

[Rita Astuti](#)

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DEBATE

## On keeping up the tension between fieldwork and ethnography

Rita ASTUTI, *London School of Economics and Political Science*

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The transition from participant observation to ethnography is full of tensions and challenges. The author argues that anthropologists should strive to keep up the tension and respond to the challenge and she suggests two ways in which this can be done: return visits and the use of experimental techniques.

Keywords: fieldwork, ethnography, participant observation, experimental methods, Madagascar

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Anthropological knowledge is produced by what we do and what we write: we do fieldwork and we write ethnography. The relationship between the two is full of tension and in my contribution tonight I want to argue that it is our responsibility—as producers of anthropological knowledge—to work hard at keeping up the tension. I will explore two ways of doing so, which have worked for me personally: return visits and the use of experimental techniques.

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We all know, but it is worth restating, that anthropology is built on its unique way of finding things out. We turn up in a place and we ask its people to allow us into their lives. Remarkably, many people around the world are willing to do just that: they let us eat, work, travel, sleep alongside them; they let us witness babies being born, the sick being healed, the dead being sent off; they invite us to their political meetings, legal proceedings, ritual events; and they answer our many questions, while teaching us which ones are the really interesting ones to ask.



This kind of apprenticeship is all consuming and engages all our senses and body parts as we observe the smallest of detail—people’s glances and body postures; their nodding and twitching and relaxing—while ourselves re-learn how to walk, sit, eat, see, and hear, and so on. As with any other kind of successful apprenticeship, gradually what we observe becomes familiar and what we learn becomes second nature and thus somewhat unremarkable. And yet, even as we participate in the flow of local life, we use various techniques—most prominently, the writing of field notes—that create a small distance between what we do and learn as participants and what we do and learn as budding analysts. It is in this space that we start to elaborate an overarching interpretative narrative for our ethnographies.

This process comes fully into its own once we return to our academic institutions. No longer participants, we work at making sense of what we now call “ethnographic data”; we streamline its complexity for the sake of managing its analysis; and we start producing thesis outlines, draft chapters, full drafts, introductions, and conclusions that spell out our theoretical contribution. . .

Moving from fieldwork to ethnography is hard. It is hard because it requires letting go of the participatory experience and, in a way, turning against it. All of us, on writing our first ethnographic monograph, have probably felt that we were doing violence to that experience just by putting it down on paper—by taming it, reducing it, simplifying it for analytical and theoretical purposes.

I mentioned that the movement in this direction starts when we are still engaged in fieldwork, but there is a big difference between the way we produce ethnography then—as we analyze our daily experiences in our field notes—and the way we produce it later (sometimes much, much later).

The difference is that, when we are still surrounded by people and immersed in the sounds, the smells, the textures of the place, we cannot ignore the pushback against our analytical simplifications or straightforward misunderstandings—by the morning, what we have written down in the evening is already in need of refinement and qualification. By contrast, once we are back home, this productive (and collaborative) back and forth fades away and our ethnographic analyses can ever so easily lose their anchoring in what they are supposedly about.

Don’t get me wrong: I’m not saying that we should not be selective in our analysis or be bold and imaginative in our theoretical arguments. What I’m saying is that we must not lose touch with the people and the lives that we analyze, and theorize, and write ethnographies about. If we do, the people can easily become cartoonish and their lives can start to appear much more exotic than they actually are.

The paradoxical truth is that the further away—in space and time—we are from the people and their lives, the easier it becomes to write ethnographies about them. But my point is that ethnographies *should* be hard to write and that it is our responsibility to nurture that difficulty and keep it alive.

How? The most obvious way—I would suggest—is by making regular return visits, which rekindle that back and forth engagement with the people and the place and stop us from turning people and place into a figment of our ethnographic imagination.

Again, let me clarify what I am not saying: I’m not suggesting that by going back multiple times one will reach a full understanding and write a perfect ethnography. This would be daft, since completeness and perfection are not achievable when one

is in the business of describing human life. We all know that our descriptions and analyses are always just a partial approximation.

What I am suggesting is that we have to ensure that, as time goes by and—through a combination of wisdom and seniority—our ethnographic analyses tend to become more ambitious, we continue to be challenged by the repeated encounter with the people we write about. To put the point in slightly stronger terms, we owe it to them that we continue to create the conditions in which they can be coproducers of anthropological knowledge.

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What I have said so far should not be too controversial. The second point I want to make might prove slightly more contentious, which is not a bad thing for a debate.

I want to make the case that another powerful way of continuously challenging one's ethnography, so that it doesn't become unmoored, is by adopting the kind of experimental techniques typically used by developmental psychologists. Let me explain.

In labs and nurseries, developmental psychologists study the origins and development of children's conceptual knowledge, emotional life, imagination, and so on. All of this presents very specific challenges. Children—especially, of course, if they are preverbal—are famously not very good at saying what they know; however, there are strong reasons to believe that they know more than what they can say, whether because of their evolved minds or their inquisitiveness or the testimony they receive and seek from others.

To test this possibility, developmental psychologists design carefully orchestrated procedures that enable children to demonstrate their knowledge without having to put it into words: they just have to suck on their pacifiers when they see something they like, to look or to point one way or the other, or just show boredom or interest. Having observed what children do, psychologists then infer the knowledge that lies behind the children's behavior. For example, if children show surprise at an event that defies basic physical or psychological constraints, it must mean that they know of the existence of such constraints.

The point here is that, given the age-specific abilities of their interlocutors, developmental psychologists are forced to come up with very ingenious ways of asking their questions. To restate: they rely on children *using* their knowledge rather than on them making a declaration of it.

My proposition, then, is that we can usefully borrow this same approach and design tasks that require our interlocutors—old and young—to *use* their knowledge to solve novel puzzles, to guess the outcome of hypothetical scenarios, to engage in unfamiliar thought experiments. This might seem like an odd suggestion given that, during fieldwork, we depend on, and learn from and with, people who are eminently able to spell out for us what they know, what they value, what they feel, and so on.

But this is precisely the point: it is easy enough for a participant observer to learn what gets so eloquently and explicitly articulated in the course of social life or in the context of an interview. It is much harder to learn what people never—or very seldom, or only in certain contexts—feel the need or desire to put into words.

Let me give you just one quick example. In the course of everyday life in a Vezo village in Madagascar, a fieldworker will routinely witness people saying that such

and such a child resembles an individual who is not related to her. For example, they will say that the child has the features of someone the mother used to dislike when she was pregnant or spent a lot of time with; or that the child resembles some one who cared for her in the crucial and demanding period after birth. In other words, people will link the child's physiognomy to *social* rather than *biological* processes. From this and related evidence – what people answer when asked where babies get their looks from – one could easily infer that Vezo have no understanding of the mechanism of biological inheritance and even venture that they do not draw the ontological distinction between social and biological phenomena.

Against this background, I found it useful to use a task – originally designed by developmental psychologists to study North-American children's understanding of the distinction between biological inheritance and social learning (Solomon, Johnson, Zaitchik and Carey 1996) – which asked participants to predict which of a number of properties a fictional child would come to have as a grown-up. Crucially, the child was said to have been born of one set of parents and to have been adopted by another; and the questions were about a number of neutral bodily traits (e.g., pointed as opposed to roundish ears) and non-consequential beliefs (e.g., whether papaya is more healthy than pineapple or vice versa). The point of the task was to establish whether Vezo would differentiate – as North-American 7-year-olds and college students do – between traits that are inherited through the biological process of birth and traits that are inherited through the social process of learning.

The task was purposefully removed from real life – the child and his parents were nameless and devoid of any kinship or other social connotation that would have linked them to my interlocutors; the story did not evoke strong emotions or concerns about the fate of the child or the parents; the participants were mostly alone when answering the questions and were asked to express their own personal opinion, which is hardly what happens when people visit a newborn and comment on her looks. In other words, because of its artificiality, the task required individuals to *use* their knowledge and think in the abstract about an issue (who do children take their looks from?) for which they normally have ready made ways of talking which they use when they meet newborn babies or children they have not seen for a while, and so on.

The results were interesting. They showed that, in the context of this task, Vezo adults had no difficulty in reasoning or hesitation in saying that the adopted child will resemble his birth parents in bodily traits. Coherently, they justified this inference by explicit reference to birth origins, to shared blood, or to the blueprint that the child has received from those who generated her. The results thus also showed that Vezo adults clearly differentiate between the transmission of bodily traits and the transmission of beliefs, which they characterized as being a matter of listening, learning, getting used to (see Astuti, Solomon and Carey 2004 for full details).

For me, this was a productive result, which prompted the all important interpretative question: if Vezo know that children resemble their birth parents, why do they say otherwise? My answer has been that when Vezo state that babies resemble people other than their birth parents, they are purposefully erasing the traces that birth parents leave on the bodies of *their own* children. In effect, such statements are a pushback against the strong, exclusionist, potentially disruptive tie

which, Vezo fully recognize, exists between birth parents and *their own* offspring (see Astuti 2009). In other words – and these are the words of my Vezo adoptive father – the comments that people make about their babies’ looks are “customary ways of talking” which – and these are my words – contribute to the creation of a kinship system that is more far reaching and inclusive than the one predicted on biological filiation.

The task was productive another ways. It has provoked a range of new empirical questions, for example, about children and the complex developmental trajectory through which they construct an understanding of the process of birth and biological inheritance; and it has highlighted the methodological dangers of focusing too narrowly, if conveniently, on what people choose to articulate explicitly, for us or among themselves.

Let me stress this last point: as I mentioned earlier, the longer we do fieldwork in a place, the more we come to take things for granted; as a result, we become more and more focused on what people discuss explicitly and passionately. As a corrective, experimental tools readdress our ethnographic attention toward those aspects of people’s knowledge and experience that—for whatever reason—remain implicit and unstated. As strange as it might sound to those who see them as simplistic and foreign intrusions, experiments can thus add richness to fieldwork and complexity to ethnography.

Needless to say, these tasks and experimental techniques are useless—if not positively misleading—if used outside the rich context of fieldwork. I’m sure that I don’t need to tell *this* audience that it makes no sense to parachute experiments across the world and hope to gain meaningful insights. But when deployed in the context of participatory fieldwork, these techniques can be a powerful way of checking, and challenging, and moving forward the partial conclusions that we have come to: in other words, as part of that same productive and collaborative back and forth between us and the people whose lives we are so lucky to have been allowed to share.

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## Conserver la tension entre terrain et ethnographie

Résumé : La transition de l’observation participante à l’ethnographie est pleine de tension et de défis. L’auteur de cet article soutient que les anthropologues doivent faire de leur mieux pour conserver cette tension et répondre aux défis, et elle

suggère deux façons de mener à bien cette tâche: les visites après le terrain initial et le recours aux techniques expérimentales.

Rita ASTUTI is an anthropologist with interests in kinship, personhood, gender, group identity and belief, and is Professor of Anthropology at London School of Economics. She has conducted extensive periods of fieldwork in a fishing village in Madagascar. She has published an ethnographic monograph on kinship, death, and identity, *People of the sea: Identity and descent among the Vevo of Madagascar* (Cambridge University Press, 1995) and, with developmental psychologists Susan Carey and Gregg Solomon, she reanalyzed her ethnographic findings through the use of experimental techniques, in *Constraints on conceptual development* (Monographs of the Society for Research in Child Development, 2004). In her publications and in her teaching, she has advocated a closer integration between anthropology and psychology.

*Rita Astuti*  
*Department of Anthropology*  
*London School of Economics*  
*Houghton Street*  
*London WC2A 2AE*  
*UK*  
*r.astuti@lse.ac.uk*