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## CONFERENCE REPORTS

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### *Cliometrics at Warwick (GB)*

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An organizing committee consisting of A.G. Ford (University of Warwick), N.F.R. Crafts (University College, Oxford), P.K. O'Brien (St Antony's College, Oxford), and G.N. von Tunzelmann (St. John's College, Cambridge), approached the British S.S.R.C. in 1977 to fund a Conference devoted to Cliometrics. The funding was granted and the Conference organized for 4-5 January, 1978. The primary objective was to bring together younger lecturers and research students who were adopting a more theoretical or statistical approach to economic history, but in many cases working in near-isolation in British universities. Most of those attending were indeed from the younger age groups, and collectively gave every indication that -- with certain adaptations recounted later in this Report -- cliometrics is gaining ground on this side of the Atlantic. There was also a sprinkling of senior economic historians (like P. Mathias and R.M. Hartwell) and of visiting academics from abroad (like P.A. David), who helped to round out the discussion. The papers were circulated in advance to an invited audience, and this procedure ensured lively and critical discussion. A resume of the eight papers and the discussion that resulted in each instance follows.

The opening paper was delivered by *J. Foreman-Peck* of Thames Polytechnic, on "Tariff Protection and Economies of Scale: the British Motor Industry Before 1939".

To establish the impact of a tariff, the author constructed a partial-equilibrium model of the British motor car industry. Supply curves were estimated alternatively by way of Cobb-Douglas production functions or by using cost functions; either way obtaining evidence of economies of scale in the industry (a 1 per cent increase in inputs being associated with a  $1.4\frac{1}{2}$  increase in output).

That significant economies of scale existed raised the possibility that tariff protection could have lowered costs and prices of British motor cars. To gauge by how much, Foreman-Peck estimated demand curves (the model was estimated by two-stage least squares.) Though generally successful, these failed to yield significant estimates for the elasticity of demand for imported cars, which crucial variable was therefore estimated using monthly data for 1924/5 when first the Labour government withdrew the tariff, then subsequently the new Conservative government replaced it. Using an elasticity of substitution of 7, Foreman-Peck found extremely high demand elasticities, with the result that he was able to show that the benefits of protection outweighed any losses to the consumer in the interwar period. Equally, before 1914 when trade was free, an equivalent tariff ( $33\frac{1}{3}$  per cent) could have done much to remedy the early sluggishness of the nascent British industry.

The author himself noted three possible objections to his model: (i) the specification of the demand equation; (ii) the requirement that imported cars were imperfect substitutes for domestically-produced ones (otherwise consumers could not benefit from a tariff); (iii) the finding of economies of scale. Discussion centred around the second and third of these. Foreman-Peck's value of 7 for the elasticity of substitution between imported and domestic cars was thought by some of the audience sufficiently close to infinity to reject the idea of imperfect substitutes. If on other grounds one was to believe in a lower degree of substitutability, then specification used here for estimating it was presumably invalid. Others objected to the data employed in its estimation, claiming that 1924/5 was a poor basis in view of the rapid exchange-rate changes (the floating franc, etc.). It was noted that the  $33\frac{1}{3}$  per cent tariff rate was misleading as applied to American cars, since they could make use of the lower Canadian rate or not as it suited them.

The economies of scale result was still more discussed. The general historical support, e.g. from increasing business concentration, was questioned — one scholar had recently found concentration in the motor car industry to be falling in the 1930s. More generally, it was felt that more careful distinctions could be drawn between concentration and scale, and between firm-level and industry-level scale. Secondly, there were doubts about the data employed. It was pointed out that the employment, capital, and average-value series were from widely differing and possibly contradictory sources. Foreman-Peck treated cars as homogeneous, whereas average car sizes were falling substantially in his period — the meaning of economies of scale in such circumstances was queried. Finally, some critics doubted the robustness of the Cobb-Douglas production functions, and suggested alternative adjustments to the data for estimation.

The second paper, by D.G. Greasley of the University of Edinburgh, also adopted two-stage estimating procedures in examining "The Diffusion of Machine Coal Cutting Technology in Great Britain, 1902-1938."

Greasley demonstrated that several authors had supposed a slow rate of adoption of machinery for coal cutting to be indicative of entrepreneurial sluggishness in the coal industry, but none had given a valid measure whereby the rate of innovation and diffusion could be assessed. Was it really so slow? In his model Greasley determined the optimal flow of the new machine technology by a threshold ratio representing the cost of services from the new to those of the old. The former were given solely by capital costs (machine prices, interest and depreciation rates, time required to produce a given level of services) and the latter — of the old technology — solely by labour costs (taken as coal hewers' wage rates.) On the basis of several estimating techniques (ordinary least squares, autoregressive methods, and instrumental variables with autoregression) Greasley found (i) that entrepreneurs adjusted quickly and rationally in their choice of technique, and showed little sign of being encumbered by past techniques; and (ii) that relative machine to labour prices were not significant in explaining diffusion, but the quality of the new technology (as reflected in the time taken to produce a given quantum of services) was.

Most criticisms were directed at the specification of the equations and the nature of the variables employed. It was noted that a high proportion of coal was machine-cut by the end of the period in question (1938), so that coal output was simultaneously acting as independent variable and (very nearly) as dependent variable by this stage — it was thus suggested that this lack of independence may have helped account for the major and surprising finding of a very fast entrepreneurial response. The efficiency variable (time required to produce a given output) was also correlated with the coal output variable on the right-hand sides of the equations, making for multicollinearity. The machine-price variable was difficult to interpret in view of the poor second-hand market for machines (the author agreed with this point.) The double source of response in the threshold variable (i.e. both relative costs and efficiency) made the conclusions ambiguous because of the heterogeneity of response possible. And the use of capital costs on one side against labour costs on the other was debated — how could labour used in machine methods be brought into consideration? It was argued that the results (the exceedingly fast response by entrepreneurs) undermined the dynamics of the model hypothesised and in turn made the conclusions spurious.

On other points, it was noted that in some mining districts the *a priori* restrictions on the coefficients were not satisfied by the estimates produced, so that regional variation needed further investigation. In this respect, it was contended that coal not homogeneous in quality across regions and that its marketing problems accordingly varied from region to region. Others asked for machine data at the individual pit level. Some pointed out that there were interdependencies with other innovations not noted in the paper, e.g. in introducing electricity the pumping and winding gear came first, or the linkages

between coal-cutting machinery and underground locomotive haulage. Finally it was noted that output of coal was falling, and clarification of the implications for investment in diffusing such an innovation in those circumstances was requested.

The next two papers were devoted to economic aspects of medieval English agriculture. The first, by *A. Ingham* of the University of Southampton, was entitled "On the Economics of Open Fields and Enclosures", and represented the views of an economist who happened to have a side-interest in local history. His study of the latter had led him to conclusions which he subsequently found to be contradicted by the argument of J.S. Cohen & M.L. Weitzman ("A Mathematical Model of Enclosures", 1974.) Cohen & Weitzman had argued that in the medieval and post-medieval periods the communal form of organization that dominated agriculture represented an inefficiency in production, because instead of *marginal* products being equalized across villages, *average* products were equalized. Thus the enclosure movement from about the seventeenth to the mid-nineteenth century removed this source of inefficiency, allowing private profit maximization at the point where marginal products were equalized. Production moved from a point inside the production possibility frontier to the frontier itself. Ingham raised several specific objections to the Cohen-Weitzman model, showing for example that it depended crucially upon the assumption that returns to scale were everywhere decreasing. The biggest problem with their interpretation, in his eyes, was that it did little explain why the communal system should have lasted so long (from about the tenth to the eighteenth century.) Ingham therefore set out to reformulate the model so as to allow of more general and realistic technical assumptions. With a variety of simplifications made for computational ease he demonstrated important non-convexities in the production possibilities, contrary to Cohen & Weitzman. Increasing returns could arise in the communally-organized village from concentrating cattle production, from overcoming high set-up costs in arable cultivation, and from lowering travel to work.

Having established patterns of optimal allocation in the open-field village, the author next sought for circumstances that would permit the required degree of co-operation to be maintained. With convex production sets under specified conditions no coalition could block a competitive equilibrium, but with non-convexities as here coalitions (like the communal organization of the village) could block individual activity and attain greater efficiency. Thus the bye-laws and regulations of the village allowed joint benefit from the communal system. With the coming of enclosure such rights and obligations were adjudicated on (e.g. by the parliamentary commissioners with parliamentary Acts of Enclosure) and converted into individual allocations of contiguous land. In this view property rights internalize externalities, and the ownership pattern of property rights is the *consequence* of changes in technology, preferences, and market organization. Ingham made only casual reference to the detailed histo-

rical literature (with which he modestly claimed only sporadic acquaintance) but showed that for the area of the East Riding of Yorkshire of which he did have knowledge the model fitted. Both technological change and changes in demand (e.g. rising demand for meat), and perhaps commercialization of agriculture, led to a reduction in the optimal scale of production which made individual farming desirable for those individuals who were able to force enclosure. Harmful effects arose as those classes who had not possessed formal rights before enclosure became purely landless labourers.

Most of the discussion aimed not at extending the historical application but at fitting Ingham's model into the prevailing literature in economic history. Did he want to explain Parliamentary enclosure or enclosure by agreements? Cohen & Weitzman did not deal so much with Parliamentary enclosure as with the movement to private property rights because their basic interest lay in the process of "primitive accumulation". Parliamentary Act changes the rules and removes the need for unanimity of action. One member of the audience drew attention to D.N. McCloskey's work on open fields, in which it was argued that they were not primarily intended for collective benefit (via economies of scale etc.) but were an insurance device for spreading risk among members of the village community; with the consequence that improvements in transportation, insurance, and so forth through time brought about the shift to enclosure. However others of the audience felt that McCloskey's work was addressed to a different problem from Ingham's, namely the scattering of strips. It was suggested that village compulsion using byelaws for organizing production might be arrived at independently of (e.g. after) the imposition of the risk-spreading system. Some empirical comments rounded out the discussion. It was noted that in the Midlands the communal system broke down before the strip system of cultivation, bearing out the earlier theoretical point. It was felt that the author had over-emphasized owner-occupiers whereas landowners were always predominant in most areas. Finally, one critic thought that the economies of scale so crucial to the argument had not existed during the medieval period but rather were attained by the expansion of demand during the enclosure era itself.

The paper of *M. Desai* of the London School of Economics, "A Malthusian Crisis in Medieval England: A Critique of the Postan-Titow Hypothesis", dealt with a shorter period (XIIIth and early XIVth Century England) but also, as its sub-title suggests, was designated mainly as an alternative formulation to existing hypotheses. The Postan view, allegedly supported by data from the estates of the bishops of Winchester, was that the second decade of the fourteenth century witnessed a Malthusian crisis of over-population, with declining yields and rising rates of mortality. Another view put the crisis somewhat earlier (beginning in the 1290s) and put the blame on the burden of war taxation. A quite contradictory optimistic view referred to an expanding economy with growing exports. Desai did not advance evidence on the last

of these (though he showed that wool exports were declining); on the second he adverted to the size of the tax burden in relation to existing agricultural activities. His argument was however principally directed to the first: the Postan model. Desai began with some formal criticisms of the Ricardian-Malthusian model put forward by Postan in his claim that yields were falling (mainly through soil exhaustion.) Like Ingham, Desai doubted the assumption of returns diminishing everywhere, but his most important criticism was of the one-good (corn) nature of the Ricardian model, such that relative prices and relative profitabilities in differing activities did not come into the picture. Hence the rising price of wheat relative to wool might simply reflect buoyant market conditions favouring grain, rather than the pressure of population on subsistence. In the same vein, the evidence of rising feudal rents collected by Postan might indicate not "land hunger" but an expanding market for grain either elsewhere in agriculture or in the towns. Even if declining profitability were discovered, it might be reflecting a diminution of manuring as mixed farming declined, instead of a rising man: land ration. Finally, a declining productivity of *some* land is no necessary indication of the declining productivity of *marginal* land (Postan seemed to have covered this point). Desai accepted that there were movements in the relative prices of wheat to oxen or wool between 1208 and 1325, and in particular that wheat prices rose relatively between 1300 and 1325 as befitting the Postan argument, but contended that factors such as taxation may have discouraged livestock relative to arable cultivation. In taking Titow's data on productivity (gross yield per seed) for 1211/1349 he found that Titow's use of arithmetical averages over long stretches of time misrepresented things. Linear and quadratic regressions gave different results, and made the acceptance of declining productivity much more difficult. In the linear case for wheat, of the 34 manors tested for, 20 showed no significant trend whatever, 4 a declining yield and 9 a rising yield. In the quadratic case 24 showed no trend, 9 went up at first more quickly than later on, and one did the opposite. Since most coefficients of determination were very low it would seem likely that to a contemporary the fluctuations would look random. For barley there were a greater number of downward trends, but again the fluctuations dominated the trends. Finally, Desai queried the use of data on heriots (a kind of estate duty) as indicating rising mortality, because if these were fixed in terms of animals, a rising relative price of grain would induce more settlements in money than animals. The rising proportion of money to total heriots again might indicate a growing degree of monetization and prosperity. Equally higher heriots might imply a more prosperous group of smallholders rather than their greater mortality.

In the discussion the question of an appropriate index for profitability arose. Titow gave both yield per seed and yield per acre though preferring the former, as did Desai, on technical grounds. What about output per acre, or still more output per man? If for example real wages were declining it would

be rational to put in more labour and in that way extract higher productivity in Titow-Desai terms, but not in terms of labour productivity. Equally one might want to standardize for manure etc. The relative price information needed further attention in this light. It was asked how Desai was attempting to explain the general price movements he had illustrated — in monetarist fashion (indicating greater prosperity)? Some of the audience felt that Desai had not given sufficient empirical attention to Postan's point about the external as well as the internal margin of cultivation. The relative price information in isolation could not establish Desai's implied results here. What about the evidence for Europe as a whole which Postan had also presented? Could direct evidence be sought on population growth, instead of the heriot data? Lastly the econometrics were probed. Desai's finding of the increase in wheat yields generally slowing down in the quadratic regressions might suggest falling productivity later on, as Postan would argue — were the maxima computed? (No). Were the coefficients tested for equality over the whole period, which covered much more than Postan's supposed crisis? Desai claimed that more sophisticated methods like spectral analysis could not be used because the sets of observations were discontinuous. Others felt that econometric methods alone could not finally sort out the various models

The following two papers jumped forward to the years of the late nineteenth and early twentieth centuries. *T.J. Hatton* of the University of Warwick spoke first on "Excess Demand in the Labour Market, 1861-1939".

He put forward for assessment the hypothesis favoured by many interwar economists that the First World War had seen a structural break in the relationship between excess demand in the labour market and the rate of change of money wages. Existing econometric work partly contradicted this hypothesis; thus *A.W. Phillips* in his well-known study of the "Phillips Curve" found no substantial change in the relationship of unemployment to changes in money wages, though some subsequent writers had done so, e.g. by including cost-of-living indices or through examining regional or industrial dispersion. Hatton however wanted to query the more fundamental point of whether the labour market could be gauged solely by reference to the unemployment rate.

(i) Were the unemployment data adequate? Hatton considered that both the revised Board of Trade data for 1861/1913 and the 1976 Feinstein figures for 1921/38 slightly underestimated "real" unemployment, the latter because it was based on extrapolations from the census year 1931 when the recording of unemployment was atypical. The labour supply data from Feinstein were also inadequate as a measure of year-to-year changes in labour participation.

(ii) Was the relationship between unemployment and job vacancies stable? Hatton was particularly worried about the internal situation within the firm which — or so many economists would argue — might not adjust to the "true" supply and demand situations in the labour market, e.g. the firm might hoard labour as demand was falling so that recorded figures for unemployment would

understate the real deficiencies in work supplied. He therefore wanted to estimate "desired" levels of employment, which he would do by estimating a constant-elasticity-of-substitution production function and allowing dynamic adjustment of the common type  $\left(\frac{E_t}{E_{t-1}}\right) = \left(\frac{E_t^*}{E_{t-1}}\right)^\lambda$  where  $E$  was percentage employment ( $E_t$  in the current period and  $E_{t-1}$  in the preceding period) and firms changed level of employment according to some power  $\lambda$  (less than unity) of the ratio of "desired" employment  $E_t^*$  and lagged employment  $E_{t-1}$ . Like Greasley Hatton used a Cochrane-Orcutt iterative method to obtain his estimates. Among Hatton's findings of general interest were: (i) support for earlier writers that the period 1925/31 while Britain was on the Gold Standard saw an abnormally low increase in wages, thus refuting R.S. Sayers's famous 1957 statement that "the Gold Standard was essentially an employment policy". Whether this was because the Gold Standard acted as a kind of incomes policy or because it reduced uncertainty and therefore allowed smaller margins of business tolerance remained to be discovered. (ii) His production function estimates indicated that the elasticity of substitution between capital and labour was low (between zero and 0.3), so that real wages would have to have been cut several-fold to secure any given increase in employment, contrary to the interwar "Treasury View". (iii) There were probably economies of scale, so that budget deficits would have had enlarged effects on output. On the main point, Hatton's estimated coefficients differed between 1861/1912 and 1921/38, but on formal tests not significantly so; thus the hypothesis of a structural break would have to be rejected, both for employment adjustment and for wage changes. However when a variable representing the cost of living was included a structural break could be accepted in that respect — this supported R.G. Lipsey's view that the important change was in the way wage bargaining after World War One took changes in the cost of living into account. Neither "predicted" nor "desired" employment were satisfactorily significant in the two-stage estimating procedure, indicating that "internal" excess demand in the firm did not help the explanation. Hatton suggested tentatively that industrial or regional disparities in unemployment rather than aggregate unemployment might give a better explanation.

As in earlier discussions the model was closely scrutinized. Why was the "desired" labour supply equal to the actual labour force (especially in view of Hatton's expressed reservations about the measurement of the labour supply variable)? It was noted that the partial-adjustment structure specified above was applied only to employment rates and not to the derived demand curve — in econometric terms it fitted awkwardly into the model, giving different error structures etc., thus making the definition of a suitable estimating procedure hazardous. There was something of a contradiction between an essentially stable labour demand curve (without dynamic adjustments) and a dynamic wage bargaining curve — Phillips on the other hand

issue of capital, some of the audience felt that information flows within the country, though perhaps poor early on, were adequate by 1914 -- Lancashire and Yorkshire were well in touch with the London capital market. The market signals even if not blocked were not clear enough to deter entrepreneurs from what they were doing. In this sense what happened after World War One represented such a discontinuity that change simply had to occur. Moreover, in M. Edelstein's work, quoted in support by Kennedy, it had emerged *inter alia* that once adjusted for risk (variance) foreign investment was undertaken because it was more profitable for the London market. How, it was asked, did rates of return compare on expanding versus contracting sectors -- were those for the former low early on and then building up? (Kennedy had noted in the paper that "no one can seriously believe that the true yield on electrical equipment manufacture in Britain was half that of food processing", i.e. the realized rates of return were distorted). One speaker noted that the growth rates were computed for deflated output series and that differing deflators would give different equations. What was the effect on the dual (prices) of the sectoral changes, i.e. where would the goods be sold (these points are partly covered above; Kennedy had admitted in the paper that the computation of an equilibrium price vector would strengthen his argument). The comparison with the U.S.A. was queried. There were interdependencies as it was observed in the U.K. pattern, e.g. the higher concentration in services affected shipping, foreign investment, etc. Some of the fastest-growing sectors in Britain were also growing quickly in the U.S. (iron and steel, coal, etc.), while others may have caused American growth to be faster had the Americans in those respects imitated Britain. Kennedy's argument had to do with a movement to the production possibility frontier as compared with a movement along the frontier (cf. the Ingham paper), but to one of the audience the Japanese experience demonstrated that such gains were temporary.

The paper by G.N. von Tunzelmann of St. John's College, Cambridge, ("The New Industries between the Wars: A New Development Block?") implicitly followed up one of Kennedy's points for the interwar period (Kennedy has since the Conference incorporated some of von Tunzelmann's findings into a revision of his own paper). H.W. Richardson had argued that the deadweight of "overcommitment" to the interrelated nineteenth-century export staples (like textiles, iron and steel, coal, and shipbuilding) had hindered expansion in the early twentieth century and only been sloughed off in the Slump from about 1930. Thanks to reasonably buoyant demand, a new "development block" of interrelated industries like electricity and electrical engineering, automobiles, aircraft, and chemicals could emerge. The current view of Richardson and D.H. Aldcroft (published in 1969) lays greater emphasis on sectoral shifts than on technical progress *within* the new industries. They did however argue that the capital-intensive technologies of the new industries limited their absorption of labour and so indirectly contributed to the paradox of high

growth amidst high unemployment in the 1930s. The author examined these theses using the input-output table of T. Barna for 1935. Looking at first-round effects the inputs of other new industries into each new industry were low, e.g. compared to those from the old export staples; in the motor and cycle industry, textiles contributed as much to the value of inputs as did electricity (including electric wiring, etc.), while iron and steel contributed four times as much as either. Deeper investigation required counterfactual analysis, and it was supposed as an extreme case that output of the new industries failed to grow one iota between 1930 and 1935. In these circumstances the impact on each new industry in turn was small (the biggest effect on gross output was for electrical engineering, of 3.2½%). If all new industries were taken into account, calculation using simultaneous-equations methods showed that the decline in final demands for all industries (i.e. in consumption, capital formation, exports, etc.) was about the same as in gross output (i.e. including the intermediate goods), so there was no special need to blow up the contribution of new industries any further to allow for inter-industry effects. Different methods of solution were tried — a more heavily constrained linear programming approach showed that, whatever the counterfactual specified, the likely impact of the growth of new industries was likely to be greatest on the old export staples. The links to housebuilding were the subject of some dispute in the literature, but it was shown that housebuilding turned upwards in the autumn of 1932 *before* electricity supply or motor-car registrations, so that the latter could not have had a powerful feedback effect on housebuilding. Finally, it was shown that on the average the new industries were rather labour-intensive, certainly compared with the economy as a whole, and their growth between 1930 and 1935 was as significant for employment as for output.

In the discussion D.H. Aldcroft (who was present) dissociated himself from the coupling of his name with Richardson's in the paper, and emphasized his earlier argument that the break came in the 1920s (not examined in the paper). Other historical points of objection were also raised. The graph of seasonally-adjusted motor-car registrations was queried — another member of the audience had found an earlier upturn than that of late 1933 shown in von Tunzelmann's graph. The 1937 Census of Production could have been used as a further check for motor vehicles. Housebuilding could have been inserted on the same graph for clarity. The seasonal-adjustment pattern was crucial to these findings. The range of new industries was thought too limited by some (the paper had pointed out some of the conflicts of interpretation in the literature) — furniture, linoleum, etc. ought to be taken as new industries when looking at housebuilding, and so on. The glamour of the new industries had an important historical effect, not necessarily exhibited in the counterfactual calculations. The demand factors stressed by H.W. Richardson were thought by some of the audience to need further examination in the paper, e.g. rising real wages or Radice's data on class incomes. The author replied to the former point (real wages) first that

he had doubts about the impact of real wages in the circumstances, so well brought out by C.P. Kindleberger in his recent book on "The World in Depression", of "structural deflation", and second that a graph of real wages showed an erratic pattern with rises that failed to coincide or square with the timing of recovery as required by Richardson. However one of the audience criticized the data on which those real wage statements were based (the data being those of C.H. Feinstein, 1972). The data of the input-output table were also brought into question — were the capital figures, for example, limited to capital equipment, or what were they? (The critic asking this question had found great difficulty on these grounds in using Barna's table for the motor car industry). More widely, an input-output table for investment was required, along the lines of Leontief's "dynamic" model, to represent growth in adequate fashion.

The final paper, by H.E. Jones of Nuffield College, Oxford ("Social Savings on English Canals"), also took an old chestnut from the cliometric brazier — the cost-benefit concept of "social savings", and applied it to English canals. Jones noted that the data for the period he was studying did not gain the advantage from the "statistical era" of the middle nineteenth century that had helped assessments of the role of the railways. He would therefore limit himself to looking at one canal — the River Lea Navigation, organized in 1739 and reorganized in 1766, running from Hertfordshire into London. Its main use over the period in question was for transporting raw materials to London brewers, especially the malt from the great barley-growing areas north and east from Hertfordshire (coal was important later in the nineteenth century). The calculation required the hypothetical closure of the canal for some years and the transportation of the same goods along turnpike roads. Jones found it difficult to specify whether road haulage was 2, 3, or as much as 10 times as expensive as canals. For the case in question the lowest figure was preferred, and gained some support from other canals (e.g. Bridgewater), but it was for a short canal — for longer haulages three times the cost of canal haulage might be more accurate. In the first half of the nineteenth century these would have raised costs for one brewery (Truman's) by 1 to 5%, depending on the particular years and ratios chosen. However canals particularly attracted the cumbersome cargoes that had earlier turned roads into quagmires — had traffic been forced back entirely on roads there could have been renewed chaos. Also turnpike trusts may then have been in a position to exact monopoly rents. Road costs in these conditions could have risen enormously — perhaps to ten times canal costs as an arbitrary guess, involving costs savings of 8 to 19½%. A "reasonable" social savings of 1.2 to 2.3% over 100 years (fluctuating between +12% and -13%). The figure was perhaps overstated because of the possibility of alternative sources of supply. Jones noted that the existence of monopoly rents on canal tariffs meant there was some confusion in the "social savings" literature, but brewers were a small proportion of the membership of the Navigation Trust

and could not have taken much of their cut there. Cautiously one would reject the idea of canals as a leading sector but generally such an assessment was too complex to handle using cost-benefit analysis alone.

The ten-fold increase in freight rates required on Jones's larger assumptions was criticized in the ensuing discussion as being arbitrary. More information on road freightage was called for, and was available in the literature. The increase of two to three times the canal rate seemed more in line with observed productivity differentials. Jones's worry about the monopolistic elements entering into road traffic need not give too much concern, since there was likely to be extensive competition between road hauliers in the counterfactual situation. The path of "social savings" in these circumstances could always be adjusted for congestion effects etc. simply by building extra roads. (allowing for their set-up costs, etc.). Jones was perhaps confusing short-run and long-run marginal costs in his fear of underestimating the "social savings", though it was possible that even the long-run curve was upward-sloping. There was also a potential error of specification in the alternative routing he conjectured. The barley-growing region reached up into East Anglia, and the northern parts at least were tapped by shipping the malt out via The Wash — a practice that would presumably have increased had the River Lea Navigation really closed down. (There was also some doubt the quality of East Anglian vs. Hertfordshire barley for malting). Road technology was constantly improving, e.g. the well-known nineteenth-century improvements in surfacing — this may possibly have caused the earlier "social savings" to be understated. Others asked about the rectitude of extrapolating from this single instance to the "social savings" in general, though it was pointed out that Hawke in his study of railways had extrapolated from the case of coal.

Generally the aims of the meeting were well satisfied. Both the papers and the discussion showed that a more analytical approach to British economic history was at last appearing in British universities. Six of the 8 papers used regression analysis, more often than not including estimation procedures beyond ordinary least-squares (Foreman-Peck, Greasley, Hatton, Kennedy); of the other two, one by Ingham adopted rigorous economic theorizing by algebraic means and the other by von Tunzelmann used input-output and linear-programming methods. The treatment of orthodox economic theory — say mainstream neo-classicism — was rather more wary, especially on the part of the audience, but there was full appreciation on all sides of the need to satisfy formal economic logic. Compared to the American versions of cliometrics there was rather more questioning not only of the appropriate theory but also of the reliability of the data, and the original sources were often heavily queried. An even greater source of contrast was the care to see that the historical dimension was fully understood and accounted for. The historical background was not to be left out on a limb but centrally incorporated into the understanding of specific problems.