CONFERENCE REPORTS

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In its fifty years of life the Economic History Society has faced many challenges and problems, the most recurrent of which has been the struggle to establish the distinctive features of the discipline which it represents. As the Society's Secretary, Professor T. C. Barker, pointed out in one of the conference sessions, this was a problem which accompanied the Society not only from its birth in 1927 — and from which Keynes witheld his all-important recognition — but stretched back to the years of conception as well, to the debates between Marshall and Cunningham in the 1890s. Although necessarily changing with the shifts and developments within the two flanking disciplines themselves, the problems of creating a new applied discipline from the not always complementary matrices of theory and empiricism, and of establishing a paternity in either historical studies or economics — or even of accepting a multiple paternity — which would be sufficiently tolerant to allow the independent development of the offspring, continue to exist. In fact, in recent years the challenges have been as strong, if not stronger, than ever, and the rise of interest in social history, on one hand, and the developments in both applied and theoretical economics on the other, led many to predict the imminent demise of economic history.

One of the general impressions which arose from the Cambridge conference, however, was that these challenges had not created the predicted crisis of identity. In part, and conforming suitably with current methological interests, the evidence for this was quantitative. After remaining fairly stable for the first twenty years of the society's existence, membership began to expand rapidly after 1947, and over the last fifteen years individual membership has doubled while institutional membership has increased six-

fold in the same period, reflecting of course the proliferation of economic history departments in the years of rapid university expansion. With or without an identity, economic history has become established.

The activities and discussions of the Cambridge week-end, however, provided indications of a more qualitative nature that, far from being submerged by the new challenges, economic historians have emerged with a new confidence and breadth, both in terms of the scale of the problems they are prepared to confront and of the technical appartus which they have at their disposal. Nor should this be surprising, for what lay behind the assaults on traditional economic history differed little from the changes affecting historical studies as a whole, and criticism was directed not so much at the relevance of the issues with which economic historians were concerned, but rather with the ways in which those issues were posed and approached. The development of both social history and the economics of growth served, in fact, to accentuate the relevance and importance of the economic historians' traditional concerns, to place them perhaps in broader contexts, but to make both more explicit and more immediate our need for precise explanations and information. On the part of the economic historians themselves the result has been a greater preparedness to view economic processes in the context of a broader and more complex concept of economic and social development — which has involved not only striking out on new paths, but also returning and reappraising some of the classical contributions within their own tradition — a tendency to avoid closed sectorial analysis, and a new interest in the problems of balancing the general and the particular, the theoretical and the empirical. But confidence, while generally a healthy sign, is not an immediate panacea, and there are still major problems and uncertainties — not all of them by any means new — to be confronted. There was perhaps evidence of a certain reticence in this respect in the fact that the majority of the papers delivered at the conference, with certain exceptions, were devoted to demonstrating the potential scope and interest of current research-in-progress and to discussing the problems of an essentially technical nature to which they gave rise, without attempting to relate these to the framework of broader assumptions on which the identity of the discipline must ultimately rely.

Although the papers delivered can be grouped around a number of general issues, there was no single theme and they served rather to indicate the main areas in which economic history research in British universities is currently being directed. Because the conference was divided roughly chronologically into two parallel sessions we can only report on certain of the topics and discussions in the present note, and we have had to leave aside those primarily concerned with the medieval and early modern period.[1]

[1] The papers given at the conference which are not discussed in the present note included: *Manorial Wages in the South of England 1200-1350* (Dr J.Z. Titow); *American*

Of the remainder, the largest group were concerned with various aspects of problems of economic growth: the measurement of capital formation in pre-statistical Britain; Professor F. Crouzet's Tawney Memorial Lecture on the role of exports in Britain's economic growth in the late XVIIIth and early XIXth century; capital formation in Central-Eastern Europe between the wars; and Dr Hopkins' paper on African economic history. A second group was formed by the papers and discussions on internal migration in Britain in the XVIIth, XVIIIth and XIXth centuries. In addition Professor R. Fogel's paper on "Cliometrics and Culture" bore witness to the continuing interest in problems of quantification, even if on this occasion from a new slant.

The range of problems presented did indicate the vitality of current interests, and also the influences which those external pressures we mentioned above have had on the internal activities of the discipline. The existence of certain general themes also served to provide a degree of coherence, but it was unfortunately one of the failings of the conference that there was too little opportunity either to discuss individual papers at any length from the floor, or to attempt to draw inferences from one session to another. Two of the papers -- those given by Dr Feinstein and by Dr Anderson — were followed by discussion sessions but these, although valuable, tended to be dominated by the formal platform discussion to the exclusion of participation from the floor. While it is clearly the case that unstructured discussions are frequently disjointed and unproductive, and also that a programme of this scale necessitates a very tight agenda, greater opportunity for open discussion would still seem desirable.

It was only natural that in addition to the various general themes mentioned above, most of the papers given at the conference bore more or less directly on the problems of quantification — both new and old style, if they can be distinguished -- in economic history. The technical problems involved in quantification were elaborated in particular in the paper given by C. H. Feinstein (*Estimates of Capital Formation in Britain 1760-1860*)[2] and in the following discussion. Professor Sidney Pollard, director of the SSRC supported research project at Sheffield Univeristy on capital formation in Britain, chaired the session, and when introducing Dr Feinstein's paper

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Precious Metals and the European Economy 1650-1800 (Dr H.E.S. Fisher); *The English Countryside in the Early Fourteenth Century* (Discussion Panel chaired by Mr E. Miller: discussants, Miss Barbara Harvey, Dr R. Britnell and Dr M. Prestwich). Research papers were also given on: *Politics and Pyrites during the Spanish Civil War* (C. Harvey); *English Government Expenditure and Industry (1686-1720)* (R. Saville); *Food Riots in Birmingham and the Black Country in the 1790s* (M. Smith); *The Shearmen and the introduction of machinery in the West of England Woollen Industry (1790-1810)* (A. Randall).

[2] Dr Feinstein's paper and estimates summarized his chapter in the forthcoming volume of the *Cambridge Economic History of Europe.*

suggested that the very fact that an attempt had now been made to compute capital formation for a period in which there are no continuous series of records or information, indicated the progress made in economic history in the last twenty years. In a field in which respected economic historians had felt justified, even for more recent periods, to rely on informed guesswork, there was now a new awareness of the need for precise and accurate information and for the development of techniques and methods for deducting that information.

Despite the enormous labour and ingenuity involved in computing his own estimates, Dr Feinstein was the first to stress the essentially conjectural nature of many of the figures on which they were based. But despite these uncertainties, he argued, the exercise was still worthwhile because the role of capital formation in industrialization could not be assessed in the absence of data series. There were serious discrepancies between recent estimates of capital formation in both the XVIIIth and XIXth centuries, and virtually no information on capital stock. The new estimates are for gross domestic fixed capital formation and domestic reproducible fixed assets in 13 sectors of the economy, and were calculated in both current prices and in 1851-60 fixed prices, which made possible internal checks and cross-references for the two series.

Dr Feinstein pointed out that the figures for industry and agriculture, the two crucial groups, were especially difficult to assess. Scarcity of evidence mean that the figures for capital expenditure in agriculture (which in his estimates showed an increase between 1761-1860 from 6% to 15% of gross rent) were tentative and that even minor alterations would have a major impact on the estimates for gross capital expenditure at both current and fixed prices. In the subsequent discussion, however, Dr Holderness claimed that his own research supported Dr Feinstein's findings for agriculture and that they were less tentative than suggested. He did, however, mention the enormous difficulty involved in computing gross capital stock in the absence of information on the number of farmsteads existing before 1860, which meant that the overall pattern of his estimates conformed with Dr Feinsteins (gross capital stock doubled 1760-1860) although the volume was in fact double.

The estimates for industry, where Dr Feinstein again stressed the difficulty of computation, were more strongly contested in the discussion. For buildings he had extrapolated backwards from data for 1860, and similarly, although using more complex procedures, for machinery and equipment. In the discussion, Dr Chapman suggested that it was extremely difficult to extrapolate retrospectively in the case of machinery, and that no simple ratio between capital and production could be assumed without more information on productivity. He also questioned the reliance on the textile industry as a basis for computation, arguing that by 1830 the industry

accounted for only one-third of the work force, and that for most of the period manufactured goods were produced on a proto-factory basis, thus making the textile industry in many ways atypical. Both he and Dr Jenkins also queried the evaluation of spindles and looms and suggested that they were too high.

For transport there is of course more information available for the earlier period, but in the discussion Mr Craig pointed out that the constant adaptation and renovation of existing stocks and assets, particularly in shipping but also in other sectors, created a number of very major difficulties for the computation of gross stock and its evolution.

The debate was then more specific and more technical than that which accompanied the launching of the Sheffield research project seven years ago, and it was clear that many of the major problems defined at that time still existed.[3] At the same time, Dr Feinstein's estimates indicate the progress that has been made in practical terms, and, even if problems remain, there are now a series of coherent and positive propositions against which they can be examined. It is also important to remember, as Dr Feinstein pointed out, that the principal interest of the estimates lies not so much in the absolute figures as in the movements they reflect. As must be the case with any pioneering venture, considerable further research will be required before the validity of the estimates can be establishd, but in the meantime, by creating an indispensable basis and guide, it will be largely due to their existence that such research can go ahead.

Another contribution to the study of capital formation was made by Professor Alice Teichova (*International Aspects of Capital Formation in Central Europe in the Inter-war Period*), in what was also the only conference paper devoted to Continental European economic history. Already well known for her important study of international business in Czechoslovakia in the interwar years, in her paper Prof. Teichova was concerned with the importance and effects of foreign investment in central eastern Europe — Czechoslovakia, Poland, Hungary, Jugoslavia and Bulgaria — as a whole, and her discussion was based on a series of estimates of national incomes, levels of foreign investment, of comparative long-term foreign investments, of rank order of industries in terms of foreign investment and other aggregate or specific examples, which were themsleves the product of very extensive and often difficult research. Like Dr Feinstein, however, she prefaced her discussion by pointing out that much more empirical information was still required before her conclusions could be confirmed. Taking the area as a whole, she argued that shortage of capital constituted one of the major obstacles to industrial development in this period. A generally poor and

[3] The proceedings were published under the title *Aspects of Capital Investment in Great Britain 1750-1850* ed. J. Higgins & S. Pollard (Methuen 1971).

over-populated agriculture could contribute little, and those banks which were not ruined by the war or the world economic crisis were hardly better off. As a result industry was either self-financing or else reliant on State assistance, by means of tariffs, taxes and subsidies. Czechoslovakia alone constituted an exception, while *étatism* reached its height in Poland in the 1930s. But even when this did occur, the levels of foreign debts served to frustrate and compromise State investments.

In such a situation capital imports were clearly of major importance, and Prof. Teichova was particularly concerned to examine their effects both on the industries in which they were concentrated, and on the process of industrialization in these countries as a whole. She argued that, in terms of production, the results were in fact slight, as most foreign loans went into sectors of consumption, luxury commodities and military expenditure, leaving only some 20% for investment in productive industries. The importance of foreign capital invested directly in banks and industries was much greater, however, and was of critical importance in moulding the structure and character of industrialization in these countries. These investments were concentrated in mining and capital goods industries, and especially in the more concentrated branches of those industries, so encouraging and accelerating the tendency toward the formation of monopolies. At the same time, the flow of foreign capital was dictated not by the needs of the receiving countries, but by the interests of the international business interests in the capital exporting countries. Despite the initial importance of such capital, then, in the industrialization of the area, it created in the long-run a series of distortions and imbalances within the recipient economies, marked by advanced mining and primary processing industries and relatively backward manufacturing industries. Prof. Teichova's conclusions were of great interest, and it would be well worthwhile to compare the situation and effects she describes with other areas developing on the perimeter of the European industrial epicentre — Italy, for example, in the decades before the First War, despite the obvious differences, would provide an interesting field for comparison, both in terms of foreign investment and of the structure of its internal north-south dualism.

The Tawney Memorial lecture was this year delivered by the French scholar and economic historian, Francois Crouzet, and was also related to problems of economic growth, in particular the role of exports in Britain's economic growth at the turn of the XVIIIth century (*Export and British Economic Growth 1783-1815*). Professor Crouzet pointed out that the development of Britain's trade in the late XVIIIth century was a relatively neglected topic, and one on which there had hitherto been little statistical information. His paper, therefore, contained a number of new and extremely useful sets of data: mean rates of annual growth of exports 1783-1815; annual growth rates for principal exports in the same period; the contributions

of individual articles to the increase of total exports over the period; and the changing geographical destinations and distribution of British exports. From these he was able first to identify a dramatic upswing in the annual rate of growth of British exports which dated from 1781. Not only did this constitute a decisive break with the rather mediocre rates obtaining earlier in the century (7% for the decade 1781-92, and 3% for the years 1744-1760), but it was also sustained until 1802. Thereafter a decline in both the volume and values of British exports is evident, and this pattern was to continue after the end of the Napoleonic Wars. The twenty years between 1781 and 1802 then stand out as being quite exceptional, in which exports played a major role in economic growth, but also a period which was not at all typical of the secular development of the British economy from the early XVIIIth to the early XIXth century.

After identifying the export boom, Professor Crouzet turned to examine more closely its composition, and the main changes and shifts experienced by the principal commodities. The picture that emerged was one of export expansion on a fairly broad front in the first decade (1783-92), in which cotton played an important but not a predominant part; the second decade saw the supremacy of cotton and the decline of other traditional exports (1792-1802); thereafter there was a return to a broader distribution of exports (1802-14). By tracing the destinations of the principal British exports during the period, Professor Crouzet was also able to demonstrate the decisive part played by the markets of North America in the export boom of the 1780s and 1790s. Immediately after the conclusion of the War of Independence, the "Americanization" of British trade was quickly resumed, and in the two years after 1788 Britain's trade with her previous colonies increased four-fold. Between 1788 and 1797, the United States absorbed nearly 49% of all Britain's exports, while in the same period Britain's exports to Europe dwindled to practically nothing. There was, Professor Crouzet insisted, no invasion of Europe by British manufactures in these years. But by 1798, however, exports to North America had reached their peak, and until the renewed outbreak of war in 1802 there was a shift in the direction of British exports back to the Northern European markets, with cotton setting the pace. After the outbreak of war another shift back to the North American markets occurred, but following the Peninsula campaign a new pattern emerged. On one hand, exports quickly adapted to the accessibility of Latin American markets, in particular Brazil, and, on the other, became directed towards the Mediterranean and Southern Europe which within a short time replaced the markets of Northern Europe in importance. For the period as a whole, then, British exports were subject to a series of shifts both in direction and composition.

Although admitting the difficulties involved in computing of national income and of export values at current prices, Professor Crouzet claimed that

it was possible to establish some notion of the value of exports as a percentage of British national income. He agreed with other scholars that for most of the XVIIIth century the contribution of exports to national income was marginal but once again stressed the unusual and exceptional character of the two decades between 1780 and 1802. From a modest percentage of national income in the early years of the century (6-7%), the contribution of exports began to increase after 1780 (to 7-8%) and by 1800 reached as high as 18%. But also, the additional exports of the period 1783-1802 constituted some 53% of the additional national income produced in these years. By 1800 Great Britain was becoming essentially an export economy, but Professor Crouzet stressed that this situation was not typical and did not last much beyond that date. Due to the effects of the war and the steady decline in the prices of manufactured goods, exports became static and their rate of annual growth slowed down. As national income continued to progress, the contribution of exports in relative terms declined and while still maintaining an important position no longer constituted the leading edge of the national economy.

Professor Crouzet pointed out that the final part of his argument was the more conjectural, as he had not taken into account backard linkages, or multiplier effects, and had also treated exports in isolation from imports and from terms of trade. But this did not affect his main conclusions as to the truly exceptional nature of the period running from the end of the American War of Independence and 1802 in Britain's economic growth. It was a period in which both cotton exports and North American markets played a decisive role, in which exports constituted a real engine of economic growth. The paper then differed from others at the conference in that, rather than reporting on work-in-progress, it presented a series of polished results and conclusions, and at the same time clearly constitutes a very important contribution to our knowldge of a critical period in Britain's industrialization and of the development of British trade in a period which can now, as a result, be said to have emerged from its previous neglect.

The discussions surrounding the second topic of general interest, internal migration in Britain, produced rather less in the way of research findings than those on economic growth, which reflects the more recent origin of interest in this field. Two sessions were devoted to the theme, the first to Dr M. Anderson's paper on *The Origins of London's Population in the Mid-19th century* and the second to a discussion panel, chaired by Dr E.A. Whigley (of the Cambridge Group for the History of Population and Social Structure, which is a unit of the SSRC), on the subject of *The Measurement and Significance of Internal Migration.*

Dr Anderson's paper was a preliminary report of work-in-progress on a project for computer analysis of the 1851 census returns which he is directing at Edinburgh University. Computerized data sets have now been processed

for 400,000 individuals in London from the 1851 census returns, but the "manual" analysis of these sets had only just begun so that Dr Anderson's findings were based on sub-samples for 14 out of the 15 area-batches into which London had been divided. He pointed out that as the processing was very recent, his paper should be taken in the light of preliminary thoughts rather than proven assertions.

The object of the study was to trace where London's population in the mid-XIXth century came from, and Dr Anderson pointed out that one major problem was that the census returns necessarily provided a static picture. In general terms, there were a number of findings which modified earlier studies, in particular the discovery that about 40% of London's population in 1851 was born in London and the fact that the numbers migrating from different geographic areas were proportional to their distance from London. The details of town and parish of birth given in the census returns (but not in the published census) also mean that it is possible to trace the geographical origins of migration with precision and not simply at the level of counties, and the sub-samples of the data-set had been plotted geographically in order to illustrate the nature and patterns of migration into the 14 area-clusters in London. From this it was apparent that the widest geographical migration was directed towards the predominantly middle-class districts, and the occupational details in the census returns confirm that this was due mainly to the movement of domestic servants. Movement into the predominantly working-class districts tended to be geographically more limited. Comparison of the different areas revealed that there was no single flow of immigrants, but a number of different flows and a variety of socio-economic variants between different destinations. Rates of in-migration varied from district to district, but without any clear distinction between middle- and working-class areas. But the movement in and out of the 14 areas revealed a very high degree of internal mobility and circulation, and for every area in-migration by individuals born in London outweighed that of those born elsewhere.

Despite the concentration in the areas closest to London of the geographical origins of the migrants, over 26% of migrants into London had travelled over 50 km, and again the distance travelled tended to be furthest for those moving into the middle-class areas. In occupational terms, distance tended to conform to certain groupings — labourers (with the exception of the Irish) rarely moved from parishes of birth, while the professional classes and domestic servants travelled the longest distances.

The conclusions regarding the socio-economic conditions of the predominant areas of emigration were among the most interesting. Dr Anderson claimed that the preliminary soundings indicated that very few of London's immigrants came from predominantly agricultural areas. In the central areas of London there were very few migrants from rural villages or from major

urban areas. While the majority seem to have originated from areas with low population densities, there was no evidence that areas experiencing economic decline contributed any significant body of migrants. In fact the majority came from areas with growth rates close to the national average.

One of the problems is of course that the significance of so many of these findings can only be assessed comparatively, when patterns can be seen developing, modifying and disappearing over time. Even though the census returns do provide the cumulative evidence of movement over preceding years at a given moment, the picture, as Dr Anderson had pointed out, remains static. The existence of a high degree of internal mobility is clearly of great interest, but to understand its significance some idea of rapidity of circulation and directional patterns and changes is needed. The age patterns of immigrant groups from different geographical origins might give some indirect clues to this and another variant which would be worth exploring is that of the incidence of married couples and unmarried individuals in the different geographical and occupational groups.

Rather than to direct discussion of Dr Anderson's paper, the discussion session was devoted to problems of studying migratory movements over time. Dr Kitch described the value of London apprenticeship records and the city freedom registers for studying migration into London in the XVIth and XVIIth centuries. These sources could also be supplemented by various others, including deposition records in ecclesiastical courts, in the High Court of the Admiralty, printed marriage licences, depositions at the Inns of Court and the records of certain London hospitals. For the early XVIIIth century there was also the first series of Inland Revenue records. Discussing his own findings arising from the analysis of apprenticeship records, he estimated that in the XVIIth century there were some 6,000 apprentices alone moving into the city annually and that at the end of the century over half were born either in London or in the Home Counties. While from early on the percentage coming from urban backgrounds was small, during the course of the two centuries there was a marked decline in the number of London in-migrants coming from the northern counties and from the West Midlands.

In a short but interesting paper D.E. Baines discussed the problems of measuring population flows over time in the XIXth century. He described the difficulties arising from the fact that age specific mortality was greatly influenced by the circumstances of the areas into which migrants moved. From his own research he suggested that in the counties internal and external migration were complementary. Emigration from rural areas in the XIXth century tended to be constant, and the peaks in emigration were produced by urban areas. Finally he suggested that it was mistaken to think of the pattern of emigration in Wales as being different from England, and in fact the two were identical in timing.

There was no opportunity for comparing the points arising from these two trend analyses with Dr Anderson's findings, although the more static nature of the latter would probably have made it difficult to do more than point to similarities and differences. In introducing the final speaker, Dr P. Spufford, as a historian interested in migration rather than a specialist in migratory movements, Dr Wrigley pointed out that one of the factors which had emerged from the previous discussion was the greater relevance of socio-economic circumstances than demographic factors such as alterations in birth or death rates for explaining changes in migratory phenomena, and that it might therefore be useful to look at the problems from a more historical perspective.

Dr Spufford started by suggesting that there were certain artificialities inherent in the typology of emigration adopted for the pre-industrial period. Referring to the distinction drawn by Peter Clark, in his recent book on Kentish migration, between "betterment" and "subsistence" migration, Dr Spufford argued that this risked overlooking the enormous importance of what he termed "normal" migration. In fact, he claimed, in pre-industrial England mobility, not stability of residence, was the norm, and that this could not be related automatically to social or occupational mobility — most migration did not involve any movement upwards or downwards in those terms. He suggested that a group such as Dr Kitch's apprentices were by definition exceptional, or "exotic", and did not necessarily reveal accurately the nature of the more general movement. Drawing on a variety of sources, he argued that in the XVIIth century short-distance migration amongst agricultural workers was the norm, and in rural Sussex, for example, two-thirds of the population lived outside the parishes in which they were born. He also cited the disappearance of surnames as evidence of very rapid turn-over, and claimed that in the XVIth and XVIIth centuries four-fifths of all surnames in a single village disappeared over a hundred-year period. Although he admitted that there were many exceptions to this general mobility, he insisted that these did not conform to regional patterns but were related to the existence of particular customs and traditions at a more restricted local level. This led him to doubt whether mobility could usefully be mapped at a regional level. As far as trends over time were concerned, Dr Spufford suggested that it is possible to detect a speeding up in normal mobility from the XVIth century onward, accompanying the breakdown of the husbandman farm-holdings, and the pace only declined with the advent of agricultural labourers' cottages.

There was unfortunately virtually no time to take up any of the questions and queries which Dr Spufford had raised, although various doubts on the validity of inferring too much from the disappearance of surnames or of identifying physical and economic mobility were raised. Although a number of interesting points had been raised in the course of the two sessions,

no coherent focus or dialogue emerged. Because the different papers were concerned not only with different issues in different dimensions, but also with more discreet problems of technique, the nature of the more general framework from which they derived remained blurred and at times even obscured. Partly this results from the nature of the phenomena and the multiplicity of aspects which they present — the causes of migration in general and in particular, the nature and composition of migratory movements, the consequences in a particular economic and social situation — and also from the number of disciplines on which are drawn — history, economics, demography, sociology etc. But this makes a rigid delimitation of objectives and a precise formulation of problems all the more important. Nor can this be done simply at the level of techniques. In introducing the session, Dr Wrigley had claimed that the work at present being undertaken on internal migration was one of the most concrete examples of that combination of history and sociology which is so often discussed but so rarely attempted. To the extent that this is true it is admirable, but it is important that in attempting the amalgamation of the two approaches the historian does not fall into the tendency from which Anglo-Saxon sociology has only with some difficulty emerged — that of concentrating on the techniques of social measurement to the exclusion of conceptualization. Despite the title adopted, we did hear much more about the problems of measurement — and on this score there is no doubt that valid points were raised — but very much less on significance.

The demographic historians were not alone in stressing the relevance of other branches of the social sciences to economic history, and it was Professor R. Fogel of Harvard University (who this year held the Pitt Professorship at Cambridge) who directed the conference's attention to the links between economic history and anthropology. In his paper *Cliometrics and Culture-some recent developments in the historiography of slavery* Professor Fogel argued that it was incorrect to think of the quantitative approach to economic history, which he has played such a major part in developing, as being concerned solely with the measurement of economic and social phenomena. It had, he claimed, not only an important but even a decisive contribution to make in the field of the history of social institutions and culture. He concluded his discussion by referring to Franz Boas, insisting that: "Culture does not exist in a vacuum. It is defined by behaviour, artifacts, institutions and beliefs of a society . . . Much of what we know about the beliefs of a society is inferential, and turns on the study of the frequency of certain types of practices and artifacts". The task of the quantitative historian was, Professor Fogel implied, to cut through the clouds of vague, subjective, inference and to establish both quantitatively and precisely the distribution, frequency, and location of the cultural and social characteristics which form the stock-in-trade generalizations in the debates over the "quality of life".

The particular cultural controversies with which Professor Fogel's paper was concerned were drawn from the current historiography of American ante-bellum slavery, and the two examples on which he concentrated were the psychological motivations of slave behaviour and the development of the institution of the family on the plantations. He took first the general problem of the ways in which the Negro slaves adapted to the circumstances of the plantation economy, and in particular the question of the extent to which the slaves voluntarily cooperated with the organization imposed by their white masters, and the consequences this had for the efficiency of the plantation economy. He traced the development of the debate from the early notion of the inherent, and even "biological", inefficiency of the Negro slave, through to the revisionist thesis that the slaves resisted their masters through a strategy of conscious "day to day" resistance, which was first advanced by M. J. Herskovits and by Raymond and Alice Bauer, and subsequently developed, although in a different form, by Kenneth Stampp (*The Peculiar Institution* 1956) and by Eugene Genovesi (*Roll, Jordan, Roll: the World the Slaves Made 1974*). Professor Fogel argued that the notion that the slaves expressed their revolt against the indignities and crushing toil of their situation through a persistent, but subterranean, guerilla campaign of petty sabotage and passive resistance, gave rise to many problems. In particular, he returned to the argument that quantitative analysis had demonstrated the much higher efficiency of plantations worked by slave gangs, compared with those run by free farmers, claiming that the former were 70% more productive. Recognition of this fact, Professor Fogel claimed, had made the original "day to day" resistance thesis untenable, and so provided a first example of the contribution of quantitative method to qualitative debates. Although he drew on other quantitative evidence to counter certain aspects of the argument in Eugene Genovesi's recent study *Roll, Jordan, Roll* (in particular he contested the claim that the slaves successfully resisted the imposition of regular and unvarying work patterns), Professor Fogel concurred in general with the claim that the slaves' work ethic was more subtle and variegated than earlier historians had allowed. Rather than an out and out attack on the system, the slave response was essentially reformist rather than revolutionary, and was based on a strategy of cooperation to win concessions, and even a degree of security, within the limitations of their situation. He argued that the system created incentives for the slave to show himself to be a good worker, while the acquisition of craft skills provided the best guarantee — albeit still a partial one — of security. He argued that this willingness to cooperate in order to improve their living conditions conformed with the notion of the economic efficiency of the plantations which the cliometicians had demonstrated, and also that it resulted in positive gains for the slaves themselves (e.g. in the acquisition of skills despite the overt hostility of the slaveocracy). He also dismissed as armchair

fantasies the suggestions that a more revolutionary strategy might have been adopted successfully (such a strategy was frequently, if fragmentarily, adopted unsuccessfully), and pointed to the absence of any of the objective conditions which Lenin was later to identify as prerequisites for successful revolution, noting in particular the complicity of Northern ruling classes in maintaining the cotton economy and staving off the eventual crisis.

The second debate which Professor Fogel chose to illustrate further the contribution of quantitative studies was the more complex issue of the nature of the family structure and the sexual mores of the Negro slaves. He traced the development of historical attitudes from the overtly racist belief in the inherent sexual promiscuity of the Negro female, to the first serious attempts to study the nature and structure of the slave family and the ways in which it was influenced by the plantation system. The problems are numerous: what norms, if any, characterized the family in the slave community, where did those norms come from, how were they affected by the plantation system, with what degree of stability did they emerge from the plantation era? The initial parameters of the debate were established in the works of early black scholars such as W. Dubois and Franklin Frazier, who had respectively argued that a clear distinction emerged between the family structures of house servants (nuclear monogamic family) and the more vulnerable field hands ("no family life, no meals, no marriage, no decency, only an endless round of toil and wild debauch at Christmastide"). This dual structure was a direct product of the plantation system, and created dual norms; on one hand a male-headed nuclear family with accompanying monogamic sex mores, and on the other the femaleheaded family resulting from the chaotic and insecure circumstances of the fieldhands, and in which illegitimacy rates were high.

More recently, in the work of H. Gutman, J. Blassingame and E. Genovesi, the debate has shifted, and the need to approach family norms and sexual mores from the standpoint of the slaves themselves, rather than treat them simply as variations on patterns set by the slave owners, has been stressed. The tendency has been to emphasise the stability of the slave family, and in the case of Genovesi to suggest a distinction not between different family norms, but between differing concepts of sexual morality amongst the married and the unmarried, which were accepted by the slaves and not seen as incompatible. So it is a debate in which frontal contradictions abound — was there a dual family structure or a "nuclear family norm"? Are the charges of sexual promiscuity justified, or did the slaves' sexual mores conform to norms established and clearly defined by the slaves themselves? Did the plantation system permit the surival of the nuclear family or destroy it? Professor Fogel argued that the contradictions could only be resolved by quantifying the frequency of the various attributes and characteristics which were contested. He referred to the analysis currently

being undertaken by S. Crawford on 1400 slave plantations, the preliminary results of which indicate that some 66% of slave households were presided over by both parents, while some 30% were motherheaded. This would seem to suggest that although the stable nuclear family held an important place, it did not predominate. In the majority of mother-headed families the absence of a male was due to the working of the slave trade. Quantitative analysis, Professor Fogel argued, then supports the earlier thesis of Frazier and Dubois rather than that of Genovesi and Gutman.

He also claimed that quantitative analysis could make an equally important contribution to the debate on the slaves' sexual mores. Referring to the preliminary findings arising from R.H. Steckel's current quantitative analysis of the causes of the very high fertlity rates shared by both whites and black slaves in the ante bellum South,[4] Professor Fogel argued that these indicated a significant percentage of non-childbearing slave women. The percentage was well above the maximum sterility rate, and suggested either abstention or use of contraception — and while there is no evidence for the latter there are strong indirect indications that it was not practised. Also, in the case of those who did bear children, there is evidence of a gap of over two years between the beginning of the period of fertility and the birth of the first child. The quantitive studies that have been carried out, Professor Fogel then claimed, point to « the "non-utilization" of roughly one quarter of childbearing capacity », and would imply that the sexual mores of slave women were on the whole "prudish" rather than "promiscuous".

Although Professor Fogel had used these examples to illustrate the ways in which quantitative economic history could be advanced into new fields and make decisive contributions, it was difficult to escape the conclusion that it was beating something of a retreat, and also seemed less "new" than expected. It was not entirely clear how the statistics of those described as cliometricians, in this context, differed from those who were not. The difference was presumably not that the former were exclusively concerned with quantitative issues alone, hardly a plausible proposition in the context of debates about behaviour, culture and institutions. It would almost seem that quantitative history was being set in an essentially subservient role, that of measuring the frequency of phenomena which the cliometrician acquired ready-defined, but which he had no part in creating. The absence of any discussion of a distinctive methodology made it the more difficult to distinguish between the "new" and the "old", and did not really serve to dispel the reservations which have been stated by H. Gutman, in particular (H. Gutman, *Slavery and the Numbers Game* 1975), about the application of quantitative methods in this field.

[4] As in the case of S. Crawford's research, Professor Fogel stressed that these results were preliminary and may be subject to change in their published form.

Professor Fogel's paper was then extremely thought provoking but, like certain of the other discussions at the conference, suggested that if the search for a new positivism in numbers will keep economic historians employed for many generations, there are few indications that it will lead to any consensus either over the figures or over their interpretation. The process of juxtaposing quantitative evidence against purely subjective inferences is clearly a move towards greater precision of data, but is necessarily subject to the problems of selection and interpretation common to all historical research. But one can also ask how novel is the concept of introducing a quantitative arbiter into a controversy over quality of life — and without forcing a comparison, it is worth bearing in mind that the interminable debate over the English « standard of living » in the early XIXth century finally sought refuge from the quagmire of statistics and counter-statistics by moving from the belly to the mind.

I have left to the end of this note a discussion of the paper given by Dr A. Hopkins (*Africa in the Economic Historican's World*) not because its subject matter set it apart from the rest of the conference, but because it was the only paper to deal with the problems of organizing and defining economic history at a general level, and because it posed with considerable clarity and elegance problems and queries which were relevant in one form or another to all the other discussions. Rather than simply describe the directions in which the study of Africa's economic history has developed over the last twenty years and list research projects, Dr Hopkins was concerned to describe and define the ways in which work on African history related to the framework of assumptions which were part of the discipline of economic history as a whole, and to a more critical examination of the problems which such assumptions posed. Taking advantage of the perspective from what he described as a "sub-specialism" — a description which is justified by the numbers involved rather than by the range and nature of the problems posed — Dr Hopkins succeeded in raising critical queries about problems which are too often, and too easily, taken for granted.

He began by distinguishing between what he saw to be "two concentric circles which together form the disciplinary matrix, that is what we do as economic historians". African economic history could be seen as an inner circle concerned with specialised research and problem solving, but existing within the outer circle formed by the assumptions and preoccupations shared by economic historians as a whole at a given moment — assumptions and preoccupations which changed and shifted with developments in the world at large. It could be argued that the analogy was more tenable in heuristic than in practical terms, but Dr Hopkins was surely right in stressing the need for those working in the inner circle to maintain a precise awareness of the relationship between their own specialised activities and the broader

and changing parameters of the outer circle, which in turn requires a precise definition of those parameters. In the case of African economic history, Dr Hopkins argued, there were two themes which linked the inner and outer circles — first the liberal inspired "modernization" theories of the early 50s onwards, and secondly the more recent theories of under-development.

In following the problem-posing criteria deriving from both these themes, Dr Hopkins suggested that the work on African economic history had made a number of positive critical contributions which had served to modify many of the assumptions contained within the original matrices. In spite of its romanticising and often self-flattering assumptions, it was "modernization" theory which had first created an interest in Africa's economic past. But once set in motion, those working on the problems quickly found the need to criticize the adequacy of accepted notions about Africa's "traditional economy" and to explore more deeply the historical reality. In the first place this led to one of the most remarkable exercise in interdisciplinary endeavour ever undertaken in the field of historical studies: the creation of evidence about pre-literate societies where none was thought to exist. Once the material began to be amassed, and there proved to be no shortage, the result was a rapid erosion of the mythologies about the African past which economic historians had inherited. Evidence of the variety and sophistication of pre-colonial economic systems, the extent of market orientated production, and even the degree of integration in an international commercial economy, together with indications of considerable rationality of economic organization in relation to resources (especially the abundance of land and the shortage of labour, which even before the colonial period had encouraged the use of slave labour) all led to a radical revision of accepted notions. Dr Hopkins claimed that the resulting studies had clearly demonstrated the ability of the indigenous African economies to adapt to, and integrate themselves with, the demands of international commerce. Where they were given freedom to cooperate by subsequent colonial regimes, this process continued efficiently and effectively — although the colonial administrators rarely had the perspicacity to appreciate this, and preferred instead to perpetuate the myth of the idle kaffir. (The comparative efficiency of the collaborative over the coercive regimes suggested here could also be related to Professor Fogel's discussion of collaboration and resistance on the slave plantations).

This more precise knowledge of the economic past of Africa in the pre-colonial period clearly demanded a major revision of the assumptions implicit in both modernization theory and imperial historiography. These findings were also being published at the time when interest in the outer circle was shifting towards the problems of under-development, in turn a reflection of the growing pessimism of the late '60s and of the failure of "modernization" theories to create any positive results in practical terms.

Dr Hopkins argued that, as far as Africa was concerned, it was still too early to assess how valuable this new orientation has been. Although he welcomed the greater awareness of the divergence betwen theory and historical realities, he also felt that there was a tendency to substitute ideology for theory which risked rehearsing in a different form many of the errors peviously committed in the name of modernization theory.

Attempting then to assess the results of these shifts in problem posing and emphasis, Dr Hopkins suggested that it was necessary to concentrate on the testability of the propositions arising from the different philosophical starting-points, rather than on the philosophies themselves, which were by nature untestable. He also argued that the two themes should not be seen as comprehensive and exclusive paradigms, in the terminology of Kuhn, but rather as separate research programmes which could be drawn on freely and eclectically. As far as the progress achieved in African economic history was concerned, Dr Hoping pointed out that there was now an increasing number of theories and assumptions about the nature and development of African economies which could be held only in the face of the evidence resulting from modern research.

In conclusion, Dr Hopkins suggested that there were in particular three issues on which the current economic history of Africa had made a positive contribution to the discipline as a whole. The first of these, he argued, was the awareness of the necessity for reinstating the study of beliefs in economic history. If the study of economic systems which bore little initial resemblance to classical Western models poses such problems in a more obvious way, the importance of reconstructing the intentions and objectives of historical actors in a given context is no less relevant in other and more familiar fields. In saying this, Dr Hopkins put his finger on a very important issue, and one might suggest that the relative neglect of beliefs, or historical values, in contemporary economic history is due to the inadequacies of the essentially ahistorical and value-laden interpretations of entrepreneurship derived from Schumpeter and still widely used. His second point was that the experience of the economic historians of Africa had indicated a wholly artificial tendency in current institutional and exclusively quantitative economic history to neglect problems of power and politics and their links with given economic systems. Finally, he pointed out that the work which had been done on indigenous economic systems revealed enormous gaps in our knowledge of the ways in which imperial economic systems functioned. The study of colonial economic policies, of transport systems, and of expatriate companies, provide examples of the research agenda which remains to be tackled.

The first two of these points are closely related, and when discussing the problems of beliefs, Dr Hopkins referred to the contribution to be made by anthropology, social psychology and philosophy. It is also worth

mentioning the relevance of Weberian sociology to the study of both beliefs and the structure of political power. In particular, the importance of the study of beliefs lies in the fact that it provides very valuable and illuminating comparative criteria against which to view widely differing situations and problems of economic development, without at the same time over-riding essential peculiarities or imposing abstract generalizations. And, as Weber again would have argued, it is precisely because of the part played by specific beliefs, intentions, objectives and goals in both economic and social activity, that all aspects of historical experience become inter-related.

But the general issue which Dr Hopkins' paper succeeded in posing was that a specialism must not be allowed to become a refuge, as too frequently occurs, but should ideally become a basis for contributing positively and critically to the more precise formulation of those assumptions which are common, to at least a significantly broad section if not to all economic historians. This involves not only attempting to relate the particular to the general and the maintenance of a continuous reciprocal exchange between the two. It also demands a critical, but open-minded, approach to the problems of conflicting ideologies and philosophies, and an attempt to assess from the perspective of the specialism their relevance to particular problems of puzzle-solving. Certainly these are not problems that are either new or solved, but they are ones that need to be confronted more explicitly in day to day research rather than left to a separate specialism of generalized and theoretical debate. To Dr Hopkins went the merit of having demonstrated that the two exercises could be conducted simultaneously without one necessarily swamping the other. He gave the conference much to think about and much to debate — not excluding the ways in which he himself posed many of the problems — and it was unfortunate that there was little opportunity to refer back to these issues in the subsequent discussions. While discussion of generalities is not always practical or even desirable, a greater awareness of the need to bring the two forms of critical thinking together might well have helped to focus debate in certain of the other sessions of the conference.