witnessed another significant innovation, the inauguration of a series of conferences independent of the Economic History Association - the aptly titled Seminar on Quantitative Methods in Economic History, at Purdue University.<sup>22</sup> So responsive was the demand, that whereas the organizers of the inaugural gathering had had difficulty in assembling a dozen or so interested scholars, six years later they had to ration invitations "among the tens and then the scores of young scholars who wanted to attend".<sup>23</sup>

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D. Morris, Goran Ohlin; and Assistant Editor Sybil T.W. Beale. Davis's article appeared as the leading item in the last volume edited under the old regime (ie Vol. XX, No. 1, March, 1960, pp. 1-30). Parker was the first member of the younger generation to serve as President of the Economic History Association, from 1969-70. The average age of the President fell by about 10 years between 1960-68 and 1969-78. During the latter period eight of the ten averaged 51 years.

<sup>21</sup> Another significant turning point about this time was the dissolution of the Committee on Research in Economic History, founded in 1940, and its replacement by the Council on Research in Economic History, which had an entirely different membership. The transition was completed by February, 1959. One reason for the change was that "a new generation of economic historians had arisen and seemed to warrant recognition - a generation that might (and really did) have concepts foreign to those of the Committee members". Evidently the Council members were selected by Frederic C. Lane.

Cf. ARTHUR H. COLE, "The Committee on Research in Economic History: An Historical Sketch", *Journal of Economic History*, Vol. XXX (December, 1970), pp. 737-8.

One may speculate as to the effects on the development of American economic history of the Committee's refusal to support Kuznets's investigation of the measurement and causes of economic change. (*ibid.*, p. 728). Although fortunately, Kuznets secured another sponsor, the gap between quantitative economics and economic history would surely have been narrower in the 1950's had the Committee grasped this opportunity.

<sup>22</sup> A list of the papers delivered at the first six Purdue meetings is given in *Purdue Faculty papers in Economic History 1955-1966* (Homewood, Illinois, 1967), pp. vii-viii. The roster includes the names of a high proportion of the leading younger contributors to the subject since the early 1960's. A rough estimate suggests that the average age of the authors of papers at the first six Purdue Meetings was under 35 years. Harold F. Williamson was conspicuous among the few elders on these occasions.

<sup>23</sup> Cf. J.R.T. HUGHES in McCLOSKEY, *Essays*, op. cit., pp. 403-4.

If Williamstown was the catalyst, Purdue became a central focus for advocates of the new ideas, and a sociological and professional location at which group solidarity could be promoted, new disciples enlisted, and young missionaries assembled and dispersed to spread the gospel.

III

So far attention has been focussed on the emergence and initial reactions to the new economic history. It is now time to adjust the lens and view the scene from a broader, more detached standpoint. Although the smoke of the original campaign has long since dispersed, it is the dramatic advent and swift diffusion of the new movement that now seem so striking. How can these features best be explained? According to J.R.T. Hughes, a perceptive participant, the new economic history "is most readily understood in terms of its own history",<sup>24</sup> and internalist explanations of this type are nowadays fashionable among such historians and philosophers of science as Karl R. Popper, Thomas S. Kuhn, and Imre Lakatos. To a foreign observer, however, there are certain transcendent characteristics which support an environmentalist interpretation, and the following paragraphs will seek to display the distinctively American aspects of the story.

The new economic history was, above all, a revisionist movement, and few of its early spokesmen would now deny that its earlier adherents frequently exaggerated the extent of their break with the past and the novelty of their methods. Nor is this in any way surprising. As noted by George Stigler, himself no Trotskyite, the techniques of persuasion and scholarly salesmanship are usually

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<sup>24</sup> HUGHES, *ibid.*, p. 402. Like Lance Davis, Hughes was a product of Douglass North's University of Washington stable, whence he proceeded to Oxford and studied with Professor (now Sir John) Habakkuk. His first book, *Fluctuations in Trade, Industry and Finance* (Oxford, 1960) reflects the influence of Rostow and R.C.O. Matthews', *A Study in Trade Cycle History: Economic Fluctuations in Great Britain 1833-1842* (Cambridge, 1954). After eighteen months with the New York Federal Reserve Bank Hughes joined the Purdue faculty in 1956 and published his first cliometric article (with Stanley Reiter) "The First 1945 British Steamships" in the *American Statistical Association Journal*, Vol. LIII (June, 1958), pp. 360-81. (Doubtless members of the older generation of economic historians would have regarded this as an appropriate outlet) Hughes has subsequently viewed the new economic history with more critical detachment than some of his contemporaries. Cf. his "Fact and Theory in Economic History", *Explorations in Entrepreneurial History*, New Series, Vol. III (1966), pp. 75-100. (Both this and the previously cited article are reproduced in *Purdue Faculty Papers*, op. cit.).

Davis, like Fogel later, worked under Kuznets at Johns Hopkins before moving to Purdue.

required in order to win acceptance of new ideas.<sup>25</sup> Admittedly, Stigler added, some great figures, (e.g. Adam Smith and Alfred Marshall in economics) possess such unusual powers and make such significant contributions that they can achieve recognition without the customary exaggerations; and Marshall's case is pertinent here since he emphasized the continuities between his own neo-classical system and its classical precursors and, in correspondence with his American contemporary Richard T. Ely, strongly deprecated the transatlantic economists' tendency to overstate their claims to originality.<sup>26</sup> Like some of the cliometricians, Ely was a young, outspoken, and occasionally extreme advocate of a "new" economics in the 1880's; he, too, stressed its methodological superiority, less because he considered methodology important as such than because he believed it was the most effective weapon with which to "break the crust" which had formed over American economics.<sup>27</sup> The parallel is obvious, for the more aggressive cliometricians of the later 1950's were seeking to break the crust over American economic history and, like Ely and his co-founders of the American Economic Association, they were asserting their right to exist professionally.

Curiously enough, although methodological controversy had been re-

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<sup>25</sup> Cf. STIGLER, "The Nature and Role of Originality in Scientific Progress", in *Essays in the History of Economics* (Chicago, 1965), p. 5:

"The techniques of persuasion... in the realm of ideas are generally repetition, inflated claims and disproportionate emphases, and they have preceded and accompanied the adoption on a large scale of almost every new idea in economic theory. Almost, but not quite every new idea. A few men have such unusual powers that their contemporaries recognize their claims without the usual exaggerations; Smith and Marshall are the only economists who seem to me indisputably to belong to this supreme class."

<sup>26</sup> A. MARSHALL to R.T. ELY, 28, October, 1901, in the Ely Collection, Wisconsin State Historical Society, Madison, Wisconsin. Having deplored the tendency to exaggerate the novelty of one's ideas, Marshall continued:

Next as to English etiquette. I think few Americans know how far that reaches in the Chinese direction. A Chinaman may be bursting with conceit that he has the most beautiful wife and the most splendid house within a hundred miles. But he will say to a stranger, of lower rank than himself and one on whom he looks down, "will you deign to honour with your magnificent presence my small and contemptible hovel, where I may have the high honour of introducing that ugly old hag my wife to you." But he would be amused if the stranger took him literally and expressed pity for his misfortune in having such a house and wife. We don't go as far as that. But our etiquette does not allow anyone to praise his own work, or even to claim originality, on penalty of being judged an offender against our rather artificial canons of reticence. So Englishmen are rather amused if Americans (whose etiquette, at all events in the West, seems to have no Chinese element, and to allow people to say whatever they think is true and useful to the reader) assume that when they do not claim originality, it is because they do not believe they have anything new to say.

<sup>27</sup> RICHARD T. ELY, *Ground Under Our Feet: An Autobiography* (New York, 1938), chapter IV. As it happened, methodology has probably been of greater importance in the case of the "new economic history" than it was with Ely's "new economics".

current and intense in American economics<sup>28</sup> and in other social science disciplines it had been virtually absent from American economic history - a point that has sometimes been cited as evidence of its intellectual stagnation.<sup>29</sup> This background helps to explain why some of the older generation were shocked by the cliometric challenge, and why they reacted so strongly with charges of "quantomania", "metrophilia" and other species of "methodolatry". No doubt some of the innovators took to the offensive too readily. Yet as Hughes has remarked, it takes two parties to make a *Methodenstreit*, and both sides must bear some of the responsibility.

To European observers it seems clear that the structure and ethos of American academic life are highly conducive to scholarly controversy and new intellectual movements. The richness of scope for the pursuit of new ideas and methods suggested by the collective reminiscences of refugee scholars from Nazi-occupied territories in the 1930's,<sup>30</sup> and confirmed by the experiences of innumerable young graduates who flocked to the U.S.A. after World War II - the influence of McCarthyite and other anti-intellectual prejudices notwithstanding. There are, of course, certain concomitant disadvantages: the over-investment of resources in academic trivia and gimmickry; the energetic pursuit of the novel and the popular; the oft-quoted pressure to publish or perish - in part a by-product of a sense of professional insecurity unfamiliar to many European academics; and - at least in some fields - the exaggerated passion for quantification and measurable standards of performance.<sup>31</sup> To the newco-

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<sup>28</sup> Cf. my unpublished Ph. D. thesis, *Methodological Controversy as an Approach to the History of American Economics, 1885-1930* (Johns Hopkins, 1953).

<sup>29</sup> See, for example, the comments on the earlier state of American economic history quoted in PETER D. McCLELLAND, *Causal Explanation and Model Building in History, Economics, and the New Economic History* (Ithaca, New York, 1975), p. 175. Methodological issues were mentioned, together with questions of scholarly status, integrity, intellectual freedom, and various other matters, in the prolonged and occasionally acrimonious dispute in *Explorations in Entrepreneurial History*, First Series, Vol. VI (1953-4), pp. 1-19, 181-4, 245-97. But curiously enough, in the next volume, Lewis Atherton commented on the absence of methodological discussion at the Center in *Entrepreneurial History*. Cf. *ibid.*, Vol. VII (December, 1954), pp. 107-8. See also *ibid.*, p. 111 ff.

<sup>30</sup> Cf. DONALD FLEMING and BERNARD BAILYN (eds.), *The Intellectual Migration, Europe and America, 1930-1960* (Cambridge, Mass., 1969); also LAURA FERMI, *Illustrious Immigrants: The Intellectual Migration from Europe, 1930-41* (Chicago, 1968). This does not, of course, mean that all new movements are fruitful for many prove abortive, and consume much intellectual energy to little effect. Nor is it denied that there are in many subjects periods of intellectual stagnation or regression, and as indicated below, there are many inferior academic institutions in the U.S.A.

<sup>31</sup> In the history of American higher education one can point to the introduction of the "credit" system with its present-day concomitants of semester-hours, quality point averages, and Graduate Record Aptitude Test Marks. Recent scholars have devoted considerable attention to quantitative measures of publication as a guide to gra-

mer it often appears that the influence of advertising, the volatility of consumer goods markets, and the high rate of technical obsolescence in manufacturing have their intellectual analogues in the academic world. No wonder Thorsten Veblen was moved (eventually) to publish his *Memorandum on the Conduct of Universities by Businessmen*.<sup>32</sup>

By European (and, more especially by British) standards the American academic community is vast, heterogenous, and unstable, with its 2,000 or so colleges and universities ranging (in baseball terms) from the so-called major league through the minor and bush leagues to "academic Alaska" - a revision of the earlier category "academic Siberia" consequent upon the realisation that Siberia now has some first rate educational institutions! Naturally enough, foreigners tend to focus their attention mainly on the leading institutions, and this bias is appropriate when considering the genesis and dissemination of new academic fashions. Yet, as will be seen later, in the present instance the number and range of participating individuals and institutions is significantly greater than might be expected *a priori*. In the social sciences the costs of entry are usually low, hence many persons or groups may be ready and able to jump on any new bandwagon.

In periods of rapid expansion like the 1950's and early 1960's there is exceptionally keen individual, inter-departmental, inter-institutional, and even inter-regional competition for resources, status, and fashionable academic stars. Salaries are flexible; there is a high premium on innovativeness; promising young scholars, like football players, are soon spotted and receive lucrative offers; and the overall mobility of resources and the fluctuations of individual reputations and institutional fortunes sometimes evoke images of the stock exchange rather than the ivory tower. There is, however, a serious foundation for these remarks. As the researches of Joseph Ben-David and other sociologists of science have shown, competitiveness and decentralization in the academic community encourage decisions favourable to the growth of scientific activity, including the recognition of new disciplines, the creation of new research facilities, and the expansion of training facilities.<sup>33</sup> All these features were present in the dissemination of the new economic history during the early 1960's, and as several previous commentators have noted, the sheer scale of

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duate faculty ratings, aids to promotion, and indexes of individual academic achievement. There is, generally speaking, much greater scepticism towards quantification of this kind among European scholars than among Americans.

<sup>32</sup> The full title was *The Higher Learning in America, A Memorandum on the Conduct of Universities by Businessmen* (New York, 1918). It was apparently written about a dozen years earlier.

<sup>33</sup> Cf. JOSEPH BEN DAVID, "Scientific Productivity and Academic Organization in Nineteenth Century Medicine", *American Sociological Review*, Vol. XXV (December, 1960), pp. 828-43; *idem.*, and RANDALL COLLINS, "Social Factors in the Origins of a New Science: The Case of Psychology", *ibid.*, Vol. XXXI (August, 1966), pp. 451-65.

the American academic community made it comparatively easy to form a critical minimum mass of enthusiasts sufficient to launch the new movement and carry it forward.<sup>34</sup>

William Parker has recently maintained that there were five distinct "*poles de croissance*" of the new economic history:

- i) the two collaborative Conference on Research in Income and Wealth volumes;
- ii) the Purdue meetings;
- iii) Gerschenkron and his Harvard graduates;
- iv) Douglass North and his University of Washington stable;
- v) and Robert Fogel, an individual scholar of exceptional power and energy.<sup>35</sup>

Of these, Purdue University is of special interest in the present context since it seems to exemplify, in academic terms, the American "rags to riches" saga. Without wishing to underrate the contributions of other individuals and groups, the Purdue cliometricians do appear in retrospect as curiously representative embodiments of the American academic culture. Purdue University had no prior standing in economic history in the early 1950's; the campus was far removed from the established academic centres; for the favoured few, salaries were unusually good, teaching duties were comparatively limited, and incentives to publish were high; research facilities (especially in hardware, rather than conventional library resources) improved rapidly, and there was an atmosphere of intense competition and intellectual excitement which was conducive to rapid and boldly innovative research output. Under the able and energetic leadership of Dean Emmanuel Weiler, a man whom Veblen would readily have acknowledged as a "Captain of Erudition", an exceptionally vigorous group of able, thrusting, and prolific young economist-historians was assembled, and they were already producing sophisticated, if not truly path-breaking quantitative studies when Conrad and Meyer's two articles appeared. In his editorial introduction to the impressive collection of *Purdue Faculty Papers in Economic History, 1956-66*, Weiler claimed, with some justification, that the new economics department's programme in quantitative economics (sic) was unique in its emphasis on the role of economic history in

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<sup>34</sup> For example, R.C.O. MATTHEWS, "The New Economic History in Britain: A comment on the papers by Hughes, Hartwell and Supple" in McCLOSKEY, *Essays*, op. cit., p. 432, where he refers to "an absolute number sufficiently large to generate *esprit de corps* and other external economies." Also the perceptive remarks by William N. Parker in "Work in Progress. A Report to Ernst Söderlund", *Scandinavian Economic History Review*, Vol. X (1962), pp. 233-31.

<sup>35</sup> From his as yet unpublished essay on "Historiography", for the *Encyclopedia of American Economic History*.

the training of professional *economists*, adding modestly, with only mild exaggeration, that their work had "sparked a revolution in the study of economics using empirical materials drawn from history".<sup>36</sup> As we have seen, this was but one of a number of growth poles. But in the early 1960's Purdue undeniably became the main centre of cliometric evangelicalism.

As it transpired, Purdue's ascendancy proved transitory. For a time, owing to an unusually favourable concatenation of circumstances, it had a batting average well above its customary standing in the academic leagues. But before long some of its leaders were keen to break away to join teams in higher divisions. Their actual high salaries — and their reputedly even higher salaries — occasionally made this difficult, both psychologically and professionally. But in due course market and status forces asserted themselves.<sup>37</sup> Within a decade or so the core group had broken up, most of its members moving to well paid chairs in other, generally more prestigious, universities. Nevertheless the seed had fallen on fertile ground. By 1966, as Fogel has noted, training and research in econometric (or "new") economic history was well under way at Harvard, Washington, Wisconsin, Pennsylvania, Berkeley, Stanford, Yale and Chicago; and doubtless the publication of this roster was intended *pour encourager les autres*.<sup>38</sup>

#### IV

Having considered the general academic conditions favouring the rise of the new economic history it is now appropriate to ask why it emerged at that particular time (i.e. the late 1950's and early 1960's). All previous commentators agree that the movement stemmed from an influx of young economists into American economic history, and that this reflected the growing scale and resources of the economics profession. During the late 1930's and 1940's American economics had been transformed, initially by the Keynesian revolution, and subsequently by the adoption of much more sophisticated mathematical techniques such as linear programming, activity analysis, operations research, input-output analysis, etc. As a result, after World War II the level of technique demanded in the leading graduate schools of economics rose sharply. Hence it followed that if a new type of economics-based economic history was

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<sup>36</sup> From his introduction to *Purdue Faculty Papers in Economic History, 1956-1966* *op. cit.*, p.v.

<sup>37</sup> According to J.R.T. Hughes, it was suggested in 1960 that "we could not hold Purdue together because it didn't make sense for any one department to get so far ahead of the university in general". From a private letter of 14, March, 1977.

<sup>38</sup> ROBERT W. FOGEL, "The New Economic History: Its Findings and Methods", *Economic History Review* 2nd series, Vol. XIX (December, 1966), p. 643.

to be established and taken seriously it would have to conform in some degree to these fashionable new standards.

Yet while American academic economics was rising to the position of world leadership which it has occupied ever since, American economic history was comparatively stagnant. In assessing the situation there is, admittedly, some danger of overstating the contrast, especially when drawing on the reminiscences of the young innovators of the 1960's, who were naturally tempted to depict the past in sombre tones in order to enhance the brilliance of the new cliometric dawn. Nevertheless, even after due allowance for this bias, it seems clear that the older generation of academic economic historians, like the pro- and anti-Keynesian economists in the 1940's, were largely ignored or swept aside by the new movement. In the words of a moderate cliometrician with a well-developed historical perspective:

By the early 1950's the profession of economic history was largely being recruited from the ranks of graduate students in "straight" history. The small number of ranking economic historians trained as economists age 45 to 65 is dramatic evidence of this history. By 1950 the major textbook used in undergraduate courses remained largely unchanged since 1924. The revolutionary changes in the logic of economic analysis had passed American economic history by.

But when the change came to economic history it seemed terribly radical because of the long hiatus.<sup>39</sup>

There is abundant evidence of the debilitated state of American economic history in the early postwar years. As early as 1927, N.S.B. Gras had remarked in the first issue of the *Economic History Review* that in the United States "the subject has no intellectual resilience",<sup>40</sup> and the *Journal of Economic and Business History*, founded in 1928, collapsed four years later for lack of support. The Economic History Association was founded in 1941, fifteen years later than its English counterpart, and the *Journal of Economic History* was launched. Yet it was said that the subject was stagnant and research was at a standstill - hence the creation of the Rockefeller-financed Committee on Research in Economic History.

Even with this backing, progress was slow. Like Gras twenty years earlier, T.C. Cochran complained in 1947 that: "No new schools of thought built around the work of any great American economic historians have as yet appear-

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<sup>39</sup> J.R.T. HUGHES, "Fact and Theory in Economic History", in RALPH L. ANDREANO, *The New Economic History: Recent Papers in Methodology* (New York, 1970), p. 48. Originally published in *Explorations in Entrepreneurial History, Second Series*, Vol. 3 (Winter, 1966).

<sup>40</sup> Quoted in D. MCCLELLAND, *Causal Explanation and Model Building in History, Economics and the New Economic History* (Ithaca, 1975), p. 175.

ed'.<sup>41</sup> The following year it might seem that his plea was answered with the opening of the Research Center in Entrepreneurial History at Harvard, an institution designed to revitalise the study of economic history rather than simply to promote the study of entrepreneurship, following the lead of that outstanding economist and economic historian, Joseph Schumpeter. However, despite some stimulating work by an able group of scholars, and the founding of a publication, *Explorations in Entrepreneurial History*, which was initially designed solely for internal circulation, direct Schumpeterian influence was much less marked than might have been anticipated, and the main thrust of work was towards sociology and anthropology, rather than economics.<sup>42</sup>

Studies in entrepreneurship and business history flourished in the 1950's, but neither provided the desired link between economic history and economics. In 1960 Hugh Aitken, one of the leading products of the Entrepreneurial Center, contrasted the healthy respect for economic history among American historians with its low esteem among economists, despite the shift of their interest towards long run problems of economic development. "The present conjuncture of circumstances" he observed:

represents a unique opportunity for both economic theory and for economic history. The possibility for reintegration of the two fields..., is greater now than it has been for a long time. The future of economic history as a discipline depends very largely on the extent to which this uniquely favourable opportunity is grasped.<sup>43</sup>

The need was patent, he emphasized, for economic history was then in a "critical condition". It "commonly uses only the crudest of the tools in the economist's tool-box, and displays almost complete indifference to the refinements in analytical methods that occupy the theorist's working time." Nevertheless, unlike some of the more extreme advocates of the new economic history, Aitken's recommendations for the "reintegration" of economic hi-

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<sup>41</sup> Quoted from "Research in American Economic History: A Thirty Year Review", *Mid-America*, Vol. 29 (January, 1947), in McCLELLAND, *op. cit.*, p. 175. Similar views are quoted from Herbert Heaton (1942) and A.H. Cole (1944). For general background accounts see A.H. COLE, "Economic History in the United States, Formative Years of a New Discipline", *Journal of Economic History*, Vol. XXVIII (December, 1968) pp. 556-589; IDEM., *The Birth of a New Social Science Discipline: Achievements of the First Generation of American Economic and Business Historians, 1893-1974* (New York, 1974); and T.C. COCHRAN, "Economic History Old and New", *American Historical Review*, Vol. LXXIV (June, 1969), pp. 1561-1572.

<sup>42</sup> There is a penetrating account of this episode in HUGH AITKEN (ed.), *Explorations in Enterprise* (Cambridge, Mass., 1960), pp. 3-19. Aitken notes that there was to some extent an anti-economic attitude at the Harvard Research Center.

<sup>43</sup> HUGH AITKEN, "On the Present State of Economic History", *Canadian Journal of Economics and Political Science*, Vol. 26 (February, 1960), p. 91.

story with economics did not, as some cliometricians seemed to imply, entail the disappearance of the former; nor did it call for a severance of ties with history. On the contrary, he pleaded for:

a substantial raising of standards; a more rigorous training in both history and economics; a more determined effort... to attract intelligent and imaginative students and to offset to some extent the competing attractions of pure theory; and a conscious effort on the part of practising economic historians to move away from the antiquarian and merely descriptive and toward the illumination of analytical problems. What is suggested here is, after all, not the introduction of something new into economic history, but rather the return to a theme which was the original concern of the founders of the discipline and which has never been completely lost sight of; the study of economics in the process of growth, stagnation, and decline.<sup>44</sup>

This moderate, common sense course of action was certainly open to the younger generation of American economic historians, as it was to their transatlantic peers. Nevertheless, in the event, they jumped on the cliometric bandwagon as it gathered momentum at an exhilarating rate. Why was this?

In order to understand the situation we must look more closely at some of the changes taking place in American economics, since this was the source of the new initiative. During the 1930's many economists were preoccupied with short run business cycles and fluctuations, whereas a few years after World War II fears of a postwar slump faded, and there was a marked shift of interest towards problems of long run economic development, especially with reference to underdeveloped countries. Admittedly there was a reaction, in due course, as economists gradually became aware that their elegant theories and and sophisticated techniques could not, by themselves, provide effective solutions to the complex socio-political and cultural problems of backward communities which differed so dramatically from the advanced Western economies that had formed the background to the evolution of modern economics. Indeed, it seemed possible that modern economics was culture-bound, as some German historical economists had maintained during the 19th century *Methodenstreit*. Yet before that realisation became general, there was a parallel growth of interest in the problem of the long run development of the U.S.A. itself. This was partly the result of the successful launching of the Russian Sputnik, in 1957, which revealed that the threat of Russian economic and technological competition was more imminent and serious than had hitherto been supposed. Anxious comparisons were made between current American and Russian economic growth rates, to the detriment of the former, and questions were asked why the performance of the American economy had deteriorated so sharply since the 19th century, when it had been the economic wonder of

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<sup>44</sup> *Ibid.*, pp. 87, 92.

the world. What was the secret of America's 19th century success? If 19th century experience was studied closely enough would it reveal lessons of the past which could be applied to the present? Of course few serious scholars posed the issues in such simplistic terms. But concerns of this kind undoubtedly affected public attitudes, encouraged young students to turn their minds to these problems, and created an environment highly favourable to the funding of research studies of past and prospective comparative economic growth performance.<sup>45</sup> For a time, at least, the interests of economists and economic historians appeared to converge.

Of course, the best known academic example of this genre was W.W. Rostow's *The Stages of Economic Growth* which was published in 1960, but which had been several years in the making.<sup>46</sup> This *Non-Communist Manifesto*, to quote its subtitle, is but one of many examples of the present-mindedness of American academics, whose ivory towers — if, indeed, they have them — are far more often located in or near the market place than their British counterparts. American scholars, especially the natural and social scientists, had every inducement to obtain funds for ambitious research projects which promised to shed light on the current and past condition of American society — both economic growth narrowly conceived, and its wider context — e.g. technological change, the role of education, and social attitudes towards innovation. It is worth noting, in passing, that Rostow's important 1940's and early 1950's studies of the British economy did not have the same impact on the British academic community as the studies of American economic growth had on the Americans a decade later. And of course in the American case, the work of Kuznets and the National Bureau formed a solid background for research.

The questions posed by Rostow and others concerned general economic processes — the historical causes of economic development rather than the economic causes of historical development. By contrast, the application of econometric techniques by Conrad and Meyer and others to the study of the past involved a considerable *restriction* of the historian's focus. The more generous perspective adopted by entrepreneurial historians and advocated by Aitken — whereby the economic historians would provide the main intellectual linkages between development economists and general historians — was scorned, in the course of repeated attacks on narrative, descriptive and institutional history. Yet despite their insistence on the need to use explicit economic assumptions and formal models, the cliometricians were remarkably reticent about

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<sup>45</sup> This helps to explain, why, as J.R.T. Hughes noted in 1970, the new economic historians discovered that there was a student demand waiting to be tapped. Cf. "Is the New Economic History an Export Product?", McCLOSKEY, *Essays*, p. 404.

<sup>46</sup> (London, 1960). The most timely, stimulating, and influential economic history was DOUGLASS C. NORTH, *The Economic Growth of the United States, 1790-1860* (Englewood Cliffs, New Jersey, 1961).

their own philosophical, political, and sociological preconceptions,<sup>47</sup> and in this respect, as in others, their approach directly reflected the contemporary climate of academic opinion.

The sociologist, Daniel Bell, described the 1950's and early 1960's as the *End of Ideology*,<sup>48</sup> a period of "consensus" historical writing in which emphasis was placed on the continuities of the American past.<sup>49</sup> Episodes of revolution, conflict and violence were played down, in marked contrast to the late 1960's and early 1970's, when historians were reacting to the serious domestic crises associated with the civil rights movement, urban violence, and the bitter divisions aroused by the Vietnam war. At the time when the seeds of the new economic history were being sown and the early crops were being gathered in, a more optimistic and complacent mood prevailed. Some years earlier the American academic community had been retreating before the assaults of the McCarthyite anti-Communist witch-hunters, and there was a natural inclination to concentrate on safe, uncontroversial topics.<sup>50</sup> In American sociology, for example, this period witnessed the ascendancy of Mertonian and Parsonian functionalism - a positivistic, analytic, ostensibly objective and scientific approach which radical and Marxist critics subsequently charged with concealing an implicit bias in favour of the status quo.<sup>51</sup> No doubt their case was overstated; but there was a preoccupation with static structures and a neglect of dynamic social processes and alternative socio-economic systems. Functionalist sociology was more self-consciously theoretical and technical than other modes, and its positivistic emphasis reflected a genuine urge to escape from earlier normative, value-laden approaches.

Much the same was true of the new economic history. Broad, vague, potentially sensitive themes like the history of capitalism, the social consequences of industrialism, and the nature and causes of poverty, were eschewed in favour of politically safe exercises deploying positivistic techniques and orthodox neo-classical models.<sup>52</sup> As J.R.T. Hughes proudly declared in 1970, the atti-

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<sup>47</sup> Cf. GEORGE GREEN, following FRITZ REDLICH, in "Comment", ANDREANO, *op. cit.*, p. 105. Originally published in *Explorations in Entrepreneurial History, Second Series*, Vol. 6 (Fall, 1968).

<sup>48</sup> (New York, 1964).

<sup>49</sup> For an account of consensus history see, for example, RICHARD HOFSTADTER, *The Progressive Historians: Turner, Beard, Parrington* (London, 1969), p. 438 ff.

<sup>50</sup> P. LAZARSFELD and W. THIELENS, *The Academic Mind, Social Scientists in a Time of Crisis* (Glencoe, Ill., 1958) for a general survey of the situation.

<sup>51</sup> Cf. ROBERT W. FREDRICKS, *A Sociology of Sociology* (New York, 1970), Chapter 4, "The Cloak of Neutrality".

<sup>52</sup> For criticism emphasizing 'the positivistic bias of the new economic history and its abstraction from non-economic issues' see, for example FRITZ REDLICH, "Potentialities and Pitfalls in Economic History", in ANDREANO, *op. cit.*, pp. 85-99, and REDLICH, "New and Traditional Approaches to Economic History and their Interdependen-

tude underlying the new economic history was "utterly foreign to those who view history through normative lenses, or as a platform for political propaganda".<sup>53</sup> The context of these remarks was his repudiation of the charge that the cliometricians were deliberately prescribing methodological rules of correct practice, a charge that was not entirely without substance in the early 1960's. In the present context, however, the relevant consideration is the presumption that a value-free history is attainable by the use of scientific methods - which is a very different kettle of fish. As Albert Fishlow, himself no enemy of the new economic history, recently observed, the new economic history was "viscerally conservative"; and in its emphasis on rationality and individual preferences conventional theory became "as much an article of faith as Marxist theory".<sup>54</sup>

It would, of course, be misleading to imply that the new economic history was attractive merely because it was a safe activity in a period of academic pressure and persecution. Indeed, it lasted too long to be explained in such simplistic terms, and the main phase of McCarthyite pressure was over before the late 1950's. Moreover, the preoccupation with techniques and quantification was not unprecedented. On the contrary, it was characteristically American, and squarely in the central tradition of pragmatism, which has sometimes been described as the "official" twentieth century American philosophy. Because there had so long been a broad consensus on values, disagreements tended to focus on methods rather than ends, as Louis Hartz and other commentators have noted. "The American", Hartz observed, "deals with concrete cases because he never doubts his general principles..."

American pragmatism has always been deceptive, because it glacierlike has rested upon miles of submerged conviction... it is only when you take your ethics for granted that all problems appear as problems of technique.<sup>55</sup>

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dence", *Journal of Economic History*, Vol. XXV (December, 1965), pp. 480-495. These papers have been reprinted in FRITZ REDLICH, *Steeped in Two Cultures. A Selection of Essays* (New York, 1971), pp. 339-74.

For comments on the parallels between the confidence of economists and of new economic historians in the early 1960's see ALBERT FISHLOW, "The New Economic History Revisited", *Journal of European Economic History*, Vol. 3 (Fall, 1974), p. 459.

<sup>53</sup> HUGHES, in McCLOSKEY, *Essays*, p. 408.

<sup>54</sup> FISHLOW, *op. cit.*, pp. 461, 463.

<sup>55</sup> LOUIS HARTZ, *The Liberal Tradition in America: An Interpretation of American Thought Since the Revolution* (New York, 1955), pp. 59, 10. For a general application of this theme to political writings see BERNARD CRICK, *The American Science of Politics* (London, 1959); and for a specific application to American economic history see FRITZ REDLICH, *Steeped in Two Cultures, op. cit.*, p. 356.

Thomas C. Cochran has linked the pursuit of scientific objectivity, consensus, and disagreement over methods, as follows:

"The American idea is an empirically based objectivity, which in practice means accepting existing institutions as the framework for analysis. Writing that openly argues for a new

Despite their preoccupation with economic theory and their assertive use of methodology as a means of crushing their opponents, the new economic historians were often epistemologically naive. They took their paradigm from mainstream economics and concentrated on revising the past in positivistic, and — in a sense — philosophically superficial terms. As William Parker, the first member of the younger postwar generation to become President of the Economic History Association, has remarked, in a variety of ways

the new work justified the ways of the price system to men. And rightly so. If a model of an economy with flexible prices does not test out in early nineteenth century America, where, one wonders, is it to be used? The great constructive contribution of this work lay — I would suggest — in its shadowy adumbration of the machinery of a growing competitive economy. A sympathetic imagination can pick up the noise of growth behind these essentially static, statistical tableaux.<sup>56</sup>

This may, however, be too favourable an interpretation, when one considers the distortions of the past which may result from an excessive reliance on price analysis — as, for example, in some of the recent American literature on slavery. A more critical attitude towards the use of econometric methods in history is that of Paul David, one of the younger exponents of the new techniques, who has complained that they represent

a form of intellectual tunnel vision. It manifests itself most frequently in a penchant for focusing exclusive attention upon the confrontation between a terribly restricted set of observations and an equally narrowly specified hypothesis concerning some facet of economy behavior.<sup>57</sup>

To a more politically radical and somewhat older economist-historian, Douglas Dowd, the same complaint is coupled with the charge that the new economic historians are, like the Keynesians, simply irrelevant to the current "social crisis". Like economists of the "neo-classical persuasion" they were

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ideology or shows a missionary bias in favour of radical change in major institutions is regarded as unscholarly and unreliable. This may explain why battles in American scholarship are so often over methods."

Cf. "Economic History Old and New", *op. cit.*, p. 1569.

<sup>56</sup> WILLIAM N. PARKER, "Through Growth and Beyond: Three Decades in Economic and Business History", in LOUIS P. CAIN and PAUL J. USELDING, *Business Enterprise and Economic Change, Essays in Honour of Harold F. Williamson* (Kent State University Press, 1973), p. 23.

<sup>57</sup> PAUL DAVID, "Comments on Gavin Wright, Econometric Studies of History" in MICHAEL D. INTRILLIGATOR, *Frontiers of Quantitative Economics* (North Holland, Amsterdam, 1971), p. 464.

“serving the needs of the business society in which they lived, whatever they may have thought they were (or are) doing”.<sup>58</sup>

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It would be neither appropriate nor wise to offer yet another general evaluation of the significance and prospects of the new economic history. The debate is continuing.<sup>59</sup> Despite earlier efforts to present it as the new orthodoxy it has not displaced “good old economic history”, as Max Hartwell has called it.<sup>60</sup> Indeed, there have been distinct indications of a convergence, even a concordat between the two. Needless to say, the character and content of the discipline has been permanently and substantially changed since the early 1960's, especially in the U.S.A. Although some of the glamour of the “star hours” has faded, and divisions have emerged within the ranks of the (now ageing) younger generation, the subject is very much larger, more widely recognised, vigorous, and creative than it was twenty years back. Moreover, despite the appearance of a somewhat misleadingly termed neo-institutional movement,<sup>61</sup> economic history in America is still dominated by the economic viewpoint, and firmly bound to neo-classical price theory. However, to end on a more speculative note, that link may nowadays seem less attractive in the light of the recent widely canvassed “crisis” in economics;<sup>62</sup> and the location in economics departments, which was so obviously a source of strength in the early stages of the new movement, may turn out to be a handicap to future developments away from economics at a time of constrained university resources.<sup>63</sup> It may

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<sup>58</sup> DOUGLAS F. DOWD, “Economic History in the Twentieth Century”, in HERBERT J. BASS (ed.), *The State of American History* (Chicago, 1970), pp. 263, 266, 268.

<sup>59</sup> See, for example McCLOSKEY *et al.* in the *Journal of Economic History*, Vol. XXXVIII (March, 1978), pp. 3-28, 71-80.

<sup>60</sup> MAX HARTWELL, “Good Old Economic History”, *Journal of Economic History*, Vol. XXXIII (March, 1973), pp. 28-40.

<sup>61</sup> See, for example, LANCE E. DAVIS and DOUGLASS C. NORTH, *Institutional Change and American Economic Growth* (Cambridge, 1971), and DOUGLASS C. NORTH and ROBERT PAUL THOMAS, *The Rise of the Western World: A New Economic History* (Cambridge, 1973).

This should not be confused with the so-called neo-institutionalist movement celebrated by ALLAN G. GRUCHY, *Contemporary Economic Thought: The Contribution of Neo-Institutional Economics* (New York, 1972), and the discussion in recent volumes of the *Journal of Economic Issues*.

<sup>62</sup> See my paper, “The Current ‘Crisis’ in Economics in Historical Perspective”, *Nebraska Journal of Economics and Business*, Vol. 16 (Summer, 1977), pp. 3-16.

<sup>63</sup> I owe this suggestion to a discussion with Paul David. See the comment in his *Technological Choice, innovation and economic growth. Essays on American and British*

yet be worth noting that in postwar Britain the process of economic history was most rapid and balanced when separate departments were established, a process that did not necessarily entail severing ties with either of the parent disciplines, economics and history.<sup>64</sup>

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*experience in the nineteenth century* (Cambridge, 1975), pp. 10-16. For other recent expressions of concern at the prospect of professional isolation see LANDES, "On Avoiding Babel", *op. cit.*, p. 12; and NORTH, "Discussion", *Journal of Economic History*, Vol. XXXVIII (March, 1978), p. 78, where he remarks that the application of neo-classical economics to the past is a contribution that "quickly runs into diminishing returns and leaves the economists with the conviction that we are marginal if not dispensable to the profession."

<sup>64</sup> For a penetrating brief background account see the introduction to NECLEY HARTE (ed.), *The Study of Economic History. Collected Inaugural Lectures 1893-1970* (London, 1971), pp. xi-xxxix.

