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Community-Based Collaborative Archaeology

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Although many still hanker for universally true, value-free knowledge of the social world—the proverbial 'view from nowhere'—some of the best, most compelling research in the social sciences is credible not because it somehow transcends all interests and contexts of practice, but because it is self-consciously situated and brings diverse angles of vision to bear on its central claims. Traditions of inquiry that actively engage non-scientific local knowledge in various ways have taken shape in a great many fields, most visibly under the banner of 'participatory action research' and 'community-based participatory research'. In these research programmes practitioners recognize that the subjects of inquiry, and any number of other stakeholders, have their own distinctive forms of expertise from which researchers stand to learn a great deal.

Archaeology might seem to be a field where this kind of community engagement is impossible, inasmuch as the subjects of inquiry are often long dead, or especially risky given worries that archaeological interpretation is notoriously vulnerable to speculation. But in fact, a growing number of archaeologists now work closely with community groups of all kinds and, despite sharp opposition from critics who fear that this cannot but undermine scientific integrity, they argue that their research is significantly enriched by these collaborations.

I focus here on archaeologists who work with Indigenous descendant communities in North America and address two key questions raised by their practice about the advantages of situated inquiry. First, what exactly are the benefits of collaborative practice—what does it contribute, in this case to archaeology? And, second, what is the philosophical rationale for collaborative practice? Why is it that, counter-intuitively for many, collaborative

practice has the capacity to improve archaeology in its own terms and to provoke critical scrutiny of its goals and methodological norms? The broader import, I argue, is a rethinking of traditional views of objectivity that takes social, contextual values to be a resource for improving what we know, rather than inevitably a source of compromising error and distortion (as discussed in detail in Chapters 7 and 9).

I begin with an account of the context in which collaborative practice has taken shape in archaeology. I consider a pivotal example of this practice and then address the two questions I pose here. I conclude with a brief comparison between lessons drawn from long-standing PAR and CBPR research traditions and those recently learned in and from archaeology.

1. The Context for Collaborative Practice: A Sea Change in Archaeology

Archaeology is undergoing a fundamental sea change. Indigenous peoples have long insisted that the sites and cemeteries and cultural material of their ancestors are part of a living heritage, not only or primarily a scientific resource to be exploited by archaeologists. Recent activism has brought these claims to the fore, and in many contexts they are now backed by law. One high-profile but by no means unique example is the Native American Grave Protection and Repatriation Act (NAGPRA), signed into law in the United States in 1990. The impact was immediate. Museums were required to inform tribal groups of any material they held that might be subject to repatriation, and archaeologists found themselves subject to regulations that require them to consult with, to get consent from, and to practise in ways that respect the traditional values of Native Americans. With this, the ground rules for archaeological practice decisively changed: archaeologists could no longer assume privileged access to and control over material they regarded as essential data, and long-standing conventions of disciplinary autonomy and self-determination were now very publicly contested.

In raising these challenges, Native Americans and a great many other descendant communities world-wide call into question a set of assumptions that had informed archaeological research for well over a century: that indigenous peoples were disappearing, or had disappeared, and that the cultural history and traditions to be salvaged are significant not to a living community but as an element of world history or, often enough, as part of natural history. Confronted with descendant communities whose cultural traditions are very much alive, outspoken critics of repatriation legislation argued that the interests of scientific investigation should take precedence as a matter of principle. David Hurst Thomas details this debate in *Skull Wars* (2000), quoting a physical

anthropologist who makes these claims explicit: 'ancient skeletons belong to everyone'; they are 'the remnants of unduplicable evolutionary events [about] which all living and future peoples have the right to know'; by extension, no 'living culture, religion, interest groups, or biological population' have the right to restrict the research mandate of scientific experts who have the necessary skills and knowledge to make the best use of surviving 'remnants' as evidence (Thomas 2000: 209–10). These arguments recapitulate a set of ideals that have been widely presumed to define the enterprise of science in the post-Second World War era: that scientific inquiry is a quest for truth that transcends local interests and beliefs; it is accountable only to standards of justification shared, in principle, by all rational inquirers, however they may be situated; and, in this, it is an intrinsically (and universally) valuable pursuit. On this account, the authority of the sciences depends upon ensuring that its practice and its practitioners are insulated from the influence of external, contextual values (as discussed by Montuschi, Crasnow, and Douglas in Part III): if a field like archaeology is to be scientifically credible, its autonomy must be protected at all cost.

The debate about 'who owns the past' has thus been cast as an intractable conflict between science and religion or, more generally, between science which has universal import and culturally specific ways of understanding the world that arise from traditional (non-scientific) belief systems limited, in their salience, to particular local contexts. Not surprisingly, in this era of 'science wars' (mentioned in the Introduction), this has been the stuff of breaking news. And, from the outset, the cases that have grabbed headlines are ones in which the most uncompromising reactions against any interference with the autonomy of archaeology are on vivid display. One of the most widely discussed has been the protracted legal struggle over 'Kennewick Man' in which the plaintiffs, eight prominent archaeologists and physical anthropologists, sued for the right to study a 9,400-year-old skeleton that had been exposed by erosion in the banks of the Columbia River near the town of Kennewick (Washington State). The US Army Corps of Engineers control the land where Kennewick Man was found and determined that his remains should be repatriated, without further scientific study, to a consortium of local tribes led by the Umatilla. This decision was immediately challenged, as it exemplified exactly the kind of worst-case scenario feared by critics of NAGPRA. Human remains of this antiquity are extremely rare in North America, and to repatriate them without study would be a terrible loss to archaeological science. The legal case brought by the plaintiffs turned on the claim that no affiliation with specific contemporary Native American tribal groups could be established for remains of such age and, in any case, that the interests of scientific research should take precedence over those of the tribes. The plaintiffs won access in a 2005 court ruling that sharply limits the scope of Native American claims of affiliation and in many quarters has reinforced

Native Americans' long-standing mistrust of science in general and of archaeologists and anthropologists in particular.

Reflecting on cases like Kennewick one especially forthright archaeological critic of NAGPRA, Geoff Clark, denounced repatriation legislation as pandering to the interests of 'various pseudo- and anti-scientific constituencies'. 'We all lose,' he argues, 'if for reasons of political expediency, Indians rebury their past.' What's at issue here, on Clark's account, is not just the loss to archaeologists of crucial data but, more broadly, a fundamental challenge to the scientific worldview. To accede to repatriation puts the religious beliefs of Native Americans 'on an equal footing with science' and this, he insists, threatens to roll back all the accomplishments of Enlightenment rationality, science having been instrumental in 'achieving the modern world'; it legitimates superstition and ignorance, returning us to a 'demon haunted world' (Clark 1998: 22, 24; see also Clark 1996). More immediately, Clark fears that it threatens the autonomy and integrity of archaeology, opening the door to the influence of parochial, politicized interests and to non- or anti-rational values that are anathema to scientific inquiry.

These underlying epistemic worries are taken up in a philosophical context by Paul Boghossian, who opens his recent book, *Fear of Knowledge* (2006), with discussion of a *New York Times* story about the NAGPRA controversy (Johnson 1996). He characterizes it much as Clark had done, as a conflict between science and a fundamentally different (non-scientific) worldview, and is chiefly interested in the stance taken by two archaeologists—Roger Anyon and Larry Zimmerman—who figure as advocates for a more conciliatory stance. They resist the polarized positions that dominate public debate, insisting that Native Americans have interests and insights that archaeologists should take seriously. In this they are a prime example, for Boghossian, of well-meaning practitioners who end up embracing a muddle-headed, self-undermining relativism that he finds pervasive in the humanities and social sciences. Boghossian's worry here is that, if we accept that different worldviews or systems of knowledge have their own distinctive 'norms of justification'—i.e. if we do not assume that there are universal standards determining what we should count as knowledge regardless of context—then there can be no basis for choosing between claims generated by different systems. They can only be assessed in terms of the standards set by the worldview in which they originate, and in this sense they are context-relative. In the selection of quotes that Boghossian reproduces from the *New York Times* article, Anyon and Zimmerman seem to endorse such a view: Anyon concedes that 'science is one of many ways of knowing the world'—the 'Zuni world view is just as valid as the archaeological viewpoint'—and Zimmerman that, 'personally, I do reject science as a privileged way of seeing the world'. Boghossian objects that this 'doctrine of equal

validity' entails a relativism that undercuts any claim to epistemic authority and is, in this, patently untenable.

Boghossian doesn't consider the archaeological debate in any more detail but it's clear that, on the 'classical picture of knowledge' he endorses, Anyon and Zimmerman have no reason to give any quarter where the authority of archaeological science is concerned. He argues that, however appealing it may be to take a relativist stance and accord credibility to alternative worldviews, in the end 'we have no option but to think there are absolute, practice-independent facts about what beliefs it would be most reasonable to have under fixed evidential conditions' (2006: 110). Moreover, on Boghossian's view, the 'norms of justification' characteristic of contemporary science are a good approximation to these facts. There is no reason to question these norms, he argues, unless an alternative worldview can be shown to have a 'proven track record' of epistemic success that has produced 'more advanced science and technological abilities' than our own, based on genuinely different 'norms of justification'—and none has been presented (2006: 101). The goals of inquiry as well as what counts as success are thus assumed to be given: a timeless and absolute set of standards that all rational inquirers would recognize if they could transcend the specifics of context and take the stance of a 'view from nowhere'. The implication seems to be that Anyon and Zimmerman should not take Native American ways of knowing seriously unless they can be shown to deliver scientifically credible results, and if these embody non-scientific goals and standards of epistemic success, then by definition they have no bearing on archaeological inquiry.

What's lost in this debate, structured as it is by stark oppositions between science and non-science, is a whole range of cases that never make the headlines, ones in which, far from being shut down or compromised by taking seriously Native American interests and beliefs, archaeological research is thriving in the context of collaborative partnerships. One that Thomas discusses in *Skull Wars* as a counterpoint to Kennewick Man is a project that was undertaken jointly by archaeologists and Native Alaskans when human remains were found in a cave in Tongass Forest on Prince of Wales Island, Alaska (Thomas 2000: 268–76). These remains, which were discovered just a few weeks before Kennewick Man in 1996, proved to be 9,200 years old—just as significant as Kennewick scientifically and of intense interest to local Tlingit and Haida communities. By contrast, however, there was no high-profile drama: no headline news, no legal battle but rather a long-term research project grounded in respectful exchange between Native Alaskans and archaeologists that continues to bear fruit. A crucial condition for the success of this project that was missing in the case of Kennewick Man is that there already existed an infrastructure for collaboration: archaeologists

based in the region had long-established working relationships with Native Americans. This is, in fact, the most often cited factor that makes a difference to the viability of collaboration and is a feature of the case I will shortly consider in more detail.

This is just one example of many that fall along a broad continuum of positive responses to the demands for accountability that are reshaping archaeological practice. At the conservative end of the continuum are cases in which the archaeology itself is little changed but research is conducted in ways that respect the values and sensibilities of stakeholders; archaeologists negotiate terms of access, engage in ongoing consultation, and in various ways give back to the communities that have a stake in or are affected by their research. Scientific and non-scientific belief systems coexist, not always easily but no longer with priority granted automatically to scientific interests. In other cases of more proactive engagement, the process of meeting obligations of respectful practice gives rise to more robust forms of collaboration: various forms of partnership with Indigenous communities that do affect the substance of the science, sometimes in transformative ways.

In all cases traditional authority structures are reconfigured but, rather than signalling a crippling compromise, the archaeologists involved in these collaborations routinely argue that there are any number of ways in which their understanding of the past has been enriched by taking seriously other 'ways of knowing the world'. The *New York Times* article cited by Boghossian includes a number of telling statements along these lines that he doesn't discuss. For example, Zimmerman's statement about science not being privileged is followed by this observation: 'that's not to say [science] isn't an important way that has brought benefit. But I understand that, as a scientist, I need to constantly learn'; what's needed is 'a different kind of science, between the boundaries of Western ways of knowing and Indian ways of knowing' (Johnson 1996).

The philosophical question this raises is: how can an openness to exploring 'a different kind of science' enrich, rather than fatally compromise, a social science like archaeology? And what are the implications for conventional views of scientific knowledge—the Enlightenment ideals defended by Clark, the 'classical picture' of knowledge endorsed by Boghossian, the conviction that autonomy and impartiality are a necessary condition for genuinely scientific inquiry—that, in their various forms, still have a powerful grip on the social sciences? To address these questions, consider a recent example of collaborative practice in archaeology that begins with formal consent and consultation but then mobilizes intellectual engagement between archaeologists and descendant communities that creatively pushes epistemic boundaries.

2. Kwaday Dän Ts'inchi

Like Kennewick Man, Kwaday Dän Ts'inchi is a case of unanticipated discovery that began when the frozen remains of a young man were discovered by sheep hunters in August 1999 melting out of a high elevation glacier in northern British Columbia near the Yukon border, in the traditional territory of the Champagne and Aishihik First Nations, the CAFN. But like the Tongass Forest project, this is a case in which a political and legal infrastructure for resource management was already in place that put the CAFN in a position of full partnership with archaeologists and provincial officials (Beattie et al. 2000). The CAFN council of elders decided at the outset that 'efforts should be made to learn something about this person' whom they named 'Long-Ago Person Found'—Kwaday Dän Ts'inchi—and they worked out an agreement with provincial authorities which would 'ensure [that] cultural concerns are respected while recognizing the significant scientific considerations inherent in a discovery of this nature' (British Columbia Ministry 2011). A news story published in the *Yukon Times* echoes Zimmerman: 'the project became a blend of traditional values and modern science; rather than claiming ownership, the First Nations shouldered the responsibility for stewardship of this remarkable discovery' (Gates 2009).

In the course of a decade, the CAFN worked with provincial authorities and the team of researchers they assembled, setting the research agenda, jointly reviewing and approving all proposals for scientific study of the human remains and associated artefacts, and ensuring that local, Indigenous values were respected in the treatment of the deceased. It was especially important to the CAFN to determine, if possible, whether Kwaday Dän Ts'inchi had any living family or clan descendants so the appropriate members of the community could handle his memorial and return to a final resting place. This collaboration made a wide range of scientific work possible, including destructive testing and a DNA study, both types of research that are, for good reason, often unacceptable to Indigenous communities. Crucially, the agreed-upon research programme included the questions about clan and family affiliations that were of interest to the First Nations but were not on the agenda for the scientists who made up the research team, in part because it seemed implausible that DNA testing, or other tools of archaeological science, would make it possible to identify descendants of such a long-deceased individual.

So what was done, and what's been learned? The CAFN approved radiocarbon and collagen dating which established that Kwaday Dän Ts'inchi lived some time between AD 1670 and 1850, and a full autopsy provided the data necessary for a pathology workup and food residue analysis, which made it possible to reconstruct what he had ingested in the three days before he died,

in roughly six-hour increments. Isotope and trace element analysis of hair, dentition, bone core, and muscle tissue produced longer term dietary profiles, and analysis of the associated artefacts—Kwaday Dän Ts'inchi's spruce root rain hat and squirrel skin robe, tool kit, and cache of food—and of the various pollens, microbes, parasites, and insects lodged in the traveller's hair, skin, clothing, and equipment, suggested cultural affinities and provided further environmental clues to the specifics of his last trip.

The upshot is that Kwaday Dän Ts'inchi was 18–20 years old and had travelled roughly 100 km in the three days before he died, likely in the summer and originating on the coast where, early in his trip, he had eaten salmon, shellfish, mosses, and flowering beach asparagus, and been exposed to chenopodium pollen. Mineral deposits in the water he'd consumed on his trip reinforce these conclusions: two to three days before he died he drank brackish water that occurs in coastal marine environments, and in his last hours he drank glacial melt water. His lifetime dietary profile indicates that he lived predominantly on the coast, eating a marine diet. However, hair composition analysis makes it clear that, in the last year of his life, he had shifted to terrestrial inland foods. His clothing and tool kit likewise incorporate both coastal and interior elements. His rain hat was woven of sitka spruce that grows only on the coast while his robe was made of arctic ground squirrels that live only in the interior. Finally, the community DNA study initiated by the CAFN, for which close to 250 community members volunteered blood samples, is reported to have identified some seventeen living matrilineal relatives of the deceased, most members of the Wolf Clan, with both interior and coastal connections.

The significance of these results lies in the fact that these multiple lines of evidence bear witness to extensive family and clan affiliations that link coastal and interior communities. This calls into question a set of assumptions about ethnic identity that underpin much conventional ethnography and archaeology: that tribal identities are spatially defined—they are social groups (communities) that are tied to a specific region—and that these geographically localized affiliations take precedence over kin or clan identities. This reflects an imposition of social categories familiar within Euro-American traditions and informed by theories of cultural evolution—a set of framework assumptions that are at odds with community understanding and oral tradition. As reported in the local news: 'the DNA research has been a scientific confirmation of something that the people have long known, that the traditional ties between the coastal Tlingit and the people of the Southwest Yukon transcend artificial political boundaries' (Gates 2009). In short, the research agenda set by the CAFN generated key insights about social organization, cultural history, and identity that have broad implications for the archaeology and ethnography of the region, and for entrenched framework assumptions of much wider import.

3. What's Gained by Collaboration?

The Kwaday Dän Ts'inchi project is an example of collaborative research that illustrates how 'the incorporation of Aboriginal worldviews, histories, and science . . . [can] inform and broaden understanding and interpretation of the archaeological record' (Nicholas 2010: 11). This is exactly the kind of collaborative partnership that a growing number of archaeologists advocate in response to the sea change I described at the outset. But it is, of course, the very idea that archaeological inquiry might be in any way influenced by, or held accountable to, Indigenous/Aboriginal understanding of their own history and cultural traditions that raises the hackles of those who defend conventional ideals of epistemic integrity and disciplinary autonomy. This throws into relief two philosophical questions to which I now turn.

- What exactly does this kind collaborative project contribute to archaeology? What are its epistemic benefits?
- How are we to understand the epistemic advantages that, counter-intuitively for some, seem to accrue to collaborative archaeology?

Where the first question is concerned, Kwaday Dän Ts'inchi illustrates three key epistemic contributions that are common to many forms of collaborative practice in the social sciences. First, the terms of collaboration enlarged the research agenda, raising a set of questions about cultural affiliation that particularly concerned the CAFN but were not high on scientists' research agenda. Central among these were the questions about lineal descendants that led to the DNA testing project. Second, pursuing these lines of inquiry directed attention to types of evidence and interpretive resources that had not gotten uptake in conventional research. For example, the CAFN partners to the project brought to bear considerable depth of knowledge about local ecology and regional subsistence practices that made it possible to interpret the dietary profiles generated by analysis of Kwaday Dän Ts'inchi's remains and the various items of material culture found with him. Combined with the resources of oral history that testify to long-standing family and clan connections spanning the region, this background knowledge was crucial in establishing that Kwaday Dän Ts'inchi had ties to and had lived in both coastal and interior communities. Third, this enlargement of the scope of inquiry and expansion of the resources on which it drew was not just a matter of adding detail to an existing body of archaeological understanding. The evidence of cross-region connections poses a significant challenge to framework assumptions that had long informed research in the region. These include assumptions about the subject of inquiry—the presumption that

tribal communities are spatially defined—as well as methodological assumptions about what counts as evidence that had discounted insights from oral tradition.

This last point is especially crucial, illustrating one of the key benefits of bringing the critical distance of an outsider's perspective to bear on disciplinary conventions. Time and again the archaeologists involved in collaborative practice report that, when they engage seriously with descendant communities, the contrast with other ways of thinking throws into sharp relief the contingent, evolving nature of their own, taken-for-granted disciplinary goals and 'norms of justification'. It forces the question of whether entrenched modes of practice—the standards that define what counts as evidence and what it is to do (good) archaeological science—actually serve stated goals and whether these goals themselves exhaust the range of questions archaeologists could or should be asking. The result is a process of critical appraisal that opens up creative alternatives that might never have arisen through internal deliberation.

4. What's the Philosophical Rationale?

Finally, then, the second of the two questions I posed at the outset: how is it that these collaborations enrich rather than compromise archaeological science? The central principle at work here is articulated by the archaeologist Larry Zimmerman when he observes that, as a scientist, he has to be prepared constantly to learn. I understand him to be taking a stance of openness not just to learning new facts but to learning about and continuously refining the science he practises. From within the perspective of this stance there's a great deal to be gained by mobilizing, rather than categorically denying the relevance of the resources afforded by context and interest-specific standpoints that lie outside archaeology.

Within philosophical contexts this point is captured by the liberal democratic conviction that more ideas, diverse voices, and angles of vision are inherently a good thing where the production and evaluation of knowledge is concerned—as discussed in Part III and by Solomon in Chapter 13. The wider the range of perspectives brought to bear on a question, or in the assessment of prospective knowledge claims, the more likely it is that error and bias will be exposed, that the full complexity of the subject and all relevant implications will be appreciated. There is, of course, the risk that inviting critique from all directions will reduce inquiry to a cacophony of voices, undermining the kind of single-minded focus and ease of communication that facilitates the smooth functioning of a productive research community; this is a problem that afflicts interdisciplinary collaborations

as well, as discussed by Sophia Efstathiou and Zara Mirmalek in Chapter 12. But, as we see from Crasnow, the risk of insulating research communities from external criticism is that like-minded peers, who share common cognitive goals and conventions of practice, will also share significant cognitive lacunae; they may be the last to recognize the limitations of their research programme, especially those inherent in framework assumptions and 'norms of justification' that they take for granted. So the wisdom from democratic theory is that, even though research communities must sometimes suspend critical self-examination of framework assumptions to move forward with a research programme, it is crucial to ensure that there are mechanisms in place which can counteract 'group-think' dynamics, bringing the resources of diverse perspectives to bear on claims made within this framework and securing the possibility that background assumptions and taken-for-granted standards of practice can, periodically, be subjected to critical scrutiny.

Taking this insight seriously requires a fundamental shift of focus where the goals and defining ideals of scientific knowledge are concerned. Rather than seeking universal 'norms of justification' that promise approximation to a 'view from nowhere', it suggests that the point of departure must be recognition that current research goals, conventions of good practice, and what counts as epistemic success are purpose-built and have a history; they have all evolved and continue to evolve in the context of ongoing practice. This, in turn, suggests that what's required is a proceduralist approach to specifying what will count as credible knowledge: a set of best practice guidelines that define in general terms the processes of critical appraisal by which a well-functioning research community will determine which claims to endorse as ones we can trust for specific purposes, ones we can take as a guide to action in specific contexts. Helen Longino offers such an account, arguing that the beliefs we should 'ratify as knowledge' are those that arise from the right kind of process of critical scrutiny, and the 'right process' is one which ensures that contending beliefs are subject to 'criticism from multiple points of view'. Crucially, these community practices must have the potential to expose error or distortion not only in specific beliefs but also in framework assumptions and entrenched norms of practice: this is what Longino means by 'transformative criticism' (2002: 129).

Longino posits a set of four jointly social and cognitive norms that, together, characterize the kinds of institutions and processes she believes are necessary to ensure a broad spectrum of critical engagement within a research community. They include, for example, the requirements that there be established venues for and uptake of criticism, that the evaluation of knowledge claims be subject to shared standards which are themselves open to critical scrutiny, and that research communities should recognize the value of critical input

from all its members, regardless of social status. This last principle, a norm of 'tempered equality of epistemic authority', is especially relevant here. It specifies that mechanisms be in place to counteract exclusionary practices within scientific communities: 'not only must potentially dissenting voices not be discounted, they must be cultivated'; to fail to do this is 'not only a social injustice but a cognitive failing' (2002: 131–133).

Although Longino recognizes that this fourth norm raises 'complex questions of community membership'—it 'makes us ask who constitutes the "we" for any given group'—her formulation articulates 'duties of inclusion and attention' that primarily apply internally, to the members of a community of scientists (2002: 132, 134). The need to widen the circle is well illustrated by the cases I have described of collaborative practice in archaeology and by many other examples of participatory action research (PAR) and community-based participatory research (CBPR) in the social sciences. I argue for an extension of Longino's norm, motivated by arguments drawn from critical race theory and feminist standpoint theory. Well-functioning research communities should not only take steps to counteract exclusionary bias internally, among fellow scientists; they should actively seek out external communities that hold relevant expertise and critical insight, and extend the norm of 'tempered equality of epistemic authority' to them so that they can play an active role in critically assessing the credibility of knowledge claims and the epistemic norms that underwrite them.

The rationale for reframing the norm of 'tempered equality of epistemic authority' in this way is two-fold. First is the point already made: insiders who share the defining goals and norms of a research community will often be the last to recognize problems inherent in the framework within which they work and possibilities for inquiry that lie beyond it. The second complementary thesis is that key epistemic insights and resources often arise on the margins, inverting standard (socially inflected) appraisals of credibility: those who are socially marginal—and whose expertise is therefore discounted or misrecognized—have to navigate dimensions of the social and natural world with which the comparatively privileged rarely engage (or are invested in avoiding). They may, as a result, draw on a distinctive range of experience and set of conceptual resources. More specifically, the experience of exclusion or marginalization may itself be a source of insight, generating a critical, sometimes oppositional awareness of the conditions under which knowledge is conventionally ratified, as when survival as an insider-outsider requires that you understand conventions that are taken for granted within a dominant culture as well as those that structure your own subdominant community. This point is ubiquitous in critical social science. It was articulated in influential terms early in the twentieth century by W. E. B. Du Bois in *The Souls of Black Folk* in which he argued that, under conditions of racial

oppression, African Americans develop a 'double consciousness', a capacity to see themselves and the social world as the dominant white community does as well as in their own terms. This complex identity comes with considerable cost but is also a source of critical and creative insight that has inspired successive generations of standpoint theorists. It gives rise, for example, to the methodological recommendation that researchers should take the situated knowledge and experience of those who are marginal as their point of departure (Harding 1991); it is from these standpoints that what remains tacit for the comparatively privileged becomes visible. The history of feminist research and critical race studies is replete with examples in which this has proven to be highly productive; I argue elsewhere that they are a primary motivation for contemporary standpoint theory. The upshot is that, depending on purpose and circumstance, those who are outsiders to a research community may be uniquely well situated to mobilize 'transformative criticism'.

Given this standpoint theory rationale for extending the scope of Longino's norm of 'epistemic authority', I propose the following principle as an answer to my focal question 'Why does collaboration matter epistemically':

> If well-functioning epistemic communities are to counteract the risks of dysfunctional group dynamics that insulate their established standards of justification from critical scrutiny and revision, they must seek out critical, collaborative engagement with those communities that are most likely to have the resources—not only to fill lacunae and correct specific errors in their substantive beliefs but also to generate a critical standpoint on their own knowledge-making and ratifying practices.

The rationale for extending duties of 'attention and response' to collaborative partners in archaeology thus arises not only from moral obligations to descendant and affected communities but also from an epistemic obligation rooted in norms of critical scrutiny that are at least as fundamental to our flourishing traditions of scientific inquiry as the ideals of autonomy and the quest for a view from nowhere invoked by the critics of collaborative practice.

As I noted at the outset, these insights about the advantages of collaboration are by no means unique to archaeology. They resonate with arguments for socially engaged research in the traditions of PAR and CBPR that have taken shape over decades in a number of other contexts. The standpoint theory rationale for such practice was articulated with particular clarity thirty years ago by sociologists Elizabeth Petras and Douglas Porpora (1993), who made the case that the poor and oppressed bring significant epistemic resources to the table when they are engaged as partners in research projects; they are in a position to generate genuine insights—theoretical, methodological, and empirical—that represent an important contribution to disciplinary sociology as well as providing a crucial basis for interventions designed to address

poverty and oppression. More recently, Louise Fortmann, a rural sociologist and advocate for community forestry, makes the case for recognizing the value of what she calls 'civil science', in its own right and in partnership with 'professional science':

> I have worked for decades with farmers in east and southern Africa and with rural communities in the U.S., coming to respect their expertise just as I do knowledge produced by scientists . . . Over time I have come to understand that scientists can answer some questions, people with other kinds of knowledge answer other equally important questions, and that some questions are best answered in collaboration, what is called here interdependent science. (2008: 2)

This brief for recognizing that valuable expertise, methodological wisdom, and critical and constructive insight are to be found outside the boundaries of scientific communities powerfully captures the rationale for collaborative practice that is illustrated by but extends well beyond the archaeological examples I have considered here: that it is not only morally, politically, and sometimes legally the right thing to do, but that it has substantial capacity to improve the research, sometimes as measured in quite conventional terms against established goals, and sometimes through a process that involves critically reassessing these goals and standards and the aligned conventions of established disciplinary practice.

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Further Readings

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