



ARTICLE

Where is the boundary between sociology and not-sociology?

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Abstract

In work on the history of sociology, how may the boundaries of study be defined, and what requires explanation as part of sociology? Becker's concept of the „art world' suggests thinking in terms of the „sociology world' which is needed to produce the sociological object. Three very different examples - Young and Willmott's Family and Kinship in East London, a cross-disciplinary quantitative sociology study group, and Hodson's use of amateur descriptions of workplaces as data - are discussed, and it is concluded that the practical methodological answer depends on the particular research topic and the resources available.

Keywords

Boundaries between disciplines; sociology, definition of;

Where is the boundary between sociology and not-sociology? This paper addresses the question put in relation to sociology, but it is equally relevant in principle to work on the history of other disciplines. The question is not raised to initiate a discussion of the correct definition of the „discipline". Indeed, a serious effort is made to avoid that concept and the normative weight that it carries; boundary work is not being conducted here. The question addressed is a purely practical empirical one: for the historian of sociology, where does sociology begin and end, and what should be treated as the implications? It is noticeable that different writers make different implicit assumptions on this, most easily seen in whether or not authors from periods sometimes well before the existence of an institutionalised university sociology named as such are described as [really] sociologists *avant la lettre* rather than as predecessors – who may happen to be open to choice of them as ancestors for some parts of modern „sociology", even if there is no traceable causal chain linking them.

A number of writers have done work which, explicitly or implicitly, raises questions about where to draw the boundaries of „sociology" when writing on its history. Two authors who have raised the issue explicitly are Howard S. Becker and Jean-Michel Chapoulie. Becker's concept of the „art world" has had considerable currency, and has an obvious potential applicability to other „worlds". What he does is to include in the account of artistic production all the cooperative activities

necessary to the appearance of the final art work which are carried out by others than the „artist“: „Works of art, from this point of view, are ... joint products of all the people who cooperate via an art world's characteristic conventions to bring works like that into existence“ (Becker 2008: 35). He goes on to examine what the roles and conventions are for „art“, and clearly we can usefully emulate this for „sociology“. More recently, Chapoulie (2009) has advocated a much wider approach to the definition of the discipline in work on its history, one less confined to what is formally labelled as „sociology“, or the abandonment of „the discipline“ as the unit of study. He sees implicit hypotheses of „conventional history“, in which excessive personal familiarity with the field sets the trap of presentist approaches and disregard of causal factors not internal to the discipline as now defined, and sees the solution in a focus on the activities of the going concern(s) which produce sociological work rather than just on their products.¹

In this paper we look at some examples of sociological work which raise further questions about what may be involved in attempting to define the boundaries of sociology as an object of historical study. These examples have no claim to be representative; all that is claimed is that they show some ways in which the boundaries drawn around „sociology“ may seem to become questionable in historical work. The particular cases used were selected – or selected me - because they were ones which drew to my attention the issue of boundaries when I was working on them for other purposes. Additional examples would no doubt draw attention to further aspects of the issues, while in other cases those might be much less salient. One could envisage useful development of a typology of empirical situations, and research styles, where some work was and other work was not satisfactorily contained within disciplinary boundaries.

Considerable documentation is available on the book we look at first, so that we can learn a lot about its „sociology world“ – by no means all of which was „sociological“. This demonstrates some of the possibilities following from the location of research in its wider setting.

Michael Young and Peter Willmott, *Family and Kinship in East London (FKEL)*, 1957

This book's enormous success means that sales must have been made to many members of the general British public; at its publication academic sociology in Britain had not yet started its great expansion. Although the British Sociological Association had been founded in 1951, at its early conferences many invited speakers were still not sociologists but practitioners in areas of social policy. FKEL was concerned, in ways which fitted well into that context, with policies on social planning.

Stuart Laing (1986: 31) provides a useful summary of the cultural situation in the late 1950s, going somewhat beyond the research community:

(T)here was ... a body of work which in defining itself as sociological analysis of working-class life did present its material, in part, in a descriptive, evocative and experiential form – as, in effect, a kind of realist writing. At one end of the spectrum this shaded off into more “rigorous” quantitative and tabular modes of research and presentation, while at the other it gradually merged into documentary, personal reportage and semi-fictional narrative.

¹ For further discussion of his arguments, see the comments in the same issue, and his reply.

Young can in part be placed in that framework, though it does not take account of his very active policy concerns. He stood at the point of intersection of that genre's world and the policy world of bourgeois and intellectual philanthropy.²

We start with Michael Young's own life, and work outwards from that. Young was the lead author; comparable detail for his co-author Willmott is not attempted.³ Young was a remarkable social entrepreneur, and his academic life, on which we focus, was only part of his range of activities.⁴ Born in 1915, he spent his childhood in a very disrupted family life until, in 1929, he was sent to the pioneering progressive Dartington Hall School, set up by Leonard and Dorothy Elmhirst, a wealthy and philanthropic couple, who almost adopted him (Briggs 2001: 17). They founded the Elmgrant Trust, which made a significant contribution to the funding of FKEL. Their milieu was a privileged and cosmopolitan one; Young was, for instance, at the age of 15 taken by them to stay at President Roosevelt's White House (Briggs 2001: 11). On leaving school he joined a London firm of solicitors, and lived for a time in 1933-5 at Toynbee Hall settlement. He started going to the London School of Economics [LSE] as an occasional student, and then took a BSc (Econ) there - without any classes in sociology or anthropology - graduating with a 2.1 in 1938⁵; he remained in touch with LSE until the outbreak of war. He was called to the bar as a lawyer in 1939. He then consulted Max Nicholson⁶ of Political and Economic Planning (PEP) about what to do next, was given the opportunity to write a piece for them, and as a result was given a job there (Briggs 2001: 49). PEP was a social-scientific think tank involving civil servants, businessmen and politicians as well as academics; it produced influential reports applying social science to current policy issues (which included evidence to the hugely important Beveridge Report, basis of the post-war welfare state), and was an important networking site (where the formation of the British Sociological Association appears to have been initiated). In 1945 he moved to the Research Department of the Labour Party, making a major contribution to its manifesto for the 1945 election, which it won; he remained there until 1951. Before 1950 he had started a doctoral thesis at LSE, under Harold Laski (a long-term activist in the Labour Party, and its chairman in 1945-6), on how parties operated at the local level. In 1951 he switched to Richard Titmuss, who had just become Professor of Social Administration, as supervisor, and to the family as topic area (Briggs 2001: 83, 107); the thesis was submitted in

² Topalov (2003), analysing the background of comparable studies done in Britain (FKEL), France, and the USA, argues that their authors were marginal to academic sociology, which gave little attention to the issues they raised, and argued with the policy world of planning. This helped to account for the novelty of their contributions, redefining "slums" as "traditional ['traditional' because they were disappearing] working-class neighbourhoods."

³ Peter Willmott came from a humbler background, starting out as an engineering apprentice and finding his way to Ruskin College (an adult education college, based in Oxford, for people who had not had the earlier opportunity to gain formal qualifications). Young had later recruited him to the Labour Party Research Department after he wrote in response to a pamphlet by Young.

⁴ For other aspects, see Briggs (2001)

⁵ In the same list of graduates is the name of Maurice Ash, who had become a good friend of Young's and had another remarkable career entangled with his, using his substantial inherited money to support favoured causes. He became chair of the Town and Country Planning Association, and at his stately home ran a farm on Steiner principles and housed a small Buddhist-oriented community. Through Young he met the Elmhirsts' daughter Ruth, and married her; he eventually became a Dartington trustee alongside Young.

The name of Raymond Goodman also appears on the same list (with a 2.2). He was Director of PEP for 1946-53, and for 1951-3 acted as the first Secretary of the British Sociological Association (BSA). Perhaps there would be some mileage in studying a cohort from this period.

⁶ Who was also, among other things, founder of the World Wildlife Fund and of the Nature Conservancy (of which he was Director from 1952).

1954. In 1953 he had set up the Institute of Community Studies (ICS) a research unit whose first publication was FKEL (with a foreword by Titmuss). The title of the doctoral thesis is „A study of the extended family in East London” and, though not cited in the book, it clearly was, or became, part of it.⁷

The ICS was based in Bethnal Green, the area of its first research. (Initially it was in Oxford House settlement premises there, until it got its own base, though that was next door to University House settlement.) It was seen as created as a „sister organisation” to the Tavistock Clinic⁸, where Young had spent a year (Briggs 2001: 82). Its aim was declared to be „to study the relationship between the social services and working-class family life“ (Young and Willmott 1961: 203), and it became prolific in the production of studies, some of them local, though many also going beyond the topic area originally defined. There was a clear policy focus:

We were young and naive enough to believe that if we could report, in a convincing way, on the needs and hopes of Labour supporters, even if only in one working-class district, it would help to bridge the gap ... with the leadership. (Young 2000)

THE BOOK AND ITS TEAM

FKEL, published in Britain in 1957, went through many editions and became enormously influential and well-known. To sketch an outline of its „sociology world“ it is useful to start from its acknowledgments. It is clear that there are some conventions about what may be noticed in acknowledgments, so in that sense some methodological caution in how much one infers from them is appropriate. However, what appears there may both reflect social patterns in the research work done, and in its less obvious names do something to define a personal milieu. Listed are people who had made at least five different kinds of contribution: funding sources, academics who gave support or methodological advice, academics who had shared research experience of the same area or topic, university support staff, research unit employees. Looking at these, we can see something of the construction of the „world“ which had characterised and generated this book. But there are also some people mentioned for undefined reasons; the implicit rules of acknowledgment politeness mean that their contributions may have been anywhere from negligible to significant.

People mentioned in FKEL’s acknowledgments, in order of appearance

	Thanked for
Edward Shils, Richard Titmuss	Encouragement and guidance
Philip Barbour	Statistical work
Peggie Shipway	Interviewing

⁷ Various sources mention the relation between them in ways which do not clarify this. Many years later, Young himself said of FKEL that “The first draft of it was my own PhD thesis with Professor Titmuss, and the second draft was the one I did with Peter Willmott, and the third draft was the one I did with Peter [Townsend]... the quantitative stuff ... wasn’t very well brought in in my original PhD thesis” (Young and Thompson 2004).

⁸ The Tavistock had started as a psychiatric clinic, but by the end of World War II was more socially oriented, and conducted work on ‘human relations’ which made important empirical contributions to industrial sociology.



Peter Townsend, Peter Marris	Discussion
Daphne Chandler	Typing
Margot Jeffreys, Ann Cartwright	Advice on design of main survey
John Mandeville	Provision of Hollerith machines
Alan Stuart	Advice on tests of significance
Ruth Glass, James Robb	Access to their data on Bethnal Green
Raymond Firth	Information on his study of kinship
Bott, Bowlby, Brome, Cooper-Willis, Donnison, Elmhirst, Madge, Moser, Peterson, Sheldon, Sparrow, Spencer, Stirling, Wilkins, [Phyllis] Willmott	Unspecified advice.

Some notes on who these people were, with special attention to features relevant to the later discussion:

Those thanked for specific contributions

Edward Shils – prominent US sociologist, who spent much of 1940-45 in London on war work, and for 1946-50 held a readership at LSE in parallel with his Chicago professorship.

Richard Titmuss – first professor of Social Administration at LSE, very influential on policy in the Labour government’s creation of the post-war welfare state; he held no formal HE qualifications, but was charismatic and well respected for his important publications.

Philip Barbour: a conscientious objector and proponent of world government, who joined ICS as its statistician and treasurer after an LSE sociology degree (Barbour 2010).

Peggie Shipway [no information found]

Peter Townsend – founding colleague at ICS. He had Cambridge BAs in philosophy and psychology and in anthropology, then spent a postgraduate year in Berlin studying sociology in 1951-2. He appears in the programme of the 1953 first conference of the BSA as Secretary of the Social Policy Group of PEP, where he worked for 1952-4.

Peter Marris – early ICS colleague; BA in philosophy and psychology; after National Service in Japan, he spent two years in Kenya in the Colonial Service.

Daphne Chandler [no information found – secretarial background assumed]

Margot Jefferys: like Young, she had a 1938 BSc Econ. (described on early BSA membership lists as in Economic History). By 1960 her post was „lecturer in social science“ at the London School of Hygiene and Tropical Medicine, with „sociology of medicine“ listed as her field; she is regarded as a pioneering leader in medical sociology.



Ann Cartwright: had a PhD in Statistics, and before joining ICS was a lecturer in Social Medicine at Edinburgh University.

John Mandeville: started a career in the Navy, somewhere along the line gained an A.M.I.Mech.E, moved into work in the early diffusion of punched-card machines and so became an authority on the machine analysis of market research, worked in various ministries during World War II, and then set up as an independent consultant on punched-card systems (Mandeville 1946: 121).

Alan Stuart: spent his whole career as an LSE statistician, with survey sample theory as one of his main areas.

Ruth Glass: held a post at University College London; Glass (1939) was a well-regarded study of a housing estate, and she continued to work on urban planning and housing-related topics. When Young and Willmott started work she had already collected data on Bethnal Green (Glass and Frenkel 1946), not all published. In 1947-8 she had been a research officer at PEP. She was the wife of David V. Glass, who became professor of sociology at LSE; nepotism regulations prevented her to having a job there.

James Robb: New Zealander working in London on a LSE PhD in social psychology, collecting data in Bethnal Green; at the same time he worked on a Tavistock project on marital casework. His background then was in psychology and social work more than sociology, though he became a founding figure in NZ sociology on his return, and carried out a number of community studies there.

Raymond Firth: of New Zealand origins, from 1933 from lecturer to professor of anthropology at LSE, where he had obtained his doctorate; had already carried out a pioneering study of kinship in London (1956). His archived papers at LSE contain work in progress from Bott, and from Young with Shipway reporting on pilot interviews.

Those thanked for unspecified contributions:

Elizabeth Bott: Canadian social anthropologist working in London, who subsequently became a psychoanalyst; her project for the Tavistock Institute of Human Relations on families and their networks had several publications before her famous 1957 book on it. She worked on the same project as Robb.

John Bowlby: a psychologist/psychiatrist/psychoanalyst with a special interest in child development, and head of the children's department at the Tavistock Clinic.

Vincent Brome: a writer; had also worked at the Labour Party Research Department (LPRD).

Euan Cooper-Willis: economist/banker; his wife was a ceramic designer who had studied at Dartington College of Arts, and they ran the Portmeirion pottery company. It is evident from Young's papers, archived at Churchill College Cambridge,⁹ that Cooper-Willis had by the late 1960s for some time had a job in banking, and held responsibility for looking after the ICS' investments, buying and selling shares for them. It sounds as though he may also have been a personal friend.

⁹ YUNG 5/14



David Donnison: from 1956 a professor of Social Administration at LSE; a strong Labour and welfare state supporter who worked on housing issues.

Leonard Elmhirst: from a landed gentry family, and by Cornell training an agronomist; he had a long concern with India and its rural problems. He and his wife Dorothy, who held a large family fortune, founded the Dartington estate to attempt to put their ideals into practice.

Charles Madge: Professor of sociology at Birmingham, though without any degree in the field; a founder of Mass Observation, and had worked with Young at PEP in 1943.

Claus Moser: a leading member of the LSE Statistics department, whose survey methods textbook (1958) was for many years widely used by sociologists.

John Peterson [no firm information found– but probably he is the Petersen mentioned in Robb's preface as on the staff of the settlement University House in Bethnal Green].

J. H. Sheldon: physician with a special interest in geriatric medicine, and author of a well-received 1948 book based on a survey on the social medicine of old age, including the relevance of housing and support from neighbours and family.

John Sparrow: [Presumed not to be the one of the same name who became Warden of All Souls College at Oxford.] BSc Econ. specialising in accountancy and finance, LSE, 1954, then employed in a chartered accountancy firm. His later career was of great distinction, in accountancy and merchant banking, with several public functions, a knighthood in 1984, and vice-chairmanship of the Governors of LSE.

John Spencer: studied „social sciences“ [at that period this was in effect social policy/social work] at LSE before the war; on return from war service he became a lecturer in „social science“ there, and gained a PhD with a thesis on the effect of military service on crime. In 1953 he left LSE for a project in Bristol which involved community organisation and group work on an urban housing estate (Sinclair 1979).

Paul Stirling: anthropologist, who as an LSE research student started fieldwork in a Turkish village from 1949, including the collection of formal household data in 1950.

Leslie Wilkins: on leaving school he started a social-work course at the University of Southampton to train as a probation officer, but could only afford evening classes, while working as a clerk in the Ministry of Labour. He worked his way up, via wartime operational research, to become a Fellow of the Royal Statistical Society and work for the government Social Survey, becoming widely recognised as an expert on aspects of survey method and their application to policy issues.¹⁰

Phyllis Willmott: Peter Willmott's wife. She had qualified as a social worker, but came to play an active but informal role in ICS research through living with their children over the Bethnal Green offices. Young (2000) says that she „became the ethnographer in chief, while Peter and I interviewed random samples.“

¹⁰ For much more on his fascinating career, see Wilkins (1999).

We note the conventional division of labour between such roles as secretaries who type, interviewers who interview, statisticians who do tests¹¹, and the more diffuse or informal academic ones included. The former roles are important ones, often consequential for such matters as the distance between the authors of the publications and their data, but may not be mentioned at all in the published presentation of the research.

It is striking how few of those listed were in any formal sense „sociologists”, even if it might seem that the character of their work gave them a plausible claim to that honorific title. Of those listed for reasons other than typing, interviewing or provision of machines, a maximum of six (24%) have good claims to be regarded as „sociologists“ by discipline, if sometimes ones of recent vintage. Some of this can be imputed to the stage of development of British academic sociology, where very few formal posts yet existed. Books by some authors from psychology, anthropology and geography were generally used as sociology.¹² Many people then whose careers started in other fields later held posts in sociology, a few changing affiliation as a result of work for ICS – but that does not seem to account for many of these, a majority of whom are known to have continued in other fields, even if those are ones like social administration, social psychology or anthropology which can be seen as very close. There is an emphasis on help which is methodological, or based on sharing data; those with related data could be unusual in their disciplinary community in having interest in relevant fields, and it is clear that there was widespread interest in policy-related research themes where practical problems could override disciplinary conventions.¹³

We can see how many other people played roles, if not all essential ones, in the production of the book we have focused on – and that is without starting to look at anything specific to the process of publication and diffusion. FKEL, like many other social science monographs of the time, was published by Routledge and Kegan Paul in the „Library of Sociology and Social Reconstruction“ series initiated under Karl Mannheim¹⁴, although ICS was identified there as a somewhat separate subgroup. It is interesting to see, from the Routledge archives at the University of Reading, that the publisher did not anticipate good sales!¹⁵ The deliberate effort made to write in a way accessible to the intelligent general reader must have helped. The reception of the book is not covered here, but the picture could usefully be extended in that direction.

¹¹ It is not known precisely what Barbour's role was in relation to the book, but he was employed as ICS in-house statistician; given that a research statistician is also mentioned it seems possible that his role was to apply the tests recommended by Stuart.

¹² This also went in the other direction. Monchaux and Keir's (1961: 158-9) review of British psychology from 1945 to 1957 includes FKEL and Townsend (1957) as contributors to the field, and goes on to say how hard it is to draw a dividing line between sociological and social-psychological studies.

¹³ It is interesting to contrast these acknowledgments with those in books from the University of Liverpool's Department of Social Science which were published at much the same time. This department in the 1950s had a distinctive pattern of organisation. On the one hand, it had permanent academic posts committed to empirical research, and on the other hand it had a commitment to work on local social problems, with the knowledge and consent of those researched. Collective expertise and local knowledge were developed with experience, so that external advice was not required as much as in other situations. Staff often worked cooperatively, so that formal authorship could be relatively arbitrary; some of the books have no individual author on the cover, but a list of those members who participated in various ways is given inside. The acknowledgments made are commonly to representatives of their subjects, both management and trade unions where applicable, while the penumbra of miscellaneous contacts and sources of advice is missing.

¹⁴ On which see Platt 2014.

¹⁵ The understanding that the authors would order 'a very large number of copies' themselves (which seems to have been a fairly common practice at the time) may have done something to tip the decision (RKP A131).

THE SETTING

Some of the social connections involved in Young's trajectory can now be sketched in. The Elmhirst connection was of overwhelming importance, in places not all of which are the obvious ones: the solicitors Young worked for were used by Dartington; Leonard Elmhirst was a founding member of PEP, and its chair 1939-53; Max Nicholson had strong Dartington connections; a grant from Elmgrant, a foundation set up by the Elmhirsts, made it possible to set up the Institute for Community Studies and produce FKEL.¹⁶

A second funding source was Edward Shils, who taught at LSE from 1946-1950. He held a Ford Foundation grant, which he could use in any way he wanted, and some of this was used to support Young's project. In effect he became a protégé of Shils. (Later, Young held his own Ford grant; this introduction may have helped in that.)¹⁷ As a doctoral candidate he attended some of Shils' seminars in 1948-9, when he offered courses on „Primary Groups in the Social Structure“, „Social Structure“, and „Sociological Research“. Shils also gave informal „tutorials“ at ICS in its first year (Willmott 1985: 146-7). Briggs (2001: 84) says that they kept in touch when Shils returned to Chicago in 1950.¹⁸

The LSE context more generally was obviously also important. LSE had a group of statisticians who focused on social science concerns, especially aspects of survey method. They were certainly not sociologists, but some of them, especially Moser (professor of Social Statistics), produced work which was effectively incorporated into sociology. There was also a Research Techniques Unit to advise on methods, making another point where it might be worth tracking back in the sequence of events. (FKEL only provides simple tables in the main text, with significance tests in an appendix; Young's thesis, based on fewer than a hundred cases, had nothing more sophisticated than percentages.)

Overlapping with LSE as a relevant node in the network was the Labour Party. LSE was very prominent in the formulation of the agenda of the 1945 Labour government, which was creating the welfare state.¹⁹ William Beveridge, director of LSE from 1919-37, was the author of the 1942 Beveridge Report which set out the proposals on which it was based, though he was not a Labour Party member. However LSE staff member Harold Laski chaired the National Executive

¹⁶ The research which became Benney et al. (1956) was also funded by a 1948 grant from Elmgrant, supervised by a committee which included Young; Benney (1966) said it was inspired by Shils. Other empirical work on rural areas (e.g. Saville 1957, Williams 1963) - much closer to the issues local to the Dartington estate - was commissioned and funded by its Trustees, and appeared in Dartington Hall Studies in Rural Sociology, also published by Routledge Kegan Paul.

¹⁷ Briggs (2001: 131) reports that projects listed in the original proposal for ICS included one with financial support from Shils, who wanted to do it himself, on Oxbridge and other universities.

¹⁸ A well-known joint article (Shils and Young) was published in 1952. For this Young 'conducted some interviews with residents of the East End. With these data, information from newspapers, and my reading on monarchies, coronations and similar ceremonies we wrote an essay...' (Shils 2006: 87) This article became notorious in England when later seen as a right-wing justification of the monarchy.

¹⁹ Its prime minister, Clement Attlee, taught 'social administration' at LSE until he was elected to Parliament in 1922, and before that was associated with settlements, including Toynbee Hall.

Committee of the Labour Party for 1945-6, and Richard Titmuss and colleagues were deeply involved in the agenda of the welfare state.²⁰

Labour ideology centred on the working class²¹, and that focus was very much in the Anglo-American tradition of policy-related social research. ICS's location in Bethnal Green was closely consistent with the settlement-house pattern which in the US (e.g. Hull-House) and Britain had been associated with that concern.²² There was considerable policy-related research on connected topics going on among the working class in London at the time, often with a psychological rather than a sociological problematic, though Rorschach tests and concern with personality were sometimes supplemented with participant observation of a kind now more often associated with sociology.²³ Some of the main books based on it are these:

- Robb (1954): in the Preface Robb, a New Zealander, declares his greatest debt to be to Shils' help and encouragement while he was carrying out the fieldwork; acknowledgments are also made to Bott and John Spencer among others. The book was based on his doctoral thesis.
- Spinley (1953): this was a doctoral thesis in Psychology, and she too was from New Zealand. The slum area she studied was in London, but outside the East End. (Her slum group was compared with a public-school one.) The preface names Robb as a friend who read her first draft.
- Bott (1957): Bott belonged to a research team which also included Robb (who returned home to New Zealand in 1954, so his contribution is less salient in the book than it might otherwise have been); the acknowledgments include Firth, and Young and his ICS colleagues, as well as many others, especially anthropologists. In 1949-51 she held an assistant lectureship in anthropology at LSE.
- Jephcott, Seear and Smith (1962 - publication was delayed by problems): Jephcott, born in 1900, had a Wales BA in History, and in 1946-8 was a PEP research officer. In 1954 she was appointed as a research assistant in Titmuss' Social Administration department, after considerable earlier work organising and researching working-class girls' clubs, and worked with John Smith, an LSE graduate and later a professor of sociology.

It is clear from the repeated recurrence of some of the same names that this was quite a small-world research community, with mutual support and exchange of ideas across disciplinary lines.²⁴ (Smith told me that there was considerable interaction among the researchers working in East London at the time.) The same names of institutions - LSE, PEP, LPRD, Dartington, Tavistock Clinic - and of individuals (Shils, Titmuss, Glass, Robb, Bott) appear, as has been shown, in several contexts, so that their work needs to be treated explicitly as a product of specific social relationships, not all of them within the gates of „sociology“.

²⁰ By 1957 Labour had lost power, and government policies differed. It has been argued elsewhere (Platt 1971: 139-140) that the Institute's somewhat romantic picture of Bethnal Green can be seen as a counter-Utopia, presenting an alternative to the new policies' model.

²¹ David Glass, in his foreword to the famous LSE collective work *Social Mobility in Britain* (1954: 3), declared that its focus was on middle-class groups because so much had traditionally concentrated on the working class.

²² It could be of some interest to consider how far ICS might be regarded as a modern settlement.

²³ Another book which in many ways seems to belong in the same set is Kerr (1958), a study of personality development in a slum, but this slum was in Liverpool and the author acknowledges Liverpool assistance.

²⁴ The list above is sufficient to draw attention to the legacy of empire, on which Steinmetz (2014) has written very usefully, and the social categories of exile/refugee and Jew, not developed here, were also a salient part of the social setting.

For someone for many years commonly regarded as a sociologist, surprisingly little of Young's activity was formally sociological; his BSc was not in Sociology, his doctorate was in „Social Administration“, and he drew on anthropology rather than sociology for ways of approaching the family. He taught sociology named as such for only three years, as a lecturer at Cambridge in 1961-3, and this did not seem to suit him well. His interest was always in policy applications, and his writing was addressed to the general reader, or such groups as teachers, social workers and town planners, not just academics. Much of his institution-building, such as the founding of the Consumers' Association, was outside academia. Was Young „really“ a sociologist? Perhaps we might apply to him a comment made by Marshall in an obituary appreciation of Titmuss: „we may begin by asking whether Titmuss was a sociologist. The question is permissible provided one does not demand a 'yes' or 'no' answer.“ (Marshall 1973: 137). Whatever his personal identity, Young's published works have certainly been used as sociology.

A brief look at the career path of another prominent British „sociologist“ – chosen for contrast, admittedly – whose prominence has overlapped in time with Young's, is enough, even with much less data, to show how different a prominent career trajectory can be even under many of the same environmental conditions. John H. Goldthorpe's UCL BA was in history, but he rapidly moved into sociology. After some years at Leicester and Cambridge universities he has for many years been at Nuffield College Oxford; he works in a highly quantitative style on empirical, theoretical and methodological aspects of social stratification. Normative judgments and policy relevance have not been absent, but it is clear that his central involvement is with the academic and disciplinary communities. There is a network of connections among individuals with whom he has done cooperative work (e.g. Breen, Erikson, Whelan), who are connected with a journal of which he was one of the founders (the *European Sociological Review*²⁵), whose departments belong to an association of others with similar styles and resources (the European Consortium for Sociological Research²⁶), who have been active in an important international group working on issues of social stratification (Research Committee 28 of the International Sociological Association), who cite and are cited by his publications, and who have been involved in the research groups associated with an EU „Network of Excellence“ („Equalsoc“) with a research group on educational inequality. Numbers of the people involved with these groups have also been student, staff, or visitor at Nuffield College or other parts of Oxford University, and probably also at appropriate institutions in northern Europe. There can be no doubt that Goldthorpe is now a sociologist, though he dissociates himself from much of contemporary British sociology. (The sophisticated level of quantification in his publications cuts many British sociologists off from the possibility of serious engagement with them, and his writing is not aimed at the general reader.) The intellectual „world“ he has chosen, indeed to some extent created, coexists with sometimes overlapping and sometimes distinct other worlds for sociologists. It is obvious that the boundaries of his and Young's worlds have not been in the same places.

The areas sketched above for Young are ones which could apply to any empirical work if the data were available. They exemplify some directions in which study can be taken in order to explore the

²⁵ Its first issue states its aim to provide a forum for an informal network of European sociologists 'who have in common a commitment to empirical research and an interest in comparative studies, and who have often followed styles of sociological analysis that are distinct both from those characteristic of their own national traditions and from the reigning North American paradigms' (Mayer, Goldthorpe and Ringen 1985: i).

²⁶ This admits as members units which 'have a demonstrated capacity to achieve and maintain high standards in all relevant aspects of the research process.'

relevance of non-disciplinary factors in the creation of „sociology“. To learn about both the wider social context and the relationships involved in the production of FKEL sketched above is surely to understand more of the meaningful history of Young's work. Two other examples below show, in much less detail, less ordinary ways in which not strictly sociological elements may appear in association with sociological work.

The BSA's Quantitative Sociology Group [QSG]

The QSG started in 1967, and aimed „to synthesise statistical, mathematical, computing and substantive aspects of quantitative sociological research“ (*Network* 6: 9, 1976). This was, of course, the period of the emergence of the computer (but not yet the word processor) into practical academic life, and many of its members were active in that.²⁷ There was still excitement about survey method, and within survey work convention made this as much the sphere of social statisticians as of sociologists, though social statisticians were by no means available wherever there was university sociology. The arrival of SPSS, much easier to use for social data than previous computer programs, figured prominently in the lives of some. Among sociologist members survey work bulked large, but network analysis and conversation analysis were also present. In practice, the QSG was strongly interdisciplinary, and cut across other conventional boundaries too by including governmental research workers, survey researchers from market research and other commercial research outfits, and members of university computer centres whose roles were formally in service, rather than research or teaching. The January 1978 issue of the BSA newsletter *Network* said that at the recent annual colloquium of the QSG „At many sessions there were no sociologists present, and where they did attend they were easily outnumbered by physicists and mathematicians.“

This was, thus, despite its formal status, by no means a group confined to „sociologists“. Where had its members come from, and what had become of them when the group folded in the early 1980s? Some suggestive light can be thrown on this by data on its post-holders.²⁸ Only 40% of the jobs they held while in QSG were in Sociology departments; the others were in computing services, social statistics, survey units... (As those became more institutionalised as distinct fields of work, some of the people concerned no longer defined their interests as within sociology.) Few of their first degrees were in sociology – only three of the 19 whose degree was unequivocally identified, one of those in sociology joint with maths. They often came from fields with much stronger quantitative traditions, whether in natural science, mathematics, or partly social-scientific fields such as psychology or geography, so these people did not need to rely on what they were taught about quantitative methods within sociology. Their shift into sociological work was part of the movement of excitement about sociology which led to the great expansion of the late 1960s and early 1970s.²⁹ (Others moved in from fields such as philosophy, and were much more often represented in the Theory study group.) The divisions within „sociology“, documented for the US

²⁷ A division of labour within a team in which one researcher had all the responsibility for computing - at that period, before the development and diffusion of programs such as SPSS, necessarily learned from scratch for the particular project - seemed to encourage obsessive perfectionism, and the production of data in forms of such complexity that they were in effect unusable, as well as incomprehensible for the colleagues who were meant to analyse and write up the material (Platt 1976: 91-92).

²⁸ No complete membership list has been identified.

²⁹ Two members embarked (independently) upon doctorates aiming to mathematize Parsons' theory; perhaps it is not surprising that neither completed a thesis.

by Ennis (1992), set limits to social integration, but in this case the integration cut across several boundaries. During the transitional period of this group, it is far from clear how to describe its members' disciplinary status.

Hodson's set of workplace ethnographies

Sociologist Randy Hodson has collected every case he could find of book-length workplace ethnographies in English with sufficient information on groups of workers for the themes he was interested in to be coded.³⁰ He has assumed that the information provided is adequate data, and used the set of coded material to provide both quantitative and qualitative data for a number of papers in sociological journals. But what is the set of cases that he has chosen? It is a set of books providing information on workplaces, not a set of work by „sociologists“; for a few writers, the data were not at all addressed to an academic constituency. A noticeable proportion of the studies were done by anthropologists, and there is a scattering from other corners of academia; but there is also an interesting subgroup, on which we focus here, by people who were academically entirely amateurs, sometimes writing autobiographically. Does their work that Hodson has used *ipso facto* become „sociology“? or should it perhaps be regarded as having been sociology even before Hodson had noticed it?

If one were, unlike Hodson, interested primarily in the history of academic sociology, arguably the amateurs would have to be omitted – except to the extent, at least, that their work has been retrospectively absorbed into the social science literature. His publications on this set of cases have in effect absorbed them, but is that just one idiosyncratic use? An impression of the extent to which the „amateur“ works have been absorbed into social science was gathered by looking at the first five pages found on some of them in Google Scholar. This is quite suggestive about the range of ways in which such materials may be incorporated, and the factors which can influence the level of attention received. One book received many references, but the largest numbers came from labour history, and from legal scholars, sometimes in association with issues of drinking in the workplace (mentioned in the text, though not its main topic). The book had also been used in teaching of rhetoric/ creative writing/literacy, and as offering data on gender identities. Another book received few social-science references, but appeared used as educational descriptive material for undergraduates rather than as research findings or theorisation to build on. Social-science references to another, written by an active trade unionist, were mostly from sources on class and trade unionism, while others were cited by hobbyists in the substantive fields of the books' topics (railroads, local history) as well as by academics. Thus such studies have been used in academic social science for a variety of purposes – and were probably used quite as much as many single empirical studies by professors. But their potential status as social science is achieved by cooption, rather than volunteering. Are they (perhaps especially if they offer some general explanations for what they report) „really“ sociology, or social science more broadly, rather than merely potential data for it? To find oneself asking such a question is to recognise that it cannot sensibly be answered, since „really“ is a social construction independent of the historian rather than an intellectual matter – though the socially made distinctions are in themselves of historical importance.

³⁰ This is discussed in more detail by Platt, Crothers and Horgan (2013).

Concluding discussion

Two main kinds of not-sociology have been discussed: intellectual material identified by boundary work as belonging to other disciplines or none, and material on factors - a necessary or occasional part of the practical production of any intellectual work - ranging from the availability of data-collection resources to the system of distribution of the results. Various issues emerge from this discussion of examples. How problematic it is to deal with the first kind depends on the extent to which disciplinary boundaries are emphasized; if the question is not contextually raised, one can perhaps ignore it. But in teaching it is hard to avoid the question; then, a tidy artificial presentist boundary can seem to meet the need better than one with trampled barbed wire to trip over, and several metaphorical goats invading from the next field. Serious historical work, however, needs to look at the messy bits – and treat them as messy. There were in fact, for instance, numbers of other studies of new housing estates and their effects, but most of them have been forgotten by sociologists – though some remain noted as part of the literature of town planning, while others which started as town planning were absorbed as „community studies“.³¹ The pattern of retrospective shifts in disciplinary identity has certainly occurred elsewhere. For instance, the volume *Politics, Social Networks and the History of Mass Communications Research* (Simonson 2006) gives a surprise to anyone who thought that *Personal Influence* (Katz and Lazarsfeld 1955) was a classic of empirical sociology; it has become „one key text in the history of mass communication theory“ (Simonson 2006: 9). Has it to be deemed to have left sociology, then? No reason why it should, but the dual claims made on it undermine consideration of what it „really“ is; in this context, at least, it really is whatever it is used as.

There are many ways in which academic work will almost inevitably be inextricably entangled with practical contributions from non-academics. Yes, we type our own papers now (but we don't run the internet), some universities have devolved internal budgetary systems (but their financial resources depend on wider social processes), and a few journals have been run by founding groups whose members voluntarily do the production work themselves without bringing in commercial publishers; to the extent that such conditions hold, some disciplinary boundaries may be maintained. When it comes to such matters as the availability of funding, or the nature of the publication system, there may be no problems of definition; the question is merely whether data on such points should be drawn on in historical description and explanation.³² The answer there is surely that it depends on the extent to which what happened in the particular cases studied was simply the normal pattern of their time and place, though attention may need to be drawn to that for readers not familiar with the general literature on it. Where what happened is more idiosyncratic, it merits more attention. We have shown a number of points at which Young's personal contacts were significant; other researchers have had very different connections, and have made different choices, so the „worlds“ of their books look rather different. The world of „sociology“ can in that sense be composed of numbers of overlapping worlds occupied by different „sociologists“, which make it hard to reach convincing empirical generalisations about the work of a period or a geographical unit – and if their absence is what the data show, so be it. However, there may be varying levels of homogeneity between schools of thought, generations, or sub-fields, and where the sets in the Venn diagrams intersect is of considerable interest in itself.

³¹ We may also note that several authors of such studies changed disciplinary identification to become 'sociologists' as the discipline became established in universities and rapidly expanded.

³² The prior question of whether the relevant research has already been done, however, arises – and quite often it has not. For some discussion of the gaps, see Platt 2013.



It is not always easy to gather data on factors such as those listed for Young; even such a well-documented case has gaps in its account, so that it may not be possible to meet in practice the historical standards to which one adheres in principle. Regrettable as this may be, one can deal with it by choosing where to set the boundaries of one's problems - or circumstances may choose them for us. The salient methodological task may then shift to how to make the best use, without over-interpretation, of the resources that are available. Where the boundaries of social science lie must, thus, in the end depend on the research topic addressed, rather than being an answerable general empirical question for the historian. Thus it becomes an ad hoc methodological question, rather than a substantive one, which is perhaps as it should be.

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