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Standing on the Shoulders of Giants: How Do We Combine the Insights of Multiple Theories in Public Policy Studies?

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The combination of multiple theories in policy studies has a great potential value—new combinations of theories or concepts may produce new perspectives and new research agendas. However, it also raises important ontological, epistemological, methodological, and practical issues that need to be addressed to ensure disciplinary advance. This article identifies three main approaches: synthesis, *in which we produce one theory based on the insights of multiple theories;* complementary, *in which we use different theories to produce a range of insights or explanations; and* contradictory, *in which we compare the insights of theories before choosing one over the other. It examines the issues that arise when we adopt each approach. First, it considers our ability to "synthesize" theories when they arise from different intellectual traditions and attach different meanings to key terms. Second, it considers the practical limits to using multiple theories and pursuing different research agendas when academic resources are limited. Third, it considers the idea of a "shoot-out" in which one theory is chosen over another because it appears to produce the best results or most scientific approach. It examines the problems we face when producing scientific criteria and highlights the extent to which our choice of theory is influenced by our empirical narrative. The article argues that the insistence on a rigid universal scientific standard may harm rather than help scientific collaboration and progress.*

KEY WORDS: punctuated equilibrium theory, Advocacy Coalition Framework, rational choice, evolutionary theory, complexity theory

Introduction

The combination of multiple theories in policy studies is like a valence issue in politics: Few would disagree with the idea, largely because the sentiment is rather vague. Who would not want to combine the insights of a wide range of theories and studies to advance our knowledge? There is more scope for disagreement when we consider the *details* of that process, for two reasons. First, we may not agree on what "theory" means and what purpose a theory serves. This article defines a theory very broadly as "a set of analytical principles designed to structure our observation and explanation of the world" (Cairney, 2012a, p. 5). More specific definitions may better guide our research but also demonstrate different uses of theory in the policy

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literature. For example, Ostrom (1999, pp. 39–40) makes the widely accepted distinction between *frameworks*, which identify relevant concepts and help organize analysis and theoretical comparison; *theories*, which make general assumptions about the causal relationships between concepts; and *models*, which make particular assumptions about particular objects of enquiry. However, wide acceptance only goes so far: Most agree that theories help "explain processes," but many would object to the idea that they help "predict outcomes" (1999, p. 40). There may be common acceptance of the idea that theories help us generalize, to identify common elements in multiple studies across time and space, but different definitions of theory are linked strongly to the different assumptions we make about the nature of the world and our ability to understand it.

Second, the policy literature contains many different ways to combine theories and concepts. This point is the focus of this article that highlights the philosophical and practical issues that arise when we adopt different multitheoretical approaches to the study of public policy. The multiple theories issue is particularly high on the public policy agenda, partly because the policy literature is often characterized (rather unfairly) as *ad hoc* and case-study based, rather than driven by the testing of hypotheses derived from "generalizeable theories" (Smith & Larimer, 2009, pp. 15–17). This may reflect the complex nature of policy studies in which "The practice of decision-making rarely arranges itself in a manner suitable for the testing of hypotheses" (John, 2012, p. 9). The use of multiple theories is also particularly relevant to an endeavour like policy studies that derives so many insights from a range of disciplines. However, most issues raised in policy studies also apply to political science and, in many cases, the social and natural sciences as a whole. For example, there is a common ontological concern about the nature of the world and how our concepts relate to it. We can also find similar debates about the methods of political science and, in particular, the search for meaning and understanding that may not involve the "testing of hypotheses." Perhaps wider still, we may identify similar debates among scholars who have adopted the same theory but disagree on how best to apply established methods. In fact, some of the most heated "theoretical" debates may relate as much to methodological issues based on our fundamental beliefs about the role of science (Poteete, Janssen, & Ostrom, 2010, pp. 8–11). In this light, the article identifies three main multitheoretical approaches in public policy research, identifies the issues that arise, and highlights their wider scientific applicability.

The first approach is "synthesis," in which we combine the insights of multiple theories, concepts, or models to produce a single theory. The meaning of "synthesis" often varies by author. Indeed, it may be distinguished analytically from "super-synthesis" if synthesis involves creating a new theory and super-synthesis involves creating a hybrid—but the distinction is not always clear. This approach raises issues regarding the intellectual origin and compatibility of synthesized theories. If we gather and combine the insights of multiple theories, can we be sure that they share the same understanding of the world and how our concepts relate to it (the ontological question) and/or our ability to gather and accumulate knowledge of it (the epistemological question)? These issues arise when we consider the compatibility of different approaches to concepts such as "new institutionalism" and are multiplied when we consider ways to synthesize theories that involve multiple concepts or cross-disciplinary boundaries. They also arise in a less direct, but more practical, sense when we consider the use of key terms—such as "evolution," "punctuated equilibrium," or "policy entrepreneur"—which may have different meanings and refer to different phenomena within different intellectual traditions. In this context, can we be sure that a synthetic theory produces an internally consistent approach based on the insights of many studies?

The second, "complementary," approach uses multiple concepts or theories to produce a series of perspectives with which to explain empirical outcomes. The most famous exponent is Allison (1971), who adopts three broad approaches to rationality (in states, organizations, and individuals) to explain the Cuban Missile Crisis. Others have extended this approach to a comparison of theories of public policy (Cairney, 2012a, p. 3). With this approach, ontological and epistemological issues may give way to considerations of the most appropriate way to design research (the methodological question). In many cases, the comparisons of explanations are often superficial. Most studies present an empirical case study, then set up a summary of several theories and use those theories to identify a series of perspectives. They do not involve the wholesale adoption of the research methods of each theory, largely because it is impractical to do so. They adopt a proxy version and compare their insights in a much more limited way. In this context, can we be sure that a complementary approach does justice to the range of insights produced by (or methods associated with) these theories?

The third approach involves the devotion of a more substantial amount of resources to individual theories before we compare them. Each theory may be associated with its own empirical research agenda and, for example, produce multiple case studies under a common banner (such as the Advocacy Coalition Framework [ACF] or the Policy Agendas Project). We may seek to combine the insights of multiple theories but accept that they are relatively independent of each other (when compared with the idea of synthesis). This may involve elements of the synthetic and complementary approaches, in which we accept insights from a mix of theories that often appear to employ very different assumptions and methods (Della Porta & Keating, 2008; Keating, 2009, p. 297). Or, it can involve the main focus of this article: the selection and rejection of theories according to their adherence to identified tenets of science. The article labels it as "the contradictory approach" and links it to the idea of a "policy shootout." Although the "shootout" analogy is made with tongue in cheek, the editors of a special issue of the *Policy Studies Journal* (Eller & Krutz, 2009, p. 1) share with Sabatier (2007a, p. 5; 2007b, pp. 326–31) the belief that widely accepted scientific principles can be used to select and reject different approaches. In this context, the issue of ontological, epistemological, and methodological compatibility *appears* less pressing because the theories are now engaged in a degree of competition. However, it does not go away, because we must be confident that we can produce a common understanding of our scientific research endeavour. Our attention turns to the problems we face when seeking criteria to determine the "most scientific" or, at least, most useful theory in different circumstances.

This article addresses these issues. First, it considers our ability to "synthesize" theories, using the examples of evolutionary and complexity theories. Second, it considers the practical limits to using multiple theories by considering the research methods employed by the Policy Agendas Project, ACF, and interpretive approaches. Third, it examines the extent to which prominent theories adhere to the same scientific principles, comparing the idea of a universal scientific standard with a "post-positivist" alternative. It argues that there is not "one best way" to combine theories.

Synthesis (and Super-Synthesis)

John (1998; 2003, p. 483; 2012) is a key proponent of the "synthetic" approach, but a detailed examination of his position demonstrates that "synthesis" can have multiple meanings. His aim is to produce a new "synthetic" theory that accounts for the complexity of the policymaking world but remains parsimonious. He argues that the policymaking world is difficult to study because it involves the complex interplay between five key processes associated with: the role of institutions as sets of rules or norms that structure or influence behavior; policy networks (subsystems), or the relationship between policymakers and the actors with which they consult; exogenous factors, such as socioeconomic conditions; the choices made by actors; and the ideas that help guide their behavior. There are significant literatures devoted to each of these factors, including new institutionalism, policy networks analysis, structural approaches, rational choice analysis and, for example, constructivist or interpretive approaches to ideas. However, John (2012, p. 12) highlights their tendency to "offer self-contained worlds from which to view the policy process," even though (or perhaps because) many "emerged in reaction to each other." Furthermore, a simple combination of these approaches would not work because it would produce "over-determined" explanation and a model too flexible or vague to ever be proved wrong: "Instead, the form of explanation needs to be a theory that integrates the five processes in such a way that the causal relationships between them are clear" (2012, p. 14). This aim raises the possibility of an analytical distinction between synthesis and "super-synthesis": The former provides a new theory to account for the relationship between the five factors; the latter provides a hybrid theory that combines the insights of a range of theories. The distinction may be difficult to maintain in practice but, as discussed below, it highlights two separate concerns when combining the insights of theories.

John (1999, p. 168; 2003, p. 488) argues that the " 'state of the art' in 1990s US policy studies" come close to successful synthesis because they try to conceptualize "the relationship between the five core causal processes." For example, Kingdon's multiple streams analysis "embraces the relative importance of individual agents, ideas, institutions and external processes" (John, 1999, p. 173). Baumgartner and Jones's (1993, 2009) punctuated equilibrium theory combines a discussion of bounded rationality, policy communities, and agenda setting to explain why long-term policy continuity is replaced sometimes by short but profound periods of policy change. Sabatier and Jenkins-Smith's (1993) ACF combines a means to theorize: a shift from iron triangles to more open and competitive group–government relations within subsystems; the role of ideas and power; action based on beliefs rather than self-interest (drawing on Majone, 1980); and the effects of socioeconomic and "external" causes of policy change. John (2003, p. 495; 2012, chap. 8) identifies limitations to each approach but argues that public policy research is still following their agenda. Thus, the challenge is to improve the same basic approach.

John (2003, p. 495; 2012) uses rational choice to provide a "theory of action and motivation," guided by a wider understanding of the role of institutional and exogenous constraints. Like Kingdon (1984) and Baumgartner and Jones (1993, 2009), he draws on a form of "evolutionary theory" to account for the role of ideas in shaping the beliefs and preferences of actors (see Cairney, 2013, for a review of evolutionary theories). The strategies of actors "evolve" as individuals learn to cooperate with each other and adapt to their environments. They learn, and their preferences change, when they formulate and adapt particular ideas that also "evolve" or "mutate" in different policy environments. This is "structured evolution" (John, 1998, p. 186) influenced by institutional, network, and socioeconomic constraints. Overall, the theory is "synthetic" because it accounts for the five main causal processes in policymaking, highlighting the power struggle to promote the acceptance of particular ideas (or ways of defining and solving policy problems) within a world of rules, relationships, and "external" factors that provide resources to, and help shape the beliefs of, participants.

We can also treat "complexity theory" as synthetic since it: (i) addresses the five main causal processes; and (ii) represents a combination of insights across the natural and social sciences. Indeed, it is this interdisciplinary nature of the theory that makes it so analytically useful (for the purposes of this article), as any problems of "synthesis" that we can identify in policy theory (below) may be multiplied when we try to engage in meaningful interdisciplinary work. Complexity theory (although in practice we can identify a number of theories under this umbrella term—Cairney, 2012b, 2013) has been applied to a wide range of systems to explain "emergent" behavior in the natural and social world—from bees swarming, to thoughts and feelings emerging from cells and neurons in the brain, and emerging forms of cooperation among social groups. In policy studies, it is often used to identify instability and disorder in policymaking, particularly when systems appear resistant to top-down central government control and, instead, policies "emerge" at a more local level (Cairney, 2012a, pp. 124–26; Geyer & Rihani, 2010, pp. 23, 29; Room, 2011, p. 7). Complexity theory suggests that we shift our analysis from individual parts of a system to the system as a whole and as a network of elements that interact and combine to produce systemic behavior (Blackman, 2001; Bovaird, 2008; Mitchell, 2009; Mitleton-Kelly, 2003; Sanderson, 2006, 2009). A complex system cannot be explained merely by breaking it down into its component parts, because those parts are interdependent: Elements interact with each other, share information, and combine to produce systemic behavior. Complex systems exhibit "non-linear" dynamics produced by feedback loops in which some forms of energy or action are dampened (negative feedback), whereas others are amplified (positive feedback). They are particularly sensitive to initial conditions that produce a long-term momentum or "path dependence." They exhibit emergence, or behavior that evolves from the interaction between elements at a local level rather than central direction. They may also contain "strange attractors" or demonstrate enduring regularities of behavior even if behavior can change radically at any time.

Although it is often sold as a new approach to the natural and social sciences, its application in political science builds on at least three established theories or concepts (Cairney, 2012a, pp. 124–29; 2012b). First, its focus on path dependence and "sensitivity to initial conditions" is shared by historical institutionalism (although the meaning of these terms may differ in different accounts—see endnote 2). Path dependence suggests that when a commitment to a policy has been established and resources devoted to it, over time it produces "increasing returns" (when people adapt to, and build on, the initial decision) (Pierson, 2000; Room, 2011, pp. 7–8, 16–18) and, in many cases, these "returns" are associated with the establishment and maintenance of institutions as "the formal rules, compliance procedures, and standard operating procedures that structure conflict" (Hall in Thelen & Steinmo, 1992, p. 2). Second, punctuated equilibrium theory (as articulated by Baumgartner and Jones, 1993, 2009; Jones & Baumgartner, 2005; True, Jones, & Baumgartner, 2007; Workman, Jones, & Jochim, 2009) employs some of the language of complexity to explain the dynamics of group–government relationships. The "general punctuation hypothesis" demonstrates that policy processes exhibit nonlinear dynamics and punctuated equilibria. Third, complexity theory's focus on emergent behavior in the absence of central control evokes the literatures on implementation and "multi-level governance" that explore the problems that central governments face when they do not recognize the extent to which policy changes as it is implemented (Barret & Fudge, 1981; Butler & Allen, 2008; Cairney, 2009; Hjern & Porter, 1981; Klijn, 2008; Lipsky, 1980).

Synthesis and Super-Synthesis: Standing on the Shoulders of Giants?

The analytic difference between synthesis and super-synthesis is useful to help identify the main problem (and John's proposed solution) associated with this approach: Its success may rely on the mistaken assumption of a common understanding of the fundamental nature of the five main "causal processes" identified by John (2012). We can identify ontological disagreements in all approaches to political science (Poteete et al., 2010, p. 13 also identify ontological problems in mixed methods research[1]). For example, there is a lively debate within "new institutionalism" about the nature of institutions: do they represent real, fixed structures that influence or determine the actions of agents, or less stable sets of ideas or beliefs constructed, and subject to challenge, by agents (Cairney, 2012a, pp. 86–87; Hay & Wincott, 1998, p. 952; Lowndes, 2010; Schmidt, 2008, p. 313)? Furthermore, any description of institutions as ideas highlights the blurry boundaries between the five causal processes (identified by John) and reminds us of the wide range of concepts that huddle under the umbrella terms "institution" and "ideas" (Cairney, 2012a, pp. 223–24). We can identify similar discussions on the role and explanatory power of policy networks/subsystems (Dowding, 1995, 2001; Marsh & Smith, 2000, 2001), rational choice (Hindmoor, 2006; Cairney, 2012a, p. 147), and the size of the "implementation gap" (Hill & Hupe, 2009) or "governance problem" (Cairney, 2009) that debate not only the empirical evidence but also what counts as evidence and how it should be gathered. The fact that such debates are often unconstructive and rarely resolved reinforces the problem. They show us that it is difficult to treat super-synthetic accounts as sources of accumulated knowledge, using the metaphor "standing on the shoulders of giants," because the nature of that knowledge is fiercely contested (see, for example, the debate between Marsh, 2008, and Bevir & Rhodes, 2003, 2006; Rhodes, 2011).

The more practical problem is that people may attach different meanings to key terms even in the absence of ontological or methodological debates. This is perhaps most common when the social sciences adopt terms from the natural sciences, often for the sake of a good metaphor.[2] For example, the term "evolution" is often used loosely in political science and more attention should be paid to its key variants and debates (Cairney, 2012a, pp. 268–69, 2013; Dowding, 2000; Room, 2012; Sementelli, 2007). Similarly, "punctuated equilibrium" appears to have emerged separately in new institutional and policy studies to describe long periods of continuity or stability punctuated by rapid change or instability (Cairney, 2012a, p. 177). We can also identify terms such as "policy entrepreneur" that appear in a wide range of approaches—including evolutionary and complexity theory—without reference to a common understanding (see Cairney, 2012a, pp. 271–72, and compare Dolowitz & Marsh, 2000, p. 10; John, 1999, p. 45; Kingdon, 1984, pp. 21, 104; McLean, 1987, p. 29; Mintrom & Vergari, 1996, p. 431; Room, 2011). "Entrepreneur" may be used to explain policy innovations linked to exceptional individuals, but we can have little confidence that different studies are talking about the same thing and building their research on common foundations (see Christopoulos & Ingold, 2011; Mintrom & Norman, 2009). Such terminological problems may be magnified if we seek to identify similar processes in the natural and social worlds (where, for example, the idea of agency may be profoundly different).

John (1998, p. 187; 1999: pp. 48–49; 2012) recognizes these problems but suggests that they are not fatal, largely because all theories have to trade comprehensiveness (which produces these problems of compatibility) for simplicity or parsimony (which produces only partial explanation). His solution is to compare the explanatory merits of his evolutionary theory with those of other theories by testing their respective hypotheses (this approach may also be adopted by some proponents of complexity theory). This is an idea to which we return below. What we should note now is that this recommendation moves us away from the idea of "standing on the shoulders of giants." In this context, "synthesis" means providing new theories that compete with the old. It can be usefully distinguished from "super-synthesis" in which the aim is to combine the insights of multiple theories to produce some sort of hybrid. Arguably, "super-synthesis" is more vulnerable to the problem of common meanings, but a bigger problem is that "synthesis" may no longer be viewed as an attempt to combine the insights of multiple theories.

The Complementary Approach

In many instances, we may be less concerned with testing the hypotheses of new theories and be more focused on using existing theories to explain cases. For example, Allison's (1969, p. 689) aim is to improve our understanding of empirical events by promoting "more self consciousness about what observers bring to the analysis." Analysts draw on explicit or implicit "conceptual lenses," and the promotion of multiple lenses should allow us to become more aware of the assumptions that underpin each lens and to compare perspectives. Allison (1971) combines a discussion of three approaches to rationality, using: the "neorealist" assumption of state rationality in which national governments are treated as "centrally coordinated, purposive individuals"; comprehensive and bounded rationality to examine the "standard operating procedures" of the organizations that make up governments; and game theory to discern an overall pattern of behavior from the rational choices of key individuals (1971, pp. 3–7, 257). Each approach provides different perspectives to explain the Cuban Missile Crisis. The value of multiple explanations is that they produce different answers to the same question and prompt us to seek evidence that we would not otherwise uncover (1971, p. 249; see also Ball, 1987, pp. 104–8, who argues that Allison did not fully appreciate the importance of his own approach). In Allison's case, we move from the type of explanation of state behavior—in terms of the costs and benefits of action—that could be done by an "armchair strategist" toward the pursuit of more detailed, relevant evidence (p. 251). Treating states as unified actors may produce parsimonious explanation but only at the expense of more nuanced explanations based on organizational procedures, the decision-making environment, and the need for policymakers to bargain within government (pp. 253–54).

This approach to empirical studies has been used more recently by Dunleavy (1990) to explain the "Westland Affair" (involving a famous split in the UK Cabinet during Prime Minister Margaret Thatcher's reign) using theories of the state (including pluralist and Marxist accounts) by Parker, Stern, Paglia, and Brown (2009) to focus on "psychological, bureau-organizational, and agenda-political" explanations for the failure of U.S. governments to address the threat of Hurricane Katrina and by Cairney (2007) and Cairney, Studlar, and Mamudu (2012) to explain (respectively) the adoption of a comprehensive smoking ban in England and Wales, and global tobacco control, using punctuated equilibrium theory, the ACF, multilevel governance, and multiple streams analysis. It can also be used to produce a framework to compare conceptual insights that use a common reference point—such as "bounded rationality" (Simon, 1976) as the basis for explanations of stability and policy continuity (using established policymaking concepts such as incrementalism, policy succession, and inheritance before choice) or instability and policy change (punctuated equilibrium theory, multiple streams analysis, and policy diffusion) (Cairney, 2012a, pp. 104–5, 282–83).

With this approach, the issue of ontological and epistemological compatibility may appear less pressing because we are encouraging different understandings of the world, whereas some difference in the meanings attached to key terms is manageable when we seek to compare a range of perspectives and, in doing so, recognize their different intellectual origins. The more practical problem relates to research design, particularly when we seek to compare theoretical perspectives in a meaningful way and use them to guide detailed empirical research. Put simply, it may not be practical to construct a research design based on more than one or two theories. For example, imagine the resources required to replicate Baumgartner and Jones's Policy Agendas Project (including tracking the long-term direction of problem definition in media reports and governmental and congressional inquiries and debates—see http://www.policyagendas.org/) to adopt the ACF's methods set out by Jenkins-Smith and Sabatier (1993), which includes documentary analysis and very detailed and specific questionnaires probing the beliefs of participants, *and* to emulate Rhodes's (2011) interpretive study, which involves the observation of senior policymakers followed by a program of in-depth, semi-structured follow-up interviews. There may be huge benefits to such a multifaceted research design coordinated by one team, but few endeavours of this kind are funded.

In most cases, advocates of this approach use a more manageable, and superficial, proxy for theoretical comparison. They produce an empirical case study, often based on documentary analysis supplemented by elite interviews, then set up a summary of several theories, and use those theories to identify a series of perspectives. In this context, we can use the exercise to draw attention to the assumptions of a dominant understanding of the research problem, but we cannot expect to do justice to the empirical research agenda associated with each theory. The analogy of a "toolkit" for explanation may be apt as it gives us the image of someone who can draw on a wide range of theories but perhaps as a "jack of all trades and master of none" (although the analogy soon becomes contentious, as the flexible theorist may describe himself or herself as someone who knows which jobs require which tools—Ostrom, 2006, p. 8). If a full appreciation of the merits of each theory requires the meaningful adoption of its methods, then few people are in the position to do so. Instead, it may often be more fruitful for different teams to fully explore different theories or frameworks using multiple methods (although this task raises similar practical and conceptual problems—see Poteete et al., 2010, pp. xxii–xxiii, 14–15 and below).

Comparison and the Contradictory Approach

It is in this context that we can best understand Sabatier's (2007a) aim: to encourage the proliferation of a range of fully fledged theories and their associated research agendas and to find some way to compare their insights. Sabatier (2007b, p. 330) highlights three main advantages to the use of multiple theories: It provides "some guarantee against assuming that a particular theory is the valid one," it shows us that "different theories may have comparative advantages in different settings," and the knowledge of other theories "should make one much more sensitive to some of the implicit assumptions in one's favoured theory." Note the emphasis, in the first two statements, on identifying the most useful theory following comparison.

This approach involves a two-stage thought process. First, we recognize that different theories employ different assumptions. This difference may range from varying conceptions about the size and membership of policy networks (subsystems) to the big ontological debates about the existence and influence of social structures. In some cases, we may find elements of similarity and difference that seem to make some comparisons relatively useful. For example, (largely European) studies of multilevel governance, punctuated equilibrium theory, and the ACF try to conceptualize a shift in the size and nature of policy networks but, for example, the ACF has a distinctive focus on the role of beliefs, not material interests, as the driving force for individual behavior (Cairney, 2012a, pp. 280–82). In other cases, the divisions are more profound, and theories may be described as "incommensurable" (see Hindess, 1988, pp. 73–75; Kuhn, 1962). The same terms might be used in each theory, but they may mean something else according to the paradigm from which they were derived (Kuhn, 1996, p. 103). Researchers may look at the same object but view and interpret it differently. They may "not share a common set of perceptions which would allow scientists to choose between one paradigm and the other . . . there will be disputes between them that cannot all be settled by an appeal to the facts" (Hindess, 1988, p. 74).

Second, however, relatively few studies take an extreme "relativistic" approach to these matters, in which we state that theories may be of equal value because we cannot demonstrate that some statements are more accurate than others (Hindess, 1988, p. 75; compare with Fischer, 2003, pp. 136–37). Instead, they recognize the value of some and reject the value of others. In most cases, this choice may be implicit but can be inferred from our chosen research agenda or, for example, the theories and concepts we include in reviews of the literature (note the similar choices of policy theories in Cairney, 2012a; John, 2012; and Sabatier, 2007a). In others, journal and book editors may seek a way to adjudicate between theories by holding them up to a common scientific standard (described in this article as the "contradictory approach"). For example, Eller and Krutz's (2009, p. 1) "policy shootout" and Sabatier's (2007a, p. 5; 2007b, pp. 326–31) edited volume argue that we can use widely accepted scientific principles to decide which theories are most worthy of our attention:

1. A theory's methods should be explained so that they can be replicated by others.
2. Its concepts should be clearly defined, logically consistent, and give rise to empirically falsifiable hypotheses.
3. Its propositions should be as general as possible.
4. It should set out clearly what the causal processes are.
5. It should be subject to empirical testing and revision.

In other words, we are comparing the respective insights of theories but perhaps only if we can first establish that they adhere to certain scientific principles. This set of principles may command widespread support in large parts of the literature, but there are at least five problems to note (see Fischer, 1998, 2003, for a set of stronger concerns). First, the extent to which theories adhere to all of these principles is unclear. For example, Sabatier (2007b, p. 327) lauds the ACF and the Institutional Analysis and Development Framework (IAD:Ostrom) because they meet these principles and have been subject to extensive testing by their primary authors and other researchers (Sabatier also rejects the "stages heuristic" and multiple streams analysis on this basis). Yet, although the ACF has been studied extensively, most ACFinspired case studies "do not explicitly test any of the hypotheses" (Weible, Sabatier, & McQueen, 2009, p. 128; Cairney, 2012a, p. 211). The IAD is more complicated as it is often seen (often unhelpfully) as a subset of rational choice theory. Furthermore, Green and Shapiro (1994, p. 179; 2005) argue that the "empirical contributions of rational choice theory" are "few" and "far between"—a charge that has become rather popular within political science (Hindmoor, 2006, 2011). In this light, proponents of the IAD may reasonably reject the idea of making rational choice's propositions as "general as possible" in favor of a more promising framework with less ambitious (or nonuniversal) propositions (Ostrom, 2006, pp. 4, 8; Poteete et al., 2010, p. 4).

Second, the principles may give a misleading impression of social scientific research or present an artificial standard. Researchers may explain their methods and, in some cases such as documentary analysis, build ways to verify the validity and reliability of codes into the research design. They may also seek to generalize their findings. Yet, in most, the results of empirical testing are accepted on trust; they are not replicated directly. For example, a theory's main method may be the (well respected) elite interview, but it would be unrealistic to expect one researcher's interview data to be replicated by another (especially when interviews are anonymized). This level of trust is not unique to qualitative analysis. Although academics are increasingly making large quantitative data sets available to others for secondary analysis, and it is possible to test competing theories, relatively few projects invite falsification along the lines proposed by Popper (1972).

Third, we should note the gap between the "formulation" and "implementation" of this scientific research policy. For example, note the difference between Eller and Krutz's (2009, pp. 1–2) image of a "shootout" based on common scientific principles and their actual project which involved "little guidance" for contributors and produced "some push-back against the theoretical premise of the workshop." Sabatier's (2007a, 2007b) analysis may be different, but it still does not necessarily lead to the rejection of many theories or concepts. Rather, empirical testing may lead to the *revision* of theories along "Lakatosian" lines (see Lakatos, 1970; Chalmers, 1999, chap. 9). In other words, we use empirical testing to revise secondary hypotheses while, at the same time, maintaining the "hard core" of the framework, which is insulated from falsification by its "protective belt" (Chalmers, 1999, p. 132). This is certainly the approach of the ACF, which resembles the make-up of the advocacy coalitions it describes (Cairney, 2012a, p. 219; Jenkins-Smith, in correspondence). Indeed, the analogy has extra meaning, as members of advocacy coalitions share a common bond based on their fundamental *beliefs*, not knowledge, about the nature of the world—and they *interpret empirical evidence through the lens of their beliefs*.

This gap between a common appeal to objective science and actual subjective science mirrors the big debates in the philosophy of science. In particular, Lakatos's approach to science can perhaps be summed up as a way to avoid the alleged relativism of Kuhn (1962, 1970, 1996), in which rival scientific paradigms are like different worlds that are difficult to compare (and may be "incommensurable"), and the "anarchism" of Feyerabend (1975, 2010), in which he rejects the idea of a commonly accepted "universal" set of principles or methods of scientific research (see Chalmers, 1999, pp. 149–50). Yet, the outcome for Sabatier et al. is an odd combination of appeals to scientific method in some cases, supplemented by the maintenance of concepts insulated from empirical testing in others.

Fourth, the idea of subjecting "empirically falsifiable" hypotheses to testing is often misleading when the object of our research is relatively complex. It may be relatively (but not completely) straightforward to test the existence of gravity by jumping off a bridge, because this may be a relatively discrete event in which we have a common focus on what is relevant and how to measure reality. This common focus may be lost when we examine more complex concepts, such as policy change, that are more open to interpretation. Our identification and measurement of policy change depends on a wide range of factors, including: the timeframe we select; the level and type of government we study; our focus on the decision made or the eventual outcome; and the policy instruments we identify and compare (Cairney, 2012a, pp. 26–30; Cairney et al., 2012, pp. 14–17). The empirical evidence is wide open to interpretation, and we can often produce competing, and equally convincing, *empirical narratives* based on solid evidence (see below).

Fifth, there is little evidence to suggest that the most prominent contemporary theories are selected on the basis of the objective scientific principles outlined by Eller and Krutz (2009) and Sabatier (2007a). Indeed, wider discussions in the philosophy of the social sciences suggest that "there is no possibility of an extra-theoretical court of appeal which can 'validate' the claims of one position against those of another" (Hindess, 1977, p. 226; see also Fischer, 2003, p. 127). Instead, we should recognize the subjective or social dimension to scientific popularity and endurance. For example, Meier (2009) suggests, provocatively, that the popularity of theories depends on the academic abilities and standing of their proponents (compare with Fischer, 2003, p. 111 on the relationship between research findings and trust in the researchers). To this, we can add a more general point about the fashionability of some concepts, and the rise and fall of attention to them, which does not seem to relate to the rise and fall of their value or the weight of the evidence produced (much like the rise and fall of issues on the political agenda). We should also note the tenacity of particular theorists. For example, remember John's (1999, p. 48) suggestion that we gauge the value of his evolutionary theory by comparing its explanation of empirical events to those of other theories. It is worth noting that the theory has been applied primarily to one case study (the "poll tax" in the UK—and subjected to significant criticism by McConnell, 2000; Dowding, 2000; John, 2000) but has still endured for at least 14 years. John (2012) uses evolutionary theory as part of "a continual quest for better accounts of decision-making," and he may be making progress, but the "policy shootout" does not provide a way to demonstrate it.

From Universal Scientific Principles to "Postpositivism"?

Overall, these scientific principles do not provide a convincing way to choose one theory and reject others. An alternative is what *appears* to be a fundamentally different approach to science: a "postpositivist" perspective. A good example of this approach can be derived from Fischer (1998, p. 136), who suggests that we produce what we call "knowledge" from an interaction between competing "interpretations" that represent our beliefs regarding the world and our knowledge of it. We produce competing narratives (stories) of events and, in many cases, provide "proof" that represents a consensus on the evidence or "accepted belief" (p. 136). Consequently, we may better see science as the production of "interpretations" of life or "beliefs" rather than irrefutable "facts" or "knowledge" (pp. 131–33). As such, we should be cautious about accepting some interpretations and rejecting others, because any such choice will be influenced heavily by assumptions and beliefs that not everyone shares at all times. We may "customarily" accept particular views of the world for long periods of time, to enable us to communicate effectively with each other and to conduct research that builds on insights generated in the past and/or by other people, but we also recognize that such practices depend on our decision to interpret empirical evidence through the lens of our beliefs (p. 134). This is a subjective process, rooted to current beliefs, rather than an objective process rooted in universal and everlasting scientific methods (p. 134; or, "objectivity" means something else, referring to agreements following conversations with "people who agree with our standards of comparison," whereas "validity" gives way to "credibility"—Fischer, 2003, pp. 153–54).

A key aspect of this interpretation of science is that, although it is described by Fischer (1998, p. 129) as a clear "alternative" to the "neopositivism" or "empiricism" of the "hard sciences" (and his critics might state this argument more strongly), there are strong parallels between this approach and the way that I have characterized the approach to science taken by proponents of the ACF (in other words, not the way that it often appears to be presented by Sabatier).[3] The ACF suggests that theories represent one set of fundamental beliefs, almost immune from refutation (this would be like a religious conversion), combined with other sets of beliefs subject more to revision in light of the evidence (as interpreted through the lenses of particular belief systems).

If we accept this commonality, it prompts us to reconsider the status of the scientific principles associated with the contradictory approach. They now represent a set of principles accepted by a *particular scientific community*. As I suggest in the conclusion, this adherence to certain principles may be a reasonable position taken to deal with scientific uncertainty and the need to collaborate to produce generalizable knowledge. However, it is not the *only* reasonable position. Others may conclude that, in the absence of a convincing, universally accepted method to adjudicate between scientific claims, we cannot resolve what *the* evidence is and we may therefore struggle to choose decisively between theories. Indeed, the most valuable theory in each case may depend on the interpretation of events, or the narrative of policy change, that we select, rather than just the nature of the theory. We can highlight instances in which theories provide competing assumptions or predictions but be less certain about how to "test" them or choose between them.

A valuable response to this problem is simply to recognize and to address it when we present our theories and evidence in scientific publications. For example, we might take from Fischer (1998, 2003) the idea that scientific knowledge accumulates following a process of competition in which different authors provide different interpretations of events and processes, which are more or less accepted in particular scientific communities. However, we should also consider the practice of presenting competing interpretations, or empirical narratives (or "narrative stories" or "narrative analysis"—Fischer, 2003, pp. 43, 161[4]), *within single publications*.[5] For example, Cairney (2007a, pp. 49–53; Cairney et al., 2012, pp. 104–8) presents competing narratives of policy change when seeking to explain the development of tobacco policy in the UK (as part of a study of global tobacco policy). In particular, he provides two descriptions of the legislation to ban smoking in the UK: as a symbol of profound change in tobacco control, from a voluntary to a regulatory approach; or, as an incremental progression from previous measures. The importance of these narratives is not only that they were produced using interviews with people presenting very different opinions (based on their own beliefs, biases, interests, and agendas) but also that they are reasonable interpretations to make from the available evidence (in other words, they are internally consistent and consistent with the evidence identified). Consequently, to present only one interpretation would be to deprive the reader of the opportunity to form a different interpretation of the presented case (although I recognize that providing only two narratives may seem arbitrary).

The practical benefit to this approach is that it furthers the openness agenda promoted explicitly or implicitly in discussions of scientific principles. Although we could encourage competing interpretations from competing scholars, the problem is often that we rely on single scholars to provide what may be the only source of knowledge on particular cases. Consequently, individual scholars command a monopoly of information, and the idea of competition to interpret events becomes more problematic. Therefore, if we accept that science is about presenting beliefs and interpretations of events and that our knowledge improves when we consider more than one interpretation, it makes sense to encourage scholars to present multiple empirical narratives or multiple interpretations of the information that they have gathered. Although this may be interpreted primarily as a "postpositivist-friendly" argument, its recommendation applies as much to interpretive as "neopositist" research. Interpretive research is about trying to "get inside the heads" of subjects and determine what they mean (and compare it with what they say) (Fischer, 2003, p. 141), but it follows logically from Fischer's arguments that in-depth interpretive researchers are not in a unique position to provide a dominant interpretation.

Conclusion: Where Do We Go from Here?

The aim of this article is to show that, although we can all support the idea of "standing on the shoulders of giants" and combining theoretical insights to accumulate knowledge of policymaking, we may not agree on how to do it. It identifies problems with each of the three main approaches. The idea of "synthesis" and "super-synthesis" is problematic because it is difficult to combine the insights of theories and concepts that draw on different intellectual traditions and give different meanings to the same terms. It may also be partly misleading, as synthetic theories may in fact be new rivals to old theories. The idea of a "complementary" approach is intuitively appealing and often valuable but also limited by resources. Most approaches have to make significant compromises between theoretical coverage and empirical depth, producing multiple "lenses" but singular research designs. The idea of a "contradictory" approach addresses this need to devote considerable resources to the research methods associated with each theory but does not provide a universally accepted way to combine their merits by showing that some research programs are more valuable than others in particular circumstances.

At the same time, it would be too hasty to reject these approaches simply because they are problematic (*all* approaches to science are problematic). Rather, the article serves to remind us of the subjective and cultural elements that underpin our scientific project. We seek a way to manage the need to conduct specialist research in some areas and rely on others to provide knowledge of other areas, by seeking the best way to communicate those findings and learn from each other's experiences and perspectives. We are also subject to factors that promote further academic specialization, such as: increasingly sophisticated research that requires specialization in a small number of fields and, therefore, a reliance on others to conduct research in other fields; and many "career incentives" associated with promotions procedures and the evaluation of academic work (Poteete et al., 2010, pp. 15–17, 20–21). We need some way to decide if the information provided by others is worthy of our attention (and, in a specialized and interdisciplinary world, a means to ensure that we *understand* the information provided by others). In that context, most of the "policy shootout" recommendations (explain your methods, define your concepts clearly, and set out clearly the causal processes) should be accepted by most. Such rules developed in part from the fear that some scientists, faced with evidence that challenged their theory, would seek to protect that theory by stretching its coverage and making it impervious to damaging criticism. Furthermore, we may fear that a valuable exchange of insights on the nature of the world, and how we should interact with it, often gives way to fruitless debates (Ostrom, 2006, p. 4; Poteete et al., 2010, pp. 266–68). Or, we may fear, simply, that the information provided by some is not as useful as that provided by others—and we have to act accordingly because we only have the ability to digest so much information and must make choices about what to ignore or pay attention to. Most of us share a concern with such problems even if we do not agree on their solutions.

In this context, the article concludes with three broad conclusions and recommendations. First, the alternative to an overly prescriptive approach to science is a form of methodological pluralism in which we accept that different studies take different approaches and we seek to compare them in a less regimented way than the contradictory approach suggests (unfortunately, this is necessarily a vaguer proposition than expressed in the contradictory approach). Although there are practical limits to the variety of acceptable methods, based on professional views expressed through the peer review process of journal publication and grant distribution, there is enough tolerance of variety and enough defences of pluralism (e.g., Della Porta & Keating, 2008; Keating, 2009) to ensure that we do not have to select "one best way." Of course, a "pluralistic" approach may also be criticized in several ways: It is difficult to know what pluralism means in this context, as it can refer as much to the competition between theories as a liberal approach to them; it is difficult to know how different this agenda is to the practices that already exist; it may set few standards for the quality of research; and it perhaps acts as a cover for an assault on the type of "positivist" political science that dominates the most cited journals. However, as Della Porta and Keating argue (in correspondence), we can occupy the vast ground between the imposition of "one best way" and Feyerabend's (1975, 2010, p. 7) provocative promotion of "anything goes"[6] by encouraging the *negotiation* of theoretical and methodological standards in academic exchanges, rather than appeals to a mythical universal standard.

This is certainly the tone of Poteete et al.'s (2010, pp. 4–6) work on the IAD, which encourages methodological sophistication based on interdisciplinary collaboration rather than the imposition of a too-rigid framework. Poteete et al. (2010, p. 4) seek commonly agreed standards to address Ostrom's (2006, p. 5) call to "overcome our own heterogeneities so as to achieve a better understanding of the world around us" and to address their concern that methodological wars are often caused inadvertently by some "poor methodological practice" when methods are used inappropriately or applied poorly. Ostrom's (2006, p. 5) impressive solution is to construct well-managed networks addressing a common research problem (a solution that may not be available to most scholars in a heterogeneous and competitive field), but the fact that Ostrom coauthored a book detailing the challenges of scientific collaboration (Poteete et al., 2010) gives us some indication of the problems involved even under the most promising circumstances. For example, there may be a basic trade-off between the size of the collaboration and the control of the research project by a small team. This *might* be partly explained, in the case of IAD, by the authors' approach; their belief that "openness" and a "diversity of approaches" encourages methodological and theoretical innovation and is therefore more fruitful than "an extreme insistence on conceptual and methodological consistency" (Poteete et al., 2010, pp. 132, 249)—but more evidence is required. A useful future comparison may be the Policy Agendas Project, which involves, on the one hand, a detailed common coding framework used by international collaborators (often with the help of Baumgartner and Jones's team) and, on the other, a much larger literature that considers issues of punctuated equilibrium theory relatively independently (see, for example, the special issue of *Policy Studies Journal* 40, February 2012).

Second, although we need some common rules of engagement for these exchanges, those rules may vary according to the venue and the participants. For example, there is a particular need to hold regular discussions to allow colleagues to exchange ideas and learn from each other in interdisciplinary settings, where we may require extra time to understand each others' intellectual traditions, quirks, and assumptions (see endnote 2). In my experience, these conversations are relatively straightforward and less heated, perhaps because there appears to be so much more to learn from each other and a greater willingness to exchange ideas. For example, preliminary discussions of complexity theory have produced plans to examine policymaking by combining periods of observation and elite interviews with experimental psychological experiments (exploring "tipping points" regarding rules of consultation and decision making) and mathematical modeling based on the experience of physicists applying theories to systems biology (Cairney, 2010; compare with Poteete et al., 2010, pp. 257–58 on the "Workshop in Political Theory and Policy Analysis"). These exchanges may have been less fruitful if we first had to establish a universal standard (outlined above) for the production and evaluation of each theory (although discussions of method would subsequently take place as part of a commonly agreed research design).

The more problematic exchanges often arise from academics operating, at least initially, in similar traditions and often with very similar approaches. In many cases, there is a tendency for colleagues to engage in parallel tracks, giving papers but not listening to papers, or presenting critiques of other approaches on their own terms rather than in a manner likely to foster the cooperation of their victims (by, for example, considering the internal consistency of competing theories—Hindess, 1977, p. 226; Poteete et al., 2010, p. 8 refer to scholars "talking past each other"). If we return to the analogy of politics, we find that academics resemble political parties that exaggerate their differences to mark out their identities and gain support. Some may feel that rigorous scrutiny and heated debate, based on the rules of the contradictory approach outlined above, may advance our understanding of science—particularly in political science, which has a rich history of debate-led knowledge exchange. Others will leave conferences with little more than a sense of having been entertained but not enlightened. This issue seems more serious when high-ranking peer-reviewed journals encourage fruitless debates in which the authors talk over each other and give each other straw men titles (including the term "positivist," which is often used to discredit the work of some scholars without considering the substance of their research or arguments) instead of trying to engage on their terms.

Third, an appeal to be open about our research methods and hypotheses should be accompanied by similar levels of openness about our empirical findings. This agenda may be most straightforward in the quantitative field where large data sets can be used by a range of academics testing a range of competing hypotheses. It may be less straightforward in the qualitative field if the research is based on the observation and interview of political elites; if the information cannot be shared; or if the reader is much more reliant on trusting the original researcher. In such cases, a potentially useful practice is to present competing narratives of policy change, or at least to be clear on the basis for the selection of a dominant narrative of change. This process may be crucial to determine the value of particular research agendas, as competing narratives of policy change may be used to support competing theories.

However, the likelihood of such exercises may be undermined by appeals to a dominant or universal scientific standard that produces the tendency for, for example, many interpretive political scientists to be defensive about their work and less likely to engage with scholars from other traditions. It is only by rejecting this idea of a universal standard and encouraging fewer rule-bound exchanges (in which some scholars evaluate the contribution of others according only to their own personal scientific rules and beliefs) that we can ensure a more open and productive scientific environment in which we are willing to expose our uncertainties without fear of immediate rejection. This is not an argument for relaxing academic standards. Rather, it is an argument for recognizing that the enforcement of a rigid universal scientific standard may be damaging to the scientific enterprise that we share. One does not need to label one's self as a "postpositivist" or "postempiricist" to come to this conclusion or to find it reasonable.

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Notes

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1. However, Poteete et al. (2010) often use the term "synthetic" differently, in at least two ways. First, they describe meta-analysis as "synthetic" because it involves coding multiple case studies to combine their insights (pp. 89–90). In this context, they identify practical problems when the diversity of case studies (produced by many different scholars) limits our ability to compare empirical findings using detailed and uniform coding. In some cases, they recommend "narrative synthesis" to bring together those findings in a verbal rather than numerical way (p. 111). Second, they describe as "synthetic" the bringing together of multiple methods (p. 215) and the production of a theory built on the insights of empirical work produced by multiple methods (pp. 220–45).
2. These issues also arose in interdisciplinary discussions I had with colleagues in physics. For example, although "chaos" may be used in the social sciences to describe a sense of randomness and unpredictability, its use in the physical sciences denotes a deterministic process and sensitivity to initial conditions. Similarly, a reference to first-, second-, and third-order change can have contrasting meanings—in political science, third-order change may be profound (Hall, 1993) but, in studies of bifurcation, first-order change is most important. The term "positive feedback" may be used very differently in historical institutionalism and complexity theory (a valuable point made by one anonymous reviewer). These problems can be solved but only if we know they exist. Ostrom (2006, p. 5) discusses the potential for similar obstacles even when an academic network focuses on a more commonly accepted theory but combines methods. Also, note the potential for terminological confusion in different countries. For example, the term "policy community" is often used very differently in the United States and UK (and even *within* the UK) (Cairney, 2012a, p. 179).
3. Furthermore, Fischer's (1998, p. 139) discussion of the limits to multiple interpretation displays similar parallels to mainstream scientific accounts and is a useful corrective to the lazy argument that "postpositivists" deny the existence of reality.
4. I am cautious about using the term "narrative" for two reasons. First, it is often too easily rejected as a form of storytelling—a practice that is difficult for some to reconcile with the presentation of science. Second, some scholars use "narrative" in particular ways. See, for example Bates, Greif, Levi, Rosenthal, and Weingast (1998) on "analytical narratives" as a way to explore rational choice theory. Poteete et al. (2010, p. 111) discuss "narrative synthesis" partly as a way to conduct meta-analysis when there is no clear way to systematically code the findings from multiple case studies (although they are careful to note that qualitative analysis is not a second-best option).
5. This is an approach entertained occasionally, for example, by Sabatier and Jenkins-Smith (1993, p. 225; see also Fischer's, 2003, p. 100 commentary) although the article recommends going much further.
6. The slogan "anything goes" largely represents a rejection of "firm, unchanging and absolutely binding principles for conducting the business of science" (Feyerabend, 1975, 2010, p. 7) rather than an admission of a fondness for tarot cards or astrology. Compare with Ostrom (2006, p. 4), who equates "anything goes" to a sloppy approach to methods but makes similar statements on overly prescriptive science—suggesting that Feyerabend and Ostrom might disagree on the meaning of "anything goes" but not about the rights and wrongs of science.

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