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John H. Goldthorpe
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Introduction

The idea for the present work came to me, perhaps appropriately, on April Fools' Day, 2017, at the conclusion of a meeting in Cologne of the International Sociological Association Research Committee 28 on Social Stratification and Mobility.

Some months previously, I had published a book, *Sociology as a Population Science*, in which my main concern was, as I put it (2016: 2), 'not to propose to sociologists how they should conceive of and practise their subject' but rather 'to suggest a way in which a fuller and more explicit rationale than has hitherto been available might be provided *for what a large and steadily growing number of sociologists already do* – although, perhaps, without a great deal of reflection on the matter'. What I argued was that most of those who shared in the goal of developing sociology as a science were in effect pursuing sociology as a population science, in a sense that I sought to explain. What was essentially involved was studying human populations across time and place by abstracting from the particular histories and attributes of their individual members in all their variability in order to focus on the – probabilistic rather than law-like – regularities in social life that were the properties of these populations themselves.

A first objective was therefore to establish such regularities and to describe them as accurately as possible – to make them *visible*. For this purpose, given the probabilistic nature of the regularities, resort had necessarily to be made to statistically grounded methodologies of both data collection and data analysis. Sociology as a science had to place itself within the 'probabilistic revolution' (Krüger, Daston and Heidelberger, 1987; Krüger, Gigerenzer and Morgan, 1987) that had in fact transformed science in general over the course of the later-nineteenth and earlier-twentieth centuries. But a second objective was then to explain the regularities that it was possible to establish and describe – to make them *transparent*. And doing this meant showing how they were the outcome, intended or unintended, of tendencies not simply in the behaviour of individual members of populations but in their intelligible *action*.

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The one crucial way in which sociology has to distinguish itself from the natural sciences is through a recognition of the – evolved – capacity of human individuals (Dennett, 2003) to act with a significant degree of autonomy and of the implications that follow for ideas of causation in social life.

What was then heartening for me at the Cologne meeting was the number of colleagues who came up to me to say that they recognised my interpretation of what they were in fact doing, and that the idea of sociology as a population science was one that helped them become clearer in their own minds about the aims of their research, and about why they pursued these aims in the ways that they did.[1] As a result of these conversations, I was led to start thinking about the origins of sociology carried out in the way in which ex post I had characterised it, and I came increasingly to the realisation that existing histories of sociology (e.g., Barnes, 1948; Nisbet, 1966; Levine, 1995) were quite inadequate in this regard, being in fact more in the nature of general histories of social thought. In the Cologne airport, waiting for a flight back to London, and in discussion with my colleague, Erzsébet Bukodi, I drew up a preliminary list of those who might be regarded as pioneers of sociology as it subsequently developed into a population science. After much further thought and some – to an extent still persisting – doubts and misgivings, I decided to concentrate on the seventeen individuals who figure in the present book: namely, John Graunt, Edmond Halley, Adolphe Quetelet, Francis Galton, Karl Pearson, George Udny Yule, Anders Nicolai Kiaer, Arthur Lyon Bowley, Jerzy Neyman, Max Weber, William Fielding Ogburn, Samuel Stouffer, Paul Lazarsfeld, Robert Merton, Otis Dudley Duncan, James Coleman and Raymond Boudon.

As someone who began academic life as a historian, I would hesitate to describe what I have produced as being history or, if it is history, it is Whig history (Butterfield, 1931) with a vengeance: that is to say, it is 'teleological' history in which the past is clearly subordinated to the present. My starting point is with sociology as science – as a population science – as I see it now emerging, and my interest in the past is then strictly with the origins of sociology thus understood. Perhaps what I have attempted to produce could best be described as a genealogy of such sociology in terms of the contributions made by a number of individuals over a lengthy historical period. Two further points follow from this.

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[1] I was also later encouraged by the remark of Markus Gangl (2018: 1), in taking up the editorship of the *European Sociological Review*, that he intended that it would continue to represent sociology as 'an analytical and cumulative population science' in the way in which I had delineated it.

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The first is that there is no need for the individuals that I select to have thought of themselves specifically as sociologists or to have been subsequently regarded as such. All that matters is that, looking back, it can be shown that they contributed, whether by intent or otherwise, something of significant value to the development of the kind of sociology with which I am concerned. As a result, the pioneers I consider – and also some omissions from the list – are very likely, I recognise, to occasion surprise and, in turn, objection. I suspect there a good number of sociologists who have never considered Halley as a contributor to their discipline, who have never even heard of Kiaer, who would think of Pearson, Yule and Bowley only as statisticians, who would, on ideological grounds, dislike acknowledging any debt to Galton – and who would at the same time look askance at my disregard of others who conventionally figure in the sociological Pantheon, and in particular, perhaps, of Durkheim. However, the main body of the book is in effect taken up with accounts of why I believe that those included deserve their place. And in the case of Durkheim, I try to make it clear why, although he did play a leading role in the founding of sociology as an academic discipline and indeed sought to assert its scientific status, I see him, and his followers, more as standing in the way of, rather than furthering, the emergence of sociology as a science in the sense from which I start out.[2]

While my selection of pioneers may then appear to many as being a disturbingly heterodox, if not a quite idiosyncratic, one, what I would wish further to stress is that collectively it does amount to more than just a miscellaneous collection of individuals. The seventeen are, in fact, closely connected by lines of intellectual descent, and indeed to a greater degree than I had myself initially realised. That is to say, one can trace, as is indicated in Figure I.1, acknowledged lines of influence – in some cases involving pupillage and collaboration – that run quite continuously from the earliest of the number through to the most recent.

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[2] Another whose omission may give rise to some dissent is Karl Marx. I would, however, regard Marx as, from an academic standpoint, contributing far more to political economy than to sociology – even though his work carried evident sociological implications and was for several of my pioneers, at some stage in their careers, a significant intellectual as well as political influence. The only other attempts that I am aware of to outline a history of sociology specifically 'as it is practised today' are those of Wright, in an apparently little-known paper (2009), and earlier Madge (1962). Wright and I agree in including Galton among the 'founders', in favouring Quetelet over Durkheim, and in recognising the importance of Lazarsfeld; but, on grounds that will emerge, I would not endorse Wright's view of the standing of Booth in the field of social survey research. Apart from a shared admiration of Stouffer, I have very little in common with Madge, who writes on the origins of 'scientific sociology' but with what appears to me to be an insufficiently considered, and thus unduly eclectic, view of what a scientific sociology would entail.

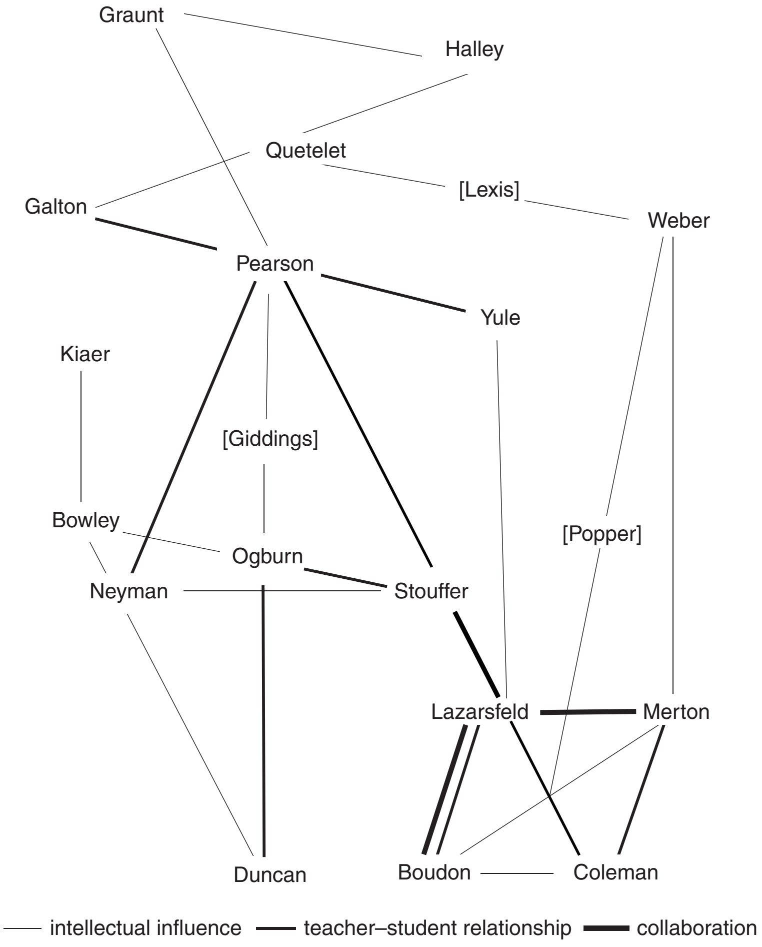
Graunt
Halley
Quetelet
[Lexis]
Galton
Weber
Pearson
Yule
Kiaer
[Giddings]
[Popper]
Bowley
Ogburn
Neyman
Stouffer
Lazarsfeld
Merton
Duncan
Boudon
Coleman
intellectual influence
teacher–student relationship
collaboration

Figure I.1 The intellectual descent of the pioneers

I would on this basis question whether any other tradition of thought and research within sociology can be pointed to that is of a comparable kind, and in particular in being similarly *progressive*. Cole (1994) has argued that where sociology most obviously falls short of the achievements of the natural sciences is in failing to develop a cumulating body of generally accepted 'core' knowledge, grounded in equally accepted research methodologies and theory. This is clearly true insofar as sociology is understood, as some would wish it to be, simply as an ongoing

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'conversation' around a changing collection of topics of social interest that can be treated in different ways and in different 'perspectives'. But the work of the pioneers I review shows that such a situation, in which unlimited pluralism appears co-existent with stasis, does not have to be the case. Advances in knowledge, based on the development of research methods, more extensive and reliable empirical findings, and theory of increasing explanatory power, can be gained – albeit, as I indeed show, with the usual accompaniment of wrong turns, blank spots, missed opportunities and the like.

The second point that follows from the general approach I take is that I do not have to make any attempt to present the work of those I identify as pioneers in a comprehensive way, nor to give much attention to its social context. I am, in fact, highly selective in focusing – though often in some detail – on those features of their work that appear of importance *from the present-day standpoint that I adopt*, while much else is neglected. And this, in turn, means that I am far more concerned with the *consequences* of the pioneers' achievements than with their provenance, except insofar as I seek to bring out the continuity that I have referred to. Thus my approach is to use the jargon of the historiography of science, far more 'internalist' than 'externalist' in character.

I do, however, in the case of each pioneer provide some brief biographical information. My aim here is not to introduce any element of the 'sociology of sociology' – in my view, a generally very dubious undertaking. It is, rather, to bring out the diversity of the individuals in question. While most did, in fact, end up teaching and carrying out research in elite universities, in Europe or in the United States, as regards their social origins, education and early careers, they have very little in common. As will be seen, they grew up in families, ranging from those of labourers and small shopkeepers through to those of wealthy merchants, industrialists and bankers;[3] and, as might be expected, degrees in sociology were held only by those born from the later nineteenth century onwards. Earlier, a training in mathematics or in law was most common. Moreover, before producing their pioneering work most engaged in occupations and activities outside of academia – draper, astronomer, explorer, engineer, civil servant, schoolteacher, agronomist, newspaperman and stage magician. All of which makes still more remarkable the extent to which they shared in contributing to what can, in retrospect at least, be viewed as a common enterprise.

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[3] One curious point that I simply note, while doubting if any significance should attach to it, is that five of the seventeen pioneers experienced some degree of economic hardship in their early lives as a result of their father dying or suffering some major misfortune.

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There is, nonetheless, one commonality that should be acknowledged. All of my seventeen pioneers fall into the category of dead white males. The deadness is my choice. Not only might picking out contemporaries who would count as their most direct and significant successors appear unduly invidious; it would also, I believe, be unreliable. Any such judgements would be ones better made with the benefit of some elapse of time. So far as whiteness and maleness are concerned, these I would see as simply reflecting historical fact. The formation of a sociological science, in the sense in which I wish to understand it, was essentially the achievement of men, native to and working in Europe and North America. However, I do seek to give due recognition to the women who also contributed as – and sometimes simultaneously – their wives, assistants, co-authors and publicists. The likes of Florence Nightingale, Emily Perrin, Margaret Hogg, Marie Jahoda, Patricia Kendall and Beverley Duncan have not often figured in previous histories of sociology of any kind.

One last question remains. What possible value do I see this book as having, other than in helping to meet any intrinsic interest that may exist in the origins of sociology as a population science? My answer would be that just as a better understanding of the nature of sociology as a population science may be of value in their work to those sociologists who are, in fact, pursuing sociology in this sense – as I argued in the introduction to my earlier book – so too may be a better understanding of its history. Alfred North Whitehead, the distinguished mathematician and philosopher, once remarked (1916) that '[a] science that hesitates to forget its founders is lost' – in response to which I recall Alvin Gouldner's comment: 'But to forget something one must have known it in the first place. A science ignorant of its founders does not know how far it has travelled nor in what direction; it, too, is lost.' Whitehead's remark certainly has some sharpness in regard to the teaching of sociology, which seems all too readily to degenerate into a rehashing of research and ideas from the rather long distant – and in some cases eminently forgettable – past.[4] But Gouldner's point has particular relevance so far as research is concerned. What is disturbing is the extent to which the growing numbers of those who are today engaged in sociological research are unaware of how the methods of data collection and analysis that they routinely use were developed, and in response to which problems; or of what is the past record of explanatory success – or failure – of the different theoretical

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[4] I am appalled to find that organisations with websites offering university students plagiarised essays can, apparently, still make money from essays relating to what has become known as the Affluent Worker project – very much a *pièce d'occasion* – in which I was involved more than half a century ago.

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approaches available to them. In consequence, enthusiasm for what may appear to be new ways ahead can lead not only to wheels being rediscovered but, more seriously, to potholes in the road still being fallen into even when previously well signed. A further qualification of Whitehead's position, from George Santayana, appears apt: 'Those who do not remember the past are condemned to repeat it.'

This book is, then, in more senses than one, history written not 'for its own sake' but rather with a purpose. If any historians should read it, I can only hope for tolerance. It is, however, primarily intended for sociologists, and indeed for those who, explicitly or otherwise, do pursue sociology as a population science. But other sociologists, I hope, may read it too. And, if so, they may perhaps ask themselves how the version of sociology that they would themselves favour, whether as a science or otherwise, stands in comparison, not only as regards its tradition but as regards its record of actually advancing our knowledge and understanding of human societies.