Adventures of an accidental methodologist

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Abstract
This article reflects on the attempt to introduce and extend two innovative methodological considerations: social network analysis and documentary research. Documentary research is widely used but little discussed in methodological literature and I show how my own methodological reflections arose from practical concerns with the use of documents. Social network analysis as a formal technique has been little used and I show how my attempts to popularise it as a methodology also grew out of practical research concerns. These are the adventures of an accidental methodologist.

Keywords
documentary research, social network analysis

Having pioneered the use of social network analysis in British sociology, I was asked to present some reflections at a Methodological Innovations conference on my encounters with this innovative methodology. In reflecting on this, I came to appreciate all-too clearly that I had no intention ever to become a methodologist. I found echoes of this feeling in two other works that inspired my own title.

Peter Berger (2011) recounts in his autobiography that, as a new immigrant to New York from Eastern Europe, and speaking little English, he reasoned that the best way to find out about the society that he had entered was to enrol for a course of study in sociology. He found, in fact, that his sociology course told him nothing at all about American society. Instead, he encountered expansive theoretical reflection from a strongly philosophical basis. To his surprise, he found the study of abstract theory enjoyable and rapidly decided to pursue these sociological interests. By accident he had discovered something that was to take up a significant part of his career.

Garry Runciman’s (1989) reflections on sociological theory, on the other hand, led him to describe himself as a ‘reluctant theorist’. His view was that theorising was neither enjoyable nor especially valuable in its own right but was simply a necessary means to an end. In order to understand the world, he reluctantly concluded, it was necessary to engage in theoretical debate about it.

I share something of each of these views, though in relation to methodology rather than to theory. I found myself engaged in methodological debates by accident, having tried to understand real world problems. My involvement in methodology was also a reluctant one, as I could see methodology as being important – to me at any rate – because it was a means to an end.

Despite this serendipity and reluctance, I have come to be seen by some as a methodologist. This has shaped the invitations that I have received to write and speak on sociological matters and it has helped to determine the direction in which my empirical work has moved. Any sociologist knows that labels are important means of social control and can lead to shifts in identity. This labelling has not shaped my self-perception and so I have never identified myself as a methodologist. I have achieved this through what Peter Berger called role distance, a mechanism through which people are able to avoid the implications of the labels that are attached to them by virtue of their roles. The example given by Berger is the obsequious waiter in a restaurant who manages to convey the impression – at least to himself – that he is not really the servile character that he appears on the surface to be. My ultimate act of role distancing is, perhaps, the delivery of this lecture and its subsequent publication in a methodology journal.

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The beginnings of my methodological interests

I have been heavily involved in two areas of methodological discussion: documentary research and social network analysis. Each of these was a relatively underdeveloped area when I became involved and so much of the work that I did was pioneering and innovative. It is difficult not to be an innovator when there are few competitors about. Because of the lack of competition in these areas my own work was particularly visible to others, and this would not have been the case if I had written on, say, ethnography or survey methods. As a result of my high visibility, I became closely identified with both documentary research and social network analysis as interest in them grew.

Both methodological interests arose during my earliest research. My first academic job was in Scotland during the early 1970s. This was a time when the oil and gas fields of the North Sea were being explored and developed, igniting political debates over devolution and independence with estimates of the wealth generated from the North Sea and the question of whether control over these resources would lie with Scottish interests or English ones.

My own research interests were in social stratification and this interest fused with these political debates for purely contingent reasons. First, I was asked by a colleague to contribute a theoretical chapter to a book that he was editing on social class in Scottish society. This got me thinking about stratification in a Scottish context and I decided to undertake a small study of the economic power of the Scottish upper class. Second, and as a result of this, I was approached by the radical student Rector of Edinburgh University to contribute a chapter on economic power to a book that he was editing and to be called The Red Paper on Scotland. The Rector – one Gordon Brown, later to be a long-serving Labour Chancellor of the Exchequer and a short-serving Prime Minister – brought together a number of then-radicals, including a later Business Secretary Vince Cable, to discuss aspects of Scottish economic and political autonomy (Brown, 1975).

I began a project, working jointly with Mike Hughes, that aimed to investigate the structure of economic power in Scotland through a study of ownership and control in its largest companies. This eventually appeared in a number of papers and a book (Scott and Hughes, 1980). In my later research, I extended this into a study of British society subject to a common focus of control.

In order to investigate these issues, we had to collect some data and the obvious sources were the paper records of directorships, families and companies that were published as yearbooks and stored in archives. The key archive was Companies House in Edinburgh, where annual lists of shareholders are deposited and that can be examined for the names of large shareholders. The principal directories and yearbooks that were used were The Stock Exchange Yearbook and the Directory of Directors, the annual volumes of the biographical reference book Who’s Who, and the genealogical directories Burke’s Peerage and Burke’s Landed Gentry. In addition to these, we consulted the financial press and the gossip columns: at that time, the columns gave more attention to aristocrats and businessmen than they did to TV ‘celebrities’.

Our first step was to collect lists of names of directors and shareholders in the companies that were being studied. We had little idea of what the problems might be and discovered these problems only through doing the research. The major problem was to identify lists of unique names of company directors. It was necessary, for example, to know whether the J. Smith named as a director of Company A was the same person as the John Smith named as a director of Company B. We then had to discern whether either or both of these people were the same as the John B. Smith named as a director of Company C. Beyond this, it was important to recognise that directorships collect data relating to each company’s reporting period and so companies might list their directors on a variety of dates. As people may be awarded titles during the course of a year, it was necessary to see whether any of the J. Smiths we found were to be identified as the Sir John Smith who sat on the board of Company D or the Lord Smith sitting on Company E. Furthermore, any of these, or any of a number of other names, might be identical to the Lord Muck sitting on Company F.

Having compiled a clean list of unique names, we went on to collect biographical information on family, education, and career background. It was immediately apparent that not all of the people that we listed were included in the biographical directories: the directories did not carry a complete census and they may not contain a representative sample. We had to
estimate the extent of any bias among those selected by the editors of the directories. In using the Burke volumes, we had to face the problem that these were not then indexed and so it was a time-consuming matter to trawl through the lengthy entries to see if particular names were included.

The collection of lists of shareholders raised similar problems of identification but also involved further problems because of the practice of using anonymous entries. Although many individuals and families listed shares in their own names, others held their shares through a bank trust company and might appear as one of a number of anonymous bank accounts. Many banks used so-called nominee companies and miscellaneous bank accounts to register the holding of large numbers of individuals. We aimed to identify those accounts that related to particular individuals or companies and to separate them from those that were simply aggregations of small shareholders. This involved a great deal of correspondence with banks and company registrars and a number of visits to the Edinburgh and London financial districts to examine brass plates on company offices.

From the lists that we eventually produced, we were able to generate statistical summaries and trend data. These were produced from manual record cards that were sorted and resorted into piles on the floor carpet. The task became somewhat easier in 1975 when I managed to invest in a first-generation ‘pocket’ calculator. We also produced family trees of prominent families, drawn out on rolls of wallpaper because of the extensive lateral ‘cousinhood’ relations that tied the various families together. Because of limitations on space, we could only ever use sub-sections of these diagrams in our publications.

The problems of collecting and recording the data led me to reflect on the methods that I was using. There was very little in the way of methodological accounts that I could draw on in working through the problems. Apart from some very early work on letters and diaries such as the study by Thomas and Znaniecki, there were just three that seemed directly relevant. Colin Bell had written on the use of Who’s Who as a sampling frame, while Ken Plummer had written on Documents of Life, which he rightly insists is not about ‘documents’ but about all ways of expressing and accounting for life experiences. Most relevant was Jennifer Platt’s two-part summary of approaches to documents, but even this gave only partial coverage of the issues that I had discovered in building my data set.

I decided that I needed to work through the problems myself and in writing down these reflections I produced a book that I hoped others would find useful. In A Matter of Record (Scott, 1991), I compiled a four-element appraisal grid for the assessment of documents on paper, stone, metal, film, audio or digital mediums. This grid identified four sets of questions that must be asked about any piece of documentary data before it can be used in a sociological, or historical, analysis:

1. **Authenticity.** Is the evidence genuine and of unquestionable origin?
2. **Credibility.** Is the evidence free from error and distortion?
3. **Representativeness.** Is the evidence typical of its kind and, if not, is the extent of its untypicality known?
4. **Meaning.** Is the evidence clear and comprehensible?

The lack of competitors ensured that this particular methodological innovation found a ready audience. As a result of the book’s success, I started to become identified as an expert on documentary research methods and was invited to present my ideas at a number of lectures and workshops in Britain and overseas. I was also invited to contribute entries to an Encyclopaedia of Social Research Methods and a Dictionary of Social and Cultural Research, which further reinforced my ‘expert’ status. In 2006, 33 years after the research had begun, I was invited to prepare a landmark reference set of articles running to four volumes that brought together source materials and applications on handling documents in social research (Scott, 2006).

### Social network analysis

Meanwhile, back in 1974, I was trying to find ways of analysing the data that had been collected. Initial analyses could report statistical figures on the distribution of directorships and company shareholdings. We were able to show, for example, that most directors held just one directorship, a significant number held two, and declining numbers held three, four or more. The distribution of the numbers of directorships held, in each year that we studied, was an exponential distribution. However, we wanted to go beyond such statistical data to report directly on the structures of the relations among companies. I was aware of a number of attempts to construct diagrams of company connections. Many of these were what I thought of as ‘spot the capitalist’ diagrams produced by radical publications that gave little indication of the methods they had used to depict the data. Some work that took a more systematic approach had begun to appear in studies by Stanworth and Giddens, Whiteley, and Thomas, but these were limited to very small subsets of companies and while they used the term ‘network analysis’ to refer to their visualisations, they made little use of any formal techniques beyond simple measures of network ‘density’.

My school background in geography and my love of maps convinced me that there must be ways of ‘mapping’ large data sets of 250 companies. I had also become involved with Bob Blackburn and colleagues in a study of occupational stratification that used techniques of multidimensional scaling and this, too, suggested ways of looking at company data.

Although I could begin to draw diagrams on my trusty rolls of wallpaper, I was aware that this involved an arbitrary placement of companies and rapidly dissolved into a thicket of intersecting lines that were impossible to inspect and...
analyse. I began to experiment with other ways of presenting the data.

In the Red Paper and the early articles and book, I used three kinds of diagrams. First, I used simple network diagrams similar to those produced by Stanworth and Giddens. These did, at least, bring out multiple links between companies (shown by multiple lines) and allowed me to position those with multiple links close to each other. This was no innovation on existing methods.

The second kind of diagram I constructed was a circle diagram. In this, companies are placed around the circumference of a circle and the order is rearranged until the number of crossing lines is minimised. This allows the identification of separate clusters and company groupings, but the position of the groupings around the circumference remained arbitrary.

The third kind of diagram was an artificial ‘artist’s impression’ of the network in the form of a bubble diagram. In this kind of diagram, I relied on my assessment of the ways in which companies were grouped together and then used intersecting circles to try to show how these groups overlapped to produce a structure of power in which regional and functional differences in structure could be visualised. While this method gave a clear picture of global network structure for a large data set, it relied on the researcher’s assessment of similarities and differences in patterns of connection.

A step forward could be made only by using radically novel methods. I realised that I had to enquire widely for a solution to my problem. I wrote a short article subtitled ‘A Cry For Help’ that was published in the newsletter of the British Sociological Association (BSA) Quantitative Sociology Group. The response to this, which I summarised in a short report, brought me into contact with other practitioners and real methodologists who had and were using computer software to address structural issues. I had available to me a number of techniques that claimed to identify the important structural properties of networks: how was I now to choose among them?

I knew that if I were to use a technique, I had to understand what it was doing and whether it was a sensible procedure to use for my data. I could not simply take a method for granted and use it just because it produced results: any method might produce results and we need to know whether the results are meaningful. I decided that I had to learn FORTRAN programming so that I could follow through the logic of the algorithms and discover exactly what manipulations were being performed on the data. Poring over lengthy computer printouts and decoding punched cards, I was able to piece together a workable set of solutions for my data and began the analysis.

This part of the research was undertaken in the early 1980s when microcomputing was barely beginning. The software available to me all worked on large mainframe computers. Using some techniques in the GRADAP program that came to be used for a comparative study in which I participated, I prepared my data on punched card, transferred it to large reels of tape, and sent the tape to the Netherlands where Frans Stokman oversaw the analysis of the data on the CDC Cyber computer for which the program had been written. Data runs were long drawn-out processes and it took 2 or 3 weeks before the analyses were returned. The scope for re-analysis and the correcting of mistakes was rather limited. Nevertheless, I had, at last, real structural analyses of my data.

Further analysis, using a technique called blockmodeling, relied on a program written by Clyde Mitchell to implement an algorithm called CONCOR. I had my own copy of this program on a large set of punched cards. It was written in FORTRAN and was far more easily transportable from one computer to another than was the machine-specific GRADAP. However, it was so demanding of computer power that there was just one computer in the United Kingdom capable of running it with the large data set that I had. This was the Cray Supercomputer at Manchester on which my own University at Leicester rented time each week. Whenever I ran the CONCOR program, it used the whole of the Leicester computer allocation for a week. Any small mistake in the data meant that I had to reinput the program the following week and this thoroughly hacked-off the physicists who felt that a sociologist had no right to use a computer.

These techniques, eventually transferred to microcomputers and working far more rapidly in the late 1990s, provided me with a set of concepts: density, centrality, cohesion, cliques and so on, and allowed me to represent the data visually. Having visualised the concepts in simple diagrams, however, it became possible to use the numbers output from the program as shorthand descriptions of social structure. The numbers allowed the researcher to imaginatively visualise the structure of large networks.

Having arrived at a satisfactory way of analysing my data, I decided to write up the results of my methodological investigations so that others could better understand their uses. I produced some early reports and then a short handbook that has now successfully gone through three editions. (Scott, 1992) This success again led me into presenting at numerous lectures and workshops, to contributions to the Social Science Encyclopaedia and the Encyclopedia of the Social and Behavioural Sciences, and to the commissioning of specialist pieces by real experts as Section Editor of the Encyclopedia of Complexity Science. I was also asked to produce a multi-volume edited collection on methods and their applications (Scott, 2002). In cooperation with Peter Carrington and Stan Wasserman, I edited a text on advanced methods of network analysis (Carrington et al., 2005), and with Peter Carrington edited The Sage Handbook of Social Network Analysis (Scott and Carrington, 2011). One particular venture was the invitation to present on network analysis in Graham Crow’s ‘What Is …?’ sessions at the Economic and Social Research Council (ESRC) Festival of Social Science on three occasions, and this led to the production of a primer called What is Social Network Analysis? (Scott, 2012).
Reinventing the wheel

As a methodological innovator in uncharted methodological areas, I have been identified as an expert methodologist. This seems to be the fate of all who are seen as methodological innovators. Recently, there has been much interest in the work of physicists who have claimed to have produced revolutionary innovations in social network analysis. What surprises those of us who have even dabbled in network methodology is that their innovations have a rather familiar feel to them.

Alberto-Lazlo Barabási wrote in 2002 that physicists have been able to highlight phenomena that are ‘unprecedented and unexpected’ in that they have discovered ‘a new and unsuspected order within networks’ and have ‘lifted complex networks out of the jungle of randomness’ (Barabási, 2002). He held that ‘For the first time in history scientists are beginning to learn how to talk meaningfully about the architecture of networks’. Bold methodological innovation indeed! The object of their innovation is the power law: the great discovery that the distribution of connections in a network tends to follow an exponential pattern.

This might sound familiar to readers of this article. I showed that one of my earliest empirical findings was exactly this: 30 years before the great discovery made by the physicists. I plotted my data for a number of years and found exactly the same pattern of the power law. I also examined the findings of a number of other studies in various countries published since 1972, and these all showed the same exponential ‘power law’ distribution.

Physicists would have received a more enthusiastic welcome from social network analysts if instead of ignoring everything that we have done and reinventing the wheel in the name of an innovation they had recognised that they were building on the bedrock of sociological achievements in network analysis. This would have allowed a more rapid, and less begrudging, recognition of their real innovation, which is to try to explain why it is that the exponential distribution occurs in so many networks and to propose dynamic models of network evolution that can show the limits within which ‘six degrees of connection’ and the power law hold.

Future identities?

It is for all of these reasons that I have emphasised that I am not a methodologist. I am not an expert in methodology but simply an enthusiastic amateur who has accidentally and reluctantly got involved in methodology because it helps me to understand the real world. Although I have exercised this role distance, the identification is difficult to escape. I recently received an invitation from a major publisher to edit an eight volume Encyclopaedia of Research Methods. Although the advance was attractive, I decided that I simply did not have the all-round methodological expertise to carry off the task. I can blag my way in social network analysis and documentary research methods but pretending to cover the whole area of social research methods is another matter. Nevertheless, I did subsequently agree to produce a fourth edition of my text on Social Network Analysis, so my accidental adventures continue.

Declaration of conflicting interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship and/or publication of this article.

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Author biography

John Scott has taught at Strathclyde University, Leicester University, Essex University, and Plymouth University. His most recent publications include Conceptualising the Social World (Cambridge University Press, 2011), Envisioning Sociology. Victor Branford, Patrick Geddes, and the Quest for Social Reconstruction (with Ray Bromley, SUNY Press, 2013), and The Palgrave Handbook of Sociology in Britain (Ed. with John Holmwood, Palgrave, 2014).