

CONTRIBUTED PAPER

# Philosophy of the Field, in the Field: Philosophy of Science Association 2020/2021 Presidential Address

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## Abstract

Recent advocates of “field philosophy” make the case that philosophy “needs to get outside more often”; alongside disciplinary modes of practice we should cultivate philosophical work that is consequential in real-world, practical terms. This takes a number of different forms, many of them captured by a framework for analyzing engaged philosophy of science proposed by Plaisance and Elliott. I draw on three examples of field-engaged philosophy of science that address the legacies of settler-colonialism archaeology to illustrate the promise of field philosophy.

## 1. The brief for field philosophy

In *Socrates Tenured* (2016) Frodeman and Briggie make the case that our sequestered, disciplinary modes of practice need to be “complemented” by philosophical work that is, as they put it, “practically engaged, stakeholder-centered, and timely” (4). Rather than valorizing a studied dissociation from the messy world of practice—“keeping our hands clean,” thus prizing “ideal theory” (*sensu* Mills 2005) and the “genius contest” culture in which such theory thrives—they argue that the “philosopher’s hands were never clean”; “dirty hands’ should [be] understood as the native condition of philosophical thinking” (9–10). In this spirit they urge us all to recognize the value of doing “externally motivated” philosophy that is undertaken in direct engagement with nonphilosophers on problems that arise in the world and is addressed to those affected by them rather than a small circle of specialist colleagues: “Philosophy needs to get outside more often. The sunshine will do it good!” (24).

The two dozen contributions to Brister and Frodeman’s *Guide to Field Philosophy* (2020) showcase an enormous diversity of sites and problems, types of partners, and contexts and modes of engagement that constitute field philosophy. Many of these will be familiar to philosophers of science given initiatives that go back more than a decade. Fehr and Plaisance introduced socially relevant philosophy of science (SRPOS)

with a special issue of *Synthese* in 2010, establishing socially relevant philosophy of/in science and engineering (SRPoISE) as a research network in 2012, and Potochnik and Cartieri's 2013 call for socially engaged philosophy of science inspired the SEPOS working group at Michigan State University and the Center Public Engagement with Science at the University of Cincinnati that Potochnik codirects. It is no surprise that philosophy of science is well represented in the *Guide to Field Philosophy* with key examples drawn from fields as diverse as agricultural, environmental and climate science, engineering and medical research, robotics, and computing.

In some cases, philosophers provide technical support or service of various kinds: mediating, facilitating, translating, and functioning (aspirationally) as "honest brokers" (Brister and Frodman 2020, 13). This mode of field philosophy is exemplified by the ToolBox Dialogue Initiative based at Michigan State University and by other contributors whose aim is to build consensus about goals and norms of practice among stakeholders and coworkers. In other cases, philosophers play the role of gadfly, "shaking things up" in what Scheman describes as "transgressive" field philosophy (2020, 178). They disembed and critically scrutinize disciplinary aims, norms of practice, and assumptions, with the goal of ensuring that these don't harden into "fixed and final doctrine" (Wylie 2022, 263, quoting Sarton 1924). Field philosophy also includes projects in which philosophers of science provide decision support for researchers and policy makers who are navigating "wicked problems" that arise, for example, from global climate change and the challenges of developing strategies for local wildlife and biodiversity conservation (Sarkar 2020, 332). Sometimes they are members of research teams, directly involved in setting the research agenda and actively engaged in the team's work; Tuana describes this as embedded philosophy that often requires "coupled epistemic-ethical analysis" (2020, 146–49; 2023). I take it that this diversity is the point; if there is a common thread here it is an appreciation of "the poverty of armchair philosophy compared to what the discipline can be if guided by field experience," as Sarkar puts it in the conclusion to his contribution to the *Guide to Field Philosophy* (2020, 346).

Responding to this proliferation of "externally motivated," socially relevant and, to varying degrees, embedded modes of practice Plaisance and Elliott recently proposed a framework for "analyzing the variety of forms that engaged philosophy of science can take" (2021, 595) which is in some respects broader and in others more constrained than what has been characterized as "field philosophy." They focus on the nature of the engagement, conceptualized in terms of continua on two dimensions (601). The first dimension is social interaction which they describe as ranging from individual to collective modes of engagement. At one end of this continuum are studies of science practice that involve minimal direct interaction with practitioners, grading into more immersive and bidirectional modes of interaction in which, at the other, philosophers and their working partners codevelop research projects. The second dimension is epistemic integration that runs from intradisciplinary approaches characterized by limited, unidirectional flows of information in which the theory, concepts, methods, and data from one domain of inquiry or practice are used to clarify, critique, or facilitate the work of another, through to transformative forms of engagement in which the "imports are combined (fused, knit, mixed, etc)" in ways that change the epistemic and theoretical framework of both fields (602).

As History of Philosophy of Science (HOPOS) colleagues remind us, and as Plaisance and Elliott's discussion of "engaged philosophy of science" makes clear (2021), the various forms of philosophical practice assembled under the banner of "field philosophy" are by no means an entirely a new departure. But their growing prominence as an expansive and vigorous movement is, to my mind, among the most significant developments we have seen in our field in recent years.

As it happens, I have spent a great deal of time quite literally outside in the field, getting my hands dirty on archaeological excavations and surveys, sometimes in the sunshine and more recently in the Pacific Northwest drizzle. I have often been pressed to explain what exactly it is I do and why it should count as philosophy, so I greatly appreciate these thoughtful accounts of how others have been venturing out from our academic enclaves and productively getting their hands dirty. In what follows I discuss three different examples of field-engaged philosophy of science in archaeology. They illustrate types of engagement that are well captured, in broad strokes, by Plaisance and Elliott's framework. As they note, however, a more complete account will require attention to other aspects of engagement characteristic of field philosophy: the diverse aims and goals that animate the work of field philosophers, the range of partners involved, the barriers they navigate, and the impact and outcomes of their work. My aim here is to bring into sharper focus the ways these factors configure the modes of "social interaction" and genres of epistemic "mixing" that arise from doing field philosophy in a field science.

## 2. Philosophy in the field

By the time I began to work on philosophical questions raised in and by archaeology at the turn of the 1980s, philosophy was already "in the field." North American anthropological archaeology was in the throes of a revolution; the advocates of "processual" New Archaeology insisted that their field must make a decisive break with traditions of practice that had prioritized the recovery and systematization of archaeological data.<sup>1</sup> Their goals were anthropological: to explain the forms of life and historical trajectories of past cultures in terms of processes operating over the long term, at the level of cultural systems. To do this, they argued, it would be necessary to institute properly scientific modes of practice and, to operationalize this agenda, they invoked Hempelian covering law models of explanation and hypothetico-deductive models of confirmation. Archaeology was to be rigorously "problem-oriented," a practice of systematically testing "processual" hypotheses. Almost immediately, internal critics pointed out a number of ways in which these philosophical models were a bad fit for a field science like archaeology, and a few noted that philosophers had been raising questions about the adequacy of these models even with respect to canonically scientific fields like physics. In short, at the very time the New Archaeologists were championing Received View philosophy of science, as Suppe described it (1977), it was widely touted as meeting its demise.

Several philosophers of science were drawn into these archaeological debates. In a review of *Explanation in Archaeology* (Watson et al., 1971), Morgan took an aggressively

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<sup>1</sup> For a more detailed account these debates and their philosophical dimensions see Wylie (2002, 57–96) and Chapman and Wylie (2016: 18–31).

intradisciplinary stance, to invoke Plaisance and Elliott's terminology (2021, 600). Attempts to import philosophical theories and terms of art by the untutored were bound to run aground, he insisted (1973, 899); if there is to be responsible cross-disciplinary trade it must take the form of unidirectional exports managed by experts in the originating discipline. This was a form of interaction, to be sure, but one that foreclosed bidirectional engagement. It wasn't lost on the archaeologists who responded to Morgan that he hadn't learned enough about the problems that had motivated them to explore philosophy to have anything constructive to contribute (Watson et al., 1974); his review was an exercise in boundary policing that set up what several later commentators described as a profoundly counterproductive dynamic. In response many dismissed philosophy as elitist and irrelevant (Flannery 1982; Schiffer 1981). Other philosophers who commented more sympathetically on these exchanges—for example, Embree (1989) and Watson (1991)—nonetheless reinforced the conclusion that philosophers of science have little to offer practitioners. They argued that, although philosophers and archaeologists may seem to address similar questions about the nature and practice of science, their motivations and assumptions are so different that philosophical answers to these questions are nontransferrable.

In the same period Merrilee Salmon published two well-received articles in *American Antiquity*<sup>2</sup> that provided an overview of then-current philosophical theories of “confirmation” and “explanation” (1975, 1976). These were, in a sense, a translational exercise that arose from ongoing discussion with archaeologists at University of Arizona and, later, the University of Pittsburgh. I think of them as a diplomatic genre of Rortyeian “kibbutzing” (Wylie 2002, 106); Salmon knew by heart the “pros and cons” of the philosophical claims and clichés that were circulating in internal archaeological debate, but she took archaeologists' motivating concerns seriously. She engaged directly in what Kim described, in a different context, as “intra-paradigmatic inquiry ... concerning the conceptual, foundational, and regulative aspects of a given paradigm” that arise when a maturing field “turns self-reflective” (1980, 595). A decade later, in *Philosophy and Archaeology* (1982), Salmon developed an innovative philosophical account of archaeological inference drawing on theories of confirmation and explanation that were then being developed on the basis of careful rethinking of the Hempelian models to which New Archaeologists had originally appealed.

During this period, I was spending every summer in the field, mainly at Fort Walsh, a Northwest Mounted Police site in southwest Saskatchewan (Wylie 2017, 2022). The field archaeologists I worked with were not following the philosophical debates, but they were quick to identify problems with the programmatic positivism of the New Archaeologists that illustrated virtually all the major issues then being raised by critics of Received View philosophy of science. In particular, it was obvious to them that perception and interpretation are inescapably interdependent, as Hanson (1958) and Kuhn (1970) had argued, and they were well aware of the contingencies inherent in establishing a “scientific fact” that Fleck had brought into sharp focus decades earlier (1935/1979). In mapping stratigraphic sections, identifying post-holes, cataloging artifacts, even at a recent historic period site like Fort Walsh, field observation is quite literally made possible by interpretive background assumptions.

<sup>2</sup> *American Antiquity* is the flagship journal of the Society for American Archaeology.

They were also clear, however, that this is not just a matter of arbitrary interpretive projection; it is an ongoing process of eliciting and making sense of empirical “resistances,” points of friction and failures of fit that can put considerable pressure on the very typological categories and conceptual frameworks that enabled us to recognize data as potential evidence and build an “archaeological record.” They effectively recognized that neither empiricist foundationalism nor radical constructionism does justice to their practice well before these positions became the explicit focus of internal debate.

By the mid-1980s “postprocessual” critics hostile to the New Archaeology drew the implication that, if archaeological data are inevitably “theory laden”—if evidence and facts are interpretively constructed “at the trowel’s edge” as Hodder later put it (1997)—then archaeologists must simply be “creating facts” (Hodder 1983, 6). In this case, they concluded, there is “literally *nothing independent of theory* or propositions to test against” (Shanks and Tilley 1987, 111; emphasis in the original): Archaeologists should just tell the stories that need to be told. Many who took such constructionist arguments seriously faced a crisis of confidence about the inherently circular nature of evidential claims that reproduce what had been described a generation earlier as the Diogenes problem: Archaeologists may “find the tub [in the town square of Athens] but altogether miss Diogenes” (Smith 1955, 1–2; Wylie 2011, 378). The conclusion that a domain-specific skepticism is warranted given examples of contingent underdetermination depends upon three problematic premises.<sup>3</sup> First, the bar for epistemic credibility must be set unreasonably high, at deductive validity. Second, all inferences that fall short of this standard must be assumed to be equally and radically tenuous; this includes not only reconstructive or explanatory hypotheses but also the evidential claims on which they are based. Finally, despite the skeptics’ suspicion of ampliative inference, particular instances of error fortuitously detected or counterfactually projected are generalized to all archaeological inference, including evidential reasoning. This framing sets up what I have described as an interpretive dilemma; archaeologists must either stick to empirical description of the record—the possibility of which these skeptical arguments call into question—or embrace speculation constrained only by interpretive convention (Wylie 2002, 117–26; 2011, 389). Postprocessual critics of the New Archaeology briefly embraced the speculative horn of this dilemma but abandoned the strongest of their constructionist claims when they recognized that these undercut their own critical arguments for reassessing entrenched archaeological narratives. A majority of practicing archaeologists went on with business as usual, sidestepping what came to be known as the “theory wars” of the 1990s. As one archaeological commentator observes, the pivotal issues were never resolved, they just went underground (Johnson 2010, 220–23), and in many respects they continue to configure internal debate about the epistemic status of archaeological claims (Chapman and Wylie 2016, 6–7, 30; Wylie 2011).

Framed at this level of abstraction I found the uncompromising constructionism of postprocessual archaeologists just as disconnected from practice and untenable as the programmatic positivism they were meant to counter. They recapitulate what

<sup>3</sup> This is a much condensed summary of arguments developed in Wylie (2011, 379) and Chapman and Wylie (2016, 32).

Kourany (2022) has described as “old worries” about science that dominated philosophical debate about the rationality of science in the period when various forms of postpositivist contextualism were displacing Received View philosophy of science. As she describes them these worries are that, if there is no “theory-neutral fact-stating language, hence no theory neutral-facts,” the empirical integrity of science is undermined by a pervasive threat of theory-driven “fact construction” (2022, 233). Kourany goes on to argue that these worries about “fact-shaping” were misplaced. Given the ongoing success of a wide range of scientific research programs, “scientific rationality appeared to be robustly healthy at precisely the moment that, to philosophers, it seemed to be seriously ailing” (2022, 230; Wylie 2022, 257, 263).

I drew similar conclusions about archaeology reflecting on the fieldwork in which I was involved through this period. A great deal of philosophical wisdom about how evidential claims are constrained, vetted, calibrated, and updated is embodied in meta-methodological norms of practice that are largely implicit (Chapman and Wylie 2016, 10). When archaeological data get purchase as a defeasible source of empirical constraint on claims about the past, it is precisely because of the inferential scaffolding provided by the interpretive background that had been seen as a liability by archaeological theorists who felt compelled by the threat of fact-shaping circularity to choose one horn or the other of the interpretive dilemma. This scaffolding consists of empirical and technical resources as well as theoretical claims and assumptions that function as warrants mediating the inferences by which data are identified as potential evidence and claims about their evidential significance are ratified. Archaeological data never confront interpretive or explanatory claims about the past as a self-warranting, autonomous empirical tribunal of their truth, and rarely do they establish these claims with certainty, but by no means are they all or only a matter of arbitrary interpretive convention.

I have made the case that there are three key aspects of evidential reasoning that account for the capacity of archaeological data to counter the threat of fact-shaping that Kourany describes as “old worries” about science (Chapman and Wylie 2016, 33–43; Wylie 2020).

- 1) The warrants that mediate inferences about evidential significance are subject to empirical constraint; their “security” can be systematically adjudicated (Wylie 2011, 383–87).
- 2) The theories that “laden” archaeological data-as-evidence are not necessarily, or even typically, the same theories as are presupposed by the hypotheses and models that archaeologists use this evidence to build and assess. This gives rise to epistemic independence in a first sense, between theories that function as a component of scaffolding and theories that are directly or indirectly the object of testing (*ibid.*, 381–83).
- 3) Rarely do archaeologists depend on a single line of evidence. Of necessity, they enlist a strikingly diverse range of background knowledge to constitute different types of data as evidence. This makes possible epistemic independence in a second sense, between lines of evidence each of which is backed by its own set of warrants. Independence in this sense puts archaeologists in a position to exploit strategies of triangulation or, more broadly, robustness reasoning (Wylie 2020, 296–98).

Archaeologists may be “epistemically unlucky,” as Adrian Currie puts it with reference to the historical sciences generally, but they are also enormously innovative “methodological omnivores” (2018, 157). Circular reasoning is always a risk, but the practice of mobilizing a diverse array of technical, empirical, and theoretical resources can ensure that the lines of evidence archaeologists construct are mutually constraining. Often enough the problem is not that they all too neatly reinforce one another but that they fail to converge on any coherent account of the antecedent events and conditions that produced them, forcing archaeologists to check for error in lines of evidence they had considered secure, and sometimes to reconsider framing assumptions and norms of justification (Wylie 2011, 387; 2020, 297). Currie significantly broadens this roster of evidence-constituting strategies, recognizing the role of nontrace sources of evidence in analysis of paleontology and geology as well as archaeology.

Countering the dead-ends created by abstract philosophical worries requires at least a modicum of immersion as an insider/outsider in research practice and internal disciplinary debate. In my case, this was a matter of taking problems that arise in archaeological practice as my point of departure and articulating the philosophical wisdom embodied in this practice, sometimes as a gadfly commentator and sometimes by collaborating directly with archaeologists, in work that was at times addressed to a philosophical audience and at times to archaeological theorists and practitioners. Postpositivist philosophy of science has undergone a dramatic reorientation as a field in the process of turning away from toy examples—textbook exemplars, popular overviews, isolated cases (Bunge 1973, 18), and a “fantasy image of physics” (Glymour 1980, 292)—to consider the details of real-world scientific practice. One might say we have been engaged in a fieldwide process of “epistemic integration” that has been transformative for philosophy of science and has also been, to varying degrees and in some contexts, bidirectional in its influence on research practice.

### 3. History and philosophy of the field

In retrospect, perhaps the most important lesson I took away from immersion in archaeological fieldwork is captured by what Kourany describes as “new worries about science” (2022, 228–29, 233–35). These are worries about fact-shaping that arise from an insight central to the aims approach to “values in science”: that the values which set a research agenda configure all aspects of research practice. While, on her account, philosophers have tended to set this concern aside as a pragmatic matter—a failure to realize ideals of value-free science in practice—scientists have taken seriously the implication that an “ongoing shaping of the facts” of this kind carries not only an immediate risk of partiality but also entrenches a path-dependent research trajectory that has downstream effects, including the potential to “preclude the discovery of other facts” (Kourany 2022, 234–35; Wylie 2022, 263).

Consider an example that illustrates how fact-shaping in this sense works and the role that critically engaged history and philosophy of science can play in addressing Kourany’s “new worries.” I draw here on *These Mysterious People* (Roy 2016), an historical account of how the cultural history of a famously rich Musqueam settlement and burial site has been configured by and has perpetuated a “civic narrative of dispossession”

(*ibid.*, 33, 150).<sup>4</sup> Located at the mouth of the Fraser River in present-day British Columbia, this site was known to archaeologists as the Great Marpole Midden, and to members of the Musqueam Tribe as *čəsnaʔəm*. Indigenous presence at *čəsnaʔəm* dates back at least 4,000 years, and it is understood by the Musqueam to be one of several major settlements that anchor their traditional, ancestral, and unceded territory. It was continuously occupied until the community was decimated by smallpox in the early to mid-nineteenth century and it continues to hold tremendous historical and cultural importance for the contemporary Musqueam community. Roy traces the archaeological exploration of *čəsnaʔəm* to 1895 when a museum collector, Hill-Tout, published the claim that the original inhabitants must have been “displaced or exterminated” by a “hostile people” who had migrated from the interior, concluding that they were a pre-Salishan people (*ibid.*, 32). On this account the “contemporary [Coast Salish] Aboriginal residents” of *čəsnaʔəm* were “relatively recent newcomers to the area.” The empirical basis for this conclusion was evidence of chronological difference in assemblages of excavated material culture interpreted on the basis of Victorian era assumptions about Indigenous cultures. Roy quotes Hill-Tout as reporting that “primitive peoples such as our Indians [are] deeply conservative”; their cultures are static, so differences in material culture over time or across a region could be assumed to represent distinct cultural traditions (*ibid.*, 53). Hill-Tout also claimed to have identified two distinct skull shapes—long and broad form crania—on which basis he drew the further conclusion that these cultural traditions were associated with racially distinct populations.

Boas recapitulated this narrative about *čəsnaʔəm* in his report on the archaeological excavations he oversaw as part of the Jessop Expedition between 1897 and 1902. He did not endorse the “two race” theory; in fact, his field director reported that he observed no clear distinction between skull types (*ibid.*, 53). And his overall conclusions, based on linguistic and ethnographic as well as archaeological evidence, were that “the people of the North Pacific coastal region no longer appear to be unchanging, ahistorical entities” (Boas 1908, as quoted by Roy 2016, 35). Nonetheless, consistent with the “salvage anthropology” convictions of the period, Boas concluded that *čəsnaʔəm* had been occupied by a sequence of “two distinct peoples” and that its original inhabitants had disappeared.

This displacement hypothesis became the cornerstone of an archaeological synthesis developed fifty years later on the basis of an ambitious excavation program directed between 1947 and the early 1970s by Borden, an archaeologist based at the University of British Columbia. Even though, from examination of his field notes, Borden’s own data do not substantiate the displacement hypothesis, to the end of his long career he maintained the conventional wisdom that an earlier “Marpole” culture (450 BCE to 500 ACE) had been replaced by a later immigrant culture; this transition was designated in his system as Whalen Phase II (Borden 1970). It was not until the mid-1990s, when a systematic reexamination of the Whalen Farm site material established that it likely represented a seasonal occupation rather than cultural displacement (Thom 1992), that Borden’s transitional phase hypothesis was decisively rejected. Nonetheless, the basic architecture of his system has had remarkable staying power; to this day it sets the terms in which archaeological sites and assemblages are described, compared, reported, and, crucially, regulated as cultural heritage in the

<sup>4</sup> I summarize here an account of this case that is developed more fully in Wylie (2022).

region. Roy makes the case that the displacement hypothesis, embedded in the conceptual infrastructure of regional archaeology, functions as an ideological commitment that both reflects and legitimates settler-colonial interests; it serves to dissociate the Musqueam from their heritage and to normalize violent displacement of one population by another as a “natural process in *all* human settlement” (2016, 33; Wylie 2022, 267). The 1948 news article from which Roy takes her title makes these eliminationist assumptions explicit:

“Who were these mysterious people who lived long ago at Sea Island at the mouth of the Fraser River? . . . They were not Indians certainly.” (Roy 2006, 70)

Archaeologists themselves routinely develop critical genealogies of the research traditions they inherit, often with the aim of reassessing taken-for-granted assumptions that have canalized inquiry. Building on Chang’s proposal for approaching history and philosophy of science (HPS) as a form of “complementary science”—a “continuation of science by other means” (2004, 235–50)—I suggest that such genealogies represent a mode of field philosophy through which historians and philosophers of science can directly and productively engage the sciences they study (Plaisance and Elliott 2021, 608, 611). Systematic investigation of the historical antecedents of current research programs can serve not only to recuperate empirical and theoretical insights that were set aside when a research program moved on but also, as Chang suggests, to recover “suppressed and neglected questions” (237), a goal well served by critical appraisal of the contingent path-dependent processes by which research programs have taken shape. This is a matter of documenting the values and interests that directed attention to specific topics and informed the choice of questions and strategies to pursue in addressing them, then tracing the downstream fact-shaping effects of originating aims that often persist long after they have been discredited. Depending on context and purpose, complementary HPS genealogies may be clarifying or “transgressive,” revisionary, or transformative.

Returning to Plaisance and Elliott’s schema, the mode of social interaction required to do field philosophy as a form of critical complementary science need not involve immersive social interaction. Sometimes it is best undertaken by scholars whose work is informed by, but who are not themselves embedded in, the research programs they study, as a form of adjacent scholarship. Certainly, too, the degree of epistemic integration will vary. The results of a practice-focused genealogical study may unsettle philosophical, historical thinking about the science in question, but its greatest value, when researchers are receptive, is to ensure that the scaffolding that was put in place to get inquiry off the ground, and that gives it focus and direction, is continuously assessed and updated. I submit that critical genealogy as mode of engaged philosophy of science has a central role to play in fostering epistemic iteration of the kind Chang advocates. As such, it is a crucial strategy for addressing Kourany’s “new worries.”<sup>5</sup>

<sup>5</sup> In a similar spirit, Tuana makes the case for bringing a “genealogical sensibility” to bear on questions of environmental injustice (2023, 5–9).

#### 4. Collaborative field philosophy

I close with an example of a final genre of engaged philosophy of science undertaken in the spirit of the resolutely embedded mode of field philosophy advocated by Tuana (2010, 2020).

In May 2021 the Tk'emlups Nation in the interior of British Columbia broke the news that a ground-penetrating radar (GPR) survey had identified subsurface anomalies—“targets of interest”—likely related to the unmarked graves of children who died while attending an Indian Residential School located on their territory (Watson and Eneas 2021). This drew national and international attention and was quickly followed by a number of other reports from GPR surveys undertaken on former residential school grounds across Canada. These facts on the ground were by no means new. In 2008 settlement of the largest class action suit in Canadian history resulted in the creation of a federal Truth and Reconciliation Commission (TRC) with a mandate to investigate the history and legacies of the Indian Residential School system. The commission’s report (TRC 2015) is a searing indictment of a century-long practice of removing Indigenous children from their communities and of institutionalized physical, psychological, and sexual abuse of these children in the residential schools to which they were sent; the drafters conclude that this can only be described as a deliberate program of cultural genocide (1–3, 54–55). From the outset the explicit goal was assimilation aimed at what the deputy superintendent of the Department of Indian Affairs described in 1920 as “the final solution of our Indian problem” (Backhouse 2021, 62), echoing a statement made by his US counterpart in 1892: that the goal was to “kill the Indian in him, and save the man” (Carlisle Indian School Digital Resource Center, *nd*).<sup>6</sup> In the event, the Canadian residential school system routinely killed both. Acknowledged mortality rates reported for schools in the Western provinces during a five-year period in the early twentieth century were between 30 percent and 60 percent (Bryce 1922). Data reported by the TRC in 2015 indicate that, in the 1940s, the death rate for residential school children was nearly five times higher than for other children in the general population in Canada, and even in the 1960s it was double this base rate (91–92). As this suggests, these residential schools are by no means a thing of the distant past. The last one to close was Gordon’s Indian Residential School in Punnichy Saskatchewan, 500 km NE of Fort Walsh; it had opened in 1888 and was run by the Anglican Church of Canada until 1996 (National Centre for Truth and Reconciliation, Gordon’s Reserve, *nd*).

When I moved to the University of British Columbia in 2017 an Indigenous/Science research network was taking shape, the aim of which has been to take up the TRC calls to action in a research context: to seek “equitable, respectful, thoughtful, and transparent partnerships with Indigenous Peoples,” this being “the primary means through which reconciliation may be advanced” (TRC 2015, 333; Indigenous/Science Research Cluster 2018). One of the regional Indigenous communities with whom members of the network had been working is the Penelakut Nation whose reservation lands include an island east of Nanaimo where a particularly notorious Indian Residential School (IRS) was located, the Kuper Island Indian Industrial School. This

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<sup>6</sup> As Deputy Superintendent William Duncan Scott oversaw the Canadian Indian Residential School system from 1913 until 1932 (Backhouse 2021). Richard Henry Pratt, a US army officer, founded the Carlisle Indian Industrial School in Pennsylvania in 1879 and ran it until 1904.

residential school, which was run by the Missionary Oblates of Mary Immaculate, opened in 1890. Students set fire to the school buildings in 1896 by which time surviving records indicate that 107 of 264 children sent to Kuper Island IRS had died; others were later acknowledged to have committed suicide or drowned trying to escape (McCue 2022; TRC 2015, 105–7; Indian Residential School History and Dialogue Centre, *nd*; National Centre for Truth and Reconciliation, Kuper Island, *nd*). The federal government took over the administration of this school in 1969 and it was officially closed in 1975. Within a few years the Penelakut dismantled the school buildings and threw the debris into the harbor. Survivors of Kuper Island IRS were among the first to attract national attention for speaking out publicly about pervasive violence and sexual abuse at the school, and in 1995 a priest who had been its director for many years was tried and convicted on charges of sexual assault and indecency. A 1997 documentary, “Return to the Healing Circle” (Campbell) was influential in “breaking the code of silence” that surrounded these abuses, and this last year the Canadian Broadcasting Corporation aired an eight-part documentary series focused on this school (McCue 2022).

An ongoing concern for the Penelakut is that they do not know where the children who died at the residential school are buried; if they were buried in any of the extant formal cemeteries their graves were not marked, and many were evidently interred in clandestine graves. The process of community healing requires that these children be memorialized, and with their graves increasingly at risk of disturbance there is an urgent sense that they need to be located and the necessary spiritual work undertaken. Martindale, co-Principal Investigator of the Indigenous/Science network, had undertaken a GPR survey of a known cemetery in 2014–16 at the request of Penelakut leadership, and had identified several unmarked child-sized anomalies in a row behind the graves of residential school staff (Harris et al. 2017). Penelakut elders called a halt to this work so they could deliberate on how to proceed. In 2018 we asked the Penelakut leadership if they would like to resume the work, and they requested that we survey a couple of areas that were slated for development, to ensure that new construction would not disrupt children’s graves. We were referred to the Penelakut Elders’ Committee for guidance, and the questions they asked when we met with them signaled the sensitivity of this work. They wanted to know not just “could we be trusted to find the missing children?” but also, would we do the work in a “good way,” would we follow through? As one elder put it: “What positive vision could we have to work together?”<sup>7</sup>

In the course of these meetings, and when engaged in doing fieldwork, we confronted any number of jointly political/ethical and epistemic/methodological questions. Most prominent are questions about our obligations as settler scholars to the Penelakut, and to our own communities. We came to think of this work as a practice of bearing witness, with reference to both Coast Salish and Euro-Canadian settler traditions, and as set out in the TRC report (Simons et al. 2021, 23–24). As an established practice in Coast Salish cultures, respected leaders, elders, and community members are called as witnesses to observe what is done at important ceremonies; they are responsible for carrying news of the proceedings back to their home communities and into the future. We have not been called as witnesses in this

<sup>7</sup> This account of the Penelakut GPR work is based on Simons et al. (2021).

formal Coast Salish sense, but when we undertake GPR surveys on behalf of the Penelakut there are at least three ways in which we bear witness.

A first sense of witnessing relates to the history of the IRS. We bear material witness, through documenting possible grave sites, to what happened at the Kuper Island IRS. When invited to formal meetings of Elders' Committee to discuss how we should proceed we are also sometimes witness to direct testimony of survivors and community members affected by intergenerational trauma. In a second reciprocal sense, when we are on Penelakut territory we are acutely aware that we are ourselves witnessed. We are continuously appraised as to our motives and integrity, what we do and do not know, how we take direction and criticism, how deeply ingrained the discriminatory prejudices of settler society are in us: whether we will hear and honor Penelakut understanding of the residential school and its legacy. Finally, the TRC calls on all Canadians to serve as witnesses: "to store and care for and share [what they witness] with their own people when they return home" (TRC 2015, 442). This third sense of witnessing is a role that comes with considerable risks of all the kinds faced by allies and advocates (Alcoff 1992; Sullivan-Clarke 2020). How do we bear witness in the first sense without reinscribing settler-colonial systems of oppression, speaking about, speaking for? The physical evidence provided by GPR survey reports drew immediate and sustained attention in the national media; for many the results of geophysical surveys evidently carry more authority than the testimony of survivors and the archival evidence painstakingly documented by the TRC. How do we ensure that our tools of inquiry, legible as scientific in settler contexts, do not reinforce norms that marginalize what Indigenous communities have long known, the witness they bore to these histories in lawsuits and public hearings as a matter of living memory and oral history? And, crucially, how do we change the institutions within which we work that sustain these norms, including the funding mechanisms, and systems of expectation and reward, that powerfully reinforce extractive modes of research practice?

## 5. Conclusion

I conclude with two thoughts about how to proceed with the difficult, uncertain, and often suspect genres of philosophical work that have recently been named "field philosophy."

First, reflecting on the examples I have discussed, it is clear that Indigenous communities have considerable interactional expertise with respect to settler society and science. It is we outsiders—scientists and philosophers alike—who need to develop interactional expertise with respect to Indigenous cultures, knowledge and ideas, values and social norms, and the histories and social-political contexts in which working partnerships take shape (Wylie 2015, 196–98). The advice offered by Indigenous partners converges at many points on that which is captured by advocates of field philosophy working in a range of different contexts: Cultivate the necessary humility to recognize that we have a great deal to learn and much unlearn, and be vigilantly on guard against the "self-arrogated hegemonic authority" that is so often legitimated within and by philosophy (Mitova 2020, 191), as exemplified by some of the early exchanges between philosophers and archaeologists.

Second, these examples of field philosophy in the context of a field science suggest some directions to take in building on Plaisance and Elliott's schema. With respect to social interaction, they suggest that this continuum runs not only from individual to collaborative engagement but can also take different forms all along this spectrum, depending on aspects of engagement that Plaisance and Elliott quite justifiably bracket for purposes of building a useful and inclusive framework. As some of their examples illustrate, the relationships in which field philosophers stand to the research communities they study vary substantially depending on their goals and intended audiences. Some operate most effectively at arm's length, attentive to the field they engage but adjacent to it. Work conducted from this vantage point need not be addressed exclusively to fellow philosophers; it may be framed as an intervention in the subject field. When their engagement is immersive it may be as an active participant in disciplinary debate rather than as a member of a research team. And when field philosophers are directly involved in the reciprocal, collaborative development of a research program their contributions are often not initially or primarily philosophical. As I found, and as Plaisance and Elliott note, the contours of a philosophical problem may take some time to crystallize in the context of field immersion. Finally, these modes of engagement, as well as the goals, outcomes, and impact of field philosophy projects, are typically configured by interaction with a wide range of stakeholders who have an interest in or are affected by a scientific research program or practice. Given how divergent these interests can be the audiences addressed by field philosophers are likely to be as heterogeneous as their partners, which adds another layer of complexity and accountability to the practice of field philosophy.

Consider, too, the diverse modes of epistemic mixing that can take shape along Plaisance and Elliott's axis of epistemic integration. Learning philosophically from adjacent observation or from immersion in the archives is a mode of engagement from which philosophy of science has benefitted greatly, shifting our philosophical imaginary in profound ways, for example, with respect to the philosophical "worries" about science identified by Kourany. Translational, kibbutzing interventions may start by clarifying philosophical terms of art that circulate within scientific contexts but they often end up troubling these terms, exposing their limitations and calling into question their presuppositions for philosophers as much as for practitioners. A critical genealogy undertaken in the spirit of Chang's HPS as complementary science has the capacity to address the "new worries" Kourany finds arising in scientific practice and, when these are a matter of active concern for practitioners, the results can be profoundly transformative for all parties. To this I add bearing witness as a mode of field philosophy practice, undertaken in contexts where the political stakes are high for all concerned. Although by no means a systematic mapping of the domain, these additional complexities are indicative of the rich possibilities captured by field philosophy.

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