‘Economics is what economists do’,
but what do the numbers tell us?

Roger Backhouse, University of Birmingham;
Roger Middleton, University of Bristol;
Keith Tribe, University of Keele

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ABSTRACT
This paper has two purposes: first, to survey existing quantitative analyses of economics and
the economics profession; and, second, to suggest that a more rigorous quantitative approach
towards certain aspects of the subject will not be possible until historians of economic
thought take fuller advantage of available database resources and modern computing
technology. This argument is sustained and illustrated by reference to the three author’s
quantitative work on British and American economics and economists.

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Addresses for correspondence:
¶ Professor Roger Backhouse, Department of Economics, University of Birmingham, Edgbaston,
Birmingham B15 2TT, UK Tel. 0121 414 6655; E-mail: backhore@css.bham.ac.uk.
* Dr Roger Middleton, Department of Historical Studies, University of Bristol, 13 Woodland Road, Bristol
BS8 1TB, UK. Tel. 0117 928 7931; E-mail: Roger.Middleton@bristol.ac.uk; URL:
http://info.bris.ac.uk/~hirm/rmhome.html.
§ Dr Keith Tribe, Department of Economics, University of Keele, Keele ST5 RBG.

POSTSCRIPT
No changes have been made to this document since it was written save that items then forthcoming by the
authors have been replaced with their subsequent publication details.
§1. Introduction

Twenty-five years ago in the *JPE* two economist–watchers, Hansen and Weisbrod (1972), proposed that just as professional baseball and football had their own Halls of Fame, so should economists. And, because they were economists, they should be selected according to a general theory of awards, which they in turn provided, and on the basis of strict empirical evaluation, in this case deriving from enumeration of individual economist’s publications in journals, as reported in the AEA’s *Index of economic journals*. Thus, unlike the then only recently instituted Nobel prize in economics, the economists’ Hall of Fame would be selected by the profession, and on the basis of explicit, ‘scientific’ criteria. Moreover, it would provide a variety of awards, supplementing those for lifetime achievement with those for current accomplishments and special category awards such as ‘most sacrifice bunts’ (most articles of ten pages or less) or ‘lifetime batting leaders by position’ (most total articles by major field).

We reproduce in summary form, and with the sporting categories intact, the results of this exercise in Table 1(RM). The author’s light–hearted tone and appeal to sporting metaphor should not, however, obscure their deeper purpose: not just the economists’ delight in counting but the premise that league tables of individual economists’ journal output (at this time unadjusted for differences in quality or page size) provide the appropriate summary measure of economists’ professional activity. Whilst their levity was a useful antidote to the gloom then infecting the profession on both sides of the Atlantic, it is arguable that Hansen and Weisbrod’s premise and methodology have had an enduring impact on subsequent work which they may not have intended.

Admittedly, this was not the first economists’ league table. The AEA had been publishing proxy institutional rankings since the 1930s, first for graduate schools and later for AEA conference attendance, with Yotopoulos (1961) the first to compile institutional rankings drawn from more than one journal (the *AER, JPE* and *QJE*) or professional association. In an early postwar work, Stigler (1949) compiled a world ranking of contemporary economists (reproduced here as Table 2(RM), but this was based on one source, an AEA survey of contemporary economics edited by Ellis (1948), and it failed the test of comprehensiveness on a variety of counts. Fifteen years later, Stigler (1964) shifted his focus to long–run trends in US journals (*AER, QJE, JPE, REStat* and *Econometrica*), providing an analysis of the occupations, nationality, language and level of technique of authors and their papers. He contemplated using the AEA’s index to examine fashion in economics, but concluded that the AEA’s major field categories at that time were ‘too broad, [with] the index itself recogniz[ing] only the categories of subjects which were fashionable in 1960!’ (p. 48). Overall, this was a tentative study, one designed to illustrate ‘a potentially large role for the statistical method in intellectual history’ (p. 49).

The first systematic quantitative assessment of the AEA’s index – the first five volumes of which became available in 1962 – seems to have been Bronfenbrenner’s (1966) paper on trends, cycles and fads in economic writing, but the focus here was the major field of

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1 The phrase ‘economics is what economists do’ has long been attributed to Jacob Viner (for example, Spiegel 1987, p. 814), but we have been unable to establish its textual origin. We have identified an early (?first) use of it in print as the opening sentence of Boulding (1941, p. 3), who after his PPE degree and a year’s postgraduate study in Oxford had spent 1932–3 on a Commonwealth Fellowship at Chicago with Viner as his adviser. In the British context, we might further observe that Viner’s functionalist definition mirrors the realpolitik implicit in one of Herbert Morrison’s maxims, ‘Socialism is what the Labour Government does’ (cited in Jenkins 1970, p. 101).

2 Hereafter, the origins of tables and graphs are identified by the initials of their author in parentheses.

3 Seasoned economist–watchers may remember the parallel efforts of Leijonhufvud’s (1973) life amongst the Econs and Cowick’s (1974) quantity theory of drink to lighten the ‘crisis’ that featured so prominently in contemporary AEA, RES and BAAS (section F) presidential addresses – in sequence, Leontief (1971), Phelps Brown (1972) and Worswick (1972).

4 On the former, see Froman (1930; 1942; 1952) and Spellman and Gabriel’s (1978) update; on the latter, Fusfeld (1956) and Cleary and Edwards (1960), and also Siegfried’s (1972) update and extension of these works.
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Award categories:
I  ‘Most total hits’ (most total articles published during lifetime, 1886–1967).
II ‘Most total bases’ (most total pages published during lifetime, 1886–1965).
III ‘Most home runs’ (most articles of twenty–five pages or more, 1886–1965).
IV ‘Most sacrifice bunts’ (most articles of ten pages or less, 1886–1965).
V  ‘Lifetime batting average’ (most total articles per year based on twenty–one or more total lifetime articles,
VI ‘Lifetime slugging average’ (most total pages per year based on twenty–one or more total lifetime articles, 1886–1965).
VII ‘Current batting champions’ (most total articles published currently, 1966–7).
VIII ‘Current total–bases leaders’ (most total pages published currently, 1966–7).
IX ‘Lifetime batting leaders by position’ (most total articles, by major field, 1886–1965. Field codes: ET=economic theory; IO=industrial organisation; HET=history of economic thought; A=agriculture; PF=public finance; LE=labour economics; IE=international economics)
X ‘Special awards to female economists’ (most total articles published during lifetime, 1886–1965).

Source: Hansen and Weisbold (1972, app.) with corrections for obvious errors.

publication rather than league tables of institutions and individuals. Thus, whether by design or by accident,5 it was Hansen and Weisbrod (1972) who appear to have initiated the economists’ modern preoccupation with league tables of their published output. Their paper was followed quickly by a flood of studies which extended and refined their use of the AEA index (for example, Lovell 1973, Stigler and Friedland 1975 and Quandt 1976, all still much cited), such that such exercises – which quickly adopted the Social Science Citation Index (SSCI, published triannually since 1966 by the Institute for Scientific Information) – have almost become a sub–discipline in their own right (perhaps class A15, ‘sociology of economics, quantitative studies’, in the current JEL schema).6 They are also perhaps the most frequently read papers in economics, yet, ironically, amongst the least cited! No comparable British sources are available, but pioneering work on the British economics profession was undertaken at this time by Coats and Coats (1970; 1973).

Table 2(RM) Economists cited most frequently in a 1948 AEA survey of contemporary economics

<table>
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<th>Rank</th>
<th>Name</th>
<th>No of citations</th>
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<td>Hicks, J.R.</td>
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<td>Pigou, A.C.</td>
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<td>Lerner, A.P.</td>
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<td>Kuznets, S.</td>
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<td>Schumpeter, J.A.</td>
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<td>Clark, J.M.</td>
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<td>Stigler, G.J.</td>
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</table>

Source: Stigler (1949, table 1).

Hansen and Weisbrod also anticipated later developments in their use of modern

5 They reported that their paper was an outgrowth of a Ford Foundation project on the determinants of economists’ earnings (Hansen and Weisbrod 1972, p. 422n).
6 See Broadus (1971) for a review of early work on the SSCI.
technology. While neither Stigler nor Bronfenbrenner made reference to use of a computer in compiling their tabulations, Hansen and Weisbrod (1972, p. 423) were quite explicit in their acknowledgement of machine-association, indeed with the computer as *deus ex machina*: ‘We trust that any errors in the tabulations and awards presented below will be attributed not to us or to our assistants, but to the Burroughs 5500 at the University of Wisconsin Computing Centre.’ Since then the IT revolution has transformed the computer hardware and software with which to pursue quantitative analyses to such an extent that a typical western academic now has on his or her desktop PC a level of computing power that is an order of magnitude greater than that provided by the most highly specified mainframe computer of this earlier era, and is available at a millionth or so of the cost. He or she also has access to off-the-shelf econometric and general statistical packages which have revolutionised the input, storage, transformation and processing of quantitative (and qualitative) data. They are thus liberated from the rigors of FORTRAN sub-routines and the clutches of computer science department programmers who, even with the best of intentions, necessarily impacted on research design and the questions asked of the evidence assembled. Finally, of course, the internet beckons, providing new opportunities for academic collaboration, data storage and dissemination. Yet, for all of this, it may well be that, in terms of the data collected, its organisation and the hypotheses tested, current quantitative work remains embedded largely in the intellectual world of the 1960s and 1970s, of league tables, of study of the great and the good and of those aspects of economists’ activities which are readily measurable as against those that are not.

In short, is current work taking full advantage of the opportunities provided by leading-edge IT? Our contention is that they are not, and that as a result such quantitative work is failing to make the impact on the history of economic thought (HET) that it should be making. To make this case, we start (in §2) by surveying the (predominantly US) quantitative literature on the (principally US) economics profession, indicating its principal achievements.

§2. A survey of quantitative studies of the economics profession

In a review article of the state of quantitative work on the profession as of ten years ago, Colander (1989) distinguished three grounds upon which the economics profession might be of interest to its members:
A. Prurience and professional interest;
B. As a case study of economic theory; and
C. As a case study in the sociology of scientific knowledge.

Beginning our survey with these categories we summarise in Table 3(RM) recent quantitative work, distinguishing between its geographical coverage (global, US, European, UK and other) and the extent to which the datasets assembled relate to the near past (ST, ≤5 years), the medium-term (MT, ≥5≤10 years), the long-term (LT, ≥10≤50 years) and the very long-term (VLT, ≥50 years). The paucity of genuinely global empirical work is immediately apparent for all three categories, as is the emphasis on the short-term, particularly with respect to the basic league tables of departmental rankings (cols 1–2). By this test, *JEL* class A15 is thus largely an American literature on American economics (institutions and journals) and American economists, and with little interest in the past. This geographical focus may, of course, be an accurate reflection of the discipline. After all, it is now ten years since Portes (1987, p. 1330) argued that, with so many economists taking their professional standards and views of what constitutes an interesting problem from the US, and with publications, citations and Nobel prizes also so dominated, ‘It is perfectly reasonable to ask whether there is now

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7 The connections between computer availability by institution and the development of empirical economics remains a largely unexplored area, but as the recent AEA conference session on 40 years of cliometrics made clear many of the cliometric pioneers did not make use of computers in what we now think of as seminal contributions to the new economic history (for example, Conrad 1997, pp. 408–10 on Conrad and Meyer 1958).
Table 3(RM) *Summary of recent quantitative work on the current economics profession*\(^a\)

A. Prurient and professional interest:

<table>
<thead>
<tr>
<th>Ranking of departments/journals</th>
<th>Ranking of departments by field</th>
<th>Rankings of individuals</th>
<th>Rankings of journals</th>
<th>‘Rankings, rankings’</th>
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<td><strong>UK</strong></td>
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<td>Taylor and Izadi (1996): RAE, research inputs and outputs, ST</td>
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<td>Johnes (1990) and Johnes and Johnes (1993; 1995): RAE &amp; research output, ST</td>
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<td>Kirman and Dahl (1994): EU research, various indicators, ST</td>
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<tr>
<td><strong>Other</strong></td>
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*Note:* \(^a\)ST>0<5 years; MT>5<10 years; LT>10<50 years; and VLT>50 years.
### B. Economics as a case study: Discrimination in economics

<table>
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<th>DeLorme et al. (1994): economists’ demand for journals, ST</th>
<th>Other</th>
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<tr>
<td>Hudson (1996): multi-authored papers, LT</td>
<td>C. Sociology of scientific knowledge</td>
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<td>Laband and Piette (1994a; c): journal editorial practices, ST; MT</td>
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<td>Holub <em>et al.</em> (1991): iron law of important papers (growth theory), LT</td>
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#### Hindshaw and Siegfried (1995): discrimination in AEA program, LT

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<td>Broder (1993a; b): gender and professional achievements, ST; LT</td>
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#### McMillen and Singell (1994): gender and first jobs, LT

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#### Alston *et al.* (1992): surveys of economists’ opinions, ST

#### Hindshaw and Siegfried (1995): discrimination in AEA program, LT

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#### Women’s Committee: report

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#### Mumford (1997): RES

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<th>Round and Siegfried (1994): surveys of economists’ opinions, ST</th>
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**Note:** ST > 0 < 5 years; MT > 5 < 10 years; LT > 10 < 50 years; and VLT > 50 years.
any economics outside and independent of the United States. Unsurprisingly, this view is contested by many European economists (starting with Kolm’s 1988 response to Portes), and has been widely and publicly discussed.


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<td>Liverpool (1903)</td>
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<td>LSE (1895)</td>
<td>A</td>
<td>53.8</td>
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<td>Loughborough (1966)</td>
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<td>Manchester (1880)</td>
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<td>32.2</td>
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<td>Newcastle upon Tyne (1963)</td>
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<td>B</td>
<td>15.4</td>
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<td>Nottingham (1948)</td>
<td>A</td>
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<td>Oxford (C12th)</td>
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<td>A</td>
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<td>Queen Mary Westfield (1915)</td>
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<td>Reading (1926)</td>
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<td>Southampton (1952)</td>
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<td>A</td>
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<td>St Andrews (1410)</td>
<td>B</td>
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<td>Stirling (1967)</td>
<td>B</td>
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<td>Strathclyde (1964)</td>
<td>B</td>
<td>23.0</td>
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<td>Surrey (1966)</td>
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<td>Sussex (1961)</td>
<td>A</td>
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<td>Swansea (1920)</td>
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<td>UCL (1907)</td>
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<tr>
<td>Warwick (1965)</td>
<td>•</td>
<td>A</td>
<td>36.4</td>
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<tr>
<td>York (1963)</td>
<td>•</td>
<td>A</td>
<td>35.6</td>
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</tbody>
</table>

**Notes:**

- **a** Date of establishment of university in modern institutional form;
- **b** Proportion of academic staff included in submissions: A 95–100%; B 80–94%; C 60–79%;
- **c** Schools of the University of London;
- **d** 1996 RAE: 5* International excellence in a majority of sub–areas and of at least national excellence in all.

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In the Colander categories in Table 3, such empirical work as there is on Britain is mainly short-term, with much of it prompted by the four Research Assessment Exercises (RAEs) conducted since 1986 (Table 4RM summarises these results for the most highly ranked institutions). After a long period, therefore, in which there had been little quantitative work on British economics since Coats’s pioneering studies of RES membership conducted over twenty years ago, the British economics profession is once more the subject of study. Unlike the US literature, however, professional self-interest is somewhat differently motivated, although arguably the market effects will be similar to the way in which US league tables influence graduate school entry patterns, private and public sector funding of research and, even perhaps, the salaries of academics, or at least the employment opportunities (and thus opportunities for research) offered by Britain’s nascent Ivy League. Viewed from the longer-run, and through the prism of professionalisation, the effects of the RAEs has been to accelerate the trend towards the Americanisation of British higher education, a process which can only be fuelled by the recent Dearing report (HMSO 1997), not with standing its clearly expressed reservations about research selectivity and the opportunity cost of research for high quality undergraduate teaching.

In respect of category A studies Colander (1989, pp. 141–2) concluded ‘that the ranking game has been beaten to death’; that with the composition of the top ten US departments largely invariant to the measure selected, with all the action much lower down the league, and with the field categories crude and imperfect market signals to guide students in selecting graduate schools, the continued production of this literature can only be explained by their ‘political (show them to the dean to support your budget increase request), psychological, and sociological (show them to your friends and to yourself to make them feel worse and you feel better) roles’ with cognitive dissonance sweeping up the residuals.

Given the current British love affair with league tables in education and other public outputs, and the belated but ineluctable development of private markets in British higher education, it seems safe to predict that category A studies will become routine features of academic competition and production. In economics their belated appearance relative to the US has much to do with the different professional roles played by the AEA and the RES since their inception. The AEA has since the 1970s been much concerned with the quality of economics education, whereas in Britain the RES was historically much more detached and laissez-faire, a stance which has only begun to change with the democratisation of the society initiated in the late 1980s and which have produced major reforms in the RES’s professional objectives and mode of operation. If, whenever, the funding councils carry out the teaching quality assessment of economics that most other disciplines have now undergone since 1992 there will no doubt be further impetus to produce institutional league tables. Yet, will they and the research league tables be of any use to future historians of economic thought? If their current consumption value, as providers of information to diverse agents, is questionable their longer-term historical value may indeed be for what they reveal about professionalisation, about economists’ value systems and behaviours. It is now nearly twenty years since Bob Coats (1980, p. 608) reminded the HET community that:

from the standpoint of the economics profession as a whole it is average rather than peak performance that concerns us, whereas the opposite is true when one is primarily concerned with scientific achievement.

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10 Beginning with Coats and Coats (1970; 1973) and, of the quantitative studies, including Booth and Coats’s (1978) examination of the market for economists and Coats’s (1991) study of British economics journals.

11 For the reservations of public sector professionals and statisticians about these trends, see Goldstein and Spiegelhalter (1996) and ensuing discussion; for an early critical survey of performance indicators in British higher education, see Johnes (1992).
but this distinction does not appear to have much influence upon how league tables are used, not at least in the US. By contrast, in Europe, state-controlled higher education systems and the maintenance of largely separate national economics professions has limited internationalisation until very recently (upon which see Coats 1997; 2000), resulting in no revealed demand for league tables. Moreover, as Eichenberger and Frey (1995) have argued in defence of their thesis that European economics ought to remain distinctively different from American economics, such league tables are utterly inappropriate for European economists whose performance has to be defined and measured differently so as to accord with European market incentives which value participation in local and national affairs, greater policy involvement and an emphasis upon undergraduate as against postgraduate education (Frey and Eichenberger 1992; 1993). In short, league tables confront what is the purpose of economics: a scientific game focusing on abstract issues, defined within the profession itself, or the direct improvement of human welfare through economists’ contribution to policy. If the latter, league tables measure inputs (publications) in the European economists’ production function and divert attention from outputs (the effects of economists on policy).

Turning now to category B studies, economics as a case study of itself, we see what we might call – with no disrespect intended – the political correctness effect in the studies of discrimination (col. 6) and, perhaps, acceptance that such is the current dominance of US economists that such enumerations are no longer interesting (the absence of entries in col. 7). Of more interest, and bearing both on the market for economists and on the sociology of scientific knowledge, we have a variety of studies which explore rationality and self-interestedness through different aspects of behaviour. Once more this is an entirely US literature, and it remains unknown whether we can generalise from the picture that emerges from structured games of economists being more stereotypically rational and self-interested than other agents. Certainly the debate that has been provoked about whether studying economics so alters cooperate behaviour, by raising the propensity to free-ride and to defect in social dilemmas, that knowledge of the subject results in the production of more ‘bad’ citizens would be viewed as quite extraordinary by Marshall and the founding fathers of professional economics, for whom ethics underpinned the quest for rational deliberateness (see, for example, Yezer et al. 1996; cf. Frank et al. 1996).

This brings us to the category C studies which are also very varied but exhibit greater preparedness to assemble longer-run datasets. We highlight five types of study here:
1. The onward march of multi-authored papers (Hudson 1996), a consequence of the ever widening breadth of economics, its increasingly technical content (both theoretically and quantitatively) and the expansion in the size of the profession from whom collaborators may be drawn. This is but one (readily quantifiable) aspect of the rise of the ‘scientific article’ format in economics journals.
2. Empirical studies on replication and of empirical results, many inspired by the Journal of Money, Credit & Banking project, initiated by Dewald et al. (1986), which reveal that in economics the replication of previous studies as part of new research remains highly unusual, as does the collection of data and its dissemination by professional journals (Anderson and Dewald 1994) and that the structure of publication biases (in favour of work that supports or overturns established results) changes over time (Goldfarb 1997).
3. Studies of whether the behaviour of journal editors and the results of refereeing practices enhances efficiency in the market for economic knowledge or produces favouritism and lower quality papers. Amongst recent contributions, two by Laband and Piette (1994a; c) suggest that, what might appear to be favouritism on the part of journal editors, is in practice the use of their contacts to identify and capture potentially high-impact papers, while the ‘blindness’ of peer-reviewing does appear to affect the papers selected for publication. Given the role of citations in such studies (the measure of final paper quality) their extension through time offers particularly interesting possibilities for the HET community, not least given what we now know about classic papers rejected first-time by leading journals (Gans and Shepherd 1994).
4. Leontief’s (1982, p. 104) complaint (first voiced in his 1971 paper), that ‘Page after page of professional economic journals are filled with mathematical formulae leading the readers from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions’, continues to excite interest (the first major update was Morgan 1988). The latest (Fels 1992) contribution uses a wider set of journals, a later time period and a different method to arrive at strikingly different conclusions: rather than economics being dominated by mathematical models without data the contrary is now the case, and with such papers displaying high empirical relevance. However, and here is a glaring example of economics without history, this research was based on only nine journals and but one recent issue (late 1989–early 1990) from each. There can be few hypothesis within the sociology of scientific knowledge more deserving of serious historical scrutiny than Leontief’s (1971; 1982) complaint, and the lack of empirical response may of itself be significant (unfortunately, Figlio 1994 did not directly address it in his long–run examination of empirical economics).

5. With most of the literature focusing on the production side of economics publications studies designed to assist consumers of this class of scientific literature are to be particularly welcomed, especially one formulated in terms of Borchardt’s iron law of important articles: that the number of scientifically significant papers increases to the extent of the square root of the total number of papers published. We here highlight Holub et al. (1991) which validates this hypothesis against the growth theory literature since Harrod (1939), and provides the depressing statistic that an economist, intending only to keep abreast of growth theory and reading just 12 papers per year in this field, will find at least one important article only every third to fifth year.

Such studies as we detail in Table 3(RM), however, do not capture the full range of quantitative work being undertaken in HET or of relevance to HET. In Table 5(RM) we thus extend our sights beyond the rankings game and Colander’s other categories to focus upon:

A. Trends in economic associations and their journals as indicators of professionalisation and the development of economic theory.

B. Departments as producers of economists and economics.

C. The influence of individual economists on other economists, as measured by citations.

D. Classic paper replications, a natural extension of the Journal of Money, Credit & Banking project.

E. Authorship puzzles, a long–standing preoccupation of the HET community.

F. Economists and policy–making, this having particular topicality by virtue of the US vs. European economics debate and Frey’s attempt to reaffirm the importance of outputs (the effects of economists on policy) in the economists’ production function.

Taking these in turn, the role of journals in economics has long been of interest (Coats 1971; 1991; Whitley 1991), but until very recently such works did not rest on quantitative foundations, with casual empiricism the norm. This is now beginning to change, with the IT revolution one reason for this. More particularly, we would cite spreadsheets as the reason, a software tool that ‘want[s] data the way lions used to want Christians’ (Solow 1990, p. 448).

Since such studies will be considered in some detail later on, we proceed to the next category, that of departments. Here we invoke Harry Johnson’s (1977, pp. 98–9) concept of the social geography of economics departments as a means of understanding institutional differences in the character and style of economics research. Johnson admitted this was an ambitious and vague concept, with his definition perhaps uncharacteristically prolix:

I mean by it generally the social relations among members of the same department, as influenced on the one hand by the hierarchy of tenure, remuneration, and power of decision or of influence over collective decision, and on the other hand by the activities and responsibilities of the department – especially teaching responsibilities, but sometimes also research responsibilities – and the cooperate efforts they may entail; also the social relations of the department with its clientele – mainly its students; with its colleagues (peers) in departments in the rest of the university or similar institution in which it exists; with the professional academic community at large; and with the larger society of which the university is a constituent and into which most of its students graduate. All these relationships have an influence on both the style and content of economic discussion with colleagues in and close acquaintance outside the department, and the choice of audience and style of addressing
<table>
<thead>
<tr>
<th>Topic</th>
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<th>US</th>
<th>UK</th>
<th>Other</th>
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<td>Plasmeijer and Schoorl (1997):</td>
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<td>Dutch economics, <em>ST–VLT</em></td>
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<td>B. Departments</td>
<td>Lampman (ed) (1993): Wisconsin, LT</td>
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<td>replications</td>
<td>(1995), n.a.</td>
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<td>technique in econometric papers, LT</td>
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<td>F. Economists and</td>
<td>Adkisson and Blum (1996): education and attainment of members of the CEA, LT</td>
<td>Goodwin and Sauer (1995): effects of government service on publishing productivity, LT</td>
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it for oral (usually unpublished) and written (published) communication at the academic and professional level.

In short, social geography underpins national research styles, a theme upon which Johnson also had much to say in connection with the sins of English economics (1968; 1973; 1975). Social geography is a potentially powerful concept, rich in hypotheses and data–collecting possibilities, and yet as the paucity of entries in Table 5 makes clear the department has not been much studied as an organisation, and especially not over the long–run. Yet, in relation to the home of British professional economics, as one LSE economist, a long–time observer of Cambridge economics, has put it:

Doing economics at Cambridge is like being a Christian during the English Civil War; you can’t just be a Christian. You have to identify yourself as to what sect, what party, what faction you belong to. Your views on the non–substitution theorem are as important as a marker for the rest of your world–view, on everything from beer to sherry to the stance on Vietnam, as your views on transubstantiation were in seventeenth–century England. [Desai 1997, p. xvii]

Our next category (C), individual economists and their influence, is of course the life blood of HET, one given a recent transfusion with the maturity of citation studies from individual/institutional league tables to detailed, tightly–specified studies of individuals’ citation counts. Biddle (1996), author of a recent exercise of this type on Wesley Mitchell, argues that the timing and nature of an individual’s intellectual impact can be more precisely specified, with light shed on the basis of their reputation, the nature of their disputes, their intellectual allies (individuals and institutions) and so on. He also argues that, when related to citations counts of their contemporaries, such exercises provide a general ‘check on the validity of citations counts as a measure of stature and influence in the economics profession’ (p. 138). With the SSCI now on–line (in Britain as BIDS) we should expect many more of such studies, for the production costs have fallen significantly (for example, from my own desktop it takes but 1 minute 42 seconds to query BIDS for the number of times Maynard Keynes is cited in the SSCI between 1981–97 (1,018 for the numerically addicted), with more precise queries of this dynaset taking 15 seconds a piece).13

Given the transformation in power and user–friendliness of modern econometric software we would also expect classic paper replications to be a growth area, and, given the continued error–proneness of this class of software (Lovell and Selover 1994), to provide a continuing flow of suitable case studies. Here we identify some very varied Category D pieces, from Hendry and Morgan’s (1995) mammoth undertaking, which attempted – on the whole, very successfully – to reproduce and test the original empirical findings of key papers from five founding fathers of econometrics, to Wulwick’s (1996) more limited historical, econometric reconstruction of the Phillips curve. One by–product of the latter has been to prompt re–examination of the historical context in which researchers worked, thereby revealing how much of early British applied econometrics was frequently done by those self–taught in econometric technique and without benefit of localised access to computer hardware (the original Phillips curve was estimated on an electro–mechanical Marchant desk calculator, although at that time Phillips was rare amongst British economists in that he had experience of computers – Wulwick 1996, p. 395).15

The next class of studies (E, authorship puzzles), however, appears in terminal decline. This used to be a very active research area, but there has been little since O’Brien and Darnell

12 Lampman’s (1993) edited volume on Wisconsin, which could hardly be called quantitative, is a notable exception but of the US departments there has been little work since Samuels (1976) or Patinkin (1981) on Chicago (although see Stigler’s 1988, p. 16 on how Columbia and Harvard lost out to Chicago in 1933), and almost nothing, apart from De Marchi (1988) on the LSE and the tail–end of Young and Lee (1993) on Oxford, on postwar British departments (see also Tribe 1997 for British economists recollections of their departments).

13 For registered users BIDS is available via telnet at bids.ac.uk.

14 Irving Fisher, Lehfeldt, H.L. Moore, H. Schultz and Tinbergen. Usefully, and commendably, they have continued to make their data available for scrutiny at an FTP site (see Hendry and Morgan 1995, p. xii).

15 See also Richard Lipsey’s (1997, esp. p. xviii n.6) autobiographical reservations about historical reconstructions, econometric and otherwise.
(1982) appeared to settle many outstanding disputes. Recent years have seen the transformation of textual analysis software,"^{16} but its major application is now in the stylistics component of the rhetoric debate prompted by McCloskey and others (for example, Diamond and Levy 1994 on AEA presidential addresses). By contrast, class F studies appear to be flourishing, at least in the US and it is to be hoped that the questions posed by Adkisson and Blum’s (1996) study of the CEA will be applied to the British case. Although we have no direct comparator, an examination of the personnel who served with the Economic Section (later the Government Economic Service) would be instructive about both government and British economics. Goodwin and Sauer’s (1995, p. 740) finding that US economists who have a spell in government service are subsequently no less productive (as producers of journal papers) will, we suspect, not be replicated for the British case, where it has been a long-standing complaint that those serving as civil service ‘irregulars’ found it difficult to maintain their academic networks and theoretical–economic human capital (for example, Little’s 1957 complaints voiced after his period as Deputy Director, Economic Section). Laband et al’s (1990) re–examination of an issue upon which young Stigler (1960) also contributed, that is the influence of events and policies on economic theory, is also to be welcomed, although the two dependent variables (the proportion of total articles in four general journals which included the words (un)employment or inflation in their title) might be thought rather crude proxies for their actual content and topicality. Finally, we have a miscellaneous (F) class of papers. Some reflect current US concerns about the decline in the number of economics majors and whether, judged against the long–term, this might be a transient development or not. Biddle (1999) tackles the spread of statistical techniques in economics, and Goodwin and Meardon (1998) use data from a survey of journal articles to establish changes in the level of US economists’ interest in ‘international’ issues, this comprising both ‘international economics’ and developments in other countries.

§3. Limitations of much current work

These studies, when taken together with those detailed in Table 3, provide clear evidence that elements of these earlier forms remain and we should, accordingly, anticipate continuing demand for rankings which satisfy professional prurience (the schadenfreude effect). Nonetheless, recent quantitative work by, or of utility to, historians of economic thought has moved on very considerably from the basic league tables and citation studies of the 1970s. But there is still a long way to go in several respects, with the result that the potential impact on HET has not been realised.

The first problem with this literature is that it is still predominantly American. Important studies of other countries have been undertaken, but it raises the question of whether British historians of economic thought, as with an earlier generation of British economic historians who resisted quantification and the ‘new’ economic history (Middleton and Wardley 1990), are lagging their American counterparts and thus failing to take full advantage of the IT revolution? This question is prompted not just by the paucity of British studies but by fact that the US versus European economics debate provides a ready–made set of questions amenable, to a greater or lesser extent, to quantification and a particular, local interest if indeed Britain does prove to be a hybrid, mid–Atlantic case.

Before examining these we must first ask whether the comparative neglect of quantitative studies by British economists might be rational. Obviously, HET, like the sub–fields of methodology and economic history, are of low status: ones populated, as McCloskey (1980, p. 213) once put it, by ‘the dolts who work the libraries instead of the bright lads at the blackboards.’ These professional rankings have recently been confirmed by Diamond and

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16 Laband and Taylor (1992, p. 676) provide a now rather dated survey of this class of software as used by economists, while Middleton (1995) surveys use by historians, social scientists and computational linguists.
Haurin’s (1995) long-run study of changing patterns of sub-field specialisation, although it is unknown whether professional disdain for HET is stronger in Britain than in the US. Might it be significant that few of the studies detailed in Table 3(RM) derive from individuals with high-ranking affiliations? Might there then be other disincentive effects which constrain British economists from taking advantage of the more favourable supply-side (advances in software, techniques and data sources) which we would otherwise expect to open up the field? Part of the answer must surely be that British academics as a whole are on average less interested in professionalisation than their American counterparts, with economists conforming to this generalised absence of disciplinary introspection.

Part of the answer must also be that historically the RES has provided far less information than the AEA upon which to base quantitative work. No index to the Economic Journal was published between 1934 and the issue of the first cumulative index in 1983 (RES 1983), while the first membership directory to include data beyond names and address was only issued to coincide with the first hundred years of the RES, and even then was the product of a joint project with the Econometric Society (RES–Econometric Society 1990). It was not until RES (1995) that any breakdown of membership was provided on the basis of JEL sub-field specialisations. By contrast, the AEA have been providing this level of detail in their membership directories since AEA (1942). We would contend also that important opportunities for long-run quantitative analysis of the British economics profession were missed in 1990, the centenary year of the RES. The RES–commissioned Hey and Winch (1990) volume contained surprisingly little history, while the celebratory January 1991 issue of the Economic Journal contained no contribution from a historian of economic thought, and indeed it was left to Milton Friedman (1991) to (very briefly) consider trends in Economic Journal papers (see also Oswald 1991 on microeconomic data, although for him relevant history appears to stretch back only to 1960).


A. Successive EJ volumes, decomposed by JEL class

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17 See, however, Tribe (1992) which provides the missing quantitative dimension for the first half century of the EJ, and also counters the misleading conclusions drawn by Jha’s (1973) attempt to quantify (for the first quarter century) using 5*3 index cards instead of a computer.
B. Successive *JEL* classes, decomposed by *EJ* volume

![successive JEL classes](image)

**JEL classes:**

- **000** General Economics; Theory; History; Systems
- **100** Economic Growth; Development; Planning; Fluctuations
- **200** Quantitative Economic Methods and Data
- **300** Domestic Monetary and Fiscal Theory and Institutions
- **400** International Economics
- **500** Administration; Business Finance; Marketing; Accounting
- **600** Industrial Organisation; Technological Change; Industry Studies
- **700** Agriculture; Natural Resources
- **800** Manpower; Labour; Population
- **900** Welfare Programmes; Consumer Economics; Urban and Regional Economics


Is it then that the potential returns to such research are lower in Britain, not to mention the high psychic (drudgery of keyboarding) costs and that the economic costs are potentially higher when individual British academics are less well resourced for small projects? If so, the quick–and–dirty quantitative work that has characterised most studies to–date may be close to the optimum. Diminishing returns may quickly set in, while potential producers may be only too conscious that most quantitative work only confirms the existing casual impressions of well–informed observers. As all three authors can attest, the derivation of long–run summary tables has to be based upon tens of hours of keyboarding and database design (for example, Figure 1(RM) on changing sub–field specialisms in the *EJ* since its inception must have cost 50 hours or so). We make some suggestions in §7 about how, through cooperation, these costs could be lowered.

A much broader problem with a large part of the literature (including some of our own, we hasten to add) is that it is in a sense ‘quick and dirty’. The reason for this is the immense amount of time required to compile even the most limited database. Researchers are tempted not to compile databases that can be used for future research, but to extract simply the information they require for the immediate task in hand. Rather than throw stones at other people, take as an example the data on the proportion of UK academics whose doctorates were obtained in the United States, contained in Backhouse (1997). This involves getting a list of staff for each institution covered, and finding where they obtained their doctorate. Ideally this would be obtained from the relevant university’s Calendar or similar document, and all staff and their qualifications would be entered into a database. The database could then be analysed for the relevant information. This, however, would take hours, so short cuts are typically used. Using the Commonwealth Universities’ Yearbook rather than the university’s own publications is immensely time–saving, but not so reliable. But the main
problem is that rather than create a database, it is quicker to count the numbers of PhDs and US PhDs on the page, and simply to record those numbers. This gets the relevant information, but it means that no resource for future research is constructed. To cite other examples, Biddle (1999) and Goodwin and Meardon (1998) did construct databases, but they contained only those articles (statistical and international respectively) that were required for the studies. Potential economies of scale from collecting data that could be used for a wider range of research projects were not realised.

A further problem with such work is the absence of any agreed standards for collecting and analysing data. Thus even when different authors have tackled similar problems, their data are difficult to combine. An obvious example here is the use of JEL classifications. Middleton used the 1983 JEL classification (because RES 1983 provided immense cost savings in coding fifty years worth of EJ papers), whereas Backhouse used the current classification. Another example is that both of us classified articles according to the type of mathematics used, but our definitions were slightly different. For specific comparisons this does not present too many problems, but it makes it difficult to combine the data into a single, consistent database. Obviously, complete consistency can never be achieved: the ways in which we wish to classify and analyse articles, and the information about economists that is considered important will change over time, but many of the problems result from taking short-cuts to save time. For example, if 3-digit classifications were used, it would be possible to convert from old to new JEL classifications reasonably accurately, but 1-digit classifications (much quicker to do) are too crude for this to be possible.

Potential new data sources are also not being realised. Foremost here is the dataset which underlay Blaug and Sturges (1983) and Blaug (1986). When the first edition was published a number of reviewers noted its potential for empirical investigations, including those by historians of economic thought (for example, Brandis 1984, p. 471). Yet, most references to Blaug–Sturges have been casual enumerations: for example, Portes (1987, p. 1331), as part of his thesis on American dominance, used the place of residence of the 586 living economists he counted in the book (with, incidentally, different results pertaining when country of birth and education are factored in, a further rational for shifting to relational databases). There has been little systematic work on this source (and nothing, of which we are aware, since DeLorme and Kamerschen 1987 and Frey and Pommerehne 1988), no doubt because it is not available as a machine-readable dataset for others to use. Thus each exercise tends to be one-off, and inevitably quick-and-dirty in style. Consider, however, what might be possible if the Blaug–Sturges dataset had the same status and availability as a research tool that the Penn World Tables (Summers and Heston 1991) have in the applied growth literature.

To overcome these problems, what is required is that instead of data being compiled study-by-study, databases be constructed that can be used in many studies. Some datasets are available (such as the SSCI, Econ–Lit, RES–AEA membership directories) but these contain different types of information and are not easily brought together. In Britain, any project in receipt of ESRC funding that generates a dataset has to deposit a properly documented machine-readable version of that data at the ESRC’s data archive at Essex, but these rules are not enforced as much as they ought, while in any case most exercises of the sort we have identified are American (where different rules seem to apply with respect to the Inter–University Consortium for Political and Social Research (ICPSR) Archive at Ann Arbor, Michigan) and, we suspect, the scale and status of most quantitative work in this field is insufficient to attract public funding in any case. In addition, the generation and use of datasets is constrained by two particular problems which are common to all of modern academia: the uncertain academic standing of datasets (as, for example, in their relegation to the low-status, ‘miscellaneous’ category in the recent RAE) and certain property-rights difficulties (for example, Wheatsheaf Books, the publisher of Blaug and Sturges (1983), has a vested interest in the underlying dataset not being made available).

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18 See Anon (1989a; b; c) for details of these two archives and their holdings. The ICPSR can be contacted at http://www.icpsr.umich.edu/; the ESRC archive at http://dawww.essex.ac.uk/index.html.
Copyright problems also arise with datasets which are published in the first instance in machine-readable format, as for example in the recent CD-ROM compilation of the current *Who's who* with the complete *Who was who* since 1897 (*Who's who* 1996). This is a potentially extremely valuable dataset for those investigating British professionalism. Because it includes so many economists in the policy community, it provides a fascinating potential counterweight to the Blaug–Sturges dataset. It also well illustrates some potential pitfalls of all machine-readable sources. Consider the following comparison. Blaug (1986) lists 97 living economists who meet our definition of membership of the British profession (UK residence and/or significant participation in British professional activities), whereas a query of *Who's who* (1996) for those with the occupation of economist yields a dataset of 239 entries, of whom there is an overlap with the Blaug dataset of only 10.\(^{19}\) Of course, Blaug, following the tradition set by the young Stigler (1964) of defining an ‘important’ economist as one who influences other economists (as against policy-makers and/or the general public), has a view of eminence which, in effect, accords with the characteristics of American professionalisation. Yet, for Britain, this is inappropriate: non-academic economists were important for the first half century of professionalisation, and the financial press in Britain is highly influential, with a tradition of economist–journalists (Hartley Withers and F.W. Hirst before the First World War; Paul Einzig and Oscar Hobson between the wars; Geoffrey Crowther, Harold Wincott and Andrew Shonfield for the early postwar period; with Sam Brittan through to Will Hutton and the Keegan brothers today). Of these, only Shonfield and Brittan makes it into Blaug, but the latter does not come up as an economist in a query of occupation=economist in the *Who’s who* dataset, a graphic illustration of how you need to know your way around a dataset before embarking upon its interrogation (J.M. Keynes is also absent from an identical query of the *Who was who* dataset)! Moreover, copyright law prevents an academic storing in their database any information derived from *Who’s who*.

The low standing of datasets and the need to ensure that they are available for others to use has long been a preoccupation of the historical computing community. Whilst no answers have been forthcoming to the former problem, it is arguable that on the latter historians have made rather more progress than historians of economic thought. Indeed, in terms of the use of relational database technology, historians are streets ahead of economists (for a recent survey of historian’s activities, see Harvey and Press 1996; see also the journal of the Association of History and Computing, *History and Computing*, now in its ninth year of publication). Historians of economic thought seem also unfamiliar with the techniques of prosopography which historians having been using for some time (Stone 1971) and which are being adopted elsewhere in the sociology of scientific knowledge (for example, Söderqvist and Silverstein 1994 on scientific meetings in immunology and Gascoign 1995 on the eighteenth–century scientific community). The computation analysis of qualitative data has also been subject to considerable technical advances in recent years, ones that have been adopted elsewhere in the social sciences (Fielding and Lee 1993 and Richards and Richards 1994). In a world in which economists have long been criticised for their over–reliance on quantitative indicators, and for their dismissal of qualitative matter, it is with some trepidation therefore that we suggest that those studying the economics profession have not yet adopted best practices exhibited elsewhere.

§4. The value of quantitative work of this type

Many readers will no doubt respond that the reason these opportunities have not been taken up is that they do not see the need for such work. Economists have no interest in the past, whilst historians are more interested in texts, great individuals and non–quantitative studies.

\(^{19}\) These are R.J. Ball, Alec Cairncross, J.S. Flemming, Lawrence Klein, Douglas MacDougall, Marcus Miller, Alan Peacock, Brian Reddaway, Andrew Roy and Ralph Turvey.
of communities of economists. This is a mistaken attitude, for whilst quantitative analysis will never (and should never) replace more traditional historical research, it is an invaluable complement to such work.

The first reason is that the use of databases makes it possible to focus on the average economist rather than on exceptional individuals. This is immediately of relevance if our interest is professionalisation and policy influence, and in particular the US vs. European economics debate.

The second is that economists and historians continually make generalisations that are quantitative in nature, but without testing these statistically. For example it is often asserted that immigrants to the US from Germany and Eastern Europe played a significant role in the mathematization of economics since the 1940s. But is this is potentially amenable to statistical testing. Did a larger proportion of articles by such émigrés contain mathematics than those by other economists? Did *Econometrica* or the *Review of Economics and Statistics* contain a higher proportion of articles by German or Russian émigrés than the *AER* or the *JPE*? It may, of course, be that the average does not matter, and that it is a small number of individuals that matters, but the challenge of testing the hypothesis statistically forces one to be much more precise about what is being claimed. If the argument about émigrés is simply that, say, Marshall, Leontief, Schumpeter and Hurwicz transformed the subject, then one would test this differently (using perhaps citation counts) from if it is that the large number of mathematically–inclined émigrés mattered.

The third reason is that statistical data can point to puzzles that need explanation, factors that might otherwise be forgotten, or suggest areas where new explanations are needed. For example, how many histories of economic thought pay attention to the movement of economists into government service during the Second World War? But a simple graph of the affiliations of contributors to leading US (and, to a lesser extent, British) journals makes it very clear that this took place on an enormous scale. We all knew this happened, but the data remind us of the fact.

In short, we are claiming that economists should require the same standards of evidence that they would expect to find applied to other problems. If a macroeconomist makes a claim about income or unemployment having risen or fallen, he or she will be expected to substantiate that with empirical data (using proxy variables if the ideal data are not available). When historians make similar, potentially quantitative, claims they should expect to justify them.

An excellent example of the potential for such quantitative research is the debate over US versus European economics debate. Frey and Eichenberger’s (1993) have offered several explanations for the distinctiveness of European economics. They see economists as market agents, responding to incentives and operating with a welfare function which might be specified as combining the maximisation of the present value of their lifetime income with certain culturally–determined non–pecuniary goals. The American academic market is large and competitive, whereas European markets are segmented and thin. This conditions the propensity of an economist to invest in acquiring local knowledge of markets and institutions, resulting in Americans maximising journal publications and Europeans seeking a more balanced portfolio which includes participation in local and national affairs. American economists concentrate on postgraduate teaching, while Europeans are largely preoccupied with undergraduate education. The differences between the US and European markets could explain why European research is more policy–oriented, less prone to fads. Thus, as Sargent (1963, pp. 1–2) put it many years ago, ‘American economists are fundamentally more serious about their subject than we are; and the result of this is that they are better economists, but less useful ones.’ In Frey’s formulation, European–style economics is superior because ‘economic knowledge is transformed more effectively into policy’ in Europe than in the US (p. 192), although the moves towards European integration and the internationalisation (Americanisation) of economics were now threatening the incentives which underpinned European economists’ production functions, leading to the pessimistic conclusion that ‘the future of economics as a relevant social science seems rather gloomy’ (p. 192).
From this literature, we can extract a list of key characteristics of American economics (Backhouse 2000):
1. Greater proneness to fashion and intolerance of heterodox ideas.
2. Greater homogeneity across departments, at least in research–oriented universities.
3. More competitive labour markets and higher mobility.
4. Greater emphasis on technique and less emphasis on applied theory.
5. Less hierarchical organisation of departments, with individuals free to pursue independent research at a much earlier age.
7. Lower social status of academics.
8. More highly–developed graduate programmes.

On many of these criteria, Britain has a hybrid status, falling in between the US and European models (Baumol 1995; Backhouse, 2000), and having moved significantly ‘Westward’ in the period since 1945 (Backhouse 1997, pp. 31–2):
1. Undergraduates increasingly use the same textbooks and learn substantially the same theory as their counterparts in other countries, notably the US.
2. Graduate coursework has increasingly become a precondition for research in economics, with the doctorate becoming essential to gain employment as an academic economist.
3. Frequent publication is important to secure professional advancement, with refereed articles published in a these being organised in a hierarchy of prestige and attracting an international cast of editors and contributors.
4. The development of conferences and pre–publication networks for the coterie, together with the exchange of staff between British and American institutions.
5. A growing emphasis on mathematical theory and econometric technique.

Many of these trends are potentially quantifiable, given suitable databases.

§5. British and American economics

Quantitative analysis is here applied to investigate two ‘transitions’: the spread of American–style professional economics in Britain after 1945, and the transformation of US economics from 1920 to 1960. The first of these involved using the Commonwealth Universities Yearbook to document the increasing spread of the PhD, and the numbers of staff in British universities whose PhDs were from overseas, in particular from the US. Our results are in Figure 2(REB). The proportion of staff with PhDs was fairly steady, at between 10% and 40%, up until the mid 1970s, but then rose steadily with the late 1970s the crucial period. Most noticeable is the rise in the Oxbridge figures from 10% to 50%, and LSE from around 30% to around 70%.

PhDs can be analysed by country. In 1945, most overseas PhDs were from Europe and only a minority from the US, but by the early 1970s the situation was reversed. The distribution of US PhDs across institutions differed greatly, and is shown in Figure 3(REB). LSE and the then ‘New Universities’ (Warwick and Essex) have a high proportion of US PhDs. Once we add to these the considerable number who have US first degrees, especially at the LSE (mostly Americans coming to Britain), we arrive at the conclusion that the proportion of staff trained in the US is very high indeed (see Backhouse 1997).

The advantage of compiling quantitative data is that it makes precise the timing of the change – the 1970s are the crucial period – which can then be related to developments within the institutions concerned (such as the influence of Harry Johnson at LSE – see Backhouse 1997 for more detail). It also highlights the difference between LSE and other institutions, notably Oxbridge, a difference which emerged during the 1970s.

Whereas a database of staff was crucial to investigating the changes in British economics, the transformation of US economics was investigated through a database of journal articles. This was analysed for changes that might be relevant to a perceived change from pluralism in the prewar period, to a more hegemonic neoclassical economics in the
postwar period. Associated with this are the decline of institutionalism and the rise of mathematical economics and econometrics. The sample used was of the three major ‘general’ journals, the AER, JPE and QJE. However, attention was also paid to the economists’ origins and training. Here the issue was not whether people had PhDs (most did), but which institution the PhD came from. Also of interest is the birthplace of economists writing in these journals. Figure 4(REB) shows that the migrations from Europe had a quantitative as well as qualitative influence on economics journals, the proportion of AER authors born in ‘Eastern’ Europe (including Germany) rising to nearly 40% by the late 1940s.

Figure 2(REB) *Proportion of staff with doctorate in different types of university*

![Proportion of staff with doctorate in different types of university](image1)


Figure 3(REB) *Proportion of staff with US doctorates in types of university*

![Proportion of staff with US doctorates in types of university](image2)

*Source: Backhouse (2000)*

That these were economists who migrated to the US is made clear by comparing Figure 4(REB) with Figure 5(REB). The proportion of AER authors who gave a US affiliation declined after 1945, but remained around 90%. This contrasts clearly with the international character of British journals (Backhouse 1997). Figure 5(REB) also makes very clear the impact of the war on economists’ careers. The proportion of contributors to the AER giving a university affiliation fell below 40% in some years. In interpreting these, note that those not giving an affiliation will include some who have pursued an academic career, but who have retired.
Everyone is aware that economics has become more mathematical over the past half century. This has taken the form of a proliferation of journals specialising in mathematical economics, and also the increasing use of mathematics in more general journals. This is explored in some detail in Backhouse (1998). The possibilities for international comparisons are illustrated in Figures 6–9(REB), which provide comparisons between three journals – two general journals (AER and EJ) and one specialist one (Econometrica). The data are derived from examining the pages of the journals and recording for each article whether it uses...
algebra, calculus, diagrams (excluding graphs of data) and econometrics (regression analysis). Figures 6–9(REB) give the percentage of articles using the technique mentioned.

Here it is worth making a comment about the limitations of the data. To reduce data collection to manageable proportions, Middleton (1998) takes every 10th year. Backhouse (1998) every 5th year but for a more limited period (1920–80), and for the period 1920–60 a complete sample. The data on *Econometrica* have been added since. Comparison of the 5–year sample with the full sample for the *AER* reveals that sampling error can be a serious problem with such samples. 1950, 1955 and 1960 happened to be years when the use of regression analysis in the *AER* was below the average: the use of econometrics in the *AER* is thus understated (see Backhouse, 1998). With the *AER*, ‘Communications’ and ‘Papers and proceedings’ were excluded.

Figure 6(REB) *The use of algebra in the AER, EJ and Econometrica*

![Graph showing the use of algebra in the AER, EJ, and Econometrica over time.](image)


Figure 7(REB) *The use of calculus in the AER, EJ and Econometrica*

![Graph showing the use of calculus in the AER, EJ, and Econometrica over time.](image)

*Source: as Figure 6(REB).*
Questions we might wish to ask of such data include the following:

1. Are there significant differences between the two general journals, and between them and the specialist one?
2. When did the use of various mathematical techniques become widespread?

   Tentative answers:
   1. From 1910 to 1950, the *EJ* was significantly more mathematical than the *AER*. By 1970, however, the difference had disappeared. If we assume that the important developments in economics during this period have involved the use of mathematics, this is consistent with the *EJ* having lost its once pre–eminent role in the profession. Though *Econometrica* remained more ‘mathematical’ (according to this criterion), the gap between it and the other two journals narrowed sharply. (The decline in the use of algebra in the *EJ* in 1990 is surprising, but caution should be applied in interpreting it, lest it be due to sampling error.)
   2. The mathematization of the *EJ* and the *AER* really starts from around 1940.
   3. The use of diagrams took off later, the sharpest rise being from 1940 to 1950.
Interestingly, there is barely any difference between the frequency of articles using diagrams in the *EJ* and *Econometrica*. Also noticeable is a decline in the proportion of articles using diagrams after 1960 for the *EJ*. The *AER* exhibits a fall in 1960 and 1980, but they are impossible to distinguish from sampling error. The use of diagrams in *Econometrica* remained fairly constant from 1960 to 1980.

4. The widespread use of econometrics (defined as regression analysis and diagnostic statistics) dates from the 1970s for the *AER* and the 1980s for the *EJ*. It is possible that this catching up was the result of deliberate editorial policy, to publish more applied articles. Interestingly, the proportion of articles using regression analysis in *Econometrica* remained very stable, and relatively low compared with the other journals.

Taken on their own, graphs such as these are of limited use, but they are useful for making more precise claims about trends in the subject and for suggesting questions that need to be answered. As regards European–US comparisons, work such as this does little more than scratch the surface, but it provides a starting point.

§6. Student databases and their uses: the Cambridge economics tripos

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**TABLE 7(KT) Economics tripos, Trinity College, Part I only, 1908–55**

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§7. Conclusions

We offer two conclusions and two suggestions for further thought and, hopefully, action.

First, if economics is what economists do, then the American–inspired league table approach generates potentially misleading answers about what European (including British) economists have done and may, if Frey’s worst fears about Americanisation are not realised, continue to do. We thus need to count, but we need also to count books, pamphlets, media output and participation in local, regional, national and international policy networks. As Frey and Pommerehne (1988, p. 109) concluded from their examination of the Blaug–Sturges dataset, not only is there something rather artificial about the American dominance of economics in its own terms, but a seriously misleading picture is being generated of what all economists do because of the confusion of inputs in the economists’ production function with outputs. Accordingly, future research needs to widen to embrace such central matters as time budgets and incentive structures. Certainly, an examination of cross–country and cross–institutional distributions of academics’ time, as between teaching, administration, research and the wider public stage, would yield fascinating results. As we have demonstrated, the US vs. European economics debate provides a wealth of potential questions amenable to empirical exploration, as indeed did Harry Johnson’s strictures about national research styles and the sins of English economics. For example, Johnson (1973, p. 66) made much of the more ‘professional’ behaviour of American than European (especially British) economists, ascribing this to the lower social standing of American academics as against Europeans who were members of the national elite. Casual empiricism suggests there is no European model (contrast the political positions attained by economists in Britain as against Italy) – but here is a topic rich in prosopographical possibilities, one which can potentially illuminate much about professionalisation and the policy influence of economists.

Secondly, if historians of economic thought are economists first and historians only latterly, then they have obligations to the present and future direction of the parent discipline. Knowing what they do about the limitations of league tables, citation studies, and the long–term trend of research and rhetoric, they need to remind their historically less animated colleagues of how the profession’s incentive structure is being transformed and with what likely consequences for economics’ future as a relevant social science. This seems particularly important in Britain where higher education, along with almost everything else in the public sector, is currently swept along in a league table fever. Anecdotal evidence has it
that in RAE–ranked 5 and 5* departments (listed in Table 4(RM) the traditional commitment to high–quality undergraduate education (identified by Johnson as a distinctive European characteristic) is under serious threat, while the effects of the RAE on the volume and value of research are potentially momentous, sufficient perhaps that, in the future, historians of British economic thought will devote considerable study to the RAE process. On a more positive note, four rounds of the RAE provide a rich store of economists’ non–journal publications with which to derive the broader enumerations that we argued for above.

From these conclusions we make two suggestions. First, just as historians are now doing, it is time to move on from the quick–and–dirty, lets build a (flat file) spreadsheet and see what we get, to use of fully–featured RDBMS with careful database design so as to reap the full benefits of the relational model. Here historians can help, having a wealth of expertise with RDBMS (Harvey and Press 1996), a distinguished track record of empirical investigations of the development of professions (see Bradley 1994 for some of the technical issues), and above all a facility with prosopography (collective biography), seen by many in the historical computing fraternity as ‘a means of getting underneath what historical actors said about themselves and explaining their behaviour with reference to their social, economic and political affiliations’ (Greenstein 1990, p. 60).

Secondly, the constraints on quantitative studies that we have identified are not likely to lessen in the near future. More data will undoubtedly become available, principally through the internet, and much of this will be useful for the types of investigations we seek to promote. Unfortunately, we will also have to start paying for much of these new datasets, as for example in the JSTOR project and the new BIDS service, Journals Online. Academics will thus need to develop trade in data and knowledge of on–going research, making more active use of the internet to not only widen the invisible college but to make more resources available to the community and to increase the rate of utilisation of them.

The History of Economics Society already has a page devoted to Archives. This could be easily supplemented, or a new site created, with a listing (and/or FTP facility) of datasets relevant to HET which are already in existence or under construction, together with details of their availability to other researchers and with full documentation of the datasets along the lines of the best practice suggested by Essex and other electronic data archives (the A15 page). The replicability of the Penn World Tables provides an obvious model, but no doubt is over–ambitious. But how to encourage trade, even if one can surmount the complex copyright problems that are entailed with many potentially useful sources? One suggestion is for HET journals to follow the practice initiated by the Journal of Applied Econometrics in 1994 in requiring that all authors deposit their datasets in a specifically designed anonymous FTP site, so as to encourage reproducibility. This will raise the status of datasets, although as historians have found what is needed is a cultural shift so that datasets become ‘editions’ in their own right (Speck 1994). And this is altogether more difficult, particularly perhaps for economists.

Acknowledgements
We would like to thank Jeff Biddle for assistance with tracing the Viner quote and for providing bibliographical references to interesting current work, Kirsten Madden for drawing our attention to her 1995 Ph.D. dissertation.

20 For example, see McNay’s (1997) investigation of the impact of the 1992 RAE on institutional and individual behaviour.
22 Available at http://cs.muohio.edu/~HisEcSoc/.
23 Available at http://qed.econ.queensu.ca/JAE.
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