

# Combining Interpretivist and Positivist Approaches in Social Science Research\*

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## Abstract

Interpretivist and positivist approaches to social science are generally believed to be at odds and combining them is discouraged. Nonetheless, we find that some scholars are combining these approaches to answer questions that require understanding human meaning-making, where interpretivism excels, and inference about causal relations, where positivism excels. We argue on behalf of combining these research traditions to investigate meaning-making and cause-and-effect in the same study, which we term *mixed-epistemology research*. We argue that the incompatibility of positivism and interpretivism has been overstated; the philosophy of science provides a basis for mixing epistemologies and the core concerns of the associated research communities can sometimes be reconciled. We examine when and how mixing epistemologies works in several examples of applied research, and draw out lessons for researchers.

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# 1 Introduction

Interpretivist and positivist traditions in the social sciences are often cast as foes. Although other varieties of mixed-method research are ascendent in the social sciences, methodologists continue to warn against combining interpretivism and positivism (Yanow 2003; Furlong and Marsh 2010; Ahmed and Sil 2012, Beach and Kaas 2020). We argue that combining interpretivism and positivism in a single piece of research – which we call *mixed-epistemology research* – is emerging as an important strategy for social science research. We document how applied researchers are combining these traditions to reach new insights, yet remaining modest about the depth of their methodological innovations. Downplaying the contributions of mixed-epistemology research to the process of discovery in the social sciences reinforces the misperception that it can't be done.

We argue that methodological theory is lagging behind the methodological practices of mixed-epistemology research and needs to be caught up. In this paper, we question the received wisdom that these traditions are incommensurable and impractical to combine. We do not urge committed interpretivists and positivists to change their research practices, but for readers who see the merits of each approach and are already inclined to wear, say, the hat of an interpretive ethnographer on one day and of a positivist statistician on the next, we offer theoretical justification from the philosophy of science and practical insights from applied research.<sup>1</sup>

Ours is not the first call for combining interpretivism and positivism in some configuration (Wendt 1999; Roth and Mehta 2002; Laitin 2003; Sil and Katzenstein 2010; Jackson 2011). For example, in a symposium in *World Politics*, Peter Katzenstein admonished that “[a] sharp distinction between a nomothetic and an idiographic social science...will do little to help us...” and “anybody seeking to answer an important and interesting question would be a fool to sacrifice the insights that can be gleaned from either perspective” (Kohli et al. 1995). Yet calls for mixed-epistemology research have not gained significant traction, perhaps because they have not

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<sup>1</sup> We recognize that unfettered methodological exploration requires “constant retooling [that] is both costly and rare” (Herrera 2006, 5) and is aided by access to training, time, money, and prestige. We can't make a mixed-epistemology approach equally available to all, but we can reduce, at least, the intellectual barriers.

adequately addressed two persistent critiques: that mixing is illogical and impractical (Yanow 2003, 12; Beach and Kaas 2020).

We take these critiques seriously as we attempt to disarm them. We acknowledge that interpretive and positive social science approaches derive from different research traditions, each with a long genealogy, drawing on disparate understandings of the world (Section 2). Many practitioners in these traditions hold distinct, irreconcilable assumptions about *ontology* (the nature of reality) and *epistemology* (whether and how that reality might be known). For many interpretivists, the research world does not exist independently of the observer, and is therefore not knowable through external observation. Interpretivists, as a result, focus on interpreting the meanings of actors according to their own (subjective) frames of reference, prioritize the body and mind of a researcher as tools for insight and inference, and tend to use empirical evidence in support of particular claims, rather than generalizations. Positivists, by contrast, generally view the research world as objectively real and observable, and tend to focus on the verification of observable causes. These ontologies, then, give rise to different kinds of social science epistemology, and the prevailing view in the methodological literature and research practice is that methodologies based on these epistemologies should remain separate.

We build a philosophical and practical case for combining interpretive and positive social science research, especially to address problems that involve inference about human meaning-making and inference about causal relations (Section 3). Philosophically, we argue that there is a coherent philosophy of science – Scientific Realism – that accommodates mixed-epistemology research. Practically, we show that mixed-epistemology research is possible and advantageous by exploring examples (Section 4). We find that the value-added of mixed-epistemology research is, as it should be, to develop new, elegant, and useful explanations for social phenomena. But it can also provide ancillary benefits, sparking research creativity, enhancing research transparency, and encouraging livelier writing.

With a clearer view of what combining interpretivism and positivism might entail, we return to the stakes in Section 5. We discuss several other criticisms of mixed-epistemology research that do not fall neatly under the headings of philosophy or practice: disagreements about the politics of knowledge, and about the politics of Political Science as a discipline. We argue that

placing mixed-epistemology work on a sound philosophical footing will allow researchers to move beyond debates about whether or not interpretive methods are merely the “summer intern” in service of a hegemonic positivist project for the social sciences (Hopf 2006, 18). Yet we cannot resolve all possible concerns, and as long as mixed-epistemology researchers keep forging ahead, we expect that trail-and-error will answer these lingering questions and raise new ones. This emergent mixed-epistemology research is inevitably a mixture of art and science, for which exact recipes do not yet exist, but with insights that are evident.

## 2 A Tale of Two Traditions

The prevailing view is that interpretivism and positivism are well-defined, incompatible, mutually exclusive approaches to research that flow naturally and obviously from incommensurable claims about what the world is made of (ontology), how we can know about it (epistemology), and what practices and procedures are best for knowing (method). Positivism is the logical descendent of Popper’s philosophical tradition, via Lakatos and Laudan (Popper 1935, Lakatos 1970; Laudan 1977), while interpretivism comes from a linguistic tradition, via Weber, Geertz, and Wittgenstein (Wittgenstein 1953 (2001), Geertz 1973). In the standard account, “[a] positivist looks for causal relationships, tends to prefer quantitative analysis...and wants to produce ‘objective’ and generalizable findings,” while, by contrast, “[a] researcher from the interpretivist tradition is concerned with understanding, not explanation, focuses on the meaning that actions have for agents, tends to use qualitative data, and offers their results as one interpretation” (Furlong and Marsh 2010, 21). In describing positivism and interpretivism as “a skin, not a sweater,” Furlong and Marsh assert that these traditions cannot be shed lightly, while invoking also, perhaps accidentally, the flaying of martyrs willing to die for their religion. If interpretivism and positivism are defined as mutually exclusive opposites in a holy war, and moving from one to the other is as invasive as removing ones’ skin, then combining them seems out of the question.

Not surprisingly, we disagree with parts of this standard account. We conceive of interpretivism and positivism as distinct, but not necessarily opposed, *methodologies*, by which we mean a framework of inquiry based on assumptions about the nature of the social world and the

place of knowledge production within it. *Methods*, by distinction, refer to the research and analytical practices and tools that researchers use to arrive at understandings of phenomena of interest. While certain practices and tools are perceived to share an affinity with either interpretivism or positivism,<sup>2</sup> methods are not inherently linked to either methodology.<sup>3</sup>

Both positivism and interpretivism are hard to precisely define, because these terms invoke both description (what do scientists do?) and prescription (what ought scientists do?), which do not always align. Positivism has a prescriptive set of norms for how scientists should conduct inquiry that are related to ideas stemming from Popper's (1935) articulation of logical positivism: a theory should generate falsifiable hypotheses, for example, and should be rejected if falsified in a test, but a theory can never be proven true. However, descriptively, positivism may be whatever self-identified positivists do. If many positivists do not immediately reject theories when a test appears to falsify them, or declare that evidence supports their theory being true, a descriptive approach is not to immediately discount this as "not positivist" but rather to recognize that positivism as a tradition is more varied than one might get from a reading of just a few core texts (which, we note, many positivists have not themselves read). Thus, Hawkesworth (2015) can correctly declare that "[t]he primary postulates of positivism have been subjected to rigorous and devastating critiques" (31), and yet fail to land a fatal blow to the enterprise of positivist social science because it was never really based on proof-texting Popper.

Interpretivism is no easier to corral into a clean definition, and the authors of a prominent volume don't even try, writing: "we have chosen not to force-fit contributors into our view of interpretive science; and so the chapters at times produce the 'wild, messy intercropping'...of interpretive research." (Yanow and Schwartz-Shea 2006, xxii). We will dive in with definitions,

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<sup>2</sup> Consider, for instance, the compatibilities between experimental methods and (positivist) causal explanations, or between ethnography and the aims of interpretivism. For more on the methods that share an affinity with interpretive research, see Yanow (2006, xix).

<sup>3</sup> We drew this distinction from lectures presented by Timothy Pachirat and Frederic Schaffer at the 2021 Institute for Qualitative and Multi-Method Research. The distinction is also made by Gerring (2012), Jackson (2006), and Sartori (1970), who wrote that methods texts have little to do with methodology, which concerns the "logical structure and procedure of scientific enquiry."

but with the understanding that these are capacious and contested terms, with both descriptive and normative definitions.

Interpretivism stems from the ontological position that knowledge is intersubjectively created by the researcher and the subject(s) of research. The goal of an interpretivist, then, is not to objectively observe a research world, but to develop interpretations of how that world is perceived by its actors and relay those interpretations to others—i.e., to interpret other people’s interpretations of their social world (Geertz 1973, 9).<sup>4</sup> In practice, this involves close attention to the meaning and significance of symbols, choices of words, and assumptions—without prejudging how these meanings will relate to the research later. It also requires reflection on alternative interpretations, and on the role of the researcher—her position, background knowledge, and internal reactions—in shaping her interpretations.

By contrast, positivism posits an external world against which hypotheses can be tested, and views the accurate representation of this (objectively existing) world as the goal of social science. Although the term “positivist” is contested, “the label has stuck despite the attempt to modify it with various prefixes (e.g. neopositivism)” (Lake 2013, 578). The term and tradition are linked to the philosophical positivism of Popper and are subject to vigorous critique (Hawkesworth 2015), but current research practices rarely involve the strict falsification of Popper. Instead, positivists maintain that “researchers can separate themselves from reality and objectively observe the world they inhabit, that science is and should be limited to observable implications and factors, and that the purpose of science is causal inference” (Lake 2013, 578).<sup>5</sup> Thus, we see the key distinction between positivism and interpretivism as epistemological: Positivists think researchers can stand outside the research world and observe it, while interpretivists see knowledge as intersubjectively created between researcher and research worlds. A positivist is likely to evaluate a claim with external observation while an interpretivist is likely to evaluate a claim with situated introspection, or in colloquial terms, “learning by

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<sup>4</sup> As Pachirat (2006) succinctly describes the enterprise, interpretivism involves “humans making meaning out of the meaning making of other humans.”

<sup>5</sup> We should note that here, too, there is contestation: Lake continues that, “Within these beliefs, of course, there is disagreement about the precise meanings of key terms, especially about what constitutes an adequate causal explanation” (Lake 2013, 578).

doing.” For this reason, we refer to research that uses both interpretivist and positivist ways of knowing as *mixed-epistemology research*.

Despite the complexity of precisely defining interpretivism and positivism, it is relatively easy to distinguish scholars operating in the positivist and interpretivist traditions. In addition to distinct methodological practices, there are differences in the style and form of writing, there is clustering in departments and professional organizations, and each has parallel publishing and prestige hierarchies that further reinforce groupness (and intergroup relations are too often characterized by ignorance, distrust, and snide comments). We agree with Hay (2002) that the gap reflects aesthetic sensibilities: purpose vs. play, design versus chance, hierarchy versus anarchy (223).

The distinctiveness of interpretivist and positivist research communities is striking because it persists in an era when mixed methods are ascendent. Emerging out of the “methods wars” and the Perestroika movement (King, Keohane, and Verba 1994; Brady and Collier 2004; Shapiro, Smith, and Masoud 2004, Monroe 2005), new generations of social scientists routinely employ multiple methods in the same piece of research and advice for their combination is increasingly sophisticated (e.g., Humphreys and Jacobs 2015, Seawright 2016). Mixed-methods research is now seen by many as a strategy for overcoming the limitations of single-method research. Advances in mixed-methods research have been accompanied by philosophies of methodological pluralism. Katzenstein and Sil (2008), for instance, argue for modes of eclectic scholarship that “trespass deliberately and liberally across competing research traditions with the intention of defining and exploring substantive problems in original, creative ways” (210). The result—what they term, “analytic eclecticism”—is distinguished by the fact that “features of analyses...embedded in separate research traditions can be separated from their respective foundations, translated meaningfully, and recombined” to create original and pragmatic scholarship—a scholarship that “eschews metatheoretical debates” and that engages different traditions in “meaningful conversations about substantive problems” (210-211).

The vast and growing mixed-methods literature contrasts with a remarkably scant literature on mixing interpretive and positivist approaches. There are exceptions. Wendt’s (1999, 47-91) *Social Theory of International Politics* advocates for a marriage of positivism and social

constructivism through a scientific realist philosophy. Wendt's (1999) "*via media*" (a "middle way") between interpretivism and positivism is to adopt interpretivist ontology -- "social life is 'ideas all the way down'" (90) -- and a positivist epistemology. This approach is defended in Wendt (2006) and debated by Guzzini and Leander (2006). Laitin (2003) advocates for a "tripartite" combination of (interpretive?) ethnography, statistics, and formal theory, though it has been criticized for making "ethnography the summer intern" (Hopf 2006, 18). Roth and Mehta (2002) make a practical argument for mixed-epistemology work by conducting parallel interpretivist and positivist investigations of school shootings to show by example how researchers might benefit from exploring the same phenomenon from different epistemologies. However, these theoretical interventions have not really been in conversation with each other, nor accumulated much of a following. We identify applied social science research using mixed-epistemology approaches, but these studies have not been placed in conversation with the methodological literature or with each other.

A more coherent methodological project urges scholars to adopt an "ethnographic sensibility," (Pader 2006, Schatz 2009), now with spin-offs such as "emotional sensibility" (Pearlman 2022). This could sound like an appeal for adding interpretivist ethnography to the positivist methodology repertoire in the spirit of Thachil (2018), but current usage is narrower. Pader (2006) coins the term "ethnographic sensibility" and loosely equates it with participant observation conducted with an interpretive perspective (163). In contrast to Pader, Schatz (2009) defines an "ethnographic sensibility" as all the parts of ethnography that are *not* participant observation, "transcending artificial distinctions between fieldwork and deskwork" (6). For Schatz, this term solves the problem of what an ethnographer is doing when they collect and analyze data non-ethnographically. It is a normative claim -- that an ethnographer should approach all of their research activities with the same sensibility, and avoid "reducing ethnography to the process of on-site data collection" (6). But for Schatz and subsequent authors, it is also a claim about how to remain interpretivist while doing "deskwork" activities that feel far removed from ethnography's core method of participant observation.

When positivists are urged to adopt an ethnographic sensibility, it is intended as a one-way conversion to interpretivism. For example, Simmons and Smith (2017) suggest that an



ethnographic sensibility might keep positivist scholars from ignoring the meaning making of their subjects, but the solution is effectively to become interpretivist, and stay that way. Positivists should “move away from the language of variables” (128) and avoid statistical control. “Designing a research project so that potentially relevant factors could be dismissed through “control” would be inappropriate for scholars approaching their research with an ethnographic sensibility” (127). While the language of ethnographic sensibility could become a rallying cry for mixed-epistemology research, it does not mean that yet.

Most scholars remain skeptical of mixed-epistemology research, and methodologists have almost universally policed the boundary. Sale et al. (2002, 47), flatly declare that “one cannot be both a positivist and an interpretivist.” Furlong and Marsh (2010) admonish that “researchers cannot adopt one position at one time for one project, and another on another occasion for a different project. These positions are not interchangeable because they reflect fundamental (sic) different approaches to what social science is and how we do it” (193). Beach and Kaas (2020) call combining them “mission impossible” (216) and declare that “[t]he only way that multimethodology research is possible is if a method from one of the methodologies is transformed into a pale shadow of itself” (215). Even Lake (2013), who professes the value of both traditions, worries that “any attempt to bridge the divide will either produce a pabulum of inconsistent approaches or profound frustration from dealing with incommensurable facts and ‘explanations’” (579). Prior calls for pluralism have not broken through this wall of skepticism.

### **3 Scientific Realism Justifies Mixed-Epistemology Research**

In the prevailing view, the purported incommensurability of interpretivism and positivism is rooted in fundamental philosophical differences, and thus combining them is logically inconsistent. Yanow (2003), for example, concludes that “given their contradictory ontological and epistemological holdings, the two approaches are incompatible” (12). Ahmed and Sil (2012) agree that “these approaches are predicated on fundamentally distinct ontologies and conceptions of causality, [therefore] the findings they generate are ultimately incommensurable and do not serve to strengthen each other” (936). Beach and Kaas (2020) reiterate that “taking

their underlying ontological and epistemological assumptions seriously means that they are fundamentally *incommensurable*." (227, emphasis original).

We contend that there *is* a coherent philosophy of science that renders mixed-epistemology research commensurable: Scientific Realism, particularly as articulated by Godfrey-Smith (2009). Scientific Realism is a view that currently dominates the philosophy of science, though it is largely neglected by political scientists (though see Wendt 1999 and Wendt 2006).<sup>6</sup> Philosophy of science is rarely how applied social scientists want to spend an afternoon, so readers who want to get straight to applications of mixed-epistemology research can skip ahead. But our defense of mixed-epistemology research is ultimately rooted in theories of science, so it is to these theories that we turn.

We start our exposition with Burrell and Morgan (1979, 3), who offer a schema of the relevant dimensions of philosophy for science, breaking the issues down into questions of ontology, epistemology, method.<sup>7</sup> Ontology can be realist or nominalist (sometimes called "anti-realist" or "instrumentalist"), where, roughly speaking, realists hold that the world exists independently of human thought and nominalists hold that it does not. Epistemology can be positivist ("objectivist"), meaning the world can be known objectively, or anti-positivist ("subjectivist"), meaning the world can only be known subjectively. And method can be nomothetic (focused on producing generalizable knowledge) or ideographic (focused on specific knowledge that may not be general).

The standard view is that positivism is an epistemological stance where the world is objectively knowable, and that this view requires a researcher to adopt a realist ontology and nomothetic methods. Interpretivism, in contrast, is the opposite epistemological stance that knowledge is necessarily subjective, and this supposedly necessitates adoption of a nominalist ontology (there is no reality independent of human meaning making), and ideographic methods.

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<sup>6</sup> "This neglect is surprising, since as one critic put it, "[t]here is little doubt that realism has come to be the predominant ontological position among contemporary philosophers of science."<sup>4</sup> [where fn 4 is to Rouse, 1987: 130]. (Wendt, 1999, 48).

<sup>7</sup> Burrell and Morgan (1979) consider "human nature" a fourth dimension, but we set the discussion of human nature aside because in their understanding, and ours, this is not a key dimension of difference for interpretivism and positivism.

What social scientists typically call “positivism” is a bundle of realist ontology, positivist epistemology, and nomothetic method, and what they call interpretivism is a bundle of nominal ontology, anti-positivist epistemology, and ideographic method. We see room to mix and match.

We agree that realist and nominalist ontological commitments are incommensurable opposites; it is logically inconsistent to simultaneously claim there is a real world out there, apart from human minds, and also that there isn't. Following Jackson (2011, 24), we argue that on the question of ontology, one must make a wager, and we prefer one that leans realist. It's a pretty radical account of the universe to believe that it literally did not exist prior to human meaning-making about it.

The epistemological commitments of positivism and anti-positivism are also framed as incommensurable opposites by the standard view, but this is only true if a researcher must choose one wager to make about these commitments for all time and all domains. This assumption seems wrong to us: we might be able to learn about some phenomena objectively and other phenomena subjectively. We do not agree with the implication of some interpretivists that if one ever commits to a subjectivist epistemology, one must remain continually committed (e.g., Simmons and Smith 2017). All that we consider necessary is a temporary commitment to one or the other in a specific domain, and perhaps a theory of which domains can be known in which ways.

The typical problem with adopting a purely realist-positivist-nomothetic approach is that it struggles to provide insight about contingent meaning-making by people. Social structures that are endowed with a lot of meaning by the humans that build them are a prime example of where realist-positivist-nomothetic approaches struggle. Yet the typical problem of adopting a purely nominalist-antipositivist-ideographic approach is that everything becomes “ideas all the way down” (Wendt 1999). We argue that the corrective is strikingly simple. Some domains of the world are best approached with a positivist, objective epistemological approach, while others are best approached with an interpretivist, subjective approach. The question of which domains should be approached in which way is itself a question of science, so it does not have to be answered ahead of time. While this might sound like trying to have cake and eat it too, it is consistent with the naturalized scientific realism of Godfrey-Smith (2009).

The key move of Godfrey-Smith is to formulate scientific realism in a way that treats some parts of reality as independent of meaning-making, while allowing meaning-making to nevertheless influence reality in many domains. For him, the “idea that reality is ‘independent’ of thought and language” is “on the right track, but it has to be understood carefully,” because “thoughts and words are, of course, real parts of the world” with a “a crucial causal role.” Thus, reality is not independent of thought and language when they affect other aspects of reality. Godfrey-Smith argues for a “common-sense” realist ontology: “We all inhabit a common reality, which has a structure that exists independently of what people think and say about it, except insofar as reality is comprised of, or is causally affected by, thoughts, theories, and other symbols...and except insofar as reality is dependent on thoughts, theories, and other symbols in ways that might be uncovered by science.” This last clause leaves open the possibility that science might conclude that human ideas *do* structure the physical world in deeper ways than we now realize. If one further agrees with Godfrey-Smith that “One actual and reasonable aim of science is to give us accurate descriptions (and other representations) of what reality is like...[including]...aspects of reality that are unobservable,” then one can conclude likewise “I am a scientific realist.” (176)

In the philosophy of science, positivism is actually often understood as an anti-realist approach, because logical positivism as articulated by Popper (and the related logical empiricism of Hempel) has no place for real unobservables. For positivists working in the tradition of falsification and covering laws, epistemology determines ontology: only that which is observable is real. This leads to problems with causality, which itself is unobservable and has been awkwardly shoe-horned in as “constant conjunction” between events. Social scientists that prioritize causal inference and speak regularly of unobservables might find themselves at odds with empiricist positivism

We suspect a common-sense realism will feel more natural than either an empiricist/positivist approach which has no place for aspects of reality that are unobservable, or an anti-positivist approach (of which there are many varieties) in which either theories do not have to be accurate because all that matters is that they make good predictions or theories must treat everything, including the perception of a shared physical world, as contingent on human

meaning-making. This formulation provides the basis for treating things like ideas from both nominalist and realist perspectives at different parts of a research process. Ideas are physical – they are chemical processes in a brain – but they are currently unobservable as such. We can summarize those chemical processes semantically by trying to understand and describe the idea, but to say that an idea is widespread (for example) is shorthand for saying that somehow chemical processes are happening in many brains that allow for the rough approximation of this idea to be accessible to a lot of humans at once. We treat this nominally because that is itself a tool for understanding the idea. Yet if we move to empirical testing about the causes and effects of the idea, we maintain a basis for treating the idea as “real” because our ontology is that its physical basis is real. Regardless of whether we have used nominal-interpretive-ideographic approaches to understanding the idea, this does not keep us from treating the idea as real, conceptualizing it as a variable, and including it (or a measure of it) in a formal model, regression, case selection procedure, or any other positivist procedure that relies on these things being real independent of interpretation. The fact that something *can* be reinterpreted does not mean that it *is being* reinterpreted at all times, and for the times when it is not, treating it as a real, independently-verifiable thing in a causal chain allows social scientists to make progress.

Because of our realist ontological wager, our version of interpretivism leaves room for a common reality with at least some structure independent of what people think and say about it. We don’t question all physical phenomena, but instead remain alive to the social construction of reality in places that positivists typically are not, allowing that physical phenomena are shared stimuli interpreted divergently. For example, we would not typically argue that when the death of a person eventually results in the end of their autonomous movement, this is wholly a social construction, but rather that the significance and meaning of the death varies greatly and is socially constructed.

Our version of positivism leaves room for unobservables, such as ideas, to have causal force in the world. We believe interpretation is especially important when ideas are part of a causal chain, whether it’s recognized or not. Positivists’ best tools for understanding ideas are primarily linguistic and interpretive – we can’t represent them as networks of neurons firing, for example, even if we ontologically hold that ideas have a physically basis in the world.

Just because we have found a version of scientific realism that we believe justifies combinations of interpretive and positive approaches does not mean that all versions of scientific realism work. In fact, we find some other formulations explicitly exclude our approach because they make alternative moves in ontology (especially about what is real) that are at odds. This is how Soss (2021) can contrast a realist approach that treats cases as things that “exist in the world...independent of the individual observer, as instances of a general kind” with a nominal approach that treats cases as sites of researcher meaning-making, where a researcher must decide what can be learned by viewing a given phenomenon as a case of one thing or another. Soss is right that applying realist thinking to cases means treating them as if they are out there, and we highlight in this paper the utility of Soss’ nominal approach to casing in the examples below. But our philosophy of common-sense realism supports this. An idea about whether a physical phenomenon is most usefully understood as a case of this versus that has both a realist element (the physical phenomenon exists independent of interpretation) and a nominal element. Human ideas are clearly on the causal path of case selection, so we can treat it as not necessarily part of a common reality that we inhabit and leave open multiple interpretations. We can have the right and wrong aspect of realism (the physical phenomenon happened or didn’t and is or is not physically like other events) *and* the relativity aspect of nominalism (there is no right or wrong interpretation of what an event means, and different interpretations might bring different physical events into similarity or dissimilarity with each other).

Our approach also differs from Wendt’s (1999) and (2006) formulations of scientific realism. Wendt’s move is to adopt an anti-realist ontology in which reality is deeply socially constructed, but maintain a strictly positivist epistemology that privileges scientific ways of knowing. By contrast, we prefer a realist ontology and are flexible about epistemology.

We conclude this section with humility. While we claim that scientific realism can provide a coherent philosophical basis for mixed-epistemology research, it is not a necessary or uniquely coherent basis. Our framework can be improved, and other accounts, such as Feyerabend’s anarchic philosophy of science (2020 [1975]), might strike a sympathetic-yet-skeptical reader as better justification for mixed-epistemology research. Luckily, the absence of a coherent, unified philosophy of science has never fully restrained the adventurousness of scientists.

## 4 Examples Show that Mixed-Epistemology Research is Productive

The practical critique of mixed-epistemology research is, bluntly put, that it isn't possible to do well. This is an understandable concern, given a yawning gap between the evaluative criteria of interpretivists and positivists, summarized well by Beach and Kaas (2020). Methodologists fret that one tradition will necessarily dilute, contradict, overwhelm, or subjugate the other, making one approach the "summer intern" (Hopf 2006, 18) or a "pale shadow of itself" (Beach and Kaas 2020). Or, if equality is achieved, the result will be an incoherent "pabulum" (Lake 2013, 579).

To assess these claims, we examine examples of mixed-epistemology research to see whether benefits can offset these risks. At the end, we distill some practical lessons for combining, but we are space-constrained, so our primary goal is proof-of-possibility. We examine the combination of interpretive and positive approaches in three works especially: Cramer (2016) *The Politics of Resentment: Rural Consciousness in Wisconsin and the Rise of Scott Walker*, about how rural consciousness structures political attitudes of ordinary Wisconsinites; Wood (2003) *Insurgent Collective Action and Civil War in El Salvador*, about how emotions and meaning-making explain mobilization in civil war; and Nielsen (2017) *Deadly Clerics*, about how Muslim clerics can turn to preaching violent Jihad when they fail to achieve a career trajectory matching their self-conceptions as scholars.

We selected these works based on familiarity, our judgement that the research involved mixing epistemologies, evidence that the research was positively received, and the availability of written reflections by the authors about their methodology. We did not systematically survey the literature and purposively select cases as most of the case study literature might recommend, in part because there is no clear population of definitively mixed-epistemology research studies from which to sample (other works we considered include Shesterinina 2016, Thachil 2018, Jones 2017, and Milliff 2023). Instead, our selection of these studies is an application of what Soss (2021) calls "casing a study." Based on our philosophical commitments, we don't think it is necessarily true that studies can be objectively classified as interpretive, positive, or a combination. These categories are a co-creation of the understanding of the author and of the reader, with meaning

and ideas appearing at the heart of how we interpret the study. (How could a reader's ideas about research *not* matter, when reading research?). We don't think there's a straightforward answer to whether these studies are "really" combinations of interpretivism and positivism. Rather, following Soss' nominal approach to casing, we believe a more insightful question is "what can we learn by viewing each of these studies as a combination of interpretation and positivism?" Yet, upon identifying portions of these works that we interpret to be positivist or interpretivist, we transition at some points to treating our interpretations as real, to learn how different parts of a study influence each other and the reader. In this way, the empirical part of our paper is an execution of our approach to moving between interpretation and positivism.

### **Cramer (2016), *The Politics of Resentment***

Cramer (2016) describes her methodology as interpretive: "[A]llow me to contrast it with...a positivist approach...that tests data to demonstrate causality and discover scientific laws that explain human behavior and society" (21). "The positivist model set-up assumes that values on one explanatory (or "independent") variable move independently of the other variables" (22). "However, the object of my study, or my dependent variable, to put it in positivist terms, is not a position on an attitude scale but, instead, the perspectives that people use to arrive at that position...how they create or constitute perceptions of themselves and use these to make sense of politics" (22).

Cramer's lively writing brings the reader along through ethnographic work at Wisconsin gas stations and coffee shops – anywhere the locals gather – to reach the insight that many Wisconsinites understand themselves and their relation to others through a collective rural consciousness. This understanding leads rural Wisconsin voters to understand conservative politics, including policies that appear to hurt rural voters, as a natural extension of their identities. "When I heard people using this lens to interpret their world, I heard them claiming that government and public employees are the product of anti-rural forces and should obviously be scaled back as much as possible. Viewing politics through the perspective of rural consciousness makes wanting less government a commonsense desire" (6).



Cramer's interpretation challenges a dominant understanding in political science about partisanship. "We political scientists often claim that whether a person feels closer to the Democratic or Republican Party is the most important predisposition for predicting what people think about politics, including how much government and redistribution people want. But in this book, I show how partisanship can be part of a broader understanding of who one is in the world and a less meaningful identity than we often assume" (6). "Instead of partisan identities, many of the people I spent time with in rural areas used identities rooted in place and class, this perspective I am calling rural consciousness, to structure the causal stories they told to each other—and to me—about the state of the economy before, during, and after the Great Recession" (6).

Cramer provides a masterclass in methodological reflection (26-44), so it is a surprising omission that there is little consideration of whether some of the analysis is positivist and how that interacts with the interpretivist methodology. Cramer's protestation that her study "is not about causation" and she is "not trying to predict how X causes Y" (21) is overly modest: causation infuses her theory and evidence. Cramer's claim that rural consciousness "functions to structure political understanding and contributes to a politics of resentment," seems causal; reasonable interpretations of the verbs "to structure" and "to contribute" require causal claims, and are compatible with a counterfactual causal model. Without rural consciousness, political understanding would be structured differently; without it, the politics of resentment would be less prevalent.

Cramer's argument intimately fuses meaning-making and causation using the counterfactual causal framework favored by positivists rather than the constitutive causation framework favored by interpretivists (Beach and Kaas 2020, 224). In particular, in chapter 4, Cramer works to discount the claim that rural Wisconsinites support small government because they don't benefit as much from government as urbanites. Cramer presents correlations of taxes and government spending, normalized per capita, showing that rural citizens get slightly *more* benefit from government than urban citizens; this analysis is persuasive primarily within a counterfactual, positivist framework. This suggests that even in a study as rooted in

interpretivism as Cramer's, positivist statistical analysis can provide supporting evidence; statistics can be the "summer intern."

**Wood (2003), *Insurgent Collective Action and Civil War in El Salvador***

Wood (2003) argues that commonly advanced explanations for high-risk collective action in civil war – class, selective benefits for participants, social networks, and political opportunity (11-17) – do not explain variation in patterns of civil war mobilization in El Salvador. Instead, drawing on interviews with 200 informants, and interpretive observations from watching informants draw maps of the land they fought over before and after the conflict, Wood argues that emotions and moral motivations are the best explanation for why poor *campesinos* (poor, rural residents of El Salvador) rebelled.

Wood develops her argument by interpreting the insurgents' own interpretations of their participation. There are two levels of meaning-making. First, *campesinos* offer reasons for their participation, which Wood attributes to "perceptions and interpretations of structures and processes by individuals" (2003, 40). Then, Wood adds her own layer of interpretation categorizing participant motives into broad categories: participation, defiance, and pleasure in agency. Wood's resulting theory ascribes causal force to the meaning-making of *campesinos* who then chose to rebel or not based on those meanings. When *campesinos* understood their actions in terms that Wood calls *participation*, *defiance*, or *pleasure in agency*, they were willing to engage in risky collective action as part of the civil war. Each of these three terms represents an interpretation by Wood of the meaning *campesinos* gave to their behavior, and how it motivated them, so this argument has the form we describe above, in which interpretations have causal force in the theory. She goes to great lengths to bring the reader along with her interpretations.

Much of the book describes her development of these interpretations using traditionally interpretive methods. Her development of the concept of *pleasure in agency* is especially instructive. This term, coined by Wood, captures perhaps the most interesting notion of all: that peasants rebelled because they felt pleasure at making history. Wood defines pleasure in agency as a "positive affect associated with self-determination, autonomy, self-esteem, efficacy, and pride that come from the successful assertion of intention" (235). The concept is so intimately

linked to her use of interpretative methods that she explains the definition with reference to moments of empirical interpretive insight. “[I]n carrying out insurgent activities, participants experienced a pleasure in agency: they had redrawn the contours of their world. Time and again I saw this pleasure relived as groups of campesinos gathered to tell me the story of their cooperative and its claiming of land or to draw for me the “before” and “after” maps of their locality” (235). Wood’s map-drawing exercises appear to have been essential for this insight. She returns again and again to the observation of pleasure as her informants drew them. The interplay of figurative and literal drawing in Wood’s prose is elegant and suggests that watching her interlocutors draw helped her understand a more symbolic redrawing of the world that their participation in civil war violence meant to them.

After extracting these motives from her ethnographic and interview-based data, Wood embeds the concept of pleasure in agency in a formal model which we understand as positivist because it encodes causal relations between variables that are fixed conceptually and can be manipulated independently to establish comparative statics (2003, 268). Wood also attends to positivist goals with her empirical work, using variation in her case-study areas to test her argument against alternatives (2003, 12-16), and evaluating the argument’s generalizability in a diverse range of out-of-sample collective actions (2003, 246-251).

Formalizing the concept of pleasure in agency highlights the strengths and challenges of integrating interpretations derived from interpretivist methods into a positivist framework. The variable version of “pleasure in agency” is arguably less rich and evocative than its interpretively-derived version, but it is also more precise. Formally, wood defines pleasure in agency as pleasure due to *successfully* changing history and some individuals simply have pleasure in agency (the variable  $\alpha$ ) while others don’t. The model makes expected success the core of this concept: “[p]leasure in agency...depends on expectations of success, in achieving valued social change, not simply on participating in a movement claiming to seek social change” (235). In the math of the model, “expectation of success” is captured solely through the number of other people participating in rebellion.

By contrast, Wood’s interpretive descriptions of pleasure, especially the interpretive map-making exercise, give the impression that it hinges less on success than the “variable-ized”

version. When she observes “pleasure relived as groups of campesinos gathered to tell me the story of their cooperative and its claiming of land or to draw for me the “before” and “after” maps of their locality,” can she be confident that this pleasure is linked exclusively to success? Surely, gatherings with old friends to relive the glory days have multiple interlocking pleasures. When she reports “the authors of the companion map (not included) to Figure 3.2 wrote, “It is a pleasure to participate together with all the companeros” (Cooperativa Las Conchas),” (215-218) we interpret a mix of pleasures that the formal model does not entirely capture. At a minimum, the passion of those participating seems like an obvious source of pleasure *and* expectation of success. When she describes pleasure in agency with the observation that “some of those interviewed recalled early strikes and marches with pride and in extraordinary detail” (241), we are left to wonder how the pleasure could be solely based on expectation of success when success at this early point was both distant and uncertain.

Yet placing the concept of pleasure in agency in formal conversation with canonical models of collective action (from Schelling 1978) has the benefit of showing other positivists how the pleasure in agency could matter in a causal theory of collective action. The puzzle of collective action is why it ever happens. By inserting her other-regarding concepts into an existing formal model with purely instrumental motives, she is able to show how “moral commitments and emotional engagements” (18) of rebellion could lead to collective action where instrumental economic motives might not (273-274). This move, we argue, is worth reifying a rich interpretive concept.

Despite reaching a striking insight via interpretive methods, Wood suggests that she could have benefitted from adopting interpretive methods earlier in her research process. Reflecting on her work, she writes “I wish I had better recorded the comments, jokes, and discussions that occurred during the map-drawing sessions. I was slow to understand their value, as I saw the maps as mere representations of land claims rather than as documents demonstrating the emergence of a new political culture...I also wish I had reflected more explicitly on the many ways in which participant observation informed my interpretation of all ethnographic data gathered” Wood (2009, 139).

### Nielsen (2017), *Deadly Clerics*

Nielsen (2017) seeks to explain why some Muslim clerics turn to preaching violent jihad. Nielsen argues that would-be clerics have similar ambitions to scholars, but differences in educational networks and professional opportunities prevent some from achieving those ambitions. Those whose ambitions are blocked are more likely to turn to preaching violent jihadist ideology. Causal inference is difficult in this setting because feasibility and ethics prevent randomization, but Nielsen argues that observational data show a correlation between various measures of network strength and the probability of producing jihadist rhetoric on the Internet. Nielsen explicitly uses interpretivist and positivist methods together to make integrated interpretive and causal inferences, and reflects on this integration in the book and subsequently (Nielsen 2020).

Nielsen's framing of the research puzzle presented by jihadism is an example of the method of "casing a study" (Soss, 2021). While much of the literature on jihadism frames its subjects as cases of fighters, terrorists, rebels, or perhaps activists, Nielsen tries to "make the case that Muslim clerics, including jihadist clerics, understand themselves as academics" (Nielsen 2020, 40), supporting this claim with evidence that prominent jihadists thought of themselves as "teachers" and engaged in various academic vanities like lauding their citation counts and taking photographs in front of well-stocked bookshelves. There is no objectively right answer as to whether jihadists should be viewed as academics or not. Appealing to jihadists self-conceptions, as Nielsen does, can't resolve the issue because it is implausible that this is the *only* way jihadists making meaning of what they do. The question is whether this re-casing of jihadists as academics leads to new insights.

Nielsen argues that his interpretation uncovers variables that have been omitted from other analyses: "Without the interpretive insight, the regressions would have been totally different. To my knowledge, variables such as "Does this person have a PhD in the Islamic Sciences?" and "Does this person report having memorized the Qur'an on their CV?" have not appeared in any other regression analysis of jihadists. (Nielsen 2020, 40). Nielsen's interpretive work helps make clear why blocked academic ambitions would make a difference in a causal pathway leading to violent activism. Failure to achieve academic and career success is causally important because it is meaningful to individuals. "In the first part of this book, I use both

qualitative and quantitative methods to interpret the culture of jihadist scholars and the broader culture of Sunni Islamic academia in which they are embedded. When I turn to testing hypotheses about the causes of clerics' divergent pathways in this culture, I turn to positivism, but maintain my reliance on both qualitative and quantitative data." (Nielsen 2017, 20).

It also affects the case definition and selection for his quantitative and qualitative analysis. For example, Nielsen takes an interpretive approach to defining who counts as a cleric (Nielsen 2017, 27-31). Because the term "cleric" conveys authority, and authority is contested in Sunni Islam, there is no "objective" definition, so Nielsen must choose, and this choice filters through to influence every subsequent aspect of the investigation (27-28). If Nielsen had considered jihadist preachers as "terrorists" or "rebels," then rather than collecting a data set of other clerics for comparison, he might have reasonably selected other terrorists and non-terrorists, or rebels and non-rebels. In this way, the interpretive work is woven through the positivist work in an inextricable way. Perhaps one is an "adjunct tool" to the other (Beach and Kaas 2020, 230), but if so, which is subordinate? Another lesson we take is that arguably any quantitative work that relies on contested definitions of social categories to define its cases relies on interpretation, whether explicit or not.

Nielsen uses "quantitative methods to interpret the culture of jihadis scholars and the broader culture of Sunni Islamic academia in which they are embedded", putting quantitative methods in the service of interpretive methodology. Nielsen devotes an entire chapter to developing a text-as-data approach to measuring "jihad scores" and validating them against a variety of sources. Some of this validation includes approaches similar to interpretive ordinary-language approaches (Schaffer 2006, 151), where Nielsen considers the way jihadists use language and summarizes that usage in a topic model to explain to the reader why (perhaps surprisingly to non-experts) the word "apostasy" is the most important linguistic marker of jihadism. Nielsen also uses a jihadist infographic decrying various non-jihadist preachers as a source of validation data, which draws on Wedeen's semiotic-practical approach of interpreting visual symbols (Wedeen 2002). And Nielsen takes an explicitly interpretivist approach to the task of labeling topic models. "The topics in a topic model do not come with labels signifying what they ought to mean; each collection of correlated words must be interpreted by the researcher. This is a

deeply subjective task. A significant part of the inference in a topic model occurs at the stage where a researcher looks at a list of correlated words and interprets them substantively as a single concept” (95).

Nielsen is not sufficiently transparent and reflexive about how his background might inform his interpretations, which is important evaluative standards of interpretive work. Nielsen is somewhat reflexive about the politics of studying jihadism (2017, 21-23), but omits a discussion of how his own religious background intersects with his understanding of his Muslim interlocutors. This omission becomes apparent in his subsequent reflection: “I found myself drawing on my childhood years of Mormon Sunday School” (Nielsen 2020, 39-40).

However, Nielsen innovates by contextualizing some of his interpretations with material that ethnographers traditionally exclude, including, for example, a photograph of a key moment of insight in the field (2017, 67) and posting redacted portions of fieldwork notes available online as part of a replication archive. While field notes aren’t “raw” data, they were recorded in the moment and provide tools for following with Nielsen’s evolving interpretation or rejecting it. The notes contain written records of key quotes from interlocutors, enhancing the “cognitive plausibility” of quotations and descriptions (Martin 2023). And in one instance, Nielsen is able to provide an university-made video recording of a cleric lecture he describes (Nielsen 2017, 70), so a reader can see the same event and offer alternative interpretations. While we are not suggesting that video recording should become a part of ethnography – a notion that Pachirat (2017, 148) criticizes – Nielsen’s choice to provide it is consonant with the vision of English and Zacka (2022) that interpretive material can be reanalyzed. More generally, coming to one methodology with the norms of another – in this case presenting interpretive participant observation with the positivist norm of making data available – is a productive, if controversial, site for innovation in mixed-epistemology research.

## **Distillations**

*Research can be interpretive theoretically or empirically, and positive theoretically or empirically in a variety of configurations.* Theory can take on an interpretive tenor when human meaning-making is part of the theoretical argument, which is typically true for interpretivists but

also true for positivists when ideas, understandings, and meaning-making feature in a theoretical causal mechanism. Research is empirically interpretive when the researcher adopts interpretive methodology to characterize and understand something. For positivists, this arises most obviously when creating classifications for cases (interpreting events as “political violence”), or when classifying texts for text analysis or evaluating topic models. Scholars can move between the four cells of this two-by-two: Wood uses interpretive empirical methods to develop an interpretive theory, which she then transforms into a positive theory for the sake of putting her ideas in combination with other theories. Interpretation as a methodology seems especially helpful to positivists who have interpretation in the causal mechanisms of their theories. Positivism as a methodology seems especially helpful to interpretivists who have interpretation on a causal path in a theory as well.

*Mixed-epistemology research usually operates by considering multiple meanings for something at one point in the research, and considering only a single meaning for the same thing at a different point of the research.* In the mixed-epistemology work we examine, there are transitions between reification and reflexivity; the researcher opens meanings to multiple interpretations for a portion of the research and then reifies again, usually solidify a new meaning for a key concept, to make a new argument. The research process need not be ordered so that consideration of multiple meanings come first and the single meaning comes later, though it often can be. For example, Wood opens up conceptual space to consider the meaning-making of her interlocutors in the El Salvadorian civil war to reach new insights about the category of meaning-making she calls “pleasure in agency.” But later, while placing this meaning-making into a causal argument with other variables, Wood reifies it, summarizing this complex meaning-making as a variable. Wood is not rejecting the rich complexity of pleasure-in-agency, but reification is a short-hand that facilitates progress on the goal of showing how this concept changes existing theory. This iterative reification and reflexification process contrasts with two extreme alternatives: (1) view everything reflexively and drowning in the sea of possible meanings from “permanent conceptual revolution” (Wendt 1999, 76) or (2) never question the meaning of key concepts at all.

The iteration of interpretivism and positivism can assist with tasks of conceptualization and classification. Rather than closing the door on alternative interpretations prematurely, scholars can consider alternatives, such as Nielsen (2017) considering whether jihadists can be usefully



seen as scholars in addition to other possible classifications. Yet eventually, Nielsen makes a choice to close the door on this interpretive exercise and transition from the nominal casing strategy of Soss (“what can we see differently if we understood jihadists as scholars”) to a realist casing strategy (“here is a data set of Muslim scholars, including jihadists and non-jihadists”) for use in a regression analysis to investigate whether academic network structure predicts jihadist ideology. This is a transition from holding many possible interpretations in mind to flattening them into one preferred interpretation is akin to a sound engineer mixing a set of audio tracks into a single music recording – it’s a necessary step for distribution but it makes the pieces difficult to pull out from each other.

All quantitative analysis is simplification, or dimension reduction; it represents complex physical and social elements of the world with a few numbers in a data matrix, and even fewer in a model. But an interpretive ethnography is also a simplification – taking everything an individual experienced in fieldwork and representing it concisely. The difference, then, between interpretivism and positivism is not about the need for simplification, but rather different answers to “what is the best tool for producing useful simplifications?” The interpretive answer is often “a human mind” and the positivist answer is often “mathematical representation.” Minds are very good at drawing connections and intuiting meaning, but minds are not good at probabilities, especially conditional probabilities, so we see value in disciplining the mind, even when we have to lock in interpretive choices to represent the world in abstract mathematical terms. Mixed-epistemology research may be attractive for scholars who value both approaches.

*Interpretation can be a vehicle to the moment of inspiration.* A single insight that restructures our interpretation of the social world can take disconnected and seemingly contradictory facts and fit them together in our minds, with the satisfaction of a jigsaw puzzle falling into place. While moments of insight are crucial to the research process, methodology texts rarely make suggestions about how researchers might place themselves in the right mindset for insight. The reticence of applied researchers to write candidly about how research questions develop reinforces a “kind of immaculate conception fantasy of how the research question evolves” as if “research questions fall from the night sky, wrapped in little bundles dropped by storks” (Pachirat 2017, 84). The moment of insight in research is effervescent, hard to describe, and difficult to recreate on demand. Yet researchers need tools to create moments of insight; to

“cultivate the kinds of sensibilities that make original and exciting research questions more likely” (Pachirat 2017, 84-85). Mixed-epistemology research cultivates sensibilities for facilitating these moments of insight.

*There is little methodological guidance about how to defend mixed epistemology research, so applied researchers struggle to be transparent about the role of mixing epistemologies in their research.* We find that while the works we analyzed were exemplary in many dimensions of research transparency, all of them struggled to some extent with articulating their commitment to mixed-epistemology research and how integral it was to their insights, theory development, design, data collection, and analysis. In Pachirat’s evocative phrasing, mixed-epistemology work sometimes reads as if the “project has swallowed not only an invisibility potion, but also an amnesiac potion” (Pachirat 2017, 84). While this lack of transparency serves, in part, to circumvent the challenge of satisfying gate-keepers from both traditions simultaneously, we urge greater transparency.

*There is no cookbook.* Our examples show that mixed-epistemology research involves switching between learning-by-doing and learning-by-observing in the course of ones’ research. It also apparently entails moving between aesthetic sensibilities (Hay 2002, 223), prioritizing play, chance, anarchy, process, and multivocality at some points in the research and alternatively privileging purpose, design, hierarchy, finality, and univocality at other points. Beyond these fundamental commonalities, emergent norms have not coalesced around a typology, spectrum, or ideal sequence of combinations, and we are hesitant to foist something that restrictive on researchers just yet, especially because interpretivism privileges improvisation (Schwartz-Shea 2006, 84; Yanow 2006, 67-89). Scholars seeking to do mixed-epistemology research might, for the moment, absorb the standards, approaches, and aesthetics of each tradition from sources such as Gerring (2012), Schwartz-Shea and Yanow (2012), and Pachirat (2017), and then draw inspiration for combinations Roth and Mehta (2002, 159-16), applied examples, and their own sense of wonder.

## **5 The Stakes of Combining Interpretivism and Positivism**

The two reasons most often given against combining are philosophical inconsistency and practical impossibility and we have addressed each. But a third reason lurks behind these concerns: that combining them might be bad politics, within the social sciences or beyond. To

address concerns about the stakes of combining methodologies, we need to surface the *political* tensions between the ontologies that guide interpretivism and positivism. A positivist tends to view the research world as objectively real and knowable through external observation, while from an interpretivist view, the researcher cannot separate herself from the research world, and knowledge must therefore be co-created by the researcher and the subjects of research. From an interpretive perspective, then, the idea that a researcher can stand “outside” the research world is, from the outset, a misrepresentation or *mystification*. Positivism, on this reading, involves acting “as if” the world is objectively observable, but then failing (refusing?) to acknowledge this act. We can understand interpretivism as a project of *demystification* against positivism—by foregrounding how the researcher and research world intersubjectively create knowledge, interpretivism renders visible a hidden misrepresentation that underpins positivist research.

One reading of our argument, then, is that we encourage positivists to *demystify* certain components of their research (by describing the interpretive insight that sparked a theory, for instance, or conducting an interpretive investigation of explanatory categories), before *re-mystifying* those components for positivist examination—by proceeding to test that theory against an external world, for instance, or by treating those explanatory categories as real and observable variables. According to this reading, we put demystification in the service of more mystification, and subvert the interpretive project of demystification with interpretivists’ own tools.

An alternative reading, however, and the one we intend, is that our recommendations aim for research that is self-aware and explicit about the mystification that occurs in positivist research. This research puts investigations of meaning in the service of positivist explanatory purchase, but also seeks to make visible the role of the researcher (i.e., her position and interpretations) in the development of those explanations. This research does not discard the authority to conduct causal investigations, but neither does it conceal the means of its production.

Indeed, what we have in mind are combinations of interpretivism and positivism that perform a kind of “double move”—i.e., that assume the authority to observe and verify causal relations in the positivist investigation, while using the interpretive investigation to foreground

the production of that authority.<sup>8</sup> This “double move,” then, does not put interpretive demystification in the service of positivist mystification. Rather, we aim to expand the explanatory purchase of our positivist research, while also acknowledging the mystification required to assume authority over the investigation of causal relations. This, we think, is a task for which interpretivism and positivism can work side by side.

## 6 Conclusion

In this paper, we have argued that harnessing the combined power of interpretivism and positivism is beneficial for some research investigations. Positivists have specialized in investigations of cause, while interpretivists have specialized in investigations of meaning. When scholars find themselves investigating both causation and meaning, one obvious option is to draw on the strengths of positivism and interpretivism. Despite calls for “analytic eclecticism,” an “ethnographic sensibility”, and other approaches to subverting this specialization, combinations of interpretivism and positivism remain widely discouraged. These methodologies rest on incommensurate worldviews, we are told, and their combination entails irreconcilable ontological and epistemological contradictions.

By showing how positivists might incorporate interpretivism into their work, we suggest a way to cut through these warnings, and illuminate possibilities for mixed-epistemology research. We argue, in particular, that interpretivism can answer the questions of meaning that arise in positivist research. For scholars who, like ourselves, wish to see the world simultaneously through radically different lenses, we have clarified a philosophical and practical basis for doing so.

We have also addressed what we suspect may be a common reaction to our paper: that combining epistemologies has political stakes.<sup>9</sup> Rather than using the demystifying power

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<sup>8</sup> We borrow the term “double move” from the feminist scholar Donna Haraway’s conception of writing (what she calls, “cyborg writing”). This writing resists “the kind of masterful “I,” a particular kind of authority position that makes the viewer forget the apparatus of the production of that authority,” and foregrounds “the apparatus of the production of its own authority, even while it’s doing it” (Olsen 1996, 5; Haraway 1985 [1991]).

<sup>9</sup> “An attempt... to bridge the two research traditions...will probably please no one. But in part this is because the two “sides” have become hung up on differences over the epistemological status of social

interpretivism in the service of positivist ends (or, as an interpretivist might put it, to enable more mystification), we argue for combinations of interpretivism and positivism that engage in a kind of “double move”—i.e., that claim authority over causal relations, while making visible the production of that authority.

We hope that scholars will pick up our ideas and run with them. Along the way, we expect both modest refinements and radical improvements to mixed-epistemology research. We think research practice – trial and error – is the key to progress on interpretive and positive combinations. In this, we follow a long interpretivist tradition of learning by doing. As scholars who are drawn to both methodologies feel permission to pursue combinations, they will likely pioneer creative new approaches that we cannot yet imagine.

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science. The state of the social sciences, and, in particular, of international relations, is such that epistemological prescriptions and conclusions are at best premature” (Wendt 1999, 425).

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