**Positivism: Paradigm or culture?**

Phil Ryan

School of Public Policy and Administration

Carleton University

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**Abstract**

Much post-positivist policy theory implies that positivism exists as a self-protecting paradigm. Inspired by a one-sided reading of Kuhn, this understanding suggests that policy positivism must be overcome as a whole. This approach is problematic. In particular, there are important contradictions between various elements commonly said to be part of the positivist paradigm, contradictions that make it difficult to believe that the paradigm can be embraced as a whole. An alternative approach views positivism as a culture. Since components of any culture can evolve independently of each other, a cultural approach would focus its critique on specific dimensions of positivism. This approach would provide more rigor to policy critique, and push post-positivists to overcome weaknesses in their own theories, in particular those concerning the question of truth.

**Keywords**

Positivism, post-positivism, Hawkesworth, Fischer, Kuhn, paradigms

**Positivism: Paradigm or culture?**

In 1993, John Dryzek and Douglas Torgerson triumphantly declared that post-positivist policy analysis “now occupies the intellectual high ground in the policy field, (even if it does not pervade the sensibility of most practitioners in the public policy trenches)” (1993, 132). Morçöl’s survey of “Positivist beliefs among policy professionals” a decade later confirmed that the trenches had yet to be won over (2001). This paper will argue that one reason post-positivism has not transformed practice in the “public policy trenches” is that it does not, in fact, occupy the intellectual high ground. The post-positivist critique has been weakened by a problematic understanding of positivism’s mode of existence, an understanding embedded in the very *form* of typical post-positivist critiques.

Specifically, this paper will challenge the assumption that positivism exists as a self-protecting paradigm, one that must be overcome as a whole. If we abandon this view, and consider positivism as a *culture*, many of post-positivism’s specific critiques remain valid, yet policy critique will be profoundly altered. I will argue that the alternative approach would encourage more rigor in policy critique, and push post-positivists to overcome weaknesses in their own theories.

One might object that there is little point in returning to this issue: the key defenses of post-positivism are dated, and no-one could claim that today’s policy theorists are focused on the debate. The objection assumes that what is vital for a discipline is whatever is being discussed *today*. Many of today’s policy theorists and analysts, however, were trained when the post-positivist critique was at its height. It is safe to assume that elements of the post-positivist critique “took” with many young scholars, shaping their views on matters such as the legitimacy of quantitative research, or the nature of truth, and that to some extent they reproduce these ideas in their current work. As with an archeological dig, what is vital in a discipline comes in layers. In both cases, the most recent layers may be the least significant.

Evidence? For many, the set of possible approaches to policy analysis remains carved up into ill-defined sub-sets related to the positivist controversy. This is revealed in the casual way writers identify approaches as being “positivist” or not, without specifying just what they mean by that.[[1]](#endnote-2) The impact of the controversy upon today’s students can be seen in textbooks – within the policy field or the social sciences in general – that identify quantitative methods *as such* with positivism.[[2]](#endnote-3)

Further, one can still observe today both the narrowness that post-positivism sought to overcome, and the intellectual muddle that it created in its turn. I would wager that any policy professor has come across certain revealing phenomena among colleagues and students. One finds, on the one hand, those who believe that work that is not quantitative is not quite serious. Many would agree with a colleague of mine who described qualitative research as “show-and-tell time.” On the other hand, one can observe among many academics and students a hesitancy concerning truth claims, a tendency to put the word truth itself in quotation marks, as if it were suspect or archaic. This is often accompanied by a sense that there is something *inherently* conservative and technocratic about quantitative research.

Finally, a critical examination of post-positivism has a broader application, by taking aim at what I will call the “paradigm paradigm,” which distorts methodological debates throughout the social sciences, and can impoverish research.

But before we contrast the “paradigm” and “culture” understandings of positivism, can we say something precise about *positivism itself*? To respond that the term’s meaning is contested does not do justice to the challenge: the term is so protean that one cannot even tease out a “family resemblance” among uses. In some fields, the term does have a precise meaning, but those meanings don’t agree between fields. In philosophy, “logical” positivism was centered on the sweeping claim that statements whose truth-status cannot be assessed through reference to sense data are “meaningless” (Ayer 1946, 9).[[3]](#endnote-4)

*Legal* positivism holds that the validity of a law depends upon its generation through “legally stipulated procedures” (Habermas 1996, 202), rather than depending upon some procedure-independent criterion, such as its conformity with “natural law” (Weber 1978, 874). In theology, positivism can denote, *inter alia*, the “assertion of divine revelation as the ‘positively’ given reality against all reason, in opposition to the possibility of natural knowledge of God” (Pangritz 2000, 75).

When we turn to the social sciences, any hope of identifying a precise and generally accepted definition of “positivism” must be abandoned. This, indeed, is to be expected if positivism exists there as a culture, rather than a clearly specified paradigm. One cannot, after all, provide a precise definition of American culture, or chess culture. Still, one can sometimes risk generalizations. In the present case, one might describe social-science positivism as the view that the natural sciences should provide the model for proper research. Just what this means in practice would depend on different understandings of natural-science practices, understandings that might or might not correspond to actual practices.

Even this very general depiction requires caveats. One difficulty is that it ignores the controversy between “positivists” and “realists,” both of whom may seek to follow “strict” scientific procedures (Keat and Urry 1975). Further, social scientists have often looked to the natural sciences, not for a truly *practice-guiding* model, but in order to fashion “cardboard imitations of the tools and products of the hard sciences in the hope that our incantations would make them real” (Almond and Genco 1977, 504).

Finally, positivism, particularly in the social sciences, has been characterized in an entirely different manner: “That we disavow reflection *is* positivism” (Habermas 1972, vii). This claim, which seems to be merely an ill-tempered outburst, may point to a key dimension of the problem of positivism in the social sciences in general, and the policy sciences in particular. We return to this below.

**1** Positivism as paradigm

Frank Fischer and John Forester open their *Argumentative Turn in Policy Analysis and Planning* anthology by asking “What if our language does not simply mirror or picture the world but instead profoundly shapes our view of it in the first place?” (1993, 1). The question reflects a shift of attention in philosophy from the simple meaning of words to their “force” and “effects” (Austin 1975, 121), from what language *says* and *claims* to what it *does*, from abstract language to discourse. Thus, while an “orthodox” policy analyst might subject a quantitative policy analysis to purely methodological scrutiny, those who have taken the “argumentative turn” might be more prone to critique “the mystifying technical languages that serve—often intentionally—to intimidate those who attempt to deliberate with the experts” (Fischer 1993, 36).

Let us turn this lens upon post-positivist discourse itself: leaving aside its specific claims, what are its likely *effects*? A crucial effect is embedded in the call to embrace *post*-positivism: the very language suggests that positivism is bad as a whole, and must be transcended as a whole. So post-positivist appeals will often open with a list of the various problematic dimensions of positivism, followed by a call to become post-positivist in reaction to all of those dimensions. Mary Hawkesworth’s work (1988; 1992) provides an excellent example, which we examine below.

This discursive style is fully consistent with a certain spirit of our time. We can term this spirit the “paradigm paradigm,” and it is the product of an enthusiastic and uncritical assimilation of certain ideas gleaned from Thomas Kuhn’s *The Structure of Scientific Revolutions*. In this process, Kuhn’s ideas are subject to selective uptake. The selected claims are his most dramatic, those that emphasize the all-determining nature of paradigms, and the consequent violence of paradigm clashes: the choice of paradigms is “a choice between incompatible modes of community life ” (1970b, 94); “after a revolution scientists are responding to a different world” (111); scientists holding different paradigms “inevitably talk through each other” (109); and, most importantly: “the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once” (150).

But what is it about paradigms that might make them like this? Why could a set of theories not evolve gradually, as weak elements of the set are corrected through their clash with observed facts? Because facts, Kuhn insisted, are observed by scientists already possessed of theories, hence a paradigm can dispose the mind to miss whatever might challenge it. In what Kuhn called normal science, phenomena “that will not fit the box are often not seen at all” (1970b, 24). As an example, he notes that Western astronomers “first saw change in the previously immutable heavens during the half-century after Copernicus’ new paradigm was first proposed,” while Chinese astronomers “had recorded the appearance of many new stars in the heavens at a much earlier date” (116). In this understanding a paradigm is thus akin to a *self-protecting organism*.

This image of an enveloping paradigm that neutralizes any data that might challenge it has been embraced by various post-positivist policy theorists. Mary Hawkesworth thus claims that “if what is taken to be the ‘world,’ what is understood as ‘brute data’ is itself theoretically constituted (indeed, constituted by the same theory which is undergoing the test), then no conclusive disproof of a theory is likely” (1988, 50). Quite similar claims are advanced by Frank Fischer (1998, 133) and Marie Danziger (1995, 436).

The embrace of Kuhn’s ideas has been selective, however. Kuhn offered, after all, not an account of paradigm prisons, but of scientific *change*. That change, for Kuhn, flowed from the very nature of scientific paradigms. Precisely by restricting the scientist’s field of vision, a paradigm gives the scientist very precise expectations regarding the results of ongoing observation and experiment. These expectations in turn allow anomalies to emerge. Hence, far from constituting a static and hermetically sealed framework in which theory is immunized from disproof, what Kuhn calls normal science “prepares the way for its own change” (1970b, 65). So while paradigms appear self-protecting in the short run, they are eventually self-undermining.

Now post-positivists might answer that while the foregoing is of interest for the history of ideas, it does not weaken their critique of policy positivism: even if a widespread reading of Kuhn’s account of *natural* science is incorrect, this reading works very well in the social sciences in general, and the policy world in particular. They could argue that, while Kuhn’s “normal science” may well function as an anomaly factory that slowly undermines the dominant paradigm, social science paradigms do not display this dynamism. The allegedly “one-sided” reading of Kuhn, then, would be a faithful reflection of the one-sided nature of social science paradigms. If so, the implication for the policy world is clear. If positivism is a paradigm, understood as a self-protecting organism, then it must be killed off *as a whole*, and replaced by “post-positivism,” a very different organism.

But is the paradigm paradigm adequate, even outside the natural sciences? It certainly produces its share of anomalies. Foremost among these is that many of those who wield the imagery of the paradigm prison appear not entirely to believe what they are saying. Paradigms, it seems, are only binding for those who have not benefitted from post-positivist insights. Kelly and Maynard-Moody, for example, firmly declare that “We cannot escape our historically and culturally determined theoretical positions” (1993, 136). Yet faced with concrete issues one can “bring together various stakeholders” and seek “intersubjective agreement through the outsider’s facilitation” (137-38). This is done by “getting participants to look beyond their own valid yet parochial views and interests” (140). So while we may not be able to “escape” our frameworks, with the help of the analyst-as-facilitator we can “look beyond” them. But when the prison is a mental one, does not peering over the wall itself constitute an escape?

Marie Danziger’s “postmodernized” policy analysis presents the same difficulty. After declaring that “a scientist’s working paradigms determine the shape of the facts he can never observe neutrally” (1995, 436), she claims that once students “can define the conflicting paradigms available to them, they are more likely to recognize the crucial weaknesses of any particular perspective, model, or system” (446). Here it seems that the well-trained student has a choice of prisons. Rein and Schön blame “frames,” which they explicitly equate with paradigms, for the fact that “policy controversies cannot be settled by recourse to facts alone, or indeed by recourse to *evidence of any kind*” (1993, 148; emphasis added). Yet they proceed to discuss the strategy of “‘hitching on’ to a dominant frame and its conventional metaphors, hoping to purchase legitimacy for a course of action actually inspired by different intentions” (151). The “frame” has morphed from an enveloping container in which “facts, values, theories, and interests are integrated” (145) to a mere vehicle to which one can “hitch on” at will. This binding-yet-not-really-binding quality of paradigms is also manifest in the critics’ very desire to provide a (presumably accurate) depiction of the positivist paradigm. If paradigms are really so all-enveloping, how exactly is the outsider able to describe them (Davidson 1985, 130)?

Further puzzles arise once we consider the depiction in greater detail. For positivism as portrayed by the post-positivist critics is an odd beast. Mary Hawkesworth (1992) provides one of the most detailed portraits of positivism, to which we add labels to facilitate discussion:

In shaping their disciplinary practices, policy analysts have drawn heavily upon positivist and Popperian arguments about the nature of knowledge and the logic of scientific investigation. The [1] verification criterion of meaning, [2] the fact-value dichotomy, [3] the covering law model of explanation, [4] the hypothetico-deductive method of scientific analysis and [5] the correspondence theory of truth have figured prominently in the justification of the methods of policy inquiry. (Hawkesworth 1992, 296)

Hawkesworth goes on to add other elements to the list: [6] a commitment to “value-free” analysis, based on “neutral observation” (1992, 297-98); [7] “the belief that the logic of scientific inquiry was the same in all fields” (298); [8] “the assumption that ‘facts’ are unproblematic, that they are immediately observable” (299); [9] the “repudiation of values as arbitrary preferences, irrational commitments, or meaningless propositions” (299); [10] “the conflation of explanation and prediction” (320); and [11] an “instrumental conception of rationality” (321).

Other critics mention further elements that are broadly consistent with Hawkesworth’s depiction: [12] a “technocratic orientation in policy analysis” that seeks “the abolition of politics” (Torgerson 1986, 43, 34); hence also [13] a “detached observer, expert role” for the analyst (Morçöl 2002, 112); [14] “quantificationism” (Morçöl 2001, 383), with a consequent focus on “quantifiable values or variables” (DeLeon 1994, 85); [15] objectivism, understood as a belief “that the world exists independently of what human beings may think of it” (Leonard 1990, 31); and [16] a “basic assumption of individual and institutional rationality” (DeLeon 1994, 80).

For ease of reference, I will call this collection of elements the “positivist list.” One might quibble with the inventory, fusing some items together, or adding others. But let us focus on another matter: is the phenomenon depicted by this list likely to constitute an enveloping paradigm, akin to a self-protecting organism? One barrier to this view might be termed the “Popper problem.” Karl Popper presents the post-positivist critics with a classification problem. For Frank Fischer, Popper is a positivist, full stop (1998, 130). Mary Hawkesworth views Popper’s work as a “qualified modification” of positivism (1988, 41), a merely “internal critique” of the paradigm (1992, 300). Yet Göktug Morçöl considers Popper a “pioneer of postpositivist thought” (2002, 69).

Why the confusion? The problem is that Popper endorsed some elements of the “positivist list” (e.g. 2, 4, 5) but vigorously rejected others: his influential early book, *The Logic of Scientific Discovery*, contained a powerful attack on [1], the verification criterion of meaning. That attack also undermined [8], the belief that facts are “immediately observable”: observation, Popper insisted long before Kuhn, “is always observation in the light of theories” (2002, 37). His social and political writings, such as *The Open Society and its Enemies*, show no affinity with [6], [7], [9], [11], [12], or [14].

Now any classification can face “hard cases”: perhaps Popper is merely one such case. But in fact the problem runs deeper: if Popper could be considered a positivist or sort-of positivist by the critics, yet build a coherent body of work that embraces only a few elements from the positivist list, it must be that there is *no necessary unity* to that list. We can go further: the Popper problem reveals a serious incoherence within the list itself. There is an obvious conflict between [4] and [1]: the “hypothetico-deductive method” is developed precisely *because* the verification criterion of meaning has fallen (Medawar 1974).

Nor is this the only contradiction in the positivist list: it does not seem possible to assert both [7] and [8]. To hold that facts “are immediately observable” [8], one has to deny a central understanding of modern science, hence undermining [7], the view that one’s procedure should be modeled on that of the natural sciences. As Hannah Arendt points out, with Galileo comes scientific awareness of the need to “hunt for truth behind deceptive appearances” (1958, 290).

Now if the positivist list is incoherent, it should be rather difficult to find any full-blooded positivists out there, though one can certainly point to specific writers who embrace some sub-set of the list. Thus, an easy way to refute the foregoing argument would be to identify *influential* figures in the world of policy who really do subscribe to the list as a whole, and in particular who embrace its apparently contradictory elements. It would *support* the foregoing argument, on the other hand, if one were to find that policy actors take a “cafeteria” approach to the positivist list. And this is precisely what Göktug Morçöl’s study of “Positivist beliefs among policy professionals” reveals.[[4]](#endnote-5) Thus, for example, roughly half of those who endorsed the “post-positivist” view of the policy analyst as ‘facilitator’ agreed with the “positivist” postulate of value neutrality (2001, 391).

**2** Positivism as culture

Given the difficulties that arise from viewing positivism as an enveloping paradigm, akin to a self-protecting organism, let us try a different metaphor: positivism as a *culture*. To view positivism thus is simply to recognize that people trained in particular places and times will tend to share and reproduce certain beliefs about analytical practice, much as people raised in a certain region can share an accent. The unity of this positivist culture could be quite real, in the sense that there is a cost to transgressing the culture’s key norms. Yet this unity would be quite different from the previous form. The elements of a culture need not have any strong dependency relations between each other. That is, a change in one element need not entail a change in others.

Apart from cases of extremely isolated “traditional” cultures, it does not seem accurate to think of cultures as self-protecting organisms. They can change over time in specific ways, while retaining continuity in others. Thus, for example, the sexism of North American culture has arguably lessened since the 1960s, while the capitalist ethos of that culture has not. This shows that one need not respond to the perceived deficiencies of a culture with a wholesale attack upon it. Indeed, in many ways a state of wholesale attack is not terribly conducive to cultural evolution, as it is more likely to trigger a conservative closure of the culture. Analogously, if the culture metaphor is more adequate, then policy positivism might be changed piecemeal, and might not be likely to change otherwise.

This approach denies that positivism is a paradigm *as understood in the previous section*, though it need not deny that positivism is a paradigm, so long as the latter is itself understood *as a culture*. Indeed, there are many hints in Kuhn that his paradigms can be understood precisely this way. Why else, for example, would immersion into a paradigm rely so much on the teaching of “exemplars,” as opposed to hard-and-fast rules? Through this initiation, the learner develops “tacit knowledge,” knowledge not easily captured in words and rules (1970b, 191; Polanyi 1962). The “intuitions” thus developed “are the tested and shared possessions of the members of a successful group, and the novice acquires them through training as a part of his preparation for group-membership” (1970b, 191). Does this acquisition of a paradigm not sound precisely like initiation into a culture?

Other cultural aspects of Kuhn’s paradigms emerge when the concept is more closely examined. In a 1965 symposium attended by Kuhn and Popper, among others, Margaret Masterman argued that Kuhn uses “paradigm” in twenty-one distinct ways in *The Structure of Scientific Revolutions* (1970, 61). In response to Masterman’s analysis, Kuhn accepted that the treatment of paradigms in the book was “badly confused” (1970a, 234). He attempted to address some of the confusion in the late 1960s. In the 1969 Postscript to a new edition of the book, he notes the “intrinsically circular” nature of the paradigm concept: “A paradigm is what the members of a scientific community share, and, conversely, a scientific community consists of men who share a paradigm” (1970b, 176). At a symposium held the same year, he depicted such scientific communities in eminently cultural terms: “Bound together by common elements in their education and apprenticeship, they see themselves and are seen by others as the men responsible for the pursuit of a set of shared goals, including the training of their successors” (1977, 461). He proceeded to comment that “clearly, communities in this sense exist at numerous levels.” At the highest level, “perhaps all natural scientists form a community.” The multiple levels bottomed out, he thought, at “communities of perhaps a hundred members, sometimes significantly fewer.” He added that “Individual scientists, particularly the ablest, will belong to several such groups, either simultaneously or in succession” (462).

But if paradigms can be of widely varying breadth, and if one can belong to “several” scientific communities at once, one must infer that a scientist can embrace more than one paradigm at a time, much as one’s membership in a multitude of groups of varying size allows one to participate in various cultures simultaneously. It would seem to follow from this that no single paradigm would be all-enveloping: no paradigm could constitute a prison.

Now Kuhn, so far as I know, did not explicitly liken paradigms to cultures. Still, given his later qualifications, and his clear unease with how the very term “paradigm” had escaped his control (1977, 471), we would do well to try thinking of intellectual frameworks as cultures rather than enveloping paradigms, and see what that exercise yields. In the case of policy positivism, we would thus view the various elements identified with positivism as cultural elements within the policy world, some widely embraced, others less so. Because cultural elements can change independently of one another, we could thus proceed to examine the various elements of the positivist list individually. What would be the advantages of this “disaggregated” critique?

**3** Towards critical, and self-critical, rigor

Attempts to attack positivism as a whole through quick reference to its many problematic dimensions have often led to sloppy “shotgun” critiques, in which positions are caricatured, critiques are imprecise, artificial polarities are set up between outlooks, and alternative positions are not clearly specified. A more focused “disaggregated” critique would lend itself to more rigor in all those respects.

**3.1** Beyond caricature

Various claims about the positivist outlook depict it as a condition closely resembling stupidity. We have already noted Hawkesworth’s claim that the positivist believes that facts “are immediately observable.”[[5]](#endnote-6) But does anyone really believe this? In discussing science, Hawkesworth also declares that “the rationality of the process cannot guarantee the eternal verities of particular conclusions” (1992, 323). Again, who exactly is unaware of this? It is nearly a century, after all, since Max Weber noted that “Every scientific ‘fulfilment’ raises new ‘questions’; it *asks* to be ‘surpassed’ and outdated” (1958, 138). The same question may be asked of Hawkesworth’s claim that positivism “fails to recognize that an erroneous theory can generate correct predictions” (1992, 320).

Of course, it is possible that these claims are *not* caricatures, and that I have failed to grasp the depths of stupidity to be found in the policy world. That claim is easily proved: offer evidence of real-life policy analysts who believe these things. If one reads much post-positivist critique, one may be surprised by the dearth of positivists cited, particularly when it comes to documenting the various shortcomings of the paradigm. By providing documented examples of the sorts of beliefs I have labeled caricatures, the critics would also be able to clarify just *where* in the policy world such beliefs exist: are they prevalent, or relatively isolated?

**3.2** Precision of critique

Disaggregating the critique can also help sharpen it. Above all, a more precise critique of a specific component of the positivist list would distinguish whether the component is *inherently* problematic, or only when misused or applied in extreme ways. Some authors, for example, imply that a quantitative orientation *in itself* qualifies analysis as positivist. But does the use of statistics, thinking in terms of variables, and so on, entail allegiance to the rest of the positivist list? A more precise critique would home in on the problematic beliefs and practices that can accompany quantitative work. These include: a) the denigration of other forms of thought; b) a hubris that downplays or ignores the challenges of operationalization; and c) carelessness concerning the non-quantitative aspects of quantitative research.

Greater precision can also shed light on some post-positivist claims. Consider, for example, the matter of “reality.” Kelly and Maynard-Moody insist that “all ‘reality’ is socially constructed” (1993, 136), while Guba asserts that “there are as many realities as there are persons to contemplate them” (1985, 13). Does this reflect anything more than semantic ambiguity? Are the post-positivists not simply using the word reality to denote “the world as we understand it, as constructed by the sum of our beliefs and theories”? This, I think, is the only plausible interpretation of their claims. Thus, what at first glance appear to be daring and odd claims are nearly tautological. If by “reality” a writer means simply “reality as people understand it,” there is nothing surprising in the affirmation that “reality as people understand it is not independent of people.” This, presumably, is how Rein can declare that reality “consists in multiple descriptions,” while still insisting that we need not deny “the facticity of the world and its underlying reality” (1983, 94). Critical reflection might be advanced if we reject such plays on words, and aim instead for clarity of communication. That clarity would be tested by the intelligibility of our statements in the eyes of those whose positions we are seeking to critique, as opposed to those already in agreement.

Specifying critiques with greater precision should also add precision to the *alternatives* being advocated. If, for example, we reject the “dichotomy” of facts and values, just what is their relationship? Similarly, if we reject the disqualification of “unscientific” methods, what would be a valid understanding of the relation between different approaches? That is, once we have erased knowledge hierarchies, what remains? Consider Hawkesworth’s pluralistic comment that “Divergent cognitive practices rooted in conventions such as common sense, religion, science, philosophy, and the arts, construe the empirical realm differently, identifying and emphasizing various dimensions, accrediting different forms of evidence, different criteria of meaning, different standards of explanation, different tokens of truthfulness” (1992, 313). Is that all we can say? Will there be dialogue between these cognitive practices? Are they able to challenge one another? Or are they all to sit in splendid isolation? Further, what happens when we look within the realms, and find out that there really is no internal unity to any of them? Within the realm of religion, for example, one might notice a few differences between the “cognitive practices” of a Hans Küng and a Jerry Falwell. Will each of the sub-realms in turn be hermetically sealed from the rest?

But if one is *not* calling for isolation between knowledge spheres, how precisely are they to interact? How are we to adjudicate their competing claims? Since the realms are said to differ in their concepts of evidence, meaning, explanation, and truthfulness, is there any possible basis for discussion between them? Similar questions could be put to Fischer, who comments that “scientific communities are not the only bodies capable of making judgments about the same reality. From competing perspectives, alternative groups grounded in other forms of rationality can make valid judgments about the same phenomena” (1998). What might “valid” mean here? Valid only within the group itself? Should their competing notions of validity enjoy equal status in public debates, or should we “privilege,” to take an example, scientists’ warnings concerning climate change over the fundamentalist assurance that God is not “going to let the Earth burn up” (qtd. in Bates 2006)? The latter view, after all, is no-doubt “valid” within some “alternative groups.”

**4** Truth

The benefits of a “disaggregated” critique may be most notable on the question of truth, the most serious area of confusion in the post-positivist critique.

**4.1** Retreat from fallibilism

Let us note, first, some striking claims. Marie Danziger quotes with approval the sophists’ view that “the highest truth for any man is what he believes it to be” (1995, 436). In the same vein, Patsy Healey argues that “‘Right’ and ‘good’ actions are those we can come to agree on, in particular times and places” (1993, 238). We noted above that the natural sciences have come to be quite conscious of fallibilism. This consciousness vanishes in the passages just cited. If truth, rightness and goodness are whatever I believe them to be, just what would constitute *error*? And does an outlook incompatible with fallibilism not represent a *regression* relative to “mainstream” approaches, based on the natural sciences?

**4.2** Inconsistencies

But it is unlikely that anyone can *consistently* hold a “sophistic” view of truth. Truth and error being concepts that are not so easily jettisoned, it should not surprise us that post-positivists tie themselves in knots on the question. Frank Fischer, to take one example, declares that “what is identified as objective ‘truth’ by rational techniques is as often as not the product of deeper, less visible, political presuppositions” (2003, 14). His ideal policy analyst will thus be engaged in “scepticism and critique rather than truth-seeking per se” (46). Yet the same Fischer informs us of “discoveries” and “new historical and sociological *observations* about the nature of scientific practices,” and assures us that “historical and sociological analysis *makes clear*,” helps us “*recognize*” the true nature of scientific communities (1998, 132-34; emphases added).[[6]](#endnote-7) So why are “scepticism and critique” abandoned when it comes to “historical and sociological” claims?

This is far from the only inconsistency in post-positivist truth-talk. Consider the claim that policy analysis must take an “argumentative turn,” and recognize that the analyst must offer, not proofs, but persuasive arguments (Fischer and Forester 1993; Majone 1989). It should be obvious that *trust* in those advancing an argument is an important factor in persuasion. The “personal character of the speaker,” argued Aristotle, “may almost be called the most effective means of persuasion he possesses” (*Rhetoric*, para. 1356a). But will anyone be inclined to trust someone who *openly* declares that truth is whatever they happen to think it is, who leaves no room for fallibilism in their outlook, or who has abandoned “criteria of conclusiveness, demonstrativeness, necessity, certainty, justification, or validity,” as Dunn recommends (1993, 265)?

**4.3** Correspondence? Consensus? Coherence?

Among other reforms, the post-positivist critique urges us to abandon a view of truth as “correspondence” with reality. In favor of what? Perhaps a “coherence theory of truth” (Hawkesworth 1992, 316). Or perhaps a “consensus” conception: “The term ‘fact’ will be used throughout this text to refer to consensual agreements about particular events or states of affairs (that is, what is widely believed to be the case) rather than to refer to a hard, firm, uncontestable reality located in an objective world” (Fischer 2003, 13).

Now it is not always clear just what these alternatives mean, and what they seek to rule out. Facts, Fischer tells us, are “consensual agreements,” and do not refer to... what, exactly? A “hard, firm, uncontestable reality located in an objective world.” Why all the adjectives? Might facts refer to a soft and mushy reality, but not a hard and firm one? And what exactly might it mean for reality to be “hard” or not, or even “uncontestable”?

Just as post-positivists don’t really believe their own injunctions to suspect all truth claims, they waver on this point as well. In a revealing passage, Hawkesworth declares that theoretical presuppositions are “neither irrational nor arbitrary. On the contrary, they embody the collective wisdom of determinate traditions. They comprise the sedimented and *thoroughly rational judgments* of generations of people who have *struggled to capture the truth about reality*” (Hawkesworth 1992, 317; emphases added). Is Hawkesworth not implying that the rationality of inherited judgments depends upon previous generations having held *something like* a correspondence understanding of truth?

This suggests the general difficulty in the post-positivist predeliction for consensus or coherence. Consider Fischer’s claim that: “Through the processes of deliberation and debate, a consensus emerges among particular researchers concerning what will be taken as valid explanation” (1998). Fine: but *what do they talk about*? Just what does that “deliberation and debate” look like? Imagine, for a moment, a set of researchers (or policy analysts, or decision-makers). They are discussing a contentious matter. Someone makes a factual claim in a very confident voice. But someone else offers clear evidence that the claim is *false*, that it is refuted in the “objective world” out there. Can anyone doubt that the standing of the first person in the discussion has taken a hit? And why should this be? Are members of the group in the grip of a “naive” correspondence theory of truth? Not necessarily. They may simply recognize that, however mysterious the notion of “correspondence,” however difficult it may be to pin down particular issues of fact, proceeding on the basis of demonstrably false information is not a brilliant idea.

No assumption is being made that the group members are virtuous lovers of truth. Consider a team of marketing experts hired by the tobacco industry to counter claims that smoking is addictive. They are, to be blunt, professional producers of lies. But they know that not just any lies will do: they must make judgments about which lies will be effective, and they want those judgments to be *true*. They are not seeking the truth *about* tobacco, but rather the truth concerning how best to *lie* about tobacco.

**4.4** An alternative

I have argued that the “paradigm paradigm” leads naturally to the view that positivism must be transcended *as a whole*. It is probably on the question of truth that this assumption has provoked the most mischief. Truth comes to be defined as consensus or coherence, *in opposition to* a “correspondence” view that characterizes positivism.

This opposition is unnecessary. Imagine a group charged with deciding whether the current balance of scientific evidence warrants classifying heavy cellphone use as possibly carcinogenic. They come to an agreement on this. Under what conditions would *they themselves* be likely to have confidence that their consensus determination was true? Certainly not if they had sat around asking “What sort of answer will please our boss?” or “How can we keep the stakeholders satisfied?” If that had been the case, they could have a firm consensus, but none of them would confuse that with *truth*. Nor would they confuse their conclusion with probable truth had they simply rushed to a consensus in order to be done with the meeting in time to watch a ballgame on TV. On the other hand, had they all tried as best they could to grapple with the available information and come to an honest summary of the current scientific evidence, they could have reasonable confidence in the truth-value of their shared assessment of that evidence.

It is thus clear that truth is related, not to agreement as such, but to agreement among those who have taken a particular stance, *an orientation to truth*. Consensus has some relation to truth only when it is reached in a group that is *seeking* the truth, not seeking something that will please someone or other, and not merely seeking consensus itself. The same could be said for “coherence”: *any* set of beliefs, however mad, can “cohere,” so long as the believer refuses to engage in “reality-checks.” So consensus or coherence are only related to truth when they are linked to something more than themselves, to a concern for truth.

A viable alternative to the post-positivist rejection of correspondence, which I sketch here without fully defending, holds that:

a) Consensus and coherence can contribute to the trustworthiness of a belief, providing that they are linked to an orientation to truth.

b) Yet they do not *define* truth: even the most responsible group of inquirers can unanimously share a belief that later proves to be false, and a belief may be consistent with all one’s other beliefs and experiences, and yet be false. Truth is not about *me*, about how one of my beliefs fits with the others (coherence). Nor is it about *us*, about whether we agree on this or that claim (consensus).

c) Truth concerns a *relation* between a knowledge claim and something in the world. To use a very old language, truth is about the adequacy of knowledge to its object (Aquinas 1952, Q.1, A.2). It is acceptable to label this adequacy “correspondence,” so long as one does not take that word too literally.[[7]](#endnote-8) This is acceptable, even though *no simple definition of correspondence exists* (Kant 1930, para. B83).

d) Understanding truth this way tells us nothing about our specific reasons for holding particular beliefs, nor does it provide us with a general tool-kit for making truth-judgments.

This last point means that a correspondence understanding of truth in no way commits one to “empiricism,” nor to believing that facts are “unproblematic” and “immediately observable.” Nor does it entail the consignment of normative statements to a purely “subjective” realm. A subjectivization of ethics requires quite different assumptions. The simplistic claims made about “positive” and “normative” statements in some introductory textbooks require, not a correspondence view, but an assumption that the nature of a normative statement can be assessed in isolation from all its supports. One declares a favorable view of press freedom to be a “matter of personal preference” (Sirkin 1999, 3), without ever asking anyone just *why* they support press freedom. It is, of course, all too easy to declare *any* belief to be an unsupported, purely subjective preference, if one has resolutely ignored its supports.

**5** Conclusion

I have argued that the “paradigm paradigm” has led to a problematic understanding of the mode of existence of positivism in the policy world. We would do better to think of positivism as a culture. The previous sections claimed various benefits for this approach. It can push critics to abandon caricatures, and can render both their critiques and their suggested alternatives more precise. It can also help address the truth muddle, into which many post-positivists have been led precisely by adopting the oppositional stance fostered by the paradigm paradigm.

Rather than saying that we should talk about positivism in a new way, as a culture rather than a paradigm, this approach may lead the reader to wonder whether we need use the terms positivism and post-positivism at all. Feminists, one might observe, could challenge the sexism of modern culture without talking about that culture in general, but about its sexism in particular. Analogously, one might argue that challenges to those elements of positivism seen as problematic may best proceed by addressing those elements directly.

Nothing said in this paper, however, questions the existence of positivism: the paper questions positivism’s *mode* of existence. Because positivism is a cultural reality, many people will assume that its various elements are interrelated, and either embrace them or reject them on that basis. So just as a post-positivist can (wrongly) believe that a quantitative approach must present itself as “value-free,” a quantitative social scientist can believe this as well. Positivism exists, and so do positivists.

The continuing importance of this cultural phenomenon is suggested by a passage near the end of Thomas Piketty’s *Le capital au XXIe siècle*. Piketty begins by urging economists to abandon their attachment to “allegedly scientific methods” that are “based above all on an excessive reliance on mathematical models” (2013, 946). At this, the post-positivist will nod in agreement. But Piketty then turns in another direction, critiquing social scientists who “flee whenever a number appears,” and who “limit themselves to saying that every number is a social construction” (947).

The polarization Piketty decries is real, and well-known to anyone who works in a reasonably pluralistic social science department. The diagnosis advanced by the present paper argues that the two camps Piketty critiques are frozen on opposite sides of a cultural divide.[[8]](#endnote-9) And how might that divide be transcended? Not, certainly, by expecting quantitative social scientists to abandon their intellectual capital. Some, at least, may respond positively to an invitation to do *better* quantitative work, work that is freed of the characteristic weaknesses of positivist culture. But such an invitation can only be issued by those who have arrived at a more precise understanding of just what those weaknesses are.

This should not be taken to mean that we must seek a “certain symbiosis” between positivism and post-positivism (Johnson 2005). This leads us in exactly the wrong direction, *away* from greater precision and a more focused critique on specific aspects of the policy world. If one wishes, for example, to blend quantitative and qualitative methods, then one should do just that, and call it just that. But recall that positivist culture can be *legitimately* critiqued not for its use of numbers, but for its denigration of other forms of analysis and its cavalier attitude towards the challenges of operationalization: clearly one cannot “blend” these flaws with competent qualitative work.

Nor should it be thought that, because it refrains from attacking positivism as a whole, the cultural approach is inherently “incremental.” Imagine a quantitatively-oriented analyst who has never given much thought to the problem of operationalization. Consciously or not, the analyst has adopted P. W. Bridgman’s influential dictum that “In general, we mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations” (1928, 5). Somehow or other, this lack of reflexivity is called into question: the analyst comes to realize that it is no simple matter to measure many of the things that are most important in our world, that every operationalization can and should be subject to critical scrutiny. To someone who holds the positivism-as-paradigm view, this change may seem minor. But to the analyst herself, it could be experienced as a radical change, calling into question much of her previous work. And to an observer not wedded to a belief in enveloping paradigms, the change may well appear to be a *leap* from “bad” to “better” analysis.

The example illustrates how a more rigorous critique can make a real contribution to policy practice. There is no reason for a practitioner to heed muddled critiques of the positivist paradigm, nor to abandon commonsensical notions of truth for alternatives that, while appearing sophisticated, cannot survive critical scrutiny. But a reconstructed critique, focusing on specific elements of positivist culture, can suggest clear reforms for policy practice.

Note, too, that our “new improved” analyst is likely to be less *arrogant*. The thinker who comes to recognize the qualitative judgments that must underpin *any* quantitative research is less likely to dismiss other approaches as merely subjective, as “belles lettres” (Riker 1982, 753). This brings us back to Habermas’s claim that positivism is the disavowal of reflection. To refuse to reflect on one’s own analytical practice is not only poor method, it provides a basis for the arrogance with which some quantitatively-oriented policy analysts dismiss qualitative research.

Finally, the cultural approach also highlights the cultural *challenge*. Hannah Arendt stressed that “we are always educating for a world,” even if it is a world “out of joint” (1968, 192). The “world” for which policy students are being trained is often one in which many of the cultural elements critiqued by post-positivists continue to hold sway. So young graduates may well find themselves in an organizational culture hostile to serious methodological critiques. Brilliant insights alone will not save them: in some situations, they may even be the graduate’s undoing. And so we must give students, not merely good insights, but suggestions on how to cope with the cultures in which they may find themselves, and encouragement to think in advance about what stance to take towards those cultures.

**Notes**

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1. Recent examples of such “it goes without saying” reference include Fransen (2011), Zittoun (2009), McAuliffe Straus (2011), and Auer (2010). [↑](#endnote-ref-2)
2. Examples include: Palys (2008), Leith and Phillips (2012), Hammersley (2013), Gabrielian, Yang and Spice (2008), Bryman and Bell (2011), Hennink, Hutter, and Bailey (2010), and McNabb (2013). [↑](#endnote-ref-3)
3. Influential for a time in the philosophy of science, this is the positivism that Karl Popper claimed to have “killed” (Popper 1974, 69). [↑](#endnote-ref-4)
4. Morçöl tested just five dimensions of the positivist list. His finding of a lack of full commitment to those five elements applies *a fortiori* to the longer list. [↑](#endnote-ref-5)
5. Fischer too declares that “the facts do not speak for themselves, as is commonly believed and affirmed by the positivist theory of knowledge” (2003, 13). [↑](#endnote-ref-6)
6. While the previous passages are drawn from different texts, Fischer is consistently inconsistent, so to speak. Though Fischer (2003) advises us to eschew “truth-seeking,” the first few pages of that work contain various references to this or that being “clear” or “made clear,” and six references to “recognizing” this or that. Later, the book even reproduces an entire table of “myths” and “findings” concerning science. [↑](#endnote-ref-7)
7. That is, there is no one-to-one relationship between knowledge claims and things in the world: many true things can be said about any given object. [↑](#endnote-ref-8)
8. Ironically, one reviewer declares that Piketty himself “does not escape from the positivism that afflicts most contemporary social science.” One of his positivist sins is that “he supports his argument by presenting numerical evidence” (Fainstein 2014). [↑](#endnote-ref-9)