THE WHITEHALL PROGRAMME AND AFTER: RESEARCHING GOVERNMENT IN TIME OF GOVERNANCE

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This article revisits the Whitehall Programme and Rod Rhodes’ crucial role in setting it up. It examines the research commissioned for the Programme and how research in this field has changed since the mid 1990s. It confirms that research on Whitehall has become more diverse and specialist – reflecting its apparent hollowing-out – but that research employing a longer historical perspective does not support an interpretation that government in the UK was ever strongly filled-in. It also suggests that the study of Whitehall now demonstrates more theoretical ambition than hitherto, and, in the light of the dominance of qualitative research in this field, calls for a more sophisticated methodological debate about qualitative approaches to the study of government and especially their potential contribution to theory-building.

INTRODUCTION

In 1993–94, the UK’s national research council for social science research, the Economic and Social Research Council (ESRC), commissioned a programme of research, co-sponsored with the Cabinet Office, on the British Government executive. The Whitehall Programme ran for five years until 1999, and funded 23 projects which together produced a considerable volume of empirical research facilitated by access to the inner workings of government. Rod Rhodes deserves much of the credit for the Programme. He took the initiative in advocating for it; mobilized discussions between the ESRC and the Cabinet Office to set it up; acted as the Programme Director for over four years; and, even after the Programme formally ceased, devoted considerable energy to disseminating its work. Rhodes also made a prodigious contribution to its academic output, by writing, co-writing, editing or co-editing some 10 books, 30 articles and 25 chapters and reports. He used them to argue forcefully for the ‘governance narrative’ and for a methodology based in interpretation rather than in formal theory building. The seminal importance of this work is reflected in the almost exclusive focus on governance and interpretivism in the chapter on the ‘central state’ in the Oxford Handbook of British Politics (James 2010). Here, I propose to take a different approach, by looking at how the study of government in the UK has changed in a time of alleged ‘hollowing-out’. I organize my comments under three headings which featured centrally in the Programme’s aims: mapping change; setting it in long-term perspective; and theoretical and methodological innovation. But first I offer a brief history of the Whitehall Programme.

THE WHITEHALL PROGRAMME’S ORIGINS AND HISTORY

By early 1992, Rhodes, then chair of the Steering Committee of the ESRC’s Local Governance Programme, had become interested in applying the ‘governance’ framework to national-level arenas and beyond. A programme of empirical research in British government would obviously require agreement at the top of the Civil Service. He therefore...
persuaded the ESRC to fund, and some senior civil servants to attend, a seminar on the ‘theory and practice’ of government (Rhodes 2000a, b). A series of meetings between the Cabinet Office and ESRC subsequently led to a formal Accord, which enabled ESRC to fund a new research programme. Two senior civil servants from the Cabinet Office were appointed to the Programme’s Steering Committee. The full membership, which agreed the final programme specification and, during the winter of 1993–94, issued calls for outline proposals, is listed in Rhodes (2000b, note 4). Rhodes was appointed Director, by open competition, from 1 April 1994.

Setting up the Whitehall Programme was an important achievement. Government departments were used to commissioning research for their own purposes, but this programme required Whitehall to be opened to what was being described as ‘curiosity research’, that is research determined primarily by academic agendas. Although the previous closure of government to academic researchers was never absolute (see, for example, Heclo and Wildavsky 1981; Gray and Jenkins 1982; Gray et al. 1991), there were no agreed procedures for securing academic access, and success, for the few who tried, depended on the whims of top civil servants and on researchers’ charm and personal connections. As a result, most academic writing and teaching about Whitehall was not informed by systematic empirical research and there were big gaps in knowledge. Why the Cabinet Office agreed to sponsor the Whitehall Programme is, therefore, an interesting question. The obvious answer is that the Programme coincided with the Major government’s new-found commitment to open government (Cm 2290 1993). The leadership of Sir Robin Butler, the Head of the Home Civil Service, was also important. In a lecture to the Public Administration Committee of the Joint University Council in September 1992, Butler called for a ‘more creative and sympathetic, and less mutually grudging, relationship’ between civil servants and academics (Butler 1992, p. 13). In particular, he invited academics to engage in a more open-minded way than hitherto with the New Public Management (NPM), and emphasized that ‘neither modernization nor efficiency – nor even the phenomenon of a single political party being in power over a long period – requires us to discard … assets of lasting value’ (p. 9) such as the civil service’s integrity and impartiality. As the call for proposals made clear, the rules governing access of the Programme’s researchers to Whitehall were based in principles laid down by the Radcliffe Committee in 1975 (Cmnd 6386): these principles still operate today. That is, the setting up of the Whitehall Programme surfaced and formalized existing rules, based on a new willingness to facilitate access where the proposed research complied with them.

The negotiations between ESRC and the Cabinet Office also led, uniquely for ESRC programmes at that time, to the agreement that it would have a special client audience, namely the Senior Civil Service: this was captured in Objective 5: ‘to foster contacts and create a common understanding between academics and practitioners’. Rhodes’ main strategy for achieving this objective was to develop a narrative about change that would help senior civil servants make sense of and cope with it. So ‘governance’ became the Programme’s ‘flagship idea’. It was intended to challenge the assumption that reform, especially that associated with NPM, could be successfully achieved by relying on hierarchy and formal authority in the context of a unitary and unified government. Instead, it presents the contemporary polity as a ‘differentiated’ one, characterized by functional and geographic fragmentation and shifting patterns of self-governing networks bounded and bonded by mutual resource dependencies amongst complex and fluid sets of governmental and civil organizations. The purpose of the governance narrative was to
sensitize academics, politicians, civil servants and other actors to the crucial importance of networking skills – particularly the diplomatic skills of negotiation and bargaining – not least by drawing attention to the unintended consequences likely to flow from relying on top-down command.

Rhodes understood that ‘impact’ comes from building long-term relationships with users throughout all stages of a research programme, rather than relying on one-off ‘knowledge transfer’ events at the end of projects. He therefore embarked on an ambitious campaign to build understanding of the governance narrative, most importantly through a series of seminars, involving civil servants, selected academics and national journalists, that ran throughout the Programme. Those closely involved believed that this strategy was successful in fostering a more relaxed relationship and greater mutual understanding between academics and civil servants. The longer-term impact of the Programme on the Senior Civil Service is impossible to gauge, but the professional scepticism of the senior civil service, the frequent movements of personnel and the shifting focus of attention as policy agendas move, all inevitably limit it. Nevertheless, the Programme led to academic access to Whitehall on an unprecedented scale and created a formal route for future researchers to follow suit. And, as we will see, primary empirical research in Whitehall has become a more normal activity in the study of British Government. These are not insignificant achievements.

As Programme Director, Rhodes also offered what was widely recognized to be effective and sensitive support of the projects funded by the Programme, particularly in their dealings with Whitehall. Rhodes reached out to the wider academy through lecture tours, conferences and media events in the UK and abroad, and edited two special journal issues (Rhodes 1997a, 2000c) and a book series on Transforming Government. All this activity meant that the Programme succeeded – probably more than most such programmes – in creating a coherent thrust and a strong identity. It also meant, of course, that the reputation of the Programme was – also probably more than most – associated with its Director.

THE WHITEHALL PROGRAMME: ITS FOCUS AND OBJECTIVES

As its name suggests, the Programme that emerged from the negotiations between ESRC and the Cabinet Office was focused primarily on Whitehall, not on governance more generally. It had seven objectives (listed in Rhodes 2000a, b). In common with most ESRC programmes, it was to encourage the development of theory, methodology and comparative research, but its specific objective was to ‘to describe, explain and create a better understanding of recent and long-term changes in British government and to compare them with those in other EC member states and Westminster systems’. Change was to be mapped under six main ‘themes’: (1) the new governance; (2) hollowing-out the state; (3) fragmentation; (4) ministers and managers; (5) the evolving constitution; and (6) delivering services. Judged by the sheer volume of its outputs, the Whitehall Programme clearly succeeded in meeting this core objective in that empirical research reported in some 40 books and over 80 articles, can be traced, in full or in part, to the Programme. The Programme was particularly successful in contributing new empirical knowledge under themes (ii)–(iv). A full list of projects funded by the Programme is in Rhodes (2000a), and their end of award reports are in the British Library. Most of them were carried out between late 1995 and early 1998. The Programme was therefore timely in providing systematic empirical evidence about the significance, for both the effectiveness
of government and for constitutional values, of change during the Thatcher and Major administrations, including: agencification (Hogwood et al. 2001); the Europeanization of Whitehall (Bulmer and Burch 1998); market testing and outsourcing (Boden et al. 1998; Newman et al. 1998); internal regulation (Hood et al. 1998; Daintith and Page 1999; Hood et al. 2002) and the regionalization of Whitehall (Mawson and Spencer 1997). Some outputs ventured provisional insights on the direction of travel under New Labour (see, for example, Deakin and Parry 2000; Marsh et al. 2001). Work funded by the Programme also contributed to the understanding of leadership, co-ordination and policy advice in Whitehall in historical context, especially in relation to the Prime Minister and No. 10 (Kavanagh and Seldon 1999; Rose 2001), Permanent Secretaries (Theakston 1999a, b) and the Treasury (Deakin and Parry 2000). However, its timing meant that the Programme was more-or-less silent on several issues that were to become highly salient under New Labour, especially ‘joined-up government’ (JUG). Although it had roots in Rhodes and Stoker’s earlier work on local governance, the seminal work on ‘holistic governance’ was developed independently of the Programme at the think-tank, Demos (6 et al. 2002).

JUG turned out to be a more complex problem than officials and politicians initially understood, but 6 (2004) and Bogdanor (2005) offer analysis of JUG’s subsequent progress and the tensions it raised.

Unsurprisingly, given the focus on Whitehall, the scope of most projects was defined by reference to formal government institutions or offices, especially prime ministers, departments and agencies. To borrow from Flinders’ (2002) classification of governance narratives, in so far as the Programme stimulated work on the ‘new governance’, it adopted a state-centric version. The Steering Committee found itself unable to fund more than two comparative projects: a particularly ambitious project led by Jack Hayward and the late Vincent Wright that involved research teams in six countries comparing co-ordination in four cross-cutting policy sectors (Hayward and Wright 2002), and a comparison by Elder and Page (1998) of executive agencies in Germany and Sweden. No projects comparing Westminster systems survived the refereeing process (Peters et al. 2000, p. x). This left the Programme exposed on its objectives to foster comparative research and an international dimension. So Rhodes filled the gap himself, by co-organizing workshops in Australia, Denmark and Newcastle that spawned two books co-edited with Weller and others. The first (Weller et al. 1997) compares national influences on core executive capacity in a number of Westminster systems. This work has since been much developed in Rhodes and Weller (2001) and Rhodes et al. (2009) which explore the development and current meanings of the Westminster model in the ‘old dominions’. The second book (Peters et al. 2000) compared the administration, organization and staffing of core executive support in eight countries.

**MAPPING RECENT CHANGE**

The Programme more or less neglected coverage of theme (vi), service delivery. This gap reflects the strong and near-exclusive emphasis then placed by textbooks and research monographs on formal policy-making in the great offices and institutions at the top and centre of British government. The Whitehall Programme greatly strengthened the empirical foundations of the academic study of central government, but it did not significantly widen its scope. Fifteen years on, however, James (2010) is surely right to claim that, like its subject matter, research on contemporary central government has become decentred, and more diverse and specialized than hitherto. As well as the huge
growth of interest in the management of public services at all levels of government (manifested, for example, in the ESRC’s Public Services Programme directed by Hood), increasing attention has been paid to relations between Whitehall and the devolved administrations, on the one hand, and with the European Union on the other (see, for example, Bulmer et al. 2002; Rhodes et al. 2003; Bache and Flinders 2004; McLean 2005; Bulmer and Burch 2009). Organizational and functional specialization can also be observed, for example, in the work of Gains (1999, 2003), James (2003) and Flinders (2008) on agencies and delegated governance, Thain’s project on the Treasury, Hood and Lodge’s work on public service bargains, James’s work on performance management and consumer satisfaction (for example, see James 2004), as well as in the increasing interest in e-government (see, for example, Dunleavy et al. 2006, 2007) and in the sensitive issue of personal information in government (6 et al. 2005, 2010; Bellamy et al. 2005).

Research in the traditional haunts of public administration has also changed. Since the Whitehall Programme, there is a much stronger expectation that it will be based in primary empirical evidence. Although there has been important work that uses quantitative data to test explanatory theories about elite behaviour (see, for example, Dewan and Dowding 2005; Berlinski et al. 2007) empirical research on Whitehall usually relies on qualitative evidence, almost always drawn from semi- or unstructured elite interviews: there is, for example, much less use of declassified public records. Consequently, we now have much richer accounts of the beliefs, perceptions, actions and relationships of contemporary actors, especially civil servants. Some innovative techniques have been developed, too, including Bevir and Rhodes’ (see, for example, 2006a) use of ethnographic methods to interpret how top civil servants understand their worlds and Richards’s project on the use of political biography to interpret what Whitehall means to politicians. Page’s painstaking work on subordinate legislation (Page 2001) is also groundbreaking in that it drills much further down into government departments than has usually been the case, in order to understand the political significance of routine administrative decisions.

The work that has engaged most directly with the governance narrative is that conducted in the Birmingham-Sheffield axis of Marsh, Richards and Smith. They have provided a great deal of systematic empirical evidence about change across Whitehall over more than a decade, both in the core executive and in government departments. They have also contributed strongly to theoretical debate about governance and power: first by arguing for the Asymmetric Power Model – which states that, as a result of the control of key power resources by governments, policy networks are both more stable and more hierarchical than Rhodes suggests – and second by arguing that the Westminster Model still possesses considerable normative force amongst civil servants and politicians, thus constituting a powerful meta-tradition in Whitehall (Richards 2008).

HOLLOWING-OUT AND FILLING-IN

Rhodes has offered few comments on how far he believes the research done for the Whitehall Programme supports the governance narrative (the exception is Rhodes 2000d). Conversely, few projects engaged directly with the narrative or used it to stipulate their research questions. Nevertheless, many outputs provided evidence of a deficit in co-ordinating capacity in Whitehall. For example, studies of PMs show them struggling, with only intermittent success, and under serious constraints, with chronic problems in steering government and co-ordinating policy. The consensus among these studies is that
this struggle has intensified in recent years: this seems paradoxical in that the support available to the PM has grown. Projects on centrally driven change in Whitehall – such as those cited above on the impact of EU membership, regional government offices, Next Steps agencies and the establishment of new regulatory agencies – all found significant variation in the degree to which initiatives were implemented and in the institutional structures, processes and tools to which they gave rise, thus reinforcing claims in the governance narrative about the differentiated polity and the limits of the core executive’s reach. This finding is also apparently paradoxical, in that, as critics of the governance narrative pointed out (Saward 1997; Holliday 2000), the period saw explicit attempts to increase the core executive’s capacity for control, including: the greater formalization and elaboration of Treasury control over public expenditure; the establishment of specialist agencies to promote efficiency and good management; the creation of new regulatory agencies, such as the National Audit Office; and the formalization and elaboration of rules and conventions governing departments and ministers.

However, as Daintith and Page (1999), and Deakin and Parry (2000) clearly show, formalization, elaboration and specialization combined significantly to increase institutional complexity, especially within the core executive. This process accounts, at least in part, for their finding that the actors involved perceived the process of centralization as one that reduced overall capacity for steering government. Hood et al. (2002) found that at the same time that government departments experienced a reduction in direct hierarchical control over service delivery associated with NPM – which, in the language of group-grid theory (Hood 1996), had pushed departments down-group and down-grid – they also experienced greater formalization and specialization of internal regulatory arrangements, which had thus been pushed significantly up-grid and up-group. Hood et al. label this paradox ‘reverse polarity’, a term that neatly captures the intense and contradictory consequences of NPM which, they conclude, have led to major and probably unsustainable tensions in Whitehall between pressures for decentralization and pressures for more central control.

This evidence appears to provide broad support for the ‘hollow state’ thesis. It also helps resolve Rhodes’ famous dispute with Saward (1997) and Holliday (2000). By suggesting some mechanisms by which this particular manifestation of Rhodes’ sour law of unintended consequences actually works, it shows how imperatives to strengthen the core executive’s steering capacity achieved the opposite effect. Indeed, Bevir and Rhodes (2006b) have since argued that the seemingly endless debate – which continued under the Blair premiership (for example, see Foley 2000; Burch and Holliday 2004) – about whether or not the British premiership is becoming more presidential arise from the failure, by politicians, journalists and academics alike, to grasp this law.

HOLLOWING-OUT IN HISTORICAL PERSPECTIVE

This evidence also sheds light on a question that has received less attention than it merits, despite being implicitly recognized by the Whitehall Programme’s objective to understand change in historical perspective. This question is how the governance narrative is best situated within the long-term history of Whitehall. Rhodes writes about governance, and particularly hollowing-out, in ways that deliberately emphasize difference from what went before. He asserts that the Westminster model ‘no longer provides either an accurate or comprehensive account of how Britain is governed’ (Rhodes 1997b, p. 3) and dates the key changes rather precisely from 1979. This claim implies that he believes that the
Westminster model was, until quite recently, a more-or-less accurate empirical description. This is a precarious claim. Much of the valuable work in concept formation done for the Programme in respect of the slippery concepts of co-ordination, coherence, power and power dependency (see, for example, Weller and Bakvis 1997; Smith 1999; Hayward and Wright 2002) casts considerable doubt on whether the capacity for governing implied by Rhodes’ definition of the Westminster model could be realized in any state or at any time in which there is any significant degree of pluralism of values and goals, or any but a monopolistic distribution of resources. Weller makes a similar point in relation to the closely-related notion of Cabinet government when he says that ‘it is difficult to describe any recognizable circumstance where it worked in a form that these critics [that is, those who bemoan its decline] would approve’ (Weller 2003).

Even if we allow that, for rhetorical purposes, Rhodes deliberately overstates the political coherence and capacity for command associated with the Westminster model, the fundamental question remains: whether the shift to ‘governance’ should be regarded as a particularly significant and exceptional break from the past, or simply as the contemporary phase in a chronic struggle to build sufficient capacity for steering government to meet the demands of its time. Is government being hollowed-out, or are we simply discovering that it was, and is, necessarily more hollow than his interpretation of the Westminster model suggests? The difficulty in answering this question lies in the problem identified by Rose (2001): we have few systematic historical accounts that enable us to set contemporary changes in long-term perspective. This lacuna has important methodological consequences. Just as the study of a single case can lead researchers too readily to claim what Weller and Bakvis (1997, p. 11, quoting Rose 1991, p. 450) call ‘uniqueness by false particularization’, so the danger of focusing research in the present is that it encourages an over-emphasis on its distinctiveness from what went before. Indeed, many accounts of governance draw inferences about its significance from the apparent magnitude, novelty and force of its alleged causes – such as Europeanization or privatization – or from studies limited to episodic or recent institutional change.

The historical perspective supplied by the Whitehall Programme is, therefore, valuable. It suggests that many features associated with hollowing-out are far from new, especially the restricted reach and contingent authority of Prime Minister, Cabinet and Treasury, the centrifugal pressures from departments and the susceptibility of governments to both exogenous and endogenous surprises, including the unintended consequences of reforms intended to shore up their power. For example, two Whitehall projects examined the Plowden Committee in the late 1950s and the subsequent emergence of PESC as largely unsuccessful responses to the Treasury’s chronic problems in controlling public expenditure (Lowe 1997a, b; Deakin and Parry 2000). Lowe links this initiative to Prime Minister Macmillan’s wider preoccupation with ‘modernization’ as a response to Britain’s relative economic decline, but shows how it was derailed by a combination of external events and competing departmental interests. In a fascinating contribution to the ‘book of the Programme’, Lowe and Rollings (2000) claim that more systematic archival research over a longer period would reveal a system of government characterized not, as many historians implicitly assume, by the order, stability and continuity associated with the Westminster model, but by constant tension between governance and government, between fragmentation and centralization. This tension – which they consider to have been particularly acute in the period after the Second World War – stems, they believe, from competing pressures deriving, on the one hand, from growing expectations placed
on the state and, on the other, from fragile accommodations made since the Edwardian period between state, civil society and external interests. Their hunch is, then, that further research would lead to a ‘modification of claims for the exceptional nature of the present and a greater appreciation of the attempt to “fill out” the post-war state, to which the ‘hollowing-out’ of the 1980s was a direct reaction’ (Lowe and Rollings 2000, p. 100).

Similarly, most historically minded projects commissioned by the Programme do not perceive institutional change in the last 20 years as constituting a sharp break from the past. Rather, they regard it as the outcome of the recent intensification of pressures arising from long-term failure to achieve a stable settlement between imperatives for filling-in and hollowing-out. This process is demonstrated by the two Whitehall projects that can justifiably claim to provide a long-term perspective of the kind advocated by Rose. Deakin and Parry (2000) and Daintith and Page (1999) carefully trace a more-or-less continuous process of change over several decades, respectively in the control of public expenditure and the growth of quasi-legal, internal control of government. Both projects emphasize the capacity for independent action enjoyed by departments until the last third of the twentieth century, as a result of tacit consensus about constitutional constraints, political conventions and the practical limits on co-ordination and central oversight. Both studies therefore present changes in the last three decades or so – that is, the huge extension, increasing formalization and bewildering complexity of regulatory instruments and agencies, and the greater intrusion of the core executive into departmental policy – as going against the constitutional grain as well as that of practical politics.

The implication is that current tensions in the British state arise from attempts to fill-in government, not from its hollowing-out: the British state is, and has been, necessarily more hollow than the Westminster Model has (according to Rhodes) led academics and practitioners to expect, and that attempts to fill it in are doomed to fail. This inference can also be drawn from the study by Bulmer and Burch (2009) of the very slow, uneven and incomplete process of imposing government-wide approaches within Whitehall for dealing with Europe since 1961, albeit one that has been significantly stepped-up since 1997. In contrast, Moran’s well-received history of the British Regulatory State since the 19th century (2003) draws a much sharper contrast between the period of stable ‘club government’ which, he believes, survived for some 50 years after the First World War, and the period since 1970. The latter, he claims, is characterized by a process of intense, unsatisfying intervention by governments in an ever-increasing range of social spheres, stimulated by the loss of social solidarity and deficits in trust. This interventionism has led to a host of unintended consequences associated with excessive dependence on hierarchy and formal regulation. These consequences create ever greater imperatives for intervention, thus politicizing yet more spheres of civil society and fuelling the hyperactivity of the state.

Books such as these provide authoritative and scholarly analyses that are likely to outlive topical accounts of more ephemeral issues and to become solid additions to the disciplinary canon. They also raise important questions about hollowing out and filling in. In particular, drilling down into the empirical evidence raises questions about variations within and between countries, and also over time. How are these variations to be interpreted and explained? This is an interesting but contested question, not least because it raises issues about the proper ambitions of theory and whether academic research in this field can properly offer explanations as well as interpretations. I therefore turn, now, to a brief survey of theory and methodology in the contemporary study of government.
THEORIZING WHITEHALL: THE CONTRIBUTION OF EMPIRICAL RESEARCH

Gamble (1990) noted the declining influence of ‘discursive’ approaches that are often derided by social scientists for being a-theoretical and descriptive but which are celebrated by contemporary historians and journalists for offering the shrewd insights and accessibility for general audiences denied to more abstract academic analysis. As Gamble recognized, much of this work is actually far from a-theoretical: its theory is implicit, but it nevertheless makes general claims about how government works, the constitutional values that should underpin it, and what makes for success in official and political life. In particular, it assumes that effectiveness, either as a practitioner or as a Whitehall-watcher, depends on political skill and on particular kinds of human and social capital based in shared experience and tacit understanding of the unwritten, idiosyncratic conventions of Whitehall. Distinguished work in this tradition continues to be published both in contemporary history and political studies (see, for example, Kavanagh and Seldon 1999; Hennessy 2005), and along with the ‘inside dope’ offered by journalists with powerful connections into Whitehall (see, for example, Rawnsley 2010), still provides much of the available evidence about how government is actually carried on. For example, a very high proportion of the references cited in Bevir and Rhodes’ (2006b) study of prime ministerial government are to such sources. More recently, Blick and Jones on the ‘British Premiership’ (2010) have provided explicit theoretical underpinning for many of the implicit assumptions made in much of this work, in a book that, unusually in this field, marries social science theorizing with historical method.

Most theoretically informed work, however, tends to privilege structural explanation of change, rather than explanations relating to agency, personality or individual capacity: for example, institutionalism was by far the most preferred theory used on the Whitehall Programme. However, it tends to be used informally, to illuminate cases rather than formally to test or extend theory. For example, the work of Bulmer and Burch (see, for example, 2005) and Gains (see, for example, 1999) is relatively unusual in attempting formally to test hypotheses derived from institutional theory. Formal building of theory about Whitehall is more likely to be associated with two other approaches. The first is rational choice theory, a variant of which was adapted to explain the behaviour of top civil servants by Dunleavy (see, for example, 1991) in his ‘bureau shaping model’ and applied to the process of agencification in British government by James in a book in the Transforming Government series (2003). James holds that the outcomes of reforms such as Next Steps agencies can be explained by the satisfaction-seeking strategies of key actors who, he claims, shape institutional changes in ways that provide them with the most congenial working lives. His work thus offers a plausible explanation of unintended consequences of specific managerial reforms, and why they fail to secure their stated ‘public interest’ objectives. It also breaks new ground, in formally testing hypotheses derived from rational choice theory by means of a single, qualitative case study. The second approach uses theory that is referred to variously as ‘group-grid theory’, ‘cultural theory’ and ‘neo-Durkheimian theory’ (Hood 1996, 1998; et al. 2002; Hood et al. 2010). Perhaps because of this confused branding, this theory is less established in the study of British government than it deserves: for example, James (2010) does not mention it explicitly, although he cites work that uses it and has contributed to some of it himself. This is a pity, because, however it is labelled, it offers a potentially powerful contribution to theorizing change in Whitehall.
Although Rhodes and others discuss change in uni-dimensional terms – more or less hollowing-out or filling-in; more or less fragmentation or centralization – it is clear from Rhodes’ discussion of the Westminster model that he recognizes two dimensions that vary independently: namely, the extent to which governments rely on command (social regulation) and the extent to which they share cohesive values and goals (social cohesion). A government that conformed strongly to the Westminster Model, as propounded by Rhodes, would thus be found in the extreme top-right corner of the well-known 2 × 2 matrix offered by (Hood 1998, p. 9): it would be a pure example of what cultural/neo-Durkheimian theory labels ‘hierarchy’ (high regulation; high cohesion), rather than one that is ‘fatalist’ or ‘isolate’ (high regulation; low cohesion), ‘individualistic’ (low regulation; low cohesion) or ‘egalitarian’ or ‘enclaved’ (low regulation; high cohesion). The theory holds, however, that few real-world institutions (whether of a formal or an informal kind) are purely of one form: most are hybrids consisting in elements of more than one form, and real-life institutions could potentially be found at all points around the matrix. It further holds that each form favours particular ways of thinking and acting, which in excess and unmodified by other forms, can lead to serious dysfunctions and tensions. For example, strongly hierarchical institutions may become rigid, inflexible and over-preoccupied with status and conformity. In turn, these problems produce reactions which tend to pull strong hierarchies towards the middle of the matrix by forcing the admixing of elements from other forms. This analysis is relevant to the governance narrative, in a number of significant ways. It draws attention to the inherent instability that would be associated with a pure form of the Westminster model, and to elements in other social forms that need careful attention if a sustainable institutional settlement is to be achieved. It thus explains why attempts to fill-in government by pulling it towards greater and purer hierarchy are likely to fail. So far as the role of ideas and meanings is concerned, this theory could predict what kinds of traditions are likely to be generated by and survive in different institutional settings, and explains why the same narrative would not be uniformly powerful in different settings. Perri 6 has also argued that, by enabling us to predict what kinds of thought styles and political judgements are likely to be favoured in any setting, the theory could also predict the particular kinds of unintended or unwelcome consequences to which particular governments, departments or networks are particularly vulnerable at different times in their history, as their social forms change (6 2010). Cultural/neo-Durkheimian theory therefore has considerable potential for refining and making much more precise the theoretical claims often made about governance.

FILLING IN: BETWEEN POSITIVISM AND INTERPRETIVISM

For Rhodes, of course, the material ‘reality’ of governance is not the main issue: as an anti-foundationalist, he is interested in the ways in which different actors interpret their worlds, and how their interpretations are shaped by traditions and in turn shape institutions and events. Other articles in this volume assess the value of this approach. I will therefore resist the temptation to do so here, other than to comment that, given the high dependence of research in this field on the subjective accounts of contemporary practitioners, an interpretivist turn is long overdue. But it is not the only possible response: indeed, it is unhelpful that so many methodological exegeses, including those of Bevir and Rhodes, offer an apparently polarized choice, one moreover that is not obviously related to the methodological territory actually occupied by much – probably most – research on Whitehall.
The claim is often made that mainstream political science in Britain is positivist (for example, see Hay 2002). This means, apparently, that is quantitative; oriented to investigating relationships between variables unhelpfully abstracted from cases and context; devoted to testing theory-driven hypotheses through the use of formal models; and driven by standards of knowledge derived from natural sciences. But, to make the obvious point, hardly any of the work cited above fits this mould, and most of it uses qualitative approaches, often in the context of case studies. Does this mean that it is best regarded as ‘lukewarm’ positivism (Bevir and Rhodes 2003)? It would obviously be crass to suggest that no research on British government approaches the standards widely associated with ‘positivism’ (see, for example, Dewan and Dowding 2005; Berlinski et al. 2007). Nevertheless, the dominance of Whitehall studies by qualitative research suggests that it merits closer examination than that offered, for example, by James (2010) who treats it simply as a less formal version of quantitative research.

In fact, the qualitative research cited above uses a variety of research designs. The most common by far is the use of single or multiple case studies. This is unsurprising, because case studies are particularly useful for researching unique, complex or otherwise intrinsically interesting events or processes which abound in this field; for example, the implementation and impact of agencification, the Europeanization of policy-making or the regionalization of Whitehall departments. For such purposes it is obviously sensible to trade-off potential for generalization in order to gain more detailed, holistic understanding of the internal dynamics of the case(s). However, the single case study also encourages a tendency to the Rose problem of potentially false particularization we noted above: indeed I am struck by the frequency with which the idiosyncratic nature of the case(s) being studied is asserted in much of this work.

Other research seeks a different trade-off, one offering more scope for achieving cross-case generalization, by adopting a more formal comparative case design. Such work typically proceeds via a process of theoretically-driven concept formation to build a typology which is used to select and bound cases and to construct a consistent frame for comparative analysis (for example, see Marsh 2001; Hayward and Wright 2002) Weller et al. offer an interesting modification of this design, in that their focal unit is the function not the country: they therefore offer a useful discussion of the problem of functional equivalence (Davies et al. 1997; see also Rhodes et al. 2010). Such studies conform more closely to Dowding’s definition of ‘nonformal models’ in political science (Downing 2001) in that they systematically identify potential explanations of the different outcomes found across the population of cases, as well as offering analysis of each case. But they cannot discriminate confidently amongst candidate explanations or control their proliferation. In contrast, the series of projects conducted by Hood and his colleagues (see also 6 et al. 2002) comes significantly closer to Dowding’s concept of formal ‘modelling’, in that they involve the testing of theoretically-derived hypotheses about relationships between variables, in order to test candidate explanations. Hood et al. use mixed methods and rely heavily for key judgements on qualitative evidence: in so doing, they provide practical demonstrations of how – despite common assumptions to the contrary – variable-oriented research can be operationalized with qualitative evidence, including that consisting in the subjective accounts of practitioners. Their work also demonstrates that variable-oriented designs need not be abstracted from cases and thus divorced from context.

It can be seen, then, that contemporary Whitehall studies use a range of designs employing qualitative methods. All depend on features that, according to Bevir and
Rhodes and others, are anathema to research modelled on natural sciences: such features are not incidental or accidental, and it is therefore difficult to regard these projects simply as half-hearted or ‘lukewarm’ attempts to emulate the standards of natural sciences. More fundamentally, none of the approaches described above could plausibly lay claim to positive knowledge, if by that is meant knowledge founded on direct observation and measurement unmediated by the investigator’s theoretical constructs, analytic frames, research instruments and coding judgements. They fit much better with Marsh and Furlong’s (2002) description of realist research in that, in varying degrees, they aim to draw explanations and demonstrate causation from the available empirical evidence. So the key issue is what inferences can be drawn from their findings (6 and Bellamy 2011). There is not the room here to engage here with the well-established debate, conducted mainly in the American literature, about the inferences that can appropriately be drawn from evidence produced by qualitative research, especially case-based and case-oriented designs, let alone whether such research can and should aspire to the standards of inferential statistics (see, for example, Ragin 1987; King et al. 1994; Brady and Collier 2010). I wish simply to argue that that, given the dominance of this field by qualitative research – including research making explicit claims to test, extend and refine theory – it is surprising and unhelpful that this debate seems not to happen here. It points to the need for filling-in the methodological territory between the two extremes of interpretivism and positivism in ways that recognize the distinctive issues relevant to knowledge claims in social sciences rather than those imposed by straw men in the natural sciences. In Rod Rhodes, the cause of interpretive research has found a vigorous and effective champion: what we now need is equally vigorous and effective championing of a debate about the nature and importance of inference in qualitative, realist research to match the greater theoretical ambitions that now characterize Whitehall studies.

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