

# Contesting Disciplinary Boundaries. Richard Kilminster and the 'sociological revolution'

*Alan Scott*

UNIVERSITY OF NEW ENGLAND (NSW)

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## Abstract

This paper examines the idea of the 'sociological revolution' in the work of Richard Kilminster. His work is interpreted as a form of late classical (or late modernist) opposition to academic fashions in contemporary sociology and social theory. This approach views sociology as a cognitive revolution that sweeps – or should sweep – all before it, both transcending and incorporating the concerns of rival disciplines, notably philosophy. The paper first examines the potential rewards and costs of this circumvention of academic fashions. Secondly, it discusses the possible unforeseen consequences of the border dispute with philosophy in which the Elias community has occasionally engaged. The final section of the paper makes the case for a more pluralistic view of the role of sociology that takes its cue from George Steinmetz's plea for an 'open' sociology in which sociology acts as a broker between disciplines rather than making 'imperialist' claims on its own behalf. Karl Mannheim's pluralist and conciliatory position is taken as an example of such a foil to a radical 'sociologism'. The fate of Mannheim's more conciliatory stance in his sociology of knowledge, however, illustrates that such an open sociology is not without its dangers.

**Keywords:** academic fashions; disciplinarity and disciplinary imperialism, George Steinmetz; Karl Mannheim; open sociology; Richard Kilminster; sociological revolution; sociology of knowledge.

## Introduction

This paper is the result of an invitation – as one of Richard Kilminster's early doctoral students – to participate in a workshop held in his honour at the University of Leeds on 5–6 April 2018. The invitation made me revisit themes – in the sociology of knowledge – that I have long neglected as my work shifted towards political sociology and has 'fallen back' behind the Mannheim-Elias position that inspired his work to one influenced by Weber and (Karl) Polanyi (neither of whom were unambiguously sociologists). <sup>[1]</sup><sub>[#N1]</sub> One stated motivation for the workshop was to redress what is seen (and what I also see) as the insufficient attention that his contribution has thus far received. In both the talk and this article I have worked on the assumption that the most appropriate way to honour someone's work is to engage with it. The focus of this discussion is on Kilminster's ambitious programme for sociology; one that views sociology as a cognitive revolution that sweeps – or should sweep – all before it, both transcending and incorporating the concerns of rival disciplines, notably philosophy. This programme – I argue – is one shaped not by the intellectual fashions of our own times, but is firmly grounded in the late classical period of the discipline's history. I have considerable sympathy for this unfashionable – or anti-fashion – stance. Where our views diverge – as will become clear – is in my predilection for more open disciplinary borders over his preference for a sociology

unflinching in its disciplinary claims. The potential danger inherent in both of these positions is a key concern of this paper.

Kilminster has a long-standing interest in the sociology of knowledge, so it seems only appropriate to interpret the reception of his work as itself a problem in the sociology of knowledge. What does its reception tell us about the nature of sociology and social theory? I want to address this question by examining, first, the role of fashion in the reception of academic work and, second, the possible unforeseen and unintended consequences of the 'border dispute' with philosophy. The former can be considered an external or environmental factor in the work's reception, the latter as inherent to this particular approach. I shall take Kilminster's analysis of globalisation as an 'emergent concept' as my main illustration. The reason for this is in part autobiographical. I asked Richard to write a chapter for a book I was editing, entitled *The Limits of Globalization*, which appeared in 1997. That chapter reappears in a modified form as Chapter 6 of *The Sociological Revolution* (Kilminster 1998). A more substantial reason for focusing on this chapter is that it contains in miniature the themes, concerns, and influences that run through his work, and is thus a part that can stand in for the whole.

## Against fashion

Over twenty years have passed since the publication of 'Globalization as an emergent concept' (Kilminster 1997; see also 1998), [2][#N2]so some contextualisation may be necessary. In the 1990s, and beyond, globalisation was a central – arguably the dominant – topic in social theory and much of sociology. The debate was generally couched in terms of what might be called 'new-worldism': an emphasis upon the epochal nature of the shift from bounded nation states and national societies to global economic, political, and cultural interconnectedness. The globalisation debate generated, or was caught up in, other epochal claims (of which more later). Paul Hirst and Grahame Thompson had already sought to dampen some of this enthusiasm in their influential *Globalization in Question* (1996) by arguing that the contemporary world was (a) less globalised than was generally thought, and unevenly so, and (b) in many respects less globalised than it had been in the early twentieth century. However, despite the attention their work (rightly) received, it did little to still the claims being made for the epochal and irreversible nature of globalisation. Hirst's and Thompson's position remains a minority view. Kilminster's analysis can be read as an intervention into this debate in which he joined them in the minority party, but arrived there via a different route. Whereas Hirst and Thomson relied heavily on empirical data to support their case and confined their view to the long twentieth century, Kilminster focused on the history of ideas and dug further back, into Enlightenment political thought.

A clue to the core argument of that chapter is captured in the quotation from Turgot (1727–1781) at its head: 'With the passage of time, nations, hitherto living in isolation, draw nearer to one another' (quoted in Kilminster 1998: 93). The implication is clear: while the term is new, the idea of globalisation is not the invention of contemporary social theory, but has deep roots in European thought. This, in turn, implies that we are not dealing with a recent break but with a long-term historical process already well advanced. Conversely, contemporary 'models of social structure' are 'a product of the current phase of the development of national and international dependencies' (Kilminster 1998: 97). It is what Elias (1987) called sociology's 'retreat into the present' that obscures this. Thus, Kilminster discards the 'overdramatic dichotomization' (du Gay 2003: 667) characteristic of epochal and presentist thinking. But it was precisely overdramatic dichotomies that were characteristic of the dominant trends in social theories at the time, and – I want to suggest – were a source of their success.

Social theorists work in a highly competitive environment in which it is tempting to seek the 'cumulative advantage' of grabbing the megaphone (Douglas and Mars 2003: 767). This cumulative advantage has since been amplified by social media. In a world of blogging and of Twitter, the 'public intellectual' is also a media intellectual. A high media profile in turn enhances reputation in the institutional context in which most public/media intellectuals work, a context in which impact has become a key criterion of worth. In this way, they acquire a source of authority and a *cachet* outside their place of work that provides greater legitimacy within it and from which the employer too can benefit. Dichotomous claims, particularly when combined with a secularised, optimistic or pessimistic, millenarianism, have proved an effective means to access the megaphone. To eschew such a rhetorical strategy is to court neglect: to place oneself on the wrong side of fashion. At a time when Beck, Giddens, and Bauman were demanding a radical rethink and a break with modernist social thought, and a rejection of its basic categories, Kilminster was writing: 'social relations at the level of integration above that of nations do not [...] represent in principle any particular technical difficulty of concept formation' (1998: 105). That all but amounted to a declaration of war on the *Zeitgeist*.

Readers of this journal will hardly need to be told where this self-conscious anti-fashion is coming from: Norbert Elias's figurational sociology, with its emphasis on long-term socio-historical processes, in sharp contrast to the dichotomous logic of social theory as *Zeitdiagnose*, as a diagnosis of our times. [3].[#N3]. Elias has often been characterised as the last representative of classical sociology. And that is precisely the issue. At the height of postmodernism, Kilminster and the wider Elias community were committed to a classically modernist perspective, methodology, and sensibility. For example, in defiance of postmodernism, Kilminster (1998: 111) coolly asserts that 'theories are not narratives'. But there is more than postmodernism at stake here. The Eliasian approach leads him to question the – political as well as conceptual – assumptions of all those trends influential at that time, and in some cases beyond. Here there is only space to briefly mention the most obvious:

*Marxism*: one characteristic of Kilminster's work – evident also in the globalisation piece – is the centrality he accords to Marx and Marxism in the emergence of social and sociological theory. Marx is here credited with being 'one of the first to try to develop social-scientific concepts systematically to deal with the social regularities and patterns set in train by the rapidly extending global trade networks of his time' (Kilminster 1998: 100). However, we are also told that Marx's social-scientific theory was 'burdened with the same teleology as that of the metaphysical theory he was claiming to supplant' (Kilminster 1998: 102) and that contemporary Marxists – he had Immanuel Wallerstein in mind – are likewise trapped in teleology by their failure to break with this philosophical – specifically Hegelian – tradition.

*Culture and the cultural turn*: similarly, while acknowledging the importance of Roland Robertson's influential – Parsonian – emphasis upon global culture, he rejects the one-sided emphasis of the much-vaunted 'cultural turn' and calls for a "third way" for sociological inquiry into globalization, moving between the economism of Wallerstein and the culturalism of Robertson' (Kilminster 1998: 105).

*Postcolonial theory*: having put Marxism and the cultural turn to one side, he then makes a statement that would be thought highly contentious by postcolonial scholars (as well as in retrospect appearing over-optimistic): 'the rich nations are now less likely to use violent means to settle disputes between themselves and the poor nations: they have been compelled to show them more respect than in the days of colonialism' (Kilminster 1998: 109).

What is interesting about this list is that Kilminster distances himself in equal measure from (i) a well-established tradition which continues to exercise enormous influence – e.g. in the literature on 'neoliberalisation' (Marxism); (ii) a trend that was highly influential at the time but whose moment may now have past (the cultural turn); (iii) an emerging perspective that has subsequently gained in influence (postcolonial theory). Given that the readership for social theory – like newspaper readers – tend to read that which confirms rather than challenges their worldview, this strategy is as bold as it is honest. But there is a price to pay. The effects of flocking behaviour among academics are magnified by a mechanism that Robert Merton identified long ago: the Matthew Effect, [4],[#N4], namely, the tendency for success to attract success; for attention to attract attention; or, in this case, for citations to attract citations (Merton 1968). There is perhaps a further point related to the sociology of knowledge to be made here: Merton's analysis is so well known that the mechanism it identified has been deliberately and widely deployed, for example by journals prominently listing or posting their most cited and/or most frequently downloaded articles. In this sense, Merton's analysis may have unwittingly amplified the effects of the very mechanism it analyses, and – to use another Mertonian term – have become a self-fulfilling prophecy (Merton 1948).

Let me conclude this section by making explicit what I have so far been hinting: the material and ideal values and rewards of academic life may be misaligned; indeed, they may occasionally be opposed. Individual scholarship within disciplinary bounds has come to be viewed as of less value – and as having less impact – than inter- or transdisciplinary co-production in teams, and those who pursue the former course are increasingly seen as 'traditional academics' who have failed to understand the nature of the modern higher education and research industry, and still believe themselves to be protected within 'sheltered workshops' [5] [#N5], that allow them to set their own academic agenda and remain firmly entrenched within their 'disciplinary silos'. Under the contemporary conditions of academic production, what we like to think of as academic values – intellectual honesty, modesty in not over-stating claims beyond what can be supported by argument and evidence, caution, thoroughness, etc. – are not necessarily (if indeed they ever exclusively were) the qualities that are selected for or rewarded. Conversely, audacious (but unsubstantiated) assertions, the over-hasty publication of results (which plagues the natural sciences), micro-empirical studies (which plague the social sciences), fashion, and academic populism may garner material and reputational reward. All this may be magnified by mechanisms such as the Matthew Effect and, arguably, by the reward structures that have increasingly shaped intellectual production. To defy those trends and avoid those strategies is a risky, if laudable, undertaking.

But to focus on these external factors alone is perhaps just a little too comforting. In my next remarks I shall switch roles and become a (hopefully friendly) critic. Here my remarks are addressed to the Elias community more generally rather than to Kilminster's work specifically. While the tide against which it was swimming has ebbed – making this special issue a timely re-evaluation – what barriers remain to the reception of work emanating from this community? To what extent might that community be said to be the author of its own fate?

## For sociology (and against philosophy)

He who fights with monsters should look to it that he himself does not become a monster. And when you gaze long into an abyss the abyss also gazes into you (Nietzsche, 1990 [1886]: 102 Aphorism 146).

Following Kilminster's example, I start from a heading quote, albeit one that is perhaps a little overfamiliar. Nonetheless, it vividly captures what I now want to argue, again with reference to his analysis of the emergence of the concept of globalisation.

The globalisation piece is wide-ranging, both in its coverage of the history of ideas and of contemporary theory. But it is striking that, when it comes to setting out the 'third way', the references focus more narrowly on works from within the Eliasian school: Elias himself, of course, but also Godfried van Benthem van den Bergh, Johan Goudsblom, Stephen Mennell, and Cas Wouters. There is nothing particularly surprising or unusual about this – it is a normal part of theory building – but there is still a potential danger, and it is this that I wish to discuss, and to do so with reference to the running border skirmish between the sociologists of the Elias school and philosophy, in which Kilminster has been a key participant. I am here not concerned with the substance of that dispute – Elias's substitution of 'homines aperti' for 'homo clausus' and rejection of transcendentalism in all its forms (see the [1968] postscript to *Elias 2012* [1939]: 491–527) – but with its potential consequences under changed institutional conditions.

On revisiting this dispute I was somewhat taken aback and puzzled by its vehemence, on both sides (e.g. *Dunne 2014*; *Kilminster 2014*). I have two overlapping concerns: disciplinary and institutional:

*Disciplinary*: the break with philosophy might be seen as a normal part of the formation of modern disciplines, with sociology following in the footsteps of the natural sciences (once part of natural philosophy) and other – more established – social sciences such as economics. Viewed in this way, it appears as an aspect of the modernisation and professionalisation leading to the contemporary system of disciplines. But what is odd here is the *way* in which (at least for figurational sociologists) this break takes place: as a *dispute* with philosophers and as an explicit attempt to define the discipline against or as 'post' philosophy (e.g. *Kilminster 2011*). The more usual pattern is simply to leave philosophy behind and stake out a distinct theoretical and empirical research agenda. Economists, for example, do not feel obliged to distance themselves from Adam Smith's moral philosophy, nor do they feel particularly threatened when philosophers (rightly, I think) point out that rational choice theory and experimental economics are concerned with the same questions as those that consumed, for example, Hobbes or Hume: trust (or its absence), co-operation between rational self-interested individuals, the private versus the public good, etc. [6]. To put the point provocatively, are we dealing here with what Freud (2001 [1917]: 199) famously called 'the narcissism of minor differences'? [7]. Is it precisely sociology's proximity to and affinity with philosophy that makes this dispute and repeated assertions of autonomy (seem) necessary? [8].

*Institutional*: figurational sociologists tend to ascribe the 'hegemony of philosophy' (*Kilminster 2011*: 92) to its more established and secure position within the academy; to its 'superior position in the academic pecking order' (*Kilminster 2011*: 99 – see also *Kilminster 1998*, 3–26). In the context in which Mannheim and Elias were working in Weimar Germany, this struggle to break away from philosophy's tutelage was understandable, but in ours I wonder whether this view has not simply been overtaken by events. I once heard a philosopher describe her discipline as the 'dethroned queen of the sciences'. [9]. Philosophy departments have shrunk (and were shrinking at a time when sociology departments were still expanding), and the discipline's position in the academy has become ever more precarious. Now, the key point is that the very same institutional pressures that have affected philosophy have been working on sociology and social theory: the demand for measurable impact, relevance, co-operation with stakeholders (usually meaning business and industry), knowledge transfer, etc. Those parts of sociology – e.g.

sociological and social theory – for which such a utilitarian regime is highly problematic may ironically find themselves in the place once occupied by theology and philosophy. [10][#N10] In other words, sociology and philosophy may by now be part of a common community of fate: [11][#N11] one, indeed, that may also come to include basic research in the natural sciences. In this institutional context, running disciplinary border disputes may only facilitate what Andrew Dunsire (1993) called strategies of 'collibration', the manipulation of conflict by a third party for ulterior ends. [12][#N12]

But my main reason for mentioning the dispute between figurational sociologists and philosophy is that it may be symptomatic not of self-confidence, but of a defensive and inward-looking form of we-identity building within the cognitive community. I see no need for this. Elias clearly produced some of the major works of social theory and sociology in the twentieth century, and scholars working within that tradition – Richard Kilminster, but also, for example, Stephen Mennell and Eric Dunning – have produced work of lasting significance. But still, there is room for spreading the word more widely. What may have happened can be characterised in a three-stage model of the dissemination of a research programme:

1. The initial impetus provided by the 'great thinker' – in this case, of course, Elias – and foundational text(s);
2. The formation of a community of scholars around that thinker and that text/those texts in which the uniqueness and specificity of the programme is emphasised, empirical and theoretical work on sub-themes undertaken, and the perspective applied to new areas. This phase creates and depends on strong ties within the community (Granovetter 1973);
3. Brokerage (Burt 2005) and the development of weak ties beyond the community that facilitate dissemination, but also dilute the distinctiveness of the programme – sharing to mutual benefit. The transition to this stage may produce a generational division of labour with those of Kilminster's generation developing and refining the paradigm and the following cohort(s) – working under different institutional conditions – moving towards brokerage. As Mannheim (1952: 293) observed, 'our culture is developed by individuals who come into contact anew with the accumulated heritage.'

The problem may nonetheless lie in postponing the transition from Phase 2 to Phase 3. My suggestion is that the time has now come to move more fully into the final phase. As I have already suggested, Elias is part of a modernist tradition of social theory and sociology. This is evident in the influence of Weber on the civilisation process thesis and of Simmel's relationalism on his figurational sociology. The tide against modernism – the second/reflexive modernity thesis, etc. – has now ebbed. This is then an opportune moment not simply for re-evaluation, but also for re-opening a conversation between those working in the tradition of Elias and other still-standing representatives of modernist sociology – e.g. neo-Weberians – or with the broader relational sociology movement of Tilly (2005), Donati (2011), etc., which figurational sociology has already influenced (see Dépelteau 2008). The risk is that the distinctiveness of the Eliasian position may be weakened, but the advantages – in terms of reach and influence – may make this a price worth paying.

## Towards an open or towards a totalising sociology?

In this section of this paper, I want to follow the route I am proposing a little further: one influenced by the American historical sociologist George Steinmetz's plea for an 'open sociology'. As a starting point, Steinmetz (2007: 50) notes that 'the way the world is carved up and distributed among the various social sciences is largely arbitrary with regard to the actual object of study'. [13][#N13] Furthermore, unlike political science or

economics, sociology has not sought to carve out some specific aspect of social life as its focus. At the most general level, its concern is with 'the social' as such – an object so broad that it does not allow for effective policing of the discipline's borders (something that became evident only after the decline of the hegemony of positivism). For Steinmetz, the consequence is that sociology ought (perhaps must) embrace fluid and porous borders and act as a broker between disciplines, not with the imperialist ambition of Talcott Parsons, but as a 'nonimperial encounter'. Like individuals located within 'weak or crisis-ridden national states' with little claim to 'imperialist grandeur', sociologists may be in a better position than most to 'promote transdisciplinarity' (Steinmetz 2007: 57). With respect to philosophy, Steinmetz's view contrasts sharply with that discussed above:

Accepting social theory as an activity that can be pursued in partial independence from empirical research will bring sociologists back into contact with philosophers – rebuilding bridges that were of critical importance for members of the discipline's founding generation like Simmel, Durkheim, Cooley and Du Bois, not to mention more recent founders like Bourdieu. These dialogues with philosophy were interrupted by the empiricist and positivist generation that gained control of US sociology after the First World War. (Steinmetz 2007: 52)

Figurational sociology is far removed from empiricism and positivism, but, at least occasionally, appears to share the – in Steinmetz's sense, imperialist – ambition to displace its rivals by absorbing their concerns into its own domain.

Steinmetz's plea may strike some readers as excessively utopian, or as self-abnegating. I nonetheless want to defend at least a moderate version of his pluralism, and to do so with reference to arguments made by a thinker whose influence on Richard Kilminster's programme for sociology is hardly less profound than that of Elias: Karl Mannheim, the key figure in the early sociology of knowledge. [14],[#N14] Specifically, I shall draw on Mannheim's essay from 1922, 'The distinctive character of cultural-sociological knowledge', first published (in English translation) in *Structures of Thinking* (1982). [15],[#N15] Mannheim, I shall argue, offers a more conciliatory approach that acknowledges the continued legitimacy of both immanent accounts and of those theoretical and disciplinary perspectives that do not take the path of socio-genetic analysis. [16],[#N16]

Perhaps due to what Green (following a distinction made by Stanley Fish) calls Mannheim's 'dialectical' – as opposed to 'rhetorical' or 'logical' – style of *writing* during his German period, reading this text is a somewhat disorienting experience. For Green, Mannheim's writing is dialectical 'because it breaches the abstractive method of interpretation, not by attacking it ..., but by using it and, in use, letting it reach and overreach its limits' (2009: 244). [17],[#N17] This makes it difficult to ascertain which arguments Mannheim is making and which he is allowing to overreach their limits, and thus indirectly critiquing. The problem is compounded by the exploratory nature of this early text in which Mannheim appears to be working out his position and clarifying it in his own mind, and in which he occasionally 'hedges his bets' (Kettler et al. 1982: 18). I shall nevertheless risk drawing some selective arguments out of this dialectical maze.

Mannheim's first theme is exactly the same as that of this special issue: the sociology of sociology. In an act of hyper-reflexivity, he seeks to identify the conditions of the possibility of the activity in which he himself is engaged: doing cultural sociology. His core argument here is that as long as one lives under conditions that we would now call monocultural – e.g. as long as the Catholic Church was truly the *universal church* – one lives *in* culture, but cannot experience it as such or articulate it as an idea. Culture appears as nature, or at least as a natural order: 'cultural creation takes place, so to speak, behind the back of the creative subject' (Mannheim 1982: 44). It is only with the separation of cultural spheres and with some degree of cultural pluralism that we

can experience culture as culture (or history as history, or society as society). [18]. [N18]. The 'way to a purely sociological inquiry lay open' (Mannheim 1982: 47) only when 'a new kind of attitude' (Mannheim 1982: 48) arose – i.e. one which takes a genetic view where everything – to borrow Kilminster's term – comes to be seen as *emergent*. In sum, the conditions for the emergence of historicism are themselves historical: 'cultural sciences are themselves part of the process that they are describing' (Mannheim 1982: 50).

The second relevant argument is that our ability to do cultural sociology is grounded in general human capacities; that is to say, it is an extension of the – initially atheoretical, intuitive, and pre-scientific – reflexivity of what he calls the 'whole man' (Mannheim 1982: 77) ('der "ganze Mensch"', 1980: 54 – original emphasis). He thus links the famous distinction between immanent and non-immanent (or genetic) interpretations of cultural objects to the notion of *style*, which is 'at once an aesthetic and a sociological category' (1982: 86). [19]. [N19]. When we say of a painting by Monet that it is an example of impressionism, we have already moved away from the object – the painting – and classified it in terms of a broader (external) concept. When we then say of a piece of music by Debussy (or perhaps an essay by Simmel) that it too is an example of impressionism, then we have broadened that category still further and moved one step closer to a non-immanent interpretation. This is the condition of the possibility of theoretical endeavours: aesthetics and art history, but not yet that of cultural sociology. What the latter adds – via the notion of worldview – is the connection to the social totality, to 'cultural formation' (*Kulturgebilde*, e.g., Mannheim 1980: 87) and 'experiential contexture' (*Erlebniszusammenhang*, e.g., 1980: 89) understood as social phenomena. [20]. [N20]. Only then can cultural objects be 'comprehended socio-genetically rather than individual-genetically' (Mannheim 1982: 88). Cultural sociology thus builds on and extends previous endeavours – e.g., those concerned with intrinsic meaning – but that does not invalidate them.

The final argument that is relevant to my theme – disciplinary pluralism rather than disciplinary imperialism – is Mannheim's treatment of the question of validity. Sociology's focus is on functionality, not merely or primarily at the technical level – e.g., as the *result* of pre-theoretical – e.g. physical – action – but in relation to manifestations of higher consciousness (*höhere Bewußtseinserscheinungen*, 1980: 74) at the level of theory and sense making. Specifically, socio-genetic analysis entails a 'bracketing of validity' (*Einklammerung des Geltungscharakters*, e.g., 1980: 88). But to bracket the question of validity is not to deny or challenge it. It merely implies that sociology qua sociology has nothing to say on the topic, its focus lies elsewhere and validity (presumably like aesthetic value) is beyond its purview: 'sociological or other genetic explanations can neither confirm nor refute the truth or falsity of a proposition or of the entire theoretical sphere' (Mannheim 1982: 82). This is important to Mannheim because he believes it addresses the criticism that the sociology of knowledge or of cognition entails a self-defeating relativism (the 'genetic fallacy'). Ironically, here he himself clearly formulates the criticism of what came to be known as 'the Mannheim problem': the assertion that all knowledge is bound to time and to existential conditions (*zeit- and seinsverbunden/gebunden*) [21]. [N21]. Itself makes a claim to timeless validity: 'as *thinker* and theorist [...] he [the sociologist] acknowledges the titles of validity that press upon him here, he is compelled to posit them himself' (1982: 82 – original emphasis). Mannheim seems to think that as long as we desist from treating socio-genetic accounts as acts of unmasking or debunking (cf. Baehr 2013) and remain agnostic vis-à-vis truth, the apparent contradiction can be avoided. [22]. [N22]. The concept of 'bracketing' is intended to serve exactly this purpose.

While this defence of the sociology of knowledge against the charge of self-defeating relativism is unlikely to satisfy critics (see, for example, Seidel 2011 or Kusch 2019), [23]. [N23]. it is nevertheless relevant to my concern with what Walsh and Boucher (2018) call the 'proper boundaries of disciplines'. Mannheim's position seems intermediate between Steinmetz's radical pluralism and disciplinary imperialism. He clearly sees cultural sociology as the end – and in some sense higher and more comprehensive – point of a cognitive development reaching from pre-scientific intuition to socio-genetic analysis, but this does not entail an *Aufhebung* of



previous stages – i.e. their elevation onto and absorption into a higher level. Non-immanent interpretation does not *displace* immanent understanding, the sociology of knowledge does not make, for example, the history of ideas (or of art, or of philosophy) redundant. Rather it places their concerns within another framework; one with a different aim, a distinct cognitive interest. At least in this early text, Mannheim's view seems to be that if the aesthetic value of a cultural object concerns you, or the validity of an argument or scientific claim, then sociology is not the place to look. It has nothing to say. But if you are interested in the cultural formations and experiential contextures in which the object or claim can be located, then turn to sociology. It is the only place to go.

## Conclusion: varieties and perils of disciplinary patriotism

I shall conclude with a paradox. It should be clear that my sympathies lie with Mannheim's moderate position, or even with Steinmetz's pluralism, rather than a more radical 'sociologism'. But there is a problem. On my reading, Mannheim's position is conciliatory. His is a kind of disciplinary patriotism that does not question the legitimacy of other approaches and respects the proper boundaries (and proper relations) between disciplines and approaches. But if his aim here was to develop a sociology of knowledge, cognition, and culture without precipitating what have come to be called the 'science wars' he manifestly failed. Conciliation and moderation only succeeded in antagonising both sides. For many philosophers, and some sociologists (e.g. Grünwald 1982 [1934]), Mannheim's name became synonymous with the genetic fallacy ('the Mannheim problem'), while for those committed to a more radical project he was over cautious – an unforgivable failure of nerve.

David Bloor (1973) offered an early and influential version of the latter response, which also served as a manifesto for the 'strong programme' in the sociology of knowledge. Mannheim's 'weak' programme treated socio-cultural claims and those emerging from the natural sciences asymmetrically. Implicitly, he had entered a pact with the natural sciences in which the latter field was immune to socio-genetic analysis. This timidity was to be overcome – and the pact broken – by a strong programme for which all knowledge claims and knowledge claim producing practices were to be treated alike. There are two problems with this critique: first, Mannheim's distinction between the social and the natural worlds is an aspect of his interpretive – rather than positivist – sociology and his rejection of the unity of science hypotheses. The influence of both the hermeneutic tradition (notably, Dilthey) and phenomenology are particularly evident in the 1922 essay. His aim seems to have been to maintain the autonomy of social-scientific approaches from the natural sciences rather than ring-fence the latter field and immunise it from socio-genetic interpretation – i.e., the barrier was there to protect the social from the natural sciences, not vice-versa. Secondly, the bracketing of truth claims and Mannheim's exclusion of the *content* of natural science do not in and of themselves preclude socio-genetic analysis. The natural sciences and the practices that produce natural-scientific knowledge are embedded in wider socio-cultural conditions and require a highly elaborate (and well resourced) institutional framework (cf. Merton 1973). But beyond this self-limiting Mertonian perspective, a Mannheimian view of science would also view it an aspect the thought style (*Denkstil*) and conduct of life of a highly specialised (cognitive) community. The emergence of this wider culture and institutional framework, and the life and thought styles in which scientific practices are grounded remain legitimate subjects of socio-generic analysis for Mannheim. <sup>[24]</sup><sub>[#N24]</sub> But these doubts concerning Bloor's criticisms are largely beside the point. It was the strong programme and its offshoots – notably science and technology studies (STS) – that carried the day while, despite the sterling and scholarly efforts of Mannheim specialists (for example, Kettler, Meja and Stehr 1984; Kettler, Loader and Meja 2016), Mannheim's reputation has languished and his influence waned (see

Pels 1996). The critiques from both camps – appealing to different audiences – have stuck. In Mannheim's efforts at reconciliation, both parties have turned against him, and the damage appears to be irreparable. [25]

[#N25]

In light of this, it becomes clear that, where Mannheim appears to falter, following Elias – as Kilminster does – into a more robust and assertive sociological programme may be the more sustainable position. This – I think – is the underlying motivation for viewing sociology as a *revolution*, thereby denying legitimacy to those discourses that in turn sought – or seek – to deny its legitimacy. In disciplinary disputes, adopting the more robust position may well be the better survival strategy. Perhaps the choice is ultimately one of faith. Thus, while my own sympathies lie with conciliation and brokerage, I can well understand the reasons for wanting to put sociology on the front foot and off the defensive. It is to this effort that Richard Kilminster's work makes a significant contribution.

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## Notes

1. Polanyi was an economic historian with scant regard for disciplinary boundaries, while for Weber, sociology was one of the disciplines from which he drew and towards which he contributed – merely one (albeit the first) of a long list of 'mir nächstliegenden Disziplinen' (Weber 1988 [1919]: 600), one of the 'disciplines closest to me'. ♣ [#N1-ptri]
2. I shall refer to the 1998 version of this piece in all further discussion. ♣ [#N2-ptri]
3. For a fuller discussion and critique, see Palumbo and Scott 2018, Chapter 2. ♣ [#N3-ptri]
4. After Matthew 13:12: 'For whosoever hath, to him shall be given, and he shall have more abundance: but whosoever hath not, from him shall be taken away even that he hath' (Bible, King James Version, available online at <https://www.kingjamesbibleonline.org/Matthew-Chapter-13/> [<https://www.kingjamesbibleonline.org/Matthew-Chapter-13/>], accessed 10 August 2019). ♣ [#N4-ptri]
5. The term has (or had) a technical meaning – e.g. in the USA – for schemes to employ disabled workers under different condition to those applying to other workers (e.g. at subminimum wage levels). But it has become a polemical and derogatory label for employers – particularly universities – who allegedly afford employees a level of protection irrespective of their performance. ♣ [#N5-ptri]
6. A point repeatedly made by my former colleague, the late Martin Hollis, for whom all the essential questions of the social sciences had been identified by the end of the eighteenth century. ♣ [#N6-ptri]
7. Freud credits the English anthropologist Alfred Ernest Crawley (1867-1924) with the basic idea. ♣ [#N7-ptri]
8. It might also be noted that the divide between sociology and philosophy has long been blurred. How do you classify, for example, Georg Simmel or, to take an example that is less well-known to sociologists, Otto Neurath (1892–1945)? Neurath was a member of the Vienna Circle of logical empiricists but had gained his *Habilitation* in Max Weber's institute in Heidelberg. His work spanned philosophy, economics, sociology, and statistics (he was the inventor of the isotype – the pictorial representation of statistical social-scientific data). ♣ [#N8-ptri]

9. The philosopher was Cornelia Klinger and the occasion was a podium discussion on 'The Destruction of Knowledge' organized by the Institut für Wissenschaft für Menschen (IWM), of which she was then Interim Rector, in the Burgtheater, Vienna, 6 April 2014. The discussion is in fact available online at <http://www.iwm.at/video/die-zerstoerung-des-wissens/> [[http www iwm at video die zerstoerung des wissens](http://www.iwm.at/video/die-zerstoerung-des-wissens/) ], accessed 9 February 2019. The phrase 'dethroned queen of the sciences' seems to have earlier been applied to theology. ♣ [#N9-ptri]
10. While writing the first draft of the original talk (17 February 2018), I checked my work email. There was a list of currently advertised posts in sociology. The two at professorial level were a Professorship in Sociology with a focus on Digitalization, New Media, and Industrial Development (Johannes Kepler University, Linz) and a Full Professorship in Leadership in the Healthcare Sector (Aarhus University). ♣ [#N10-ptri]
11. This shared fate occasionally becomes manifest politically. In 2015, all Japanese universities received a letter from the Minister of Education, Hakubun Shimomura, asking them to find ways of cutting or closing humanities and social science departments in order to 'serve areas that better meet society's need' (see the report by Jack Grove in the *THE*, 14 September, 2015). The Japanese government later rowed back from this suggestion in the face of national and international reaction. In 2017–2018 the Australian Education Minister, Simon Birmingham, vetoed funding for eleven research projects – all of them in the humanities – that had been approved by the Australian Research Council (ARC) on the grounds that the Australian taxpayer would prefer to support other research, presumably that more obviously aligned with national priorities, as determined by government (see the commentary by Jon Piccini and Dick Moses in *The Conversation*, 26 October 2018). Even more relevant to the point I am making, in April 2019 Brazil's newly elected far-right government specifically targeted philosophy and sociology by withdrawing state funding for all programmes in those disciplines (see *OpenDemocracy*, 7 May 2019). ♣ [#N11-ptri]
12. Andrew Dunsire (1924–2015) was a leading UK scholar of public administration. Scales librate when they come into balance and oscillate 'gently around the horizontal' (Dunsire 1993: 12). 'Collibration' is Dunsire's coinage to describe the way a third party can manipulate a self-perpetuating conflict by tipping the scales to the advantage of one of the partners in the conflict. In this way, the third party – usually a government – can have a significant impact on an outcome via minimal or small technical interventions. For example, the UK *Trade Union Act 2016* introduced the new specification that a 50% turnout was required in order for the outcome of a strike ballot to be legally valid and introduced a double strike threshold (50% turnout plus 40% 'yes' among *eligible* members) in essential public services. Such legal/'technical' changes can dramatically tip the balance of advantage in favour of employers over employees. In Dunsire's own words, collibration is 'an intervention by government to use the social energy created by the tension between two or more social groupings habitually locked in opposition to one another to achieve a policy objective by altering the conditions of engagement without destroying the tension – unless deliberately' (Dunsire 1993: 12). Interestingly, this analysis also illustrates the occasional influence of sociology beyond its disciplinary boundaries. Dunsire draws upon Georg Simmel's figure of the *tertius gaudens*: the laughing (or rejoicing) third [party] who seeks advantage by intervening in a conflict between two parties; a possibility – and social dynamic – that only emerges with the shift from a dyadic to a triadic relationship (Simmel 1950 [1917]: 154–62). ♣ [#N12-ptri]
13. Not simply 'arbitrary', but also shaped by national variations; see Anderson 2003. ♣ [#N13-ptri]
14. The orientation towards Mannheim may also be seen as an anti-fashion move on Kilminster's part. As Pels (1996: 35) notes, 'since the early 1970s, the mainstream sociological tradition has been silent about Mannheim' and influential – 'strong programme' and constructivists – analyses of science 'largely developed without Mannheim, if not in conscious opposition to his work' (1996: 30). Pels (1996: 35) also suggests that Elias too tended to downplay his debt to Mannheim. This view is supported by Kilminster who sought to reconstruct Mannheim's affinities with, and influence on, Elias. Where the two thinkers differ, Kilminster views Elias's position as more advanced than that of Mannheim whose outlook

- remained 'essentially belletristic' (1993: 106). An updated version of that paper appears as Chapter 3 of Kilminster 2007. [↗\[#N14-ptr1\]](#)
15. That is to say, only two years after it was first published, by the same team of editors (David Kettler, Volker Maja and Nico Stehr), in German. I shall give *Structures of Thinking* as the reference. I have checked Mannheim's key terminology against the German original (Mannheim 1980) and have given these in brackets where it seems appropriate. [↗\[#N15-ptr1\]](#)
  16. Here my reading differs from Kilminster's (1993: 94) view that Mannheim was committed to sociology as a 'master discipline'. See for example, Mannheim's remarks on the continued indispensability of philosophical and epistemological enquiry (1936 [1929]: 260). [↗\[#N16-ptr1\]](#)
  17. Green's argument is that it is precisely this dialectical writing style that makes Mannheim's German period texts potential classics, in contrast to the later English works (notably on planning) which adopt a more rhetorical style. [↗\[#N17-ptr1\]](#)
  18. A similar thought may lay behind Mannheim's charging the socially free-floating intelligentsia with the task of denaturalizing values, beliefs, and customs. See Seidel (2016). [↗\[#N18-ptr1\]](#)
  19. Style is a central concept in both art history and in Mannheim – i.e., 'thought style' (*Denkstil*). Kwa (2012: 611-612) argues that the former had a direct influence on Mannheim. He also suggests that Mannheim's notion of a 'will to the world' (*Weltwollen*) (e.g. Mannheim 2012 [1929]: 60) is an adaptation of will to art/artistic volition (*Kunstwollen*). [↗\[#N19-ptr1\]](#)
  20. The term *Kulturgebilde*, which has its roots in phenomenology and which might be translated, without too much of a stretch, as 'cultural configuration' is, I would suggest, closely related to, and may even be the precursor of, Elias's 'figurations.' [↗\[#N20-ptr1\]](#)
  21. For a discussion of possible differences in nuance between *Seinsverbundenheit* (existential relatedness) and *Seinsgebundenheit* (existential determination) in Mannheim's thought, see Seidel (2016: 122). [↗\[#N21-ptr1\]](#)
  22. Mannheim's target here is the Marxist tradition of ideology critique, which he believes falls precisely into this trap. This is the argument that he would later elaborate in *Ideology and Utopia* (1936 [1929]). The attempt to debunk or unmask assumes the kind of static conception of truth which Mannheim's relationism purports to overcome. [↗\[#N22-ptr1\]](#)
  23. While defending Mannheim against the criticisms levelled against him by proponents of the strong programme in the sociology of knowledge (see fn. 24), Seidel (2011: 188) remains unconvinced that Mannheim's relationist epistemology amounts to more than the trivial claim that there will be 'no problem with relativism once you accept relativistic epistemology'. Kusch's view is that neither Simmel nor Mannheim resolved the problem of relativism (e.g. by substituting 'relationism' for relativism), but that this does not undermine the worth of their 'brilliant' historical studies because there is only a loose coupling between definitively resolving a highly complex problem and the intrinsic value of substantive analysis. [↗\[#N23-ptr1\]](#)
  24. Given the influence of the strong programme on the contemporary reception of Mannheim, the reading I am suggesting here is unorthodox. For an elaboration and detailed justification for such a 'deviant' interpretation, see Seidel (2011). [↗\[#N24-ptr1\]](#)
  25. This assertion may need some qualification. In my reading of more recent secondary literature, I was struck by the irony that it now seems to be philosophers of science rather than sociologists who are rediscovering – or at least revisiting – Mannheim and the early sociology of knowledge. Furthermore, by bringing Mannheim back into a conversation with his contemporaries – e.g. Georg Simmel, Ludwik Fleck, and Otto Neurath – this literature may come to act as a corrective to the anti-Mannheim polemic of the strong programme in the sociology of knowledge, and a more nuanced picture may yet emerge. I have already referenced Kusch and Seidel, but see also Uebel (2000). Uebel argues that despite the distinction between the context of discovery and the context of justification that we associate with the

logical empiricists of the Vienna Circle, some members of that circle (Otto Neurath and Phillip Frank) were by no means opposed to a sociological understanding of science; indeed they may have gone beyond Mannheim's exclusion of the *content* of science from socio-genetic analysis. ↗[#N25-ptr1]

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