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REVIEW ESSAY

Complex Realism, Applied Social Science and Postdisciplinarity: A Critical Assessment of the Work of David Byrne

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David Byrne, *Applying Social Science: The Role of Social Research in Politics, Policy and Practice*. Bristol: The Policy Press, 2011. 240pp. ISBN 978 1 84742 451 8, hardback £65; ISBN 978 1 84742 450 1, paperback £21.99.

In this review essay I offer a critical assessment of the work of David Byrne, an applied social scientist who is one of the leading advocates of the use of complexity theory in the social sciences and who has drawn on the principles of critical realism in developing an ontological position of 'complex realism'. The key arguments of his latest book, *Applying Social Science: The Role of Social Research in Politics, Policy and Practice* constitute the frame of the review; however, since these overlap with those of his previous books, *Interpreting Quantitative Data* and *Complexity Theory and the Social Sciences*, I consider all three books together. I identify aspects of Byrne's ontological position that are in tune with the principles of original and dialectical critical realism and aspects that are not. I argue that these inconsistencies, which Byrne must resolve if he is to take his understanding of complexity further, stem from the residual influence of various forms of irrealism in his thinking.

KEYWORDS social complexity, statistical modelling, transcendental realism, pure and applied science, irrealism, postdisciplinarity

Introduction

One of the more recent 'turns' to have taken place within the social sciences is that associated with the theory of complexity. As John Urry has observed, the origins of this turn lie in developments in the natural sciences going back three decades or so, with its emergence in the social sciences being marked by the conclusions of the report of the Gulbenkian Commission on the restructuring of the social sciences[1] and the publication of edited collections on social science and complexity.[2] As one might have expected, complexity theory has attracted the attention of applied social scientists, especially those concerned with public policy making.[3] One of those applied social scientists is David Byrne, whose work should be of interest to critical realists given his declared aim of combining the insights of complexity theory and critical realism in the manner advocated by Michael Reed and David Harvey.[4] This combination, which he calls an ontology of 'complex realism' (p. 21), is the basis on which he presents the arguments of his latest book *Applying Social Science: The Role of Social Research*

[1] Gulbenkian Commission 1996, cited in Urry 2005, 2.
[2] Kiel and Elliott 1996 and Eve et al. 1997 cited in Urry 2005, 2. See also Bogg and Geyer 2007.
[3] See, for example, Haynes 2003, Geyer and Rihani 2010, and Room 2011.
[4] Reed and Harvey 1992.

in Politics, Policy and Practice, and which is the main object of the following critique. However, because Byrne refers in this book to arguments that he developed in previous work – most notably his critique of linear modelling in *Interpreting Quantitative Data*[5] and the nature of complex social systems in *Complexity Theory and the Social Sciences*,[6] I consider the arguments of all three books in my critique.

In the first section I give a brief overview of some of the main themes and strengths of *Applying Social Science* as a prelude to a critical assessment of Byrne's declared ontological position – that of 'complex realism' – in the second section, where I discuss four examples of theoretical inconsistency. I argue that these must be understood as the product of contradictions in Byrne's thinking about the nature of reality, which reflect a tension between the influences of transcendental realism on the one hand and empirical and conceptual realism on the other.[7] In the third section I consider Byrne's understanding of the nature of science and challenge his claim that the distinction between pure and applied science is of little or no value. I argue that this claim reveals the influence of pragmatism and interpretivism in his thinking, which contradicts his implicit commitment to (transcendental realist) critical naturalism. In the fourth section I turn to the issue of the production of knowledge. Here I call into question Byrne's conception of applied science as 'postdisciplinary', arguing that this overlooks the reality of the social and intellectual conditions for scientific activity (whether pure or applied) and contradicts his implicit recognition of the reality of these conditions in his critique of the UK's Research Assessment Exercise and in his discussion of the relationship between applied social science and society. The common thread in my critique of Byrne's conception of reality and of science is the finding that the contradictions and confusion in his thinking stem ultimately from the influence of (different forms of) *irrealism*. Therefore, my review may be seen as underlabouring for a more coherent theory of complexity – at least with respect to Byrne's work – and as indicating the need for him to resolve the contradictions in his thinking before elaborating further on the nature of a complex realist (social) ontology.

1. Overview

In *Applying Social Science* David Byrne is concerned, not so much with elaborating on the nature of applied social science and how it differs from that of pure social science, but with how it is produced, how it informs so-called evidence-based policy making, and how it is (mis)used by politicians and policy makers. What explains this particular take on applied social science is the ultimate objective of Byrne's argument, which is to re-define applied social science: to move from a definition that focuses on application of the results of pure (or abstract) social science to a definition that emphasizes the ways in which knowledge of social reality 'is actually constructed in the very process of active intervention in that world' (p. 4) – a definition that challenges the traditional way of doing social science, premised as that is on a distinction between pure and applied work, where the 'pure' always comes before the

[5] Byrne 2002.
[6] Byrne 1998.
[7] Note that I am following Bhaskar's definitions of 'transcendental realism', 'empirical realism', and 'conceptual realism'. See Bhaskar 1986/2009, 5–10.

'applied'. In Byrne's view 'there is a serious question to be asked as to whether there is any value at all in a conception of pure/disciplinary sciences separate from application' (p. 4). His answer to that question is that thinking of social research (at a fundamental level) as a social process precludes the possibility of making a valuable distinction between pure and applied science. In support of this answer, Byrne shows, through a plethora of examples – some drawn from his own experience of working as an applied social scientist – how the formation, implementation and evaluation of policy interventions in society plays an important part in the production of knowledge about society – a part that is often overlooked in standard textbooks on social research. In this respect his selection and discussion of examples of various social policy interventions, such as urban development and employment programmes in chapter four, is one of the strengths of the book.

Byrne's critical exploration of the relationships between the work of social researchers (whether they are based inside or outside the academy) and the work of non-academic professionals (such as social workers and urban planners) and policy makers (in local and central government) is another strength of the book. Across the main part, from chapters two to nine, Byrne evaluates continually the nature of much applied social research and points to the explanatory (in)adequacy of that which is positivist-inspired. Indeed, he states explicitly that he wants to encourage the production of 'good applied social research', where by 'good' he means, first, 'useful empirical knowledge about social reality' that is respectful of 'past, present and potential futures' and, second, 'truthful accounts of reality' that can inform democratic 'processes of governance' (p. 6). He is also critical of the use of applied social research as 'policy-based evidence', by which he means 'the selective use of research findings to assert that policies have worked, continue to work, and will work in the future' (p. 5); and, building on this point via the work of Colin Crouch,[8] is critical of the way in which much applied social science is fuelling the emergence of a 'post-democratic' society – a society imbued with an 'ideological consensus' among governing elites about 'the relationship between state and market' and 'the subordination of politics to business interests', with the result that competition between major political parties is reduced to the issue of 'technical and managerial efficiency' – of who is best equipped to deliver the fruits of capitalism (pp. 7 – 8).

Of course, in criticizing politicians' and policy makers' misuse of evidence from evaluation studies Byrne exposes the characteristically *covert* ideological agenda of policy-making elites – that is, the legitimization of policies favourable to dominant, private-sector interests. As he makes clear in chapter six, where he discusses the relationship between social science and public consultation, social scientists indirectly (and, I would add, typically unintentionally) support covert exercises in policy legitimation through, for example, generating survey data that inform 'processes of target setting and measurement' and through designing 'modes of quantitative engagement' such as public focus groups. The 'rhetoric of empowerment' that usually accompanies such exercises ensures that they neutralize public opposition and thereby work against the establishment of effective democratic forms of governance in which policy making starts not from the top down but from the bottom up – with the concerns and interests of those in the subordinated majority in society (pp. 135–7). At least in being explicit about his own ideological position – a position rooted in the principles of 'radical and

[8] Crouch 2000.

solidaristic egalitarianism' – Byrne cannot be accused of attempting to conceal an ideological agenda (p. 6).

2. Theoretical inconsistencies

In committing himself to a politics of emancipatory social transformation guided by a critical social science, Byrne is clearly working in sympathy with the ethical orientation of critical realist philosophy – that of eudaimonia. However, at various points in *Applying Social Science* Byrne appears to be out of step with the principles of critical realism and appears to show a lack of concern with elaborating a (transcendental) realist social ontology. Byrne declares his meta-theoretical position to be that of 'complex realism', which he describes as a 'synthesis' of critical realism and complexity theory (p. 20). Now, if that is the case, complex realism is first and foremost a particular ontological position entailing a particular epistemological and methodological position. Yet Byrne rarely, if at all, makes clear the epistemological and methodological principles that follow from a complex realist social ontology, despite telling us in the introduction to the book that 'Chapter One will address "the methodological foundations of applied social science"' (p. 8). In the first chapter Byrne focuses on ontology rather than methodology and mainly on the 'complex' part of complex realism when outlining the differences between 'the simple, the restricted complex, and the general complex' (p. 26). He admits to setting out 'in brutal summary the essentials of critical realism' (p. 21); but many critical realists will find his summary a little too 'brutal' – even superficial – and somewhat misleading. Granted, Byrne does give a plausible account of the principle of multiple causal determination, replete with examples from science; but his account would have been stronger if he had referred explicitly to the concepts of vertical and horizontal ontological depth (entailing the stratification and differentiation of reality). Moreover, his account of the three domains of reality – the real, the actual and the empirical (now expanded to the subjective) – fails to make clear that these are *overlapping* domains such that the empirical is a subset of the actual, which, in turn, is a subset of the real. He makes this mistake in *Complexity Theory and the Social Sciences* where he writes:

> It has to be said that Bhaskar uses the term 'real' in a more restrictive sense than that applied here, and that there is some value in his usage. For him the term 'real' should be reserved for the complex and contingent causal mechanisms and the entities which compose them … The events which happen in the world are actual. Those things which we experience are empirical.[9]

However, in failing to acknowledge that the three domains of reality are overlapping subsets Byrne is the one who is placing an unwarranted restriction on the domain of the real – not Bhaskar. In *Applying Social Science* Byrne develops further his account of the three domains and tells us that '[t]he third level is the level of the knowledge we construct about the actual and the real, the level of the empirical' (pp. 21-2). Yet, one of the basic principles of critical realism is that the production of knowledge pertains to the transitive dimension of science and is not synonymous with 'the level of the knowledge we construct about the actual and the real', as Byrne thinks, because the knowledge we produce about reality encompasses all three domains – the real, the actual and the subjective.

[9] Byrne 1998, 37–8.

Byrne's misunderstanding of the relationship between the three domains of reality in critical realism is symptomatic, I suggest, of the residual influence of irrealism, which generates inconsistencies in his understanding of the nature of social science. Let me discuss four examples of this. The first comes from Byrne's discussion of the nature of measurement in social science. Byrne is quite clearly critical of the traditional understanding of statistical modelling in social science, where identified relationships between empirical variables – the brute facts of positivist social science – are (implicitly) treated as defining of social reality. He says of this type of modelling:

> It remains predominantly linear, trapped in a view of causality which defines effects as incremental changes in disembodied continuous variables rather than state changes in whole systems, and too often fails to recognise that modelling of complex and emergent social reality is not a second-class substitute for experimental approaches to a seldom available simplicity. (p. 146)

It is the reification of variables that is the focus of Byrne's critique of positivist-inspired statistical modelling – a critique he develops at length in *Interpreting Quantitative Data* where he argues that 'the things we can measure are not "variables" but traces – the expression of the real systems that compose the world'.[10] To put this another way, the 'traces' of reality that we detect through statistical measurement are the effects of a complex conjunction of generative mechanisms, so that to explain such effects we must refer to the existence of real entities, such as social structures and social systems, having distinctive causal powers, liabilities, propensities, and so forth. Byrne acknowledges this too:

> From the traces we can reconstruct a version of the real entities *and* of the relationships among those entities *and* of the emergent forms which are the product of and producers of the relationships among those entities. We can glimpse the entities and the systemic relationships among the entities.[11]

However, the last sentence of the above quotation is problematic because the idea of glimpsing entities suggests that we can observe or measure them, and so acquire knowledge of them, directly. For example, during his discussion of the nature of measurement in chapter two of *Interpreting Quantitative Data* Byrne distinguishes between 'two types of traces that can be measured', telling us that '[o]ne type is a trace of the system that constitutes our case, whether that system is an individual, household, city, nation, bloc or whatever'.[12] But just before this part of the discussion he tells us that 'what we measure are the systems/cases' and that in order to 'avoid reifying "variables" and abstracting from real systems/cases, then we have to measure the systems/cases'.[13] Now, if our case is an individual person – and Byrne does use a person as an example of a case in his explication of 'polythetic classification'[14] – we will be dealing with a highly complex, concrete entity – a 'laminated system' as Collier has it.[15] Yet, it is because people constitute complex physical, chemical and biological systems that we cannot acquire knowledge of these systems directly through measurement; we can measure only the effects of these systems. Equally, in social research what we are measuring

[10] Byrne 2002, 42.
[11] Byrne 2002, 36.
[12] Byrne 2002, 37.
[13] Byrne 2002, 36.
[14] Byrne 2002, 37.
[15] Collier 1989, 194.

or recording when we talk to people and observe their behaviour are the effects of social systems – not the systems in themselves. That Byrnes thinks we can measure, and so acquire knowledge of, causal objects directly – what I call the empirical fallacy – is testament, I suggest, to the residual influence of empiricism in his thinking about the nature of social science.

The second example of theoretical inconsistency comes from Byrne's discussion of ways of modelling the emergent complexity of social reality. In chapter seven of *Applying Social Science* he emphasizes repeatedly the lack of 'interaction' in standard linear regression models, telling us that '[i]nteraction terms which describe complex relationships among variables are seldom fitted and almost never interpreted' (p. 143); that '[o]ther than in the all too seldom attention paid to interaction, conventional modelling cannot really handle complex causation' (p. 46); and that

> all forms of model in relation to any kind of complex social process are superior to randomized controlled trials. This is because they all, even if only in the form of interaction terms inserted into regression equations, can make some sort of allowance for complexity and emergence. (p. 153)

The problem, though, is that to model social reality either through simulating the 'interaction' of individual agents in 'multi-agent models' or through inserting 'interaction terms' into linear regression equations is to presuppose that complexity pertains only to the level of the actual and empirical.[16]

Now, there may well be good reasons to describe patterns of events, whether those detected through standard statistical techniques or those produced artificially through computer simulation, as complex. However, in both *Applying Social Science* and *Interpreting Quantitative Data* Byrne treats complexity as an emergent property of reality and thereby implies that it is a characteristic of causal entities – specifically, the nature of the relationships between them.[17] But, I suggest that, if it is the influence of transcendental realism that leads Byrne to an implicit understanding of the relationships between causal entities as complex, it is the residual influence of actualism and empirical realism in his thinking that undercuts that understanding and leads him to an explicit understanding of observed events and/or states of affairs as complex. It is only by postulating the existence of these two contradictory influences in his thinking that we can make sense of his comments that a 'simulated complex system might be considered to represent a valid metaphor for a real complex system'[18] and that 'agent-based modelling … cannot deal with the Durkheimian social, with a social reality which has an existence over and beyond the elements within the system' (p. 152). In both comments the influence of transcendental realism is undercut, and so distorted, by the

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[16] Byrne discusses computer simulation and loglinear techniques in chapter seven of *Applying Social Science*.

[17] In chapter two of *Complexity Theory and the Social Sciences* he writes:

> Chaos/complexity, because it is founded in a recognition of the non-linear character of reality, is absolutely concerned with the implications of local context expressed in terms of time and space. Chaos/complexity, because it recognizes the significance of emergent properties, asserts the emergent, distinctive and non-reducible character of the social (Byrne 1998, 47).

See also note 22, below.

[18] Byrne 2002, 138.

influence of irrealism, resulting in the (mis)representation of a complex system by an invalid metaphor in the former instance – a 'simulated complex system' – and the illicit reification of social reality in the latter – 'the Durkheimian social'.

The third example of theoretical inconsistency is Byrne's ambivalent understanding of simplification in social science. On the one hand he recognizes implicitly that social scientists, by virtue of the nature of social objects, cannot intervene in social reality in order to isolate a particular causal mechanism. As he puts it, 'In the domains to which we apply social science we are never working with the sort of simplicity which can be addressed by the bench-controlled experiment' (p. 30). (One might note in passing that he ought to acknowledge that conceptual abstraction – in this case, the analysis of complex social systems in thought – is still a possibility for social scientists.) On the other hand he appears to suggest that, because reality is so complex, the only way we can try to understand it is by producing a simplified model of it. As he puts it at the start of chapter seven of *Applying Social Science*, 'So models are representations of the world but are necessarily simplified. Something has to be left out' (p. 139). Similarly, when discussing the modelling of complexity in chapter one he tells us that 'we always simplify' (p. 28). Now, we can agree with Byrne if by simplification he means the abstraction of an object of inquiry from its context. However, it is clear from his subsequent remarks that this is not what he means; rather, he understands simplification as a way of assuming away complexity so as to generate a model that has predictive power. However, in doing so Byrne assumes implicitly that what exists at the level of the actual and empirical is complex rather than what exists beyond these levels.

Moreover, Byrne's identification of the need for simplification is premised on a correspondence theory of truth so that simple theories are a direct reflection of simple objects and complex theories a direct reflection of complex objects.[19] According to this way of thinking it is impossible for us to produce a complex theory of a complex object: therefore, we must simplify. However, as Lawson explains in his critique of similar arguments put forward by orthodox economic modellers,

> Such a conception is not sustainable … We can certainly express holistic entities, without knowingly fictionalising.… The point is that any discussion of such an explanatory causal process takes the form of words and symbols, etc. The complexity of an object is not inevitably mirrored in a similar form of complexity in the description of the object. Knowledge and its object are typically different types of 'things' with their own modes of being.[20]

The implicit correspondence theory of truth in Byrne's discussion of simplification in social science reveals, once more, the influence of empiricism in his thinking. Because this influence contradicts and so distorts the influence of transcendental realism, the combined effect of

[19] Byrne defines the 'necessary limitation of any scientific description' of a complex system as follows:

> When we are dealing with the simple we can through reductionist techniques produce adequate models of systems and of causation which are simplifications of the things we are seeking to understand. We cannot do that with complex systems. An adequate description would have to be as complex as the system itself, which … is a major problem for the simulation of complex systems. (p. 28)

[20] Lawson 2003, 307.

these influences is such that Byrne falls into the trap of the epistemic–ontic fallacy and ends up understanding complexity as something immediately observable and describable.

The fourth example of theoretical inconsistency comes from Byrne's comments about the ontological status of 'variables'. Now, as I have already indicated, Byrne is particularly concerned to reject the reification of scientific facts implicit in positivist-inspired approaches to statistical modelling, which presuppose that the events of the world are disembodied – that their existence is not be explained by any kind of underlying generative context. Byrne is quite explicit about this rejection, especially in the concluding chapter of *Interpreting Quantitative Data*, where he writes that he has been arguing against 'the abstraction and reification of aspects of real complex systems from those systems and the consequent treatment of these reified abstractions as having real and independent causal powers'.[21] Indeed, in also arguing against 'Analysis', 'Universalism', 'Linearity', 'Mathematical formalism' and 'Hypothesis fetishism' it is clear that Byrne has the panoply of positivist assumptions about (social) science in his sights. Again, it seems to me that the influence of transcendental realism is clearly in evidence here, as it is in his summary of what he is arguing for – most notably, 'Complexity' and 'Non-linearity'.[22]

Yet, in arguing against the disembodiment of variables earlier on in *Interpreting Quantitative Data*, he ends up denying that they are real. In chapter two he writes:

> So, death to the variable – or rather let us understand clearly, once and for all, that variables don't exist. They are not real. What exists are complex systems, which systems are nested, intersecting, which involve both the social and the natural, and which are subject to modification on the basis of human action, both individual and social.[23]

The problem, though, is that in denying that variables are real Byrne is denying that 'traces' are real because he is simply replacing the concept of variables with that of traces. Recall that, in Byrne's view '[v]ariables describe properties of cases but the real things are the cases, not the traces of them which we measure as variables'.[24] However, if the traces of reality that we measure when we carry out statistical modelling are not real, how can we infer from them the existence of complex systems, which we must if we are to explain them without running into the atomistic and empirical fallacies? In other words, if we accept that reality is something more than just what can be measured – that it is characterized by vertical and horizontal ontological depth – we cannot deny the existence of events that conjunctions of causal mechanisms give rise to; for the events that we observe in the domain of the empirical

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[21] Byrne 2002, 162. He makes a similar claim in the first chapter of *Applying Social Science* where he writes:

> A complex realist take regards measurements as generally being not accounts of things real in themselves which can be abstracted from the complex systems they describe, the cases – the traditional understanding of variables – but rather as variate traces of the trajectory of those cases. (p. 32)

[22] Byrne 2002, 163. By 'Complexity' he means 'understanding of the character of real complex systems in terms of wholes, parts, interaction of parts with parts, parts with wholes, and of systems with other systems in their environment, within which they are embedded, and which they contain'. By 'Non-linearity' he means 'recognition that the interesting and significant shifts in the trajectories and hence characters of complex systems are those that involve radical shifts of kind' (Byrne 2002, 163).

[23] Byrne 2002, 31.

[24] Byrne 2002, 2.

are Byrne's traces of reality and they are also the positivist's variables. As such, they do not exist as reified entities – that is, in abstraction from the systems that generate them – but they are still real; and the concepts that we devise to describe them are also real.

In this example Byrne commits the linguistic fallacy – that is, makes the mistake of assuming that the language we use to construct knowledge of social reality is defining, and so exhaustive, of social reality. Hence, in denying that variables exist, Byrne is supposing that a variable is not a concept through which we can know about an independently existing reality but is simply constitutive of reality. That is why Byrne can simply replace one concept, variables, with another, traces, and in the process re-define social reality. Yet, if both concepts are simply different ways of expressing the reality of an intransitive object, by denying that variables are real ipso facto Byrne is denying that traces are real. In short, Byrne's thinking on this issue becomes trapped in a contradiction: between denying the intransitivity of the objects that variables and traces refer to on the one hand and accepting the intransitivity of complex systems on the other. This contradiction reflects, I suggest, the dual influence of transcendental and conceptual realism in his thinking.[25]

If Byrne is to avoid the problem of theoretical inconsistency when reflecting on the nature of applied social science, he will need to clarify his understanding of the principles of critical realism before developing further the ontology of complex realism. At the very least he needs to avoid collapsing the domain of the real into the actual (actualism) and into the empirical (empirical realism), on the one hand, and avoid treating these three domains as mutually exclusive, on the other. He also needs to avoid collapsing the intransitive into the transitive dimension of science because, as I have argued elsewhere,[26] ultimately it is the failure to sustain a notion of intransitivity that makes it impossible to make sense of the differentiation and interconnection of reality. Without sustaining the idea of existential independence,[27] it becomes impossible to think about vertical and horizontal ontological depth, which is the condition of possibility for differentiation and integration in science and which Byrne

[25] Note that Byrne explicitly rejects the epistemological position of strong social constructionism and the ontological position of conceptual realism (which it presupposes):

> The realist position, which is the foundation of the complex realism argued for here, certainly allows for knowledge to be contextual – limited in the scope of its application – and relative, in the sense that it is related to that context and the way in which researchers work in that context. However, the subjective element, while present in realism, is confined to the requirements that those producing knowledge should be aware of their own subjectivity and explain how this relates to the character of the knowledge generated. It is perfectly possible to have a social constructionist understanding in a realist frame. Indeed it is essential. What realism does not accept is that social construction in the sense of the construction in the research process is the only element in the construction of knowledge. Reality itself has a voice. (p. 43)

[26] See Holland 2013, ch. 2.

[27] Our attempt to express the truth of independently existing objects presupposes the possibility of referential detachment, which Bhaskar defines as '[t]he detachment of the act of reference from that to which it refers. This establishes at once its existential intransitivity and the possibility of another reference to it, a condition of any intelligible discourse at all' (Bhaskar 1993/2008: 402–3). Byrne, though, comes very close to denying the possibility of referential detachment when he writes that '[i]t matters enormously that we are not in any real sense external observers. We are actually part of the systems themselves' (p. 28). As social scientists we are part of social reality, of course, but that does not mean that in the process of scientific inquiry we cannot 'detach' ourselves from our object of study and thereby reflect on what we normally take for granted when we are not acting as social scientists.

implicitly accepts in characterizing complex systems as involving 'both the social and natural'.[28]

Moreover, I suggest that, at the same time as clarifying his understanding of the principles of critical realism, Byrne would do well to reflect on the nature of the entities that make up complex social systems and how these entities, as parts, are related to each other and to the system – the whole – that emerges from these relationships. I make this suggestion because often Byrne fails to make clear exactly what the components of a complex system are. For example, in chapter six of *Complexity Theory and the Social Sciences*, where he discusses 'The Complex Character of Health and Illness', he does not tell us exactly what the social determinants of the spread of tuberculosis in Britain in the nineteenth century were – by which I mean which particular social structures and systems were the underlying conditions for the rapid spread of the disease. He refers generally to material poverty and inequality but still we are left wondering what were the social relations and causal mechanisms that made these material outcomes possible. Perhaps we should look to the influence of the capitalist system of production and appropriation for part of the answer. However, Byrne appears interested in discussing only the spatial aspect of the spread of tuberculosis and in the process comes close to reducing a social system to its spatial aspect when he writes: 'Here we have four systems in a nested hierarchy. The individual, the household, the community and the nation state.'[29] Yet, although social systems do extend through space (and time), they cannot be reduced to a spatial arrangement. Therefore, without a clear understanding of the nature of social structures and systems and how they are related to biological structures and systems (such as the TB bacillus, human genes and the human immune system), it becomes difficult to understand just what Byrne is referring to when he writes of 'interactive effects'.[30] To put it bluntly, what is interacting with what and how?

Having elaborated on the nature of a complex realist (social) ontology, Byrne might then turn to the epistemological and methodological implications of this – as he originally intended – and, in doing so, clarify the exact relationship between ontology, epistemology and methodology. So, at the level of epistemology, a complex realist ontology – if it is to be consistent with a critical realist ontology – ought to entail, at the very least, the principles of epistemic relativism and fallibilism (and as subject to judgmental rationality);[31] and, at the level of methodology, the principle of unity-in-diversity, which expresses both the essential

[28] Byrne 2002, 31.

[29] Byrne 1998, 111.

[30] Byrne 1998, 111.

[31] Granted, Byrne does acknowledge the principle of epistemic relativism: see the quotation from chapter two of *Applying Social Science* in note 25, above. However, it is far from clear that Byrne also accepts a position of fallibilism, for he repeatedly asserts that complex realism is foundationalist. This tendency is especially prominent in his earlier work where he tells us that '[c]omplex accounts are foundationalist, although they are absolutely not reductionist and positivist' (Byrne 1998, 35); that 'thinking in terms of society as constituted as a dissipative and evolutionary system … is inherently foundationalist'; and that '[t]he combination of the philosophical ontology of critical realism and the scientific ontology of chaos/complexity constitutes a very general, indeed absolute, claim about the nature of scientific understanding and the character of scientific investigation' (Byrne 1998, 46 – 7). Now, we could accept the characterization of the epistemological position of critical realism as 'foundationalist' *if and only if* it meant knowledge that is grounded in reality. However, Byrne's implied reference to epistemological absolutism suggests that he is thinking of foundationalism – if only implicitly – as certainty and incorrigibility in our knowledge of reality, because epistemological absolutism is the dialectical counterpart of epistemological foundationalism.

logic of inquiry that applies across all sciences – retroduction (in pure science) and retrodiction (in applied science) – and the necessary differences in the methods of scientific inquiry stemming from differences in the nature of the objects of scientific inquiry. Indeed, in developing a methodological position specific to the social sciences, perhaps Byrne might examine the concept of 'critical methodological pluralism',[32] which would help him to understand how to overcome the long-standing dichotomy between quantitative and qualitative research – an objective that is the subject of the section 'Beyond quantity versus quality' in chapter one of *Applying Social Science*. Byrne concludes this section with the comment that 'the dominant tradition in quantitative causal work, regression analysis and its derivatives, is not compatible with a proper understanding of causality in relation to complex systems but systematic comparison integrating qualitative and quantitative reasoning is' (p. 33). The problem here is that Byrne does not show how qualitative and quantitative 'reasoning' can be integrated; rather, he makes the (valid) point that both qualitative and quantitative research depends on interpretation – hence his claim that '[t]here is no meta-theoretical difference between the products of quantitative and qualitative social research' (p. 33). But, as I have argued elsewhere, the categories of critical realism do offer us a way of overcoming the dichotomy between the quantitative and qualitative research paradigms and thereby of solving a problem that mixed-methods researchers have raised – that is, how to make sense of apparently contradictory empirical outcomes arising from the use of both quantitative and qualitative modes of inquiry.[33]

3. Pure and applied social science

Let me now turn to the question of whether or not we should make a distinction between pure, abstract or theoretical science and applied, concrete or practical science. One of the conclusions that Byrne draws from his consideration of the nature of contemporary social research is that this distinction is no longer of any value. Taking his cue from definitions of 'Science', 'Applied', 'Applied science' and 'Social science' as found in the *Oxford English Dictionary* and *Wikipedia*, he informs us:

> This book will challenge the ordering of 'pure' or discipline-based science and applied science.… it will argue that much and perhaps most of the useful knowledge about the social world is actually constructed in the very process of active intervention in that world … Pure/disciplinary science does not come first. Indeed in relation to the essentially social, there is a serious question to be asked as to whether there is any value at all in a conception of pure/disciplinary sciences separate from application. (p. 4)

Yet, we can recognize that knowledge of social reality is produced through practical engagement with social reality *and also* recognize the value of distinguishing between pure and applied social science. The basis for this dual recognition is of course the nature of reality. If this is characterized by vertical and horizontal ontological depth – that is, if it is structured, stratified and differentiated – events and states of affairs must be understood as subject to 'multiple determination' – that is, produced by a multiplicity of causal mechanisms lying at

[32] Danermark et al. 2002, 153.
[33] See Holland 2013, ch. 2.

different levels of reality.[34] Pure, abstract or theoretical science, therefore, is necessary in order to identify different types of structure and their ways of working – the generative mechanisms set in motion when the causal powers, liabilities, and propensities pertaining to structures are triggered. Bhaskar has set out the sequence of processes defining this type of science. 'Theoretical explanations', he writes,

> are *analogical–retroductive*, exhibiting what I shall label the DREI schema: i.e. *description* of law-like behaviour; *retroduction*, exploiting analogies with already known phenomena, to possible explanations of the behaviour; *elaboration* and elimination of alternative explanations; issuing (ideally) in the empirically-controlled *identification* of the causal mechanisms(s) at work.[35]

In theoretical social science, then, our goal is to identify different types of social structure and thereby to produce a classification of the social order – a social scientific ontology. In doing so we move backwards – that is, we follow a 'retroductive' logic – from description of some significant pattern of events – a causal law – to the mechanism and hence structure responsible for producing it. Hence, in our theoretical work we are dealing with causal objects *in the abstract*: that is, we are trying to understand how a particular type of structure would work if its causal powers, liabilities, and propensities were triggered in isolation of the effects of other types of structure.[36] Of course, owing to the nature of social objects of inquiry – viz. the fact that they depend on people – we cannot isolate the effects of individual social structures and so test our theories about them in an experiment; hence, we have to produce explanations of social phenomena entirely in thought and assess their validity not according to their predictive power but according to their relative explanatory power.

By contrast, applied, concrete, or practical science is necessary if we are to understand which of the various structures and mechanisms that we have identified through our theoretical work are responsible for generating particular events and states of affairs of interest. In this type of scientific work our concern is to understand how a conjuncture of causal mechanisms is articulated and differentially weighted; that is, our concern is to understand the nature of the relationships between the component structures and how these relationships modify the effects of the component structures when their causal powers are triggered. Indeed, to the extent that the relationships between the component structures are internal as well as external and contradictory, so that there is a constant tendency towards change, the conjuncture must be described as a 'partial totality' and as exhibiting 'holistic causality'.[37] Now, because we could never have deduced the outcome of this complex interplay of causal mechanisms from knowledge of how each structure works in isolation, we must employ a different sort of inquiry – practical or applied inquiry – that takes us from the abstract back towards the concrete. Bhaskar defines the logic of practical scientific inquiry as follows.

> Practical explanations, i.e. explanations of particular concrete phenomena, are especially tailored to open systems, the normal condition of things. They are *decompository–retrodictive* in structure,

[34] Bhaskar 1993/2008, 82–3.
[35] Bhaskar 1986/2009, 68.
[36] As Bhaskar puts it, in pure scientific work we are concerned primarily with 'elaborating the implications of the structure, empirically identifying and confirming its nature and properties, developing the explanatory-taxonomic niche into which the structure fits, etc.' (Bhaskar 1986/2009, 214).
[37] On the nature of 'totality' and 'holistic causality' see Bhaskar 1993/2008, 126–7. See also the discussion of these concepts in Norrie 2010, 90 – 6.

> exhibiting the RRRE schema: viz. *resolution* of a complex event (situation, etc.) into its components; *redescription* of these components in theoretically significant terms; *retrodiction*, via independently validated normic or tendency statements, to possible antecedents of the components; and *elimination* of alternative possible causes.[38]

Practical or applied explanation is 'decompository' because we 'decompose' an event or state of affairs into its antecedent causes through analysis, and, through synthesis, we show how the antecedent causes combine to generate the event or state of affairs of interest (the explanandum). Thus, we are not identifying previously unknown causes; rather, we are deciding which *known* causes are relevant to the situation of interest – a logic of *retrodiction*.

Now, if in practical or applied explanation we draw on pre-existing knowledge of causal mechanisms, it follows that abstract or theoretical work is the pre-condition for applied or practical work. Therefore, it is in this sense that we can say that pure science 'comes first'. However, this does not preclude the possibility that in the course of investigating a concrete phenomenon we will move back and forth between pure and applied modes of inquiry in a dialectic of the abstract and concrete. As social scientists we tend to employ both modes of inquiry when addressing concrete problems because it often happens that, in the course of applying pre-existing knowledge of causal mechanisms in relation to empirical evidence, we have to revise our understanding of those mechanisms in order to construct an adequate explanation of the particular event in question.[39] Yet, although we do tend to employ both theoretical and applied modes of inquiry within the same investigation, this does not mean that we should not make a clear distinction between the two; for such a distinction is valuable if it helps us to avoid the dangers of theoretical reductionism and ontological actualism – that is, of supposing that the effects of the causal mechanism that we have understood through abstraction are always actualized. In other words, recognizing that pure and applied modes of inquiry are distinct from, yet related to, one another helps us to avoid engaging in 'four modes of *illicit abstraction*, viz. destratification, deprocessualization, demediation and desingularization',[40] and so helps us to comprehend both the differentiation and interconnection of reality.

If it is the nature of reality, ultimately, that licenses a distinction between pure and applied modes of scientific inquiry, does Byrne's claim that 'much and perhaps most of the useful knowledge about the social world is actually constructed in the very process of active intervention in that world' call into question this distinction? What his claim alludes to, I suggest, is the relationship between theory and practice. Now, when policy makers intervene in social reality in order to change it – which is what Byrne appears to be alluding to by the phrase 'process of active intervention in that world' – they are engaging in a particular kind of practice, and they will develop practical explanations in line with the (scientific) theory informing the design of the policy intervention. But, if we accept, as critical realists do, that all theory is fallible, we should not be surprised to find that policy makers' expectations are

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[38] Bhaskar 1986/2009, 68.

[39] Bhaskar appears to have recognized as much when he revised his model of applied explanation to include an 'I' and a 'C', where the 'I' stands for the identification of 'a full enough set (which may comprise a totality) of causes for a concrete applied explanation to be said to have been provided' and the 'C' stands for 'correction' – so that we have the extended acronym RRREI(C). See Bhaskar 1993/2008, 133.

[40] Bhaskar 1993/2008, 130. These errors fall under the problem of the 'fallacy of misplaced concreteness'. See the entry 'concrete/abstract' in Hartwig 2007, 71 – 3.

often confounded by people's experiences following the implementation of the policy. Ideally, any inconsistency between expectations and actual results should lead us to revise the theory that informed the intervention; the revised theory should, in turn, become the basis for a new policy of intervention so that we have a process of dialectical reasoning in which theory informs practice and practice informs theory.

The duality of theory and practice is a relationship that is distinct from (although related to) the relationship between pure and applied science. The two relationships are distinct because applied scientific inquiry is a mode of explaining particular events and states of affairs by means of antecedently established knowledge of reality (normic statements) and is thus defined in relation to pure scientific inquiry, whereas practice in relation to theory refers to human activity in general – to the power of human agency (or intentional transformative praxis) in exercise. Indeed, if we see pure and applied scientific inquiry as different types of practice, and as both (explicitly) informed by (philosophical and social scientific) theory and (implicitly) presupposing such a theory, we can see the relationship between pure and applied scientific activity as constellationally contained within practice. The value of the distinction between pure and applied science, though, is that it helps us to locate the sources of error in the process of producing knowledge and thereby helps us to find a way of reducing the degree of inconsistency between theoretically-informed practical expectations and actual, practical experiences. In claiming that the construction of knowledge via practical engagement with reality invalidates the distinction between pure and applied social science, therefore, Byrne appears to be denying the possibility of vertical and horizontal ontological depth – a denial that may well be the result of the influence of pragmatism and interpretivism in his thinking.[41]

Let me discuss one particular theory–practice inconsistency as a way of clarifying the preceding argument. Suppose a social scientist noticed that higher education policy makers' expectation that establishing independent interdisciplinary research institutes and centres would facilitate the production of genuinely integrative research had not been realized in practice. The social scientist, having examined critically the theory presupposed by such a policy, might conclude that it was inadequate to the extent that it reflected an erroneous positivist understanding of knowledge and of the production of knowledge – an understanding of science as a purely individualized process involving the recording of constantly conjoined, naturally-given facts. With this critique in mind the social scientist might argue that, in effect, policies intended to facilitate the production of integrative, interdisciplinary research assume (erroneously) that, if scientific specialists collect specialized facts (political, economic, sociological, geographical, and so forth), interdisciplinary science will be a matter of bringing together individual specialists, who will identify regular, empirical relationships between different types of facts. Turning to the distinction between pure and applied social science, the social scientist might show how this implicit and inadequate

[41] Byrne discusses the philosophy of pragmatism in chapter eight on action research and appears to be critical of it: 'The problem is that the pragmatic turn is a turn away from any kind of non-contextual knowledge, however limited the bounding of application of that knowledge. We lose structure when we take this turn' (p. 159). I suggest that it is in virtue of the influence of (transcendental realist) critical naturalism that Byrne recognizes the problems associated with the pragmatist position. Nevertheless, his questioning of the distinction between pure and applied scientific inquiry and his emphasis on the construction of knowledge of social reality through practical engagement with it – as opposed to the transformation of pre-existing cognitive resources through a process of dialectical reasoning – indicates the residual influence of pragmatism and interpretivism in his thinking.

conception of the social and intellectual basis for scientific research (whether monodisciplinary or interdisciplinary) reflected – through a misunderstanding of the nature of abstraction – a problem of pure social science; that is, the social scientist might show how a conception of scientists as simply individuals collecting facts in a regular fashion presupposes that the way of working of the social and intellectual context of knowledge production is always actualized. In other words, by re-situating the initial problem within a (dialectical) critical realist ontology and so remedying the faulty abstraction, the social scientist would be in a position to explain two important tendencies related to the initial problem: the tendency for scientists to produce specialized rather than integrative forms of knowledge and the tendency for scientists whose intention is to produce integrative forms of knowledge to do so relatively incoherently. From this explanation the social scientist might draw the following policy conclusion: that, in order to encourage the production of integrative, interdisciplinary research, higher education policy makers would need to (a) transform the social conditions of knowledge production by making it possible for scientists to pursue academic careers on the basis of the production of integrative as well as specialized forms of knowledge; and (b) transform the intellectual conditions of knowledge production by making transcendental realism the explicit basis for the education and training of scientists. If such policies were implemented, the social scientist might expect to detect a weakening of the tendency for scientists to produce specialized rather than integrative forms of knowledge and a weakening of the tendency for scientists to produce incoherent forms of knowledge intended to be integrative. The social scientist might try to detect such changes through quantitative investigation of the actual types of research that had been produced over a restricted period of time and through qualitative investigation of the philosophical coherence of any research outputs produced and proclaimed to be integrative forms of knowledge within the same time period.

Now, if the social scientist's practical expectations were confounded – that is, if the social scientist concluded on the basis of the empirical evaluation of the policy changes that the tendencies under examination had either retained the same strength or had weakened only marginally – the social scientist might re-visit the explanation that informed the policy intervention and identify possible sources of error. One source of error in the construction of the explanation might be the process of abstraction; another might be the process of concretion, through which the social scientist understands how the various social and intellectual structures identified through the process of abstraction fit together as systems or totalities. Perhaps the social scientist misunderstood the nature of the connections between the funding of scientific research, peer review of research proposals and outputs, academic employment, and the structure of scientific fields. Alternatively, the confounding of practical expectations might be the result of the (inadequate) design of the policy intervention, reflecting a fault in the way the policy maker translated general policy conclusions into specific policy proposals. Perhaps the policy maker overlooked the importance of transforming the intellectual as well as the social conditions of knowledge production with the result that the UK academic community still tended to misunderstand the nature and importance of scientific integration. Indeed, the social scientist and policy maker might conclude that they should work together closely in order to overcome both the problem with the construction of the explanation and the problem with the design of the policy intervention. As Danermark argues, the relationship between scientist and policy maker 'must be a reciprocal learning process' in which the scientist helps the policy maker to understand

the causal complexity of reality and the policy maker helps the scientist to understand how that causal complexity is manifest in particular outcomes.[42]

The upshot of the preceding discussion is that the distinction between pure and applied modes of scientific inquiry has considerable value – all the more so once the implications of the relationship between theory and practice are understood. Because we often employ both modes of inquiry when investigating concrete phenomena, it may appear as if the distinction between the two is of little consequence. However, it is important to understand both the distinction and the connection between the abstract and concrete for the reasons just elaborated.

4. Postdisciplinarity?

I suggested above that the influence of pragmatism and interpretivism in Byrne's thinking leads him to deny that the distinction between pure and applied social science is of any value. I also want to suggest that Byrne's questioning of this distinction may stem from his forming an identity between pure science and what he calls 'discipline-based' or 'disciplinary science' (p. 4), which is the implicit contrast for his forming an identity between applied social science and 'postdisciplinary' work:

> we can see applied social research as it is done by all disciplines and fields and wherever it is based in terms of departmental identity as generally postdisciplinary. That is to say, geographers, health studies people, sociologists, educationalists and so on do not do applied social research in any discipline- or field-distinctive fashion. (p. 185)

These identity relationships are illicit because the basis for 'disciplinary science' may be either pure or applied science. Why is this? In referring to the disciplining of scientific activity we are referring to the necessary effect of the underlying social system of knowledge production; therefore, how this system works will determine whether the basis for the disciplining of scientists is pure or applied work. Indeed, in claiming that '[p]ostdisciplinary work in effect returns to the original position of the human sciences before the development of disciplinary boundaries and academic and intellectual empires' (p. 178) Byrne appears to acknowledge, implicitly, the underlying conditions for scientific activity. Byrne also appears to acknowledge the existence of these conditions in his discussion of 'the pernicious influence of the UK's research assessment exercises'. In recognizing that research funding and peer review mechanisms are interconnected through the Research Assessment Exercise (RAE) – now the Research Excellence Framework (REF) – and that 'the RAE process has operated to the disadvantage of all of interdisciplinary work, applied work and work which relates to professional practice', Byrne is appealing implicitly to the effect of an underlying set of social structures (p. 187). Moreover, in his discussion of a British Academy report on social science and public policy making Byrne takes note of one of the report's statements, 'that policy makers want research findings that … support existing ideologies and are uncontentious',[43] and concludes that '[w]hat is required from applied social science is work which accords with

[42] Danermark 2002, 62.
[43] British Academy 2008, 3.

the hegemonic status quo' (p. 180). In drawing this conclusion, Byrne acknowledges – again implicitly – the pre-existing ideological context in which scientific inquiry is embedded.[44]

Nevertheless, in arguing that concrete fields of science such as urban and health studies have become 'postdisciplinary' Byrne reveals, I suggest, the much greater strength of positivism in his thinking. On the one hand the influence of positivism undercuts the (implicit) transcendental realist conception of the disciplining of scientific activity through the social system of knowledge production and leads him to conceive of the present tendency towards scientific specialization as a fixed, universal, regularity; on the other hand the strength of the influence of positivism – acting most probably in conjunction with interpretivism (the dialectical counterpart of positivism) – leads Byrne to see applied scientific fields such as urban and health studies as devoid of any materially-based structuring so that, in effect, scientists are free to produce whatever form of knowledge they choose – whether this is called 'postdisciplinary' or something else. It may be the case that applied fields of science such as urban and health studies tend not to be practised within their own academic departments. However, the fact that these fields of scientific inquiry exist and continue to be reproduced suggests that the scientists working in them are disciplined just as much as those working in pure fields. In other words, the reproduction of applied fields of science would not be possible without an underlying context of social structures and intellectual forms; for, whether one is working in an abstract field (such as political science), a concrete field (such as urban studies), or an intermediate concrete field (such as economic history), the academic expectations of scientific elites – in a context in which scientific specialization is the dominant tendency – will be largely the same: to produce scientific work that can be identified as

[44] In acknowledging the influence of underlying intellectual forces on the sort of knowledge that social scientists produce, Byrne touches on an important relationship – that between social science and society. His point about the 'politicized' nature of social research – the way in which applied social science enters public debate through 'the selective use of research findings to assert that policies have worked' (p. 5) – also touches on this relationship: specifically, the interdependence of social science and society, where the maintenance of a particular organization of society – one dominated by capitalist relations of production and appropriation – depends on the production of positivist-inspired social science (because positivist knowledge obscures the reality of underlying social structures and hence the basis for radical social transformation), and where the continuation of social science depends on the material resources that society provides. Byrne appears to have overlooked the significance of this relationship. For example, in chapter nine of *Applying Social Science* he asks:

> Why … did … the UK ESRC and the AcSS Commission attach so much value to competency in complex modelling and mathematical deductive procedures that the second argued for higher rates of grant for PhD students working in that mode, and the first accepted this argument and constantly demands ever greater levels of mathematical sophistication across the social sciences as a whole? (p. 183)

Byrne's answer turns on the relative level of functional innumeracy of UK social science undergraduates. There may well be some truth in this. However, a deeper explanation, surely, must look to the influence of dominant, taken-for-granted irrealist forms of thought within society – especially positivism. These forms of thought are the pre-conditions for the funding of the majority of social research and help to explain the fetishization of quantitative social science and mathematical modelling among higher education research funders and policy makers. Because social scientists depend on society for material resources, society can influence the sort of knowledge that social scientists produce; and because society depends on social scientists to produce knowledge of it, social scientists can influence how those outside the scientific community understand the nature of society. (The entry of social scientific research findings into public debate via the media and the employment of social science graduates in non-academic professions, such as the civil service, are two important ways in which social science affects society.) In short, the relationship between social science and society is one of causal interaction as well as causal interdependence. See Holland 2013, ch. 5 for a full discussion of this important relationship.

belonging to a particular field of inquiry and can be evaluated by experts working in that field. Of course, what signals in part the identity of the work scientists produce is the type of journal that they publish it in. But, to talk about journal publication and peer review is to invoke all the other social structures that make up the social system of knowledge production – and the ideas that constitute the intellectual system – by virtue of which scientific fields, whether pure or applied, are reproduced.[45]

Further evidence in support of my argument that Byrne's thinking about the nature of science is infected by the dual influence of positivism and interpretivism are his claims that 'the actual way the research is done bears little relationship to the actual discipline or field label of those doing it' and that 'the actual nominal classification of those doing it [applied work] has almost no influence on how it is done' – claims apparently motivated by his observation that applied social scientists draw on 'the whole repertoire of social research techniques' (pp. 185–6). Yet, many fields of science that Byrne would not classify as 'postdisciplinary' are just as methodologically diverse as applied fields such as urban studies. Political science is one example.[46] Moreover, Byrne also seems to be suggesting that the classification of fields of scientific inquiry has no real basis – that there is no relationship between the 'nominal classification' of scientific fields and the ways of working within them. The implication of this line of thought, which betrays the influence of the philosophical position of conventionalism, is that the label 'urban studies', for example, is just that – a label – and not an attempt to express, however fallibly, the truth about the nature of a concrete object of inquiry – in this case, the urban environment. I doubt that Byrne would want to accept such an interpretation of the classification of scientific fields but that is the outcome of the influence of irrealism in his thinking; for the empirical realist and conceptual realist ontology implicit in positivism and interpretivism respectively makes it very difficult for us to understand both vertical and horizontal ontological depth, which is the real basis for the differentiation (and integration) of scientific fields. This implies that the labels we use to define scientific fields – 'political', 'economic', 'sociological', 'geographical' and so on – have a real reference point (that is, an intransitive object of inquiry) and that such labels may have to be revised, and new ones added, as our knowledge of reality develops (the principle of epistemological relativism). A positivist conception of science, therefore, cannot sustain a coherent distinction between pure and applied forms of science because it cannot justify either scientific differentiation – the rationale for pure science – or scientific integration – the rationale for applied science. With a positivist conception of science in mind, it is not surprising that Byrne should deny that the distinction between pure and applied social science is of any value.

Conclusion

In this review essay I have evaluated David Byrne's philosophical position as set out in three of his books: *Applying Social Science*, *Interpreting Quantitative Data,* and *Complexity Theory and the Social Sciences*. Byrne professes to have developed a philosophical position of complex realism – a marriage of complexity theory and critical realism – in conjunction with a critique of positivist-inspired statistical modelling and measurement and a questioning of

[45] For a full exposition of this argument see Holland 2013, chs 3, 4, and 5.

[46] The methodological diversity of political science is discussed in comparison with the methodological position of economics in Holland 2013, ch. 4.

the traditional distinction between pure and applied scientific inquiry. However, I have argued that Byrne's actual philosophical position is characterized by a series of contradictions, manifest in

(a) a misunderstanding of some of the key principles of critical realism – most notably, the relationship between the three domains of reality and between the intransitive and transitive dimensions of science;
(b) a tendency to commit the empirical fallacy when reflecting on the nature of measurement in social science;
(c) confusion about the location of complexity in his proposals for quantitative modelling of the complexity of social reality;
(d) an ambivalent understanding of simplification in (social) scientific inquiry;
(e) confusion about the ontological status of variables and traces of reality in statistical modelling;
(f) a denial of the validity of distinguishing between pure and applied modes of (social) scientific inquiry and thus a denial of the real grounds for the differentiation and integration of science.

I have argued that these contradictions are symptomatic of the residual influence of various forms of irrealism in his thinking – principally, positivism, interpretivism, and pragmatism. Therefore, I suggest that, before Byrne elaborates further on the nature of a complex realist (social) ontology, he should clarify his understanding of the principles of original and dialectical critical realism – at least if he wants to claim the badge of theoretical consistency. Doing so will help him to define a clear methodological position that is consistent with an ontology of complex realism and that can accommodate the integration of quantitative and qualitative modes of social scientific inquiry, as well as to specify the exact nature of the components of complex social systems and their modes of articulation.

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