Conclusion

In this concluding chapter, I do not aim to summarise what has gone before. I noted in the Introduction that readers wanting an overview of the argument of the book could simply read through the propositions with which the central chapters are headed. I hope that those who at this stage feel in need of some recapitulation might find it sufficient to do likewise. What I wish to consider here is *what might be thought to follow* if my case for an understanding of sociology as a population science were to be accepted. More specifically, I am concerned with the implications for sociology itself as an academic discipline, for its relations with other disciplines and for its public role.

For sociology as a discipline, what would perhaps most obviously follow insofar as it became understood as a population science is that the scope to which it presently pretends would be significantly reduced. That is to say, in taking population regularities as its proper explananda, in focusing on establishing the extent and form of such regularities through statistically grounded methods and on developing and testing mechanism-based explanations of their generation and persistence, there is no question that sociology would address a narrower range of topics, through less diverse research styles and with a more limited conception of its ultimate goals than is presently the case. Abbott (2001: 5–6) has observed that sociology 'is not very good at excluding things from itself' and that 'once an area makes a claim for sociological attention, the discipline doesn't have any *intellectually* effective way of denying that claim' (emphasis in original). Sociology as a population science would have such a way, that is, in being based on a relatively clear definition of what are, and are not, its appropriate objects of study – and of what are in turn its appropriate methods of data collection and analysis and modes of explanation.

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From this point of view, to echo Mies van der Rohe, less would be more.

However, it is, I recognise, on the grounds that it would imply a clear diminution of the extent and diversity of the sociological domain that the idea of sociology as a population science is most likely to be disputed and resisted. In anticipation of such a reaction, I might then restate a point that I already emphasised in the Introduction. My concern in arguing for sociology as a population science is not so much with advancing a normative programme – that is, with telling sociologists what they should do – as with setting out a more considered rationale for the way in which a large and increasing number of them appear in fact already to practise sociology, and with helping in this way to provide the basis on which, in my view, the development of a scientific sociology could best proceed. The counter-arguments to my position that I would therefore take as being of main relevance and weight, as again I stressed in the Introduction, would be ones put forward by sociologists who have a commitment to this same project but who would see other routes ahead as being more promising. And responses made on such lines – responses indicating alternative models for sociology as a science – I would regard as representing a very positive outcome of the present work. As for those sociologists who would favour quite different agendas – as, say, for some form of 'humanistic' or sociopolitically 'committed' sociology – they will no doubt still seek to carry through their own projects, and it could thus be expected that the highly pluralistic character of sociology, considered overall, will in fact be maintained.

In this connection, though, two further points might be made. The first is the rather obvious one that there is a stage when pluralism within a discipline becomes a liability rather than an asset: that is, when the existence of disciplinarity has itself to be called into question. For example, as a British sociologist, but one working chiefly within a European context, I have to observe that in the light of an examination, on the one hand, of the majority of papers appearing in such British journals as *Sociology* or the *Sociological Review* and, on

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the other, of the majority of those appearing in the *European Sociological Review* or *Acta Sociologica* (edited from the Nordic countries), it would be no easy task to explain in just what sense these could be said to reflect work in one and the same discipline. This cannot be a favourable circumstance as regards either the standing of sociology within academia or its chances of maintaining public support, moral or material.[1] Furthermore, at least for those seeking to develop sociology as a science, pluralism must always stand in some tension with the aim of achieving what Ziman (1968) has referred to as 'consensible' knowledge. While on the frontiers of research, divergent and conflicting views, debate and controversy are to be expected and indeed play a crucial role in the scientific process, at some stage frontier disputes have to translate into the growth of 'core' knowledge on which all competent workers in the field can agree. Insofar as this does not happen in sociology, claims that it might make to scientific status are clearly undermined (Cole, 1994).

The second, and related, point is then that within the pluralism of present-day sociology, those whose concern is primarily with its development as a science can legitimately assert, and exploit, a right to criticise, on what they would regard as scientific grounds, work emerging from versions of sociology directed towards other goals: that is, on grounds ultimately of the quality of the data and the data analysis involved and the logical consistency of evidence and argument.

[1] It might, however, be thought that the European situation has more potential for development than that existing in the US: that is, insofar as differences in conceptions of sociology and in its actual practice are becoming more structured *among* university departments and research centres, professional associations and even countries. Thus, possibilities would appear to exist for at least some *de facto* reorganisation, allowing those who wish to pursue sociology as a social science to go their own way. In the US, in contrast, pluralism – or, one might say, fragmentation – would appear more strongly embedded *within* universities and associations. In particular, the American Sociological Association, viewed from outside, does appear as something of a whited sepulchre, having all the paraphernalia of a professional association serving an academic discipline while revealing a serious lack of internal consensus over what the essentials of this discipline are. By way of illustration, see the debate over 'public sociology' as conducted in, for example, Clawson et al. (2007), and taken up later in the text.

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In other words, those who reject the idea of sociology as a science cannot, through such a rejection, create some kind of immunity for themselves against challenges to their own knowledge claims. This is a matter that I pursue further in regard to the public role of sociology discussed later in this chapter.

Next, however, I turn to the implications of understanding sociology as a population science for its relations with other academic disciplines, and the reorientation of sociology that I would envisage is, I accept, again likely to be a cause of dissension.

Most disturbingly for some will no doubt be what appears to follow for the relationship between sociology and history. The argument of Chapter 4 in particular clearly entails a rejection of the idea advanced by authors such as Giddens (1979) and Abrams (1980) that no significant borderline between sociology and history as academic disciplines need in fact be recognised. What I maintain here is that the historical mode of explanation as applied in the case of singular events, or complexes of such events, is clearly different from the sociological mode as applied in the case of demonstrated regularities in events. Historical explanations, while perhaps drawing on theory from various sources, remain time- and place-specific narratives of action and interaction in which an important role has almost always to be given to sheer contingency – to essential chance. Sociological explanations aim to be narratives as generalised in time and place as possible, and while the causal mechanisms or processes to which they refer will incorporate chance, this is a chance that is 'tamable', in that explananda and explanations alike are of a probabilistic character.[2]

To avoid any misunderstanding, I would at the same time emphasise that the idea of sociology as a population science in no way precludes or is inimical to research undertaken *in the context of historical societies*. To the contrary, insofar as it is possible to

[2] I have elsewhere (Goldthorpe, 2007: vol. 1, ch. 2) sought also to show basic methodological differences between history and sociology in regard to the kinds of data on which they are able to draw, and to bring out some of the implications of these differences.

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obtain appropriate data, the investigation of population regularities in such societies can be of major value. I noted in Chapter 2 that research of the kind in question – whether carried out as 'historical sociology' or as 'social science history' – has played an important role in undermining naïve notions of 'traditional' societies and communities deriving from the holistic paradigm. And work in a similar vein is now contributing substantially to our understanding of both commonalities and variation across early modern and industrialising societies in such areas as family formation, household structure, the occupational division of labour and social stratification and mobility. I would see work produced by, for example, the Cambridge Group for the History of Population and Social Structure or the Historical International Social Mobility Analysis (HISMA) network, centred at the University of Utrecht, as indeed providing notable illustrations of sociology as a population science.

While the relationship between sociology and history is then one that would call for some clearer differentiation if sociology were to be understood as a population science, other instances do at the same time arise where such an understanding would in fact weaken disciplinary boundaries, if not effectively remove them. This is most clearly the case with sociology and demography – an already established population science.

In the middle decades of the twentieth century, especially in countries such as the US and Britain, and also France, the two disciplines were in fact already close. Researchers, whether nominally sociologists or demographers, treated similar issues and used essentially similar methodologies – as, for example, in such fields as the determinants of fertility, homogamy and heterogamy, migration patterns, residential segregation by ethnicity or social status, educational inequalities and social mobility. But, subsequently, some greater distancing became apparent. On the side of sociology, this would seem to have been mainly the result of the 'reaction against positivism': that is, against quantitative research in general. On the side of demography, fears arose that a focus on micro-level social processes meant

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that the discipline was 'abandoning its core' (Lee, 2001): that is, the study of human populations at the macro-level on the basis primarily of census and registration data, rather than survey data, and through established measurement procedures and formal models.

However, what would now appear to be emerging within demography is a recognition that little conflict is in fact here involved – and with a consequent reopening of opportunities for, in effect, disciplinary integration with sociology under the auspices of population science. As Billari (2015) has argued (see also Xie, 2000), the first stage of demographic inquiry can indeed be taken as that of the description – often the highly sophisticated description – of regularities in population movements over place and time; but a second stage naturally follows of seeking for explanations of these regularities in terms of causal processes or mechanisms operating at the micro-level of individual action and interaction. The parallels with an understanding of sociology on the lines for which I have argued are evident enough. While demography viewed in the way that Billari proposes has itself to be an interdisciplinary subject – the causal processes invoked in the second stage may be ones involving, say, biology or psychology as well as sociology – the linkages with sociology could be expected to be of central importance.

A further, rather less obvious case where the shared idea of population science could make for greater and potentially highly rewarding collaboration arises with sociology and epidemiology. Here also, a relatively close interdisciplinary relationship in the mid-twentieth century tended later to weaken.[3] In sociology, the focus changed from 'sociology in medicine' to 'the sociology of medicine', with associated criticism of the 'medical model' of illness and treatment, which

[3] This close relationship was especially apparent in Britain. Some of the most impressive seminars and conferences that I attended as a young sociologist were ones in which epidemiologists of the calibre of Jerry Morris and Abe Adelstein came together with medical sociologists such as Raymond Illsley and others from his Medical Research Council centre in Aberdeen and social policy specialists such as Richard Titmuss and Brian Abel-Smith.

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had the effect of alienating many medical researchers (in my view, quite understandably so). At the same time, in epidemiology, a growing emphasis came to be placed on the empirical determination of individual-level risk factors for diseases, with often only a limited interest being shown either in the population distributions of diseases or in the causal mechanisms underlying risks.

Again, though, promising counter-tendencies would now appear to be in train. In epidemiology, a reaction against the dominance of 'risk-factor' and 'black-box' approaches and a renewed insistence on epidemiology as a population science can both be traced from the later 1990s (see e.g. Susser, 1998; Pearce, 1999, 2011). The importance is stressed of retaining, on the one hand, a concern with the description and analysis of disease distributions, since in this way hitherto unrecognised public health problems can often be revealed – or, that is, new epidemiological explananda created – and, on the other hand, a concern with underlying causal processes operating at all levels from the molecular to the societal. In this latter respect, then, significant opportunities are opened up for collaboration between the now increasing numbers of self-identified 'social epidemiologists' (Galea and Link, 2013) and those sociologists who, moving on from medical sociology, now work in the more widely conceived field of the sociology of health and illness, with a population orientation providing the common ground.

One can, I believe, realistically envisage the development in the years ahead of research centres in the human population sciences in which sociologists, demographers and social epidemiologists would be brought together and in which research would be pursued on such lines that disciplinary boundaries would be in large part transcended.

There is one other academic relationship that rather obviously calls for attention here: that between sociology and economics. How would this be affected by the understanding of sociology as a population science? In certain respects, it might be thought that in this way sociology and economics would be brought somewhat closer

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together: that is, through the adoption in sociology of the individualistic paradigm – of methodological individualism – to which economics has always been committed, and further through the privileging, from both explanatory and hermeneutic standpoints, of individual action that can be treated as in some sense rational. However, major divergences are still in fact to be expected.

First of all, on the side of economics the consequences persist of what has been described (Bruni and Sugden, 2007) as 'the Pareto turn' at the beginning of the twentieth century, as a result of which economics sought explicitly to establish itself as a 'separate science' from psychology and sociology: that is, as one founded on theory derived deductively from axioms of rational choice that claimed objective correctness and that were in no way dependent on research into how individuals actually did make choices and act upon them. In contrast, on the side of sociology the rejection of the holistic and the acceptance of the individualistic paradigm of inquiry have been associated with a concern for as realistic an understanding as possible of the psychological and social processes involved in individual choice and in turn, so far as rationality is concerned, with a refusal of demonic in favour of bounded conceptions. Moreover, it is by no means clear that the more recent development of behavioural economics has done much to bridge the gap. Aside from having doubts about the external validity of much of the experimental work being undertaken, sociologists might also wish to question the degree of commitment to empirical realism: that is, whether all that is involved is the creation of more complicated utility functions through the inclusion of 'social' preferences (see e.g. Rabin, 1998), while little interest is shown in the way in which decision-making in everyday social life is actually carried through (see Berg and Gigerenzer, 2010).

Second, and relatedly, the approach to questions of the 'fit' between theory and the results of empirical research would appear very different as between at least mainstream economics and sociology practised as a population science. Economists dealing with some substantive issue tend to regard this as primarily a matter of applying

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deductively derived theory and then treat empirical findings as serving to illustrate, or further, perhaps, to quantify, the theory. The idea that such findings could serve to *test* theory and might therefore lead to its rejection – implying a falsificationist methodological approach – has never found much favour among those committed to the idea of economics as a separate science (Blaug, 1992; Hausman, 1992). *In extremis*, resort can always be made to the argument that, since the theory embodies objectively correct principles of choice, any deviation from it indicates that it is the actors involved who are 'wrong' – that is, who display ignorance or error – rather than the theory itself. In contrast, sociology as a population science would begin with establishing empirically the regularities that constitute the explananda in some area of substantive interest and then seek for mechanism-based explanations, which, while quite possibly lacking the degree of theoretical coherence found in economics, would always have to be open to evaluation, positive or negative, in the light of further research.

Insofar as there are any indications of disciplinary convergence, they stem from the development – exciting but still minoritarian – of what has become known as the 'new economics thinking', and especially as this focuses on a more empirical approach to the analysis of economic issues.[4] As perhaps the most notable examples of work in this vein, one could cite the studies of economic inequality, based on extensive research into the distributions of income and wealth and their variation over place and change over time, that have been carried out by economists such as Tony Atkinson, Thomas Piketty and their associates (see esp. Atkinson, 2008; Atkinson and Piketty, 2010; Piketty, 2014). What is emphasised in these studies, and is clearly suggestive of a population approach, is the importance – to draw on Piketty (2014: 3, 20, 31–2) – first of all of determining through detailed statistical research and analysis relevant 'facts and patterns'

[4] The two leading centres in this connection are the Institute for New Economic Thinking in New York and the Institute for New Economic Thinking at the Oxford University Martin School.

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concerning inequality, instead of producing 'purely theoretical results without even knowing what facts needed to be explained'; and then, having established the specific explananda, of hypothesising at the level of individual action 'economic, social, and political mechanisms that might explain them', and with the obvious implication that 'economics should never have sought to divorce itself from the other social sciences and can advance only in conjunction with them'.

Finally, I come to the implications of the idea of sociology as a population science for its public role. In recent years, discussion of the role that sociology might play outside of academia has largely turned on the idea of 'public sociology', as advanced by Michael Burawoy in his 2004 Presidential Address to the American Sociological Association (Burawoy, 2005). I can then take this idea as a convenient point of reference, and all the more so since the position for which I would argue stands in more or less direct opposition to that of Burawoy.

As regards the extra-academic significance of sociology, it is crucial for Burawoy to make a distinction between what he refers to as 'policy sociology' and the 'public sociology' that he aims to promote. Policy sociology, Burawoy claims, is 'sociology in the service of a goal defined by a client'; its *raison d'être* is to provide solutions to predefined problems or to legitimate supposed solutions already in place. In contrast, public sociology, especially in its 'organic' rather than 'traditional' forms, involves 'a dialogic relation' between sociologists and a variety of publics over issues of shared political and moral concern in which a mutual adjustment of interests and values is sought, with the possibility of action towards common ends then being pursued (Burawoy, 2005: 9).

From the standpoint of sociology as a population science, the distinction here set up has little merit. While Burawoy's conception of public sociology envisages a significantly larger role for sociology, as a social science, than it can in fact legitimately seek to fulfil, his conception of policy sociology is, on the other hand, quite unnecessarily restricted. At the heart of the matter is a further, and yet more questionable, distinction that Burawoy wishes to make:

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that between 'instrumental' and 'reflexive' knowledge. Instrumental knowledge, according to Burawoy, is the knowledge that derives from the professional practice of sociology, and that may be applied in policy sociology. In contrast, reflexive knowledge is said to result from the dialogues of public sociology and is knowledge concerned not with means but with the ultimate ends of society (Burawoy, 2005: 11). However, what Burawoy then quite fails to explain is just *how* such reflexive knowledge is obtained, tested and codified, let alone *why* – on what grounds – he supposes that the ends of society can in any event *be* an object of knowledge as opposed to a matter of value choice – choice that sociology in fact best serves to clarify and to sharpen.[5]

In response, what has then to be insisted on is that, while sociologists are of course as free as other citizens to engage in sociopolitical action, there is no reason to accept that through such engagement they gain access to some special kind of knowledge. Any knowledge supposedly emergent from public sociology, as Burawoy would understand it, must stand exposed to exactly the same critical examination as that which derives from professional sociology. In other words, reflexive knowledge – whatever it is supposed to mean – cannot be knowledge that has a privileged status simply in respect of the sociopolitical values that it is taken to underwrite. And, as commentators on Burawoy have observed (e.g. Turner, 2007), many of the works he cites as prime examples of public sociology have in fact been far more successful in attracting public attention than in convincing other sociologists of their soundness. Neither 'commitment' nor 'impact' provides any guarantee of scientific quality; and sociologists should be ready to recognise that this is so just as much in the

[5] Burawoy at one point states that 'Public sociology has no intrinsic normative valence' (Burawoy, 2005: 8). But he also argues that since sociology owes its existence to 'civil society', sociologists have an obligation to commit to the values of civil society (Burawoy, 2004). The difficulty then is that these values are spelled out in ways either that are so general as to beg all crucial questions or that some sociologists could quite reasonably wish to dispute. Note Nielsen's (2004) pertinent observations on 'the vacant we' in Burawoy's public sociology.

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case of work which appears to reflect their own value positions as in that which does not.

While, then, Burawoy's public sociology, based on reflexive knowledge, pretends to a role that any sociology with claims to scientific status cannot – and should not attempt to – fulfil, it is difficult to see his conception of policy sociology, based on instrumental knowledge, as being other than a deliberately impoverished one. The idea that policy sociology can operate only in the service of a goal defined by a client or so as to legitimate existing policies is far removed from reality. Many sociologists in fact enter the policy field in order to argue that policies, whether those of governments or of other agencies, are misconceived and thus likely to fail in attaining their objectives or to have damaging side-effects; or, yet more radically, to argue that the problems towards which policies are directed have been inadequately understood. And in these respects, sociology as a population science can have particular force, since it is very often the form of population regularities and the social processes through which they are generated and sustained that is centrally at issue.

For example, in Britain since the 1990s, as now more recently in the US, social mobility has become a major political concern. But, in Britain at least, most related discussion of policy has been predicated on the view that social mobility is in decline, even though survey research indicates in most respects a rather remarkable degree of stability (see e.g. Bukodi et al., 2014); and at the same time, the difficulty experienced by politicians and their policy advisors in grasping the standard sociological distinction between absolute and relative mobility rates (see p. 15) has led to deep confusion over what would be entailed in increasing mobility and over what policies would and would not have some chance of success. If I (Goldthorpe, 2013) and others have attempted to bring these points out, why should this not be regarded as policy sociology?

Or, as a further example, one could take the concern apparent in family policy in many countries over the possibly negative effects on children – on, say, their emotional development or educational

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progress – of rising rates of parental break-up. In this case, too, much confusion has arisen in regard to what negative effects are and are not securely established, and further, and more seriously, in regard to the causal processes involved. Policy has often been based on the assumption that it is break-up itself that is the key causal factor and has therefore been aimed at reducing its frequency. But this position has been called into question by sociologists and demographers (for a valuable review, see Ní Bhrolcháin, 2001), who have pointed to evidence that other variables may lie behind *both* break-up and adverse outcomes for children – most obviously, although not only, parental conflict – and with then the implication that fewer break-ups need not have the positive consequences hoped for and that in some cases break-up may in fact be the least damaging option. Again, why is this not to be regarded as a case of policy sociology in action?

In sum, sociology as a population science could appropriately and effectively fulfil a public role in providing the grounding for a policy sociology far less passive and uncritical than that characterised by Burawoy, while at the same time making no claims to give access to special – or, one might well say, spurious – forms of knowledge that can determine what the ultimate ends of a society should be.[6]

Max Weber (1921/1948, 1922/1948), in his two magnificent essays on 'Politics as a Vocation' and 'Science as a Vocation', saw a major extra-academic objective of science as being 'to gain *clarity*' as to its public role (1922/1948: 151, emphasis in original): that is, clarity as to what are those issues on which science can properly speak – issues of fact, analysis and theory; and what are those that lie beyond its limits – issues arising in 'the various value spheres of the world [that] stand in irreconcilable conflict with each other' (1922/48:

[6] Rather remarkably, Burawoy (2005: 23) appears to accept that sociology cannot compete with economics in 'the policy world' – in part because of the greater intellectual coherence that economics possesses – and should not in fact attempt to do so. I would regard this as quite misguided and sadly defeatist. Sociology should always be ready to compete with economics in exerting influence on policy issues – aggressively so, if need be. And, at least if understood as a population science, it has the capacity do so to very good effect.

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147). As Weber well appreciated, the two kinds of issue do inevitably interconnect, and especially so in the social domain: for example, in regard to the extent to which particular values can, under given conditions, be realised; through what forms of policy and indeed polity; and with what further, unintended and possibly unwanted, consequences. And he also observed that there is an ever-present tendency for political actors to seek to show that the facts, analysis and theory are 'on their side', and often to resort to selectivity, distortion or downright misrepresentation in order to strengthen their case. This tendency in itself Weber could actually view with some detachment, since he appreciated that embracing politics as a vocation did entail accepting that means might have to be justified in terms of ends. He reserved his most scathing criticism not for political actors who tried to exploit science but rather for those individuals who tried to exploit their scientific positions and authority in order to give some privileged status to what were no more than their own sociopolitical preferences. And, in turn, he insisted that those for whom science was truly a vocation – those who had a primary value-commitment to science – should always assume responsibility for maintaining the necessary clarity over the knowledge claims that science could and could not legitimately make, and should at the same time always be ready to face up to scientific findings that were, from their own extra-scientific standpoints, 'inconvenient' (1922/1948: 145–50).[7]

So far as their public role is concerned, proponents of sociology as a population science should then be well placed to take on the responsibility that Weber demanded. Since they start out from a recognition of individual variability and human population heterogeneity,

[7] Weber's criticism was directed equally at professors who were fervent German nationalists, such as Treitschke and his followers, and at Schmoller and other *Kathedersozialisten* – even though Weber was himself always a nationalist and, while never a socialist, had sympathy with some social-democratic positions and policies. He was also, it should be added, well aware, from his own experience, of the difficulties and inner conflicts that were likely to follow from the principled position he advocated when the same individual wished to be *both* a scientific *and* a political actor (see e.g. Mommsen, 1984: chs 7 and 8 esp.).

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they should have little difficulty in accepting heterogeneity in, and conflict between, values – Weber's 'unceasing struggle' of 'warring gods' – as an aspect of the human condition from which they, no more than anyone else, can escape. And since their concern is to investigate the social regularities emergent in human populations and their underlying causal processes through methods that, they can maintain, are those most fit for this purpose, they are in a strong position to challenge, where necessary, those who seek to shore up their value positions and their associated sociopolitical objectives with sociology of a less well-founded kind.

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