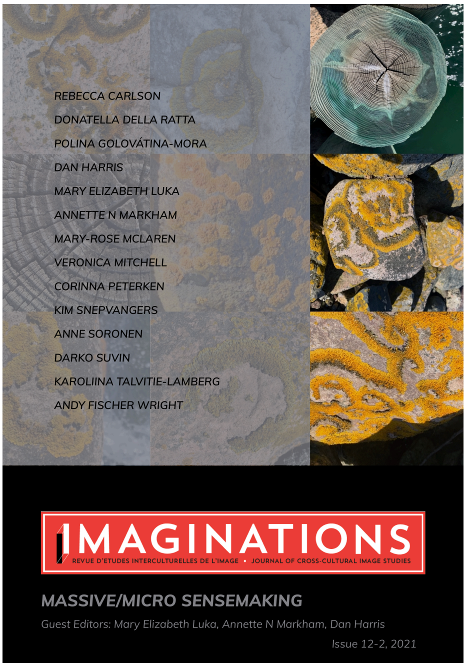
IMAGINATIONS
REVUE D'ETUDES INTERCULTURELLES DE L'IMAGE • JOURNAL OF CROSS-CULTURAL IMAGE STUDIES



IMAGINATIONS:
JOURNAL OF CROSS-CULTURAL IMAGE STUDIES | REVUE D'ÉTUDES INTERCULTURELLES DE L'IMAGE

Publication details, including open access policy and instructions for contributors: http://imaginations.glendon.yorku.ca

Massive/Micro Sensemaking: Towards Post-pandemic Futures
Guest Editors: Mary Elizabeth Luka, Annette Markham, Dan Harris
December 23, 2021

Image credit: Annette N Markham

To cite this article:
Carlson, Rebecca. "(Imagining) Science for Troubled Times: A Mouse, a Bird, and a Threshold for Collaboration." *Imaginations: Journal of Cross-Cultural Image Studies*, vol. 12, no. 2, December 23, 2021, pp. 171–200, doi: 10.17742/IMAGE.MM.12.2.9.

To link to this article:
http://dx.doi.org/10.17742/IMAGE.MM.12.2.9

(IMAGINING) SCIENCE FOR TROUBLED TIMES: A MOUSE, A BIRD, AND A THRESHOLD FOR COLLABORATION

REBECCA CARLSON

Although biological life and human social complexity are fundamentally interdependent, biological and social researchers continue to perceive each other from across divides of theoretical, methodological, and institutional skepticism. This paper considers conversational boundary work between qualitative and quantitative scientists as an institutionalized rhetorical performance which throttles their cooperation, even in the face of the COVID-19 pandemic when it is most urgently needed. As an example, I look at the way familiar epistemological conflicts emerged out of collaboration between myself and a bioscientist during the spring of 2020, when co-participating in the Massive Microscopic Sensemaking Project, a 21-day international auto-ethnographic writing experiment.

Bien que la vie biologique et la complexité sociale humaine soient fondamentalement interdépendantes, les chercheurs en biologie et en sciences sociales continuent de se percevoir à travers les clivages du scepticisme théorique, méthodologique et institutionnel. Cet article considère le travail de frontière conversationnel entre les scientifiques qualitatifs et quantitatifs comme une performance rhétorique institutionnalisée qui limite leur coopération, même face à la pandémie de COVID-19 lorsque cela est le plus urgent. À titre d'exemple, j'examine la manière dont des conflits épistémologiques familiers ont émergé de la collaboration entre moi-même et un bioscientifique au printemps 2020, lorsque j'ai participé au Massive Microscopic Sensemaking Project, un projet internationale d'écriture auto-ethnographique de 21 jours.

A MOUSE AT THE THRESHOLD OF CIRCULATION, OTHERWISE KNOWN AS A PROLOGUE

Figure 1: Mice with various coats, including piebald (top left), from the 1787 Japanese book Chingan Sodategusa, public domain. (Modified by author.)

The first bioscience presentation I attended at the institute where my research has been located began with an aside about a Japanese mouse. The presenting scientist, the head of a European lab visiting Japan for a virology conference, introduced his lab to the audience with a photo of the "JF1/Ms," or Japanese Fancy Mouse 1. The scientist explained that this Japanese mouse, marked grey and white due to its piebald allele, is an important part of his lab's research, and he laughed a bit as he held the remote for the projector in his hand. By way of introduction, I imagined the scientist hoped that the JF1/Ms would work like a bridge to link him to the mostly Japanese postdocs and lab heads in attendance. After he moved on to the purpose of his talk, I was left wondering about mice who travel across oceans, just like we do.

As scientific material in global circulation, I later found that calling the JF1/Ms a 'Japanese' mouse was rather misleading, a flattening of its more convoluted history. Although the JF1/Ms is said to have derived from "wild mice that inhabit Japan widely" (*nihon ni hiroku seisoku suru yasei hatsukanezumi*),[1] and appears in the 1787 book *Chingan Sodategusa* (pictured above), or *How to Raise Rare Mice* (see Ruben 2005, and also Tanave and Koide 2020), the Japanese National Institute of Genetics (re)discovered the JF1/Ms in Denmark where it had been available as a pet mouse and brought it back to Japan for breeding and research. (The institute even made a map to depict the JF1/Ms's complicated geographic and genetic travels.[2]) Today, researchers anywhere (with a sufficient budget) can purchase this inbred strain from institutes in Japan or from the Jackson Laboratory in Maine, who provide mice models as material for genomic research. I was collecting this background information in order to use the JF1/Ms analytically for my larger research project about the globalization of Japanese bioscience and the circulation of bioscientific materials. But this presentation—just as my research was really getting started in full—was both the first and the last I was to attend, in person, at the institute.

In my very first interview with the lab supervisor where I am conducting my ethnographic research, he complained about how hard it was for him to get scientific materials from outside Japan. An American MD-PhD, the lab supervisor had been heading his own laboratory for about six months at the institute by the time I began my research there, and the administrative frustrations he'd experienced in setting up his laboratory and securing the materials he needed were still a recent memory. At that time, our interests coincided: he was a foreign researcher in Japan and I had been studying forms of Japanese globalization, including the immigration of Americans, just like him, to Tokyo. He was an ideal informant because his experiences spoke directly to my research question, and he knew how to direct his observations to what he thought would be useful for me. What mattered more was that he was interested in what I was doing and wanted to support my research.

As part of my weekly visits to the laboratory for participant observation, the lab supervisor agreed to let me interview him for fifteen minutes. I'd join him in his glass office, a small space enclosed inside the larger office that was separated from the research labs across the hall where the lab members did their wet experiments. In that first interview, he told me about the expectation he'd encountered in the institute that all the lab's scientific materials, such as reagents, would be secured within Japan. Whenever he'd asked through administrative channels for approval of materials from the United States or the United Kingdom, he described the reaction of the Japanese staff as, "Do you really need this?" Then he read to me an email from a scientist he knew in the United States who wanted to access materials from a Japanese laboratory and was having the same trouble in reverse, complaining in the email of how slow the process was. This other American scientist wanted to know if the holdup had something to do with peculiarities of Japanese scientific practice. For the lab supervisor, let's call him Tom, this limit on access was a clear barrier to doing good science. In talking together later about how personal connections between scientists can impact access to materials, Tom told me that these networks are, "important [...] to get reagents [...] to get emails replied to [...] to get papers published. They're important in all the ways it is important to do science."[3] In adapting to the logics of scientific circulation in Japan, Tom was finding it necessary to build new local networks to access materials and practices.

When the first COVID-19 emergency declaration started in Tokyo in April 2020 and the research institute asked scientists who could to work from home, I began to join the laboratory members online for their weekly meetings. Tom soon wrote to me suggesting that I should find a way to share my "academic perspective on the global spread of an emerging virus." At first, I hesitated, and in response he joked through email about ambulance chasers and virtue-signaling in Twitter feeds, but then reminded me: "[Y]ou have spent a year studying globalization of virology research, visiting and even doing experiments in a 'global' lab [...] It would be valuable to put it in writing, that's all I'm saying." Around the same time, I saw the call for the Massive Microscopic Sensemaking (MMS) project and proposed to Tom that we participate collaboratively. I suggested he could generate his own field notes about working from home (the wet experiments in his laboratory were largely on hold and his post-docs shifted to planning in the slow down), and I would use them as ethnographic material in the final paper which we would co-author. He replied immediately that the project seemed interesting to him since it was "a venue that prevents treating the communication/result as too precious, which I really like." But he clarified he wouldn't be able to do much *de novo*. Tom started a Google document the same day, titling it *Field notes on starting COVID-19 research while working from home*, and forwarded me the link with his first entries already completed. They were dated like a diary and settled mostly on the somewhat frustrating transition he was making to working online. His first posts from the shared document state:

> "4.27.20 - Discussion in the animal room with a colleague at my institute who is already working on COVID-19. This only happened because we are both physically present in this same space. Collaborative science often grows out of informal communications about existing work, and we will need to think of how to create new spaces to bump into one other in the increasingly teleworked future.
>
> 4.28.20 - Brainstorm with Toshi [pseudonym] about how to represent this collaboration in an intuitive and concrete figure. This was an in-person interaction using a whiteboard, which I cannot do virtually very easily, yet. Need to find a way."

Tom's transitional concerns along with my research focus on scientific globalization and general fixation with boundary crossing inspired me to write our abstract in application to the MMS project about the impact COVID-19 was having on scientific circulations. After reading my abstract draft, Tom reminded me:

"In addition to building new borders, [telework, COVID...] removes some. E.g. we started a journal club with colleagues in Kyoto, and I participated in one international journal club. But these don't get the work done when it comes to starting work requiring new biological materials, etc."

Starting with these observations, I made a table in our shared document to help track the slowing down and speeding up of the various scientific materials and interactions that Tom directed my attention to in his notes. But before we were even accepted to the project, Tom and I began what would develop into a months-long conversational interview, inside the document and through email.

In the end, we wrote very little (productively) for the MMS project, Tom even less as he was busy with his work, and very little about scientific circulations. Instead, we veered more into staking out our different disciplinary perspectives on topics such as objectivity and scientific truth-making. We evaluated each other's broad conceptual tools, at times skeptically, and drew boundaries around our ideas to see where they overlapped. For a time, we hovered liminally (and for me, hopefully[4]) between our assigned subject positions as 'quantitative' and 'qualitative' scientific researchers, and our rhetorical differences, subjecting our ideas and concepts to classification and reclassification; and, I think for both of us, wanting to cross over those disciplinary-informed borders. Somewhere in the middle of our conversation, Tom asked me if I had ever read the book *Flatland: A Romance of Many Dimensions* (1884). He told me: "I read that book in middle school, and I really want to read it again. I want to have the humility of a flatlander who is made aware of another dimension." In the end, the genealogy of our discussions shows our attempt to come together across this dichotomous gap between our knowledges and techniques, and to see these other dimensions. It was perhaps in part because of our divergent priorities for how to communicate our shared views through language, and the question of their value, that Tom chose to withdraw from our collaboration in this text (although he continued to read and comment on the many drafts and tangents that followed).

SUBJECT/OBJECT INDISPENSABILITY, OR WHAT SHOULD BE AN INTRODUCTION

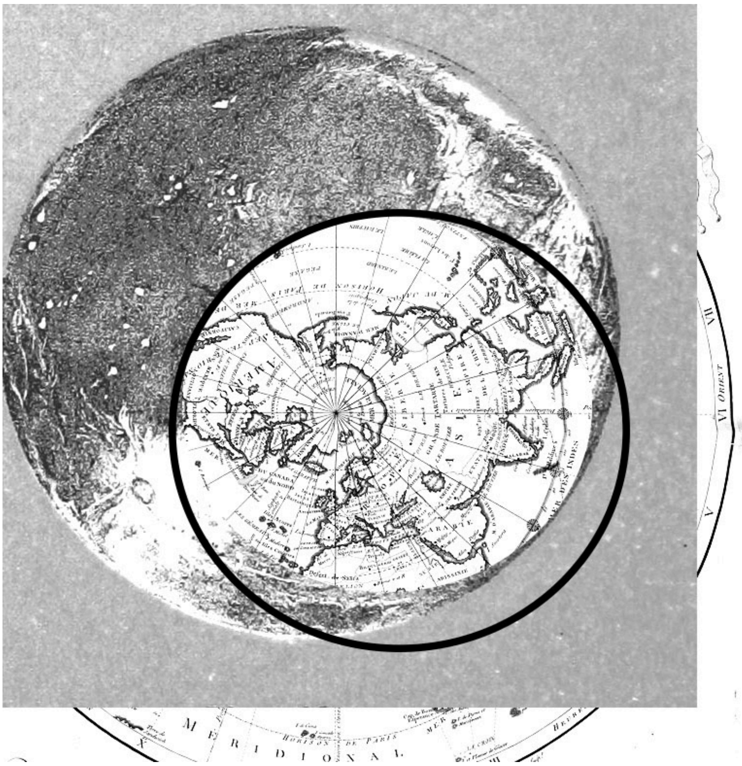


Figure 2: Objects crossing borders, becoming subjects. Images from British Library Collection, public domain. (Collage by author.)

Circulations always trouble seemingly ready-made, common-sense categories. Things and people on the move cross over practical and symbolic boundaries, often rattling them or making them come undone. Objects in circulation, Christopher B. Steiner explains, get removed from their "original cultural contexts," reinterpreted under new institutional logics and stripped of their charisma (1996, 208). Although Steiner is focused specifically on the legal practices which act on transnational artwork to assess their value and right of entry, his observations on the reclassification of goods as they pass through national "border zones" apply equally productively to people and ideas, to the JF1/Ms and even Tom. Steiner's analysis is a reminder that things don't travel unconstrained—mice don't get to Europe and come back to Japan without passing through various transformative practices—just as their circulation is always subject to, and participating in, "boundary work" (Gieryn 1999, Moats & Seaver 2019). In my own research in the laboratory, I was specifically attuned to, or looking for, these border zones; for example, the moments when scientific materials and practices might become, for example, culturally re-signified—marked as Japanese or American, subject to revaluation under this label and then inserted into predetermined channels of movement, like getting earmarked for distribution along prioritized (national) networks. When the pandemic began and I transitioned to remote research, I began to think of Steiner's border zones, which I have drawn on in my previous research, as equivalent to Victor Turner's threshold stage of liminality, that moment of extended ambiguity as people-things cross over or defy categorization and hover for a time in between (1969).

But what does any of this have to do with thresholds and boundary work, or scientific materials in circulation like the Japanese Fancy Mouse 1, and even Tom himself? I think this is typically where I am expected, if I am a competent academic, to bring the threads of this beginning together, to insert a few sentences to explain what this article is really about. Here, I should address the significance of the collaborative conversation Tom and I engaged in during the emergency declaration, and why it might be valuable for others to read about. After all, this is paragraph eight already and you, reader, may reasonably be wondering where all this is going. In an earlier draft of this article, I attempted to use our discussions as a call for bioscientists and anthropologists to work together more on topics related to human health,[5] and to find better ways to talk to each other; but it felt too naive and overrun by a growth of too many topics and ideas (thank you to anonymous reviewers for stressing particularly this last part). Although below I attempt to give a clearer framework for what precedes and follows, truthfully, I feel all my attempts at analysis are continually unraveling, and maybe worse, potentially misleading. Neat, complete answers, ethnographic ribbons which tie up all the messy threads of data, as if suddenly, easily crystallized (out of what are really other people's complex lived realities, with me tangled in them), feel like too much artifice right now. But I remain motivated by the desire to find a shared space for cultural anthropologists and biologists to work more collaboratively on issues of public health than I think they tend to. This feels vitally important to me in the face of a viral pandemic that is, like all human disease, shaped powerfully by global social inequalities (see for example, Wise 2020). In reading an early draft, Tom suggested to me that this paper could be valuable for other bioscientists, who "might feel similarly compelled to try to see if there is a shared framework about truth with postmodern thinkers/critical theorists. Which is I think one of the things this paper is about." (I always objected to the fact that Tom classified me as postmodern, particularly as I worried he wielded it sarcastically, but eventually I accepted, writing to him with a degree of hyperbole, "If you consider po-mo to be the breakdown of the 'grand (universalizing) narrative' and a turn to reflexivity, then that is me, to a T".) Tom often pushed me to think more about and analyze the difficulties bioscientists face today in reaching others and in communicating their research, despite the growth of prefabricated publication announcements on Twitter. He once told me in his glass office something like, "science not communicated isn't science,"[6] and his insistence is influencing me and the direction of this work more than anything else. Still, I imagine biologists—in finding little direct depiction of, or connection to, their daily practices in the lab—will have little patience to read this; or, more likely due to disciplinary skepticism, will be less likely to appreciate it as science.

For the rest of this article, then, I examine our conversations as an act of cross-disciplinary boundary work that reflects the ways categorical, conceptual, and institutionalized borders are always being negotiated, reworked, and reaffirmed in everyday conversation. This is similar to the observation made by David Moats and Nick Seaver in their study of the apparent divide between "data scientists" and qualitative researchers:

> "When we speak of a "divide," we are not arguing that it is desirable, natural, or inevitable, but are rather pointing to an empirical phenomenon which manifests in practice as conversational tension, miscommunication, and, sometimes, disputes." (2019, 3)

When Tom called me "postmodern" in the flow of our conversation, it felt like an accusation. At one point, he wrote something similar back to me: "Your tone here is a little different, maybe implying what you assume about me." At times, we didn't approach each other as people, but as cut-outs of our disciplines. These everyday practices described by Moats and Seaver are important to analyze because they are the rhetorical performances which scientists on either side of the divide use to make sense of, and often dismiss, the other. Perhaps they are not 'natural,' yet these divisions do become naturalized, and reinforced by institutional separations; and as commonsense, they close off opportunities for speaking, thinking, and working together.

Despite writing to each other on the cusp of a threshold, in the introspective intersection of a public health crisis and a period of forced isolation, our goal to eventually write this paper together for the MMS project came undone. Just like (the meaning of) a mouse or a reagent shifts and transforms as it crosses national borders, I found the possibilities for our collaboration channeled and constrained by institutional processes and assumptions of meaning and value that I had barely recognized previously—and which we were enacting, perhaps even unconsciously, in text. Those structural barriers were present in the way we spoke to each other, the questions we asked of each other, and the way we did or did not listen to our answers. Liminality, I realized, is not the free-form site of transformative possibility I had idealized; instead, it is weighed down by codified and institutionalized rituals which define the purpose, and necessary outcome, of time spent "betwixt and between" (Turner 1969, 95). I argue in conclusion then that to achieve any meaningful change requires a rescripting of these structural codes.

WHEN A BIRD IS NOT A BIRD, OR WHAT BECOMES A PERFORMANCE OF EXPERTISE

> "Or should we rather bring the sword of criticism to criticism itself and do a bit of soul-searching here: what were we really after when we were so intent on showing the social construction of scientific facts?" (Latour 2004, 248)

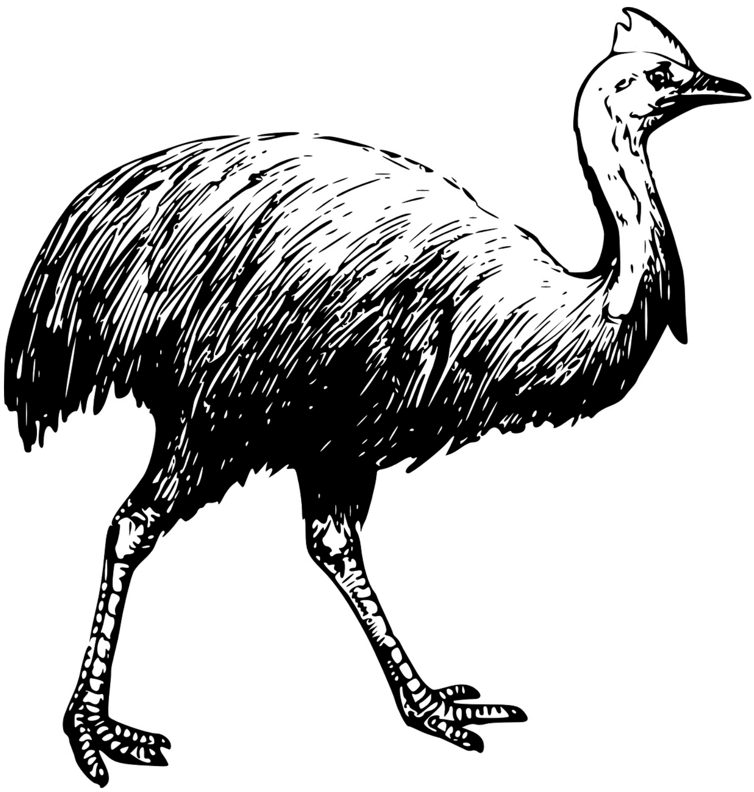


Figure 3: The cassowary, public domain. https://pixabay.com/vectors/cassowary-bird-feathers-neck-48000/.

In the beginning of 2020, after I had been visiting the lab in person for data collection for over six months, I approached Tom through email with a request to start learning how to do some wet experiments. I titled the email "crazy idea?" He responded:

> "It isn't crazy, it could be helpful.
>
> But to be honest I don't feel entirely comfortable with it right now, but only because I wonder if I fully understand your motivation. I hope that doesn't come across as too cynical…"

When we met in his office later in the week for our usual fifteen minutes, he explained his hesitation in more detail, describing his concern over the disjuncture that might come from me working at the bench alongside the postdocs who are "science realists" when my perspective is very different. He stressed that scientists are motivated by the importance of the process, to be a "capable doer," and that reliability in experimental results was the mark of a good scientist. Was I trying to use it as an opportunity to find work in the future as a laboratory scientist, he wondered? How could he evaluate me if he couldn't understand my motivation? In the end, Tom insinuated that in contrast to the other scientists in the lab, I believed bioscience has no meaning. I tried to clarify by explaining that I think bioscience, in fact, is overcome with meaning which was why it was so interesting to me as a research topic. At that point, I had been teaching medical anthropology and global health to Japanese medical and dental students for seven years. I was acutely concerned with the way the 'facts' of human nature (coming out of bioscience research) were often turned inside out to justify, in public health for example, why some populations were naturally more at risk or more protected than others (see for example, Fullwiley 2011).[7] I was concerned then with the "social life" of bioscientific facts (Appadurai, 1986).

Worried I still wasn't communicating my perspective clearly, I emailed Tom the day after our meeting, addressing the multiple concerns he'd mentioned in turn (listed as subheadings that I enumerated), in the hopes of clarifying:

> 3. On the meaning of science and the dissonance of my presence in the lab as a (possible) non-believer:

> Science is bursting with, frequently unacknowledged, meaning as I said. And precisely because it is often unacknowledged, makes it all the more fascinating for me. But that doesn't mean I don't believe in the value of what you all are doing and don't genuinely want to contribute. My goal is not to tear down or 'deconstruct' your work, but to build up a patina of textures and descriptions of the deep ways of thinking and acting and deciding that are already taking place in the lab.

Science and technology studies (STS) researchers, from at least as early as Bruno Latour, have described the limitations of mere observation of "laboratory life" (Latour & Woolgar, 1979). Moats and Seaver similarly describe the frustration of ethnographers who, when working with quantitative scientists, remain observers "from the sidelines" despite their best efforts to collaborate (2019, 3). In fact, what I wanted was a more holistic understanding of the research they were doing in the lab, which I, as an anthropologist, understood could only come from "getting my hands dirty," as Tom often described his own experimental work. Because the importance of participant observation is so central to my thinking about good research (and not reliability of experimental outcomes), I was surprised by Tom's skepticism and questions about my true motivation. In this exchange, I began to see that although Tom was interested in and supported my research, he had concerns over my role as a "critical theorist" in the laboratory and also harbored his own assumptions about the nature and limitations of qualitative research, as well as my status as a "non-believer."

When we began writing together, this general skepticism, or the sense of our overriding assumptions, became a central frame for our dialogue. Tom would send me articles to read or post snippets of COVID news in our shared document that he wanted to draw my attention to or talk about. In preparation for the MMS project, I started writing field notes directly into his shared file and began wrestling with my own criticisms and understanding of concepts I had been encountering in the lab, which he in turn often replied to with the document's comment function. We went through in turns mulling together over paradigms and challenging each other, starting with topics related to genomic science and genome wide association studies (GWAS). From the beginning, we often used terms like "baiting" or "fishing" to describe the introduction of topics or questions we anticipated might result in controversy and disagreement. When we debated objectivity and metrics, discussing IQ tests as an example, at one point I teased him, "But those 'metrics' aren't objective…right?" Tom highlighted this phase and replied in a comment box: "Maybe we are back to our fundamental difference in how we interpret the world. I think the metrics themselves are potentially 'objective,' at least as I define that word."

As part of our broader conversation on the nature of objectivity and reality as a measurable quantity, Tom shared an article about using autosomes for sex in GWAS studies[8] and asked me: "Do you think their case and control populations are more cultural ideas or more genetic ideas?" He was echoing something I had written in the document about my concern, and even confusion, over taking national populations as units of analysis in GWAS.[9] When I hesitated to answer him, writing instead that I needed some time to think over everything we had been discussing up to that point, he started a comment by writing, "Thanks and sorry." He continued:

> "I need to admit that this was a bit of a dishonest question from me. I think this paper/study makes a nice point about limits of GWAS, the assumptions of GWAS (Do you assume that GWAS makes more assumptions than I do?), and what GWAS overlooks. […] But I think this is because I am testing you in some way, fishing. When I look deeply, I am doing this because I value"the scientific method" hegemony as a way of interacting with the world more highly than what I assume to be the hegemonic modern anthropology-approved viewpoint/context, and I want to justify this to myself in some way. I wonder if this poisons the well of your anthropology, and apologize if so.

> Also, now you know that in my imagined secret place or "hidden" context I reject both of these hegemonies in some ways, so no need to treat science with kid gloves…"

I replied by email:

> "[Y]eah, when you ask me about whether I think case/control is cultural or genetic, of course I know it's partly in jest, a tease. And honestly, and because it entertains me, it makes me want to be, a little bit unproductively, polemic."

I found then that I was often exaggerating my opposition to him, getting stuck—and maybe wanting to poke a bit at him for his insistence—on objectivity, and perhaps overlooking his relative nuances; just as he seemed similarly unable to move away from his assumptions about our "fundamentally different" ways of seeing the world.

At one point, I emailed him the following block quote without explanation from Stuart Hall's lecture, *Representation and the Media* (1997). In the video recording, Hall says:

> "The statement, "Nothing exists outside of discourse," is a sort of claim that, as it were, there is no material existence, no material world form, no objects out there, and that is patently not the case. But to say that "Nothing meaningful exists outside of discourse" is a way of summing up what I think I've been trying to say to you." (Hall 1997)

Although Tom asked for clarification at the time about why I thought it was important to share this quote, now, it seems to summarize the key difference we imagined between us, or at least a recurring sticking point. I told him then that I was worried it might seem to him, in my tendency for hyperbole, that I was the one saying that "nothing exists outside of discourse." (Once I even added, "Maybe I am always just pushing the relativist side too much for the sake of discussion; that's a habit of mine.") He joked in reply, "Now I understand this quote's context, at least as a statement from you to me that you are not a material-world-form-denier." In our discussion about GWAS, after qualifying my tendency to be polarizing, I stressed again that for me, since scientific classification schemes like cases and controls are heuristics—"manufactured tools to 'think' with"—they have, like any tool, "a limited area of effect." I added that:

> "[It] shows us one thing, really what we ask it to, but it shuts down other ways of seeing; and we have to imagine what we want to see in the first place, before we can design a way to measure it. And we use cultural ideals and values about what we think nature and life are, to do this. I'm not saying genetics aren't real; I am saying we can never see or understand them without parsing them culturally."

He replied:

> "If [heuristic] means any model of human genetics is made by humans, I agree. But I also think genes are words we use to describe things that are"there" and measurable in the real or concrete or physical world (which humans don't see perfectly, but is there). E.g. I think chromosomal sex is "real" (yes still problematic, yes can be ambiguous, yes to be approached humbly) in some ways that gender isn't (of course problematic, of course ambiguous, of course humbling).

"The tension between the existence of knowledge as pregiven and its creation by actors has long been a theme which has preoccupied philosophers" (Latour & Woolgar 1979, 174-175). This fission of fact takes on moralizing dimensions when described as "truth," which we began to do.

Before my research in the lab, I believed that, in its quest for universal truths as I understood them, bioscience is rarely interested in conscious self-reflection and critique. I was also dedicated to seeing the way "acquisition[s] of truth" are a function of power (Foucault 1980, 131), and how histories of political dominance, like those described by Anna Tsing for botany (2005), have had a profound effect on what gets defined as universal truth in bioscience. Similarly, in *Laboratory Life* (1979), Latour and Woolgar demonstrated the way scientific knowledge, or a certain kind of truth, always emerges in the daily "conversation exchanges" between scientists and their "continual generation of a variety of documents" (1979, 168, 151).

Take the cassowary as an example (figure 3 above). Linnaeus' decision to include the cassowary in the same genus as the ostrich for his *Systema Naturae* (1748) was criticized when he was accused of having "imperfect knowledge" on zoology, and ornithology more specifically (Allen 1910, 317). In fact, J.A. Allen describes Linnaeus's exposure to ornithology literature as "exceedingly defective" and to extant birds of the time as "deprived" by his isolation (317-318). How to classify the cassowary, or anything else for that matter, then always emerges within the cultural and political context of scientific practice. Yet, for a long time anthropology positioned bioscience and taxonomic schemes as if inert and neutral. In another text, Latour chided:

> "Since the time of Levi-Bruhl, anthropology has always been interested in science, but in the sciences of the Others. [...] [H]ow come that for Them the cassowary is not classified as a bird, this was a legitimate question; how come that modern taxonomists do classify the cassowary as a bird was not in the purview of anthropologists." (Latour 1990, 145)

This broader bias in scientific inquiry—seeing 'local,' or really, non-white, knowledge as culturally and socially constructed (cassowary as not-a-bird) but internalizing Western science as *epistemology* (cassowary as bird)—can limit the accessibility of bioscience to anthropological investigation even today. In Tom's case, while he welcomed me in the lab, he had clear ideas of what my "postmodern" investigation would be useful for. I challenged him once:

> [You trust me to say] something useful and valuable about science? Even in a relativistic bent? Or only as long as I keep to talking about things like SciComm and not about telomere position effects?

His reply was reassuring but he capped it by clarifying that "there are also some questions the lab is striving towards for which I am not so welcoming for a po-mo analyses e.g. 'what is the genotype of mouse #3')."

I wondered then whether there really was a fundamental conflict between a bioscientific quest to uncover a universal biological truth, and an anthropological perspective that sees a process of multiple, divergent, and never settled truth-making at the core of human relationships. I think Tom and I actually saw the world in very similar ways; we both defined realness and reality as things and ideas that had weight, that could muster or even move the material and ideological properties around them. But while Tom shared my view that bioscience is a rough tool to measure the "real" or material world, he added that it can be "useful to approximate, recognizing that this can only be done imperfectly, for good and useful, and potentially 'just,' purposes." In other words, scientific models are "good to think" (Levi-Strauss 1962, 89). I had already observed the constant emphasis in the lab on experiments as acts of *modeling* reality and I found that bioscientists don't dismiss meaning in their work; rather, they confront the question of whether what they are anticipating as universal truth is in fact meaningful, even socially contingent, reality. In my conversations with Tom, I came to appreciate the ways that bioscientists are just as concerned with self-conscious reflection, and already draw from a rich critical tradition within the natural sciences (for classic examples see Kuhn 1962, Popper 1959). It was perhaps largely our disciplinary training then to initially privilege "reality" on one hand, and "meaningful reality" on the other, as if incompatible, when they are inseparable. And our habits of presentation made it sometimes harder to see, or concede on, the ways our thinking productively merged. More than once, I wrote to him something along the lines of, "Actually, that isn't so different from what I am trying to say…" Still, we came to our discussions with the typical classificatory schemes, and boundary work, that serve as disciplinary crutches for thinking about the world, in turns playful and hyperbolic but also at times suspicious, and maybe concerned we were barging into profane territory. We battled over the things we wanted to take for granted and felt protective over, and in my case wrestled as much internally as in conversation with our set, prescribed views and our divergent training. If we were suspicious of each other, I think we were equally suspicious of the limits of our own knowledge, although he wrote to me once, “I am most deeply satisfied in acknowledging that there are things I don’t know.”

GETTING FROM DISCIPLINARY DELUSIONS TO MATTERS OF CONCERN, OR WHAT HAS TO BE A CONCLUSION

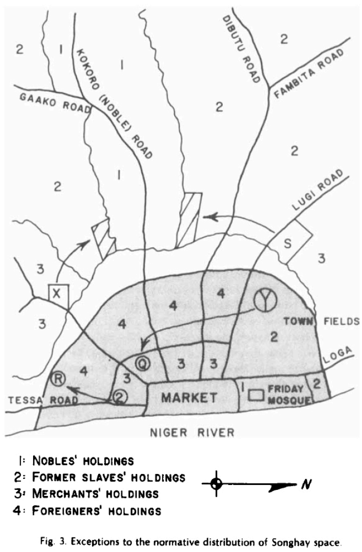


Figure 4: Mapping exceptions, Paul Stoller, 1980. (Used with permission of the author.)

Somewhere in the middle of the 21-day MMS project, stuck at the bottom of an email about an upcoming meeting, Tom sent me a quote from Latour:

> “If this were possible then we could let the critics come ever closer to the matters of concern we cherish, and then at last we could tell them: “Yes, please, touch them, explain them, deploy them.” Then we would have gone for good beyond iconoclasm.” (Latour 2004, 248)

The reading had been suggested to us by the MMS prompt #15 which pointed to Latour's insistence that, as the prompt described, "we rebuild a different sort of critique by moving from 'matters of fact' to 'matters of concern.'" As I began to realize later, it was the combative, polarizing history between qualitative and quantitative sciences that Tom and I were staging and redrawing in text. Maybe with this copy and paste of Latour's statement, snuck noncommittally into a routine correspondence, he was asking me, or even both of us, to overcome this binary, and for me to handle my observations of the lab as "matters of concern." But our different disciplinary language, even the different goals we may have had in communicating in the first place (it was initially my research project, after all), made it difficult to cross over boundaries when we reached them. Ultimately, that meant it was much more difficult to (be motivated to) find a way to innovate or redraft our epistemologies into a syncretic and useful set of shared concepts that could become a basis for further, or future, collaboration, as I'd hoped.

The skeptical divides we faced, acquired in training and structurally reinforced by our disciplinary institutions, were reproduced by us here in action, in talking about and with each other, and in our presumptions and the rhetorical manifestation of our inherent incongruities. The positioning of anthropology as a form of social critique, under which bioscience itself falls to examination, has perhaps only exacerbated this relative acrimony. At the same time, there remain key differences in the operationalization of concepts of objectivity and truth which do seem discordant, if not incommensurable. Over at least the last half-century, a major thrust in anthropology has been a critical engagement with concepts of 'truth' and the production of knowledge as shifting social discourse negotiated at a specific time and place, amid specific relations of power. The biosciences tend to emphasize truth as an independent objective factor which can be discovered by the researcher, through increasingly sophisticated technological and intellectual feats. Yet, we share an interest in the process of discovery, founded on the assumption that there is in fact something a priori to discover. To suggest that the appearance of an asocial and ahistorical material reality is merely a consequence of the practices and projections of science implicates bioscientists themselves in participating, even unwittingly, in this construction. Further, targeting bioscientists and their daily work in the lab for critical analysis then appears to accuse them of oversight and inattention. It is the application of etic forms of showmanship, where epistemologies of critique trump emic, "native distinctions, significances, and meanings" (Harris 2001, 576). It is no surprise within this clash that any productive intersections might suffer decapitation under the "sword of criticism" (Latour 2004, 248).

Over thirty years ago, after fearing he had misinterpreted Songhay village organization as a "static reification" of social order, Paul Stoller reflected that anthropologists "must struggle to comprehend systems of symbolic and social relations that are, for the most part, outside the scope of their experience" (Stoller 1980: 420). He wondered:

> "Do most anthropological analyses suffer from significant omissions generated from the "delusion" of the anthropologist's perception? Are most anthropological theories based upon misconceptions stemming from the inability of the anthropologist to perceive something his or her informant takes for granted?" (1980: 419)

In his text, Stoller describes gradually overcoming his theoretical assumptions, arrived at through a careful consideration of the history of relations between Songhay royalty, former slaves, and the emerging merchant class, as he mapped out their "field allotments" (1980, 426; see figure 4 above). Returning to what he first viewed as "exceptions" to the norm, Stoller realized that land holdings defying categorization, or really theoretical assimilation, were signs of conscious and contentious political transformation. Stoller's description of the incongruities he encountered in the field, trouble arising when an "ethnographer [...] sees roads which intersect, while his informant see[s] roads which end in forks" (Stoller, 427), describes for me the challenge of single disciplinary approaches to human problems such as this pandemic. Within a single discipline, the potential for exceptions to be easily filed away as "noise in a theoretical system" (Stoller

1980, 427)—what may at first look like dead ends or meaningless meanderings—is multiplied. A pandemic like COVID-19 dramatizes the fundamental interconnection between human social complexity and intricate biological life, but "social complexity" and "biological life" are already divided heuristics, terms which merely describe the same phenomenon, just as forks and intersections describe differing ways of parsing and experiencing the same crossroad. My tentative and temporary collaboration with Tom, for me, reconfirmed the necessity of merging distinct epistemological visions—to bring together the intersections and the forks—just as it showed me the institutional and preconceived assumptions which remain at the crux of why this may be so difficult, for now.

For a time, Tom and I were liminally suspended, inside a "moment in and out of time" (Turner 1969, 96). Working from home like others, transfixed in place, and communicating together exclusively online, we both agreed that in these troubled times, our experiences were, as he wrote, "not so troublesome."[10] But still together we passed through "a limbo of statuslessness" (97). It is at these times that, Turner said, "In such a process, the opposites, as it were, constitute one another and are mutually indispensable" (97). And for a time, I saw the promises that this indispensability might bring. But as much as living within threshold moments might be transformative, such frames of experience themselves by definition abide by ritualized rules which reinsert us into persistent, pre-established social roles afterwards, through "reaggregation" (94). Liminal states themselves are codified, an ambiguity that maintains, embodies, and enacts all sorts of cultural and institutional procedures. In Steiner's example, border zones—those moments of crossing over—similarly work to fix and transform value in socially (and legally) acceptable ways even as they disambiguate objects from other previous contexts. These rules then guide our possible figures or potentials for transformation along predictable, well-worn routes; at times returning us to where we came from, or sending us off exactly to where we are expected to go. When the emergency declaration was over and our conversations ended, I worried that I resettled too easily, like glue, into previous ways of thinking and doing. And that an opportunity for change had passed me by.

In another context, Nick Seaver wrote, “the boundaries around corporations, field sites, and algorithms are enacted socially, and they carry with them ideologies of access and knowledge” (Seaver 2017, 4). Disciplinary boundary work then must invariably involve a negotiation of these ideologies. But negotiation, and even recognition, does not guarantee a passage through difference out to some other syncretic side. Recently, I have listened to more than one anthropologist complain about the difficulty of working with engineers and computer programmers who, like bioscientists, don’t “share the same epistemologies as us.” And I have observed the way boundaries between academic disciplines get reified in simple, everyday ways: performed and maintained across preprint servers, prioritized government funding, journal subscriptions, departmental orientations, public attention, news cycles, and correspondences just like this one.

Yet, to present my conversations with Tom as emblematic of the kinds of “empirical [...] conversational tension” that Moats and Seaver describe (2019, 3)—a meta-comment on a century-long disciplinary divide that was doomed in advance to fail—is also, in a way, to fail to do justice to its complexity, and even its everydayness. After all, Tom told me more than once that his participation in the MMS project and seeing where my “tradition/style of anthropology and sense-making is situated within postmodernity and humanities/poetics” was useful for him. What doing useful science looked like was a topic we returned to often (although in our conversations, it was his biological research that we both tended to privilege as “science”). As he was reviewing and commenting on an early draft of this paper for me, Tom forwarded me an article about epigenetics. In the article, Scott F. Gilbert critiques genetic reductionism and argues for the inverse of the isolated unit of analysis. He reminds bioscientists that, “In the epigenotype, the gene is not an autonomous entity; it is part of a network of interacting components” (2003, 90, 91). We can hardly see the network and its mechanisms, then, without a grasp of its parts, or the parts without a sense of the whole; we can hardly see the cassowary without the question of if it is really a “bird,” or the Japanese Fancy Mouse 1 without a view of its global circulation as a scientific material. But of course, as Tom was reminding me, it isn't only anthropologists who can see this way. Moats and Seaver describe a similar moment for qualitative scientists when they "attempt to collaborate with data scientists." They:

> [...] often realize that their counterparts are well aware of many questions around complexity, politics, and performative effects, but make sense of them in distinctive ways. (Moats & Seaver 2019: 2)

We can choose then to use critical approaches, not as a dead end, an empty critique, or a sword for the sake of deconstruction, but as a useful way to see what bioscience does in the world, and more importantly to me, to do things together *with* bioscience. It is Latour again who for me offers an encouraging way forward: "What would critique do if it could be associated with more, not with less, with multiplication, not subtraction?" (Latour 2004, 248). It would be easy for me to end here, simply with this noncommittal and elusively positive statement, but alone, as a statement about statements, it can hardly advance any lasting change.

Once during our banter about postmodernism, I asked Tom if he could see the possibilities for a postmodern bioscience. He answered:

> [S]ure, it could be possible. I can imagine that a postmodern natural science would develop, but I would probably not read the papers, and my bias is that it would be less useful... but I hope I would humbly judge it by its fruits.

On their own, researchers can strive to overcome the delusions of their training, to move to respective "matters of concern," to merge intersections with forking paths, to craft "moments in and out of time" (Turner 1969: 96), and to find better ways to talk to each other. But whether we decide to read the papers or not, that exponential potential for syncretic multiplication remains powerfully constrained by institutional structures and biases outside the control of individual researchers, even as we learn to internalize and reproduce disciplinary boundary work. If "science not communicated isn't [even meaningful] science"—science defined then through its communicative imminence or failure—why do we continue in practice to so distrust, and disambiguate our disciplinary visions from those in other fields? Tom and I, like so many others teleworking, reaching out electronically and hesitantly, spontaneously joining and collaborating, were personally motivated because the pandemic confronted us with death and sickness, and an isolation we could barely make sense of with the rough tools we had been given. In response to one prompt from the MMS project, Tom wrote that: "COVID and COVID-19 are loud energy proclaiming death is coming, and death lays to total waste my sense-making and significance-making." Even in a moment such as this one, a chance to reach across a gap we normally barely ever attend to, a moment that demanded we come together for the sake of scientific advancement to speak together about the "awful unity of all living things," as Tom once described it, we eventually lost sight of, or came to the end of, that thread.

In the final stages of our collaboration, Tom and I drafted a letter to the president of his scientific institute, supporting an initiative for the integration of the natural and the social sciences. Together, we wrote:

> "In any disciplinary field, researchers learn to draw relatively arbitrary boundaries around their object of study. Collaborative interdisciplinary research requires researchers to confront these boundaries, to understand them better and to rewrite them when necessary; it is in this process that meaningful innovations in scientific thinking and practice can occur."

I know now that it isn't enough to make an independent push for a collaborative cross-disciplinary approach to research on the ground, although we tried in our own way. What's needed is a supportive institutional structure that sees the value in crossing that ground in the first place.

ACKNOWLEDGEMENTS

I want to thank Tom [pseudonym] for his generosity and insight, and the time and effort he devoted to this collaborative project. I also want to thank Paul Stoller who wrote recently of "Troubled Times" (2017) and whose work helped to inspire me to reflect on the complexities of relations in the field and the limits of anthropological (in)sight. I want to further thank three anonymous reviewers who provided helpful and detailed suggestions for improvements. Jonathan Corliss greatly influenced my vision for this paper, just as he does for everything else. Of course, the many, many blunders are all 100% mine. This research is supported by the Japan Society for the Promotion of Science's Grant-in-Aid for Scientific Research (C) 20K01188.

WORKS CITED

Allen, Joel Asaph. "Collation of Brisson's genera of birds with those of Linnaeus." *Bulletin of the American Museum of Natural History*, vol. 28, 1910, pp.317-335.

Appadurai, Arjun, ed. *The Social Life of Things: Commodities in Cultural Perspective.* Cambridge: Cambridge University Press, 1986.

Foucault, Michel. "Truth and Power." *Power/Knowledge. Selected Interviews & Other Writings 1972-1977 by Michel Foucault*, edited by C. Gordon. Brighton: Harvester, 1980, pp. 109-133.

Fullwiley, Duana. *The Enculturated Gene: Sickle Cell Health Politics and Biological Difference in West Africa.* Princeton: Princeton University Press, 2011.

Fullwiley, Duana. "The Molecularization of Race: Institutionalizing Human Difference in Pharmacogenetics Practice." *Science as Culture*, vol.16, 2007, pp.1-30.

Fujimura, Joan H., Bolnick, Deborah A., Rajagopalan, Ramya, Kaufman, Jay S., Lewontin, Richard C., Duster, Troy, Ossorio, Pilar and Jonathan Marks. "Clines Without Classes: How to Make Sense of Human Variation." *Sociological Theory*, vol. 32, no.3, 2014, pp.208-227.

Gieryn, Thomas F. *Cultural Boundaries of Science: Credibility on the Line*. Chicago: University of Chicago Press, 1999.

Gilbert, Scott F. "The reactive genome." *Origination of organismal form: Beyond the gene in developmental and evolutionary biology*. Edited by Gerd. B. Müller and Stuart A. Newman. Boston: MIT Press, 2003, pp. 87-101.

Henley, Paul. *Beyond Observation: A History of Authorship in Ethnographic Film*. Manchester: University of Manchester Press, 2020.

Kuhn, Thomas S. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press, 1962.

Latour, Bruno. "Why has Critique Run Out of Steam? From Matters of Fact to Matters of Concern." *Critical Inquiry*, vol.30, 2005, pp.225-248.

Latour, Bruno and Steve Woolgar. *Laboratory Life: The Construction of Scientific Facts*. Beverly Hills: Sage Publications, 1979.

Levi-Strauss, Claude. *Totemism*. Translated by Rodney Needham. Boston: Beacon Press, 1962.

Linnaeus, Carl. *Systema Naturae*. Laurentius Salvius: Holmiae, 1748.

Lock, Margaret and Vinh-Kim Nguyen. *An Anthropology of Biomedicine*. Oxford: Blackwell, Ltd, 2010.

Moats, David and Nick Seaver. "You Social Scientists Love Mind Games'': Experimenting in the ''divide'' Between Data Science and Critical Algorithm Studies." *Big Data & Society*, January-June, 2019, pp.1-11.

Popper, Karl. *The Logic of Scientific Discovery*. London: Hutchinson & Co, 1959.

Ruben, Robert J. "The Mouse: From Pet to Paradigm." *Otology Japan*, vol.15, no.3, 2005, pp.259-264.

Seaver, Nick. "Against Access: Two Ethnographic Vignettes, One Malinowskian Anecdote, a Claim about Corporations, Algorithms, and Anthropological Field Sites, and an Argument about the Sexual Politics of Knowledge." *Society for Social Studies of Science*, Boston, 2017.

Serre, David and Svante Pääbo. "Evidence for Gradients of Human Genetic Diversity Within and Among Continents." *Genome Research*, vol.14, 2004, pp.1679-1685.

Steiner, Christopher B. “Rights of Passage: On the Liminal Identity of Art in the Border Zone.” *The Empire of Things: Regimes of Value and Material Culture*, edited by Fred R. Myer, Santa Fe: School of American Research, 1996, pp. 207-232.

Stoller, Paul. “Doing Anthropology in Troubled Times.” *HuffPost*, 27 Nov. 2017. https://www.huffpost.com/entry/doing-anthropology-in-troubled-times_b_5a1c4300e4b0e580b35371c5

Stoller, Paul. “The Negotiation of Songhay Space: Phenomenology in the Heart of Darkness.” *American Ethnologist*, vol.7, no.3, 1980, pp.419-431.

Tanave, Akira and Tsuyoshi Koide. “A Role for the Rare Endogenous Retrovirus β4 in Development of Japanese Fancy Mice.” *Communications Biology*, vol. 3, no.53, 2020, pp.1-3.

Teienshi. *Chingan Sodategusa (How to Raise Rare Mice)*. Kyoto: Zeniya Chobe, 1787.

Tsing, Anna L. *Friction: An Ethnography of Global Connection*. Princeton: Princeton University Press, 2005.

Turner, Victor. *The Ritual Process: Structure and Anti-structure*. Ithaca: Cornell University Press, 1991.

Wise, Jacqui. “Covid-19: Known Risk Factors Fail to Explain the Increased Risk of Death Among People from Ethnic Minorities.” *BMJ* 369: m1873.11 May 2020.

Image Notes

Figure 1: Mice with various coats, including piebald (top left), from the 1787 Japanese book Chingan Sodategusa, public domain. (Modified by author.)

Figure 2: Objects crossing borders, becoming subjects. Images from British Library Collection, public domain. (Collage by author.)

Figure 3: The cassowary, public domain. (https://pixabay.com/vectors/cassowary-bird-feathers-neck-48000/).

Figure 4: Mapping exceptions, Paul Stoller, 1980. (Used with permission of the author.)

NOTES

1. http://www.med.miyazaki-u.ac.jp/AnimalCenter/mouseDB/labomice/html/041.html↵

2. https://www.nig.ac.jp/nig/2013/05/research-highlights/20130528.html↵

3. When Tom read this quote later in another paper I was drafting, he highlighted the word "important" and replied to me in a comment box: "I'm not sure what I meant by this 'important,' but I guess I made my point. Trump-ian."↵

4. A key point of context for this hope is that I have long idealized the collaborative, provocative approach of the anthropologist and ethnographic filmmaker Jean Rouch. His work insisted that collaboration "afford[s] a much more profound understanding of the subjects' world than one posited, in the name of science, on a radical separation between observer and observed" (Henley 2020, 225).↵

5. Although there is already lively work on this topic within disciplines such as biological and medical anthropology, these fields at times often fail to reach, or fail to be appreciated as relevant to, the broader bioscience community.↵

6. In his final review of this paper, Tom wrote in a comment box: "To qualify this a bit, I think I meant that while the 'tools' of science applied to any question might reveal something of truth to an individual, unless it gets communicated, I would not consider it fully a part of the social activity we (most?) recognize as science."↵

7. Global health presents countless examples of "local biology" (Lock & Nguyen 2010, 90) and behavior that is well adapted in context reversed by policy makers and public health practitioners who cite it as the root cause of poverty and disease.↵

8. When reviewing the final version of this paper, Tom wrote to correct my description: "This article looked for genetic variants on autosomes that were statistically associated with sex. This is interesting because the simple assumption is that the genetic variants associated [with] sex are the sex chromosomes themselves (i.e. NOT the autosomes). This description of the article doesn't reflect that understanding, to me, although I am not sure it matters." His correction for me demonstrates again the necessity for collaborating and calibrating across disciplines.↵

9. Although more recently referred to as ethnic groups, where “trans-ethnic” comparisons for GWAS are a common method, such studies might take national populations as genetically salient. This tendency has been critiqued by biological and medical anthropologists, along with others (see for example Serre and Pääbo 2004, Fullwiley 2007, Fujimura 2014).↵

10. Paul Stoller has used this very phrase recently (2017). I want to acknowledge his general influence here, as I borrow from one of his titles in my own for this article, as well as his mapping of Songhay social space which I discuss above (1980). This phrase also appeared as part of prompt #17 from the MMS project.↵