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Cabinn'd, Cribbed, Confined: the past, present and future of research methods

1. Background and Context

Although an autobiographical element is permitted in such talks, my defence is that in order to contextualise what I shall want to say about the present and future, the past needs its say. You may relax in the knowledge that it will be very selective.

Leeds

My scholastic and academic background is very different from quantitative social science. Being an anglican ordinand from the age of 14, my teachers believed that Classics were the only possible background, and disallowed my desire to continue Science subjects. Though I have never regretted my classics background (it is a continuous resource for pedants) proof of its unsuitability lay in the fact that, having passed both Latin and Greek at "O" level, my Latin " A" level result was so poor that it was not even awarded a compensatory "O" level; proof positive of deterioration! When I started at Leeds University in 1958, I was determined to include a social science subject (Psychology – I had been a mental nurse for two years), and though the Religious Community training us disallowed it, they grudgingly allowed Sociology, and so the die was cast. The following year I moved on to Sociology and Philosophy Honours, and then on to a PhD in Sociology.

There were only three Departments of Sociology in the UK at the time, and Leeds was headed by a mathematical demographer called Eugene Grebenik. He took a somewhat disparaging view of Sociology, and it was said that the only thing which united the department was common opposition to his view. But because I had chosen a research topic with empirical content – in effect a parallel to and expansion of Robert Merton's The Student Physician applied to anglican Clergy (The Student-Priest?) – he decided that I needed both remedial mathematics and thorough tuition in quantitative methods and burgeoning computer programming. I began in a graduate class of 5 and finished as the sole member, having an hour of total mystification each week and the rest of the week finding out what we had done. It was a steep, but ultimately rewarding, learning curve. It was considerably eased by the popularization of "New Math" heralded by Kemeny and Snell's Introduction to Finite Mathematics and celebrated in Tom Lehrer's famous song"[1]

[1] Contemporaries will remember my then-colleague Tom Lehrer's tribute: A YouTube sound/animated version is on: http://www.youtube.com/watch?v=a81YvrV7Vv8 "Hooray for new math/ New-hoo-hoo-math/ It won't do you a bit of good to review math/ It's

<Animated YouTube audio http://www.youtube.com/watch?v=a81YvrV7Vv8 >

It was also my introduction to computing and programming, in Algol, using paper-tape (I did not encounter a punch-card until 1968) and the KDF9 computer, common to most British Universities[2].

This was also the era of rapid expansion in Higher Education in the UK, following the Robbins Report[3] in 1963. Having obtained the PhD in 1965, I became an assistant lecturer in the department at Leeds, featuring as the Department's "George", thus named by Lazarsfeld, – the friendly fellow in every department who became "the" methodologist; the token "Methods Man" available to all. I was also loaned out to help teach other Methods courses at Durham and at Nuffield College, Oxford – the latter already the focus of quantitative excellence and the home to Chelly Halsey and Jean Floud who personified the burgeoning interest in social mobility. Even then talk was of having a parallel English study to the U.S. "Occupational Changes in a Generation" (which resulted in the epoch-making Blau and Duncan The American Occupational Structure, of which more anon).

Class, Quantitative Methods and Measurement

Leeds was not only important for providing an environment for my methodological development, but also my political and substantive development. The 1960s were awash with the concept of "Class" in both political and social science circles. In the heady and effervescent atmosphere of an emerging New Left, Sociology students like myself at Leeds University (claimed by the Daily Telegraph to be the place where the militants of the coming revolution would be drawn) learned at the feet of Marxists of all hues and of Sociologists of all varieties about the centrality of class in sociological discourse and in political action."False class consciousness" became a by-word for explaining the (then) current condition of the Working Class, and because purely mechanistic interpretations of the emergence of class consciousness were eschewed, it was a challenging analytic puzzle to explain precisely what was happening. Ossowski (1957[Polish], 1963) in particular had made class-images the dependent variable of interest, postulating that the concept of social class involved a "common model and discrepant definitions", treating them neither as derivative distortions of an independently established structure, nor as final accounts of co-equal validity.

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so simple/ So very simple/ That only a child can do it! "

[2] The KDF9 still occasions a good deal of nostalgia, and this may be refreshed by visiting http://www.cs.ncl.ac.uk/events/anniversaries/40th/images/kdf9/index.html

[3] The first principle in the terms of reference contained the clarion call: *"There should be maximum participation in initial higher education by young and mature students and in lifetime learning by adults, having regard to the needs of individuals, the nation and the future labour market. . ."*

On the more Fabian shores of empirical sociology, the topics of embourgeoisement and of social mobility were also exercising the best practitioners under the "Affluent Worker" banner in Cambridge (Goldthorpe et al 1968), and at Nuffield, Oxford under the "Social Mobility" banner (Floud, Halsey & Martin 1956; Goldthorpe et al 1980). This issue: how different "[class] images of society" relate to each other, and to a notional "objective" occupational structure became my main substantive interest for the next two decades, and also that of Charles Jones whom I was to meet subsequently at Edinburgh.

Meanwhile in the United States, equally important and related developments were taking place in research methodology the areas of measurement and multivariate data analysis. Of particular relevance was the rebirth of the causal controversy in the 1950s (Bernert 1983) as to whether causal reasoning and inference was relevant or appropriate to the social sciences. In this debate, Blalock and Simon called upon econometrics to develop "Causal modelling" and Duncan called upon Sewall Wright and other developers of biometric "path analysis". Unsung but equally relevant was the work of Hanan Selvin at UC Berkeley, who had been commissioned by its Survey Research Centre in the early 1960s to assess the impact of computers on survey analysis. In his unpublished (but widely circulated) Report he very cogently delineated the logic of causal analysis as used in survey analysis (later published in Hirshi and Selvin 1967). He went on to argue that with the advent of computers it became possible to take advantage of the much more powerful multivariate procedures of the General Linear Model to deal with the inherently multi-variable and complex causal models needed in the social sciences and that this would effect a methodological revolution[4]. The main hesitation was that the procedures of Multivariate Statistical Analysis almost always required interval level data - real numbers - whilst the social sciences' variables are (at least in terms of Measurement theory) rarely even ordinal, and are usually nominal . An all too frequent way of dealing with this was (indeed, still is) "pseudo-quantification" whereby ordinal data (such as ratings) are treated as interval data – measurement by fiat. Robyn Dawes (1972, p 96) points out the ubiquity of the rating scale in sociology, political science, and shows that in almost two-thirds of social psychological studies, the dependent variable is measured by the rating scale - which of course has no consistency checks on the respondent's rating behaviour, making it at best Index measurement. Because there is no demonstrated isomorphism between real numbers (or indeed test of ordinal status in many cases), then one can never know if (say) the failure of an hypothesis was genuine or due to artefactual properties ascribed to the numbers.

Similar concerns exercised the large social science community of methodologists at University of Michigan, Ann Arbor. Clyde Coombs (1950, 1954, 1964) had developed

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[4] I cannot retrieve at this point the exact timing of the commissioning of the study nor of the Report itself (entitled: "The Logic of Survey Analysis"); the latter would be no later than 1964.

a geometrical (representational) theory of data in the '50s and he argued that social science should choose the "option for the weaker assumptions", i.e. rather than assuming the data are interval-level, assume that they are at lower (ordinal or "non-metric") level, making less restrictive assumptions. The axiomatic approach to measurement (Krantz et al 1971), also associated with Ann Arbor, certainly inspired the Representationalist account of measurement, even if Coombs' version of the rules of measurement procedure have been honoured more in the breach that the keeping.

<FIGURE 1 here: Coombsian Measurement procedures>

Nonetheless, Coombs was able to show in his development of Unfolding theory that rank-order data could be shown to imply more than purely ordinal information – specifically, information on the order of the intervals between points, the so-called "ordered metric" scale – if certain qualitative measurement requirements held in the data. Given that demographers in particular had an intrinsic interest in such information (in the form of the size of birth-intervals), it is perhaps not surprising that the first application of Unfolding Analysis was in just this area (Goldberg and Coombs 1962). Though the numerical procedures used satisfied only three-quarters of the subjects it was possible to infer quantitative information in the form of systematic differences in birth-intervals by parity:

<FIGURE 2 here = Fig 5>

But even at best, there were serious problems in developing feasible scaling procedures, and even Coombs himself only attempted to recover the *rank-order* of co-ordinates on his version of non-metric Factor Analysis. Nonetheless, commitment to the "non-metric approach" meant that Ann Arbor had staff (Lingoes, Guttman (actually in Israel), Andrews, Sonquist and Morgan) who later became pre-eminent in the development of ordinal equivalents of multivariate methods and multidimensional scaling.

Convergence of Interests, Research and Teaching

We move now to the summer of 1968 - the student revolution in Paris, the Vietnam war and the year Harvard blew up. We are four years after Jim Coleman's Introduction to Mathematical Sociology and Clyde Coombs' A Theory of Data . For a junior lecturer and budding methodologist,

> *Bliss was it in that dawn to be alive,*
> *But to be young was very heaven!(Wordsworth The Prelude.Book xi.)*

In those days UNESCO ran a month-long international annual Summer School in Mathematical Social Science, held in that year in Gummersbach , west Germany, to which I was invited. The Instructors included Paul Lazarsfeld and Neil Henry (latent structure analysis); Anatol Rapoport (graph and network theory) and Johann Pfanzagl (measurement systems).

- To sit at the feet of Lazarsfeld, the *doyen* of Research Methods (and

accompanying family, including his second wife Patricia Kendall, was privilege enough. Although we learned perhaps more than we wished about the algebra of dichotomous systems from him (Lazarsfeld 1961), it became very clear how Latent Structure Analysis (and especially Latent Class Analysis) represented an implementation of the classic Lazarsfeld conceptualization of the empirical research and measurement process.

- Anatol Rapoport elaborated not only the area of social networks but showed how the properties of large network structures can be studied by probabilistic models; we were also in awe at a mathematical biophysicist who was also a professional concert pianist ... was it worth even thinking of an academic career for oneself?
- Johann Pfanzagl gave an entré not just to theories of measurement and to the formal algebra of axiomatic measurement theory, but also impressed on us that most of social scientists' "measurement" was *stricto sensu* nothing of the sort.

But the UNESCO school was also a valuable lesson in the power of networking in the sense of meeting friends who remained in contact as the years went on – and methodologists often *need* friends! The Czech member of the school watched anxiously as the situation in Prague worsened, but he stayed to complete the course – then returned home and joined the street demonstrations against invading Russian tanks, having first secreted a magnetic data tape from his Census employment safely at home! First things first.

Following the School I spent an academic year in Cambridge, Mass. as a visiting lecturer at MIT, working with Hayward Alker on simulation (a topic I am afraid I lost interest in), with the late Phil Stone of Harvard on the General Inquirer (as it was being re-designed and re-programmed in PL/1) and attending Harrison White's impressive graduate class on social network analysis. I also learn about new packages for data analysis – "Data-Text" by David Armor at Harvard, SPSS at Chicago (which will feature later) and an interactive analysis package named "ADMINS" at MIT. I also underwent a conversion experience, from LSA to multidimensional scaling.

Although I knew of non-metric MDS before, its truly revolutionary potential as an answer to the "quantification problem" mentioned earlier and as applicable to an enormous range of types of data only dawned as I read Shepard 1966 (p288 "if nonmetric constraints are imposed in sufficient number, they begin to act like metric constraints" and guarantee in the limit a metric solution). I was also introduced to a newly defended (1968) PhD thesis by Eddie Roskam of Leiden (later, Nijmegen) University: Metric analysis of ordinal data in psychology which integrated and extended a set of basic programs emanating from Louis Guttman (Israel) and Jim Lingoes (Ann Arbor) in a straightforward way.

in 1969 I returned to a new post at Edinburgh University, bringing a copy of both SPSS (yes, I am afraid it was I who was responsible for bringing it to Britain, but

that's another story[5]) and Roskam's MDS programs with me. Here I met Charles Jones, who was to be my main collaborator during my time at Edinburgh and beyond, and who shared the research interest both in occupations and cognition and the relevant research methodologies.

So came together the main threads of my methodological interest – Variables, Networks and Meaning[6], of the substantive interest in subjective aspects of social stratification, and an indication of my learning and teaching process and institutions in which they had occurred. Within two years I then crowned my academic endeavours by teaching at the ECPR and ICPR Summer Schools!

2. The Middle Years

From this point I want to become much less expansive and focussed on my own experiences and home in rather on two main themes:

- First, to examine the developments into the present time of two main areas of methods that concern and interest me, and then
- Secondly, to work from the point at which I discovered my over-estimation of the British system of research methods teaching to the so-called "crisis in research methods skills" and lessons which I think need to be learned for future development.

Moving to Edinburgh marked the convergence of several lines in my professional development. It saw the start of my funded research career, my engagement with software development and with the development of my graduate methods teaching experience. To elucidate later acronyms, the relevant funded Projects were:

- **POOC**: The Project on Occupational Cognition and subjective aspects of social stratification (SSRC, with Charles Jones), 1972-75
- **MDSX**: Multidimensional Scaling Programs (SSRC, with Charles Jones, David Muxworthy and Stephen Tagg), 1974-82
- **INQR**: General Inquirer: Content analysis as a social science resource
- (SSRC) 1972-75
- **SIGMA**: Socio-sexual Investigations of Gay Men and Aids (MRC, with Peter

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[5] John Hall in a talk to ASSESS at York University in July 2006 comments: "{SPSS} came from Chicago to Edinburgh in 1970 via Tony Coxon and was implemented at ERCC (one of the few places with an IBM) by David Muxworthy and Marjorie Barritt (thereby scotching university plans to commission a survey processing facility at great expense from scratch) and when first installed was reputedly called more times than the Fortran compiler"

[6] The name I gave my inaugural lecture in Cardiff in 1975

Davies and Dr TJ McManus) 1986-2002.

POOC (Coxon & Jones, 1978,1979a,b) was the framework within which many of the other developments took place. It was designed initially to test and evaluate the measurement assumptions and cognitive assumptions implicitly written into Social Mobility studies, especially those in the Duncan Path Analysis models[7].

- that prestige increment scores are genuine interval level measures, and/or that a real-valued continuum underlies the ordinal category of 'class' (or occupational status)
- that this 'dimension of movement' [prestige] is UNIdimensional, thus allowing use as a quantitative/interval variable to measure mobility and therefore able to feature as dependent variable/s in SEM / 'Path Analysis'
- that prestige data can sensibly be analysed as simple evaluative judgments, independent of any cognitive conceptions of occupational structure
- that there is social consensus over Occupational Structure and its perception - a common shared perspective with no significant individual or social differences.

It is important to remind oneself that at this time occupational prestige hierarchies were held to be a "great [international] empirical invariant" by social functionalists (Kerr 1960, Marsh, 1971), hence our questioning of it in the Project. Later this "great invariant" was shown to be dramatically wrong by various later national studies (Penn 1975 and Wegener 1987) and (as we had argued) arose in large part from the use of suboptimal methods of measurement and aggregation error.

Research Methods and Computing

The research methods that were clearly relevant to these research questions were of course increasingly computer-based. But computing procedures in the 1970s were far from user-friendly – some, indeed, actively user-hostile! – and proceeded at a rate now considered intolerably slow. Major technical constraints thus existed.

The General Inquirer, we used to content-analyze the verbalizations of subjects as they performed the various forms of data-sadism we used and devised. (Coxon and Jones 1979b). It contained the large Harvard Dictionary which needed to be in core in order for us to modularise it as part of the Edinburgh project . But this required the largest partition of core, which was only accessible once a fortnight ! Time-sharing and parallel processing was far in the distance...

Multidimensional Scaling The technical constraints applied to multidimensional scaling. But in addition, our comprehension of the expanding area was limited, and we were faced by a bewildering variety of programs, from different research “stables”

[7] The work of POOC is described most succinctly in Davies, Coxon & Jones (1999), and recently re-evaluated in Coxon 2006

(especially Bell Laboratories and Ann Arbor – who carried on their confusingly parallel but separate ways until a joint Conference in 1972[8]), and contained (almost) identical programs with different names and numerical techniques and programs with similar names but very different models. Because the nonmetric procedures were all iterative they were therefore susceptible to local minima solutions: how was a poor user to decide how close s/he was to a global minimum or choose between programs? It was not until 1973 that the answer came – at least for the basic model (Lingoes and Roskam 1973), and fortunately the best combination of procedures was implemented in the Roskam programs we were using. Having grasped the main differentiating characteristics of the plethora of programs we launched MDSX Project[9]. The purpose was :

- to produce a library of stand-alone programs from a variety of sources
- to substitute where relevant the latest more accurate numerical estimation procedures (such as eigen-routines) and rewrite Fortran code as necessary
- to provide a common SPSS-like command structure (to replace the "1 in col. 6 is non-metric" Fortran convention, then all but universal)
- to document the programs in a common style ,
- to write a utility program to allow control of input/output formats and calculate appropriate measures of dis/similarity. (This had the whimsical acronym: WOMBATS – "work out measures before attempting to scale").

It was maintained and distributed by the Program Library Unit in Edinburgh.

It is worth adding parenthetically that a survey in the early '70s of computing resources in the social sciences found that the most common activity was taking data in one format and re-formatting it to the requirements of another program. I suspect that despite improvements, that may well still be true!

It is worth reminding ourselves just how tedious and time-consuming such programs were, and how frustrating the "cafeteria" system of batch-processing run was. Fortunately Edinburgh had not only an IBM 360 -50 computer , (which made possible the direct transfer of software packages from North America, unlike many universities pressured into "buying British" by government pressure) but also a parallel System4/75 on which was mounted an interactive time-sharing system [EMAS] – allowing us not only to debug programs online, but also to allow data capture and

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[8] Bell Laboratories/University of Pennsylvania, Advanced Workshop on multidimensional scaling 1972

[9] See www.newmdsx.com . The documenter was Dr PM Davies. The original MS-DOS programs and test data are still available and downloadable from http://www.newmdsx.com/new_version.htm

data-checking of some of our subjects' data. But MDS programs still needed the mainframe for runs, and the a 16x16 data matrix typically took up to 2000 iterations to settle down (and even then it could be a local minimum).

How have matters improved since then?

- Lessening of technical [especially computing] constraints and increase of speed
- Ability to deal with very large data sets
- Development of more appropriate models
- Interactive manipulation of data and fitting models
- Visualization of data

These trends are likely to continue, and have an impact on the teaching of Research Methods. Two briefs examples will illustrate this:

<PERMAP Run and links for Horseshoe {Ord,S1, 0.115} vs {Ratio, Sstress, 0.066}>

A: Ron Heady's PERMAP[10] ("perceptual mapping") is an interactive scaling program with good diagnostic, animation and visualization facilities. In particular, it is an effective solution to the local minima problem, as it permits repeated restarts and potentially huge numbers of iterations can virtually ensure against the possibility of a local minimum. Having obtained a satisfactory solution, diagnostic tools can enable the detection of simple structures and interpretation of the configuration. In this case, we use the average similarity ratings of the standard 16 occupational titles of POOC and inspect its potential unidimensionality

[The classic Horseshoe phenomenon, of a (unidimensional) sequence contorted due to scaling factors into a 2D shape … OR genuine 2D linked clusters?]

<FIGURE 3: INDSCAL interpreted Configuration, IoOP Fig 3.6>

B: Social Networks: UCINET / Netdraw

Classic sociometric networks were often restricted to class-room size, and although Rapoport's Random/biased net models were intended for large networks, there was a definite limit on how large was large. So using network programs as I did for ordinands' friendship, job-information flows through occupational communities (Breiger's: the strength of weak ties) was feasible, but random-walk sampling of hard-to-reach communities like gay men (SIGMA) and of their sexual contact patterns (Klovdahl 1995, Coxon 1995) dealt with numbers impossibly large in earlier years. More recent work in citation and co-authorship however makes it a relatively simple matter to represent (if not to show!) very large nets indeed.

<FIGURE 4: Netdraw : A: GLA-ALL / B:GLA-MC / C: GOWER CONFIG>

[10] http://www.ucs.louisiana.edu/~rbh8900/

Thus problems which were only recently difficult have become increasingly tractable.

3. The Future

But now I turn to the present and anticipated Future, and it is near my closing time. I must therefore turn to more personal musings about the current state of the Research Methods, and to concerns about future training and needs.

It was as late as the 1990s that I was forcefully reminded that my own experience of teaching (and assessing the teaching of) Research Methods (in under- and postgraduate courses, at Summer Schools and as external examiner) was in many ways unrepresentative of the field, over-rosy in its perception and that there really was a major crisis boiling up especially in graduate training in Research Methods . The turning point was acting as a consultant on the original ESRC investigations of the "research skills deficit" which began in the late '80s.

The evidence for the skills capital deficit is strong, compelling and continuing. The incipient decline in research skills identified originally in the ESRC's Horizons & Opportunities report (1987) has been neither halted nor reversed. The lack of appropriately trained researchers is so serious in some areas that it approaches being a crisis. More accurately, it is not so much a crisis in methodology, so much as a crisis in substantive research, caused by the fact that in substantive fields practitioners often do not have the skills to proceed in a methodical manner.

British Universities are still not producing competent social scientists of sufficient quality and in sufficient numbers

- The problem initially arises because students recruited to (or self-selecting for) the social sciences are often weak in mathematical skills and often have an intrinsic antipathy to methodology
- Before research skills can be transmitted, they have to be developed and fostered. But the incipient decline is also evident at the level of Postgraduate intake. Those who have monitored it have noted modest increases in computer literacy (use of word-processing and e-mail, access to the Net, use of word-processing) but a worsening situation in terms of other research skills and technical competence.
- At both the Masters and PhD levels, the quality of training and achievement in quantitative and qualitative methodology is generally felt to be poor (with some notable exceptions) and in some institutions standards are actually declining.
- The problem is also structural and self-reinforcing; expertise is often weak, so the capacity to teach is small; few are taught and fewer encouraged. Many departments are weak in methodological skills, some are actually hostile to them, and there is a well-founded suspicion about whether the training provided actually matches up to what those institutions promise when they

complete the relevant forms!

Re-skilling Ourselves

Current postgraduate training is only the start of the process of change. Those of us who teach research methods were often trained in what is fast becoming an outdated methodology and associated technologies and have had little time or opportunity to re-skill or up-skill. Researchers - and even expert methodologists - feel the urgent need to become competent in areas where there has been rapid innovation or development (such as structural equation modelling, multi-level modelling or visualization areas), and to do so without extensive time-commitment. In brief, there is a need for remedial, re-skilling and new skilling at all levels to bring researchers (and, teachers) "up to speed", for the learning curve is steep. Ideally, all staff who are researchers and/or teaching research methods should be willing to share their own skills and experience and be engaged in Continuing Professional Development in the Methods area.

I am part of that "Baby-boom generation" whose retirement is often pinpointed as the reason for the crisis, I have serious concerns about the shape of the future of Research Methods. But being now free of institutional employment, which might cramp my style! --I feel a special responsibility both to alert my successors to the seriousness of what is happening, but also to go beyond my professional involvement in recent search in this area – the 2005 Demographic Review of the UK Social Sciences, (Mills et al 2006) and ; the ESRC/SFC Scoping Study into Quantitative Methods Capacity Building in Scotland) – and also offer personal opinions about what could ameliorate the situation. In the course of this, the title of this talk will be explained.

1. Inter-disciplinary differences are often under-emphasized. A Biplot representing the demographic profiles (op. cit. p 34) makes this clear:
    a. Management and Business Studies are very different to other social sciences in almost every way
    b. Psychology and Economics take steps to ensure the numeracy skills of their entrants[11] and do not exhibit to the same extent the range of problems seen in other disciplines
    c. Sociology (and to a lesser extent Politics) are the disciplines which exhibit the full range of skills deficit.

<Figure 5: BIPLOT here>

[11] For instance, the Scottish Graduate Programme in Economics (encompassing all Economics departments) runs an annual pre-session course to ensure a common level of numerical and econometric competence of their starting graduates students.

2. Comparison of the content and level of Research Methods syllabi might be an invidious task to undertake but it would be instructive, especially in disciplines like Sociology where there is little consensus over content and even considerable dissension about what constitutes the subject matte. So far is the discipline from a common mind that departments with established research courses often need to lay on remedial courses to bring graduate students from other universities up to a common level, not only of numerical competence, but in Research Methods itself.

3. The Range of Graduate Research Methods courses and specialisms on offer is often "cabinn'd, cribbed, confined", giving little attention (for instance) to Combinatorial and Spatial data analysis (Hubert et al 2001) – Cluster analysis, sequencing/seriation as well as multidimensional scaling and factor analysis – to Social Networks, or indeed to genuinely integrative qualitative-quantitative analysis (Tashakkor & Teddle 2002, Coxon 2005)

4. Not only ... but also: Funding agencies currently appear to think that Masters courses are the only satisfactory form of Research Methods training. Not so; what about:

   - Post-experience Degree/Diploma in Social Research Methods, intended for those wishing to improve their methodological skills and/or prepare for a career in social and policy-related research (KU Brussels provides a fine example of this)
   - Creation of national or regional Summer schools - the experience of a number of other European countries is that a national Summer school can be a valuable, flexible, relevant supplement or alternative to Essex or Michigan International Summer Schools.
   - The opportunities which Essex Summer School provides for training and re-skilling both junior staff and (primarily ESRC-funded) graduates should be an important resource
   - More ad hoc, one-off intensive courses, master classes, and skill-based and/or data-set-based weekend schools, appealing to those in full-time research or who work with data.

But enough ... and it would not be like me to finish in any way but pretentiously; so I quote the last words of Britten's Spring Symphony, from the Elizabethan playwrights Beaumont and Fletcher ...

Which to prolong, God save our King, and send his country peace
And root out treason from the land!
And so, my friends, ... I cease.

BIBLIOGRAPHY

Bernert C (1983) The Career of Causal Analysis in American Sociology, The British Journal of Sociology, 34(2), pp. 230-254

Coombs, CH, H Raiffa, R Thrall (1954) Some General Considerations on Mathematical Models and Measurement Theory, Psychological Review, 61, pp 132-144

Coombs CH (1950) Psychological scaling without a unit of measurement, Psychological Review, 57, pp 147-158

Coxon, APM (2006) The meaning of occupational worlds: talk at "Great Escapes Conference" , University of Aberdeen .

Coxon, APM (1978) Variables, Networks and Meaning, Cardiff: University College Cardiff Press (see www.tonycoxon.com)

Coxon APM (2005)
Integrating Qualitative and Quantitative Data: What Does the User Need?
Forum Qualitative Research, Volume 6, No. 2, Art. 40 – May 2005
http://www.qualitative-research.net/fqs-texte/2-05/05-2-40-e.pdf

Coxon APM (1995) Networks and Sex: some social contexts of Gay Men's Response to HIV/Aids, in R Parker and John Gagnon, eds, Conceiving Sexuality: Approaches to Sex Research in a Postmodern World, London: Routledge, pp 215-234

Coxon, APM & Jones, CL (1978) The Images of Occupational Prestige, London: Macmillan

Coxon, APM & Jones, CL (1979a) Class and Hierarchy, London: Macmillan

Coxon, APM & Jones, CL (1979b) Measurement and Meanings, London: Macmillan

Davies PM, APM Coxon and CL Jones (1986) Images Of Social Stratification: Occupational Structures And Class London: Sage Publications

Goldberg D, CH Coombs (1963) Some applications of unfolding theory to fertility analysis, in Emerging Techniques in Population Research, Proc. 1962 Annual Conf. Millbank Memorial Fund, New York

Hirschi, T and H C Selvin 1967 Delinquency Research New York: Free Press

Kerr, C et al. (1960) Industrialism and Industrial Man, Cambridge, Ma.: Harvard University Press

Klovdahl AS, (1995) Social networks and infectious Disease: HIV/AIDS, Social Networks, 17, pp 163-165

Krantz, D H, RD Luce, R D, Suppes, P, and Tversky, A (1971), Foundations of measurement, Vol I: Additive and polynomial representations, New York: Academic Press

Lazarsfeld, PF (1961) The algebra of dichotomous systems in H. Solomon, ed Studies in Item Analysis and Prediction, Stanford: University Press (pp 111-157)
Lingoes, J C, & Roskam, E E (1973) A mathematical and empirical analysis of two multidimensional scaling algorithms, Psychometrika Monograph Supplement, 38 (4, Part 2,Monograph No 19)

Marsh RM (1971). The Explanation of Occupational Prestige Hierarchies, Social Forces, 50, pp 214-22

Merton RK, Reader GC, Kendall Pl (1957) The Student-physician: Introductory Studies in the Sociology of Medical Education, Harvard, Ma : University Press

Meyer, Walter (2007) The Origins of Finite Mathematics: The Social Science Connection, The College Mathematics Journal, 38(2), pp. 106-118(13)

Mills D, A Jepson, T Coxon, M Easterby-Smith, P Hawkins, J Spencer (2006) Demographic Review of the UK Social Sciences, London: ESRC (downloadable at: http://www.esrc.ac.uk/ESRCInfoCentre/Images/Demographic_Review_tcm6-13872.pdf

Penn, R (1975) Occupational Prestige Hierarchies: A Great Empirical Invariant? Social Forces 54 (2) pp 352-364.

Shepard RN (1966) Metric structures in ordinal data, Journal Mathematical Psychology, 3, pp287-315

Tashakkori A, C Teddle (2002) Handbook of Mixed Methods Social and Behavioral Research, London: Sage Publications

Wegener, B (1987) The illusion of distributive justice, European Journal of Sociology, 3(1), pp 1-13

FIGURE 1

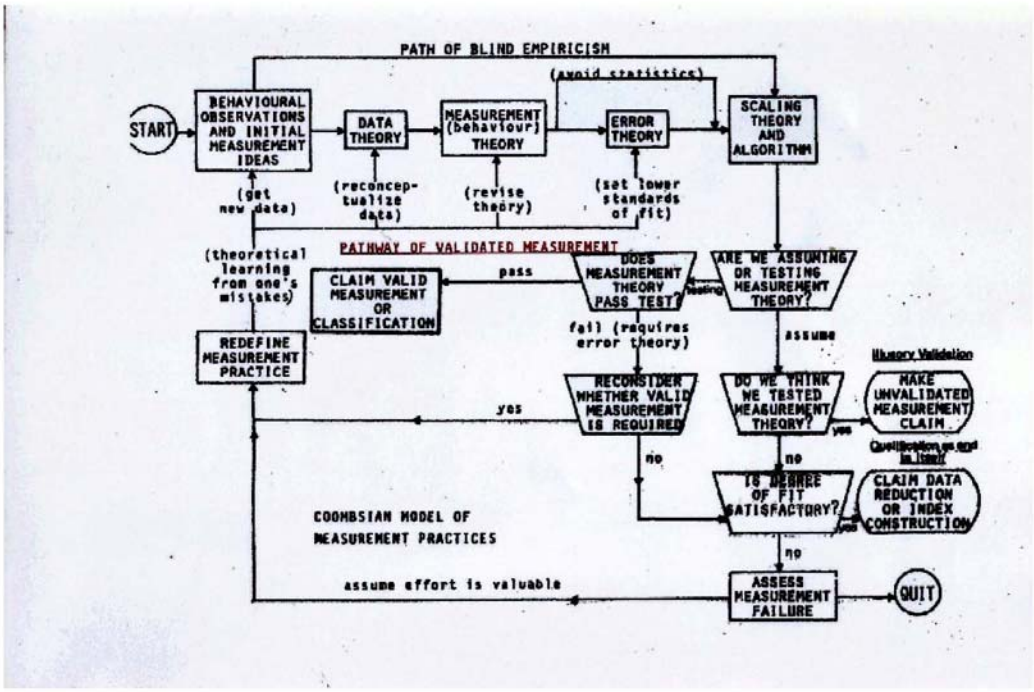


FIGURE 1

FIGURE 2

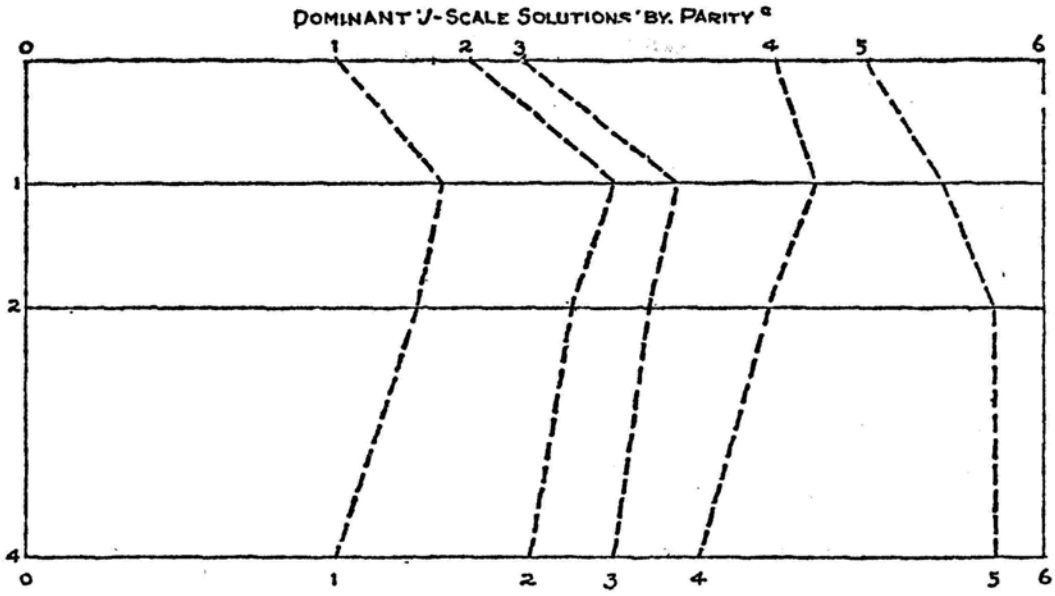


Fig. 5. Dominant *J-scale* solutions by parity.

FIGURE 3

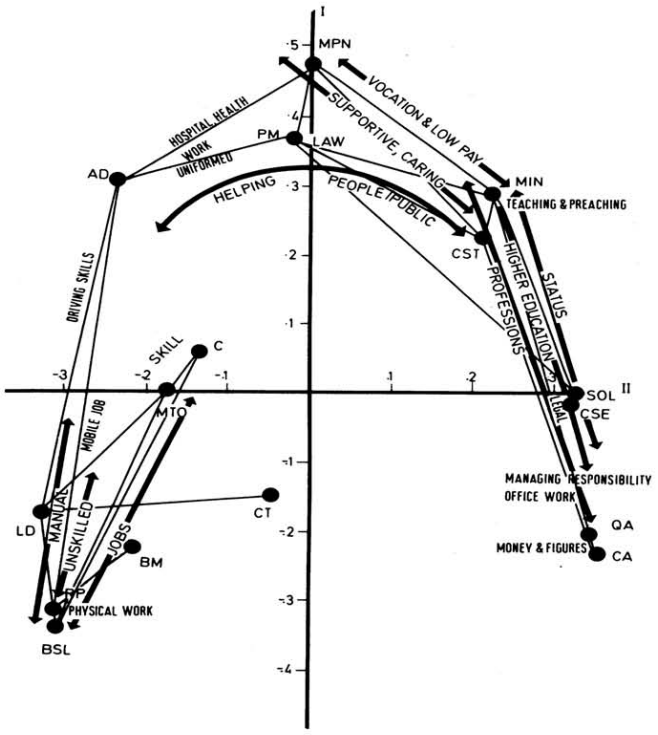


Fig. 3.6 Horseshoe sequence and range of predicates

FIGURE 4A

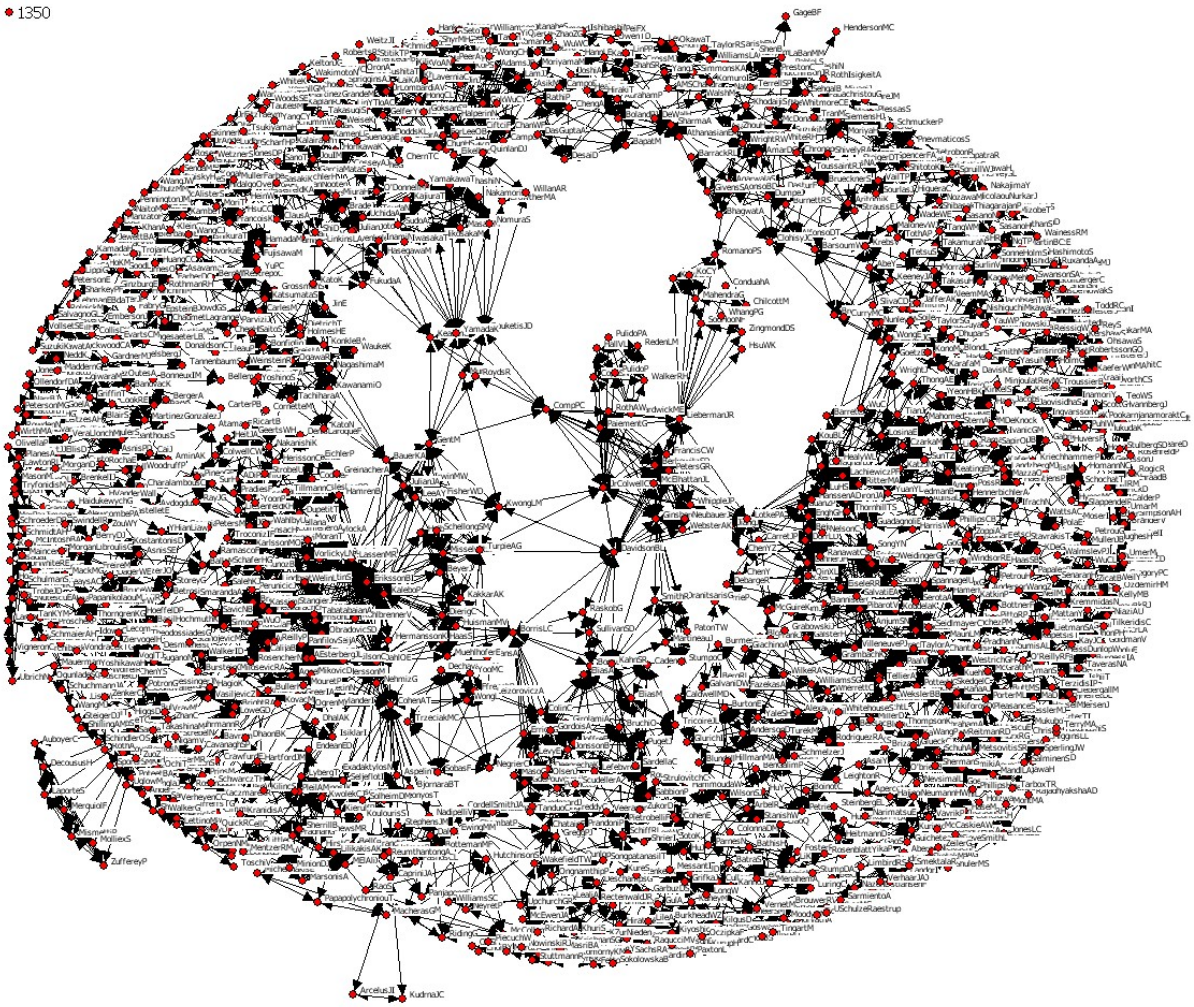


FIGURE 4B

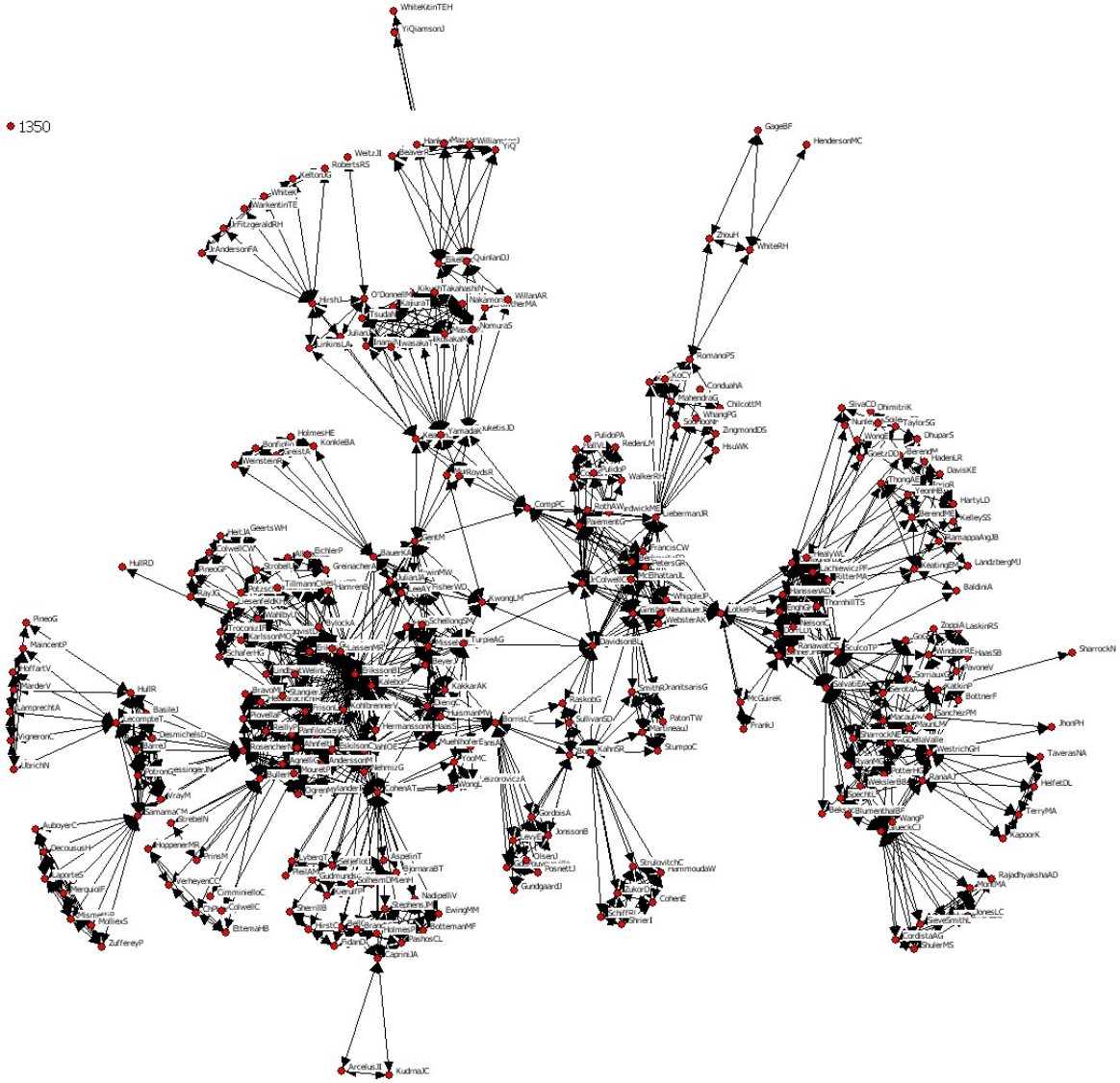
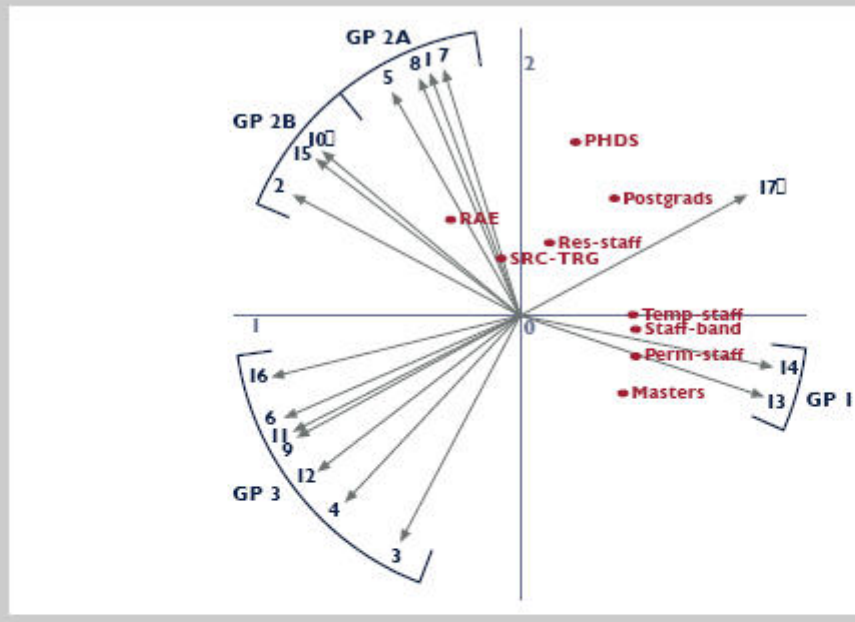


FIGURE 5

Figure 3.6 Two Dimensional Bi-Plot of Discipline Descriptors and Discipline Groups



Group 1	13	Business
	14	Accountancy
	17	Education
Group 2A	7	Anthropology
	1	Economics
	8	Politics
	5	Psychology
Group 2B	10	Geography
	15	Linguistics
	2	Sociology

Group 3	16	Economic History
	6	Social Work
	11	Development
	9	Socio-Legal
	12	Planning
	4	Social Policy
	3	Media