

DEBATE

Gender and Critical Realism: A Critique of Sayer

John Holmwood

School of Social Sciences
University of Sussex

ABSTRACT In a recent article in this journal, Andrew Sayer has argued that much feminist research on the gendered nature of organisations, such as bureaucracy and the market, confuses a contingent association of gender and organisational forms with a stronger claim that they are intrinsically gendered. Sayer accepts that this research has shown that the empirically found, concrete forms of organisations are gendered. However, deeper theoretical reflection, he suggests, reveals that, when considered as ‘abstract realist models’, bureaucracy and the market are, in fact, identity-blind. He makes two claims, one concerned with explanation, the other concerned with the political consequences of social inquiry. The first is that the construction of abstract models, rather than the ‘associational’ thinking concerned with the delineation of empirical regularities, is necessary to the proper understanding of the operation of causal mechanisms and their mode of determination in social life. The second is that this will enable a more progressive and positive politics beyond a fatalism which he attributes to associational thinking. This paper takes issue with both claims arguing that the abstract theory he defends has no positive role in social inquiry and that his political critique is misplaced.

KEYWORDS Bureaucracy, gender, lifeworld, market, realism, system.

Sayer (2000) argues that his critique of feminist research is concerned with wider issues of the nature of social inquiry.¹ In essence, he defends a realist ontology for social inquiry where social structures are understood as having causal powers, but where these powers need to be understood in their ‘pure’ forms, abstracted from the contingencies of their normal operation in contexts where other structures operate (Bhaskar 1979, 1986; Sayer 1984; Archer *et al.* 1998). For Sayer, *necessity* is to be understood as an issue of the way in which entities are conceptualised and should not be confused with the issue of the actual occurrence of any effects attributed to their causal mechanisms. Feminist research stands accused of precisely this kind of confusion. Thus, he writes of his article (2000:708) that:

while the main focus is ... a set of issues about how we interpret substantive phenomena like markets and bureaucracies, the paper tackles this through a critique of what I call ‘associational thinking’, that is a form of analysis which attaches significance to empirical associations or regularities according to their pervasiveness rather than according to their necessity, and which is resistant to abstraction in social science.

Although Sayer's critique of 'associational thinking' can be applied to research which is not specifically feminist in its orientation, there is particular significance to the fact that he chooses to direct it against *feminist* research. After all, feminists have frequently criticised abstraction as a masculine orientation to theory and, on the face of it, Sayer's defence of abstraction looks like a standard response to such critiques. It would seem that Sayer accepts that sociological research agendas have been transformed by feminist research and demonstrations of the empirical significance of gender, since he does not deny the empirical regularities identified in feminist research (and he does not claim that the nature and pattern of those regularities were known in advance of that research). At the same time, however, he argues that underlying and fundamental theoretical principles are unaffected by this new knowledge.

From the perspective of Sayer, and others who defend abstract theory, it is not only markets and bureaucracy that are identity-blind, but also theory itself; each of these domains is only contingently gendered. The domain of theoretical discourse is conceived to be *meta-theoretical* and, as such, to be independent of empirical research (whether feminist or not). Whatever the differences between them, these arguments are characteristic of other proponents of general theory in sociology such as Alexander (1982, 1988), Giddens (1976, 1984), Habermas (1984, 1987), Archer (1988, 1995) and Mouzelis (1990, 1995 and 2000, the latter in the same issue of *Sociology* as the article by Sayer). It is precisely this type of argument that has led some feminists to refer to a 'missing feminist revolution in sociology' (Stacey and Thorne 1985) and others (Stanley 2000) to complain of a gender-neutral conception of theoretical discourse in sociology, largely the preserve of male theorists, that just happens to find no necessary reason to engage in dialogue with (or, at least, be open to the possibility of change in response to) feminist theory and research.

Ironically, a significant aspect of this general meta-theoretical argument in sociology is the emphasis upon reflexivity and the claim that theory must be conceived as a 'dialogue', in contrast to the 'monologic' character of positivistic approaches (Habermas 1984). The relation between sociological argument and feminism (conceived as a movement which is both within and outwith the academy) that Sayer's article addresses is an opportunity to assess the nature of this dialogue. I shall return to a direct discussion of the issue at the end of this paper, but I should state at the outset that I do not presume any particular outcome of a dialogue; if a feminist critique of sociology is possible, so, too, would be a sociological critique of feminism. What I am concerned with in this paper is how the metatheoretical claims of social theory function to set the terms of any dialogue to the advantage of one of the parties. In this context, it is significant that Sayer cites Hartmann's (1981) article on the 'unhappy marriage of Marxism and feminism', but neglects her critique of the way in which feminism was always rendered subordinate in the partnership. I shall suggest that Sayer reproduces the same subordination.²

Abstract versus associational thinking

The general metatheoretical argument is frequently supported by a self-consciously *postpositivist* understanding of explanation. According to positivism, explanation should proceed in an 'accumulationist' way, gathering intelligence of external objects and discovering increasingly accurate knowledge of their interrelationships. For positivists, the truth of objects is the substance of their positive transcendence and the advance of science consists in a process by which observations beyond current comprehension are absorbed to understanding in the extension of theory. However, within the philosophy and history of science this view of science has come under increasing challenge as an account of scientific development. For example, as Kuhn (1962) argued, scientific theories did not seem to offer themselves for critical tests and 'anomalies', were frequently 'explained away' by *ad hoc* modifications, rather than being allowed to falsify the theories in which they arose. Notwithstanding, science continued, for the most part, to be seen as progressive and successful, in the sense of generating new extensions, new insights and new explanatory resources. From a postpositivist perspective, these were increasingly understood to occur by *reconstructing*, rather than accumulating, 'truths'. In that reconstructions re-order 'truths' (including their accepted 'facts'), science appears discontinuous in terms both of its categories and objects and as organised into 'paradigms' (Kuhn 1962) or 'research programmes' (Lakatos 1970).

There is no single postpositivist position which has replaced the once dominant positivist conception of science and the debate is certainly not closed. For many commentators, and for realists in particular, the danger of postpositivism is that, by emphasising that observations are theory-dependent, it can seem to allow a dangerous and debilitating relativism. In some way, it is argued, an external reality must impinge as a discipline upon the construction of theory. This is the background concern of Sayer's critical realism and it is how it is worked through from an understanding of the natural sciences into social inquiry that informs his critique of feminism.

According to critical realists, positivism (and empiricism) is based upon a mistaken ontology, one which is too 'flat' and fails to recognise the ontological 'depth' – or stratification – of the world as being made up of many causal mechanisms and their associated hierarchies (Bhaskar 1979, 1986; Sayer 1984; Archer *et al.* 1998). Where there are multiple mechanisms, the effects of any causal mechanism may not occur because they are cancelled out, or otherwise distorted, by the operation of another mechanism. Thus, realists argue that it is necessary to distinguish between the 'real' and the 'actual' since underlying causal mechanisms can be understood as 'real' even where they are not 'actualised'.³

What Sayer calls 'associational' thinking, then, is a form of empiricism which fails to recognise the distinction between the real and the actual. Associational thinking

would, thereby, mistakenly regard the non-occurrence of 'necessary effects' as an indication of the falsity of a theoretical claim – for example, the gendered nature of concrete forms of organisation would be regarded as indicating the falsity of any claim of their gender-neutral, or impersonal, character. By extension, associational thinking is also mistakenly inclined to regard found conjunctions of effects or events (again, such as gender and concrete forms of organisation) as 'necessary' when they may be no more than the contingent associations of different structures whose 'real' tendencies are quite other than might be inferred from mere empirical conjunctions. The task of abstract thinking is precisely to identify these 'real' structures, by abstracting from the contingent conjunctions which mask their operation. For Sayer, then, the feminist research that he criticises is truly a form of empiricism, as some – for example, Harding (1986) – even within feminism, have alleged.⁴

There is, however, something paradoxical about this sort of argument. The emergence of postpositivist philosophy of science (including realism), as I have argued, is usually held to be necessary in order to understand the way in which theoretical objects and relations are reconstructed and transformed in the problem-solving activities of science. To be sure, this involves a different understanding of theory (involving the theory-dependence of observation, etc.) than is the case in earlier, positivist accounts. However, theory – 'paradigms' (Kuhn 1962), the 'hard core' of 'research programmes' (Lakatos 1970), or whatever – is precisely what is transformed in scientific research activities. Indeed, when Bhaskar (1978) argues for an ontological realism grounded in a conception of the real objects of science as unchanging (intransitive) objects external to scientific accounts, he does so precisely because he is aware of the changing (transitive), historical nature of scientific knowledge of those objects. He believes a conception of external, real objects to be necessary precisely in order to render the processes of change in scientific knowledge intelligible.

On this conception, natural science, it seems, changes in relation to its reconstructions of understandings about real underlying mechanisms. These are assumed to exist outside theoretical representations of them, but can never be known except within theoretical representations which are subject to change in the development of science. In contrast, in Sayer's critique of feminist research what is being claimed is that research agendas can be transformed *without* a reconstruction of fundamental categories. If Sayer's distinction between 'abstraction' and 'associational thinking' is accepted, it would seem that theoretical categories in social inquiry can be protected from the negative implications of research findings simply by designating these as occurring at a 'lower-level' of empirical analysis at which 'higher-level' theoretical categories could not be called into question.

Even if we were to accept the force of the distinction between the 'real' and the 'actual', we would have to accept that *sometimes* the observed non-actualisation of effects might be an indication of the *mis-recognition* of underlying causal

mechanisms, rather than deriving from countervailing tendencies generated by the operation of other relevant mechanisms. The non-occurrence of ‘necessary effects’ *might* indicate that the claimed mechanism was false. Falsification is certainly not straightforward, but it must be a possibility. In other words, if we have misidentified a causal mechanism, its tendencies will not be *real* and that error of identification will be the explanation of the non-occurrence of anticipated effects. If misrecognition is a possibility, the task will then be to transform understandings in order correctly to identify the appropriate causal mechanism. Of course, ‘misrecognition’ on the part of other social actors, and the transformation of their understandings consequent upon a proper recognition of their circumstances, is presented as what makes possible sociology as a *critical* undertaking, or what constitutes realist social science as *critical realism*. For example, Margaret Archer writes that critical realists are ‘particularly concerned to explain how [the] capitalist market can be mis-recognised in popular understanding through ideological manipulation’ (2000:470). The possibility must be that the market can also be misrecognised in the accounts of social theorists. Wherever there is an issue of misrecognition in competing accounts, those of social theorists and those attributed to actors, there must be a *possibility* that the problem lies with the former, or any claim of ‘dialogue’ would ring hollow.

In circumstances where theorists were inclined to accept that the problem of understanding was *theirs*, a ‘conjunction of effects’ would be one of the indications of how their understandings might be reconstructed. We could still allow that the task of research must be to move beyond conjunctions to reconstruct theoretical understandings and provide an account of explanatory mechanisms. More importantly, any reconstruction is likely to re-define and re-specify the identification of effects just because the prior understandings of mechanisms (of objects and relations), through which their initial identification as divergent effects was made (and which made us aware of our problem of understanding), have been transformed. This is what is meant by the theory-dependence of observations. Where theory is reconstructed, so, too, will observations be reconstructed. What does not fit with pre-existing explanatory claims will be re-defined in the process of being fitted to new explanatory schemes. However, this is precisely what Sayer seems to deny in his discussion of feminist research, since it must make it possible that gender might be *intrinsic* to the understanding of the causal mechanisms of bureaucracy or the market. At a minimum, his argument must be that the boundaries of stratified domains are given independently of any research activity such that any reconstruction of explanatory mechanisms leaves pre-existing boundaries in place. In the next section of the paper I shall address his arguments that such boundaries can be established as a matter of theoretical reasoning independently of any specific programme of research.

From ideal types to general theory

In the absence of any statement of what would count to call into question any claim to have identified a causal mechanism, realism in the social sciences (despite the qualifying term ‘critical’) would seem to serve a conservative approach to theory, where theory, apparently, can both inform research and be beyond research (in the sense of not being subject to transformation in research practices). This latter claim is quite at odds with postpositivist understandings of science and mobilises a quite different sensibility. In the realist understanding of natural science, the claim put forward, for example by Bhaskar (1978), is that the research activity of experimentation can create a ‘closed system’ in which causal mechanisms can be established, or falsified. What would be the analogue for social science?⁵ When Sayer justifies abstraction in social scientific constructs as an alternative, he does so by arguing that their *counterfactual* status can establish necessity. If it can, then the form of necessity is logical, or *conceptual*, established by definition, rather than by research. The question that might be posed is: how is what is logically or conceptually necessary, to be established? The answer whenever such a question is posed is usually that it is a matter of the necessary form of sociological reason. This frequently involves the claim that a general frame of reference which precedes substantive inquiries is a presupposition of social inquiry and this sort of argument has a general currency beyond realism (see, for example, Alexander 1982, 1988; Giddens 1976, 1984; Habermas 1984, 1987; Archer 1988, 1995; and Mouzelis 1990, 1995).

In common with other theorists, Sayer develops his argument by paying obeisance to Weber. He claims kinship with Weber’s argument that social scientific constructs are ‘ideal types’. Elsewhere, Sayer had recognised that falsification is a necessary issue, and he believed that one of the flaws of ideal type methodology was that, ‘it has a built-in protection from refutation’ (1984:238). Not only this, but ideal types were also understood to betray associational thinking and, ‘the arbitrary freezing of contingent patterns, regardless of the structures that produce them’ (1984:238). In addition, Weber called his constructs ‘fictions’ and denied that they could be regarded as establishing ‘real forces’, something which any realist, apparently, must reject (Outhwaite 1983). Now, Sayer expresses himself more sympathetic to ideal types, at least as Weber used them. ‘In practice,’ argues Sayer, ‘what Weber came up with went beyond his ideal type model and was more like a realist abstract model, albeit, as we suggest a flawed one’ (2000: 723). This is because Weber ‘did not select out just *any* features of situations, but those which hung together, were interdependent, and perhaps even necessarily related, while he generally left out those relations which are only contingently related to others’ (2000:723).

What Sayer evidently has in mind as an approximation to an appropriate realist abstract model is the ideal type functioning as what Weber calls the ‘idea’ of historical

phenomena, say bureaucracy or the commodity-market, where the 'conceptual pattern brings together certain relationships and events of historical life into a complex, which is conceived as an internally consistent system' (1949:90). Weber calls the resulting concept, or idea, a 'utopia' because, 'in its conceptual purity, this mental construct cannot be found empirically anywhere in reality' (1949:90). However, the construct must refer to historically specific events, since the particularity of a social structure, say bureaucracy, when compared with a natural structure is precisely that it is not found everywhere. *Social* structures, Sayer argues, vary over time and space, and 'unlike atoms, such objects have histories and geographies' (1984:145). An ideal type, then, must refer to *some* historically specific constellations of events, while, at the same time, being a 'one-sided' abstraction from those events, such that in its purity, it 'cannot be found empirically anywhere'.

The two aspects of the argument are in some degree of tension. Empirical association, apparently, is a guide to the kind of things of which we might wish to form theoretical representations. However, those representations are one-sided accentuations. Their lack of reference in specific cases does not, Weber argues, render any representation invalid.⁶ It remains as a 'conceptual possibility', as an idea that could be realised. Indeed, it is precisely because events in social life are changing as a consequence of the active intervention of human beings (that social structures are not natural structures) that justifies Weber in saying that social scientific constructs must also utilise axioms about action – for example, what would be rational in specific circumstances – while actors may behave 'irrationally', or 'non-rationally' choose other 'ideas', such that the putatively rational is not actualised.

Sayer seems to be saying quite simply that because we can represent bureaucracy (or the commodity market), 'in its conceptual purity', as identity-blind, this is the causal necessity which is to be attributed to its 'abstract idea'; any regular conjunction of organisation and gender is a reflection of the contingencies of social life. Of course, Sayer must reject the other side of Weber's argument that because any 'idea' is an 'abstraction', other 'abstractions', say of patriarchy, which draw upon other associations and seek to represent *them* as an 'internally consistent system', are equally possible, and would have a similar status. Indeed, Hekman (1997) has suggested that the methodology of ideal types can serve a feminist standpoint precisely because of this feature. Equally, Sayer must reject Weber's suggestion that precisely because they are abstractions, ideal types can serve 'heuristic' purposes as a source of hypotheses, because that would imply further development (including reconstruction) of any construct, precisely in the light of its heuristic function.⁷

Sayer attempts to resolve the tension in Weber's argument (truly it is a tension in his own as well), to go beyond its flaws, by arguing for a deeper underlying theoretical logic to a conception of 'realist abstract models', than he believes is provided by 'ideal types'. Paradoxically, this involves an inversion of the realist understanding of science in the move from natural science to social inquiry. In the former, realism seeks to

establish that the underlying objects of science are *unchanging* in order to make sense of the *activity* of science as *changing*. In contrast, social inquiry, it seems, must be concerned with *activities*, that is with historically changing objects, while its underlying theoretical logic is presented as *unchanging*. The argument seems to be that there is a logic of theoretical elaboration that can be based upon what is common to all social events – that they are all the product of action. The elaboration of this general statement of action is what produces a distinction between *system* and *lifeworld* which, for Sayer, serves to define (and confine) the proper domain of gender.

System and lifeworld as a necessary distinction

In setting out the distinction between mere empirical association and necessity, Sayer makes the further claim that, ‘problems such as those posed in my opening questions cannot be adequately addressed without using counterfactual thinking and abstraction and without distinguishing system from lifeworld’ (2000: 708). The latter distinction is, of course, taken from Habermas. He, in turn, derives it from Weber’s distinction between ‘pure types’ of instrumentally-rational action (what Habermas terms, strategic, or purposively-rational, action) and value-rational action (or, communicative action). Habermas (1984) challenges Weber’s methodological individualism, or, as he terms it, ‘subjectivism’, introducing the idea of two distinct domains. One is the domain of *systems* of strategic action, while the other is the domain of intersubjectively established meanings constitutive of the *lifeworld*.

I do not want to get into the details of Habermas’s arguments which have been criticised elsewhere (Holmwood and Stewart 1991). What I want to do here is consider how Sayer uses this conceptual distinction. In essence, he argues that it is the system side of the distinction which is the domain of a particular kind of ‘causal necessity’, while the lifeworld refers to subjective experience and the contingencies of social life. Thus, he writes (p. 710),

the ‘lifeworld’ refers both to the world as given in experience and to the relatively informal aspects of life which are contrasted to administrative and market systems. It is a product of the relation between embodied actors and the cultures into which they are socialised, though it can of course become an object of reflection by actors.

In contrast to this, *systems*, he writes (p. 711),

are relatively formal and have a logic and momentum of their own which go beyond the subjective experience of actors, in so far as they impart a formal logic to action, and through their interlacing and steering of the consequences of actions, whether intended or unintended.

According to Sayer, this distinction between system and lifeworld is *analytic* not *concrete*. What that means is that, analytically, bureaucracy is to be understood in

terms of the logic of systems, while giving due recognition that concretely, it will always be embedded and embodied in the lifeworld. Gender is a phenomenon of the lifeworld and, thus, concrete bureaucracies will be ‘gendered’, but they could not be intrinsically gendered precisely to the extent that the distinction between system and lifeworld makes sense. In so far as it does, concrete bureaucracies are also defined by an identity-blind logic of systems. Thus, Sayer writes, ‘To the extent that systems operate beyond the influence of the lifeworld, they can be argued to be identity-blind, to operate “without regard for persons”’ (2000:711). Although the lifeworld may be characterised by ‘hard and durable structures’ (2000: 712), these are stable patterns which, apparently, do not express causal necessity, or at least, it must be a different kind of causal necessity from that of systems. Sayer writes,

thus gender relations may form durable structures and have what in everyday language might be termed a ‘systematic’ character, but they do not have the characteristics of systems as defined above, even though they may be *associated* with systems in organisations and indeed gain reinforcement from system relations.

It is the *logic* of social theory which requires that ‘patriarchy’ cannot be conceived as a ‘system’; ‘patriarchy would not qualify as a system despite being called one under the looser definition implicit in discussions of “dual systems” theory’ (2000:711). However, given that systems are argued to be ‘identity-blind’, it is difficult to see how ‘durable structures’ in the lifeworld could gain reinforcement from them. The ‘causal necessity’ of systems must always tend to dissolve ‘identity-bound’ structures – or just what is causal and necessary about such systems – unless, that is, there is a single system, undivided by supposedly different ‘logics’ of system and lifeworld.

I shall return to a more formal discussion of the categories of system and lifeworld, but first I want to address the specific examples of bureaucracy and markets to show that neither is fruitfully approached via a distinction between system and lifeworld and that the distinction itself is deeply problematic. I shall begin with a brief treatment of Sayer’s example of bureaucracy, and go on to a more detailed treatment of the market, which also contains the clear implication of the theoretical priority of class over gender.

The example of bureaucracy raises issues wider than those of feminist critiques. Indeed, a quite extensive literature in the 1950s and 1960s challenged the representation of bureaucracy – or formal organisation as it was more frequently termed – as a technically-efficient and rule-governed organisation. Many writers, from a variety of perspectives, emphasised informal organisation and taken-for-granted meanings that were necessary to the day-to-day operation of any complex organisation (see, for example, Blau 1955; Gouldner 1954; Crozier 1964; Garfinkel 1967). Sayer seems to acknowledge some of this kind of argument in his comment that ‘bureaucracies need some degree of support from non-bureaucratic forms of organisation, particularly in terms of *ad hoc* action and interpersonal relations among members of such organisations, if they are to function effectively’

(2000:712).⁸ However, this does not go far enough. It is only from the perspective of the 'abstract type' of bureaucracy that these 'supports' can be represented as *ad hoc*; the burden of the critiques was that what was designated as 'informal' was integral to complex organisation. In the language frequently used at the time, the abstract type would, at best, describe bureaucratic 'dysfunctionality' not its 'functionality'. In other words, what is represented in the abstract type is not merely not *actualised*, but *unrealisable* precisely because it does not contain an adequate understanding of the very conditions of organisation. It was for this reason that theorists held it to be necessary to extend the theory of organisation beyond the misleading simplicities (that is, abstractions!) of the ideal type of bureaucracy to a theory of complex organisations. In going beyond Weber, it went beyond his distinction between formal and substantive rationality in a way that must call into question any new version of that distinction in the form of system and lifeworld.

Similar problems occur in the case of the market. Once again, the argument is seemingly straightforward. Sayer states (p. 710) that,

although, as yet, there do not appear to be clear empirical examples of non-patriarchal capitalism, it has yet to be demonstrated that the fundamental features of capitalism, such as money, class and capital accumulation require patriarchy as a condition of their existence.

Capitalism is to be thought of as an abstract type where money, class and capital accumulation are represented as forming an internally consistent system, while gender forms only a contingent association with it, since it occupies a different dimension, that of the lifeworld. However, system and lifeworld do not, 'correspond respectively to economy and culture, since some important economic activities – in particular domestic labour – are part of the life world rather than systems, and some systems, particularly the legal system – are not primarily economic' (2000:712).⁹ It is not entirely clear why domestic labour should be assigned to the lifeworld, or why it should be regarded as not integral to the capitalist system. Domestic labour, apparently, is part of the economic system, but the argument for assigning it to the lifeworld, rather than to system, apparently, is that the 'logic' of the system must be expressed in abstraction from the role of domestic labour within it.

Sayer says that capitalism as an abstraction is gender-blind, but, of course, he reaches that position by following a theorist, Marx, who was himself gender-blind. As Humphries and Rubery (1984) point out, Marxist class theory gives priority to *production*, but fails to recognise the equivalent status of *reproduction* (see also, O'Brien 1981, Young 1981). Human reproduction would clearly be a condition of existence for the reproduction of the capital–labour relation. If this is so, it suggests that production and reproduction should be analysed as integral within a system.

Indeed, one of the central features of Marx's statement of the capital–labour relation (considered as part of the 'logic of the system' in Sayer's sense) is the structural polarisation between the market for commodities produced by labour

and the market for labour as a commodity. This polarisation generates a tendency towards the reduction of conventional differences between types of labour (whether of race and ethnicity, gender or age), the reduction of skilled to average labour, etc., in the creation of an impoverished proletariat. Yet it seems evident that any such tendency depends upon a specific moment in the demographic transitions that have occurred alongside capitalism. These transitions have involved a shift from the historically dominant pattern in human populations where high fertility and high mortality lead to stable, but low, population levels, to a period of lower mortality and rapid population growth, until, with lower fertility, population again stabilises (albeit at a much higher level). It is the middle period which seems to produce a proletariat in Marx's sense (see Levine 1987). Once the third phase is reached, capitalism is associated with rising living standards and a declining proletariat (at least, as Marx initially defined it). At the same time, with proletarianisation no longer evident as the defining characteristic of work, work itself moves from the simple sphere of necessity to become an index of a different kind of social participation (even becoming absorbed into the language of citizenship) that is differentially experienced by men and women.

In these circumstances, issues of gender become central to subjective experiences of inequality. Crudely, Sayer's argument seems to be that we can get insight into these subjectivities by reference to an abstract theory of class which has no place for them and in which a causal mechanism is identified which would dissolve them, despite the absence of the effects associated with that mechanism! How much more likely is it that current pressures towards gender justice at work are produced by the equalising effects of transformed demographic conditions and changes to fertility and life cycle than it is by a tendency towards the commodification of labour which the apparently 'real' structure of class relations contains? It is the former which facilitate changes in participation at work and the re-organisation of work outside a 'male' conception of time (which itself derives from the earlier conditions which now lack necessity).

Of course, capitalism might not have developed in this way, but it has. Why should we regard the determination of what might have been as more fundamental, more 'real', than the determination of what is and has been? In any case, were the outcome that Marx anticipated to have been actualised, that outcome would no less arise in the integration of issues of production and reproduction; its actualisation would not demonstrate the truth of the independence of production from reproduction. Were proletarianisation to be actualised we would be able 'counterfactually' to imagine different fertility conditions to demonstrate its non-necessity and to show that causal mechanisms operate in the interdependence of production and reproduction (though, of course, in those circumstances it would also be reasonable to infer that the other transformations associated with capitalism are necessary in order to bring about the transformation of fertility).

There is a further asymmetry in the treatment of system and lifeworld which shows the logical priority which Sayer is seeking to assign to class (production) over gender (reproduction). Thus, it is a feature of Sayer's argument that while gender is a phenomenon of the lifeworld only, class is argued to be a phenomenon of both system *and* lifeworld. Sayer writes (p. 714, my emphasis)

that the partial autonomy of market systems from the identity of those involved in them also provides an important reason for retaining a 'structural' concept of class as product of those systems, *in addition to concepts of class in the lifeworld*. In the latter case, the way in which class behaviour and identities develop is always in and through gender, ethnicity, age and sexuality.

This reflects a quite standard move in class analysis, albeit one which is more usually associated with Weberian approaches, in which the 'structural' aspect of (now, 'economic') class is deemed to be separate from the contingencies of the formation of (social) classes. What is the force of the 'partial autonomy' of 'social' class from 'economic' class?

Like Sayer, mainstream theorists of social stratification (for a recent example, see Scott 1996) argue that although stratification arrangements are frequently gendered, gender should *neither* be regarded as a principle of stratification, *nor* as calling into question the identification and articulation of those principles of stratification previously adduced, such as 'class' or 'status'. 'Class' continues to make sense as a principle of stratification, and 'gender' is argued to refer to social conventions which come under the principle of 'status'. Research into gender, then, merely adds to our knowledge of the phenomena associated with status and how they interact with those of class, but it does not require any revision of the analytical principles of class and status. Adequate understanding of these principles preceded feminist research and rather than being challenged by it, they are, in fact, demonstrated to be confirmed by it.

The underlying argument is that the question of class structure – conceived as a structure of 'empty places' – is analytically distinct from the question of allocation to those places and from the question of class formation. However, the reason why 'class' occurs as an issue of system *and* lifeworld is precisely because of an underlying 'logic' *which occurs across system and lifeworld*. It should be apparent that, in any Marxian version, the distinction arises in terms of a shift from what was once held to be the actual movement of capitalism – the movement from class-in-itself to class-for-itself – to a statement that, although this movement contains the underlying necessity of capitalism, it is contingently unrealised because of disturbing 'social' factors. In that sense, the logic of systems is not identity-blind since the central statement of class polarisation is very much a statement of class *identities*, now presented as a statement of basic interests which are subjectively unrecognised, but deemed 'objective'. The fact that class identities are not actualised does not make the theory identity-blind, just inadequate as a theory of identity!

Of course, mainstream class theorists of a Weberian leaning have long accepted both the contingency of class formation and that its actualisation is extremely unlikely. Nonetheless, the 'idea' of class society as a 'conceptual possibility' organises their statements of concepts and their relations. The standard argument that 'patriarchy' is a problematic concept because the realisation of patriarchal society is not empirically feasible (see Lockwood 1986; Scott 1996) must also be undermined by the way in which an equivalent 'idea' of class society is argued to make sense by the very theorists who doubt its empirical feasibility.

Even from a realist perspective, the distinction between 'system' and 'lifeworld' is problematic. It played no part in Sayer's earlier defence of realist methodology where reference was made to 'structures' and the causal powers of the mechanisms integral to them (patriarchy, for example, would qualify as what previously was termed a 'structure', even if it is now not to be regarded as a 'system'). Now, there appears to be a distinction between 'structures' in terms of the kinds of causal powers integral to them, one set being argued to be more fundamental than the other. Yet, the characteristic of *social* structures, for Sayer, must be that no structure is fully durable, since all can be transformed by action. Any more or less durable structure must be sustained in action, and, therefore, even the operation of the logics of systems must involve 'subjectivities' and forms of recognition (including misrecognition, or incomplete recognition, in so far as the logics 'go beyond' the subjective understandings of actors) integral to the operation of those logics. At the same time, Sayer defines the lifeworld in terms of subjective experience, but those subjectivities, too, must include the possibility of meanings beyond those of actors, or what he identifies as the issue of those meanings possibly becoming an 'object of reflection for actors' (2000:710) would have no resonance. Indeed, in earlier work, Sayer cited the interlacing and steering of consequences, intended and unintended, in illustration of how social structures are reproduced in action with examples from either side of what he now presents as divided in terms of system and lifeworld. Thus, he (1984:96) cited Bhaskar (1979:44) to the effect that

'people do not marry to reproduce the nuclear family or work to reproduce the capitalist economy. Yet it is nevertheless the unintended consequence (and inexorable result) of, as it is also a necessary condition for, their activity.'

Whose knowledge?

It is difficult to see just what the 'higher-level' distinction between system and lifeworld provides that would add to *adequate* specifications of substantive causal mechanisms at a supposedly lower-level where no distinction between system and lifeworld need be made. The implication of my argument so far is that the distinction is serving a conservative function where a particular (inadequately specified) causal mechanism has a problematic relation to its expected effects, but where it is argued

that the absence (or distortion) of effects can be understood through the postulation of a mediating mechanism *without* the requirement of any re-specification of the terms, relationships, etc. of the original mechanism. At the same time, those unanticipated effects are manifest as the behaviours of actors who apparently reproduce their behaviours without the problems that would be anticipated from the operation of the claimed causal tendency. Their behaviours are assigned to a domain of subjective experience where they are actual, rather than *real*.

If the distinction between 'system' and 'lifeworld' is examined more closely we can see that it is also presented, more or less explicitly, as a distinction between two claims for knowledge (and as an attempt to reconcile them). Thus, Habermas (1987:151) states that the system incorporates the 'external perspective of the observer', while the lifeworld incorporates 'members' intuitive knowledge'. However, since sociologists participate in many of the structures they seek to account for, then, it is not quite correct to say that the system is defined by 'logics' which go beyond the subjective experience of actors, for there is one group of actors who claim those 'logics' as their subjective experience (albeit, by calling their own subjectivity objective), namely social scientific 'observers'. At the same time, what is assigned to other actors as their subjective experience in the lifeworld is not unproblematically *their* intuitive knowledge, but what is intuited on their behalf by sociologists in making sense of behaviours which deviate from what is 'necessary' from the perspective of the apparently 'systematic' understanding of sociological 'observers'.

Obviously, in the case of gender and the associated rise of feminism, the actual experiences that are being 'intuited' are those of women. My argument, however, is not that feminism can be represented by a simply conceived standpoint of women, but that relatively stable gender inequalities must be indicative of deeper processes which require explanation and which must call into question prior explanatory structures in terms of which those inequalities appear problematic. The point of this argument is not to say that we must choose one or other perspective, but that where there is a division of this form, there is a problem of understanding which is internal to pre-existing understandings and could only properly be addressed by accepting the need for a reconstruction of those understandings. The irony of Sayer's position is that as a Marxist – or perhaps now, post-Marxist – he would have no difficulty in recognising that a similar dynamic to the one I am arguing might have been integral to the genesis of Marxism itself. After all, Marx raised the issue of competing knowledge claims and posed the question of who is to 'educate the educators'. The theory – in effect, class theory – that is being so assiduously defended by Sayer has its origins in the apparent rise of a proletariat whose experience lay outside the categories of pre-existing social theory and appeared to negate it. For Sayer, the experience of gender inequality and its impact upon professional social theory would seem to be of a different order. Theorists frequently emphasise dialogue and

criticism, but they seem not to include their own learning and self-criticism within the conception of that dialogue.

It is in the light of these issues that Sayer's claim to a more progressive, and less fatalistic, politics should be judged. He argues (p. 722) that,

the impulse of critical social science is to explore every nook and cranny of society to seek out hidden sources of domination, be they soft or hard. But if this is coupled with associational thinking, then it encourages fatalism; in the case of gender and organisations, the more features of society are seen as *unavoidably* associated with patriarchy, the more difficult the task of abolishing patriarchy will seem.

The reality of the situation is otherwise. The paradox of Sayer's position is that he is arguing against an apparent reduction of system to lifeworld, while associating the 'hardest' structures with the former. These 'hard' structures apparently contain mechanisms which would dissolve gender and other inequalities were they to be actualised. Yet the argument about system and lifeworld only arises in that 'conventions' attributed to the lifeworld apparently have proven resistant. It would seem that the structures of the lifeworld are more durable than the structures of the system!

Sayer is in the strange position of implicitly recommending the less restricted operation of markets and bureaucratic principles as a means of realising gender justice (or, what makes his position less fatalistic?). For example, as the earlier discussion of bureaucracy and the market shows, he accepts the 'rationality' of those systems even where they are not actualised. This might encourage the view that in not being actualised, policies could and should be formed to help realise their rational possibilities. Of course, as the discussion of bureaucracy above indicates, to the extent that what Sayer represents as contingent is, in fact, entailed, then the attempt to realise the supposedly rational by removing 'contingencies' would be to actualise the dysfunctional! Moreover, feminist research has shown that gender inequalities are greatest precisely where market and bureaucratic principles are allowed most dominance and form the rationale of public policy (Folbre 1994; Sainsbury 1996), and it is difficult to see what arguments Sayer could muster from his critically realist perspective against these consequences of public policy.

The paradox of fatalism that Sayer claims to have identified can be overcome once we allow that the recognition that organisation is always embodied (or gendered) is not the same as saying that it is necessarily patriarchal. We can easily accept that organisation is intrinsically gendered without at the same time being required to argue that it is, thereby, intrinsically patriarchal. The achievement of gender justice does not require the elimination of the 'personal' in favour of the 'impersonal'. The claim that any denial of the necessity of a system and lifeworld distinction involves a reduction of system to lifeworld with the consequences identified by Sayer is false. If I understand the implications of feminist theory and research correctly, what is being claimed is that whatever is systematic is also

embodied and what is embodied is also systematic (and further, that the task of research is precisely the elucidation of the systematic character of embodied practices and relations). The separation of system and lifeworld seems to offer the system as the solution to the problems of embodiment, but there are no 'out of body' experiences, nor need we regard *embodiment as such* as the problem of gender injustice. Simply, the solution to problematic forms of embodiment is different forms of embodiment.

ACKNOWLEDGEMENTS

I should like to thank Dave Campbell, Maureen O'Malley, Steve Kemp, Sue Scott, the anonymous reviewers of *Sociology* and the participants in the Social and Political Theory Seminar at Warwick University for their helpful comments on this paper.

NOTES

1. Sayer discusses the issues in the context of the work of Hartmann (1981); Acker (1990); Ferguson (1984); Walby (1986, 1990); Savage and Witz (1992); and Halford, Savage and Witz (1997); among others.
2. Lawson (1999) defends the 'universalistic' orientation of realism against feminist critiques of universalism by accepting that it is frequently the case that universalistic claims do smuggle in a particular point of view, but that this is not necessarily so and that realism is exempt from the charge because it is 'ontological', rather than 'epistemological' in character. The nature of this distinction will become clear in the course of this article and we shall see that the ontological argument of critical realism – as presented by Sayer – does have the consequences that Lawson attempts to deny. It is interesting in this context, that Lawson should present the problem as being the marginalising of realism within feminism: 'it may just be (from where I am situated it seems likely) that if feminists, including feminist economists, allow realism, and in particular ontological analysis, to come more fully out of the margin, the opportunities for advanced opened up thereby will prove to be to everyone's advantage' (1999:51).
3. Mouzelis does not elaborate the full realist ontology in his article published in the same issue of *Sociology* as that of Sayer, but he implies it in his distinction between the 'social-relational' which is 'virtual', and the 'actual' (2000:743). See also, Giddens (1984:17).
4. There is an immediate puzzle evident in Sayer's suggestion that feminist research is 'empiricist', by virtue of its characteristic 'associational thinking'. If it were truly 'empiricist', then it would be committed to the unchanging standards of science. However, Sayer argues that feminism is challenging those standards and finds that challenge misplaced and deriving from 'associational thinking'. Something of the same paradox is evident in Harding's original argument. As we shall see, the problem of 'universal standards' is not resolved by retreating from 'epistemology' to 'ontology' as Lawson (1999) proposes. See also Harding's (1999) reply to Lawson. For a more sympathetic account of 'feminist empiricism' which qualifies the standard understanding of the term 'empiricism' as applied to feminist research, see Nelson (1990, 1993) and Holmwood (1995) where it is argued that external, universal standards are not necessary in order to make judgements between competing accounts and that the critique of universal standards does not necessarily entail relativism.
5. One possibility might be that statistical reasoning can function as an equivalent 'research technology' to that of experimentation. Realists criticise statistical reasoning precisely

because, in their view, it involves 'associational' thinking (Sayer 1984; Pawson 1989). Yet one area of major success in feminist research has been the use of statistical reasoning to demonstrate the gendered nature of labour markets and the gendered patterns of social inequalities. It should be evident that I neither regard statistical reasoning to be assimilable to positivism, nor believe it to be marginalised within a feminist paradigm as Oakley (1998) suggests.

6. Thus, Weber distinguishes ideal types from theoretical constructs in the natural sciences, writing that, 'an hypothetical "law of nature" which is definitively refuted in a *single* case collapses as an hypothesis once and for all. In contrast, the ideal typical constructions of economics – if they are correctly understood – have no pretensions at all to *general* validity' (1975:190). Furthermore, in social inquiry, ideal types, 'can function as hypotheses when employed for heuristic purposes. However, in contrast to hypotheses in the natural sciences to establish in a concrete case that an interpretation is *not* valid is irrelevant to the question of the theoretical value of the interpretive scheme' (1975:190).
7. Papineau (1976) has argued that because Weber refers to ideal types as 'heuristic', they could, therefore, be construed along the lines of Lakatos's research programmes. However, as he also observes, this is not how ideal types have been used in social inquiry and Weber himself does not develop his argument in this direction, a judgement he shares with Bruun (1972).
8. In fact, although Sayer makes reference to the 'informal', his argument also points to something else which is equally problematic from his perspective. The two forms of organisation which are associated with the system domain, the market and bureaucracy, are not only abstracted from the contingencies of the life world, but from their own mutual constitution. Thus, the quotation cited above is preceded by the statement that 'no single type of economic or organisational system can exist entirely on its own, without the support of different forms of organisation: markets need non-market forms of organisation such as state regulation to make them sustainable' (2000:712). The example in economics which is analogous to the problem of bureaucracy discussed here is the theory of the firm. Coase (1988), for example, pointed out that pure markets could not operate because of the existence of transaction costs. However, his point was not to sanction the abstraction from transaction costs by pointing to its real-world, contingent limitation, but to challenge pure theory and its deductions precisely because it did not represent these limits in theory. In other words, transaction costs are not contingencies. They are irreducible and abstract theory distorts our understanding. See Campbell and Klaes 2001.
9. I shall pass over Sayer's comment about the legal system which, from his own perspective, seems to confuse the everyday appellation of the legal system with his own counterfactual usage. Most writers who distinguish system and lifeworld place the legal system in the lifeworld as one of its 'hard and durable structures'. See, for example, Habermas (1987); Giddens (1984); Mouzelis (1990).

REFERENCES

- Acker, J. 1990. 'Hierarchies, Jobs, Bodies: A Theory of Gendered Organisations'. *Gender and Society* 5:390–407.
- Alexander, J.C. 1982. *Theoretical Logic in Sociology, Volume I, Positivism, Presuppositions and Current Controversies*. London: Routledge & Kegan Paul.
- Alexander, J.C. 1988. 'The New Theoretical Movement', in N. J. Smelser (ed.), *Handbook of Sociology*. London: Sage.
- Archer, M. 1988. *Culture and Agency*. Cambridge: Cambridge University Press.

- Archer, M. 1995. *Realist Social Theory: The Morphogenetic Approach*. Cambridge: Cambridge University Press.
- Archer, M., Bhaskar, R., Collier, A., Lawson, T, and Norrie, A. (eds.) 2000. 'For Structure: Its Reality, Properties and Powers: A Reply to Anthony King'. *Sociological Review* 48:464–72.
- Archer, M., et al. (eds.) 1998. *Critical Realism: Essential Readings*. London: Routledge.
- Bhaskar, R. 1978, 2nd edn. *A Realist Theory of Science*. Brighton: Harvester.
- Bhaskar, R. 1979. *The Possibility of Naturalism*. Brighton: Harvester.
- Bhaskar, R. 1986. *Scientific Realism and Human Emancipation*. London: Verso.
- Blau, P. 1955. *The Dynamics of Bureaucracy: A Study of Interpersonal Relations in Two Government Agencies*. Chicago: University of Chicago Press.
- Bruun, H. H. 1972. *Science, Values and Politics in Max Weber's Methodology*. Copenhagen: Munksgaard.
- Campbell, I. D. and Klaes, M. 2002. 'The Nobel Paradox in the Writings of Ronald Coase: The Regulatory Critique of Intervention'. *Journal of Law and Society*, forthcoming.
- Coase, R.H. 1988[1937]. 'The Nature of the Firm', in R. H. Coase, *The Firm, the Market and the Law*. Chicago: University of Chicago Press.
- Crozier, M. 1964. *The Bureaucratic Phenomenon*. London: Tavistock.
- Ferguson, K. E. 1984. *The Feminist Case against Bureaucracy*. Philadelphia: Temple University Press.
- Folbre, N. 1994. *Who Pays for the Kids? Gender and the Structures of Constraint*. London: Routledge.
- Garfinkel, H. 1967. *Studies in Ethnomethodology*. Englewood-Cliffs, NJ: Prentice-Hall.
- Giddens, A. 1976. *New Rules of Sociological Method*. London: Hutchinson.
- Giddens, A. 1984. *The Constitution of Society*. Cambridge: Polity.
- Gouldner, A. 1954. *Patterns of Industrial Bureaucracy*. Glencoe, NY: Free Press.
- Habermas, J. 1984. *The Theory of Communicative Action, Volume I: Reason and the Rationalisation of Society*. London: Heinemann.
- Habermas, J. 1987. *The Theory of Communicative Action, Volume II: Lifeworld and System*. Cambridge: Polity.
- Halford, S., Savage, M., and Witz, A. 1997. *Gender, Careers and Organisation*. London: Macmillan.
- Harding, S. 1986. *The Science Question in Feminism*. Milton Keynes: Open University Press.
- Harding, S. 1999. 'The Case for Strategic Realism: A Response to Lawson'. *Feminist Economics* 5:127–33.
- Hartmann, H. 1981. 'The Unhappy Marriage of Marxism and Feminism: Toward a More Progressive Union', in L. Sargent (ed.), *Women and Revolution*. London, Pluto.
- Hekman, S.J. 1997. 'Truth and Method: Feminist Standpoint Theory Revisited'. *Signs* 22:341–65.
- Holmwood, J. 1995. 'Feminism and Epistemology: What Kind of Successor Science?' *Sociology* 25:411–28.
- Holmwood, J. and Stewart, A. 1991. *Explanation and Social Theory*. London: Macmillan.
- Humphries, J. and Rubery, J. 1984. 'The Reconstitution of the Supply Side of the Labour Market: The Relative Autonomy of Social Reproduction'. *Cambridge Journal of Economics* 8:331–46.
- Kuhn, T. S. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- Lakatos, I. 1970. 'Falsification and the Methodology of Scientific Research Programmes', in I. Lakatos, A. Musgrave (eds.), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.
- Lawson, T. 1999. 'Feminism, Realism and Universalism'. *Feminist Review* 5:25–59.
- Levine, D. 1987. *Reproducing Families: The Political Economy of English Population History*. Cambridge: Cambridge University Press.
- Lockwood, D. 1986. 'Class, Status and Gender', in R. Crompton and M. Mann (eds.), *Gender and Stratification*. Cambridge, Polity.

- Mouzelis, N. 1990. *Back to Sociological Theory: The Construction of Social Orders*. London: Macmillan.
- Mouzelis, N. 1995. *Sociological Theory: What Went Wrong? Diagnoses and Remedies*. London: Routledge.
- Mouzelis, N. 2000. 'The Subjectivist-Objectivist Divide: Against Transcendence'. *Sociology* 34:741–62.
- Nelson, L. H. 1990. *Who Knows? From Quine to a Feminist Empiricism*. Philadelphia: Temple University Press.
- Nelson, L. H. 1993. 'Epistemological Communities' in L. Alcoff and E. Potter (eds.), *Feminist Epistemologies*. London: Routledge.
- Oakley, A. 1998. 'Gender, Methodology and People's Ways of Knowing: Some Problems with Feminism and the Paradigm Debate in Social Science'. *Sociology* 32:707–31.
- O'Brien, M. 1981. *The Politics of Reproduction*. London: Routledge & Kegan Paul.
- Outhwaite, W. 1983. *Concept Formation in Social Science*. London: Routledge & Kegan Paul.
- Papineau, D. 1976. 'Ideal Types and Empirical Theories'. *British Journal of the Philosophy of Science* 27:137–46.
- Pawson, R. 1989. *A Measure for Measures: A Manifesto for Empirical Sociology*. London: Routledge.
- Sainsbury, D. 1996. *Gender, Equality and Welfare States*. Cambridge: Cambridge University Press.
- Savage, M. and Witz, A. (eds.), 1992 *Gender and Bureaucracy*. Oxford: Blackwell.
- Sayer, A. 1984. *Method in Social Science: A Realist Approach*. London: Hutchinson.
- Sayer, A. 2000. 'System, Lifeworld and Gender: Associational Versus Counterfactual Thinking'. *Sociology* 34:707–25.
- Scott, J. 1996. *Stratification and Power: Structures of Class, Status and Command*. Cambridge, Polity.
- Stacey, J. and Thorne, B. 1985. 'The Missing Feminist Revolution in Sociology'. *Social Problems* 32:301–16.
- Stanley, L. 2000. 'For Sociology, Gouldner's and Ours', in J. Eldridge, J. MacInnes, S. Scott, C. Warhurst and A. Witz (eds.), *For Sociology: Legacies and Prospects*. Durham: Sociologypress.
- Walby, S. 1986. *Patriarchy at Work*. Cambridge: Polity.
- Walby, S. 1990. *Theorising Patriarchy*. Oxford: Blackwell.
- Weber, M. 1949. '“Objectivity” in Social Science and Social Policy', in Weber, *The Methodology of the Social Sciences*. New York, Free Press.
- Weber, M. 1975. *Roscher and Knies: The Logical Problems of Historical Economics*. New York: Free Press.
- Young, I. M. 1981. 'Socialist Feminism and the Limits of Dual Systems Theory', in L. Sargent (ed.), *Women and Revolution*. London: Pluto.

Biographical note: JOHN HOLMWOOD is Professor of Sociology and Director of the Graduate School in Social Sciences at the University of Sussex. He was previously Professor of Sociological Theory at the University of Edinburgh.

Address: School of Social Sciences, University of Sussex, Falmer, Brighton BN1 9SN.