

ANALYTICAL ESSAY

Coping with Complexity: Toward Epistemological Pluralism in Climate–Conflict Scholarship

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Over the last two decades, climate security has become an increasingly salient policy agenda in international fora. Yet, despite a large body of research, the empirical links between climate-change and conflict remain highly uncertain. This paper contends that uncertainty around climate–conflict links should be understood as characteristic of complex social–ecological systems rather than a problem that can be fully resolved. Rather than striving to eliminate uncertainty, we suggest that researchers need to learn to cope with it. To this end, this article advances a set of principles for guiding scholarly practice when investigating a complex phenomenon: recognizing epistemological uncertainty, embracing epistemological diversity, and practicing humility and dialogue across difference. Taken together we call this ethos epistemological pluralism, whereby scholars self-consciously recognize the limits of their chosen epistemology for understanding the climate–conflict nexus and engage with other approaches without attempting to usurp them. Reviewing the last decade of climate–conflict scholarship, we show that climate–conflict research already manifests many of these ideals; however, we also identify problematic patterns of engagement across epistemological divides and thus plenty of scope for improvement. To illustrate why a diversity of methods (e.g., qualitative and quantitative) will not suffice, the article critically discusses prior research to illustrate why at least two epistemological approaches—constructivism and positivism—cannot be synthesized or integrated without significant analytical cost, and elaborates why excluding insights from any one would lead to an impoverished understanding of the climate–conflict nexus. We conclude with five practical recommendations of how scholars can help realize the ideal of epistemological pluralism in practice.

En las dos últimas décadas, la seguridad climática se ha convertido en una agenda política cada vez más destacada en los foros internacionales. Sin embargo, a pesar de existir un gran número de investigaciones, los vínculos empíricos entre el cambio climático y los conflictos siguen siendo muy inciertos. Este artículo sostiene que la incertidumbre en torno a los vínculos entre el clima y los conflictos debe entenderse como una característica de los sistemas socioecológicos complejos, más que como un problema que pueda resolverse por completo. En lugar de esforzarnos por eliminar la incertidumbre, sugerimos que los investigadores deben aprender a lidiar con ella. En este sentido, este artículo propone una serie de principios para guiar la práctica de la investigación de fenómenos complejos: reconocer la incertidumbre epistemológica, aceptar la diversidad

epistemológica y practicar la humildad y el diálogo a través de las diferencias. En conjunto, llamamos a este *ethos* pluralismo epistemológico, por el que los investigadores reconocen conscientemente los límites de su epistemología elegida para entender el nexo del conflicto climático y se comprometen con otros enfoques sin intentar usurparlos. A través de la revisión de estudios de la última década sobre conflictos climáticos, mostramos que la investigación sobre conflictos climáticos ya manifiesta muchos de estos ideales, aunque también identificamos patrones problemáticos de compromiso a través de las divisiones epistemológicas y, por tanto, un amplio margen de mejora. Para ilustrar por qué una diversidad de métodos (por ejemplo, cualitativos y cuantitativos) no será suficiente, el artículo analiza críticamente investigaciones anteriores para ilustrar por qué al menos dos de los enfoques epistemológicos—el constructivismo y el positivismo—no pueden sintetizarse o integrarse sin un coste analítico significativo, y explica por qué excluir las ideas de cualquiera de ellos llevaría a una comprensión deficiente del nexo entre el clima y los conflictos. Concluimos con cinco recomendaciones prácticas sobre cómo los académicos pueden ayudar a materializar el ideal del pluralismo epistemológico en la práctica.

Au cours des deux dernières décennies, la sécurité climatique est devenue un sujet majeur dans les débats politiques internationaux. Toutefois, malgré un vaste corpus de recherche, les liens empiriques entre changement climatique et conflits restent particulièrement flous. Cet article affirme qu'une telle incertitude concernant les liens entre climat et conflit doit être appréhendée comme une propriété de systèmes socio-écologiques complexes, plutôt que comme un problème susceptible d'être résolu. Ainsi, plutôt que de nous efforcer d'éliminer cette incertitude, nous suggérons à la communauté des chercheur-euses d'apprendre à l'intégrer dans son travail. Dans cette perspective, le présent article propose une série de principes destinés à orienter la pratique universitaire dès lors qu'il s'agit d'analyser des phénomènes complexes, à savoir : accepter l'incertitude épistémologique, promouvoir la diversité épistémologique, faire preuve d'humilité et pratiquer un dialogue incluant la différence. Nous appelons « pluralisme épistémologique » cette approche par le biais de laquelle des chercheur-euses reconnaissent d'eux/elles-mêmes les limites de leur propre épistémologie pour comprendre l'articulation entre climat et conflit, et adoptent d'autres points de vue sans les usurper. En examinant la littérature portant sur le climat et les conflits de la dernière décennie, nous montrons que la recherche sur ce sujet exprime déjà une grande partie des idéaux contenus dans l'approche que nous défendons. Néanmoins, nous identifions également des manières problématiques d'aborder les fossés épistémologiques et, par conséquent, une grande marge d'amélioration. Afin de démontrer pourquoi une diversité des méthodes (ex : qualitative et quantitative) n'est pas suffisante, l'article analyse de manière critique des travaux antérieurs et explique pourquoi il n'est pas possible de synthétiser et intégrer au moins deux approches épistémologiques (ici constructivisme et positivisme) sans efforts analytiques considérables. Il développe également la raison pour laquelle l'exclusion des connaissances apportées par l'une ou l'autre de ces approches compromettrait la compréhension de l'articulation entre climat et conflit. Nous concluons avec cinq recommandations pratiques destinées à aider les chercheur-euses à mettre en œuvre l'idéal que constitue le pluralisme épistémologique.

Keywords: epistemological pluralism, complexity theory, climate–conflict

Palabras clave: pluralismo epistemológico, teoría de la complejidad, conflicto climático

Mots clés: pluralisme épistémologique, théorie de la complexité, climat et conflit

Introduction

The last decade has witnessed a rapid growth in scholarship exploring relationships between climate change and conflict (von Uexkull and Buhaug 2021) while climate-related security risks have become an increasingly salient issue for states and interstate organizations (Bremberg, Sonnsjö, and Mobjörk 2019; Conca 2019). Yet, despite more than one thousand peer-reviewed research items having been published (Sharifi, Simangan, and Kaneko 2021), policymakers will be disappointed if they expect a straight answer to the question: *Does climate change cause conflict?* The early proponents of scarcity-induced “climate wars” in the Sahel and Syria have been comprehensively questioned and arguably debunked.¹ Meanwhile, the high-profile claims of Berkeley economists (Burke et al. 2009; Hsiang Burke, and Miguel 2013) linking climate change to conflict across time and space prompted a series of sharp rebuttals from the field’s leading political scientists (Buhaug 2010; Buhaug et al. 2015). The recent IPCC (2022, 53) summary of the links between climate and conflict is couched in uncertainty, noting that “there is insufficient evidence at present to attribute armed conflict to human-induced climate change.” Complicating matters further, a series of strident critiques have called into question the entire enterprise of quantitative climate–conflict research (Selby 2014; Scartozzi 2021; see also Simangan, 2020). For instance, following a systematic review, Cesare Scartozzi declared climate–conflict scholarship “degenerative” and doomed unless it leaves behind its “19th century” assumptions and down its positivist toolkit (Scartozzi 2021, 719).

While the policymaker may feel exasperated surveying the field, this article suggests that amid all this heat light is emerging. If we accept that the crudest climate change-causes-conflict theories are now dead in the water,² it becomes apparent that the state of the art is increasingly in *meta*-agreement about the nature of the relationship between climate change and conflict, namely that it is highly contextual, socially mediated, and above all *complex* (see Mach et al. 2020; von Uexkull and Buhaug 2021, 5). Crucially, these conclusions are no longer limited to political ecologists, who have long mounted a battle against what they have deemed “deterministic” and unidirectional climate–conflict research (Abrahams and Carr 2017), but are now supported by leading quantitative scholars too. For instance, in a recent expert commentary in *Earth’s Future*, fourteen leading climate–conflict scholars noted that major “uncertainties” about the climate–conflict linkages are “unlikely to be eliminated or even reduced” given that some “system interactions are unknowably complex.” They go on to note the need to move beyond “large-scale, continent-wide analyses to within-country multidisciplinary evaluations” (Mach et al. 2020, 4). Indeed, quantitative scholars now routinely call for “better specification of contexts and intermediary linkages” (e.g., Rosvold 2021, 473) and other methodological refinements designed to reckon with complex social phenomena.

While it may sound counterintuitive to the policymaker, we argue that this recognition of the inherent complexity, and thus unpredictability, of the climate–conflict nexus provides a firmer basis for both knowledge production and policymaking. In short, once one recognizes that both social and ecological systems are ontologically complex, and therefore the futility of trying to identify universal law-like relationships within and among them, it enables scholars and policymakers to con-

¹ See Benjaminsen (2008, 2016), Benjaminsen et al. (2012), Selby et al. (2017), and Selby (2019).

² The empirical, methodological, and theoretical case against their claims strikes us as overwhelming (see Buhaug et al. 2014).

centrate their energies on how best to cope with this reality (see [de Coning 2018](#)). This article takes this emerging consensus as its starting point: if we accept that climate–conflict relationships are inherently complex and thus never fully knowable, what does this mean for the future of climate–conflict research and practice? To answer this question, we draw upon insights from complexity theory (e.g., [Cilliers 1998](#); [Meadows 1999](#); [Mitchell 2009](#)). While climate–conflict research has increasingly acknowledged the complexity of its object of analysis and calls for modeling complexity have become commonplace (e.g., [Scartozzi 2018, 2021](#)), it has not yet systematically utilized complexity theory to inform its meta-theoretical assumptions about how climate–conflict research should be conducted. In doing so, this article cross-fertilizes this scholarship with insights from earlier and ongoing meta-theoretical debates in the social sciences.

To begin, the first section contends that complexity theory provides the ontological basis for an explicitly *epistemological* pluralism characterized by humility, diversity, and dialogue. Embracing the principles of epistemological pluralism—rather than merely pluralism of method—we argue offers the best guard against tunnel vision when making sense of a complex phenomenon. The second section explores this risk of epistemological monism as it stands in climate–conflict scholarship. We show that while the field is currently characterized by a degree of epistemological pluralism, many scholars' *mode of engagement* across epistemological divides undermines the potential for productive dialogue and that many scholars' *visions for the future* of climate–conflict research threaten epistemological pluralism. Rather than relying upon an abstract case for epistemological pluralism, the third section makes the substantive case for why the climate–conflict research agenda needs *at least* two epistemologically distinct approaches to climate–conflict—constructivism and positivism. In short (but elaborated below), we differentiate between scholarship that systematically seeks to *understand* and interpret climate–conflict relationships and scholarship that seeks to *explain* by utilizing methodologies inspired by natural science (see [Hollis and Smith 1990](#); [Moses and Knutsen 2016](#); [Theisen 2017](#)). Without both traditions, we argue, climate–conflict research will risk both overstating and understating the relationship between climate and conflict. Hence, our article is quite different from those of climate–conflict scholars who reject outright positivism and seek to replace it with their preferred approach (e.g., [Selby 2014](#); [Scartozzi 2021](#)). We pick these two epistemological traditions because we only have limited space, they are the most established, and because there is existing climate–conflict research to use to illustrate their added value. Our argument would also support promoting other distinct epistemologies, *provided* they have systematic rules governing knowledge production that can discern good research from bad. Indeed, the argument for epistemological pluralism is not an argument against standards, but an argument against universalizing and thus homogenizing standards across incompatible epistemological approaches. We conclude by outlining five practical recommendations for how climate–conflict scholars can move toward realizing the ideals of epistemological pluralism in practice.

Before we proceed with the argument, some clarifying remarks are in order. First, we do not claim to have invented the broad idea that epistemological pluralism is a normative ideal worth striving for; rather, this article should be understood as an attempt to translate insights from earlier and ongoing meta-theoretical debates in social science into a concrete field of research (climate–conflict research). That said, we depart from and extend these broader calls for pluralism by developing a specific warrant for epistemological pluralism grounded in complexity science and by fleshing out what we see as its key principles.³ We reason and hope that this warrant for pluralism provides the best chance of convincing not only climate–

³Jackson (2010, 54) uses the relative lack of proven success of positivism to justify pluralism; meanwhile, Levine and McCourt (2018) argue that it guards against reification. We believe that both are valid in general and for

conflict scholars, but also social scientists of all stripes, who, despite many disagreements, increasingly recognize the complexity of their subject matter. Finally, we should clarify why we call it *epistemological* rather than say *methodological* or even *ontological* pluralism. In short, although each of these could be justifiable—it is the ontological and methodological⁴ implications of constructivism and positivism that are responsible for narrowing the gaze of the scholar—we think that these ultimately stem from the conceptions of *what scientific knowledge is*, and thus we consider it to be most intuitive to name the pluralism we endorse as “epistemological.”

Why Ontological Complexity Warrants Epistemological Pluralism

This section draws upon the insights from complexity theory to make the case for what we call *epistemological pluralism* within climate–conflict scholarship. The use of complexity theory is growing in political science and international relations (IR) (Rosenau 1990; Jervis 1997). It has had a faster uptake in related fields such as development studies (Rihani 2002; Ramalingam 2013), and it is increasingly applied to international conflict resolution and peace and conflict studies (Hendrick 2009; de Coning 2020; Hunt 2020). Apart from those that consciously make use of complexity theory, many others have been influenced by the premises and insights derived from the study of complexity. These influences can be traced by the contagion throughout the social sciences of many of the key concepts of complexity such as feedback, bifurcations, self-organization, and emergence (Byrne 1998; de Coning 2020). Within the field of climate–conflict and related research fields, scholars have begun to recognize the complexity of their subject matter and there have been growing calls to apply concepts from complexity theory within climate–conflict scholarship (e.g., Scartozzi 2018, 2021; Hermans and Ide 2019, 43).

However, our goal with this paper is somewhat different from calling for the empirical application of complexity science. While we also ground our argument in complexity theory, we aim to draw out its meta-theoretical implications for the practice of climate–conflict research as a whole rather than using it to shed light on any specific empirical case. We thus answer the question: if we accept the notion that climate–conflict relationships are ontologically complex, what sort of research principles and practices does complexity theory imply would best enable scientific knowledge production? To answer this question, we begin by discussing the difference between complex and complicated phenomenon; we then draw out four meta-theoretical implications for the conduct of climate–conflict research that we suggest would help climate–conflict research cope with the inherent complexity of its subject matter.

Complex or Complicated?

Social and ecological systems, both separately and combined, are particular types of holistic systems that have the ability to adapt, and that demonstrates emergent properties, including self-organizing behavior. Such systems emerge and are maintained as a result of the dynamic and nonlinear interactions of their elements, based on the information available to them locally, as a result of their interaction with their environment, as well as from the modulated feedback they receive from the other elements in the system (Cilliers 1998, 3). The multifaceted nature of climate change, combined with the highly dynamic and continuously evolving nature of the societies it affects, and the ability of these societies to reflexively adapt in response in ways that often contribute to compounding and cascading effects on their security,

climate–conflict, but we consider complexity science likely to prove most compelling to climate–conflict scholars given their increasing recognition of the complexity of their subject matter.

⁴ Further, although methodology has a broader meaning (e.g., Jackson 2010; Moses and Knutsen 2016), we know that in practice many consider methodology as just a fancy way of saying methods.

make climate–conflict relationships a textbook example of a complex phenomenon (Preiser et al. 2018).

One way to highlight the unique characteristics of complex systems is to contrast them with complicated systems. A complicated system can potentially be fully understood and predicted, provided sufficient information is available. Designing, building, and launching a rocket into space is highly complicated, but once it is mastered, the same process can be repeated with a reasonable degree of certainty and predictability. In contrast, nonlinearity plays a critical role in the emergence and self-regulation of complex adaptive systems (Preiser et al. 2018). As social–ecological systems are highly dynamic, nonlinear, and emergent, complexity theory contends that it not possible to find general laws that will help us predict with certainty how a particular society or community will react to changes in the environment (Cilliers 2002). What factors produce an outcome in one context cannot be assumed to operate in another context, nor in the same context later; in fact, small changes may produce wildly different outcomes (Scartozzi 2021, 714–716). This is why complexity theory suggests that our ability to fully understand complex systems is inherently limited and what Mach et al. (2020) meant when they argued that some aspects of the climate–conflict relationship are simply “unknowably complex.” Hence, Barnett recommends that scholars of a complex phenomenon cultivate “a spirit of epistemological uncertainty” (quoted in Benner, Mergenthaler, and Rotmann 2011, 225). Taking these insights from complexity science as our starting point, we will now discuss their implications for the way we research climate–conflict linkages.

Implications of Complexity Theory for Climate–Conflict Research

First of all, complexity theory explains why climate–conflict research cannot definitely answer the “does climate change cause conflict?” question. In fact, it clarifies why, in the context of complex phenomena, the search for timeless or decontextualized answers is quixotic. Coleman (2004, 226) notes that one contribution of a complex-systems approach is “that it shifts our understanding away from static, simplified views of conflict” and helps us to appreciate the “complex, multilevel, dynamic, and cyclical nature of these phenomena.” From a complexity theory perspective, given the emergent behavior of complex systems, even if one independent variable seems to explain some aspect of system behavior today, this provides no guarantee that it will continue to have the same effect in future, let alone in another context altogether (de Coning 2016). One epistemological implication of treating socioecological systems as complex is not that we must understand the whole system, but rather that we critically deal with the fact that we never do (Biggs et al. 2021, 40). Thus, complexity theory cautions against either seeking or relying upon universal knowledge claims and highlights the temporal specificity of contextual knowledge. Instead, knowledge production and policy interventions in complex social–ecological systems will need to involve continuous and iterative cyclical processes of analysis, experimentation, feedback, and adaptation (de Coning 2018). As we noted above, this epistemological *humility* is broadly consistent with recent climate–conflict scholarship (see below) and the suggestions of recent high-profile reviews of the literature (e.g., Mach et al. 2020; von Uexkull and Buhaug 2021).

The second implication for climate–conflict scholarship is that the uncertainty inherent in knowledge about complex phenomena warrants valuing *diversity* in knowledge production techniques. Indeed, Cilliers (1998, 23) makes the point that when dealing with complex phenomena, no single approach “will yield the whole truth.” The logic here is that because scholars can never fully know what they do not know, they need to maximize the chances of capturing as much of the complexity as they can by avoiding narrowing their research perspective a priori by sticking to one approach. This is why Cilliers (1998) calls for trans-disciplinary interaction and

argues that utilizing models from different disciplines broadens the evidence available in pursuit of understanding a specific problem. Such a pluralistic approach allows for different avenues of advance, different viewpoints, novel ideas, and ultimately a better if still imperfect understanding of a complex phenomenon (Cilliers 1998, 23). Here, the likelihood that diversity may produce contradictory findings is a feature rather than a bug; it ensures that scholars and policymakers are reminded of the limited nature of their knowledge and thus encourages epistemic reflexivity rather than complacency (Levine and McCourt 2018). Counterintuitively, it is precisely because complexity theory insists we cannot know the limits of our own knowledge that it recommends *against* scholars *always* modeling climate–conflict relationships using a complexity theory–based empirical and conceptual toolkit. Instead, complexity theory implies that scholars embrace plurality of approaches that could include but should certainly not be limited to modeling complexity, while reminding scholars to remain humble about the epistemological status of their findings. It is worth noting that this implication of uncertainty and the need for humility are overlooked by Scartozzi (2021) and Selby (2014) when they use the complexity of climate–conflict relationships to suggest scholars do away with positivist quantitative methods.

This value placed upon diversity of knowledge production techniques partly aligns with climate–conflict scholars’ existing intuition that complex subject matter requires diversity of approaches (e.g., Mach et al. 2020; Von Uexkull and Buhaug 2021). However, as Levine and McCourt (2018, 93) have noted, endorsements of scientific pluralism among social scientists often mask “profound differences as to what pluralism means and the proper basis on which it should be defended.” Some see pluralism as a stepping stone toward the eventual unity of the sciences (Levine and McCourt 2018). Meanwhile, in climate–conflict scholarship, some envision a pluralism of methods rather than epistemologies (e.g., Ide 2017; Van Baalen and Mobjörk 2018). We think that these calls are moves in the right direction but by largely eschewing epistemological reflection, they do not fully protect against the risk of tunnel vision. The danger here is that without due attention, the diversity established—for instance, of quantitative and qualitative methods—may hide an epistemological monoculture: a commitment to a single set of standards that limit a priori the ontological gaze of the scholar and thus the knowledge produced (Smith 1997; Lapid 2003; Jackson 2010). In other words, the rules that define what counts as valid knowledge in any given “epistemology . . . determine what we take to be the ‘reality’ to be explained” (Smith 1997, 331). Hence, the third implication we draw from complexity theory is that if we accept that the climate–conflict nexus is complex, then it warrants not just a pluralism of method, theory, or discipline, but an explicit commitment to *epistemological* pluralism.

Fourth, complexity theory reminds us that making the most of *epistemological* pluralism in practice involves more than just ensuring that a *plurality* of epistemologies coexist but self-conscious efforts at dialogue and collaboration across divides (Biggs et al. 2021, 41). Indeed, it requires that scholars from different epistemologies engage in a constructive dialogue that recognizes not only both the substantial differences between them, but also that the other approach has its distinct legitimate value (Lapid 2003). Crucially, this implies recognizing and communicating *across* difference rather than striving to eliminate difference via grand plans to “unify,” “integrate,” or “synthesize” alternative approaches (Lapid 2003). Such dialogue also has important instrumental advantages in terms of cross-pollination: while no neutral test exists for knowledge gathered by different epistemologies (Lapid 2003; Jackson 2010), knowledge from one can certainly *inspire* new insights and be *translated* into the other, rendering it recognizable but not the same. Yet, in order for this to occur, it depends upon each side recognizing the others’ existence as well as understanding the value of diverse perspectives when trying to understand complex phenomena. Finally, we should emphasize that we do not endorse a “flabby

pluralism,” whereby anything goes (Lapid 2003, 130). Instead, bad research can be weeded out through the application of its own epistemological and methodological rules (Jackson 2010). However, this requires each epistemology to have its own distinct research space within the field without being subsumed under the other side’s epistemological criteria and goals (see Schwartz-Shea 2015).

To sum up, we have drawn four interconnected implications from complexity theory for the field of climate–conflict research. First, recognition of complexity implies an unavoidable uncertainty around knowledge and in particular the possibility of finding universally valid knowledge across time and space. Second, complexity theory suggests that the best way to cope with the uncertainty is by self-consciously fostering a *diversity* of approaches to avoid unduly narrowing the researcher’s perspectives a priori. Third, if we accept that the climate–conflict nexus is complex, then it warrants not just a pluralism of method, theory, or discipline, but an explicit commitment to *epistemological* pluralism. Fourth, practicing epistemological pluralism requires self-conscious efforts to conduct *dialogue across difference* such that cross-fertilization can take place without integration or usurpation. Taken together, we suggest that complexity theory provides the grounds to strive for an explicitly epistemological pluralism characterized by humility and dialogue. The following section attempts to diagnose whether and to what extent the pluralism practiced by climate–conflict researchers reflects these ideals and identify where and how it falls short.

Does Climate–Conflict Scholarship Practice Epistemological Pluralism?

Climate–conflict scholarship has its roots in environmental security research agenda that emerged in the 1990s as the field of security studies broadened its horizons in the aftermath of the Cold War (see Dalby 2020; Scartozzi 2021). This empirical research agenda—centered around Toronto and Zurich—explored how “environmental scarcity,” generated by over-demand, undersupply, and unequal distribution of renewable resources, could lead to “violent conflict” (see Homer Dixon 1994; also see Baechler 1998). While the early movers in this debate were criticized for instrumentalizing the concept of security in order to draw political attention to environmental issues (Deudney 1990; Levy 1995), the empirical research agenda that eventually emerged provided an empirically rich and diverse set of case studies that seemed to demonstrate the multifaceted and often indirect links between environmental scarcity and internal (but not international) conflict. It is important to note that although this agenda is retrospectively often referred to as “neo-Malthusian” and determinist, the protagonists were quite clear that their cumulative findings suggest that scarcity was not a direct cause of conflict. For instance, Baechler (1998, 32) made clear in his summary of the Zurich groups findings that whether conflict broke out “depends upon socio-political factors” rather than “the degree of environmental degradation as such.”

We need not discuss the ins and outs of this agenda, except to note that it prompted a heated methodological debate in the 1990s that bears a striking resemblance to the one climate–conflict scholars are having today. Perhaps the most famous exchange involved a back-and-forth between the leading figures in the Toronto school of environmental security, Homer Dixon and colleagues on the one hand and the environmental security skeptics, Marc Levy and Nils Petter Gleditsch, on the other hand.⁵ This debate had several points of contention but by their own account the core of their dispute concerned the methodology. Environmental security skeptics suggested that absent systematic attempts at comparison, the case

⁵ See Homer-Dixon and Levy (1995), Levy (1995), Gleditsch (1998), and Schwartz, Deligiannis, and Homer-Dixon (2000).

studies amounted to little more than “anecdotal” evidence for environment–conflict links (Levy 1995, 56; Gleditsch 1998, 391). Against this, the Toronto school contended that zooming in on likely cases and identifying causal pathways via process tracing was a crucial *first step* to get the research agenda moving *before* proceeding to “more rigorous testing” with “quasi-experimental methodology” (Homer-Dixon and Levy 1995, 194). Hence, while the method of process tracing can certainly be used by constructivist research (Guzzini 2014, 338), the environmental security debate addressed the relative value and its correct place within a broadly *positivist* research agenda: one explicitly concerned with identifying systematic covariation between putatively independent and dependent variables (Homer-Dixon and Levy 1995, 194).

Yet, in fending off demands for following what they call the “straitjacket” of methodological “orthodoxy,” at times the debate also foreshadowed our argument here. A major criticism leveled at the Toronto school was that their models were too complicated to test empirically using statistical methods (Levy 1995, 58; Gleditsch 1998, 381). While this argument was intended as a critique of Toronto, it could also be read as recognition of the limitation of positivist research when it comes to analyzing and explaining a complex empirical phenomenon. Indeed, concluding their rebuttal to Gleditsch (1998), leading members of the Toronto school (Schwartz, Deligiannis, and Homer-Dixon 2000, 83) reflected that “environment–conflict research brings us face to face with some of the most intractable issues in the philosophy of science, specifically whether causal generalizations describing the social world have the same status as those describing the natural world.” They went on to reason that because their subject matter is “fundamentally complex - characterized by huge numbers of components, causal interactions, feedback loops, and nonlinearity - environment–conflict researchers can gain insights from complexity theory.” This article can thus be read as an effort to follow this advice by drawing out the implications of complexity theory for the climate–conflict scholarship that would emerge from the environmental security agenda in the first decade of the twenty-first century.

Indeed, following a lull in attention during the early stages of the “global war on terror,” environmental scholarship received a renewed attention in the wake of hurricane Katrina, which to many highlighted the potential for climate change to induce human crises (Dalby 2020). Similar to the environmental security debate in the 1990s, the idea that climate change would generate conflict provoked bold claims among policy-actors that went out ahead of the empirical evidence. As Betsy Hartmann (2010, 234–35) notes, the UN Environmental Programme and UN Secretary General were rather hasty in attributing the violence in Darfur to climate change. Meanwhile, several high-profile think tanks around the same time began warning of climate wars and refugee crises that lay ahead should the world not take immediate action to mitigate global warming (Hartmann 2010). Mirroring the unfolding of the environmental security debate of the 1990s, these claims spurred more systematic scholarly efforts at identifying the potential links between climate change and conflict. Indeed, in an influential review for the World Bank, Buhaug, Gleditsch, and Theisen (2008, 95) called for a “multidisciplinary” research agenda to address the dearth of peer-reviewed research and assess the evidentiary basis for what they feared was “alarmism” of the nongovernmental organization (NGO) and policy world. With the publication of a seventeen-article strong, and well-cited, special issue in the *Journal of Peace Research* (Gleditsch 2012), it is fair to say that they got their wish. It is this post-2009 research agenda that emerged that we focus on here and evaluate in terms of the epistemological pluralism framework outlined above.

The Climate–Conflict Research Agenda

A cursory reading of any review of the climate–conflict literature can confirm that the research agenda is *pluralist* to the extent that it counts multiple approaches under its umbrella that disagree profoundly about the nature of the climate–conflict nexus and how to study it. Yet, as we saw, just observing a plurality of approaches tells us little about how pluralism is practiced (Levine and McCourt 2018). This section empirically examines the quality of pluralism practiced in climate–conflict research by surveying the citation practices of the field as a whole and mapping the different research clusters before qualitatively examining the mode of engagement between clusters and exploring the visions of the future of research.

The majority of the reviews of climate–conflict research over the course of the last decade identify some kind of schism dividing researchers and thus imply a pluralism of some kind.⁶ The most common divide noted by reviews is between qualitative and quantitative methods (e.g., Busby 2018; Van Baalen and Mobjörk 2018), and it has become a routine refrain from climate–conflict scholars to “integrate” insights from each (Solow 2013; Ide 2017; Van Balan and Mobjörk 2018). To be sure, a pluralism counting qualitative and quantitative research is better than a hegemony of either. However, the terms “quantitative” and “qualitative” have no specific epistemological referent (Jackson 2010; Yanow and Schwarz-Shea 2015); therefore, organizing pluralism in climate–conflict research around these poles would offer *no* protection against an epistemological monoculture developing (see below). Indeed, although quantitative research tends to be positivist, it need not be (Jackson 2010; Moses and Knutsen 2016); meanwhile, *qualitative positive* research is increasingly practiced and endorsed by climate–conflict scholars (e.g., Ide et al. 2020; Busby 2022).

A deeper, explicitly epistemological notion of pluralism is sometimes identified by climate–conflict scholars. Ole Magnus Theisen (2017) identifies two broad camps of climate–conflict research, what he calls the “naturalist” and “constructivist” traditions, which differ in their ontology, methodology, and epistemology (Theisen 2017, 211). As Theisen explains, climate–conflict has long been dominated by quantitative “naturalist” (or, we prefer “positivist”) agenda, which “assumes there are systematic regularities in the social world, and that these can be objectively sensed, recorded, and accumulated to build knowledge using tools that allow for generalizations.” Theisen contrasts this camp to the smaller, “constructionist” branch of climate–conflict research, which “holds that the patterns in the social world” result from context-specific meaning at the “personal or at a discursive level, reflecting norms and the dominant view on a topic at any time” and hence constructivists tend to favor “contingent, context-dependent, and in-depth idiographic knowledge over generalizations” (Theisen 2017). For the sake of brevity, we use Theisen’s (2017) broad definition here, which also aligns with Moses and Knutsen’s broad definition (Moses and Knutsen 2016, 9). However, it is important to acknowledge that within this umbrella term, there is a diversity of approaches that have both a distinct philosophical warrant and a proven track record producing useful systematic knowledge.⁷ It is also important to note that constructivism is a contested concept within IR and some major variants in IR can be reconciled with some variants of positivism. For instance, some consider constructivism to be about identifying systematic covariation between ideational and social factors rather than so-called material factors (Jackson 2010, 43, 202–203). Ide et al.’s (2020) pathbreaking work combining event coincidence analysis, qualitative comparative analysis, and case studies to explore when, why, and how climate-related disasters produce conflict provides the most sophisticated example of this kind of research within climate–conflict scholar-

⁶ See Buhaug and Von Uexkull (2021) for a review of these reviews

⁷ For instance, Jackson (2010) provides monograph-length explanation of why the fundamental ontological differences among what he calls Critical Realism, Analyticism, and Reflectivism mean that Neopositivism rules of knowledge production do not apply.

ship. However, because of its goal of uncovering systematic covariation and its use of case studies to test a priori models (grievance versus greed), by our definition it counts as positivist. To be clear, we do not think that there is anything wrong with this approach and certainly not mixed methods,⁸ but it is not what we refer to here as constructivist.

While Theisen's categories elide the many variants and hybrids of positivism⁹ and constructivism,¹⁰ unlike qualitative–quantitative, these categories capture substantial ontological and epistemological divides that cannot be fully overcome nor synthesized. We have no intent of taking sides in what some scholars refer to as a centuries-long social-science-wide “paradigm war” (Von Ingen 2021) between these positivists and constructivists. In our view, the endurance of both camps speaks to the distinct value of each approach for understanding the social phenomenon.

Instead, we use these categories to map the degree and quality of *epistemological* pluralism within climate–conflict research. In order to generate a big picture view of the extent to which positivist and constructivist research coexist and engage one another, we first conducted a bibliographic analysis to map the field. We used Scartozzi's (2021) bibliographic dataset of 640 climate–conflict research items and coded those published after 2009 as either positivist or constructivist.¹¹ We used Theisen's definitions to guide the coding. In order for something to become coded as positivist (blue) when it set out to generalize and/or use statistical inference, those that produced idiographic knowledge we coded constructivists (orange). This means that our coding had a relatively low bar for constructivism and a high bar for positivism. As a result, the estimation of positivist dominance is likely conservative. Our coding method is no doubt imperfect, but we consider it sufficient to generate a broad-brush snapshot of the epistemological makeup of the field. Moreover, it allowed us to identify two major works that have crossed epistemological boundaries.

Visualizing the climate–conflict citation network (2009–2019), with nodes scaled according to the number of citations, highlights how positivist scholarship dwarfs the constructivist scholarship in terms of a brute number of articles and number of citations. To be sure, some scattered non-positivist research clusters exist, but they are very much asymmetrical in terms of the number of citations. Nonetheless, it provides an empirical basis for the claim that there is what Levine and McCourt (2018) call a “plurality” of research, even if it suggests an imbalance of impact and perhaps influence. Second, in terms of intra- and intercamp engagement measured by co-citation, positivists engage more with positivists; meanwhile, constructivists are less inclined to cluster together, emerging as islands somewhat disconnected from each other as well as positivists.

While this bibliographic network analysis offers a useful snapshot of the field, mapping citations can only take us so far in assessing the *quality of the pluralism* practiced and envisioned by climate–conflict research. While positivism is dominant, there are significant clusters of constructivist scholarship (figure 2). Moreover, it could be quite possible for a limited number of citations to be doing a significant job in cross-pollinating research agendas. Indeed, the centrality of Selby (2014) and Benjaminsen et al. (2012) within the positivist core (figure 1) suggests that this might be the case (a possibility we explore below). Moreover, bibliographic analysis

⁸ In fact, we would argue that this sophisticated mixed methods approach to climate–conflict is optimal for positivist research; our point is that it should not held up as *the* gold standard for non-positivist research to aspire to.

⁹ For instance, King, Keohane, and Verba (1994, 59) make an important distinction between deterministic positivists, who hold that “given the right explanatory variables, the world is entirely predictable,” and humbler probabilistic positivists, who hold that “random variation” in “social and political worlds and can never be eliminated”; therefore, “even if all variables were measured without error and “included every conceivable explanatory variable,” analyses would still never generate perfect explanations and predictions.

¹⁰ Covariation between putatively independent and dependent variables.

¹¹ *Note.* We coded reviews, policy papers, and normative theory as neither positivist nor constructivist. Further, we deleted book reviews, errata, and corrigendum from the dataset. This left us with 260 articles, books, and chapters.

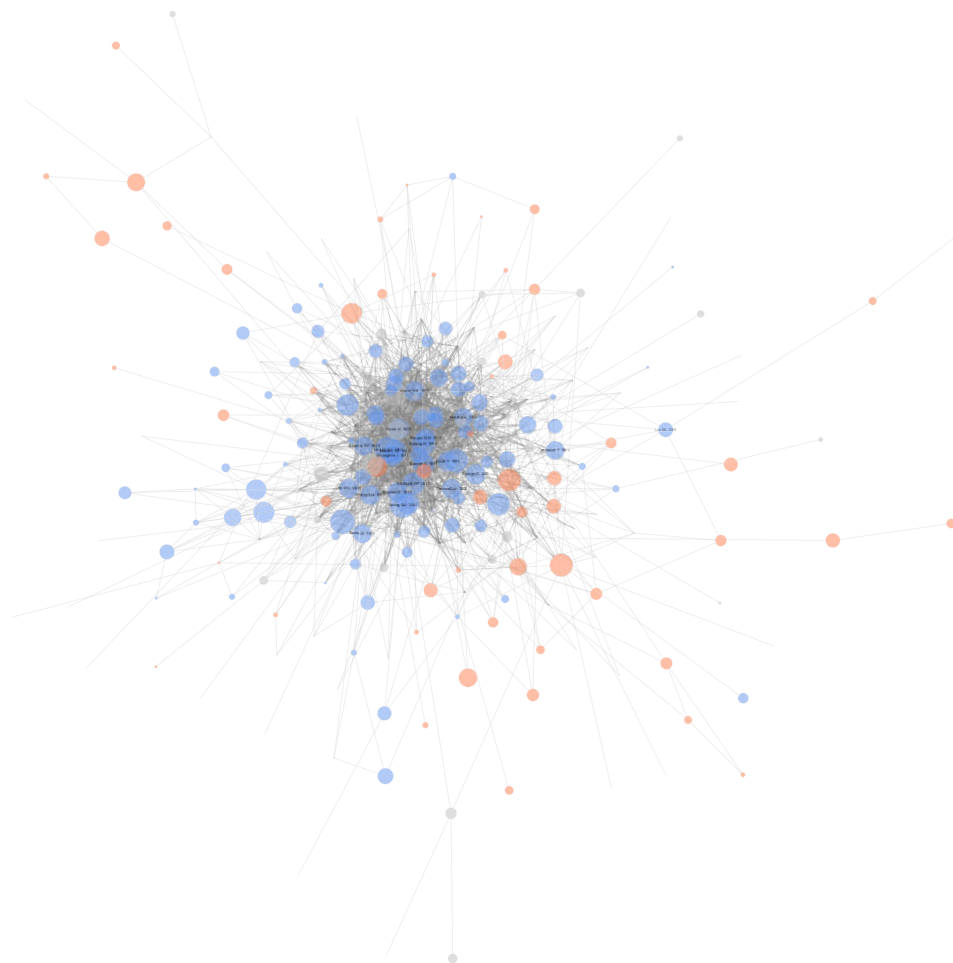


Figure 1. Climate–conflict citation network 2009–2019 (top twenty most cited positivists labeled).

cannot shed light on how these different camps understand pluralism nor the quality of pluralism they endorse. The next section thus zooms in upon the networks' constituent parts to examine the *quality* of the engagement.

Climate–Conflict Scholarship's Imperfect Epistemic Pluralism

First of all, it is important to emphasize that climate–conflict research does manifest significant and productive intra- and inter-epistemological engagement. Within the positivist camp, a robust debate has ensued between the political scientists centered around the Peace Research Institute in Oslo¹² (PRIO) and economists from Berkeley.¹³ With a now widely cited article on the influence of climate change on conflict, [Hsiang, Burke, and Marshall \(2013\)](#) sparked a substantive debate among positivist quantitative climate and conflict scholars concerning the methodology and the con-

¹² This group's network stretches quite far beyond, but PRIO is host to the most members who constitute this research cluster—identifiable by co-citation patterns ([Scartozzi 2021](#)) as well as its most cited member (Halvard Buhaug).

¹³ Two (Hsiang and Miguel) of the three leading members of this research cluster are based at Berkeley, while Marshall is based at Stanford.

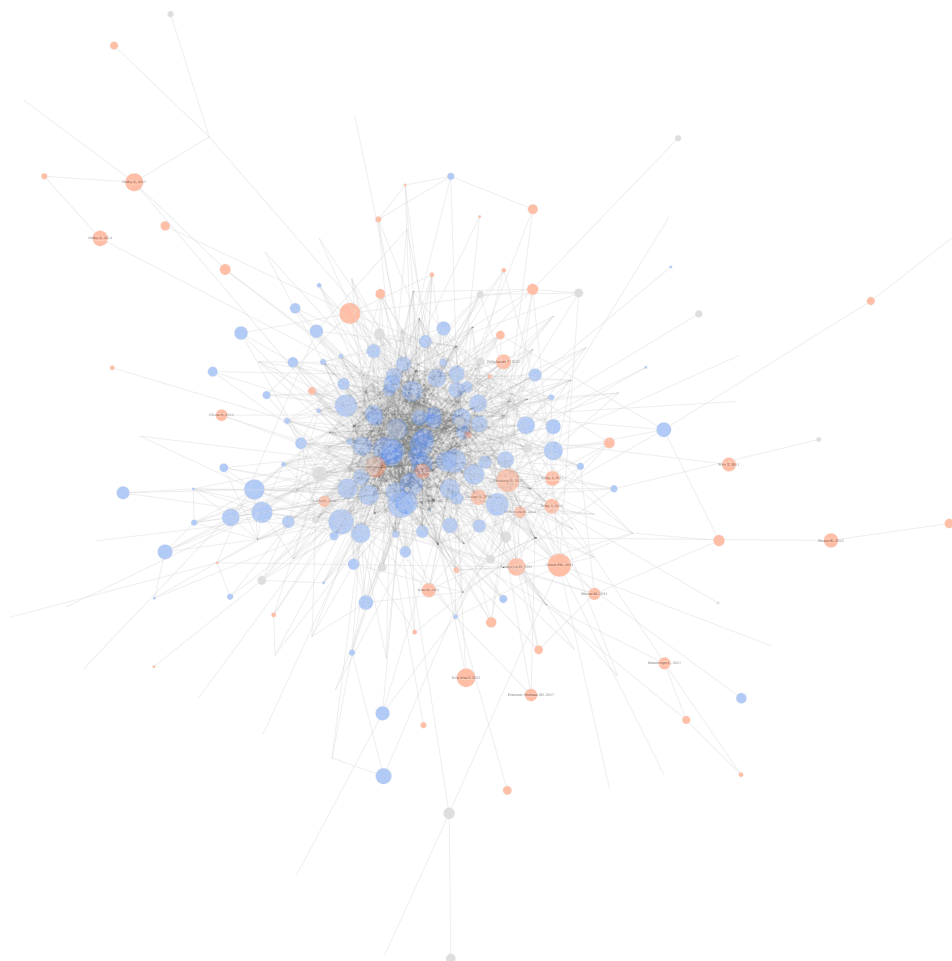


Figure 2. Climate–conflict citation network 2009–2019 (top twenty most cited constructivists labeled).

clusions that could be drawn from their analysis. After several articles “back-and-forth,” Hsiang and Burke’s (2014) meta-review resulted in twenty-six researchers joining a response refusing to accept the “attempt to quantify the ‘true’ climate effect on conflict,” concluding that “aggregating and generalizing results from selected studies serve no larger purpose unless the sample constitutes a representative subset of all relevant scientific research” (Buhaug et al. 2014).¹⁴ In layman’s terms, Buhaug et al. (2014, 395) explained the problem as “a bit like averaging the prices of apples and oranges, sampled in different locations and time periods, at different scales, using different metrics in the absence of a strong basis to expect that they are driven by the same factor(s). The exercise is mathematically feasible but the outcome may have no relevant meaning.”

In parallel, a series of idiographic case studies have explored and contested the causal role of climate change and drought in the Syrian War (e.g., Gleick 2014; Kelley et al. 2015; Selby et al. 2017; Selby 2019; Daoudy 2020). Further, there has also been important inter-camp engagement across epistemological divides. For instance, tracing the coauthorship and co-citation patterns within Tor Benjamin’s research—one of the two constructivists¹⁵ located at the center of the pos-

¹⁴For a more thorough recap of the debate, see Theisen (2017).

itivist network—shows how his work has long cross-pollinated with the positivists based at or associated with PRIO. Going by recent climate–conflict reviews, one significant result of this intra- and inter-camp engagement has been to move the dominant strands of positivist research toward more contextualized analyses of climate–conflict relationships, while fatally undermining the most reductive “climate war” narratives (Mach et al. 2020; von Uexkull and Buhaug 2021). In rejecting global correlations and singular theories of climate–conflict and by pursuing a research agenda focused upon exploring multicausal and contextualized understandings, we would agree with von Uexkull and Buhaug (2021) that this does indeed reflect a “decade a progress” albeit at the meta-theoretical level: recognition of the complex nature of climate–conflict relationships and the scholarly challenge ahead in trying to making sense of it.

Positivist Scholarship

Nonetheless, reviewing the inter-camp engagement does reveal some problematic tendencies that impede inter-camp engagement and threaten epistemological pluralism from flourishing in the future. Beginning with the positivist camp, there is a tendency to judge constructivist scholarship by positivist epistemological standards with the result that it is found to be of lower value. For instance, Berkeley economists Hsiang and Burke, who are the most prominent of the positivist climate–conflict researchers by citations, refer to qualitative research as “anecdotal” (Hsiang, Meng, and Cane 2011, 438). Similarly, in a recent review of reviews, von Uexkull and Buhaug (2021) conflate quantitative research with empirical research.

Here, we take stock of scientific progress by benchmarking the latest decade of empirical research against seven core research priorities collectively emphasized in 35 recent literature review ... These reviews were selected as they concentrate on the positivist, empirical, and predominantly quantitative climate–conflict literature ... We do not consider review studies focusing exclusively on qualitative, case-based research. (von Uexkull and Buhaug 2021, 4)

No further explanation is provided for excluding reviews of case study research. Elsewhere, Busby’s (2018, 341) review of literature writes that “qualitative work in this space has not methodologically *matured* alongside the quantitative literature. Most of the qualitative work in this space consists of single case studies (. . .).” In other words, single case studies are not mature, *because* they are single cases. He then suggests that in order to mature, they would need to conduct careful paired comparisons. Embodied within Busby’s methodological suggestion is an ontological-cum-epistemological assumption that climate–conflict knowledge must take the form of cross-case covariation and thus accept *his* understanding of positivist epistemological goals.¹⁶

These are just notable examples of a broader pattern whereby positivist climate–conflict scholars reflect and reproduce what amounts to an a priori positivist hierarchy of science that valorizes nomothetic knowledge over idiographic (see Moses and Knutsen 2016, 133–34). In other words, some leading climate–conflict scholars reproduce a positivist meta-assumption that the “key distinguishing ark of *scientific*¹⁷ research is the goal of making inferences that go beyond particular observations collected” (King, Keohane, and Verba 1994, 8). Non-general claims,

¹⁵Tor Benjaminsen himself (private communication) identifies as a Critical Realist as he understands constructivism in a narrower sense than we conceive of it here: where critical realism is understood as one potential set of ontological commitments consistent with constructivist knowledge production and that does not take systematic covariation between variables to be the (only) goal of “science.”

¹⁶Many positivists restrict themselves to explaining change within the same case, rather than attempting to validate by cross-case comparisons.

¹⁷As Jackson (2010, chapter 1) discusses how the term “science” is wielded by positivists as if they had a monopoly upon what counts as scientific knowledge, eliding the extensive and ongoing disagreement in philosophy of science.

from this perspective, are just “summaries of historical detail” (King, Keohane, and Verba 1994, 34) or mere hypotheses awaiting validation by intra- or inter-case comparison (King, Keohane, and Verba 1994, 38–40). As decades of scholars have pointed out, this notion of science elides the ongoing disagreement among philosophers of science, ignores the fallibility of positivist methods of validation, and delegitimizes knowledge that does not take the form of systematic covariation (Jackson 2010, chapter 1).

Yet, this narrow and contested notion of science underpins many high-profile calls for diversity of method in climate–conflict scholarship. Hence, it is common for scholars calling for “integration” of methods to implicitly demand qualitative scholars to submit to positivist epistemological goals. For instance, writing in *Nature*, Andrew Solomon (2013, 180), while ostensibly aiming for dialogue, not only suggests that each side should “talk more to — and less about — each other,” but also maintains that “the goal of both should be to develop statistical models that better reflect the real drivers of civil conflict.” Solomon thus offers a textbook example of “methodological imperialism” under the “guise of tolerance” (Jackson 2010, 209). Elsewhere, Von Baalen and Mobjörk (2018) provide a more sophisticated example of how positivist scholars’ plan to “integrate” qualitative and quantitative methods in practice often means usurping constructivist knowledge production under positivist goals:

Turning to future qualitative research, we believe that three considerations are *imperative* to probe the mechanisms. First, there is a need for more theoretically driven case studies that *explicitly formulate hypotheses* about the process itself and strive to interrogate the mechanisms through sound causal inference. The majority of the qualitative studies in our sample are idiographic case studies that, while valuable in generating context-dependent knowledge, *struggle to generalize* their findings to a broader population of cases. Intensive studies of single instances of climate-related violent conflict can produce *valid generalizations* if adequate attention is given to case selection or process-tracing. (van Baalen and Mobjörk 2018, 571)

As well meaning as these suggestions may be, the authors do not consider how the knowledge produced by constructivist case studies is likely to be compromised should the research be designed with the primary goal of generating nomothetic knowledge. First, “sound causal inference” here implies identifying systematic covariation, which requires that the cause must operate in a similar fashion across space and/or time. Making this an “imperative” both precludes acknowledging case-specific or unique causes and assumes that scholars have the technological capacity to measure and standardize independent and dependent variables such that covariation can be successfully identified (see below). Second, “attention to case selection” again implies selecting cases based upon their value for generalizing rather than upon the intrinsic value of knowledge about a given case. To be sure, in *isolation*, using positivist criteria for case selection is significantly less limiting for constructivists than other methodological imperatives, particularly when the goal is to question a dominant positivist theory, but it is still necessarily a priori limiting to the sort of research undertaken. Whether or not one agrees that such advice to constructivists constitutes “synthesis via subsumption” (Hellmann 2003, 148), if such advice were heeded by constructivists, it would prestructure and thus limit climate–conflict knowledge production. This example neatly illustrates why a plurality of qualitative and quantitative scholarship would offer no protection against epistemological monism.

Constructivist Scholarship

Given that constructivist research tends to embrace epistemologically skepticism in the abstract (Moses and Knutsen 2016; Theisen 2017), we might expect a humbler

style of engagement. However, our analysis of non-positivist works also indicates patterns of engagement that manifest analogous misunderstandings that likely undermine inter-camp engagement and thus epistemological pluralism. First, constructivist scholars frequently label climate conflict research as “neo-Malthusian” and “deterministic.” Yet, as Tobias Ide and Jurgen Sheffran (2014, 268) highlight, the label Malthusian is misleading because “the large majority of studies supporting climate conflict perspectives do not make use of deterministic arguments.” Instead, as Theisen (2017) notes, “the deterministic climate-conflict model is much more present in parts of the gray and popular literature.” Second, non-positivist critiques often rely on a narrow understanding of what phenomenon and methods positivist epistemology is compatible with. For instance, Scartozzi (2021) implies that positivism necessitates “linear vision” and risks imposing “reductionist explanations upon complex multi-causal phenomena.” For Scartozzi (2021, 719), leaving behind positivism would allow the climate–conflict research agenda to refocus on “qualitative understanding of the mechanisms and feedbacks that couple conflict with climate change.” Yet, as we saw above, positivism is quite compatible with generating a qualitative understanding of causal mechanisms, while positivists can and do use nonlinear models. Third, these caricatured understandings of positivism lead non-positivists jump hastily into declaring that positivism and particularly quantitative methods serve *no useful* purpose for understanding the climate–conflict nexus (e.g., Selby 2014) or that positivist research agenda has “reached its natural end” (Scartozzi 2021, 719).

These problems are scattered among constructivist scholarship, but the clearest example is Selby’s (2014), whose conclusion manages to make all three errs in one paragraph:

There is no good reason to believe that there exists a *single* “underlying mechanism” linking climate and conflict. Correlations between climatic and conflict variables *do not by themselves provide evidence of a causal association*, and do not provide a solid basis for prediction. In any case, quantifications of climate-conflict links are of dubious value, since they inevitably rest upon coding and modelling premises that are arbitrary and sometimes even untenable ... The progressivist ideal of a research programme well on its way to producing clear actionable evidence – once more numbers have been crunched – is a dangerous chimera. And there is still less reason to accept any analogy between epidemiology and climate conflict research – an analogy which rests upon the positivist fallacy that *social relations are just as mechanistic as those that are the focus of the natural sciences*. In sum, positivist-quantitative methods *are ill-suited* to the challenge of analysing the climate-conflict nexus. (Selby, 2014, 848)

While each critique holds for *some* positivist quantitative climate–conflict scholarship, it certainly does not apply to all, hence rendering the conclusion far too general. First, positivist quantitative approaches do not require there to be just not one underlying mechanism linking climate and conflict. As Von Uexkell and Buhaug (2021, 7–9) note, the challenge animating the positivist research agenda currently is identifying the multiple pathways and what they self-consciously consider to be the narrow scope conditions for their operation. Second, while not all positivists are as careful about avoiding conflating correlation with causation (e.g., Hsiang and Burke 2014, 41–42), positivist philosophy of science embodies a long tradition of remaining skeptical about identifying causation (Hume 2000 [1748]). Third, the fact that quantitative positivist’s early bold hypotheses have been rejected or contested offers no firm guide to whether humbler hypotheses will not find firmer ground. When Selby uses this scholarship’s inability to find robust general relationships as a reason to argue that positivist methods are futile, he misunderstands that revising bold hypotheses is an everyday part of the positivist epistemology (King, Keohane, and Verba 1994). Fourth, positivist climate–conflict scholarship has critically reflected upon their early dubious coding choices (Gleditsch 2012), improved

robustness checks of their models (e.g., Ide et al. 2020), and generally made more explicit the limitations of their use of proxies: for instance, whether it really is climate change they are studying or merely weather variation (e.g., Busby 2022, chapter 8). Finally, a positivist approach can operate at different degrees of generalizability. To be sure, positivists may have the *aspiration* of discovering highly general covering laws, but this does not exhaust the potential utility of positivist methods to reveal patterns within narrowly defined temporal and spatial scope conditions. Indeed, this is the direction of travel the positivist climate–conflict agenda is going in (Mach et al. 2020; Buhaug and Uexkull 2021) and it would seem hubristic to claim to know in advance this outcome.

There are a host of examples of climate–conflict positivists conducting research that recognizes the epistemological limits of their methods for coping with complexity, and generally making humble knowledge claims consistent with the spirit of uncertainty. To take but one example, Petrova (2021) examines the relationship between climate-induced migration and social unrest in Bangladesh between 2010 and 2015. Combining multilevel data and incorporating georeferenced household surveys and fine-grained data on natural hazards and social unrest, Petrova (2021, 34) zooms in upon “a single case, Bangladesh, in order to unpack the complex relationship while considering the context of these local dynamics.” Notably for our purposes, she explicitly warns that “projecting these findings into the future is increasingly challenging” given “climate change impacts on political stability are still future conditional,” notes explicitly the limits of statistical inference for fully comprehending her object of analysis, and draws upon non-positivist research to identify possible lurking factors that cast doubt on her conclusions (Petrova 2021, 44–46). While constructivists could certainly pick holes the quantitative indicators used, whether or not the approach is perfect is not a bar any research can pass. Instead, what is crucial is that Petrova’s scholarship displays little of what Selby (2014, 848) terms the “positivist fallacy that social relations are just as mechanistic as those that are the focus of the natural sciences,” and, we would argue, illustrates how positivist quantitative science can be consistent with the humble ideal of epistemological pluralism. This attitude is well summarized by Mach et al. (2020, 3), who could scarcely be more explicit about the limits of generalizing climate–conflict relationships:

Identified relationships between climatic variability and increased instability may well hold over a given window of time or set of contexts, yet fail beyond that. It is important to understand such dynamics to shed light on the generalizability versus context specificity of different theories and results.

In other words, they are seeking out provisional, contingent regularities. Indeed, these pragmatic positivists clearly recognize that their object of analysis embodies complex, socially constructed and unknowable dynamics, but suggest that it may display sufficient stability that statistically significant relationships can be discerned. Conversely, this is in-line with a pragmatic understanding of complexity theory: the world does manifest statistical regularities, but they are unlikely to hold far across space, time, and social context. The fact that non-positivists tend not to recognize that positivist climate–conflict research can and does serve humbler goals is an ironic consequence of their tendency to reify positivist scholarship based upon the most extreme examples, while ignoring the diversity of positivist scholarship.

Indeed, a close reading of Selby (2014) shows that he provides a compelling argument for why striving to find a universal theory of climate–conflict—in Burke and Hsiang’s words, akin to “epidemiology”—is indeed likely futile but *cannot* sustain rejecting using positivist quantitative methods for more limited purposes. Thus, although Selby makes what some positivists admit are “important” critiques (Theisen 2017, fn 10), the “sweeping”—and unwarranted—nature of his conclusions makes them easy to dismiss (e.g., Buhaug 2015, 268). Ultimately, the conclusion that positivist methods need to be rejected *in toto* lends Selby’s argument a

zero-sum game quality that undermines and understates the potential for productive engagement. Needless to say, were Selby (2014)—and other high-profile calls for quantitative positivist climate–conflict scholarship to down tools (e.g., Scartozzi 2021)—to be heeded, it would threaten the field’s epistemological pluralism. However, even though these calls are unlikely to be heeded, the tendency of misrepresenting positivism and positivists no doubt hinders inter-camp dialogue.

Encouraging Epistemic Pluralism

The preceding analysis has shown how although there is a plurality of epistemologies (positivist and constructivist) within climate–conflict research, some of the practices of engagement across epistemological divides run counter to the goal of fostering an enduring and constructive epistemological pluralism. To be sure, the competition among those sharing an epistemology is healthy. For instance, the disagreement between PRIO- and Berkeley-linked positivists should not be incommensurable and thus infighting is a constructive check upon relativism. However, some modes of engagement are less constructive. These dynamics undermine knowledge production about the climate–conflict nexus by hindering productive engagement and seeking to limit in advance the types of questions, cases, and causes that climate–conflict scholars explore.

The Dangers of an Epistemological Monism: What Is Lost in Translation

This section makes a substantive epistemological case for not only why constructivism and positivism warrant their own *distinct* research space in climate–conflict research, but also how they can nonetheless complement each other in the context of climate–conflict research to a degree hitherto often underappreciated by protagonists from each camp. The goal is to engender an improved degree of epistemological reflexivity within climate–conflict scholarship that encourages epistemological pluralism. While climate–conflict scholars sometimes call for diversity, they seldom specify *what exactly* they have in mind or fully elaborate *why* specific approaches would prove useful to keep distinct. This is problematic because given the current power imbalance in climate–conflict research and IR, this risks a pluralism *within positivism*. The following section not only outlines and discusses some key meta-theoretical assumptions of positivist and constructivist sciences that account for their incommensurability but also uses concrete examples of research to illustrate each approach’s value-added to climate–conflict research.

The Dangers of Epistemological Monism

Without going too deep into philosophy of science,¹⁸ constructivists hold that positivists’ epistemological goal—identifying general systematic relationships—and the methodological tools—comparing cases to identify correlations between variables—necessarily have ontological implications liable to narrow the gaze of the scholar and thus produce myopic understanding of the social world *if* used in isolation (Smith 1997; Jackson 2010; see above). One does not need to believe that positivist epistemology is useless to agree that its standards limit knowledge production. First

¹⁸For a more thoroughgoing discussion of the philosophical disagreements, see Jackson (2010) and Smith, Booth, and Zalewski (1996). To take a couple of examples, Jackson (2010) notes how testing hypothesis presumes the mind independence of the world and limits valid knowledge to experience (phenomenalism), two positions that remain heavily contested within the philosophy of science. Meanwhile, the imperative to identify independent and dependent variables elides how social life can also be conceived of as formed through various kinds of social and material relations, rather than bounded units with properties that can be treated as distinct and measured (Jackson and Nexon 1999). The point here is not to take sides, but to identify how these ostensibly neutral methodological imperatives of positivism limit the way scholars see the world, prior to any analysis.

of all, to set the bar for knowledge at systematic covariation between variables presumes the methodological capacity to adequately measure and standardize the variables at play, something that even most positivists would agree is often lacking. More fundamentally, while most constructivists would probably agree that *some* aspects of the social world can be turned into discrete variables and correlated, they would suggest that much of the social life cannot (Abbott 1988, 176–77). Instead, when attempting to translate the specific into the general and comparable, positivists will not be able to adequately capture or need to omit the unquantifiable, multivocal, relational, emergent, unique, or nonlinear relationship that does not manifest as observable, systematic regularities across cases (see Selby 2014; Theisen 2017; Scartozzi 2021). These omissions can also work the other way: overlooking lurking contextual variables can lead positivists to mistake ephemeral or spurious patterns for general laws. As we will discuss below, climate–conflict scholarship counts several examples of constructivist scholarship that do precisely this: identify climate–conflict relationships that would be difficult to apprehend or validate with a positivist epistemology but are no less useful for it.

Crucially, quite besides the technical problem of generalizing, from a constructivist perspective, whether a factor identified in a case-specific analysis systematically covaries has *no* bearing on the validity or epistemological status of the case study. Instead, constructivist explanations rely upon the *judgment* of the scholar, whose intimate familiarity with their object of analysis enables them to develop plausible causal explanations for the singular case (Jackson 2010).¹⁹ Whether or not one finds similar dynamics in the future or in other cases is immaterial to the validity of the explanation at hand because constructivists do not expect discourses²⁰ to result in systematic regularities and reject the idea that disparate, complex social–historical processes can adequately be standardized and compared. Such constructivist explanations are certainly open to refutation, but by other careful scholarly analyses of *the same case*.²¹ As Selby et al. (2017; and Selby 2019) illustrates, it is quite possible to evaluate and refute a case study without more cases; one does not need to use comparative positivist logic to systematically assess causality or validity (as for instance, Busby [2018, 383] suggests). To be sure, one could attempt to turn the factors identified in a constructivist case study into climate–conflict into independent variables that one could attempt to correlate with some kind of conflict outcome, but whether or not one found systematic covariation over time or across cases would have no bearing on the original studies’ epistemological status (Jackson 2010, 110,152).

What Is at Stake?

To move beyond meta-theory, it is useful to illustrate by way of concrete examples why subsuming constructivist research under positivist epistemology would im-

¹⁹ If this sounds problematic, it is worth noting that even King et al. (1994, 91–94) concede that utilizing positivist statistical inference outside of laboratory rests upon extremely bold assumptions about the similarity of cases (“unit homogeneity”) and the “constant causal effects,” and that “no matter how perfect the research design what they call the “fundamental problem of causal inference” can never be fully overcome and only “partial attempts [made] to avoid it” (King et al. 1994, 73). To emphasize this is not to suggest that statistical modes of inference are useless, but that they also come with major shortcomings we would suggest mean that they cannot be assumed to be a priori superior to expert judgment based upon deep familiarity with one’s case (see Jackson 2010).

²⁰ Or other concepts describing multivocal or complex phenomenon.

²¹ To be clear, constructivists’ judgment here is not subjective but orientated around a large and ever-growing body of methodological principles for conducting systematic constructivist and/or non-positivist scientific inquiry (see Ackerly, Stern, and True 2006, Hansen 2006; Jackson 2010; Moses and Knutsen 2016; Yanow and Schwartz-Shea 2015). Schwartz-Shea (2015) is particularly clear on why positivist evaluative criteria (e.g., internal and external validity) cannot simply be used by constructivists (or interpretivist scholars) because they inevitably embody assumptions about the objects under analysis and the goals of research that constructivists would not subscribe to. See also Audrey Alejandro’s “trilogy” of articles (Alejandro 2021, 2022; Alejandro and Knott 2022) seeking to systematize reflexivity in research practice.

poverish climate–conflict research. While a great deal of constructivist scholarship is orthogonal positivist climate–conflict research, it neither disputes nor supports their empirical claims but can be used to problematize the political consequences of climate security research and policy practice (e.g. McDonald 2021; Maertens 2021; Oels 2013; Daoudy et al., 2022)—the *Political Ecology* strand of constructivist climate–conflict research operates on the same empirical plain as most positivist climate–conflict scholarship (for review, see Abraham and Carr 2017). Many of these works explicitly explore whether the underlying mechanism embodied by theories of climate–conflict are manifested in practice and whether or not the outcome can be explained by factors overlooked by positivist scholarship. For instance, Tor Benjaminsen has undertaken extensive ethnographic research exploring the relationship between climate-related scarcity and conflict in the Sahel (Benjaminsen 2008, 2016; Benjaminsen et al. 2012, 2021), which is a region commonly covered by large-*N* positivist climate–conflict research (e.g., van Uexkull 2014; Bell and Keys 2018). In his case study on the farmer–pastoralist conflicts in Mali, he identified three confounding historical–political processes that better accounted for the conflicts than the drought-induced scarcity theory promoted in the policy world and by some quantitative positivist scholarship:

These factors include first agricultural encroachment on productive key resources for pastoralism and on livestock corridors, obstructing the necessary mobility of herders and animals. This trend is primarily caused by agricultural policies and laws promoting farming at the expense of pastoralism. Second, decentralization from the early 1990s caused a political vacuum that led rural actors to follow opportunistic strategies to claim ownership of land and natural resources. Third, rent-seeking among government officials has undermined rural people’s trust in government institutions and the willingness and interest of officials to solve conflicts. This lack of trust may have contributed to some actors taking action on their own, including using violence to lay claim to resources. (Benjaminsen 2016, 110)

Several features are worth noting about this explanation. First of all, unencumbered with positivist goals of generalizing beyond the case at hand, Benjaminsen could identify complex interrelated factors that would likely be missed or distorted had he followed even a qualitative positivist case study design: marginalization of pastoralists, decentralization, and especially trust would prove hazardous to render as measurable independent variables, not least because these processes are interwoven. Indeed, it is the “causal complex” *in combination* that is adduced to produce the outcome, not the cumulative effect of putatively independent variables (Jackson 2010, 110). Yet in order for this to occur it depends upon each side recognizing the others’ existence as well as recognizing the value of diverse perspectives when trying to understand complex phenomena (King, Keohane, and Verba 1994, 37–38), it would require comparing this spatially or temporally with another where one or more of these factors were absent, so he could hope to parse the relative impact of rent-seeking, trust-levels, or decentralizing processes. It is also worth noting that if these factors did not sufficiently vary within the case or if no suitable case could be found to compare it to, then a positivist has no means of producing what they consider scientifically valid knowledge about the phenomenon. Hence, the methodological demand of striving for systematic *covariance* limits what types of inquiry are possible, unless supplemented with other epistemologies. Thus, were a positivist epistemology hegemonic within climate–conflict it would prestructure Benjaminsen’s case study and thus limit the *knowledge production*, or were he to ignore these imperatives, it would undermine the *knowledge reception* by rendering his study as just a hypothesis awaiting validation via intra- or inter-case comparison.

To understand why Benjaminsen’s political–ecology–constructivist approach benefits from *not* following positivist research design principles, it is instructive to compare his analysis with Joshua Busby’s forthcoming book, which conducts an

explicitly *qualitative positivist* analysis of climate–conflict relationships. In short, Busby conducts a series of paired case studies to test his new general theory that (1) weak state capacity, (2) exclusive state institutions, and (3) blocked or evenly delivered aid make a state especially vulnerable to “negative security outcomes” when extreme weather events strike. In other words, his theory posits a general systematic relationship between climate events, intervening state-level variables, and security outcomes. To test his theory, Busby employs “Mill’s method of difference” to select his cases: identifying similar countries that “face very similar circumstances but produce different outcomes” (Busby 2021, 59). By this approach, Busby hopes to isolate and verify the independent effect of his theories’ key variables. Busby then proceeds to conduct careful process tracing of the qualitative evidence for this theory via three paired comparisons of six cases.

There is much to like about Busby’s book, which does bely the sort of attention to context and process that quantitative positivism usually lacks. Nonetheless, his commitment to positivist epistemology and research design comes at a major cost, ironically for reasons that the inventor of the “methods of difference,” John Stewart Mill, foresaw 150 years ago when he sternly warned against trying to use his method on social kinds because of the “inextricably interwoven” nature of the “plurality of causes” that exist in “politics and history” (Mills 1887, 334, cited in Tilly 1997, 44). To be concrete, Busby’s trouble is two-fold. He needs to find countries similar enough to be compared such that they vary on characteristics of importance for his theory but little else of significance. Meanwhile, he needs to find a plausible means of measuring and rendering comparable state capacity and exclusiveness. On both counts, Busby is forced into making questionable methodological choices: for instance, claiming that India and Bangladesh as well as Lebanon and Syria are sufficiently similar that he could plausibly employ the method of difference. Meanwhile, in order to render his variables comparable, he is forced to rely upon country performance indicators, which reduce state weakness or exclusivity to numerical values, thus flattening the complex and idiosyncratic ways in which states can prove “weak” and “exclusive.”²² Hence, rather than developing a context-sensitive conceptualization of historical–political processes that led to a negative security outcome, Busby is left working with generic concepts and measures designed for quite different purposes.

These methodological issues are not esoteric, and Busby (2021) is refreshingly transparent about both throughout the book (e.g., Busby 2021, 266–69). Yet, unfortunately for Busby, his chickens came home to roost even before his book was completed: two of the three countries that served as negative cases for his theory suffered “negative security outcomes” in between the time when Busby had finished his case studies and was finalizing his book (Busby 2021, 271). This does not do fatal damage to Busby’s theory—these states may have been weaker and more exclusionary than his measures indicated. However, with half of his negative cases turning positive, he ends up no closer to parsing the independent effect of his variables. Ultimately, his problems here were a direct consequence of his positivist epistemology: his need to claim that India and Bangladesh are “similar” on all but key variables and the need to use a country performance indicator at all were both direct consequences of the perceived imperative to privilege cross-case covariation over other modes of explanation and inference.²³

Yet, even if qualitative positivist case studies come with a significant analytical cost vis-à-vis constructivist case studies, this does not imply that their research cannot cross-fertilize one another. Again, political ecologists can serve as an example. Even

²² In particular, the state weakness indicator Busby uses has been specifically roundly critiqued; however, country performance indicators suffer from severe generic problems (see Beaumont and Towns 2021).

²³ Positivists do not all share Busby’s goal of cross-case covariation (some aim for intra-case temporal covariation). However, all strands of positivism demand systematic covariation between putatively independent and dependent variables, which produces analogous limitations upon research for reasons outlined above.

if political ecologists/constructivists reject positivist research design, they can still serve as a crucial check upon the bolder claims of large-*N* positivist scholarship, triangulating—often negatively—the general claims of positivists. In this sense, political ecology’s brand of constructivism is quite complementary to positivism. Falsifying bold hypotheses is part and parcel of the positivist epistemology whereby observed empirical relationships are treated as *always* provisional pending *indefinite* rounds of testing and revision. Hence, while political ecologists embody a distinctly non-positivist epistemology, this research agenda nonetheless manifests a constructive dialogical relationship with positivist and particularly large-*N* positivist research.

Finally, although political ecologists/constructivists eschew the search for general laws and systematic covariation across cases, nothing prevents positivists from *attempting* to abstract and quantify independent variables based upon political ecologist’s research and *attempting* to identify more general relationships. Indeed, by identifying the crucial role of institutions and the political economy of resource allocation in so-called climate wars, political ecologists’ (constructivist) critiques have given rise to a burgeoning line of positivist research attempting to generalize these “indirect” pathways between climate change and conflict (e.g., Ide et al. 2020; Busby 2021). Similarly, constructivists were the first movers to identify that those measures taken to adapt to climate change could themselves produce conflict; this in turn has given rise to an emerging positivist scholarship investigating this potentiality (e.g., Javeline 2014; Hegre et al. 2016; Mirumachi, Sawas, and Workman 2020; see also von Uexkull and Buhaug 2021, 8). These examples illustrate how even when a constructivist’s goal is to refute the correlation, it still serves a useful purpose for positivist climate–conflict scholarship. The danger however, as noted above, occurs when positivists conflate inability to generalize across time or space with the absence of systematic evidence: confusing positivist *epistemological* standards of evidence with the *ontological* presence of a relationship.

Symbiosis without Synthesis

With this final section and preceding discussion, we have aimed to show that although constructivist scholarship has tended to emphasize critiques of positivist climate–conflict research, it also has the potential to both provide a check upon overgeneralization (e.g., Hsiang and Burke) and help avoid systematic *underestimation* relationships between conflict and conflict. While the former mode of complementarity is the most obvious, the latter is also quite possible: constructivists can identify discourses and local institutions that are influential in accounting for a specific climate-related conflict but not amenable to standardization and comparison. In this way, constructivist research design can provide evidence of climate–conflict links that would be overlooked by positivists. However, we contend that constructivism can operate in a productive dialogical relationship with positivist research *but only so long* as positivists do not subsume constructivist work under a positivist research design or devalue context-specific knowledge a priori. In other words, this dynamic *depends* upon avoiding some of the more pathological practices of engagement outlined above: when positivists treat the absence of statistically significant regularities to be akin to the absence of empirical evidence or when single case studies are deemed necessarily “immature” unless they are designed according to positivist principles. To be clear, this does not mean that qualitative positivist research is not valuable: purposive sampling, small-*N* comparisons have huge value to positivist research agendas. Rather, it means avoiding assuming that *only* case studies designed according to positivist logic are valuable or valid.

Conversely, as symbiosis implies, this complementarity goes both ways. Constructivists benefit from engaging with positivist works in at least three ways that would not involve being subsumed under positivist goals to identify systematic covariation. First, positivists’ general models can be reconceptualized or translated into ideal

types whereby they are not tested against an independent reality (all *ideal* types by their nature can never pass such a test) but used as baseline against which to order and compare a messier reality and identify case-specific or complex processes that help us understand why and how the ideal *is not realized* (see Jackson 2010, 153–55). Indeed, without necessarily making it explicit, political ecologists have been tacitly undertaking such a research design when they identify the shortcomings of the climate-scarcity-conflict theory.²⁴ Second, once one accepts that quantitative methods have no necessary connection to positivism, it significantly opens up for sharing and collaborating on the descriptive data that quantitative positivists rely upon. For instance, Adler-Nissen, Eggeling, and Wangen (2022, 1–3) discuss two ways in which “big data” can be utilized by constructivists: (1) “making big data thick”—where the brute numbers provide the starting point for ethnographic interpretation of their meaning, and (2) “making thick data big,” whereby by researchers reflexively use difficulties encountered in coding processes in order to identify the frequency of ambiguous cases where meaning overwhelms the coding categories. Indeed, it is not the method of collection that makes something positivist, but what you do with it (see Moses and Knutsen 2016, 256–312, for a more extended discussion). Third, on a more practical level, we think that constructivist scholars could learn from positivists in terms of being more explicit about the epistemological and methodological principles underpinning their claims. Indeed, despite the welcome systematization of constructivist (and “post-post positivist”) methodological guides, within climate–conflict scholarship the methodological principles underpinning these works are seldom made as explicit as their positivist colleagues.

Conclusion

This article has drawn upon complexity theory to advance a set of normative principles for guiding scholarly practice when investigating a complex phenomenon: recognizing fundamental epistemological uncertainty, embracing epistemological diversity, and practicing humility and dialogue across difference. Taken together, we call this ethos *epistemological pluralism* (see also Lapid 2003). Indeed, our core argument is that a diversity of epistemologies offers the most productive means of producing knowledge about climate–conflict and that ensuring this epistemological pluralism should be a self-conscious goal for the field. While some may consider these principles self-evident, by reviewing current practices in climate–conflict research, we showed why these principles cannot be taken for granted and hence why our intervention here is warranted. Conversely, some may agree with our argument on principle, but suggest that the ideals of epistemological pluralism are utopian given the current institutional incentives in the field. This is not an unreasonable objection, but we would reply that ideals are useful to strive toward even if they cannot be fully realized: epistemological pluralism is not all or nothing but can be practiced to a greater or lesser extent. Currently, the field of climate–conflict reflects to some extent epistemological pluralism; however, as we saw, there is a great deal of scope for improvement. Recognizing the institutional constraints on the production of knowledge, we make five recommendations that require varying degrees of time and effort. Although we have in mind climate–conflict scholarship, these recommendations may also have more general applicability to all scholars studying what they self-identify as a complex phenomenon (e.g., civil war, peacebuilding, global environmental politics research).

The first and the easiest involves researchers taking care with how they refer to research undertaken by different epistemologies. As the second section documented, scholars from both sides of climate–conflict scholarship are guilty of using

²⁴ See Favell (2011, chapter 5) for one of the most methodologically explicit and compelling examples of ideal types being used in this manner.

disparaging terminology that at best misrepresents and at worst strawmans the other side. Breaking these habits would be a useful step toward communicating across difference and avoiding “losing” readers from the other camp at the outset. Second, we propose that climate–conflict scholars improve their epistemological reflexivity. This implies reading up on the epistemology and methodology of the other side in their own words rather than going by second-hand interpretations of it from one’s own camp, which is how we suspect the aforementioned strawmen get reproduced. This does not mean that scholars need *master* the other’s approach, but to learn enough to *understand* the principles animating the other side’s research sufficiently that they can appreciate and intellectually engage with the other side’s work.

The third and fourth recommendations concern increasing degrees of collaboration. The most straightforward is to solicit feedback on draft papers from those working in a different epistemology. This would not only reduce the risk of strawmanning, but also help foster reflexivity about the limits of the scholars’ chosen approach. Indeed, because scholars working in different epistemologies see the same world differently (Smith 1997), they can help the other see the rabbit when they had hitherto only appreciated the old lady. Beyond highlighting the limits of one’s own approach, such dialogue may also open up new questions and new problems for the other. Fourth, climate–conflict scholars would benefit from coauthoring those who ordinarily work in another epistemology and learning how to navigate the meta-theoretical differences when they inevitably arise. For instance, Tor Benjaminsen and Halvard Buhaug’s long-term collaboration reflects an excellent example of the advantages of inter-epistemological collaboration.²⁵ Coauthoring across epistemologies will prove challenging, but we wager that such learning by doing is the optimum strategy for fostering mutual understanding and facilitating cross-pollination across epistemological camps. Fifth, the most ambitious recommendation is for policy and research institutions to self-consciously foster epistemological pluralism by ensuring that their hiring decisions encourage epistemological pluralism.

Finally, our analysis has implications for how policy-relevant research is conducted. The competing truth claims among climate–conflict research over the past decade resulted in the fifth and sixth IPCC (2014, 772; 2022, 20–21) report finding that the overall result of the field’s research has been inconclusive. This lack of certainty is frequently cited by policymakers that want to stall decisive action, for example, by China, India, and Russia in a Security Council debate about climate security on December 10, 2021. Russia ultimately vetoed the passing of the resolution. Although there are geopolitical reasons for this impasse, the IPCC’s headline findings certainly do not help. Yet, if one accepts that the relationship between climate change and conflict is complex, then it suggests that the IPCC’s focus on the general question “does climate change cause conflict?” will never be answered once and for all. Thus, rather than chasing this chimera, complexity theory would suggest that the IPCC should instead refocus on more realistic goals: exploring how climate change and societal security is interrelated in specific contexts and thereby generating a taxonomy of pathways through which climate change may influence societal security. This taxonomy would then serve as lenses through which policymakers can reflect upon the specific challenges they face in specific contexts, and help them devise local strategies for adapting to and fostering resilience to the influence of climate change. To be clear, a great deal of climate–conflict scholarship is already contributing to this task²⁶; thus, we mention it here as an example of how the knowledge produced by climate–conflict can both recognize the complexity inherent in social–ecological systems and be policy relevant.

²⁵ For instance, Buhaug and Benjaminsen (2015) as well as Benjaminsen et al. (2012).

²⁶ See, for example, Tarif, Møbjork, and Krampe (2020).

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