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Measure, value and the current crises of sociology

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Abstract: This paper returns to C. Wright Mills' *The Sociological Imagination* to make an argument about the crisis of sociological method and theory today. Mills' famous text opens with a stinging critique of abstracted empiricism and grand theory on the grounds that they fetishize either methods or concepts. It is argued that Mills' critique can be applied to current sociological practices and thinking. The first part of this paper centres on questions of method, and reads between Mills' critique of abstracted empiricism and a recent debate over what Mike Savage and Roger Burrows call the 'coming crisis of empirical sociology'. In the light of this, it is argued that two crises currently haunt empirical sociology: a crisis of *imagination* and *measurement*. The second part of the paper then moves to the analysis of what Mills calls 'grand theory'. Here, two parallel crises are identified: a *generational* crisis within social theory that is tied in turn to what might be called a crisis of the *concept*. The conclusion of the paper returns to Mills in order to rethink his vision of the promise or *value* of sociology. It is argued that innovative conceptual work must lie at the heart of future sociological thinking if it is to move beyond the parallel traps of what Mills calls abstracted empiricism and grand theory.

Keywords: concepts, crisis, empiricism, measure, method, Mills, theory, value

Introduction

It has been said many times before that sociology is in a state of crisis, and indeed it might be argued that sociology has been in crisis as a discipline since its formal inception at the turn of the 20th century. However, whereas previous 'crises' have tended to centre on the displacement of one theoretical trend by another, or the shift from one set of epistemological or methodological commitments to a new agenda or style of working, the current crisis of sociology appears to be something different, for today there appear to be no clear theoretical or methodological grounds or procedures upon or through which unity or, perhaps more importantly, a common sense of *value* can be restored to the discipline. The excitement and invention of theoretical sociology, for example, has, with a few notable exceptions, long disappeared, and quantitative and

qualitative methodological work that was once unique to sociology is now performed equally well, if not better, outside the discipline or even the academy (see Savage and Burrows, 2007). Sociology seems to have lost its identity and to some extent lost its way. And if this is not worrying enough, the discipline is facing a range of new institutional pressures, particularly in the United Kingdom, where the future of state funding for the social sciences in general, and sociology in particular, is looking increasingly precarious. In the face of these developments, not to mention the unprecedented de-centring and specialization of the discipline, sociology genuinely seems to be in, or at least threatened by, a condition of crisis.

The present paper examines some of the key aspects of this situation. It does so by arguing that a number of these seemingly new developments are anticipated by C. Wright Mills in his now classic work, *The Sociological Imagination* (1959). This book is famous for its opening argument that the promise of sociology lies in its ability to connect private troubles and public issues, or more broadly questions of biography and history. The present paper, however, is concerned with the presentation of the discipline that underpins the core arguments of this text. For Mills' claim that sociology should attend to questions of biography and history, while interesting and important in itself, is a provocative response to a discipline that was in a deepening state of crisis. Mills' characterization of the sociology of his day is now famous, for he argues that it was torn between, on one hand, 'grand theory', exemplified by the dense writings of Talcott Parsons, and on the other, the 'abstracted empiricism' of 'specialists in method' such as Paul Lazarsfeld. What unites these seemingly unconnected strands of sociology is a common tendency to fetishize either theory or method. Mills argues that on one hand, grand theory fetishizes its concepts, and as a consequence works at such a high level of abstraction that it 'outruns any specific and empirical problem' (1959: 58), while on the other, abstracted empiricism loses its grip on the problems of the empirical world by confusing 'whatever is to be studied with the set of methods suggested for its study' (1959: 61). The argument of the present paper is that while Parsons and Lazarsfeld barely feature in contemporary sociology, this double impasse of theory and method remains with us today, and for this reason, among others (see Back, 2007; Fraser, 2009; Kemple and Mawani, 2009), there is a strong case for returning to Mills' work. The aim of this paper is to re-read Mills' *Sociological Imagination* to consider current crises of theory and method, and to pose again the question of the 'promise' or *value* of the discipline. The paper has two main parts that together question the continuing role of imagination in the discipline. The first part addresses questions of methodology by returning to Mills' arguments about the pitfalls of abstracted empiricism in the light of a recent exchange over the 'coming crisis of empirical sociology' (Savage and Burrows, 2007). It will be argued that mainstream empirical sociology today faces a crisis of methodological invention, particularly where qualitative and quantitative techniques are adopted as *a priori* commitments, and also a crisis of measurement as the academy loses the relative sovereignty it previously held over quantitative

research techniques. The second part of this paper returns to Mills' arguments about grand theory to outline a parallel but not identical set of problems in the world of theoretical sociology. It will be argued that social theory is currently suffering from a lack of conceptual renewal, innovation and excitement, and that this, at least in part, is because theoretical sociology is today passing through a state of generational crisis. In returning to Mills to make these arguments about invention and imagination in the discipline, particular attention will be paid to sociological concepts of measure and value; concepts which, historically, have been central to thinking about sociological method and theory respectively.

Abstracted empiricism

A key but neglected text for thinking about the value of measurement in sociology, and the value of quantitative approaches to methodology more generally, is C. Wright Mills' *The Sociological Imagination*. This book, which was first published in 1959, tends to be read today for its opening account of the 'promise' of sociology (something we will return to in the concluding section of this paper), and its appendix on 'intellectual craftsmanship'. However, other parts of this text, while sadly neglected, are equally as important. In the third chapter of this book, for example, Mills launches a stinging attack on what he calls abstracted empiricism: the practice of applying an established set of methodological techniques or conventions to the study of any empirical problem. Mills argues that the mistake of such an approach is that it elevates a prior commitment to specific research techniques (commonly called 'methods') over the challenges presented to us by the empirical world: 'Methodology, in short, seems to determine the problems' (1959: 67). Mills terms this an 'inhibition' which blocks rather than exercises the sociological imagination, for in his eyes a truly empirical methodology has to be flexible and innovative as it is formed in response to questions arising from the worlds we are attempting to study. In this view, the empirical is both a source of creativity and inspiration as well as being something that is to be explained (see Gane, 2009), and because of this should never be subordinated to a prior methodological commitment. Mills insists: 'no method . . . should be used to delimit the problems that we take up, if for no other reason that the most interesting and difficult issues of *method* usually begin where established techniques do not apply' (1959: 83). The challenge then is to think creatively about sociological methods in the face of the complexities of the empirical worlds with which we are engaged. To do so, Mills advises us not to move from an initial selection of a method to the analysis of a particular problem but the reverse: the problem, including its scale, should be used to determine the method. This means that method is tied to context, and is not something that can be assumed to have a universal application. Hence, Mills declares: 'Every man his own methodologist! Methodologists! Get to work!' (1959: 137).

Mills' critique of the tendency to fetishize methods within 'empirical' sociology (if it can truly be called this) can potentially be applied to qualitative and quantitative methodologies alike, but, his primary target in *The Sociological Imagination* is the statistical bias of applied social research, and in particular the work of Paul Lazarsfeld. Mills states that this type of sociology tends to fall into 'a more or less standard pattern' for it 'takes as the basic source of its "data" the more or less set interview with a series of individuals selected by a sampling procedure'. He continues: 'Their answers are classified and . . . used to make statistical runs by means of which relations are sought. Undoubtedly this fact, and the consequent ease with which the procedure is learned by any fairly intelligent person, accounts for much of its appeal' (1959: 60). Mills' objection to this type of quantitative research is that it subjects any potential problem – such as 'public opinion' or 'voting behaviour' – to standardized forms of statistical investigation. His argument is that such research starts with a concern for numbers or measurement, which it then elevates over the specific qualities of the empirical world it is attempting to analyse. The problem with this practice, as stated above, is that it works by moving from method to the empirical rather than vice versa, and this means that the challenges of the empirical are not always heard. For Mills, it is not the case that quantitative methodology per se is the problem (for depending on the empirical question this might be fitting), but rather the stamping of quantitative techniques upon all aspects of the social world regardless of the specific contexts and demands of this world. Such work, which makes an *a priori* methodological commitment in advance of whatever it seeks to study, can only frame and deal with empirical problems in narrow and repetitive ways, or in the words of Mills: 'within the curiously self-imposed limitations of [an] arbitrary epistemology' (1959: 65). It is through the exercise of such an epistemology that empirical sociology becomes what Mills calls 'abstracted empiricism'. For when research techniques determine the scope, range and focal points of investigation, sociology becomes abstracted from the empirical data that should be posing the questions, and as a consequence is destined to produce a 'thinness of result' (Mills, 1959: 76).

Mills, however, pushes this position further, for he is concerned not just with the epistemological limitations of abstracted empiricism, but also with its institutional dynamics, many of which can still be observed today. He proposes, for example, that the cost of large-scale statistical research cannot but direct its scope and focus. He states: 'as such studies are usually quite expensive, they have had to be shaped by some concern for the problems of the interests that have paid for them' (Mills, 1959: 75). Mills' point is a sensitive one: that the reliance of quantitative sociological work on sources of external funding means that the research problems addressed by such work are not always dictated by the academy but by agencies (commercial, state or even military; 1959: 115) that provide the cash. For Mills, this brings a potential conflict between the 'economics of truth', by which he means 'the costs of the research', and the 'politics of truth' or 'the use of research to clarify significant issues and to bring political controversy closer to realities' (1959: 75). Abstracted empiricism subordinates

the latter to the former as increasingly it relies on money that influences the agenda of its proposed research. This development is accompanied by the emergence of a new breed of administrative intellectuals within the discipline, and a new businesslike ethos within the academy more generally. Mills explains: 'As the costs of research increase . . . there comes about a corporate control over the division of labour. The idea of a university as a circle of professorial peers . . . tends to be replaced by the idea of a university as a set of research bureaucracies . . .' (1959: 116). This situation, which was identified by Mills only in its nascent state, has led many since to trace and question the corporate ethos of universities today. One notable statement on this question is by Bill Readings, who in his book *The University in Ruins* argues that we are witnessing the 'reconception of the University as a corporation, one of whose functions (products?) is the granting of degrees with a cultural cachet, but whose overall nature is corporate rather than cultural' (1996: 11).

Mills himself addresses the new capitalistic and bureaucratic ethos of the university in the fifth chapter of *The Sociological Imagination*. His position is uncompromising. He argues that abstracted empiricism is accompanied by a new conservatism of 'a managerial and manipulative sort' (1959: 113), for increasingly universities are populated by 'intellectual administrators and research promoters' and by 'younger recruits, better described as research technicians than as social scientists' (1959: 117). The former are, in the words of Mills, 'members of The Committee; they are on The Board of Directors; they can get you the job, the trip, the research grant. They are the executives of the mind . . .' (1959: 117). The latter are the new recruits of abstracted empiricism who are trained solely in 'this one perspective, this one vocabulary, this one set of techniques' and who know nothing else but to work with a specific set of 'explicitly coded' methods (1959: 119). For Mills, this new generation of 'research technicians', who at best offer a 'formal and empty ingenuity', are largely immune to other ways of thinking and working as they do little more than apply their methodological techniques in routine and blinkered ways. He complains: 'I have seldom seen one of these young men . . . in a condition of genuine intellectual puzzlement. And I have never seen any passionate curiosity about a great problem, the sort of curiosity that compels the mind to travel anywhere in any means, to remake itself if necessary, in order to find out' (Mills, 1959: 118). Again, the problem for Mills is that this fetishization or 'ritual following' of methodological techniques narrows or even blocks our capacity to listen to and learn from the empirical world, as for this to happen method must not be preconceived but secondary to the questions or problems in hand. Mills here treats abstracted empiricism as a bureaucracy of intellect that mirrors that of the academy; one that is slowly starving sociology of its creativity and imagination and which therefore is to be opposed at all costs.

These fierce comments by Mills are all too often bypassed by contemporary readers of *The Sociological Imagination*, but interestingly a number of Mills' broader statements on method and methodology frame a recent exchange over what Savage and Burrows (2007) call 'the coming crisis of empirical sociology'.

This exchange, while taking its lead from the work of Gouldner (1970), centres on a further but connected argument made by Mills: that the sociological imagination is not confined to the discipline of sociology or even the academy. Mills makes this argument in at least two places in the opening chapter of *The Sociological Imagination*. The first is in his observation that the sociological imagination can be found routinely in 'factual and moral concerns, in literary work and in political analysis', and because of this it is becoming 'the major common denominator of our cultural life and its signal feature' (1959: 21). The second is in a footnote in which Mills argues that much of what the sociological imagination means 'is not at all expressed by sociologists. In England, for example, sociology as an abstract discipline is still somewhat marginal, yet in much English journalism, fiction, and above all history, the sociological imagination is very well developed indeed' (1959: 26).

These statements are more telling than they might at first appear. They propose, first, that within the academy, some of the most important sociological thinking takes place not just outside of abstracted empiricism, but outside of the discipline. Mills sees the working of a sociological imagination, for example, within some forms of political and literary analysis. Today, we might add the social is being theorized in a range of other disciplines, including women's studies, media theory, cultural studies and geography (see below, and Gane, 2004), that often seem better at doing this than sociology itself (Rustin, 2010: 48). Second, the above quotes from Mills suggest that sociology is becoming, in the words of Gouldner (see 1970: 4–7), a form of popular culture (see also Beer and Burrows, 2010), for the sociological imagination can now be found in media industries that seek to understand 'the intimate realities of ourselves in connexion with larger social realities' (Mills, 1959: 22). Savage and Burrows develop this line of argument in their paper on 'The Coming Crisis of Empirical Sociology' by proposing that qualitative methods such as the interview that were once distinctive to the discipline of sociology can now be found almost everywhere. They reflect: 'although it was sociologists who pioneered the use of these methods in allowing popular narratives to be made "public", the routine use of such methods in all forms of contemporary journalism, from the colour magazine to the Oprah Winfrey show, marks a clear shift of expertise away from the academy' (Savage and Burrows, 2007: 893; for a more detailed analysis of this development and other connected trends, see Savage, 2010: 237–249). This shift of expertise also applies to quantitative methodologies, which are now often deployed in commercial settings in ways that are more powerful and innovative than anything found within academic sociology. One example of this new breed of commercial sociology is the geodemographics industry (see Burrows and Gane, 2006), which produces spatial mappings of social class by working with transactional datasets that are unprecedented in terms of their scale and complexity. In the face of such developments, quantitative sociology stands on the brink of a crisis, for it can no longer compete with the methodologies and datasets that are used routinely by commercial agencies. For Savage and Burrows, this is one instance of a wider process whereby the academy is

losing its sovereignty over the methods it once pioneered (for an earlier expression of this argument, see Law, 2004: 3). Indeed, they argue that the days when sociologists could lay claim to 'a series of distinctive methodological tools' (2007: 886) are now firmly in the past.

Savage and Burrows give a number of examples that support this position, some of which are striking. Savage recalls a conversation at an ESRC methods festival with someone with an attachment to a leading telecoms company who had 'the entire records of every phone call made on his system over several years, amounting to several billion ties' (Savage and Burrows, 2007: 886–887). Savage reflects that this data 'dwarves anything that an academic social scientist could garner. Crucially, it was data that did not require a special effort to collect, but was the digital by-product of the routine operations of a large capitalist institution. It is also private data to which most academics have no access' (2007: 887). The data available to quantitative sociologists is, then, a key issue (see Abbott, 2006: 57), as is the continuing value of their core methodologies. Savage and Burrows pick the sample survey as their primary example. They argue that the 'glory years' (roughly 1950–90) of this method are over for the following reasons: first, the response rates of such surveys are falling; second, their sampling frames tend to be restricted to national spaces; and third, large-scale survey research now takes place almost routinely throughout the commercial world. On this latter development, they suggest that a 'telling issue is the proliferation of survey research in private companies, especially in areas of market research. Such survey research now has very limited reference to academic expertise' (Savage and Burrows, 2007: 890). More tellingly: 'contemporary capitalist organizations now simply don't need the empirical expertise of quantitative social scientists as they go about their business . . . Most powerful institutional agents now have more effective research tools than sample surveys' (2007: 891). The argument here is two-fold: the sample survey is now outdated in the face of advanced quantitative techniques developed in the commercial sector, and for this reason, among others, this sector is less reliant on the academy for its methodological prowess and innovation than in the past. In the face of this situation, Savage and Burrows issue a stark warning: 'those sociologists who stake the expertise of their discipline to this method might want to reflect on whether this might leave them exposed to marginalization or even redundancy' (2007: 892).

Rosemary Crompton (2008) has responded to this paper by Savage and Burrows in a paper entitled 'Forty Years of *Sociology*'. She makes two main arguments. The first is that Savage and Burrows downplay the continuing importance of quantitative methodologies such as the sample survey by privileging classification over causality as the primary concern of current forms of empirical sociology. This objection has been contested by Savage and Burrows (2009: 769; see also Webber, 2009), but is interesting nonetheless for it raises the question of whether classification and causality can be treated as two separate things. One of the most important features of emergent geodemographic classifications is that they have causal and even normative effects. This is not

just because of their symbolic meanings (see Burrows and Gane, 2006: 806), but because increasingly they are used to make decisions about the distribution of material resources on the ground. This means that even if such classifications are fictional they are at the same time very 'real'. Given this, one task of empirical sociology might be to reconsider the interplay between classification and causality, or epistemology and ontology more broadly, by addressing the lives of such classificatory systems and the effects they produce.

Second, Crompton argues that the main problem facing quantitative sociology today is not that its methodological tools are outdated, but that there is a lack of 'expertise' in this area (2008: 1223; see also the recent ESRC International Benchmarking Review[1]). At the same time, Crompton is sceptical of the value of what might be called commercial sociology: sociology that takes place outside the academy in commercial settings. For while she concedes that geodemographic analyses of social class 'can generate precise contemporary social mappings', she argues that they are also problematic for 'as with any multiple measure deriving from consumption patterns, the measure is likely to be unstable, and to consume considerable resources in its maintenance' (2008: 1221). It is not altogether clear on what grounds geodemographic classifications such as Mosaic might be termed unstable. Indeed, it might be argued in reply that such classifications are more flexible, fast-moving and therefore more robust than any comparable classification produced either by the academy or the state. Moreover, there is perhaps a connection between what Crompton calls the lack of 'quantitative expertise' in the discipline and her observation that such large-scale yet intricate geodemographic work requires serious financial backing. For increasingly such work can only take place in the commercial sector: in companies that not only have the resources to develop innovative, large-scale quantitative methodologies, but also the financial power to attract and keep the requisite sociological talent (the geodemographic package Mosaic, for example, is produced by the FTSE 100 company Experian).

So where does this take us? By revisiting Mills' critique of abstracted empiricism it is possible to argue that empirical sociology is facing a crisis along two interrelated fronts. First, is a crisis of *imagination*. The fetishization of ready-made quantitative and qualitative methods is more apparent now than it was when Mills wrote *The Sociological Imagination* in the late 1950s. John Law, one of the few commentators to have addressed this situation, identifies an emergent 'methodological hegemony' that threatens to impose 'a set of constraining normative blinkers' on sociological research (Law, 2004: 4). It is still common, for example, for mainstream sociological research and teaching to display an *a priori* commitment to the value of standardized quantitative or qualitative methods. For Law, the common dictum here is: 'If you want to understand reality properly then you need to follow the methodological rules' (2004: 5). This means, in Mills' terms, that empirical problems are commonly determined by prior methodological commitments. Law responds by questioning the ways in which such 'methods *produce* the reality they understand' (2004: 5). Equally, one might ask about the empirical 'realities' that such methodological practices

leave untouched. It is noticeable, for example, that many of the urgent and pressing events of our times barely feature within mainstream sociology: the wars in Iraq and Afghanistan, the terrorist attacks in New York, Madrid, London and Mumbai, the ongoing financial crisis, to name but a few. This may be because of the complex relationship of history and sociology, and a sociological aversion to the analysis of events (see Gane, 2006), but it could also be because of a 'fetishism' of method that, in Mills' words, subsequently 'determines the problems'. This 'methodological inhibition' is today deeply engrained within the institutional structures of the academy. In the UK, it is drilled into future academics at an early stage, for postgraduate students are expected to be trained in 'methodology' before they embark on doctoral study. This training involves the learning of core quantitative and qualitative methods, not to mention 'ethics', which are then supposed to frame any subsequent empirical research. This takes us back to the very heart of Mills' *Sociological Imagination*, which warns against such 'training' in the strongest possible terms:

> 'crash programmes' in methodology are not likely to help social science develop. Really useful accounts of method cannot be forced in that way, if they are not very firmly related to the actual working of social study, a sense of significant problem and the passion to solve it – nowadays so often lost – cannot be allowed full play in the mind of the working social scientist. (1959: 136)

In line with his earlier position, such 'training', for Mills, should be reversed so that it starts with problems in the empirical world which challenge us to think creatively about the means for their study. Method is never ready-made, and, to use the words of Becker, is 'too important to be left to methodologists' (cited in Burrows, 1993: 46). The challenge of conceiving methods out of the empirical is, for Mills, one of the great endeavours of sociological thought, and is one that cannot proceed without creativity and imagination. For in Millsian terms, a discipline dominated by stock quantitative and qualitative methods is a discipline not only lacking in imagination, but also one that in spite of its claims can never be empirical in any meaningful sense.

Second, is a crisis of *measurement*, or more specifically a crisis in the value of quantitative sociologies that centre on the production of data through techniques of measurement. In the third chapter of *The Sociological Imagination*, Mills is critical of Lazarsfeld's quasi-Comtean view that the sociologist is to become 'the methodologist of all the social sciences' (1959: 70). Mills' rejection of this statement is multifaceted: it rests partly on a critique of Lazarsfeld's method and partly on the belief that sociology might learn from the ways in which other academic disciplines exercise a sociological imagination. Further to this, Mills raises the key but neglected question of the connection of academic sociology to its commercial other. This connection is central to the recent exchange over the coming crisis of empirical sociology, which asks of the continuing value (economic or otherwise) of quantitative research within the academy now that commercial organizations are able to conduct such research

with greater resources, and arguably greater imagination and accuracy. What is the future of quantitative forms of sociology that in the past have been valued for their capacity for measurement but today have lost their sovereignty over quantitative research techniques, and with this some of their previous methodological prowess? This concern lies at the heart of Savage and Burrows' warning that quantitative sociology within the academy is threatened by marginalization and redundancy. For in an economic competition for the production of quantitative 'truths' (see Lyotard, 1984: 46–47), quantitative sociology is unlikely to win, especially given the precarious state of higher education funding in the UK.[2] Against this backdrop, Mills' comments regarding the costs of large-scale empirical work, and more generally the connection between the politics and economics of truth, seem more pertinent than ever. For how can the academic forms of quantitative research possibly compete with the vast transactional datasets and technical powers of the private sector? Savage and Burrows address precisely this question, and respond with the following declaration:

> Welcome to the world of 'knowing capitalism': a world inundated with complex processes of social and cultural digitization; a world in which commercial forces predominate; a world in which we, as sociologists, are losing whatever jurisdiction we had over the study of the 'social' as the generation, mobilization and analysis of social data become ubiquitous. (2009: 763)

This might sound bleak for the future of the discipline, or at least for the future of quantitative sociological research, but perhaps Mills offers us some ways out of this situation? One might be to revisit complex questions regarding the value of measurement, and of the value of a sociology that is not tied to the latter. Another, more concretely, might be to acknowledge that the academy cannot compete in terms of access to and processing of data, and to think instead more creatively about what method is and what it might help us to achieve. To do this it might be useful to follow Mills' assertion that a sociological imagination exists outside of the academy, for one might then ask what academic sociology might learn, methodologically or otherwise, from the study of the operation of this imagination in commercial settings (see Burrows and Gane, 2006). For those interested in descriptive sociology, this might provide insight into new techniques for studying large-scale and complex social phenomena, while for those with more analytical or critical leanings, it might enhance our understanding of the methodological and technical practices that lie at the heart of contemporary 'knowing' capitalism.

Grand theory

A crisis of method, however, is only half of the problem, for Mills argues that a number of parallel but not identical crises are to be found in the world of theory. In similar fashion to the above, this section will draw upon Mills' argu-

ments in order to address the crisis of imagination that lies at the heart of theoretical sociology today. To do so, it is first necessary to revisit Mills' account of the theoretical work that was characteristic of his time, and more specifically his attack on the ascendency of what he called 'grand theory'. Mills' position will be re-read alongside more recent interventions on the status of grand theory by Quentin Skinner and William Outhwaite. This will lead in turn to a questioning of the role of concepts in social theory and, finally, if only in brief, to a reflection on the continuing value or 'promise' of sociology more generally.

For Mills, the problems of grand theory run parallel to those of abstracted empiricism (see 1959: 60), for whereas the latter proceeds through a fetishization of methodological techniques in place of a genuine engagement with empirical problems, grand theory fetishizes abstract concepts and systems to the extent that all contact with both empirical reality and history is lost. Mills states: 'The basic cause of grand theory is the initial choice of a level of thinking so general that its practitioners cannot logically get down to observation' (1959: 42). The main advocate of such theory, for Mills, is Talcott Parsons. Throughout the course of Chapter 2 of *The Sociological Imagination*, Mills takes a series of quotes from Parsons' *The Social System* and translates them 'into English'. This ridicule of Parsons' work comes as little surprise as there was a history of animosity between the two thinkers dating back to the mid-1940s (see Mills, 2000: 74). In *The Sociological Imagination*, Mills gives the following reason for his attack: 'My purpose in all this is to help grand theorists get down from their useless heights' (1959: 42). Beyond this playful sparring with Parsons, there is a serious point to Mills' argument. His starting point is the observation that grand theory proceeds through the 'associating and dissociating of concepts' (1959: 34). Such theory, he argues, connects together concepts to form an abstract and general system instead of addressing their potential use and meaning in connection to pressing empirical problems. For Mills, 'Grand theory is drunk on syntax, blind to semantics' (1959: 42). The parallels between abstracted empiricism and grand theory are clear, for whereas in the former, techniques of research take priority over questions raised by the empirical world, in the latter analysis of empirical problems is displaced by 'an arid game of Concepts' (1959: 43). Mills stands against this 'formalist withdrawal' of theory at all costs. He argues that the key to the development of an imaginative yet systematic and effective sociology lies in our ability to control the level of abstraction at which we work. This means that there can be no universal scheme or system through which problems arising in the empirical world can be understood, for what is needed instead is a 'variety of . . . working models' (1959: 57) as well as a detailed understanding of the historical contexts and complexities of the phenomena under study. Mills returns to this point in a later chapter of *The Sociological Imagination* – 'The Use of History' – where he argues that 'To fulfil their tasks, or even to state them well, social scientists must use the full materials of history . . . All sociology worthy of the name is "historical sociology" ' (1959: 162). He adds that this attention to history is missing from grand

theory and abstracted empiricism alike, for the tendency is to retreat into the 'undue formality' of either abstract concepts or research techniques, and in both cases this blocks the possibility of truly comparative work (for a recent statement on the complex connection of history and sociology, see Savage, 2010).

Mills' critique of grand theory may at first sight seem less far-reaching than his critique of abstracted empiricism. Echoes of Mills' objections to the abstractions of Parsonian thinking, however, can still be detected today in debates over the status and future of theory within sociology. A key point of orientation is Quentin Skinner's *The Return of Grand Theory in the Human Sciences* (1985), which examines the fate of grand theory after Parsons. Skinner opens this book with a brief summary of the argument of *The Sociological Imagination*. He states:

> Mills isolated and castigated two major theoretical traditions which he saw as inimical to the effective development of . . . *The Sociological Imagination*. The first was the tendency – one that he associated in particular with the philosophies of Comte and Marx, Spencer and Weber – to manipulate the evidence of history in such a way as to manufacture 'a trans-historical strait jacket'. But the other and even larger impediment to the progress of the human sciences he labelled Grand Theory, by which he meant the belief that the primary goal of the social disciplines should be that of seeking to construct 'a systematic theory of "the nature of man and society" '. (1985: 3)

Close readers of Mills' work will notice two peculiarities in this reading of *The Sociological Imagination*. The first is that Mills objects to grand theory because it works at too high a level of generality because it fetishizes its concepts (1959: 44). This is *the* basic argument of the second chapter of *The Sociological Imagination*, and one that is strangely absent from Skinner's reading. Skinner's only mention of concepts comes in a brief observation on the relationship between philosophy and 'other cultural disciplines' that characterized postwar scepticism to grand theory: 'A philosopher was taken to be someone whose basic concern is to explicate general concepts by way of analysing the meanings of the terms used to express them' (Skinner, 1985: 4). This argument that concepts belong to the realm of philosophy is a common one and can be found in thinkers as far removed as Isaiah Berlin and Gilles Deleuze (see Gane, 2009). However, in the context of the current paper it is important for it raises the question of the role of concepts within sociological thought, and this is something I will return to below. The second peculiarity of Skinner's reading lies in its separation of trans-historical theoretical work from grand theory. Mills argues that grand theory is problematic because it privileges conceptual formalism over the empirical richness of historical analysis. This is a charge he levels at both grand theorists and abstracted empiricists. He argues: 'Social scientists may – in fact many do – attempt to retreat from history by means of undue formality of Concept and technique. But these attempts require them to make assumptions about the nature of history and of society that are neither fruitful nor true' (Mills, 1959: 174). Contrary to Skinner, this is not a charge that Mills levels at either Weber or Marx, as, quite rightly, he argues that their 'conceptions about society were

closely joined with historical exposition' (Mills, 1959: 58). The problem, for Mills at the time of writing *The Sociological Imagination*, was a quite different one, namely 'the amnesia of the American scholar' and the 'formalist withdrawal' from history of Parsonian sociology in particular.

Skinner's separation of trans-historical sociology from systematic theories of the 'nature of man and society' is not without consequence, for it enables him to define the detailed historical work of Michel Foucault or the *Annales* school as 'grand theory'. This is a little strange given Foucault's attention to local, subversive forms of historical knowledge, along with his call for specific rather than general intellectual work. A similar objection might be made to terming the philosophy of Jacques Derrida (who also appears in this book) grand theory in the Millsian sense. Skinner himself concedes that:

> If there is one feature to all the thinkers I have singled out, it is a willingness to emphasise the importance of the local and the contingent, a desire to underline the extent to which our own concepts and attitudes have been shaped by particular historical circumstances, and a correspondingly strong dislike . . . of all overarching theories and schemes of explanation. (1985:12)

It is not altogether clear that the figures discussed in *The Return of Grand Theory* – including Jürgen Habermas, Thomas Kuhn, Hans-Georg Gadamer and Claude Lévi-Strauss – do indeed share a comparable commitment to 'the local and the contingent'. In any case, how could such commitment be reconciled with theory that is at the same time systematic and grand? For Skinner, the answer is two-fold. First, he argues that these thinkers 'almost in spite of themselves . . . have proved to be among the grandest theorists of common practice throughout a wide range of the social disciplines' (1985: 13). Second, he adds that 'there has been an unashamed return to the deliberate construction of precisely those grand theories of human nature and conduct which Wright Mills and his generation had hoped to outlaw from any central place in the human sciences' (1985: 13). Skinner cites Habermas as an example, for he takes rationality as his central theme – in Millsian terms this is presumably the concept he fetishizes – by developing 'abstract theories of social structure' via Parsons on one hand, and reconstructing historical materialism via Marx on the other (1985: 17). For Skinner, this places Habermas very much within the tradition of grand theory outlined by Mills. This judgement seems well founded, but whether the same can be said of thinkers such as Foucault who do not deal with abstract theories of social structure in any sense is, to say the least, questionable.

This question of whether the thinkers discussed by Skinner are in fact 'grand theorists' in the Millsian sense cannot be pursued at further length here. It is worth noting, however, that this text on the return of grand theory remains influential today. William Outhwaite – who contributed a chapter on Gadamer to Skinner's collection (1985: 21–39) – has recently considered the position of such theory within British sociology. Outhwaite traces the emergence of a new canon of social theory made up of four thinkers who have been 'ascribed a com-

parably prominent superstar role' (2009: 1030): Zygmunt Bauman, Ulrich Beck, Pierre Bourdieu and Anthony Giddens. One might ask why none of the figures previously addressed by Skinner – in particular Foucault and Habermas – have entered into this canon of British sociology. This is a complex question that concerns the historical influence of different types of continental thought on British sociology (see, for example, Rex, 1983), and which cannot be addressed here. Outhwaite touches upon this question only in brief in a footnote (2009: 1039), for his main task instead is to trace the biographical and intellectual trajectories of the above four thinkers by arguing that 'for practical purposes' they 'can be assigned to the same generational cohort' (2009: 130). This idea of a 'generational cohort' is a key point of interest. For Outhwaite, a 'period of 33–35 years' is 'the crucial accounting unit in the history of thought' (2009: 1029). He draws this observation from the work of Randall Collins, who in his *Sociology of Philosophies* argues that 'A 33-year period is the approximate length of an individual's creative work. By the end of that time, a cohort of thinkers will be virtually replaced by a new adult generation' (Collins, 1998: xix). Outhwaite identifies 1971 as the year 'to start the Collins clock' for the formation of the new canon. He explains: 'Bauman has just arrived in Leeds; Bourdieu is well established in Paris and Giddens at Cambridge; Beck is completing his doctorate' (Outhwaite, 2009: 1030). Outhwaite proceeds to give an overview of each thinker's life and works, before asking what appears to be the central evaluative question of this paper: 'Is the return of "grand theory" likely to be permanent?' (2009: 1037). His answer, in the very last line of the conclusion and which is given without any accompanying justification, is yes: 'the style of theorizing' which has been introduced by these four thinkers 'seems to be here to stay' (2009: 1038).

For the purposes of the present paper, the above arguments of Skinner and Outhwaite are useful in at least two respects. First, they place into question the generational shifts that underpin the canonization of social theory; and second, they return us to Mills' question of the fate of conceptual fetishism, or what he called 'grand theory'. First, I will address the former: what might be termed the current *generational* crisis of social theory. It is not altogether clear why, for Outhwaite, 1971 is the key year for the formation of his canon, for at this time Beck, Bauman, Giddens and Bourdieu were not only at different stages in their careers but were also far removed from each other in terms of their theoretical interests and positions (one might add that these four thinkers are 'theorists' in quite different senses of the word). But if we do start the clock at this seemingly arbitrary date, and we accept Collins' view of the lifespan of generational cohorts, then where did things stand in 2004, 33 years on? For if Collins and Outhwaite are right, this should have been the point at which the creative work of these theorists started to fade and was displaced by a younger generation of thinkers. But why did this not happen and why is this canon still so dominant within British sociology? To use Outhwaite's words, why is the grand theory of Bauman, Beck, Bourdieu and Giddens seemingly 'here to stay'?

This question of generational change within sociology, which Mills only touches upon in his remarks on the new methodological recruits to the disci-

pline, and which Outhwaite raises rather than answers, is explored by Andrew Abbott. In a paper on knowledge accumulation within sociology, Abbott (2006) collects data from five leading North American journals in order to determine the age of materials that are still influential within the discipline. In so doing, he raises questions that are not simply confined to the analysis of North American sociology. For Abbott disputes the idea that increasingly 'the old stuff' in sociology is simply forgotten, for his findings are 'inconsistent with the claim that contemporary scholars pay less attention to earlier work than their predecessors. That is . . . I think the glass is half full, not half empty, and that it is getting fuller' (Abbott, 2006: 59). This judgement is questionable, for surely the key point is not simply the extent to which previous sociological resources are drawn upon today, but how such materials are cited and in what context. Transposed into a British setting, for example, is it a sign of the health of the discipline that the four canonical theorists named by Outhwaite are cited so commonly within mainstream contemporary sociological work? Or might this indicate the absence of a new generation of theoretical sociologists that should by now have displaced the existing canon? Abbott, like Collins, has his own vision of generational change, and argues that the lifespan of most sociological literatures or careers is roughly 25 years. He states that this:

> . . . is about the length of time it takes a single group of individuals to make up some new ideas, seize the soap-boxes, train a generation or two of students, and finally settle into late career exhaustion. Their students may keep things going, but their students' students tend to be fairly mechanical appliers of the original insights. The really creative people don't make their careers by hitching themselves to other people's wagons. (2006: 61)

Setting aside questions of whether Outhwaite, Collins and Abbott are right to tie generational cohorts and generational change to rigid time-spans (which in itself is highly debatable), the following question might be posed to Abbott by way of response: where are the 'really creative people' who are thinking beyond the canon outlined by Outhwaite or even Skinner before him? To put this bluntly, and to paraphrase Rosemary Crompton's complaint regarding the current absence of methodological expertise: what has happened to the next generation of theoretical talent within the discipline?

One answer is offered by Steve Fuller, who in many ways echoes Mills in arguing that the scope for creativity and imagination in sociology is today constrained by audit measures such as the Research Assessment Exercise (RAE) that alienate 'ideas from the thinker' by reducing them to 'outputs' that are judged by their market value. Such measures, at least in the UK, centre increasingly on the 'impact' of published work, and promote the assumption that there is a direct correlation between the mass citation and intrinsic worth of sociological research. For Fuller, what is disturbing about this development is that it tends to bolster the existing canon at the expense of risk-taking – what he calls 'bullshit' (see Fuller, 2009: 143–163) – and more broadly the production of anything qualitatively new:

> the problem with this practice is not what it says about the relatively few who receive the lion's share of citations – but the many more who prop up these market leaders by citing them so much. It fosters a dependency culture whereby academics are rewarded for feats of ventriloquism, that is, an ability to speak through the authority of others. The result is institutionalized cowardice. (Fuller, 2009: 86)

This might, at least in part, explain the citation of what Abbott calls 'old stuff' in his survey of North American journals, along with the continued stability of Outhwaite's canon. For increasingly the social sciences are dominated in these settings (the global picture is undoubtedly more complex) by an institutional culture that privileges the repetition of the same, both in terms of citation practices and the content of outputs, over work that genuinely changes the rules of the game and with this the game itself (what Lyotard might once have called parology). Hence, Abbott's 'really creative people' that refuse to hitch themselves 'to other people's wagons' are increasingly difficult to find. To understand exactly why this is the case, further sociological work is needed on the audit measures and inscription practices that underpin this new bureaucracy of the intellect. Such work might ask the following: are particular academic journals and publishing houses privileged within these audit measures, and if so, why? Is it a sign of the health of the discipline when the volume of published outputs of individual scholars is rewarded regardless of their thematic and analytic repetition? Can the value of sociological work legitimately be measured in terms of its 'impact'? And what is the future of the research monograph (historically the primary medium for theoretical work)? Such questions, which will find different answers in different national contexts, force us to think further about both the institutional underpinnings of the discipline, as well as the presentational forms of sociology and the media of its transmission. These latter questions are particularly important if it remains an aspiration of the discipline to have any kind of 'public' presence or audience (see Back, 2007: 160–161).

Such questions cannot be addressed at any length here, but nonetheless they point to an underlying politics of publishing and citation that is neglected by Abbott. This politics is tied to broader questions concerning the continuing promise or value of social theory, and of what *value* itself is or potentially can be outside of institutional or quantitative notions of measurement. One might ask, for example, of the underlying politics of audit measures such as the RAE/REF in the UK, and the impact of such measures upon the ambition, imagination and scope of sociological research over the past 20 years. Fuller is forceful on this point, for he declares that such measures encourage a narrowness of mind because they reward scholars not for taking risks but for publishing repeatedly in specialist research areas. For Fuller, the consequence of this development is alarming: 'Ask even a distinguished professor something related to but slightly off his official topic and you might as well have flashed your headlights at an innocent deer' (2009: 84). What is surprising, given that classification is a major point of critical interest within sociology, is that the politics and

consequences of audit measures such as the RAE have so rarely been questioned from within the discipline (exceptions are Fraser, 2009; Burrows and Kelly in the present volume). Les Back (2010) rightly observes that 'there is a timidity and cynicism that hangs over the anxiety ridden sociological habitus preoccupied as it is with the ever changing measure of intellectual value and audit'. This is perhaps set to change in the face of the crisis of funding that presently looms over social science and the humanities in the UK, for questions regarding the distribution of resources within the academy, or what Mills called the politics and economics of truth, are increasingly taking centre-stage.[3] With this, theoretical questions regarding the underlying politics of knowledge production and the commodification of intellectual work are likely to make a return, including those identified by Lyotard over 30 years ago: 'who decides what knowledge is, and who knows what needs to be decided?' (1984: 9).

A further pressing question concerns the relation of sociology to its fellow disciplines – again something that Mills touches upon in his *Sociological Imagination*. As stated above, the year 2004, according to Outhwaite's clock, should have seen the existing canon of social theory come to an end. This does not seem to have happened, but at the same time it depends on where one looks, and on what is to count as 'social' theory, for some of the most powerful exercises of a sociological imagination are to be found at the institutional margins or even outside of the discipline as it is commonly recognized (equally, it might be argued there are academics who work in sociology departments who would not consider themselves to be 'sociologists' in any conventional sense). A key consideration is of the status of the discipline in relation to its others, for sociology has been characterized variously as an 'exporter discipline' (Holmwood, 2010) and as a 'parasite' (Urry, 2002). There are also important questions to be addressed about whether the boundaries of social theory should be relaxed, or whether the discipline should look inwards to ask of its own core aims, grounds and objectives in a bid to (re-)define its future. Against the backdrop of such questions, the early 2000s are perhaps more interesting than they might at first seem, for while they did not bring the collapse of the paradigm outlined by Outhwaite, they did see the publication of three very different works that questioned the future prospects for theoretical work: Eagleton's *After Theory* (2004), Butler, Guillory and Thomas's *What's Left of Theory?* (2000) and my own *The Future of Social Theory* (Gane, 2004). The first of these texts questions the ambitions of cultural theory after its 'golden age'; the second reflects on the status of literary theory following the apparent demise of the Left; and the third draws together key thinkers from sociology, cultural studies and philosophy to think again about what the concept of 'the social' might mean today (for a more historical reading of this concept, see also Joyce, 2002). These three texts are quite different in terms of their aims and scope, but are united by their questioning of the social and the cultural through the exercise of a conceptual vocabulary and theoretical imagination that cannot be found exclusively within sociology (a related and pressing question is of what counts as 'theory' across different disciplinary domains; see Hunter, 2006). This is nothing new, for some of the

most important works of social theory of the late 20th century were forged out of disenchantment with the disciplinary and institutional limitations of sociology: in women's studies, cultural studies and (new) media studies. Mills rightly reminds us that the sociological imagination is routinely at work outside of both sociology and the academy. Given this, it would perhaps be worth returning to these disciplinary and institutional boundaries to ask what type of theoretical work it is possible to produce within the discipline today, and why mainstream sociology, despite its stated intentions, often remains unwilling to listen to its disciplinary others?

In an oblique way, Outhwaite's recent paper, while focusing on a predominantly British canon, leaves us with the question of what is left of theory within sociology. By way of response, it might be argued that social theory, at least as it is found in the mainstream, is passing through something of a generational crisis. Outhwaite's canon is living on beyond its expected, and some might argue useful, lifespan. It is, to borrow Beck's own phrase, a *zombie* canon. Increasingly, theoretical ideas, and more specifically sociological concepts, are drawn in a ready-made fashion from the writings of a select body of thinkers rather than being re-forged or invented anew. This fetishism of the concept – be it risk, reflexivity or liquidity – lies at the heart of Mills' critique of grand theory, and is the key part of his argument missed by Skinner. Such work blocks rather than exercises the sociological imagination because it starts with a meta-concept or process that is then stamped on every aspect of the so-called 'empirical' worlds under study. A common practice is to invent a new type of 'society' or 'modernity' under which all empirical details and complexities can be subsumed: 'risk' society (Beck), 'network' or 'information' society (Castells), 'liquid' modernity (Bauman), 'high' or 'late' modernity (Giddens), to name but a few. In each case, these leading concepts are fetishized in the Millsian sense.[4] Castells (who I would add to Outhwaite's canon) talks, for example, of network or information society but says little about the empirical complexities of either networks or information (see Gane and Beer, 2008: 15–33). A similar charge might be levelled at Beck or Bauman's theory of individualization, which sees a shift of powers and responsibilities down from the state to the individual, but says little about the emergence of new authoritarian state (bio-)powers over territories or populations post-9/11, or the intricate connections between the state and market capitalism that have emerged through the recent financial crisis (see Gane, 2012). Others, meanwhile, have taken issue with the thin concept of reflexivity that underpins such thinking as well as the structural constraints of class that these meta-theories tend to downplay (see Atkinson, 2007, 2008). In each of these cases, such grand theoretical work starts with a meta-concept or process – be it risk or liquidity – that frames all subsequent analysis and understanding of 'empirical' events or examples. In the writings of Bauman, for example, there is little attention to basic questions concerning the when, where and who of 'liquid modernity'. This a-historicism or trans-historicism, contra Skinner, is a central feature of what Mills calls grand theory, and for this reason Mills' cri-

tique of Parsons is still of contemporary significance even if interest in Parsons' work per se has long since faded. This leaves us in turn with Outhwaite's question of whether grand theory is here to stay. This question is impossible to answer, but by way of response one might instead ask the following: what would it take for this (grand) theoretical canon to be displaced? One answer, in part inspired by Mills, is that sociology needs to renew its theoretical imagination. For this to happen, flexible and inventive conceptual work is needed that addresses the pressing empirical demands of the day while at the same time retaining a sense of historicity and scale (which Howard Becker has long-argued that British sociology in particular lacks, see Becker in Mullan, 1987: 140). Such work would give the discipline new tools for thinking, and would enable the 'zombie' concepts of existing cannons to be refined, reinvented or finally laid to rest, depending on the problem in hand. This, I would argue, is a key step in re-animating the sociological imagination, even if it is not one that is core to Mills' own concerns. Indeed, it is perhaps here, with the intricacies of concepts, in particular their formation and displacement, that we reach the limits of Mills' text.

Concluding remarks

The paper has attempted to draw into question some of the problems of contemporary sociology through a re-reading of a classic but in many ways neglected text: Mills' *Sociological Imagination*. One of the appealing features of this text is not simply its attention to the connections between biography and history, as commonly stated, but also its ability to work beyond an apparent impasse between sociological method and theory: between abstracted empiricism, on the one hand, and grand theory on the other. For Mills, parallel problems run through these approaches, as the former proceeds through the reduction of methods to a set of preconceived techniques, and the latter through trans-historical abstraction and the fetishism of concepts. The argument of the present paper is that Mills' double-pronged attack against the then prevalent forms of method and theory is instructive today, for it suggests how we might move beyond some of the current limitations of the discipline. One such limitation is the widespread stand-off between theoreticians and methodologists, some of whom go as far as blaming each other for the demise of sociology. Goldthorpe, for example, writes: 'there is a manifest lack of integration of research and theory . . . Here, I believe, chief responsibility has to lie with theorists . . .' (2000: 2). Mills offers his own provocation, for he argues that the fetishization of either theory or method is equally mistaken. In so doing, he returns us to primary questions of what is meant by 'the empirical', and what sociology is or can be as an empirical discipline. These issues, which have been the subject of renewed interest (see Adkins and Lury, 2009), arise primarily because Mills treats theory and method as second-order questions that only come into play following an

initial encounter with an empirical problem. This takes him close to the radical empiricism of more fashionable thinkers such as Deleuze (see Fraser, 2009; Gane, 2009), who not only draws on Hume to think about 'the flux of the sensible', but also asserts the role of imagination and mind in forging a 'collection of impressions and images, or a set of perceptions' out of raw empirical data (Deleuze, 1991: 87). It is precisely at this point, for both Deleuze and Mills, that the work of philosophy and sociology respectively – through the subsequent forging of concepts and methods – begins.

Working in the spirit of Mills, Les Back argues that for sociology to be attentive to the empirical it must develop an art of listening that takes us beyond existing methodological and theoretical dogmas. Furthermore, he suggests that literary practices of inscription are needed to bring sociology alive, for too often sociologists 'swim unnoticed at the shallow end of the literary pond' (Back, 2007: 177). This idea of a 'literary sociology' (see Back, 2007: 164) is intriguing in the light of Abbott's (2007) recent call for a 'lyrical' sociology, which itself is far removed from poetic forms of sociology that reside at the limits of the discipline (see, for example, Baudrillard, 1990). I would add to this, however, that concept formation must also lie at the heart of empirical sociology. At first sight, Mills and Deleuze appear to depart on this point, for whereas Mills is reticent to see sociology reduced to 'an arid game of Concepts' (1959: 43), Deleuze argues that concepts are indispensible bridges between empirical reality and its presentation in thought. But to be clear: Mills' objection is to work that 'sets forth a realm of concepts' (1959: 44) that are then elevated to such a level that they then outrun 'any specific and empirical problem' (1959: 58). This means that concepts, like methods, should be developed out of, and in connection to, the complexities of the empirical world (the recent attempt to formulate 'mobile methods' is interesting in this respect, see Büscher *et al.*, 2010). Concepts should not be static, abstract devices that are devised and deployed in any general or universal way. This, for Mills, is exactly the problem of grand theory, where they become little more than 'sponge-words' (1959: 53): weak analytic descriptors or tools that do little more than absorb the energy of the worlds they are attempting to study. This crisis of concepts lies at the heart of grand theory, and runs parallel to the crisis of measurement that today haunts abstracted forms of empiricism. Mills responds by reminding us that the task in both cases is not to festishize the ready-made, but rather to invent or re-forge useful analytical devices and practical techniques out of the empirical problems that we face. This is an ongoing and difficult challenge. It may involve breathing new life into concepts that currently lie neglected, including, for example, the concept of 'value': a concept that was once central to Marxist and Weberian sociologies. For in the face of renewed institutional pressures for sociology to justify itself, one might ask again what value is outside of quantitative notions of measurement? In asking such questions, we return to where Mills begins: with the promise of sociological work. For in the face of the crises outlined in this paper, this question of the promise or *value* of the discipline requires renewed attention.

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Notes

1 ESRC International Benchmarking Review, available at: http://www.esrcsocietytoday.ac.uk/ESRCInfoCentre/Images/Sociology%20IBR%20Report_tcm6-36279.pdf
2 See http://hereview.independent.gov.uk/hereview/report/
3 This is especially the case in the UK following the release of the recent Higher Education White Paper, see http://www.bis.gov.uk/assets/biscore/higher-education/docs/h/11-944-higher-education-students-at-heart-of-system.pdf
4 A figure not dealt with in the course of this paper is Bruno Latour. Latour's work, while deeply influential in the sphere of science and technology studies, is not part of the current canon of mainstream sociology, in part because it has been concerned predominantly with the pursuit of an anthropology of science, but perhaps also because it has refused, on the one hand, the Durkheimian formulation of the social as society, on the other Marxist conceptions of the commodity, fetishism and reification (see Latour in Gane, 2004: 77–81). Instead, like Deleuze and Guattari (1987: 218–219), Latour turns to the work of Gabriel Tarde in an attempt to explore the social as a form and practice of association. This may be read as an attempt by Latour to resist the fetishization of concepts by calling into question their continued empirical purchase (see Gane and Beer, 2008: 27–31). The most famous instance of this is where Latour questions the canon of actor-network theory that his own work helped to found: 'there are four things that do not work with actor-network theory: the word actor, the word network, the word theory and the hyphen!' (Latour, 1999: 15). Two observations might be made here. First, Latour's disavowal of actor-network theory by no means signalled the end of this type of work, which in many ways continues to be fetishized by its followers. Second, Latour's subsequent writings have had, at least to date, comparatively little impact on the discipline (see, for example, Latour 2004, 2011). One might draw an unlikely comparison here with the work of Jean Baudrillard, which beyond its attention to hyperreality, simulation and war has barely been read (see, for example, Baudrillard, 2001, 2002, 2003). In both cases, what appealed to the sociological mainstream was a ready-made theoretical or methodological framework with related concepts that could be deployed to a range of empirical situations: in the case of Latour this is actor-network theory and an anthropology of the modern, and in Baudrillard it is symbolic exchange and four orders of simulacra. As soon as both thinkers moved on to think respectively about the politics of nature (Latour) and singularity and alternative forms of exchange (Baudrillard) their mainstream sociological popularity and circulation faded. This situation can perhaps be read as an example of the continuing methodological and theoretical timidity of the discipline.

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