Collaboration and critique
In between the Battelle Diagnostic Inventory and ethnography

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Abstract

What is the place for critical ethnography in global health and other related forms of international intervention? This essay explores this question through my experience collaborating with a nongovernmental organization (NGO) working to improve children's well-being in El Salvador and with a team of quantitatively oriented researchers evaluating the NGO's project. I adopt as an analytic lens one of the metrics the researchers used – the Battelle Developmental Inventory – in order to explore the possibilities for dialogue among quantitative and qualitative methods (and researchers). At issue is the place that critique can have in such dialogues, particularly when the aims of NGOs, public health researchers, and ethnographers can sometimes be at odds. Ultimately, I make the case that viewing critique as a praxis, keeping an eye on the commensurability of aims, and being prepared for occasional productive friction are some practical steps for bringing critical ethnographic perspectives into contact with interventions.

Keywords

The Battelle Diagnostic Inventory-II is an early-childhood assessment tool that is used to measure the personal-social, adaptive, motor, communication, and cognitive skill sets of children aged between six months and eight years. Sold by Riverside Publishers, each Battelle

kit comes with an embroidered bag, plastic toys, score sheets, and booklets, and costs about US$1,500. Battelle kits are also pretty heavy. In July of 2015 several arrived at my doorstep. My colleagues, university-based population health scientists in public health, had asked me to carry the kits into El Salvador and deliver them to a nongovernmental organization (NGO), Helping Hand,[1] which was preparing an intervention in childcare centers across the country. My colleagues had been hired by Helping Hand to conduct an evaluation of that intervention; the Battelle kits would be one of the ways my colleagues would understand whether the NGO's planned intervention would improve child well-being.

As the social relations assembled around this suitcased object indicate, I was ethnographically betwixt and between: part research collaborator, part NGO consultant, and part independent anthropologist. In this essay, I use the Battelle kit as a leaping off point to consider two questions: What possibilities and pitfalls does such in-betweenness offer critically engaged anthropologies of international aid? And, more specifically, what happens to 'critique' under collaborative research conditions? I understand critique in a Foucauldian way, as thinking reflexively about how knowledge and power constitute the present (Foucault 1997).[2] Critique, according to Judith Butler (2000), is a practice that interrogates 'the limits of our most sure ways of knowing'. As this pertains to international interventions, critique means exploring both the intervention on its own terms as well as the broader relations of power within which the intervention unfolds.

In what follows, I draw on my collaboration with Helping Hand and my research colleagues between 2015 and 2017 in order to reflect on the potentials and limits of critique in the context of international interventions. I want to suggest that when working in between, the practice of critique need not change, but the ends – what critique is for, and how it comes to be used and voiced – may well require adaptation. It is important that these two dimensions are not conflated; nonetheless, not conflating them can be difficult. The practical strategy I discuss here involves keeping an eye on the commensurability of aims. In some cases, making critique more commensurable with interventions can provide NGOs, and the ways our colleagues in other disciplines and fields know their efforts through evaluation, with an

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1   A pseudonym. Helping Hand is a Euro-American NGO that works internationally on issues related to children's health and well-being.

2   Judith Butler (2000, paragraph 9) points out that, for Foucault, critique has no essence. Rather, it is defined situationally in relation to its objects. Critique is also nonevaluative: it 'will not be to evaluate whether its objects – social conditions, practices, forms of knowledge, power, and discourse – are good or bad, valued highly or demeaned, but to bring into relief the very framework of evaluation itself. What is the relation of knowledge to power such that our epistemological certainties turn out to support a way of structuring the world that forecloses alternative possibilities of ordering?'

important critical perspective. Commensurability is also a two-way street: discussions with those colleagues can prompt questions for anthropologists about what critique is for. At the same time, some modes of critique – particularly structural ones – may not be easily 'actionable'. In such cases it may be necessary that critique be less commensurable with interventions. Attending to the commensurability of aims is a way to do critical scholarship that is also proximate to interventions. But a vital prerequisite is refining how one thinks about and does critique. Critique has to be a praxis, not a theoretical or ideological position.

In-betweenness

My complex involvement in Helping Hand's project as an evaluator and collaborator grew from conversations with colleagues in global health. In contrast to my uninformed stereotype of numerically blinkered social scientists, my colleagues were acutely aware of what they called the 'cultural inappropriateness' of many international interventions. They were eager to better ground their work in regional contexts. We also shared an interest in children's well-being and the transnational circulation of psychological idioms. Out of their interest in qualitative work, my research colleagues invited me to participate in evaluating Helping Hand's intervention in El Salvador.

Helping Hand is a Euro-American NGO that seeks to improve child well-being by promoting psychological attachment between children and their caregivers – in this case, *educadoras* who work in public childcare centers across the country. To do this, Helping Hand offers training to *educadoras*, center directors, and government officials. A few assumptions underlay their intervention: first, caregiving can and should be improved in El Salvador, second, fostering 'secure attachments' is the way to do it, and, third, secure attachments promote child well-being.

Fred, my research colleague, was to guide Helping Hand in structuring the intervention in ways that would yield statistically robust results, and then, down the line, conduct an evaluation. Thus, he advised Helping Hand on randomization procedures. He helped them identify which psychosocial instruments they should use to measure results, and how to use power analysis to determine how many intervention sites were needed to produce statistically significant results. Those results, if positive, would, in turn, be important for future projects and future project funding. Fred asked me to organize the qualitative part of the evaluation. And, so, while he and his team would deploy the Battelle (along with one other tool), I would be deploying ethnographic research methods.

Helping Hand had chosen to work in a particularly challenging context. Since the end of the decade-long civil war in 1992, El Salvador has been subject to extensive transnational gang

violence (Zilberg 2011), crime, and general public uncertainty (Moodie 2011). Nearly one-third of Salvadoran children live in poverty (World Bank 2014). Approximately one-quarter of children do not complete primary school (UNESCO Institution for Statistics n.d.). Families are frequently divided as one or both parents work in the United States and send remittances back home (cf. Wiltberger 2014). Young people also experience forced gang conscription, compounding the social effects of poverty and vulnerability. Those who flee to points north risk falling into the hands of US border security (Kennedy 2014) or worse the 'desert deathscape' at the US southern border (De León 2015). Helping Hand's working hypothesis was that an intervention into caregiving might ameliorate some of the negative, and often life-threatening, dimensions of these social dynamics.

Between 2014 and 2017 I worked closely with five Salvadoran ethnographers to document daily life in the childcare centers that would later be targeted by Helping Hand. We focused on how *educadoras* conceived of care, attachment, and health. As with carrying any heavy suitcase, my role was a bit awkward. Departing from the model of strictly objective evaluation, our team used ethnographic research to inform the intervention before it started, providing recommendations to Helping Hand about how their trainings might better respond to the needs of *educadoras*.

During and since that time, I have been reflecting critically on this project and the politics of 'attachment' in the context of El Salvador's epidemic of gang violence, policing, and poverty. These roles have required me to learn to do, and think, things a bit differently. In my first book project (Matza 2018) I worked alone, doing all my own fieldwork; in the case of my work with Fred and Helping Hand between 2014 and 2017 I worked collaboratively with other ethnographers, relying to a large degree on their observations and insights. That reliance also meant sacrificing a degree of 'being there' in the field (Borneman and Hammoudi 2009). This work also presented new sorts of questions: To what degree will my scholarship remain legible as a form of critical anthropology? In adopting the role of insider, what do I owe my collaborators? Would the role of an outsider be preferable? Finally, and most central to this essay, to what degree is critique possible in light of these collaborative commitments?

Critical (applied) ethnographic research

I am far from being the first anthropologist to wonder about the relationship between critical scholarship and applied work. Applied anthropology goes back at least to Franz Boas' use of craniometry to upend eugenics, and in health to W. H. Rivers (cf. Whiteford 2004),

and, later, George Foster (cf. Packard 2016). Many others have followed.₃ My pathway into critical, collaborative ethnography is unique only inasmuch as it is part of a professional trajectory that began at the intersection of critical theory, interdisciplinary studies, and sociocultural anthropology at Stanford University in the early 2000s. At that time, for many good and important reasons, I could not have imagined working with an NGO or with quantitative researchers. I was immersed in writings that explored the colonial legacies of the Western episteme in general and anthropology in particular. Critiques of development and the 'NGOization' of assistance in neoliberal times were abundant. My aim in this essay is not to overwrite the many foundational contributions to the discussion of critical applied work, but rather to contribute a consideration of how I have struggled to fit critique into this form of engaged research. I emphasize the word 'struggle' here, and note, again, that this is grounded in my specific professional experience. Perhaps these comments will resonate with others.

For the purposes of framing, consider two examples in critical applied anthropology. Both combine some degree of critique and some degree of engagement. One, a form of public anthropology described by Didier Fassin (2013), foregrounds the practice of critique and is less collaborative in nature. The other, a form of feminist praxis described by Louise Lamphere (2004) and Julie Hemment (2007), foregrounds collaboration and seeking out mutual aims with research participants, but is somewhat differently concerned with critique than in the way I have defined it. I want to plot a course that builds on each of these forms of critical ethnographic practice.

In his article 'A Case for Critical Ethnography', Fassin (2013) describes his awkward positionality vis-à-vis the pressing contexts of HIV-related research in South Africa and the policing of immigrant neighborhoods in Paris. He considers the aftereffects of scholarship on contemporary problems in order to ask: What role(s) can critical ethnography have in shaping public discussions? What kinds of challenges – ethical, political – do scholars face as their work 'goes public', and how should they respond to them? Fassin argues that ethnographic research unfolds in time, and includes not only fieldwork and academic writing, but also sharing scholarship in public ways. As such, it involves different critical and ethical engagements. A consideration of each of these as distinct 'moments', he argues, will enhance critical ethnography.

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3   Many scholars have tried to reconceptualize critical applied anthropological praxis. For some key examples see Hymes (1972), Harrison (1998), Rappaport (1993), Lamphere (2004), and Hackenberg and Hackenberg (2004).

There are things with which I identify in Fassin's article, namely his interest in the question 'what can critique do?' At stake is a form of critical independence whose primary concern is to be emphatically non-normative. 'My task, as a social scientist', he writes, 'was to account for things are they were rather than how they should be' (Fassin 2013, 123). But in comparing Fassin's experiences to mine I see some important differences. Fassin's positionality is avowedly independent. The lone-wolf anthropologist moves between different actors; pieces together social lives, cultural meanings, and structural logics; and crosses discursive boundaries. The approach involves 'risk' but it is a risk that takes place when ethnography goes public. By contrast, my work has not been of the lone-wolf type. Rather than independence there is interdependence. I have had multiple collaborations (and therefore a different sort of research sociality). As for risk, there is not only the kind that follows when one's writing hits the pages of *Le Monde*, as it has for Fassin (not that I think that mine would!) but also the kind that comes from putting one's ethnography and critical commitments at risk through participation.

The shift from the lone wolf to the collaborative ethnographer is quite a familiar one in feminist anthropology, which emerged with the social movements of the 1960s and 1970s, new kinds of anthropological collaborations with indigenous groups, postmodern critiques of voice, and postcolonialism (Lamphere 2004; Said 1989). A particularly helpful example is Hemment's (2007) work in Tver', Russia, with the NGO *Zhenskii Svet*, in which she deploys participatory action research (PAR) as a way to merge critique with participation in women's empowerment. She terms this 'critique plus', defining it as 'a mode of engagement that holds on to the important insights of critical ethnography and the goals of cultural critique, but which enables us to push beyond the deconstructive moment to engage in collaborative projects for social change' (Hemment 2007, 303).

As with Fassin, I find overlaps and divergences between Hemmet's approach and my own. I identify with Hemment's interest in collaboration and forms of engaged scholarship. But the mode of collaboration, and therefore also the aims of critique, are different. In a similar way to Hemment, Lamphere (2004, 432) prioritizes collaboration that 'involves members of the subject populations and shifts the balance of power toward partnership'. Critique in these instances is more likely to take aim outward, at a shared adversary: the state, for example, or a multilateral institution, or even a foreign NGO. By contrast, I am not collaborating locally, but rather 'collaborating up' (cf. Nader 1999) with a transnational NGO and fellow university-based social scientists. This changes the dynamics. A key difference in my collaboration from that of Hemment and Lamphere, then, is the possibility of a kind of 'friction' (Tsing 2005). Depending on one's partners, critique may mean asking questions that they deprioritize or consider settled, thus leading to internal frictions. Nonetheless, as Tsing (2005, 3–4) argues, friction among actors, projects, and ideas is an important

dimension of global connections. Those frictions are also myriad in their effects. They can be 'awkward, unequal [and] unstable' but also 'creative' (Tsing 2005, 4).

To summarize, then, my experiences resonate with Fassin's mode of critique, but multiple research commitments have also presented different sorts of risk. And my experiences resonate with Lamphere and Hemment's mode of collaboration, but the presence of friction – that is, of instances where different visions and aims can emerge – has posed a less straightforward research politics. What does this look like in practice? What would it mean to combine Foucault's (1997) 'limit attitude' – that is, thinking reflexively about how knowledge and power constitute (and limit) the present – with the challenging work of collaborating up, application, and intervention?[4] Conversely, how should one think about collaboration when aims may not always be aligned?

Quantities and qualities

Let's return to the Battelle, now unpacked. As a research technique, it is really very different from ethnography. This has as much to do with method (numbers, surveys, and standardization) as with expense and proximity to a larger research apparatus. A researcher can administer a Battelle test with one child in less than two hours, using several different kinds of screening activities, including involving the child in play-based activities with various kinds of objects, making naturalistic observations, and doing a structured interview. Researchers convert all of these activities into weighted scales, yielding, in the end, a series of scores. The scores are then matched up with developmental norms. (These are primarily based on US data).

The way that the Battelle scores intersected with the ethnographic work my team and I conducted is a good example of the possibilities for overlap, and also divergence, that I have been discussing so far. In terms of overlap, the Battelle scores were fascinating and potentially valuable for ethnographic site selection. And our qualitative work also had the potential to help reveal the worlds behind the scores. Yet these methods also diverged: where the former reached for comparability at nearly global scales (and thus sacrificed fidelity), the latter insisted on thickness at the expense of potential comparability.

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4   Foucault (1997) outlines the 'limit attitude' as a 'positive' form of critique in his essay 'What is Enlightenment?' The limit attitude is a philosophical ethos that 'consists of analyzing and reflecting upon limits', meaning that criticism, then, is practiced 'as a historical investigation into the events that have led us to constitute ourselves and to recognize ourselves as subjects of what we are doing, thinking, saying' (Foucault 1997, 315).

The Battelle is also an interesting object because it entered the field ahead of me. Before arriving at the Children's Welfare Center (also a pseudonym) in July 2015, I learned that some of the quantitative evaluators had already been there to collect Battelle scores. From having observed the training of the evaluators I knew that this involved taking biometric data as well as conducting tests of cognition, social skills, and emotional control. Fred, my colleague in public health, had offered to share the scores with me. In a few minutes' time the Excel file appeared on the secure, password-protected shared drive, and I opened it with some curiosity.

The scores showed that, while the children's motor skills were in the normal range, communication was one standard deviation below the average, placing it at 'low average', and cognition was two standard deviations below, placing it at 'mild delay'. My colleagues were particularly focused on the cognitive scores, which showed delayed perceptual discrimination, memory, reasoning/academic skills, and conceptual development. These suggested the kind of room for growth that Helping Hand was hoping to foster through its intervention.

As an ethnographer, I found these scores fascinating if also strange. Fascinating because they offered a transparency and comparability that was, ethnographically speaking, unattainable. At a glance the scores indexed a range of deeply interior characteristics, including intelligence, physical development, and emotional maturity. They also had a power that was enviable, making it possible to take the local and render it as a standardized object that could be dissolved into the national (nationwide scores) or the transnational (through comparison with developmental averages in the United States). Finally, the scores made manifest the usual ways in which ethnographic knowledge can feel too narrow to be of broad relevance.

But the scores were also strange. They spoke more forcefully than seemed warranted by three days of standardized engagement with a group of young people in a complex social space. And for all of the scaling limits of our ethnographic work, the occlusions of scaling up were apparent the moment we moved into and beyond the centers: in our ethnographic accounts of daily life in and around the centers, the difficulties of children's lives in the community continually seeped in. Key factors such as family life (where children's other primary caregivers resided) as well as the troubling presence of ordinary and extraordinary violence, were immediately apparent. These were also the kinds of factors that would likely also shape what the scores were measuring.

When I visited the center, which was eventually to be a site for Helping Hand's intervention, I got a clearer sense of those 'factors'. On a tour organized by Margarita and Angeles, two *educadoras*, I visited a community living on marginal land in a remote part of the country. Many families were also living with little money, and had to take their children out of school

to collect produce to sell at the market. Like many parts of El Salvador, gang violence engulfed the community; the canton in which it was located straddled a boundary between rival groups, making it dangerous for children to attend school because they had to cross that boundary.

In light of the Battelle scores, Jacqueline Duran's excellent fieldwork really stood out, giving crucial context. One set of conditions possibly shaping the scores was the difficulty of attaining a good education in this community: Jacqueline described a scene in which Karla, aged thirteen, and Lupita, ten, appeared in the local center. Both were too old to attend and should have been in school, but the *educadoras* made an exception for Lupita, the younger sister. The sisters were on their way to sell at the market, and Karla had brought her younger sister there to eat. Jacqueline noted how longingly Karla watched Lupita eat breakfast. They told Jacqueline that hopefully next year their mother would finally sign them up for first grade. They wanted to learn to read and write.

Jacqueline later visited their home and chatted with their mother, Rosa. Karla and Lupita also had a younger sister, Ana, eight, who attended the center. Ana loved to go to the center, not just to eat but also to play and study. But sometimes she had to stay at home to help her mother look after the house and clean while her father was farming. Since Rosa was pregnant, it was possible that Ana, too, would end up working once the baby comes.

Economic precarity merged with the loss of parents at home was undoubtedly another factor contributing to the scores Helping Hand collected: Isela, another child at the center, did not know where her mother was. Her grandmother Andrea suspected she was a sex worker in Guatemala. Jacqueline heard Andrea tell Isela, 'Don't follow the example of your mother, who is a drunk and a prostitute'. They had subsisted on selling produce for years, but now that Andrea, seventy-six, had lost the ability to move her right arm, she and Isela were begging for money to buy food. Andrea said that people tell her to give Isela away, but she answered, 'My granddaughter is not a chicken or something to give away'. Isela had started first grade, but not completed yet. The demands to subsist were too great. Andrea cried, lamenting that she cannot support her granddaughter's studies.

Jacqueline also visited Fernando, age six, who has a maternal home near Isela. His mother sent him to the center at a year and a half after his father left and she had no support. She explained, 'I was working for my mother, cleaning the house in exchange for food, and having Fernando with me made the work difficult'. She remarried and had two daughters. To her dismay, Fernando had decided to live with his grandmother. But he was badly treated there, sometimes beaten for waking his aunt's young baby. He continued to have incontinence issues, for which he was also punished.

What is a number?

A number, in the case of a Battelle score, is something with the power to tell a story about populations. By comparison, a case in ethnography is a personal narrative with a possible message about a community, a local economy, a family, a set of social relations. To what degree are these forms of knowing, and caring, commensurable? The scores point to population-level information, and are crucial to making arguments about results within the discursive regime of global public health. Ethnography cautions about scaling up, foregrounding the immediate concerns of particular people and contexts, but historically has been far less consequential for interventions. The scores are part of a practice of care for populations; ethnography, a practice of care for persons (cf. Stevenson 2014, 18).

It was exciting that Fred and Helping Hand saw value in ethnography. Seeking to merge Battelle with ethnographic research, they put into practice an awareness of the partiality of any knowledge form. But how were we able to make our ethnographic work useful? I confess that articulating these forms of knowledge and care with one another has not been straightforward. Nonetheless, our team pursued this through providing Helping Hand with an internal report. We approached that report with several aims in mind: First, we compiled moments from fieldwork that we thought would give voice to several kinds of *educadora* perspectives that might not otherwise be heard in the intervention context. These included statements on center needs and the kinds of expectations they had from the NGO, based on their prior exposure to NGOs. Second, we collected detailed life histories in order to humanize the targets of the intervention. Fieldwork indicated that most *educadoras* had experienced some kind of trauma. We felt that identifying these experiences (in anonymized form) would be important for the NGOs trainers to be aware of. Third, we documented the ways that the political economy and gang-related activities impacted the centers. These structural features, we reasoned, might prove major barriers to the NGO's work; knowing about things such as food shortages, theft from the centers, and surrounding violence might also support Helping Hand to better tailor their training. Finally, we documented as much as we could the home lives of the children. In conjunction with *educadora* life histories and structural barriers, we felt that descriptions of children's challenges at home, where they spent the better part of their days, would help temper the NGO's expectations about what a focused intervention could achieve.

As it would turn out, the intervention as originally planned experienced a funding setback and was paused for what, initially, seemed an indefinite period of time. After over a year of grant seeking, however, Helping Hand is taking up the project again. As before, they plan to make use of ethnographic research, much to their credit. My own research is now carrying me into other, related spaces in El Salvador; nonetheless, I understand from my colleagues that several aspects of our work have affected the organization's current plans. Initially,

Helping Hand did not plan to provide much in the way of material needs to the centers, feeling instead that high quality care could make a difference, no matter the conditions. Our documentation of *educadoras'* perceived needs, however, prompted the NGO to allocate more of their funds towards material assistance. As to our work on *educadora* life histories: in this instance Helping Hand's trainer was highly empathetic and skilled, with much experience working as a trainer. I believe that we essentially replicated information she already had about the *educadoras* and thus may not have much shaped the approach she took to the trainings. Nonetheless, I can say that we provided the kinds of life history context that may have helped the NGO leadership to better appreciate the challenges the women they were trying to train have faced.

And what of our work on placing the centers in their political-economic contexts, by describing the interpenetration of gang worlds and children's home lives? Here, I think, it is very hard to know what difference our particular ethnographic engagement made. Certainly, documenting the likely factors underlying the Battelle scores, as well as the likely impediments to improving well-being, was useful. But, as is well known, structural factors are among the most complex and difficult to transform. Both NGO and ethnographer meet the same wall here, one built of poverty, precarity, and community suffering. When viewed from the perspective of a single NGO, one can only feel extremely humbled in the face of such compounding and complex social challenges. Critique of this sort, at least in this case, was more difficult to translate into concrete action. Nonetheless, writing about these issues is no less important.

What is critique?

In closing, let me return to the questions I started with: What possibilities and pitfalls does a complicit, in-betweenness offer anthropologies of global health? And what is the nature and stakes of critique under conditions of involvement?

I do not think that my in-betweenness entailed an altered practice of critique. It remained possible to problematize the Battelle's truth claims by pointing to the lifeworlds it masked, to raise concerns about the quantitative 'evidence base' in global health work, and to indicate the overlaps of metrics like the Battelle and the neoliberalization and NGOization of social welfare, which here play out in the context of US-sponsored postwar trade and security policies in Central America.

But in-betweenness certainly alters the ends of critique, requiring compromise. The commitment to a particular politics that is necessary to perform critique with tenacity is vulnerable. And problematic entanglements may follow. Yet working in between can also

deepen critique. Working from within, our ethnographic team built a different sort of evidence base, one that documented worlds behind the Battelle. As we tried to indicate in our report, our evidence could be used to show that a flat Battelle score may be a function of structural factors rather than the *educadora*-child dyad. In-betweenness also made it possible to provide materials Helping Hand could use to make their trainings more responsive, to seek cooperation with local organizations, or to consider directing resources to infrastructural needs. Finally, thinking about the limit attitude of critique, in-betweenness allowed for a more proximate, even embodied view of the breaking points of power/knowledge – cultivating a feel not just for the incompleteness of metrics, but also a sense of what, and more importantly why, my colleagues try to transform and measure, what they hope to achieve. A structural critique, while absolutely crucial to mapping out the problems of interventions, is on its own and after the fact unlikely to achieve the kinds of things that our colleagues hope to do.

In the spirit of exchange, Fred and I later discussed the differences in perspective that emerged through the different methods we used to understand the same center. As he explained, the individual stories and differences were important, but what they were particularly interested in was seeing whether, at a population level, Helping Hand's intervention would make a difference. A statistically significant uptick in Battelle scores – and in particular one that would be attributable to Helping Hand's work – he reasoned, would indicate as much. We have since wondered together about how ethnographic evidence might be merged with his team's quantitatively based evaluation, but so far we have been unable to move beyond the narrower idea that ethnography can help to give 'texture' to things that emerge from the data or explain intervention failure. Can ethnography help a number say something more specific and structurally attentive about child well-being? I have also been thinking about the limits of ethnography itself through a set of (as yet) rhetorical questions: Can a number help the ethnographic case say something on a global stage? Are the ideas that 'it's complicated' and 'beware of unintended consequences' the only things I as an anthropologist can offer?

In other words, the exercise of comparing my methods with Fred's team's was hardly one of self-valorization. Apart from the self-imposed limitations many anthropologists place on their research findings (for good reason, of course), it was also striking that Fred's team's approach prompted me to reconsider ethnographic representation. From this point of view, the account of family and community life around the center risks participating in what Butt (2002, 2) has referred to as anthropology's deployment of 'suffering strangers', that is, people whose stories are 'valuable because they provide support for claims about justice and well-being'. Pointing to an irony, Butt (2002, 2) notes 'within other public contexts, anthropologists have been quick to criticize a cavalier use of images and tales [of suffering]'.

And, in the face of their engagement with social change, I have also had to consider the aims of my own work vis-à-vis the social needs we ethnographers documented in El Salvador.

On the other hand, collaborative models, on their own, may lose sight of how sometimes collaboration is impossible or even undesirable. In the process of writing in more detail about the challenges of Helping Hand's project and critiquing the hierarchy of evidence that often pervades aid work, I have also had to work through my colleagues' disagreements with what I have to say. I have had to weigh my arguments against their commitments to assist. As Fred and I have learned together, collaborations can strain in the face of critical commitments, on the one hand, and commitments to help others, on the other.

What I am trying to say – in maybe too roundabout of a way – is that I have found it possible to 'be critical' in collaborative/engaged work such as this, but that the trick comes when it is time to figure out how to make the critique known. It has felt important to offer critical insights to Helping Hand but at the same time keep open the possibility to write critically in other registers, for other audiences, where less compromise is required (for example, in anthropology journals). By being able to play with the difference between the practice and the ends of critique, I and my ethnographer colleagues have found ways to practice critique and also to offer something to the NGO that might bring the everyday concerns of Salvadorans into view.

I want to end by suggesting a theoretical concept to think about critique as form of praxis, rather than a theoretical position (cf. Enslin 1994). According to Elizabeth Povinelli (2001), commensurability results when radical worlds are domesticated, flattened, incorporated, swept into biopolitical norms. In contrast, 'incommensurability' refers to instances in which practices remain foreign to said norms. Not quite 'critique', they are nevertheless critical (cf. Matza 2018). A similar distinction could be used to think about an anthropological praxis with(in) global health. The question becomes not 'is ethnography complicit?' but, rather, how can ethnography be made productively incommensurable with global health agendas through offering constructive critique? And how can ethnography be made commensurable through making grounded research accessible to our colleagues? What seems clear is that we have a lot to learn from one another. In my case, I have benefited greatly from understanding how my colleagues work with numbers in interesting ways. Inasmuch as metrics can serve communities by bringing attention and resources, as well as improving interventions, intersecting those metrics with critical ethnographic insights has the potential to orient interventions positively.

(In)commensurability acknowledges critique, partnership, mutual learning, and the possibility for disagreement. Collaboration is a tricky praxis. It is one that requires communication,

negotiation, and sometimes discomfort. And it also puts at some risk one's own intellectual certainties. But eventually, these risks strike me as worthwhile because they allow for intimate views of international aid, more possibility to share critical knowledge, as well as more research openness. As for the Battelle itself, I remain curious about the heavy kits I carried in my suitcase. How will their results speak across the lifeworlds we set out to describe, and vice versa? Will the costs of the kits and the personnel required to deploy them justify their products? I remain curious about how my colleagues understand this intervention. And I remain curious about how and why attachment is the thing that Helping Hand wants to affect. Curiosity and a bit of intellectual risk, it seems to me, are prerequisites for good critique.

About the author

Tomas Matza is a medical anthropologist with an interest in the psychology and psychiatry of children, the politics of psychosocial interventions, and the political economy of care institutions. He is the author of *Shock Therapy: Psychology, Precarity and Well-Being in Postsocialist Russia* (Duke University Press, 2018). He is Assistant Professor of Anthropology at the University of Pittsburgh.

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