Fields of Commitment: Research Entanglements beyond Predation

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Abstract: *The boundaries of fieldwork not only define the scope of research but also circumscribe and delimit the bounds of responsibility. This essay proposes a return to the where-ness of the field as an antidote to treating the powers of description and historical dispersal as absolute and uncontested. Linking classic critiques of social science's mapping of nature and culture, of the authors and subjects of research, to contemporary debates about the ethics of field research and anthropology's complicity with colonial systems of rule, it offers a reappraisal of the field as a ground from which to build new solidarities across incommensurable political and scholarly commitments. By approaching fields not as empty retainers but as comprised of and defined by research interlocuters and their politics, scholars can better account for global slippages and dispersals without subtly reviving the figure of an inert nature under duress, in/organic or otherwise.*

How do the boundaries of fieldwork—often known as "the field"—not only define the scope of research but also circumscribe and delimit the bounds of responsibility? This essay proposes a shift to such fields of commitment centered upon the entanglements that bind researcher and researched while also co-defining each in ways that neutralize fantasies of unmitigated access. Parting ways with poststructuralist critiques of subjectivism and objectivism alike, I propose a return to the where-ness of the field as an antidote to treating the powers of description and historical dispersal as absolute and uncontested. This requires fostering attunements to relations that exceed the facile positing of an object of study but stop short of imagining the domain of expertise, and global distribution, as limitless and without obstruction. Collaboration and compromise, rather than protection or predation, can offer routes of ethical relation that do not reproduce the model of the ethnographer as savior or intellectual vanguard who alone guards against absolute loss.

Fieldwork, or human research as unfolding in a field of inquiry, spans back at least to the late nineteenth century. Writing in 1871, Edward Burnett Tylor specified the distinctiveness of a "field of inquiry narrowed from History as a whole to that branch of it here called Culture, the history,

not of tribes or nations, but of the condition of knowledge, religion, art, custom, and the like among them" (5). Borrowing from natural history, including efforts to draw organic and inorganic nature into a comparative, evolutionary frame, Tylor insisted that "Culture" too could be traced through various stages and fields. Hence, "[j]ust as certain plants and animals are peculiar to certain districts, so it is with such instruments as the Australian boomerang, the Polynesian stick-and-groove for fire-making, the tiny bow and arrow used as a lancet or phleme by tribes about the Isthmus of Panama, and in like manner with many an art, myth, or custom, found isolated in a particular field" (8). Field here connotes the where of evolutionary culture, the more modest and particular place where "inorganic nature" can be classified and its laws of cause and effect recognized (2).

Taking stock of this genealogy of fieldwork as a distinctive method and episteme of knowledge of Culture begs the question of whether there is something recuperable about this method. If the notion of a field as an object where research unfolds has historically been premised upon the colonial-era collapsing of landscapes, tools, and non-Western peoples, as Tylor's words lay bare, is fieldwork worth defending? Is there something distinctive about such a method that could stand up against charges of obvious ethnocentrism? Can the slippages of subject and object, of researcher and researched, be rethought not as lines to be guarded but rather as a domain of relation that could afford a new, arguably urgent, reorientation to research at large (TallBear)? My approach emphasizes forms of accountability that emerge out of groundedness in a specific place of research. Rather than holding fast to the refusals of commitment that have defined the ethnographic method, could the field be reoriented as privileged sites of competing commitments, alliances, and compromises? Following TallBear, I call for "standing with" interlocuters, not as passive objects to be surveilled, but as political actors whose demands transform the research endeavor. Reorienting ethnographic research toward "compromise" can allow scholars to navigate field obligations in light of incommensurate ethical and political commitments while nonetheless remaining grounded by and accountable to specific places and research partners (Liboiron 2021; Tuck and Yang 2012: 35).

Questions about the ethics of "the field" and of "fieldwork" continue to spill beyond academic debates. On January 14, 2023, for instance, National Public Radio reported that the University of Southern California (USC) was to remove the word "field" from its curriculum, as well as named buildings on campus. USC's School of Social Work decided to rename the Office of Field Education as the Office of Practicum Education. The article cites a USC memo as explanation: "This change supports anti-racist social work practice by replacing language that could be considered

anti-Black or anti-immigrant in favor of inclusive language" (Heyward). It continued, "Language can be powerful, and phrases such as 'going into the field' or 'field work' may have connotations for descendants of slavery and immigrant workers that are not benign" (Heyward). The decision received mixed support, including by USC students. Students reportedly told the campus newspaper *Daily Trojan* that "they were unsure whether the term 'field' truly had racist connotations" (Heyward).

This essay attends to the history and temporalization of "the field"—whether absolutely colonial in its trappings, or perhaps rather a response to colonial hubris and colonial destruction—in order to rethink its fraught ethics as a methodological *where* (Malinowski; Boas; Simpson, "Why"). Part 1: Culture as a Field of Inquiry attends to the transformations of the field and field methods in the wake of early social science and "ethnological" critiques of arm-chair anthropology. I then consider how the project of "salvage anthropology" was rooted in a recommitment to the field: to take stock of the losses of colonial expansion and cultural displacement, anthropologists would have to go to the field and collect the artifacts of destruction (Boas).[1] As George W. Stocking (212) points out, with Boasian anthropology the humanist view of culture as absolute, progressive, and singular became the plural cultures of modern anthropology. In doing so, he highlighted difference as indicative of cultural plurality rather than developmental inferiority. His work the relative "plasticity of human types" sought to discredit racial (hierarchical) formalist approaches which presumed that race could be linked to mental capacity (Stocking 1982: 170, 194). In staging this intervention, US ethnographers like Boas often appeared as virtuous proponents of cultural relativism, contravening racist and racialized depictions of non-Western peoples as incapable of adaptation and change.

As Audra Simpson has argued, that project of salvage went hand in hand with a "grammar of Indigenous dispossession" ("Why" 166). She points out that even as U.S. anthropologists sought to recover the rubble of colonial destruction, they also erased that destruction and their complicity in it to appear as sympathetic allies uniquely positioned to collect and record the shards of dying cultures. This was achieved in part through a definition of culture that suspended researchers' own complicity and embeddedness in the historical formations they were ostensibly only studying. In this move, the political appeared as objectlike—as systems of governance and hierarchy that could be named and sorted (Simpson "Consent's"). But the ongoing settler colonial violence that led to the fragmentation of traditions and elicited Indigenous efforts to revive religious and political systems in order to reclaim sovereignty over people and land, were left uninterrogated; this violence constituted a condition of possibility for the anthropological pursuit, it was, to use Edward Said's

language, what allowed the researcher *to be there*.[2] And all this required and took place through a re/turn to the field. As contributors to this volume make evident, such questions of complicity and critique take on renewed urgency in the context of scholarly engagements with climate disasters the world over. Disasters like these urgently require new modes of scholarly attention and attunement. However, that attunement also risks further ensnarement in the fieldwork heroism and settler innocence against which Simpson warns.

Given the yoking of field methods and Indigenous and Black dispossession, what of anthropological and social scientific methods? Whither fieldwork? What of the where of research? Part 2: Ethnography as Theory examines efforts to recast ethnography and the place of fieldwork in it. I focus on debates about the end of ethnography, including calls for methodological innovations that part ways with fantasies of objectivism that continued to guide ethnographic approaches to the field into the early twenty-first century (Clifford; Rosaldo; Ingold). While some scholars have called for abandoning the presuppositions of field and fieldwork, others have turned instead to a rethinking of ethnography as a mode of grounded theory that flourishes within the uncertainties and slippages of subject and object, researcher and researched (Haraway, "Situated"; Nader; Bonilla and Rosa; Marcus). What if researchers' milieu is the field? What of digital ethnography? What of ethnographic approaches to fields that have no discrete where, such as ethnographies of world systems or global surveillance or the dispersals of matter responsible for climate change?

With Part 3: Fields of Commitment, the essay closes by asking whether the field is overdetermined by its colonial origins. In dialogue with efforts to account for grounded sites (and fields) of refusal (lewallen), can the where of research be redeployed not to shore up discrete notions of place, ethnos, race, or objecthood but rather as an insistence on answerability to the political and ethical concerns that saturate a given problem-space at a given time. Doing so takes us a step beyond the ethics of witnessing, which maintains the observer's partial distance and authority of moral judgement (Behar; Huang), to ask rather about vulnerabilities and grounded commitments that underlay all research, whether they are conceded by the researcher or not. While attunement to how research interlocuters' concerns disrupt liberal formations of subjectivity and justice has been a key insight of critical ethnography (Mahmood), one that takes us beyond abiding tendencies to dismiss interlocuters' opinions and perspectives as suspect,[3] collaboration additionally points to modalities of inquiry that supplant a version of fieldwork based on the researcher's exemplary spatial, relational, and political distance from the field.

What answerability is opened by rearticulating, rather than abandoning, the field as such a scene of commitment?[4] In closing, I propose a reorientation to the field, and fieldwork, that begins from the premise that interlocutors' practices and activities articulate their own conceptual stakes. What kind of research unfolds from relations of "standing with" (TallBear) interlocutors, both as a spatial situatedness in a shared texture of relation and as a commitment to write from the place of that entanglement, what I elsewhere call the researcher's "knotting" into the research (Winchell, *After*)? How can the claims that research interlocuters place upon scholars be accounted for not just as a retrospective process of giving-back but rather as integral to fieldwork design? This moves us toward understanding theory as already immersed in worlding practices in ways that do not depend for their revelation on the ethnographer's magic (for instance, as the conjuring of theory from raw data, or as an intellectual or political vanguard). Allowing this slippage of field and theory into research holds the power to reframe scholarly commitments, disrupting tendencies toward depoliticizing the field as an expression of timeless Culture or, more common today, as an inexorable outcome of a corrosive, late capitalist present.

Part 1: Culture as a Field of Inquiry

For many social scientists, it seems obvious that "the field" is not natural: it is not inert matter but rather something generated in part through the activity of research. But this has not always been so, and indeed contemporary researchers, especially in the fields of geography, science and technology studies, and environmental anthropology, have pushed for a return to the nonhuman as a site of research. Tylor, discussed in the opening paragraph above, proposed the field to methodologically specify the study of "inorganic nature" by grounding philosophies of history in each site of inquiry. Culture here is not necessarily coterminous with tradition, but it is still singular: *Kultur* as an evolutionary arc of knowledge whose movement through stages anticipated and confirmed the exceptionality of modern man. This produces a conundrum: if "the field" can be extended to account for "inorganic nature," how to distinguish that nature from that of the ethnographer? Or, put differently, if there is already more than one nature (organic and inorganic) and culture (here *Kultur*) is not universally shared among humans (Latour), how to distinguish object from subject, researched from researcher, in the tangle of an emerging ethnological research design?

In fact, if human relation is not *a priori*, then intimacy becomes a problem for the ethnographer as the scientist of inorganic nature. Hence,

Malinowski recounts: "I remember the long visits I paid to the villages during my first weeks; the feeling of hopelessness and despair after many obstinate but futile attempts had entirely failed to bring me into real touch with the natives, or supply me with any material" (4). Fieldwork is premised on the field not only as culture but also as a practice of being "close to a native village" as well as (frustrated) efforts to get "into real touch with the natives" (4). Eventually, of course, Malinowski discovers the "secret of effective field-work," what he calls the "ethnographer's magic" (6): "As usual, success can only be obtained by a patient and systematic application of a number of rules of common sense and well-known scientific principles, and not by the discovery of any marvellous short-cut leading to the desired results without effort or trouble" (6).[5] The ethnographer's magic hinges on his capacity—not only as ability but also as authority—to insert himself within "touch," physical and relational, of "the natives." The shared culture that could be posited through such ethnographic work relied upon predacious forms of imposed sociality and touch. Unsurprisingly, Malinowski's piece reads like an anthropologist's coming of age story, in which adolescent frustrations and childish faux pas give way to successful fieldwork.[6] Later he writes: "With this, and with the capacity of enjoying their company and sharing some of their games and amusements, I began to feel that I was indeed in touch with the natives" (8). Mimicking this training of sensibilities of conduct, Malinowski descries learning to apply "deeper conceptions and discarding crude and misleading ones," thereby "moulding his theories according to facts" (9). Fieldwork arises as transformation, both of the ethnographer's bodily dispositions and theoretical attachments. Thus, he concludes, "the field worker relies entirely upon inspiration from theory. Of course, he may be also a theoretical thinker and worker, and there he can draw on himself for stimulus. But the two functions are separate, and in actual research they have to be separated both in time and in conditions of work" (9).

Malinowski's insights reveal how the study of non-Western people as "inorganic nature" is forged through the positing of the field not just as an empirical where, but as a site of the white man's transformation: his reduction to childlike ignorance and his eventual formation as a different kind of person. Despite this, Malinowski defines the field as atheoretical, as a place of "rules and regularities" that must be "soberly" attended to (ii). Ethnography requires fieldwork be "taken up by men of science" (12) who commit themselves to the "collecting of concrete data" (13). In this way, the relational components of fieldwork—of imposed intimacy and refuted touch alike—fall away in lieu of a more materialist definition in which people are figured mainly through the idiom of a place: as elements of geography. Critical attention to this geographic formation of difference as achieved through the positing of "the field" is especially important

today. Ecological debates are defined by climate change denialism and opposition to science both from the right and left, leading some scholars to explore alternatives to the poststructural critique of empiricism (Green). Moreover, in the turn to empiricism in the study of climate change, social scientific narratives at times erase oppositional subjectivities by recentering ruinous geographies in new materialisms. Here, as Max Ajl discusses, fields or rural hinterlands emerge as solutions to unsustainable urbanism. In this move, do unpeopled landscapes—the field as inorganic nature—slip back into our methodologies? What fantasies of access and capture underwrite such methodological designs and desires?

Since its early enunciation, appeals to a science of the concrete presumed fieldworkers' access both to the structure and to the spirit of the studied. Alongside collecting information about rules and regulations, tribal constitution and structure (what Malinowski calls the "skeleton"), the anthropologist has also to glean its "flesh and blood" or spirit: "the natives' views and opinions and utterances" (22).[7] This worried Malinowski, who thus asked whether this is possible given that "certain psychological states" cannot be put into words by actors themselves (22). "Without trying to cut or untie this knot, that is to solve the problem theoretically," Malinowski turns to the "question of practical means" to overcome these difficulties. The question of knowability—of whether the white ethnographer can truly get "in touch" with the natives—is resolved through and as method. Ethnography, and scientific fieldwork in particular, offers the answer. The opacities of native life to ethnographic transparency are to be resolved through fieldwork, particularly by the white ethnographer's forced physical and relational insertion into the field of the researched. This move erases the violent, colonial force that underlays such a method, instead celebrating the virtues of empiricism as an exemplary attunement to the object of study. Empiricism, even or precisely where shot through with "affective impulses" to arrange and order culture (Bunzl 17 citing Boas), offered a language by which to naturalize fieldworkers' authority: their ability to be there.

In her critique of Franz Boas and his fetishized place within American cultural anthropology, Audra Simpson challenges Boas's *The Mind of Primitive Man* not as liberating Indigenous peoples from colonialism but rather as establishing a "dualistic binary regarding the value of cultural and bodily differences and their presumed vitality and value as well as their suitability for state and settler absorption" ("Why" 167). This binary determines how lines are drawn between "who will live and who will die within a new political state: who will be worthy of salvage, sympathy, and, ultimately, incorporation—enfranchisement and equality" (167). Boas works within the tide of the destruction of Indigenous life, which he sees as inevitable, a foregone conclusion. By positioning himself

as an ally who recovers or salvages shards of culture before they are lost, Boas conceals that he "worked in concert with a settler state that sought to disappear Indian life and land in order to possess that land and absorb that difference into a normative sociopolitical order" (167). Simpson brilliantly clarifies what Malinowski conceals as the "scientific" method of fieldwork, premised upon proximity to "the natives." Not only is this method abetted by colonial economic and political pursuits in those places, but ethnography itself in some ways buttressed the project of erasure. It promised to recover what was taken as valuable—the native's place as illuminating global patterns of culture and human adaptation—thereby making ethnocide less appalling to Western eyes, as lost objects could nonetheless be catalogued and classified to advance Western scientific knowledge (Tuhiwai Smith). Proximity of method is also the proximity of colonial power, and empiricism is possible because "the natives" cannot refuse the "touch" of both colonialism and colonial era anthropologists. But where such touch dissolved into acculturation, anthropologists like Boas had little interest: it was only in their "primitive" state, and certainly not as politicized actors mobilizing for the survival of their traditions or political orders, that anthropologists took interest (Simpson, "Why" 175).

Even as ethnographers reveled in the promise of proximity to the native, natives' proximity to ethnographers emptied them of their "value" to Western science. This secured the ethical claims to ethnographic distance as empiricism while allowing ethnographers like Boas to dispense with unsavory topics of resistance and refusal, both of anthropology and colonialism. Indigenous opposition to the ethnographer's (imposed) magic and to the accompanying infiltration of (settler) colonial projects of land dispossession and forced assimilation alike could thus be framed as outside the scope of inquiry. By appealing to an ideal of ahistorical culture or tradition, ethnographers like these dismissed interlocuters' assessments of the stakes of their own practices as insignificant, as external to anthropology. This acted to close down obligations, but also reaffirmed the ethnographer and his field, culture and (inorganic) nature, as objects in the world outside of the dynamics of forced access and intimacy guiding ethnographic research.

Part 2: Ethnography as (Field) Theory

The integrity of the "field" of research and its relation to specific fields of inquiry, especially anthropology, has faced robust critique since at least the 1970s. Questions have emerged about the relation of fieldwork to colonial geography, "field studies," geopolitical hierarchies and state violence, and

the slippages of the virtual and the real, the digital and the material, as sites of inquiry. These debates might be read as neurotic turns toward self-reflexivity and doubt about the discipline, but they can also be reread as points of insight into shifting ideas about ethnography and, or as, a kind of (field) theory. What kind of a field does ethnography produce? What ideas of theoretical production within or after fieldwork undergird such methods?

In "Some Reminiscences and Reflections on Fieldwork," Evans-Pritchard challenges ideas of the absolute separation of theory and "data." As he writes, "what one brings out of a field-study depends on what one brings to it" (2). Moreover, while "the layman's [pre-conceived ideas] are uniformed, usually prejudiced, the anthropologist's are scientific" (2). That is, they are biased by theoretical dispositions. This was not so much lamentable as a condition for research in the first place, for one "cannot study anything without a theory about its nature" (2). At the same time, Evans-Pritchard insists that one "must follow what he finds in the society he has selected to study" (2). This requires researchers to "live the life of the people among who they are doing their research" (3). And even as one "remains oneself" (3) one also comes to "believe" what one encounters.[8] By "entering into the thought of another people," the anthropologist is "transformed by the people they are making a study of, that in a subtle kind of way . . . they have what used to be called 'gone native'" (5). This theory of self-transformation through an encounter with alterity constitutes a theory of the field that has been definitive of modern anthropology as an ethnographic activity. Yet this narrative of rapport-building and self-transformation has often elided the fact that ethnographers rely for their experience upon the servitude and labor of native informants. Evans-Pritchard admits that he "relied mostly on my two personal servants and on two paid informants" (6). Care must be taken in selecting such labor, for "it is only a particular sort of person who is prepared to act in this capacity, possibly a person who is ready to serve a European as the best way of escaping from family and other social obligations" (6). Informants could also be sneaky and subversive, prevaricating on "secret matters about which an informant does not wish to speak" or pretending "to know nothing about them" (3, 6). At the same time, he is aware of what he terms an "entanglement" with colonialism, specifically missionary violence. In this regard, the anthropologist, like the missionary, is "part of what he is supposed to be studying" (8).[9]

This ensnarement in research would seem to go against the ideas of *a priori* fields described in the preceding section. How, then, to reconcile these two contrasting impulses? We have, on the one hand, the positing of a field of transformation that draws in and remakes the anthropologist as subject, believer, person, and that relies on what are

taken as optimal mediators, those who themselves occupy marginal positions vis-à-vis their culture and hence are well-positioned to accept positions as servants for white anthropologists. On the other hand, there is the insistence—here on the part of Evans-Pritchard—that this field cannot and does not exist outside of the "total entanglement" of the researcher and the researched, but also anthropology and the colonial and missionary encounter.[10] Indeed, he notes that in Kenya, where anthropologists were loathed just as British officials and settlers were, it was "difficult for a white anthropologist to gain their confidence" (11). Downplaying the imposed nature of such entanglement, he asks: "Why should anybody object since one does no harm and is a guest?" (11). Moreover, Evans-Pritchard implicitly defends this activity for its salvage potentials. What is not written down is "forever lost—the picture of a people's way of life at a point of time goes down into the dark unfathomed caves" (12). Despite opposition and against local hostility, he defends fieldwork as a method of inscription against loss.

Erasure of the violent conditions of research through an appeal to a naturalized field have faced robust critique. Among other works, George Stocking's *The Ethnographer's Magic* challenged the idea of ethnographic fieldwork as an ahistorical and atheoretical methodological exercise that makes anthropologists what they are (see Gupta and Ferguson 1). Nonetheless, Gupta and Ferguson have argued that the "idea of 'the field' . . . remains a largely unexamined one" (2). Despite robust critiques of notions of culture and ethnography as a genre of writing about it, "the field" as "the place where the distinctive work of 'fieldwork' may be done, that taken-for-granted space in which an 'Other' culture or society lies in waiting to be observed and written . . . has been left to common sense" (2). Against that naturalization, the authors insist that the field is complicit in notions of locality whose spatial and conceptual policing secure territoriality not just as the methodological where of ethnography but as a value system that implicitly sanctions the violence that produces it: the "field is a clearing whose deceptive transparency obscures the complex processes that go into constructing it" (5).

The authors link the emphasis on territoriality to anthropology's origins as a field science, what I have insisted is the production of some forms of human life as "inorganic nature." Drawing on Henrika Kuklick's *The Savage Within*, Gupta and Ferguson write: "Like other 'field sciences,' such as zoology, botany, and geology, anthropology at the start of the century found both its distinctive object and its distinctive method in 'the detailed study of limited areas'" (6). Echoing languages of primatology, those "living outside their native state" were "less suitable anthropological objects because they were outside 'the field'" (7). In this way, ideas of appropriate sites of "the field" reveal "unspoken assumptions of

anthropology" (8). Yet, I disagree with the idea that this where has been atheoretical or merely a matter of "common sense" or "unspoken assumptions." In the writings of Tylor, Malinowski, and Evans-Pritchard, the field might appear as *a priori*, merely some vague where of a colonial country that is optimal for fieldwork, but it does contain a theory. The field must be available to the researcher. It must not offer dramatic resistance to the ethnographer's magic as proximate touch. And it must be sufficiently different, that is, capture some sort of "pure" primitivity that thereby allows the anthropologist to escape charges of complicity for colonial violence and globalized acculturation (Simpson "Consent's"; Appadurai 191). At its broadest, then, modern fieldwork relies upon an epistemic faith in an empirical outside that good ethnographers can separate from inherited theory and from their own grounded cultural sensibilities. That exposure produces not only a researcher but also a kind of liberal, relativistic subject that comes into ethical being through an encounter with (racialized) difference.[11] All of this is definitive of ethnography as a theory of the field.

In fact, while territorial approaches to the field have been robustly critiqued (Haraway, "Situated"; Bonilla and Rosa; Marcus), the broader appeal to fieldwork as an empirical correction to abstract and ungrounded theories—and theories of the political—remains strong (Nader; Mahmood), leading to continued methodological calls to align figure and ground (Fortun 2017).[12] Alongside rethinking territoriality, there has been an insistence on positioning the ethnographer as part of the formations they were previously thought only to study (Collins; Haraway, "Situated").[13] Like Evans-Pritchard's emphasis on "entanglement," such approaches expose the researcher but they have not always asked how the broader commitments of anthropology can follow suit. For instance, Gupta and Ferguson propose a rethinking of fieldwork as purposeful "dislocation" based on "interlocking of multiple social-political sites" (37). They also call for ethnographic attention to "acculturation" (21), to global processes such as diffusion and destructive change (20), and for examples of "action anthropology" (24). Such studies would rethink the field not as "bounded localized community" but rather as "a multistranded transatlantic traffic of commodities, people, and ideas," or what Fernando Ortiz, quoted in Gupta and Ferguson, calls "intermeshed transculturations" (28). Against boundedness, they argue, the field should be treated as expansive and imploding, mimicking the traffic and flows of global systems (Choy et al.; Marcus).

While this recasting of the field to include global flows and slippages has been celebrated as new, it in fact inhabits a similar conceptual foothold to earlier salvage anthropology. The field is in a state of loss and disorder, and it was the fieldworkers' job to try to fix it or, where

that was impossible, to record the loss. Like narratives of cultural ethnocide (Kaunui, Simpson "Consent's"), shifting the field from a bounded state to a site of inevitable acculturation risks naturalizing a set of violent dislocations and dispossessions as mere qualities of a "global" present. This suggests there might be something worthwhile about retaining an orientation to the field that does not take for granted and thereby neutralize a telos of flows. Instead, we must be attentive to where and when such traffic occurs and to the normative stakes of such dis/locations from the position of research interlocuters. Even in digital spaces and with online protest movements, actors inhabit material and relational worlds that shape their politics (Bonilla and Rosa). How to account for forced flows as well as the refusals of movement such flows elicit, including grounded efforts to stave off the conversion of places into land that is available, or disposable, not only to capital but also for climate action?

By fetishizing the field, anthropologists have claimed for themselves not only regional expertise that operates to naturalize "cultural difference as inhering in different geographical locales" (Gupta and Ferguson 8; Strathern) but also, more broadly, a form of mastery over culture imagined as the "inorganic nature" of non-Western worlds. But a question remains: Does this split remain intact today? Present scholarly interest in mapping out the ruinous landscapes of late capital often leads ethnographers instead to narrate locales imagined as thoroughly mediated by capital, climate, histories of plantation violence and monoculture, or other expressions of forced acculturation or global toxicity. There can be no discrete field anymore. Or if there is, it is a field that needs to be made available to offset carbon emissions or for renewable energy (Ajl). In this scene, efforts to posit absolute where-ness may seem naïve, romantic, stilted, backward. But where does that leave ethnographers? How does one map not only radiating leakages—those presumptions of land's disposability that underwrite capital and green alternatives alike (Liboiron; McCarthy)—but also people's abiding insistence on locality and on bounded where-ness as a mode of contesting such unwilled intimacies? How does the erasure of the field also erase possibilities for accountability that do not take the global as their frame or referent? And how might an implosion of the field (Dumit) unwittingly facilitate the proliferation of new abstractions: to whom is anthropology accountable when it dispenses not only with the possibility, but with the very existence, of an unmediated ground?

Part 3: Fields of Commitment

Kim TallBear has suggested that ethnographers think past the imperatives of "giving back" or of reciprocal exchange, notions that rely on a binary understanding of researcher and researched, and, with it, firm boundaries between "those who know versus those from whom the raw materials of knowledge production are extracted" (2). This means that a researcher is not only willing to "stand with" a community of subjects but also "to be altered, to revise her stakes in the knowledge to be produced" by virtue of that standing (2). Within this vision, research questions, subject populations, and knowledge production inhabit a "shared conceptual ground" (2). TallBear's provocation powerfully intervenes in fieldwork as an empiricist paradigm based on bifurcating raw data and (theoretical) knowledge. Instead, scholars should participate in research defined by a "co-constitution of one's own claims and acts of the people(s) who one speaks in concert with" (4). Moreover, as Sophie Chao insists, "ethnographically grounded examinations," including of the patchiness of plantations, urgently intervene in the inexorability implied by theoretical abstractions, including of the plantation as ideology (169).

TallBear and Chao's interventions urge a return to ground that resonates with feminist critiques of distance as the methodological standard for empirical research (Haraway, *Simians*).[14] In lieu of celebrating this gap or turning away from grounded sites of struggle, Max Liboiron has asked about "compromise" as that which emerges when you have "obligations to in-commensurabilities" (136; Tuck and Yang), such as to an anticolonial science as a project of moving forward "with, in, and around impossible bedfellows" (137). What do such difficult and overlapping commitments mean for anthropological ideas of "the field"? How might a field as a set of recurrent relationships across varied obligations entail a weaving or "knotting" of researcher and interlocuters (Winchell, *After*), rather than a discrete field that the researcher enters and exits, perhaps to return through future visits or promises of "giving back"? How to allow these webs of knowledge production, in which theory or knowledge is not discovered by the researcher after the fact but rather braided into research design and interlocuters' speech and practices, to reshape ideals of objectivity (raw data) and territoriality (locality, region, ethnos), giving way instead to fields of commitment? Such fields illuminate contemporary formations of devastation and loss as shot through with alternate scenes of attachment and grounding that can be mobilized to push back against abstracting narratives of planetary apocalypse.

In an article about multispecies ethnography, Kirksey and Helmreich call for attention to "becomings" as "new kinds of relations emerging from nonhierarchical alliances, symbiotic attachments, and the mingling of creative agents (546). These are "contact zones where lines separating nature and culture have broken down" (546). But, as the

authors indicate, this slippage is hardly new. Lewis Henry Morgan, too, worked "across boundaries later secured against traffic between the social and natural sciences" (Kirksey and Helmreich 549), and the Humboldt brothers, often credited as the creators of field-based social sciences, sought to extend a natural science model to non-Western peoples and landscapes. The risk, then, is that the "savage slot" is smuggled back into efforts to rethink fields, as anthropologists search for new frontiers of alterity—"alterworlds of other beings" that have not (yet) been narrated as fully entangled with human socialities (553). Hence, the authors ask: "How can or should or do anthropologists speak with and for nonhuman others?" (554). How are fields defined by forms of ventriloquism that can only succeed where their interlocutors are treated as fundamentally mute, as incapable of articulating their own commitments? Here, "[n]ature begins to function like 'exotic' culture" (qtd. in Kirksey and Helmreich 562).

This essay has taken up this problem of compromise to examine the challenge of combining accountability to discrete places and their politics on the one hand, and historical attunement to the violent production of the field as the production of difference and indifference (the refused accountability to the predations that have historically defined field research) on the other. These are problems for which there are no easy solutions. But I have emphasized the urgency, and difficulty, of reconceptualizing fields in ways that do not reproduce either naturalized telos of acculturation, ethnocide, and contact, or the hubris of the sympathetic anthropologist who is willing to risk life and limb to be transformed by the field even while retaining a privileged position as the defender of or spokesperson for such alterity. Following Berry, Argüelles, Cordis, Ihmoud, and Estrada, there is a need to develop a "decolonial research praxis that advances a critical feminist ethos" (538). This ethos requires "flight from an intellectual garrison, in which the idealized radical subject within leftist struggles figures as a martyr for the movement," an ideal of a "self-sacrificing subject [that] coincides with the institutionalized notion of fieldwork as a masculinist rite of passage or an exercise of one's endurance" (Berry et al. 538). Such a model continues to reproduce the idea of the fieldworker as savior, if not in a salvage role then as an agent of radical political transformation.

I have suggested that we begin instead from the premise that interlocutors' practices and activities carry their own conceptual stakes—they are doing theory. Theory, then, does not depend on the ethnographer or his distance from or transformation by the field. The trick is to allow this slippage of commitments, the theoretical stakes already built into a given set of practices, into research design as a recurrent threading rather than entry into and exit from a bounded field. These recurring commitments neither begin or end with a writing project nor do they depend upon

academic outputs alone as a measure of good or bad relation. Instead, they enable ongoing collaboration, compromise, and indeed refusals to collapse multiple obligations or to assume that the researcher's political stakes must or even can map onto those of interlocutors.[15] In ongoing collaborative research about how Chiquitos ancestors inhabit landscapes ravaged by Bolivian wildfires, I have had to reassess the idea that climate change is experienced as thoroughly mediated by the global, and that the planetary is the only world toward which actors (and ancestors) must be accountable (Winchell, "Climates").

Where an earlier field method relied upon ethical claims to ethnographic distance to dispense with the violences of anthropology and colonialism, fields of commitment recenter the difficult and at-times uncomfortable alliances of researcher and researched. These are spaces that do not exist naturally, as relational counterparts to empiricist approaches to the field, but rather are created through recurrent methodological vigilance and conceptual compromise. To engage in this work is to take stock of the field's constitutive haunting by colonial-era field methods and epistemic faith in an odd mixture of nominal distance and forced intimacy (Gordon). This reorientation to the field interrupts an instrumental approach to methods as "tools" standing outside of prior commitments and ongoing entanglements. It was this assumed separation of content and form, of instrument and knowledge, that allowed anthropologists like Evans-Pritchard to recognize their entrenchments in ongoing colonial histories of violence while also defending fieldwork as a methodology innocent to that violence (Tuck and Yang; Berry et al.). To "stand with" builds answerability to such concerns not only into what researchers do, including field methods and collaborations, but also into broader interdisciplinary debates about what research is, and why and for whom its pursuit matters.

Standing with is not a project that affords a smooth synthesis; the just cannot be imported as an empty metaphor but rather must be gleaned from a specific field of political practice, one that is often disruptive, unsettling, and incompatible with more universalist, rights-based definitions of emancipation as awakening. As Tuck and Yang (2012: 28) write, "These are interruptions which destabilize, un-balance, and repatriate the very terms and assumptions of some of the most radical efforts to reimagine human power relations. We argue that the opportunities for solidarity lie in what is incommensurable rather than what is common across these efforts." Forging scholarly answerability to anthropology's complicity in histories of violence by building such "opportunities for solidarity" across the incommensurable requires rethinking inherited distinctions of nature and culture, of the authors and objects not only of research but of the global histories to which fieldwork

belongs. Solidarities like these cannot dispense with the affordances of the field as the limit or obstruction to the temptations of universalism that define research and politics alike. Approaching fields not as empty retainers but as made up of and defined by research interlocuters and their politics can allow for forms of solidarity that account for global slippages and dispersals without subtly reviving the figure of an inert nature under duress, in/organic or otherwise.

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[1] As Matti Bunzl (1993: 24) points out, it would be simplistic to view the "culture turn" as a mere product of Franz Boas' arrival and subsequent correspondences with leading evolutionary anthropologists in the U.S. at the time. Boas was heavily influenced by German Romantic thinkers like Wilhelm von Humboldt and Johann Gottfried von Herder. Humboldt's influence on is evident in Boas' work on Inuit languages. He argues that linguistic practices reveal the social/psychologized nature of a people (for instance, the multitude of words for snow that he attributed to them). Rather than insist that non-European cultures "adopt the standard of 'European civilization,' especially in the face of the 'unimaginable suffering' that had been brought upon the Naturevölker when exposed to 'our cultural standards,' German anthropology at this time was influenced by Herder's humanistic relativism (Bunzl 46).

[2] As Edward Said (15) wrote, "The scientist, the scholar, the missionary, the trader, or the soldier was in, or thought about, the Orient because he could be there, or could think about it, with very little resistance on the Orient's part."

[3] For instance, Malinowski (11–12) commented about the challenges of "depicting the

Constitution" of Trobriand society, given that "the 'natives obey the forces and commands of the tribal code, but they do not comprehend them'" (cited by Sillitoe 2). As Paul Sillitoe (1) points out, "It seems odd that an anthropologist should declare that he could not engage with what was of interest and concern to the people he lives with, because it is not relevant from his research perspective." Why has a discipline that claims to further understanding of other cultural ways produced "work in which the subjects themselves cannot recognize their behaviour or ideas"?

[4] See Bharat Venkat's insistence on commitment as constitutive of ethnography.

[5] These include three central "principles of method" including "real scientific aims," "good conditions of work" based on living "without other white men, right among the natives," and finally, "special methods of collecting, manipulating, and fixing his evidence (6).

[6] "Over and over again, I committed breaches of etiquette, which the natives, familiar enough with me, were not slow in pointing out. I had to learn how to behave, and to a certain extent, I acquired 'the feeling' for native good and bad manners" (8).

[7] For, rather than being separate, these "ideas, feelings, and impulses are moulded and conditioned by the culture in which we find them, and are therefore an ethnic peculiarity of the given society" (22).

[8] "In their culture, in the set of ideas I then lived in, I accepted [Zande notions of witchcraft]; in a kind of way I believed them" (4).

[9] "I am not going to pursue this matter further now beyond saying that in the end we are involved in total entanglement, for having chosen in a native language a word to stand for 'God' in their own, the missionaries endow the native word with the sense and qualities the word 'God' has for them" (8).

[10] In fact, Evans-Pritchard attends to what he calls "a hostile attitude to anthropological inquiries" in (non-Western) countries where "there is the feeling that they suggest that the people of the country where they are made are uncivilized, savages" (9).

[11] Gupta and Ferguson eloquently describe this paradigm, drawing from Kuklick, in terms of "Romantic notions of (implicitly masculine) personal growth through travel to unfamiliar places and endurance of physical hardship (17). For questions of racial fixing and fetishization, see also Trouillot and Tuhiwai Smith. On the refusal of ethnography, see Simpson (2014).

[12] For a critical review, see Ingold.

[13] See also Behar; Bird; Jacobs-Huey; Pels.

[14] This critique of distance between researcher and researched, theory and data, belongs to what TallBear calls a "feminist objectivity" that emerges from co-habitation and from recognizing the conceptual and theoretical stakes of the activities of research interlocutors.

[15] Max Liboiron (*Pollution*, 31) points to this challenge of the shared: "how do we write and read together with humility, keeping the specificity of relations in mind? How do we recognize that our writing and reading come out of different places, connections, obligations, and even different worldviews, and still write and read together?" Compromise arises as one answer to this question of how, approached not as a limit but as an invitation or opening to experimentation with new relations across divergent political and epistemological commitments.