Perspectives

Newsletter of the ASA Theory Section

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A CRITICAL TRIBUTE TO DOROTHY E. SMITH

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In 1992, Barbara Laslett and Barrie Thorne organized a symposium in Sociological Theory with the aim of tearing down a "wall of silence" between feminist theory and the mainstream of sociological theorizing. For help the editors turned to the work of Dorothy E. Smith, who over the preceding two decades had established herself as a renowned theoretician and methodologist, having published writings that spanned intellectual traditions from phenomenology to Marxist materialism. The esteemed panel of commentators—Raewyn Connell, Patricia Hill Collins, and Charles Lemert—warned that sociology overlooked Smith and feminist theory more generally at its own expense.

Smith's theorizing today carries even greater appeal, having expanded from a sociology

for women to a sociology for people. This wider scope never sacrifices her project's theoretical versatility and nimbleness, and disdain for abstraction. Smith's view of the social is at once interpretivist (stressing historical standpoints but spurning Weberian ideal types), structuralist (emphasizing how experiences emerge from specific historical and social conditions, but are not determined by fixed categories), and antiessentialist (rejecting postmodernism's denial of the subject in favor of a view of people as agentic). Yet at the heart of Smith's sociology is a commitment to beginning with the concrete experiences of people in the work we do. This means Smith's "theory" of the social world is more accurately "a skill, a how-to, rather than a content" (Smith 1997:820). In offering a critical tribute to Smith, who passed away in June 2022 at the age of 95, Sociological Theory has organized a symposium with essays from three scholars—Paige Sweet, Rebecca Lund, and Marjorie DeVault— which will appear in the December 2023 issue.[1]

Dorothy Edith Smith grew up in rural Northern England in the years leading up to World War II. At the close of the war, Smith worked in a factory

and in the election campaign for a Labour Party candidate, which presaged her later interests in socialism and support for worker's rights (Carroll 2010). With this growing political consciousness, Smith gravitated towards sociology as an undergraduate at the London School of Economics. She later completed her Ph.D. in Sociology at the University of California, Berkeley in 1963. At Berkeley, Smith took transformative courses with Tamotsu Shibutani, but she will remember her academic training as mostly "fundamentally at odds with"[2] the sociology she pledged to build after graduate school, one that begins with "the local actuality of people's lives" (Smith 1994:54).

Smith's notion of a "bifurcated consciousness" grew out of her dissonant experiences in graduate school: of being pressured to conform to the institutional rules of university life while also a mother who was raising two children. What Smith observed as the "lines of fault" among home, family, and work—and the crisscrossing fractures between women's experiences and the ruling relations— would serve as modes of inquiry for Smith's feminist sociology. After joining the faculty at the University of British Columbia in 1967, Smith organized a consciousness-raising group with female graduate students that drew on the energy of the emerging women's movement. Smith's early work examined how men's authority structured "extralocal, objectified relations of ruling" (Smith 1990b:65)

Through the 1970s Smith would develop a sociology for women that deeply engaged Marxism: both to show how women and families were essential to the exploitative "productive enterprise" of a new corporate capitalism (Smith 1975); and to develop more broadly a manner of thinking about social relations that resonated with Marx and Engel's view of ontology from The German Ideology, of "the inseparability of consciousness and individual" (Smith 1987:123, emphasis in the original).

Smith published a trio of influential books— The Everyday World as Problematic (1987), Texts, Facts, and Femininity (Smith 1990a), and The Conceptual Practices of Power (1990b)—which served as the basis for the 1992 symposium in Sociological Theory. Among the major foci of the symposium was the distinctiveness of Smith's understanding of standpoint epistemology, and Smith's exchange with the three panelists helped clarify her positions in relation to major theoretical debates that would swell in significance that decade and beyond. In her comment for the 1992 symposium, Patricia Hill Collins observes in Smith's project a powerful critique of sociology's inner circle. But Collins wonders: what happens when marginalized others enter the inner circle and take part "in the very relations of ruling which Smith abhors"? (Collins 1992:79). Smith's view resonates Collins's notion of an "outsider-within," but she says that Collins and others ascribe incorrectly an oppositional stance to Smith's view of marginalized insiders.

In her comment, Raewyn Connell (1992) lauded Smith's "meta-analysis" of the core assumptions of sociological theorizing. Connell, however, wonders if Smith's critique is more "anarchist" than Marxist in nature and sees Smith as prioritizing individualism above the collective in the production of knowledge. Smith asserts that Connell confuses individualism for Smith's preferred site of human experience. Standpoints for Smith are not necessarily vantage points on the operation of power, ones that privilege any single knower, "woman" or otherwise (Smith 1992). They are not given, fixed locations, but *an achievement* where there is the possibility, though no guarantee, of "politicizing such knowledge in communities of other knowers" (Sweet 2018:229) and of collective resistance.

In his 1992 comment, Charles Lemert will muse that Smith's idea of a standpoint pushes "subjectivity to its limit." Yet Lemert questions if it is possible to locate "Smith's null point of subjective experience" amid the reality of people's many, fragmented identities (Lemert 1992:71). Smith responds that "anyone's," and not just women's, experience could "become a beginning-place inquiry" (Smith 1992:90). She next asks why sociologists like Lemert are so quick to default to categories (in this case, fractured identities) within ideology. Smith expresses skepticism over how poststructuralism locates standpoints within "text-mediated discourse" (Smith 1992:91). She had earlier called Foucault's rendering of power and knowledge a

i "mystical conjunction," where both are ascribed an ontology separate from the materiality of people's lives (Smith 1990b:79). And crucially, Smith's method of inquiry "begins one step back before the Cartesian shift that forgets the body" (Smith 1992:91), an insight that would become foundational to work on embodiment (e.g., Pitts-Taylor 2015).

But post-structuralism—or even opposing accounts of the social such as realism and positivism, of which Smith was also suspicious—was not Smith's primary target. Rather, Smith was frustrated more generally with empirical inquiry that begins within "a particular theoretical enclave" (Smith 1997:820), and what she saw as a tendency to make proprietary claims to theoretical frameworks. Obsessive attachments to such enclaves resulted in the defense and reification of concepts in a process Smith (2005) termed "blob ontology." So when ethnomethodologists expanded the influential framework of "doing gender" to "doing difference" (West and Fenstermaker 1995), Smith will take aim at the blob ontology of categorization. According to Smith, the leap from one category (of gender) to many others under the rubric of difference only thingified categories as "discrete phenomena" (Smith 2009:79). However, when scholars at the turn of the century will identify Smith as an architect of the "new feminist epistemologies" that blends modernist (and structural) accounts with postmodernist (and poststructural) insights (Mann and Kelley 1997), Smith will not disagree with the association. Instead,

so long as sociologists remain open to how people actually live their lives, then for Smith discourses, ideologies, and other concepts could be *pulled down* into explaining the social organization of human activity (Smith 1997).

Researchers have adapted Smith's ideas and taken them in exciting directions, from the immersive study of the hyper-surveillance of Black and Latino youth (Rios 2011) to writing on how a subaltern epistemology challenges Eurocentric social theorizing (Go 2016). Paige Sweet (2018) has drawn on Smith's insights on difference and embodiment to smooth out discrepancies between feminist standpoint theory and critical realism. S. L. Crawley, MC Whitlock, and Jennifer Earles (2021) have found in Smith's antiessentialist view of the social a method of "queering" the normativity of social relations: a way of avoiding territorial disputes between the material and the discursive and seeing practical people as always on the move. Practitioners have adopted Smith's work as a guide for an engaged ethnography in the spirit of Participatory Action Research (Jordan and Kapoor 2016).

Since the 1992 symposium, no area of Smith's work has resonated as widely as her framework for institutional ethnography (IE). The trailblazing 2005 volume *Institutional Ethnography: A Sociology for People* amplified the statement she made in her 1992 reply that sociology was for "anyone." The book brought together Smith's various insights dating

back to early in her career, into how "textual realities" consolidate the ruling relations of institutions under contemporary capitalism (Smith 1990b). These insights proved invaluable to early writings on gendered organizations and labor (Acker 1990), but Smith's method stressed a more expansive understanding of what counts as (and how people themselves understand) "work." The empirical objective of IE, then, is to consider how people's "everyday/everynight" experiences are linked in textually-mediated, "institutional circuits" in larger capitalist relations. At the end of her life, Smith remained committed to making IE and sociology as accessible as possible. As the title of her final book—published with the late Allison I. Griffith—announced, IE was "simply," and most profoundly, a way to bear witness to "the social as actually happening among people" (Smith and Griffith 2022:xiv). Smith's legacy of a sociology for people was an insistence that sociologists—from all theoretical enclaves—never lose sight of those people. Just as important, Smith modeled a sociology that never lost its sense of wonder.

Thirty years after Laslett and Thorne's edited symposium, the wall between sociological theory and feminist theory has cracked. The forthcoming symposium in *Sociological Theory* invites readers to consider how Smith's writings can help us understand and overcome the lines of fault that remain. [3]

NOTES

[1] This essay for Perspectives is an abbreviated version of the Introduction to appear in the Sociological Theory symposium (with the author as guest editor), for issue 41(4).

[2] See Smith's profile on the alumni page of the website for the Berkeley Department of Sociology: https://sociology.berkeley.edu/dorothy-smith-1955.

[3] The percentage of women in the ASA section on Theory remains small, with women accounting for only one-third of all members. For data, see: https://www.asanet.org/diversity-equity-inclusion/dei-at-asa/asa-membership/current-sections-2022/. For a recent reflection on the continuing marginalization of feminist theory within sociological theory, see Kimberly Hoang's 2022 Coser Lecture, "Theorizing from the Margins."

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Modernity's Corruption

An Interview with Nicholas Hoover Wilson

Vasfiye Betel Toprak, *University of Virginia*
Abigail Cary Moore, *Wake Forest University*

Vasfiye Betul Toprak (VBT):

Can you tell us how the idea for this book emerged, and the different routes it took you to arrive at your argument as it stands in the book?

Nicholas Hoover Wilson (NHW):

Sure! I think that question really does lie at the heart of doing comparative historical, and then also theoretically informed, sociology because, on the one hand, the answer to that kind of question means inevitably going into my own intellectual biography, but then, on the other hand, it also means doing so in pursuit of a particular set of theoretical, big questions.

So, I would say my intellectual background is really in big-think cultural sociology, a kind of cultural sociology that really has at the heart of it some of the big questions of what modernity is, what it means to be a modern person, how power is organized in modernity, and so on and so forth. But that

approach I think is also tempered by a sort of empirical tethering that I really got beaten into me in my days at UC Berkeley, and on that count I particularly owe a lot to my advisor, Ann Swidler, who came out of this large, big-questions-about-modernity school of thought. She's one of the co-authors of the Habits of the Heart book, a key text in this "big think" tradition, but also was really adamant about tethering these big questions about modernity to very specific empirical phenomena.

On top of that, right as I was starting my intellectual career, this book, Remaking Modernity, edited by Adams, Clemens, and Orloff, came out. One of its central frameworks is to try to move away from large-scale teleological explanatory frameworks for modernity and think about it as a much more variegated, interlaced and multiple process. And I remember being very, very influenced by that idea as well, because I like to think about all of our scholarship as a kind of ongoing process; it's a matter of asking questions rather than

settling or answering them really definitively. I think that's also part of what it is to be a modern person. So, on the one hand, I think I was very highly influenced and wanted to pursue the sort of big questions, what modernity was, especially what it's cultural architecture, and the intersection of that architecture with the organization of power was. But on the other hand, I was also skeptical of, and didn't really want to accept, the idea that there was a single, grand, teleological explanation for what was happening, so I didn't particularly like, say, one reading of Weber as this kind of formal, rationalization process spreading it everywhere, nor did I particularly like the idea of a very crude reading of Marx that everything was reducible down to economic class conflict, etc. etc.

Much closer to the "empirical specifics" I mentioned a moment ago, I'd done my undergraduate honors thesis on charismatic authority and its portrayal in the works of T.E. Lawrence, and as part of that undergraduate project, I got very interested in doing history. While I was still an undergraduate I took a couple of graduate seminars, one of which was with a historical sociologist, Meyer Kestnbaum, at the University of Maryland, and was so taken with the approach that I said, before I was even really sure what it was, "oh, I want to do historical sociology!" So when I got to Berkeley I took a history graduate seminar which was on the British empire and modernity taught by a historian who ended up being on my dissertation committee. And as part of that, I read a fascinating book by

a historian named Eric Stokes called The English Utilitarians and India, which is all about what it says on the label: the influence of utilitarian thought on social policy in India, or colonial social policy. Right in the preface of that book Stokes said (to paraphrase) "well, before there were these two systems; before utilitarianism became sort of hegemonic in India, within the British colonial apparatus in India, there were these two competing systems of organizing administration; one was called the ryotwari, one was called the zamindari system," and so I said "oh, I wonder what those were about," so I wrote my term paper for that course about those two systems. While I was studying those systems, I was getting interested in fiscal questions and I hooked up with Monica Prasad, Isaac Martin, and Ajay Mehrotra who had a working group of students from across sociology. They were trying to assemble a movement to consolidate fiscal sociology as a field of study, so as part of that I said, "oh, I wonder if these two Indian administrative systems had different forms of tax administration." And it turned out that they did have different forms of tax administration (and that wasn't the only difference) - which system got instituted where was not really a matter of what the underlying social structure was, in contrast to then-standard arguments that that was the case because part of the way implementing one system over another system was justified is to say "this is a natural fit for underlying social structures"; that turned out to really not be the case at all. At a minimum the system was so uncertain

that you couldn't really tell which system should or should not have been implemented in one place or another, so that turned into my Masters thesis and first publications. And then, you know, I sat in my closet for a while and said, I really enjoy the intellectual questions surrounding fiscal sociology, but I'm not sure I can spend the next 25 years reading 18th century tax returns and cadastral surveys from a single district in India; I'm just not sure I have that in me. And so I thought, okay, that was really cool stuff and I was interested in it, but hey, is there a way I can zig and zag back up into these bigger questions about modernity that were still getting me out of bed? I noticed that administrators in tax disputes would often also accuse each other of being corrupt, and they would get to the point of fighting duels with each other, and all the secondary literature said the East India Company (EIC) at the time was experiencing this period of notorious corruption, everyone thought they were corrupt, and I asked, well wait a second —what does this actually mean? These folks are really intensively fighting with one another about being corrupt but, from my modern gaze, all of them seem like they're corrupt! That is to say: literally every single administrator in 18th century EIC was doing stuff that, from my white, middle class, American perspective, made me think "oh, wow, that's just so obviously taking bribes, so obviously not how you would organize public administration if you wanted to be efficient. What's going on here? How can it be that we seem to be

saying today that this was a notoriously corrupt period at the time, but emically they didn't seem to have a settled definition of what was and wasn't corrupt?"

From that point on, I was launched: I said, on the one hand, on the theory side, we need to rethink what we mean when we say something is corrupt, and also empirically it's just so rich because from the standpoint of where corruption edges into scandal, all of these corruption incidents are also incitements to discourse as well.

Abigail Cary Moore (ACM):

Thanks very much for that background. That really helps us understand a little bit better how you narrowed in on the EIC as the case for this project. I'm curious within the broader frame of your argument, are you offering the EIC as an illustrative example of this shift from situational to universal modes of corruption in action, or are you arguing that the transformation that you highlight throughout this particular company as an institution actually contributed to the broader transformation in modernity writ large? That is, is the EIC more of a symptom or a cause of corruption's transformation in modernity?

NHW: Once again, fantastic question that gets to the heart of my intentional ambiguity in the book. My answer is the classic, improvisational-comedy answer, which is "yes, and." Which is to say, I gently

refuse that dichotomy of it being a symptom or cause because I think that history, or just the social world, is so complex, and lively, and rich, that to ask that kind of question about "is this really the origin of the modern form of corruption or is it some form of expression?"; I think that's in a sense to kind of misphrase the question. At the same time, as social scientists that we are all still in dialogue with some version of positivism. We all want to be able to give some kind of general explanatory, "here in this set of circumstances you're gonna have this set of outcomes" kind of argument--a more or less disembedded causal pathway that you can plunk down in a bunch of different places and expect to work similarly. And on top of that, we're also empiricists in the sense that we're going around looking at the expression of these causal systems as they work themselves out over the course of history, or in the present day.

That said, I don't want to completely dodge the question, so here's what I will say: I think that the book is trying to do two things. On the one hand, I am trying to suggest a model with some generality of what this group of family resemblances that we call corruption is. I'm trying to take a stand on what corruption is, then I'm also trying to give, on another level, a bit of a model of how it works, which is to say these things that we're sensibly calling corruption in general I think involve a moral accusation; they involve some kind of abstraction; they involve some kind of

claim to either personal or organization biography and then they involve escalation of that claim to some kind of audience. That is deliberately formal and general because then I think the next step, and this gets from the situational to the universal bit, the next step is to embed that model in one layer of more specific history, or one layer of more specific empirical social reality. And then when I think you take it that one step further down, I think you can get to the place where you have two different configurations: one's universal; one's situational. And then finally you have the sort of full richness of empirical reality in the present and over history and there I think there are plenty of different structures, plenty of different phenomena that are more or less analogous to what you saw going on in the EIC.

So my central empirical claim in the book is basically that when the EIC's organizational protection around it that insulated it from the metropolitan gaze into its affairs in Britain, when that organization insulation broke down, universalist claims to what corruption was, which had been bubbling up out of the company but had been suppressed pretty successfully for organization reasons, those heretofore ignored or diminished claims suddenly got a new kind of attention that changed the politics of the EIC and therefore changed which kinds of administrators were going out to India which therefore changed what kinds of corruption claims those new administrators were going to make, etc. etc.

In other words, I would say that there are

really multiple forms of explanation going on in the book, but then the other half of the question that I don't want to dodge is yeah, I think that it's really, at least for British administrative history, what went on in the EIC was an important turning point for the path of administrative history, even in domestic Britain.

This didn't really make it into the book, but one of the most famous administrative reform acts in domestic Britain was something called the Northcote-Trevelyan Report of 1854. One of the historiographical stands that the book takes is that there is a lot of historiography that's trying to push English administrative modernization earlier and earlier and earlier. Some folks now basically want to say that the key turning point is the Glorious Revolution of 1688, maybe even a little earlier, but I'm on the team that says well no, the British state administration was still very patrimonial until the middle of the 19th century. It really only bureaucratizes and modernizes in a recognizable way around the same time as the American state, roughly the middle of the 19th century. In the US, this was in the aftermath of the American civil war, when the federal government had gotten much larger but they also tried to unwind the whole patrimonial spoils system by which presidents could basically appoint postmasters in little towns and there's this system of political rewards that we would today see as really corrupt. In Britain, one of the main events that catalyzed administrative reform was the publication

of the Northcote-Trevelyan report. Charles Trevelyan, the co-author and half namesake of the report, was an administrator who immediately prior to that had been Lord Lieutenant of Ireland during the Irish potato famine and began his career as a writer in the EIC in the 19th century, and was involved in really terrible corruption scandals there and was an early advocate of this highly universalist framework.

So, to put all this a little bit differently, I'm claiming on the one hand that there are multiple kinds of explanation, but I'm also claiming that one of those explanatory threads pretty directly connects what's going in the EIC and the subject matter of the book to the administrative modernization in the British state. Finally, let me just say that I am not claiming that the EIC is the big bang of all administrative modernization anywhere and everywhere, but to take one step of abstraction back up, I do think that something like colonial administration, which is to say the problem of having a metropolitan polity looking at the affairs and hearing claims about the affairs going on in geographically and culturally distant places, is a very important factor in administrative modernization. So, to again restate, you're not going to find the EIC in the story of administrative American political development, for instance, but you are going to find structures and phenomena that are analogous. You're going to find things like the system of bossism in big American cities, machine politics, etc, etc. Those things look a lot like neo-colonial administrative problems. The same thing is true of

something like the management, colonial administration of the American Bureau of Indian affairs. So I think if you're doing a different empirical project, you'd look for analogous structures to this kind of colonial administrative problematic.

VBT: My next question relates to what you were about to touch on, I think: what would be the relationship of the argument in the book to theories of state formation? I did read your dissertation and there it was more explicitly discussed. I did realize the shifts from the dissertation to the book, and the dissertation was more explicitly engaged with the state formation literature, so my next question would be: what's the relationship of the book to these theories, and what was the reason you moved away from sort of this explicit engagement with state formation theories?

NHW: Now that's a little bit more of a tough one. Part of the answer I think is biographical, which is to say I needed to get the book done in order to get tenure, and it felt like such a monumental task that would have been so demanding to do the sort of project that was in the dissertation justice. So the dissertation is organized a little bit differently, which is to say it's organized around an internal comparison of three different parts of British Colonial India and the empirical claim in the dissertation is that in each of these three different areas a different abstract and universal moral anchor emerged from the administration. And I ended up moving away from that

intellectually. Why did it seem too big? For two reasons: the first is that I do not consider myself a South Asian specialist, but if you know anything about contemporary, to say nothing about historical, India, if you're going to do a project that compares a bunch of different regions in India, you're going to have to learn so many different languages. You're going to have to deal with so many different archives. At a minimum, I really would have had to learn 18th century Persian, probably Bengali, probably Marathi, probably what at the time was called Hindustani, just in order to be able to understand what was going on with indigenous elites in different parts of India. People spend their entire careers just becoming Mughal/Persian specialists, so that was just not going to be possible for a dissertation.

But then also I'm thinking back to it now and I felt, I think, also a little bit of intellectual frustration and exhaustion with the kind of comparative framework that I was putting in, that I was expressing in the dissertation argument. For historical sociology of my generation and the generation before mine, the idea is that you get these two or three cases and what you show is that empirical variation is an expression of the explanatory framework that you're developing, and you tether it to a theoretical apparatus, and bing-bang-boom, there, you know you've got this beautiful thing. I think I'm increasingly starting to see that as a bit of a legacy of the kind of structuralism and materialism that really made the scale of the second wave of historical sociology possible. In the second

wave we're talking about these big books—not that all of them necessarily had a case-comparative framework, as Bill Sewell famously noted!--like Immanuel Wallerstein's The Modern World System, Tilly's Coercion, Capital, and European States, Thomas Ertman, Birth of the Leviathan, and of course Skocpol's States and Social Revolutions, but when you start talking about meaning and culture as a significant factor I think that kind of very clean comparative framework where the explanatory apparatus and its empirical expression get really compressed together in a way that makes that comparative framework possible, that doesn't feel like it works quite as well.

So, long story short, one reason the book ended up the way it did is that I got increasingly frustrated with the architecture of how I set up the dissertation and after I filed the dissertation, once I was on my postdoc, the more I looked into those cases carefully, I said, "can I really say that Bombay is the economy-is-dominant case?" It felt like I had to look away from a bunch of other different stuff that I was seeing in Madras and seeing in Bengal to make that kind of argument work. So, it just didn't feel like it worked. But, while I've been kind of frustrated about this, some more examples of how to do this are starting to come out, so I tried to absorb the theoretical work of people Craig Calhoun, Vicki Bonnell, and Margaret Somers' later work; I got substantively influenced by the work of, say, Julia Adams, Emily Erikson, Phil

Gorski, and Isaac Reed while Julian Go and George Steinmetz were giving a warrant to directly study empires; and I had great conversations with Adam Slez, Damon Mayrl, and Sarah Quinn. These examples turned me away from that compression that I talked about earlier of saying, "well, I have this explanation that I think is true about the world and so now I have to go find places where the empirical variation I'm seeing matches in such a way that I can comparatively sustain in this pseudo-Millian way the intersection of this apparatus with reality."

What I was seeing instead was a bunch of examples where very smart people who I thought were doing great historical sociology were making an argument that was like "well, no, here I'm going to give you what I think is a real explanation of the way stuff works in the world, but the empirical work then becomes a matter of tracing out how that process gets mediated, expressed, potentiated, sometimes dampened in the case that I think is significant and important." In the book, I use the language of William Sewell, who of course is another major figure in the turn that I'm articulating. Sewell uses this lovely metaphor of instead of trying to be like laboratory scientists, the work of historical social scientists is much more like being like a naturalist, and so instead of looking for the kind of natural experiment that substantiated my argument, I realized that what I was seeing within the EIC was instead the way that in, say, the Badlands of Montana, this sort of obscuring empirical material gets washed away until you can see this process kind of exposed to you in really sharp relief. One technical bit that I have realized was an important factor for me is a shift from classical causal language--those of us who are influenced by neo-

positivist comparative frameworks, we all want to run back to Mill and the method of similarity and difference primarily, which is predicated on what is ultimately a kind of mathematical logic, and the mathematical logic behind it is highly analytical in the sense that ultimately what it wants to do is get rid of the richness of language in causal descriptions. This is no shade, this stuff is empirically hugely productive ad I'm a huge fan of it, and I use it in my own work all the time, but I note that one of the things that it wants to do—if you look at modern versions of the Millian comparative framework like in Charles Ragin's work, you want to create literally algebraic truth tables that are derived from mathematics. And that to me also feels a little bit frustrated because it gets away from the richness of the causality that I think I have in mind when I think of my own work. It's only in the past couple of years that I've begun to think, "I am a Stanford school of the philosophy of science, Nancy Cartwright, kind of Ian Hacking, kind of fellow." Cartwright has this beautiful, beautiful description in her book The Dappled World, where she says that instead of trying to reduce and analytically narrow our causal language, let's instead recognize that just in ordinary language, and particularly in precise ordinary language that we as scholars pursue, what we're actually doing is using this incredibly rich causal vocabulary. And Isaac Reed and Dan Hirschman have a great analogous argument of the kind of distinction I'm after here in their paper on forming versus forcing causes. Just that

single cleavage of saying a cause, this thing that we've said is this singular thing instead is this cloud of associated concepts is very Wittgensteinian in its way. There's a family resemblance of things that we are saying can all be subsumed under this broader sense of cause, but there's so much nuance in that causal language that I think we can enrich our explanation in a really productive way if we recognize that variety.

VBT: I think both Abbie and I are very onboard with this idea.

NHW: Let me note that my colleague, Damon Mayrl and I have a whole sort of second life, second career, or cottage industry of trying to work out all of this stuff in language that is more methodological. We recognize that we're doing this stuff in our own work; we recognize that a bunch of historical sociologists are doing stuff like this, so we're starting to try to do an inductive mapping of this kind of work for historical social science.

VBT: I'm asking all the state formation questions! So this question is about the connection between accusations of corruption and the question of legitimacy. What would you see as the relationship between corruption and legitimacy, and under what conditions do these moral accusations of corruption lead to an undermining of legitimacy?

NHW: Boy, you guys did a good read of the book because you found all the tensions and hanging points, but that's actually good, because one headline I want to come out of this book is I want this to be conceptually and empirically generative. I make no warranty of having figured all this out. I know there is supposed to be a dour tradition of theory and historical sociology of saying "this is my statement of having figured it out and it will stand as the final word," but I prefer to think of my work as an incitement to discourse, an opportunity to kind of think these things through collectively.

I see why you're asking: part of what I'm saying in the book is on the escalation side, some claims appear to be translated as some claims were legitimate to certain audiences and therefore empowered.

Here I'll take (another) step back though and say I feel frustrated with the ultimately-Weberian idea of legitimacy as it's conceptualized in a lot of political sociology. I really struggle with the concept of legitimacy because for Weber famously there's no predicting what's going to be legitimate, instead the only thing you can ask is what does the audience believe is legitimate? So it's the eye of the beholder; it's whatever the audience thinks is legitimate. That's really nice because it's empirically rich and generative because then you get to do all of this associational, cultural, sociological work to try to chart these different ideas of legitimacy, but I think that what what Andreas Wimmer has called the

methodological nationalism of legitimacy studies has snuck in. This is this subtle, but not very satisfying, assumption that legitimacy is something co-terminus with a social formation, is somehow automatically this thing that bubbles up from below and is then matched by this assortative process with an administrative or regime framework, and I just don't buy it particularly for the early modern world.

There's a historian by the name of John Elliott who wrote this article called "A Europe of Composite Monarchies," and that has been very conceptually influential for me because the idea is that if you look at England, particularly up to about 1688, it's this interwoven, interlaced world of different claims to legitimacy, and the audiences were so heterogenous, the different styles of claim making, the different sources of authority were all so different that to talk of legitimacy in the singular more or less at any times has never really made sense to me. So when I think about this, I don't think I actually use either concept in their technical sense, but I've always found myself caught between legitimacy and hegemony. Because of course, for Gramsci, hegemony is a version of something like legitimacy that is imposed, at a minimum negotiated, but probably really imposed, and so to be honest, I really struggle because I think some times corruption accusations are cries for legitimacy in an administration or in a regime.

So if you take as one example in contemporary Turkey. In the aftermath of those just horrendous building collapses which are the result, we know, of horribly corrupt building codes where regularly there's a huge gap between what it says on paper versus what the building standard actually is.

There I think an accusation of corruption is a cry literally for the regime to be more legitimate, which is to say, for it to match the real, substantively rational needs of the population it's governing. At other times, though, I think corruption accusations can be quite deliberate attempts to impose a hegemony or a hegemonic vision of the political and social and moral order on a recalcitrant population that's kind of trying to just do its own thing. So if you look back at Robert Merton, in his famous essay on unintended consequences, he has a long passage on bossism and machine politics in the United States, where he says look, what's actually going on in white suburban, protestant middle class people would say is a horribly corrupt political machine in the city, what's actually going on there is politics by other means. It's a form of politics, a version of substantively-rational life that is getting labeled as corrupt because it's threatening to a different political order, and there a corruption accusation is an attempt to exert hegemonic force on what's going on in a subsidiary part of the polity.

So I think in other words, yes, I think there are a multitude of connections between legitimacy and accusations of corruption, but I think it's not so simple because I don't think we can take for granted, just like we can't take for granted our concept of corruption. I think a concept like legitimacy, or a concept like hegemony for that matter, is another one of these composite concepts that's historically situated and embedded and defined as we go along.

ACM: As Vasfiye hinted, legitimacy and problematizing the concept and figuring out how we can use it in a constructive way and how we can unpack exactly what we mean when we're talking about legitimacy is really at the heart of both of our works, so I appreciate hearing you elaborate on that a little more. So, I'd like to end with this final question: you end with a section contextualizing your own personal identity within the framework of corruption that you've identified throughout the text, followed by a reflection on the rise of Trumpism as a potential indicator that the age of a universal metric for corruption is potentially coming to an end. More broadly, how do you feel comparative historical sociologists ought to approach reflexivity and contemporary analysis or contemporary claims in their work?

NHW: There's a whole other book in that question, so let me confine it to maybe a couple of different things. So in the first place, when you look at the architecture of comparative historical sociology and its relationship to sociology on the one hand and history on the other, I think that we have a bit of a special place, which is to say that historians are professionally, epistemologically, and quite appropriately allergic to what they would call presentism, which is the idea that you take events in the past and you try to directly articulate them to a present day question or phenomenon. Sociologists, because historical sociology is so deeply tied up with modernity and we are still all living in the modern world, historical sociology has much more warrant to do that, right? Which is to say, you would be very hard pressed to find a work of historical sociology that is just purely about the historical materials. And I don't say that as a knock; I don't say that to say that we're all anachronists or bad historians or

anything like that, because there's a lot, I think, of really incredible historical and archival work that's coming out of CHS right now; my point is rather different; it's just that what we're doing as a subdiscipline, what distinguishes us from just being historians is that I think we're turning that interest in history to different purposes, or at least allied but significantly different purposes.

And then I think in relation to the discipline of sociology itself there's an increasing historical turn in sociology more broadly and I'm all for that. I think that's incredibly welcome. I think, for example, it's harder and harder for a fairly standard ethnography to avoid having a historical sociological chapter about its field site or phenomenon. I think that's just great, because it's a sign of the field increasing metabolizing what comparative historical and theory are about.

Now, why that section in the book? To take it down to the substantive level a little more directly, a couple of different reasons. The first is that I was finishing the book in the middle of this going on and I was so shocked by how the contemporary anti-corruption structure in the developed world had broken down in the face of someone like Trump. Because as I say in the book, the universalist mode is premised in one sense on an elite person having shame, and elite person being a unitary enough moral agent that they can plausibly be held to account for actions that they've taken in the past. Trump is nothing like that, and I mean this in the strict technical sense: Trump is hardly recognizable as a modern person in the sense that he does not recognizably have a moral

agency that spans his biological life. He can say, "I am saying this right now, now I'm saying something very different, but I'm not going to interface those two claims; I refuse to be held to account for the hypocrisy that is evident in those two claims." And you see this with populist leaders kind of all over the place, in this era of democratic backsliding.

So I was very frustrated by that, and also I was thinking of Aurthor Stinchcombe's sense of deep analogies: I think that in an explanatory sense it is also true that these political and cultural institutions that are premised on this kind of moral universalism I'm talking about have in the United States, in our moment, they've been really badly undermined, and they've been undermined over generations. For instance, I've been reading a lot about the federalist society and the way that it has systematically worked to get what it wants out of the Supreme Court and that took 40-45 years depending on how you count it, and the American conservative political movement committed to undermining some of the major universalist cultural institutions back in the 1970s and early 1980s, and it feels like we're at a threshold where all the sudden that is coming back to the surface, the sort of breakdown of these sort of cultural certainties that are a big part of what Gabi Abend calls the "moral backgrounds" through which we've lived a lot of American political life—at least for white protestant middle class men like me! (I want to be very emphatic that I see the kind of moral background that I'm talking about as a kind of certainty, hegemony, or legitimacy for people who look, talk, and sound like me, and come from a background like mine, because if you had you know a scholar from disadvantaged backgrounds looking at the same material, you'd probably end

up with very different way of thinking about this.)

But I think that it just felt so urgent to express or at least give a try to talk about what I was seeing in that moment, and the deep analogy to the materials that I talk about in the book really just felt natural, like I think I wrote that last bit, I wrote that in a day and it just came out. The one last thing I'll say about the question of reflexivity in historical sociology is that I also think that the allure of positivism is still strong for all sociologists (Damon Mayrl and I have an edited volume in press—After Positivism, new approaches to comparison in comparative historical sociology—which talks about this allure further). And I don't mean that in a scary way; I mean that we want to be able to make claims about what the world is like, and have those claims be more general than just some kind of ultra-subjectivist or postmodern discursive utterances. We want them to have a reality to them. Now, on the other hand, everybody also recognizes that we can't do that in the framework of an ultra-view-from-nowhere universalism, because decades of the philosophy of science and social science have said that basically doesn't work; basically it turns out that scientists of any kind but particularly social scientists are embedded in what they're studying. So I think the future comes from some version of acknowledging our embeddedness. I hate the word positionality, but our positionalities, acknowledging all of those things at the same time that we recognize that we're all still striving for an, if not universal, if not general perspective, at least an intersubjectively

valid one. So the point is we're still all trying to talk to each other but one starting point for doing that are people who look and sound like me acknowledging, you know, of course what I'm saying about corruption is inflected by and shaped by who I am, my background, my class, my race, my gender, my nationality.

ACM: Well that feels to me like a very clear call to action, and as such a great place to end this discussion, so thank you so much for sharing with us a little bit; this has been just a fascinating discussion that I'm sure will be really interesting and engaging for our readers, and I've enjoyed being able to chat with you.

NW: Thanks! Finally, I'd love to also give props to Marina Zaloznaya and Marco Garrido with whom I've been doing a project on the sociology of corruption. We're working on an edited volume together that's in its final stages and my work with them has inflected a lot of what made it into the book–I really hope that my work with them, too, turns into a fruitful intervention in the sociology of culture, theory, soc of development and CHS.

VBT: That sounds really exciting and I'll look forward to reading it when it comes out.

In Praise of Value-Plural Sociology

A Review Essay of Moral Minefields: How Sociologists Debate Good Science, Shai M. Dromi and Samuel D. Stabler, Chicago University Press, Chicago, 2023, 232 pp., US$ 27.50 (paperback) ISBN: 9780226828183

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Pierre Bourdieu famously likened sociology to a martial art. In *Moral Minefields: How Sociologists Debate Good Science*, Shai Dromi and Samuel Stabler propose a different metaphor: they envisage American sociology as a kind of moral labyrinth, rife with academic "no-go zones," moral pollution, and passionate, if also plural, commitments to the common good. Dromi and Stabler suggest these metaphors are incommensurable, but there's a sense in which they need not be. For while this enlightening book can, and should, be understood as an important contribution to that eminent tradition of sociological inquiry that Robert Friedrichs (1970), in his classic, referred to as the *sociology of sociology*, it can also be read as a kind of practical *how-to* guide. Thus, combing these metaphors, we might say, if sociology is a moral minefield, then Dromi and Stabler have provided us with a primer in the martial art of dodging mines. (In fact, at the risk of seeming unserious, I came away from this book thinking that, in addition to being a significant contribution to sociological theory, cultural sociology, and the sociology of morality, it could also plausibly be considered the sociological equivalent of Dale Carnegie's self-help classic, *How to Win Friends and Influence People*; only the more apt title would be something like, *How to Avoid Losing Colleagues and Get Published*).

All joking aside, there is much erudition in this book, such that I'm confident sociologists of all theoretical persuasions will get something from it. For what Dromi and Stabler are ultimately concerned with are questions that strike to the heart of our vocation (or profession, if you prefer), and which no working sociologist can avoid grappling with: What constitutes "good" sociology? By what criteria should our work be evaluated? And how should our discipline contribute to the common good? However, what is innovative about their approach is that, rather than enter the fray and offer their own hot takes on these issues, they instead strive to rise above it by examining how we—the sociological community—go about debating these questions.

It may be useful, at this point, to spell out some of the theoretical and normative convictions that animate this book. First, following recent developments in the sociology of morality, Dromi and Stabler contend that evaluations of "good sociology" are always, in a formal (as opposed to substantive) sense, moral. Or, put more bluntly: we are all moralists—and this includes even the most "value-free" among us. (Although this may frustrate positivists, it's worth noting that this view was actually espoused by the GOAT "value-free" sociologist himself, Max Weber). Second, and following from this, "sociological debates are simultaneously evaluative and empirical" (123), and the reason for this is because—to invoke

Weber once more—all sociology is value-related (Wertbeziehung). Third, against the partisan claims of conservative pundits who dogmatically decry the spread of "political correctness" in sociology, the moral universe of our field is in fact remarkably plural. Last, —and perhaps most contentious—Dromi and Stabler maintain that this moral diversity, far from a weakness, is actually a strength, as it pushes the discipline forward.

How do Dromi and Stabler make their case? Inspired by recent work in cultural sociology, and adopting a purposive method of case selection, they analyze the "meta-communication" across a host of subfields in order to identify the "moral repertoires" that we sociologists commonly invoke in order to both justify our research and critique that of others. In their conceptualization of these repertoires they lean heavily on the neo-pragmatism of Luc Boltanski and Laurent Thévenot—specifically, their well-known On Justification: Economies of Worth. In fact, just as Boltanski and Thévenot identified seven cités (often translated as regimes of worth), so, too, do Dromi and Stabler find seven moral repertoires, each of which evaluates "good sociology" vis-à-vis a distinct set of moral criteria. They are: an efficiency repertoire (objectivity, efficiency, and expertise), a civic repertoire (equality and social justice), an anchored repertoire (community, tradition, and local experience), a creativity repertoire (originality and nonconformity), a charismatic repertoire (deference to academic stars), a marketability

repertoire (ability to attract funding and public attention), and a network repertoire (fostering collaboration and interdisciplinarity). Dromi and Stabler argue that, while not exhaustive, these seven grammars of worth constitute a significant amount of the moral debate that takes place in contemporary sociology. And I tend to agree with them; most readers will likely have little trouble recognizing them, either in our own judgments or those imposed on our work (even if we are partial to some more than others).

This, then, is the first achievement of Moral Minefields: Dromi and Stabler successfully demonstrate that we sociologists make use of plural moral criteria when going about our work. Sometimes we judge a piece of research "good" because it is methodologically rigorous (efficiency repertoire), at other times it's because we think helps to realize a more just world (civic repertoire), and at others it's because it clarifies and refines the thought of a sociological giant (charismatic repertoire). Further, sometimes we think less of research that lacks originality (creativity repertoire), is devoid of public importance (marketability), imposes foreign categories on local life worlds (anchored), or is too mired in disciplinary specialisms (network). In other words, our moral world

is not Manichaeistic; on the contrary, much like the societies we labor in, ours is a polytheistic world of warring gods/values.

But even more noteworthy is Dromi and Stabler's analysis of the myriad maneuvers and strategies that we sociologists undertake, as we navigate the moral minefield that is our discipline. For instance, in Chapter 2, they discuss what they refer to as "no-go zones," meaning topics that sociologists consider to be morally taboo. Although it's certainly true that there exist ideas and claims that are widely considered beyond the pale, Dromi and Stabler challenge the popular perception that sociologists, blinded by political ideology, refuse to engage with controversial topics, by highlighting how the scholarships on race and genetics, as well as culture and poverty, have evolved over time. So while Charles Murray's racist and pseudo-scientific *The Bell Curve* may have made him *persona non grata* in sociology, while simultaneously polluting discussions of genetics and social inequality, it is simply not the case that sociologists have had to steer clear of this research area. And the reason for this is that scholars working in this subfield have done precisely what Murray did *not*—i.e., made a point of explicitly acknowledging the moral risks attending this work, while also making an effort to address the legitimate moral concerns (for instance, regarding the field's historical ties to the eugenics movement) of their colleagues.

Similarly, while the infamous 1965 Moynihan report, which blamed poor American blacks for their poverty, may have provoked moral suspicion (fuelled by the civic repertoire) of studies that focus on the relationship between culture and urban poverty, scholars such as Orlando Patterson and William Julius Wilson were able to justify further research in this area by invoking the anchored repertoire, which emphasizes the moral importance of bringing to light the local experiences of one's research participants.

According to Dromi and Stabler, these cases demonstrate, first, that the moral boundaries that delimit what counts as "good" sociological inquiry are not necessarily constraining in a prohibitive sense, but rather function to ensure that our research contributes to the common good. In other words, while American sociology is characterized by plural visions of the good, no, not everything goes, but that is not necessarily a bad thing. Second, moral debates within sociology can, and do, spur theoretical innovation, by forcing us to rethink the nature and consequences of our work. And finally, rather than facing a binary choice between conducting one's research or being "cancelled," sociologists in fact have multiple means available to them when facing moral critique and controversy, which—to invoke Bourdieu—good martial artists use to turn crises into opportunities.

In Chapters 1, 3 and 4, Dromi and Stabler consider three possible responses to moral critique—what they call, *delegitimation*, *partial reform* and *reconstitution*. The first refers to attempts to *delegitimize* and replace a particular research stream with a new one. For example, in the 1980s, nationalism as a topic was subject to scathing critiques by cosmopolitan sociologists (invoking a combination of *efficiency, civic*, and *network* repertoires), who argued the "nation" as a category was outdated and morally suspect, and thus should be replaced with new analytic tools suitable to a global context. The second refers to a case where scholars acknowledge the legitimacy of a critique, and then do their best to integrate it without discarding the entire research trajectory. An example Dromi and Stabler give is of critiques of the secularization thesis, which drew on an *efficiency* repertoire to paint the theoretical paradigm as ahistorical and insensitive to national differences. In response, a group of sociologists of religion initiated a *partial reform* of the paradigm, revising it so as to account for historical and political variability. The last refers to the process where scholars, sensing a moral stand-still between two camps, seek to move the academic discussion onto new, noncontroversial terrain. Dromi and Stabler give the interesting example of scholarship on breastfeeding. For some time, the field was deadlocked between traditionalists who

lauded the benefits of breastfeeding and critical feminists who saw the practice as intimately bound up with patriarchy. To overcome this stalemate, a third camp emerged which drew upon *anchored* and *civic* repertoires in order to *reconstitute* the debate, moving it away from the merits of breastfeeding per se toward the subjective experiences of mothers, and how the practice of breastfeeding exacerbates gender inequalities within families.

 In the book's concluding chapter, Dromi and Stabler take a step back, and offer their own contributions to our discipline's meta-communication. What their cultural sociology of American sociology makes clear, they contend, is that we must stand with Isaiah Berlin and embrace value pluralism—for the moral diversity that characterizes our field, far from being something to lament, is something we ought to celebrate. In a sense, then, Dromi and Stabler seek to revive and apply the classic American motto, E pluribus unum, to American sociology. (This said, given that Dromi and Stabler repeatedly reference the wider context within which they write, I couldn't help but think that, in pleading for an embrace of moral diversity, they were (indirectly) speaking not merely to their fellow sociologists, but also to their fellow Americans).

In closing this review, I'd like to offer a few (moral?) reflections on this praiseworthy book. First, while sociology may be replete with multiple moral repertoires, it would seem sound sociological reasoning to suggest that not all repertoires are equal, which is to say some hold more salience and authority than others. For instance, my sense is that the dominant repertoires in American sociology are the *efficiency* and *civic* repertoires—which, not coincidentally, roughly correspond to what Friedrichs (1970) typologized as the "priests" and "prophets" of the discipline. This raises a number of issues, one being whether the value pluralism Dromi and Stabler advocate is consonant with large inequalities in the moral landscape. Second, repertoires are open to varying interpretations (e.g., what is "just" to one group may be "unjust" to another). Admittedly, Dromi and Stabler acknowledge this in passing (see p. 122) but choose not to dwell on it. While this was probably a wise choice (it's potentially a very big mine!) it is nevertheless something that warrants further discussion. Lastly, it remains an open question what type of moral repertoire that Dromi and Stabler rely on to justify their endorsement of value-plural sociology. My own answer to this is that, in good pragmatic fashion, they attempt to marshal each of the seven repertoires at different points in the book to support their cause, however, the normative core of their case actually relies on an *eighth* repertoire, of which they do not explicitly speak. I came to think of this as a *progress repertoire*, which justifies value pluralism in American sociology on the grounds that it *contributes to the progress of science*, *sustains the discipline over time,* and *strengthens the sociological community*. Of course, I may be profoundly wrong about

this, but if I'm not, it means that Dromi and Stabler ground their normative case in what they hope is something like a widely shared moral vision that we sociologists are part of something so important and weighty that we would be wise not to spoil it in pursuit of our more parochial moral differences. Now, whether enough of us are moved by this vision to reject value monism, I do not know. But they can consider this reader persuaded.

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Emerging Social Theorists

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Jorge Daniel Vásquez is a Changemaker Postdoctoral Fellow in the School of International Service at American University. In 2023 he received the Erik Olin Wright Distinguish Article Award for his work on Latin American populism and the Best Graduate Student Paper Award from the ASA Global and Transnational Sociology section for his work on Du Bois and Latin America. He received his Ph.D. in Sociology from the University of Massachusetts-Amherst (2023), and his recent publications can be found in Du Bois Review (forthcoming), Critical Sociology, Journal of Historical Sociology, and the Brazilian Journal Sociologias.

He is the author of Transforming Ethnicity: Youth and Migration in Southern Ecuadorian Andes (2023) and is currently working on the book manuscript The Sociology of the Global Color Line: W.E.B. Du Bois, Irene Diggs, and the Critique of Race in the Americas.

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I have built my sociology career on a foundation in Global and transnational studies, Sociology of Race and Ethnicity, Historical Sociology, Critical Theory, and Decolonial studies to analyze the intersections of race, gender, politics, and culture, as well as processes of knowledge production and anti-racist trans-American networks. My work relies on qualitative and archival research methods.

My current book manuscript, The Sociology of the Global Color Line: W.E.B. Du Bois, Irene Diggs, and the critique of race in the Americas, analyzes historical and theoretical elements for a global historical sociology of the color line and contributes to developing genealogies of anticolonial and decolonial networks across the Americas and the African diaspora. I particularly focus on African American scholars W.E.B. Du Bois and Irene Diggs's trans-American networks, field notes, letters, research reports, unpublished manuscripts, and translations regarding race and colonialism in Latin America and the Caribbean between 1930 and 1976. My research shows how Du Bois's and Diggs's analysis of the global color line was significantly challenged by their experience of Latin America and the Caribbean.

The main argument of The Sociology of the Global Color Line is that between 1930 and 1976, Du Bois (from 1930 to 1950s) and Diggs (from 1941 to 1976) engaged in the study of the color line by doing generative work for re-framing sociology as a powerful tool for analyzing white supremacy, colonialism, and capitalism as part of a global racial project. The book particularly addresses Du Bois and Diggs's historicized theorizing engaged with anticolonial politics and anti-racist struggles and debates in Cuba during the 930s and early 1940s, Diggs's fieldwork in South America (Uruguay, Argentina, and Brazil) in the 1940s and 1950s, their joint work as research team of the Division of Special Research of the National Association for the Advancement of Colored People (1944-1947), Diggs's fieldwork in South America (Uruguay, Argentina, and Brazil) in the 1940s and 1950s, their connections with Latin American Scholars (1950s and 1960s), and Diggs' Du Boisian sociology until the late 1970s. I frame Diggs's sociology as a contribution to the legacy and future of Black Feminist Sociology.

Different from rethinking the contributions of European figures such as Durkheim, Weber, and Marx from the conceptual perspectives of W.E.B. Du Bois, my research contributes to the decolonization of sociology by analyzing how critical knowledge is produced through connections between Du Bois and Black sociologist Irene Diggs or public intellectuals in the Americas. I point out not only Du Bois's insights and strategic analysis of the color line in Latin America but also his limitations and mistakes. One of the crucial references for this critique, but also for overcoming Du Bois' limitations, is the work of Irene Diggs. In this sense, my research implied not only transcending a sexist framing of Diggs as Du Bois's secretary and research assistant but proves how gender inequalities in their intellectual relationship produced a misrecognition of Diggs' critical work in developing a global historical sociology of race. Thus, I develop how Diggs explored colonial capitalism,

"colonial sexual behavior," "the denial of racism," "amalgamation," and "attitudes towards color" in Latin America. She approached racial hierarchies, policies, and gender violence from a trans-American perspective. Such contribution is relevant today to thinking about the configuration of the color line. In short, Du Bois and Digg's sociology of the global color line is a critique of modernity that requires the analysis of colonial dynamics both in their regional difference and as part of a civilizing project of a racial and patriarchal nature.

Emerging Social Theorists

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Blair Sackett is an ethnographer who studies the intersection of social inequality and forced migration. She holds a Ph.D. in Sociology from the University of Pennsylvania. She is currently a Postdoctoral Fellow at the Watson Institute for International and Public Affairs at Brown University.

Current research:

My research examines inequality and the role of institutions in the distribution of rights and resources for refugee families. To study these questions, I have conducted two ethnographic projects: one following recently resettled refugee families in the United States, and one with refugee families in a refugee camp in Kenya.

In one line of research, I examine refugee resettlement to the United States, as a deeply unequal society. My book, We Thought It Would Be Heaven: Refugees in an Unequal America, co-authored with Annette Lareau, conceptualizes the types of obstacles refugees face upon resettlement to the U.S. as they navigate institutions to find jobs, manage finances, interact with government offices, and navigate American schools. While refugees are entitled to limited, yet essential assistance through the federal refugee resettlement program (and thus have been theorized to have a relatively favorable context of reception), the book reveals how the very social service organizations meant to help can derail their progress in building a new life in the United States.

To access services, refugee families must overcome countless institutional hurdles, fulfilling complex rules and requirements: to enroll their children in Early Head Start programs, for instance, requires 15 different documents. Nor are these rules neutral: requirements to prove deservingness disproportionately burden people of color (Ray, Herd, and Moynihan 2023). With so much complexity, processes are rife with errors. Seemingly small organizational errors—missing a deadline, mistaking a rule, or misplacing a form—can tangle processes into institutional knots. These minor mistakes grind systems to a halt, creating catastrophes as food stamps are cut off, educational opportunities are missed, and benefits are not accessed.

While each institutional error may seem unexpected, unpredictable, and unique, many of the system errors are not a product of individual mistakes or failures per se, but need to be understood as part of the social systems that have been created to offer services to people who are in need. In particular, policymakers in the United States have been keen to prevent fraud and have recipients prove their need for services. In these organizations, the complexity, the scrutiny, and the necessity of proving deservedness all increase the likelihood of system errors (or what sociologist Charles Perrow called "normal accidents.")

Moreover, because refugee families (like all families) are navigating a web of multiple social agencies, routine errors and mishaps in one agency can reverberate to create a series of entanglements in another. Although scholars have studied the barriers immigrants and low-income families face in isolated institutions, such as workplaces or schools, the intersecting nature of obstacles suggests the need to expand our analyses beyond institutional silos. While some of these obstacles and blocked resources may seem individually inconsequential, overtime and across institutions problems intersected and reverberated, creating pathways of upward—or downward—socioeconomic mobility for resettled refugee families.

In another line of research, in my second book project I bring to the fore the refugee camp as a context of reception to show how institutions shape and constrain pathways of socioeconomic mobility for refugee families in the Global South. Prior research has illuminated the role of institutions in the context of reception for migrants' socioeconomic pathways; yet, too often, these theories of incorporation focus on the experiences of labor migrants in contexts in the Global North. While most economic migrants move to wealthier countries, most refugees remain in the poorest regions of the world (Castles 2009). Countries in the Global North have implemented restrictive

policies, making it increasingly difficult for refugees to reach their shores, containing refugees in the Global South (Arar and FitzGerald 2022). Yet, less is known about the dynamics of reception for refugees in these contexts. Thus, our theories of migration and incorporation need to account for a wider range of migration and contexts of reception.

My ethnographic study in Kakuma refugee camp in Kenya shows how refugee families faced institutional barriers to getting ahead. Displacement depleted their resources. As they started a new life in the camp, they made many efforts to rebuild their resources—but at each turn, they hit barriers. Although humanitarian organizations provided essential assistance, budget cuts coupled with ideological shifts placed economic responsibility on refugee families. At the same time, formal work was legally restricted, and policies prohibited refugees from leaving camp to find informal work or move for economic opportunities—paths commonly pursued by immigrants in other contexts. In the camp, refugees turned to their social ties for help. Yet, refugee networks were heavily stressed and food shortages were routine. For most, incorporation in the refugee camp context was tied to impeded socioeconomic mobility.

Thus, my research seeks to advance our understanding of how forced migration intersects with inequality, and to contribute to our theories on the role of the context of reception for socioeconomic mobility for refugee families in the Global North and South.

Emerging Social Theorists

Ben Kaplow,
Yale University

Ben Kaplow is a PhD Candidate in Sociology at Yale University and holds a BA in Sociology from Haverford College. Broadly speaking, he engages the comparative-historical study of empire, post-colonial politics, and institutional transformation, with a particular focus on the entanglements of empire, state-building, and the environment.

My dissertation, Dynamic Durability: Land Policy and the Colonial Legacy in North Africa, examines how the postcolonial states in Morocco and Algeria cemented their rule in the aftermath of French colonization. In radically different ways, state actors in each country built stable political control through patronage networks and clientelist policies. In doing so, they drew upon preexisting colonial patterns of rule and economic organization.

Focusing on the foundational land and water policies of the colonial agricultural economies in Algeria and Morocco, my dissertation traces how colonial institutions were carried into the postcolonial era. Particularly, I study how these institutions were drawn upon and transformed by postcolonial political actors. By shifting attention from colonial institutions in isolation to their transformation by postcolonial actors, my works seeks to illuminate the understudied mechanisms by which colonial institutions continue to affect postcolonial countries, not as relics or static determinants of developmental outcomes, but through the layers of political action built upon them.

My research builds from archival sources in France and Morocco to trace the evolution of land policy from colonial expropriation to decolonial land recovery. Most significantly, I am constructing a parcel-level, longitudinal dataset for the entirety of Morocco with detailed landholding and agricultural information. This dataset will include all lands held by French colonists during the colonial period as well as the time and means by which the land was transferred back to Moroccans, whether through sale or expropriation. This dataset, when merged with contemporary socioeconomic data, is used to examine not only the effects of colonial presence on development and local politics, but the ways in which the postcolonial state's responses to colonial institutions determined developmental and political outcomes. In doing so, I aim to link the broader discussion of postcolonial state building with empirical strategies centered on local politics and economic development.

Theory Section Award Winners ASA 2023

Lewis Coser Award for Theoretical Agenda Setting:

Claudio Ezequiel Benzecry, *Northwestern University*

Theory Prize (Book)

Ellis Monk. 2022. "Inequality without Groups: Contemporary Theories of Categories, Intersectional Typicality, and the Disaggregation of Difference." *Sociological Theory* 40(1): 3-27.

Junior Theorist Award

Winner: Mathieu Hikaru Desan. 2023. "Realist and Historicist Modes of Critique in Critical Sociology." Critical Sociology 49(4-5): 589-612.

Honorable Mention: Jason L. Ferguson. 2021. "'There Is an Eye on Us': International Imitation, Popular Representation, and the Regulation of Homosexuality in Senegal." American Sociological Review 86(4):700–727.

Honorable Mention: Ioana Sendroiu. 2022. "'All the Old Illusions': On Guessing at Being in Crisis." Sociological Theory 40(4):297–321.

Best Student Paper Award

Winner: Krystal Laryea, "Playing up Difference: How Identities are Interactionally Navigated in Groups"

Honorable Mention: Ankit Bhardwaj, "The Soils of Black Folk: W.E.B. Du Bois's Theories of Environmental Racialization"

New Publications

Blume Oeur, Freeden, and C.J. Pascoe. 2023. Gender Replay: On Kids, Schools, and Feminism. New York: NYU Press. [Get 30% off at the NYU Press website using the code NYUAU30.]

Barrie Thorne's Gender Play was a landmark study of the social worlds of primary school children that sparked a paradigm shift in our understanding of how kids and the adults around them contest and reinforce gender boundaries. Thirty years later, Gender Replay celebrates and reflects on this classic, extending Thorne's scholarship into a new and different generation.

Freeden Blume Oeur and C. J. Pascoe's new volume brings together many of the foremost scholars on youth from an array of disciplines, including sociology, childhood studies, education, gender studies, and communication studies. Together, these scholars reflect on many contemporary issues that were not covered in Thorne's original text, exploring new dimensions of schooling, the sociology of gender, social media, and feminist theory. Over fourteen essays, the authors touch on topics such as youth resistance in the Trump era; girls and technology; the use of play to challenge oppressive racial regimes; youth activism against climate change; the importance of taking kids seriously as social actors; and mentoring as a form of feminist praxis. Gender Replay picks up where Thorne's text left off, doing the vital work of applying her teachings to a transformed world and to new configurations of childhood.

New Publications

Theory and Society: Special Issue on Stefan Bargheer's 'Moral Entanglements'

Theory and Society (Volume 51, Issue 6) published a special issue on Stefan Bargheer's *Moral Entanglements: Conserving Birds in Britain and Germany* (University of Chicago Press, 2018). At the center of the book stands the question of how values change over time and how individuals develop moral commitments. Based on an historical analysis of bird watching, field ornithology, and nature conservation in Britain and Germany, "Moral Entanglements" formulates a novel sociology of morality informed by a pragmatist theory of value. The special issue engaging this theory comprises of six essays and a response by the author. The contributors are Shai M. Dromi (Harvard), Erika Summers-Effler (Notre Dame), Iddo Tavory (NYU), Philip S. Gorski (Yale), Caleb Scoville (Tufts), Rebecca Elliott (LSE), and Stefan Bargheer (Aarhus Institute of Advanced Studies).