

FORUM

What Is Analysis? Between Theory, Ethnography, and Method

INTRODUCTION

The Analysis of Analysis

Martin Holbraad

Recent years in anthropology have seen a noticeable trend, moving from debates about theory to a concern with method. So while some generations ago we would tend to identify ourselves as anthropologists with reference to particular theoretical paradigms—for example, Marxism, (post-)structuralism, cognitivism, cultural materialism, interpretivism—these days our tendency is to align ourselves, often eclectically, with proposals conceived as methodological: entanglements, assemblages, ontologies, technologies of description, epistemic partnerships, problematizations, collaborative anthropology, the art of noticing, and so on.

In an attempt to get a handle on this shift and explore its implications, this forum section focuses on the activity of analysis—itself an ambiguous notion in the practice of anthropology. Analysis, it seems, shifts unstably between theory, method, and ethnography. One way to think of it is as the set of activities that take place in the ‘middle ground’ between ethnographic materials and their anthropological theorization, that is, as the interface of the empirical and the conceptual. But if method is increasingly occupying the slot of theory in anthropologists’ preoccupations, then has analysis and its cognates become increasingly indistinguishable from theory? Is one’s analytical approach the same as one’s theoretical approach? What difference does it make, in any event, to think of such diverse activities as anthropological description, evocation, explanation, interpretation, and conceptualization as ‘analytical operations’? In fact, can a distinction between theory, method, and analysis be stabilized at all? Or is the ambiguous movement between them a characteristic part of anthropological practice—an element of its particular form of creativity, even?



Raising such questions also puts the spotlight on the ‘language’ of analysis, that is to say, on a range of terms that have come to operate within the procedures anthropologists might imagine as analytical. This includes terms such as ‘the relation’ (or ‘relational’), ‘scale’, and ‘proportion’, and such distinctions as quantity and quality, particular and general, local and global, continuous and discontinuous, connection and disconnection, collective and individual, and so on. Neither theoretical as such, nor just descriptive, such terms and distinctions seem to format many of the procedures we think of as analytical in the practice of anthropology. So what might their epistemic status be? How does their apparently formal character color the way in which ethnographic materials are handled, and how do they serve to transfigure the contingencies that anthropologists encounter during fieldwork into objects of contemplation? And how do such analytical tropes and procedures operate in anthropologists’ attempts not only to theorize but also to narrate their ethnographic experience? What kinds of stories does analysis tell?

Based on a round-table discussion held on 2 December 2017 at the annual meeting of the American Anthropological Association in Washington, DC, this collection of short think pieces brings together a deliberately diverse selection of anthropologists to explore the creative potentials of anthropological analysis. Contributors were invited to reflect on the role that analysis plays in their own work, comment on its development within the discipline at large, or indeed treat analysis and its diverse operations as ethnographic objects in their own right. Helping in this way to problematize the operative concept of the journal’s title, this venture in the analysis of analysis seeks to add new dimensions to the intellectual profile of *Social Analysis*, marking also my own conception of its outlook as the journal’s new editor.

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ANALYSES ACROSS CONTINENTS

Sarah Green

The motives anthropologists have for trying to understand their ethnographic material can be broadly separated into two types. The first involves pursuing some notion of research that is understood to be either scientific or poetic. In either case, the outcome (the analysis, one could say) should provide a valid account of other people's lives, actions, and thoughts. The second motivation entails some notion of justice that is used to pass judgment on what anthropologists observe or understand to be happening and provides a commentary that evaluates the conditions in which other people's lives and thoughts exist.

There is no necessary contradiction between these two types of interpretive work, and many analyses appear to combine both, with a greater emphasis on the one or the other. The now famous distinction made by Trouillot between the 'savage slot' and the 'suffering slot' hints at the difference between them. The first type is apparently motivated by trying to describe and make sense of other people and their activities and creations due to some notion that it is good to have knowledge, as such. The second type seems to be motivated by having an opinion about these same things and is often in the mode of revelation or critical commentary. The first approach is regularly spoken of as 'analysis', and it has led to endless arguments about its validity and inherent partiality. The second is more often qualified by the adjective 'critical' (analysis) to mark the judgment implied—usually, the intention to speak some kind of truth to power in the name of some wider principle of justice, or on behalf of the people being studied. The debates in that mode have concerned the source of the values used to make the critical judgments and questions about the right to speak for others.

Some might say that there is a third motivation for ethnographic analysis, one in which the object of attention is not the people being studied but instead a kind of mirror dance, an analysis of oneself, in every meaning of belonging implied by that term. A few years ago, this was usually referred to as the reflexive turn, although it had other names in previous decades. In practice, I think that has been part of the second motivation for analysis: it constitutes one of

the many versions of critique, of an attempt to pass judgment. In the 1990s in particular, that debate became somewhat obsessed with highlighting the fact that anthropologists are not innocent bystanders: there are personal and structural embedded biases in what anthropologists have to say about anything, including themselves. This, of course, led to endless questions about the value of ethnographic analysis of any kind.

Yet more importantly, to my mind, it did two other things as well. First, it led to a relatively short-lived but discernible shift of focus away from the relation and toward the subject—from kinship to identity, to put it crudely. Focusing on the self is perhaps inevitably going to lead away from the many and toward the one as the object of attention, even if relations remain crucial to the discussion. That shift has been debated in a variety of ways, but often critiques of the move from relation to subject have been implicit rather than explicit, for example, in arguments that begin with questions about sexual and ethnic identities rather than kinship.

Second, which I think was a good thing in the end, the moment of the reflexive turn highlighted the contingency of knowledge. It was not simply the idea that knowledge changes across space and time, which perhaps anthropologists always knew in one way or another, but also the idea that the relationship between places and times affects knowledge—that is, it affects what can be known. The various debates surrounding what colonialism did—not only in terms of the mess that was made of colonized people's places, ideas, status, capacities, and livelihoods, but also how it messed with people's heads, not only the heads of the colonized, but also the heads of the colonizers—was a key focus in that discussion. The lengthy and highly varied arguments about gender and sexuality, as well as persons, made an important contribution as well. These discussions inevitably drew attention to the relationship between places and times and the entanglements between them, and not only the fact that colonizers colonized, in all senses of the term.

This leads me to a consideration of one of the more recent motivations for trying to understand ethnographic material: the attempt to untangle the relations and separations between the places where anthropologists do their ethnographic fieldwork and the places to which they retreat in order to write. These two places are usually separated, in imagination if not in terms of different patches of the earth. That separation remains crucial to the analysis that anthropologists carry out, but the relations between these places are also crucial. Analyzing across these continents is what anthropologists have always done, but perhaps what is becoming more explicit is the constant journey between them and the attempt to keep them simultaneously in view. The interesting thing is that this raises the possibility of there being more than two locations to consider. This suggests a shift back to the many, rather than the one or the pair, and to the relation, rather than the subject—but in a different way.

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ETHNOGRAPHY HALF-SEEN

Alberto Corsín Jiménez

I would like to offer three commentaries on the question of analysis: first, regarding how analysis becomes ‘visible’; second, regarding its aesthetic tensions vis-à-vis ethnography; and, third, regarding its own taking form and ongoing redeployment as a material presence with others. Let me start with a myth of the origins for naturalist observation. In proper modernist fashion, I go back to the seventeenth century.

Double Vision

Robert Hooke was an assistant to Robert Boyle, for whom he built the vacuum pumps that Boyle famously used in his experimental demonstrations of the gas laws. Hooke went on to achieve fame of his own with the publication in 1665 of *Micrographia*, where he painstakingly described the use of a microscope to observe miniature aspects of the natural world.

To establish a descriptive relation with nature, Hooke (1665) deployed a complex form of ‘double vision’, such that the things he saw inside the scale of the microscope (as he zoomed in on the empirical) were kept on a par with the observations that he made outside the microscope (as he zoomed out to the analytical). Holding the inside and the outside simultaneously in view accomplishes a fertile if somewhat fragile form of knowledge as ‘symmetry’, an image that would become a trademark of the experimental method over the centuries.

The notion of double vision was anathema to Thomas Hobbes, a long-time political adversary of experimentalists such as Hooke and Boyle. Hobbes thought the idea of ‘seeing double’ invited dangerous conceptions of ontological plurality, as, for example, when clerics spoke of the division of reality into spiritual and temporal affairs (Shapin and Schaffer 1985: 98). The world, he insisted, is one, and there is only one sovereign reality to which one should yield. For the experimentalists, however, the key question was not whether they

were mistaken in seeing the world double. If anything, their challenge was in designing instruments and experimental set-ups that multiply and enhance our capacities for double vision.

Dazzling Symmetries

The concept of double vision is not extraneous to social theory. In the *Manifesto for Cyborgs*, for example, Donna Haraway (1990: 196) speaks of “double vision” to describe our “joint kinship with animals and machines” as a form of “political struggle [that invites us to] see from both perspectives at once.” Double vision has also been used to characterize the purchase of ethnographic knowledge. In *The Network Inside Out*, Annelise Riles (2001: 22–69) speaks of networks as figures “seen twice,” that is, figures that are capable of performing in both analytical and empirical registers. A ‘figure seen twice’ is the heuristic that Marilyn Strathern (1999: 262) draws on to characterize the ethnographic moment itself as one where “either observation or analysis ... may seem to occupy the entire field of attention.”

The symmetry of ethnography and analysis, their kinship in the heuristic of double vision, partakes therefore of the modern legacy of experimentation. This is a method of enchantment that dazzles and reveals—and revels in—the possibilities that ethnography and analysis uncover and afford for one another as they zoom in and out of the worlds of wonder and understanding.

Yet perhaps it is worth pausing for a moment to think about this dazzling of symmetries. As Strathern (1999: 20) notes, sometimes descriptions run the risk of auto-dazzling themselves: “Knowledge involves creativity, effort, production; it loves to uncover creativity, effort, production!” If ours is an age where the “arts of noticing” (Tsing 2015: 17–25) or the arts of cultivating surprise take the status of method—if we are dazzled by the delights of the wondrously asymmetrical and unexpected—might we not risk forgetting the role that symmetry once played as an original figure of auto-dazzle?

Half-Seen

Double vision and the symmetries of dazzle have traditionally functioned, I have intimated, as techniques for producing descriptions of complexity whose capacities are at once contained within yet exceed the legacies of naturalism. Let me bring my commentary to a close with a salvo for imagining a figure of description that offers a capacity for proliferating worlds, not by duplicating or multiplying them, but by half-seeing them. The figure I have in mind is the draft.

On the way to becoming a description, a description is always and everywhere a ‘draft’ of itself, in the sense that it is tentative but also that it ‘drafts’

and enlists the capacities of the people and the things around it. We may think of drafts in this sense as exercises in concurrency: how analysis and description partly anticipate, partly intuit, partly inscribe one another—how they half-see one another—in the material concurrency and enviroing of the social. Description and analysis do not so much double or supplement each other's fields of action here as counterpoint and outline their ongoing fugue. Drafts are situated overtures whose virtue is not that they multiply our vision but that they permanently divide it through and across the accompaniment of others, becoming forms, then, for an ethnography half-seen.

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References

- Haraway, Donna. 1990. "A Manifesto for Cyborgs: Science, Technology, and Socialist Feminism in the 1980s." In *Feminism/Postmodernism*, edited by Linda J. Nicholson, 190–233. New York: Routledge.
- Hooke, Robert. 1665. *Micrographia*. London: Jo. Martyn and Ja. Allestry, Printers to the Royal Society.
- Riles, Annelise. 2001. *The Network Inside Out*. Ann Arbor: University of Michigan Press.
- Shapin, Steven, and Simon Schaffer. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton, NJ: Princeton University Press.
- Strathern, Marilyn. 1999. *Property, Substance, and Effect: Anthropological Essays on Persons and Things*. London: Athlone Press.
- Tsing, Anna Lowenhaupt. 2015. *The Mushroom at the End of the World: On the Possibility of Life in Capitalist Ruins*. Princeton, NJ: Princeton University Press.

ANALYSIS

Between the Empirical and the Conceptual

Veena Das

Martin Holbraad asks if analysis might be seen as lying on the interface between the empirical and the conceptual. The terms ‘empirical’ and ‘conceptual’ as they function in anthropology are, however, not transparent. For starters, the empirical is not equivalent to the ‘given’ as this operates in some strands of philosophical thinking, nor is there an agreement on how concepts are generated. I propose to briefly interrogate the air of obviousness around these concepts, although what I present here is subject to considerable modification in light of the diversity in practices of actual fieldwork and in the act of writing.

The process of producing an anthropological text has been described variously as generating a ‘bigger story’ based on similarities between one’s own ethnography and that of others through a method of heuristic comparison; interpreting the categories of one system with the categories of another through analogy or polarity; and constructing concepts as ideal types against which particular social or cultural forms might be measured. Underlying all of these procedures of producing a text that is meant to circulate among colleagues or students is an aspiration toward generality that is assumed to require a measure of decontextualization. Accordingly, markers of context are thought to be akin to local sightings of a general concept that could be deployed across cultures, as well as a judgment as to what is essential and what is inessential in the details that usually accumulate in ethnographic diaries or other material media through which fieldwork experiences are recorded or archives accessed. Such a craving for generality has been famously criticized by Wittgenstein as a disease and comes under considerable attack in Foucault’s method of eventalization. Without minimizing the difficulties of thinking through the relation between the concrete and the abstract, I submit that the tendency to think of concepts as always general rather than particular, and the confusion between ‘general concepts’ and ‘generality of concepts’, muddies the waters. In particular, many anthropologists ignore the great insights of Wittgenstein and of Foucault on the production of veridical regimes—the former calling for specification of context

and the latter for the historical moment through a genealogical analysis—as if anthropological thinking was immune from applying these insights to itself (despite claims to reflexivity).

Due to the brevity of this piece, I content myself with two basic points. First, I propose that we pay more attention to singular concepts whose mode of generality is different from that of comparison between different objects or cases. In the case of singular concepts, the issue might be that of recurrence over time: how do I determine that the person I knew as Max who always treated me with kindness is the same Max who has just been convicted of domestic violence? Considering this example may take me to the region of a general concept—what is a person?—or to the region of a singular concept—who really is Max? One of the consequences of ignoring singular concepts has been the tendency to describe experience in ‘typified’ forms. For example, a mother may curse her son for neglect, especially if she is old and relies on others for support, as we learn for the Nuer from Evans-Pritchard. Yet this typification involves the unstated judgment that a rendering of experience in coarse terms (a typical mother) is adequate here, ignoring the fine grains of experience that attention to variation within the general concept of mother would have entailed.

Second, even if we concede that data culled in the field gather more efficacy once they are transformed through analysis into the anthropological text because they can now speak to a wider audience, questions remain. Under this description, data that are at the same scale as constructs fail to tell a larger story. Something else must be done to make the data speak—protocols introduced, methods standardized, patterns extracted. What remains of the relation, then, between the ethnography and the analytically processed anthropological text? Under the best of circumstances, and after we have discarded notions of imitation or mimesis, we might say that the ethnography provides exemplification, apt illustration, or instantiation of the theoretical points. We must then ask further, what is the way we absorb the gap between the concept and its example? In grammatical textbooks, for instance, the example is arbitrary but adequate if it illustrates the rule. But an alternative way of thinking of an example might be that it does not illustrate the concept as much as it gives it flesh—endows it with life. The efficacy of the example, said Wittgenstein, is that it allows us to project the concept to new instances as we move from one example to another. Yet an inner constancy in the concept will not allow it to be haphazardly extended. In fact, some concepts will tolerate greater flexibility, while others will be more rigid or might be muscled down to specific regions of the real through external interventions.

Consider why Evans-Pritchard contends that *kwoth*—the Nuer word for spirits—can be translated as God but cannot be extended to the Zande ideas about Mbori since these are too loose and vague for Mbori to count as God. The normative standards here are derived from Christian conceptions: the Nuer remind

Evans-Pritchard of Old Testament figures, while the Zande do not show the proper attitudes of reverence and awe with regard to Mbori. This should surely provoke us into thinking about the difference between philosophical reflections on the normativity inherent in a concept and anthropological considerations on how standards underlying a concept are generated in our practice so that what counts as the empirical already bears the imprint of the conceptual.

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SCALAR PARADOX AND THE ROLE OF ANALYSIS

Nurit Bird-David

We have been invited to reflect on the role of analysis in our work, especially on its place between theory and method. In the short space I have here, I propose that analysis precedes theory and method in the study of indigenous communities and that keeping its place central in anthropology is anything but marginal to this round-table discussion. Starting with a brief comment on the cross-scalar paradox at the basis of this study field, I draw on my work to sketch how analysis prefigures such communities as objects of ethnographic, comparative, and theoretical interventions.

Expanding scales of practice and imagination have driven anthropologists to explore tiny indigenous communities and then to scale up their findings, whether to understand humankind, to service colonialism, or simply to engage in debates with colleagues. Although indigenous populations include many whose order of size is estimated at less than a few thousand, they are compared with other societies irrespective of size, with their own population numbers getting a rare mention beyond a line or two. Researchers have rarely addressed indigenous scales of practice and imagination or, indeed, indigenous people themselves as scalars. The discipline's large-scale cross-cultural project eclipses its cross-scalar basis, and analysis plays its part in the process.

In the 1970s, when looking for an appropriate forager people in the Nilgiri Hills of Tamil Nadu for my PhD research, I followed local horizons of engagement and knowledge. In each hamlet I asked to be taken to another one deeper in the forest. The hamlet in which I finally settled comprised six huts. Their occupants continually visited relatives living in several smaller hamlets within a day's walk. All of these people called themselves "us, relatives," consistent with a widespread practice among indigenous peoples of designating themselves by vernacular terms translatable as "real people," "humans," "kinspeople," and so on. Their identity category could encompass different life forms, from an elephant-shaped relative to myself, the anthropologist, as a white-skinned relative.

Expandable to everyone “living in the forest with us,” this concept was highly pluralistic but not scalable beyond the foragers’ habitat.

Returning to university, I faced the problem of naming ‘my people’. In the room I shared with PhD students back from other parts of the world, photographs and maps decorated the walls facing our desks, and we asked each other, as anthropologists do, “Whom did you work with?” Neophytes enter the ranks of the profession as ethnographers of ‘the X’. The ethnonym identifies the researcher’s professional self as much as it does his or her study people; its use is absolutely necessary for writing a legible ethnography. How does an ethnographer name ‘people with no names’ who, precisely for this reason, are confusingly known by multiple names given them by outsiders, some names insulting to them? In my fieldwork I came across wonderful naming stories, as I recount in my 2017 book *Us, Relatives*, and for my Nilgiri Hills foragers I settled on Nayaka, the appellation used by their close non-forager neighbors. This name, however, not only identified them (and me); it also restricted my scope for depicting and theorizing their world.

An ethnonym is a Trojan horse that refigures the social world we study from within. Ethnonyms distinguish between human collectives and between humans and non-humans. They prefigure communities of serialized and standardized members who share sameness (even if only constructed) irrespective of whether they live together or are dispersed. The ethnonym is a highly scalable identity category that applies to one individual (a Nayaka) or multiple ones (four hundred Nayaka). Named societies can be compared, leaving their scalar disparity transparent (e.g., comparing Nayaka, Tamil, and Western cultures). Analytical terms can be eased into ethnography with an ethnonymic prefix, for example, ‘Nayaka men/women’ when one studies a few dozen people who primarily approach each other as kin of one relation or another. Using proper names is essential in prefiguring indigenous communities as objects of ethnographic and theoretical work, yet *a priori* it obscures indigenous scales of practice and imagination, their categories of belonging and plural worlds. The ‘properness’ of the name lies precisely in its legibility in such contexts, and using indigenous ‘we’ designations as if they are proper names only compounds the problem.

My tale does not end here. Kattunayaka (with *katu* meaning forest) is one of India’s ethnonymically listed disadvantaged ‘tribes’, whose members are entitled to substantial benefits under India’s distributive justice policy. Kattunayaka identity for this purpose is certified by government administration, and occasionally it is pursued in court, sometimes using anthropological reports. One entrepreneurial claimant got his people to enact for me Nayaka practices as I had described them in my articles, and then he asked me to certify (a job usually performed by government anthropologists) that his people are Nayaka—“the same,” he said, as those I had studied. Strangers whom the foragers would not regard as ‘us, relatives’ exploit the ethnonymic plural logic to claim that

they are Kattunayaka. Their claims are based on similarity of names (which the foragers do not have) and on evidence of similar ‘cultural traits’ (contra the foragers’ plural ontology). The state-certified Kattunayaka population has increased exponentially, reaching 45,000 by 2001, yet many are urban dwellers who have never been to the Nilgiris district. By 2014, their exponential increase even alerted the National Commission for Scheduled Tribes in New Delhi.

This case, which is not unusual, leads me to propose not only that analysis precedes theory and method in this field, but also that analytical language is our profession’s ‘native’ language, with some terms working as ontology shifters, creating ‘what is’ for our study subjects as well as for ourselves.

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ANALYTICS AND ANTHROPOLOGY

Eduardo Kohn

Martin Holbraad has asked us to reflect on the status of analysis in anthropology. What is it? Is it useful as a bridge between the empirical and the theoretical in our discipline, committed as it is to an immersive, field-based form of thought? I want to reflect on this, not by discussing analysis per se, but by thinking about an ‘analytics’. I pilfer this term from Foucault, quite aware that my use of it is neither faithful to Foucault nor in line with a Foucauldian approach. In *The History of Sexuality*, Foucault (1990) discusses how best to attend to power. He speaks of power as something inescapable—something that forms us in such a way that there is no outside position from which to see it. In such a situation, he tells us, it is not so much that we need a “‘theory’ of power,” but an “‘analytics’ of power” (ibid.: 82). The move away from theory and to an analytics is a move “toward a definition of the specific domain formed by relations of power, and toward a determination of the instruments that will make possible its analysis” (ibid.). There are many who may understand this passage better than I do, but I take Foucault to be saying that the correct form of attending conceptually to power is to use and make apparent the form that power takes. His is therefore not a theory, insofar as the term ‘theory’ seems to imply a style of contemplation we bring to an object of thought. Rather, it is an exercise in discerning in that object of thought a mode of attention adequate to that object of thought and then—only later, perhaps—tracing whether that kind of concept work can travel beyond the specifics of that engagement. This further elaboration, I suppose, would be a theoretical one.

Concerning anthropology, insofar as an analytics is an ethnographically formed mode of concept work, it bridges the empirical, the methodological, and the theoretical. An analytics is empirical in being a kind of concept work that is experientially derived in field encounters. It is methodological in the imperative to find ways to do ethnography that permits us to attune ourselves to discovering those analytics that are empirically given and not imposed by us. It is theoretical in that it can encourage us to critically reflect on how such an analytics might (or might not) be applied to other conceptual objects, and to ask

what it is about us as thinkers, and the places from which we think, that makes us receptive (or not) to certain kinds of analytics. Because an analytics seems to require a form of anthropological thinking that is isomorphic with that which is thought, it appears that developing an analytics always involves an epistemic privileging of the iconic (i.e., of the resonant, the mimetic, the imagistic, the aesthetic, the formal), which is buttressed by some sort of ontological claim (implicit or explicit) as to why this mode of thought should be privileged. A discussion of why we privilege the iconic would be a theoretical one.

Let me give a few examples of approaches in anthropology that seem, to my mind, to work with an analytics. I think of William Mazzarella's (2017) recent masterful book *The Mana of Mass Society*, in which attention to mana, and how it moves through social thought, allows him to create an analytics informed by mana's properties. This leads Mazzarella to propose a 'resonant' theory of culture that mimetically draws on an archive of mana's own mimetic movement. One can also think of Marilyn Strathern's (1988) attention to Melanesian concepts of gender and personhood to develop an analytics of relation that encompasses the anthropological relation itself, or Eduardo Viveiros de Castro's (1998) multi-naturalism—an analytics itself developed from an Amazonian perspectival analytics. Martin Holbraad and Morten Pedersen (2017) have recently sought to further generalize multi-naturalism as an analytics inherent to the ethnographic method itself.

An analytics is a form of thought that is not necessarily limited to anthropology or, for that matter, to humans. I am thinking now about my own work in the Ecuadorian Amazon with the beings—human and non-human—that think with thinking forests, as well as my current collaborative work with those who are struggling to bring this topic into larger ethical and political domains in these times of planetary ecological crisis. In this latter work, I have become increasingly interested in thinking with psychedelics, by which I mean a series of techniques for thinking that manifest in the minds of their thinkers in ways that dissolve those thinkers into the manifestation of the minds that think them, in what could be called a psychedelic analytics.

In following this concept, I keep coming back to Gregory Bateson's (2002) development of what he terms 'double description', which is a prime example of an empirically derived analytics. Bateson illustrates what he means by double description through binocular vision. By recognizing the similarities and systematically comparing the differences between what each eye sees, the brain, in performing this double description, comes to interpret each of these inputs as part of something more encompassing at a higher logical level. As a result, something novel emerges: the perception of depth (ibid.: 64–65). Bateson then goes on to ask: "What pattern connects the crab to the lobster and the orchid to the primrose and all the four of them to me? And me to you?" (ibid.: 8). His answer: double description is operative in the form-generating dynamics that

makes these entities, with their differentiating doubled appendages, what they are and how they are connected. But double description, he tells us, is also the mode by which we can both discern this pattern and see ourselves, with our doubled but differentiating thoughts and appendages, as the product of the same analytics.

This analytics belongs neither to Bateson nor to anthropology. Rather, it is a kind of thought that thinks us, a kind of thought that we can think with if we can use it in such a way that its psychedelic—that is, mind-manifesting—properties can become apparent to us.

Eduardo Kohn is the author of the book *How Forests Think: Toward an Anthropology Beyond the Human* (2013), which has been translated into several languages and won the 2014 Gregory Bateson Prize. His research continues to be concerned with capacitating sylvan thinking in its many forms. He teaches anthropology at McGill University. E-mail: eduardo.kohn@mcgill.ca

References

- Bateson, Gregory. 2002. *Mind and Nature: A Necessary Unity*. Cresskill, NJ: Hampton Press.
- Foucault, Michel. 1990. *History of Sexuality*. Vol. 1: *An Introduction*. New York: Vintage Books.
- Holbraad, Martin, and Morten Axel Pedersen. 2017. *The Ontological Turn: An Anthropological Exposition*. Cambridge: Cambridge University Press.
- Mazzarella, William. 2017. *The Mana of Mass Society*. Chicago: University of Chicago Press.
- Strathern, Marilyn. 1988. *The Gender of the Gift: Problems with Women and Problems with Society in Melanesia*. Berkeley: University of California Press.
- Viveiros de Castro, Eduardo. 1998. "Cosmological Deixis and Amerindian Perspectivism." *Journal of the Royal Anthropological Institute* 4 (3): 469–488.

WHAT IS AN ANALYST?

Ghassan Hage

Rather than asking, what is analysis?, I want to consider the question, what is an analyst? The latter is more helpful in allowing me to make the points I want to highlight here.

In all the human (social and psychological) sciences, the concept of analysis conjures a division between subject and object, analyst and analyzed. While it is generally accepted that everyone is capable of, and engages in, some form or another of reflexive analysis of themselves, the world, their situations, and the events confronting them, it is also largely accepted that lay people's analysis is more 'applied', driven by the practical matters they are facing. Professional analysts, on the other hand, even when aiming to solve specific problems facing them, are driven by 'analysis for analysis's sake'. Analysis in itself constitutes a source of satisfaction to them. This is hardly an either-or matter, though, and it is perhaps preferable to say that analysis for analysis's sake is a more important dimension for professional analysts than it is for others. Just as important, if not more so, the idea of a person being an analyst implies a certain professionalism and a certain set of acquired skills, as well as a certain training in the exercise of these skills. One of these skills involves the capacity to negotiate the relation between professional and folk analysis, between one's own analysis of people and people's analysis of themselves. There is clearly an inherent tension in the moment of analysis. It involves two contradictory fantasies: a fantasy of social space as an object that can be understood through an analysis of the social, historical, and structural forces that brought it about and a fantasy of the analyzed social space as made out of people generating a multiplicity of analyses of that space, with each analysis being a constitutive component of what is being analyzed. An analyst who thinks that he or she is the only analyst around can only be a poor analyst, so to speak.

The extent to which lay, folk, or self analysis is included in the analysis of the professional analyst—that is, the extent to which the division between analyst as subject and analyzed as object is polarized—has always differed between analytical traditions and among individual analysts. There is no doubt, however, that

social analysis, because of the scientific (social physics) imaginary it has borrowed from the natural sciences, can be exceptionally violent toward its object precisely by reducing 'it' to an object. Social analysis often involves imaginaries of 'capture', 'breaking down to components', and 'dissection'. If it does not itself kill its object, it sometimes imagines it dead or seriously unable to speak, let alone analyze itself. And this is not only true of conservative positivist social analysis. When it has a strong social justice dimension, social analysis might not itself kill its object, but it imagines it to have already been murdered (sometimes literally, as in the case of genocide studies), or that at least a serious crime has been committed against it. The oppressed/dominated/exploited/dispossessed are left with very little capacity for self-reflexivity and self-analysis. In such situations, social analysis develops a forensic imaginary: the object of analysis becomes akin to a crime scene where something unspeakable was done to the working class, women, the colonized, the natives, and so on. Certain forms of feminist, colonial, and Marxist analysis often have this forensic ethos. It is an ethos that is more Columbo than Hercule Poirot in that we know from the start who the murderer is. It is patriarchy, racism, or capitalism that did it, of course. The point of analysis is not to find out who, but to find out how.

Today we are moving away from the tradition that silences 'the analytics of the subaltern'. We even have a reverse situation where the mode of inclusion of folk and subaltern analysis by professional analysts has become a marker of analytical sophistication. This has acquired a particular importance in the continually reimagined relation between anthropologists and their informants. Here, another colonialism-related critical dimension has come to perturb the neat anthropologist-as-analyst/indigenous-people-as-analyzed dualism. The non-reflexive tendency to see Western cultures as the source of the categories of analysis even when the analysis itself claims to be exceptionally open to the cultures of the other is being questioned. We are increasingly faced with many sophisticated attempts at treating indigenous knowledge not simply as an interesting or a rich object of analysis but as a knowledge that has analytical powers in its own right.

This opening of the analytical space for indigenous knowledge to participate in the making of analytical subjects is clearly an important positive development. Nonetheless, there is always a risk of seeing it as a 'solution' or an overcoming of the analyst/analyzed contradiction mentioned above. The desire to 'resolve contradictions' and to find solutions to some analytical cul-de-sac remains a strong driving force in the field of professional analysis. Yet, at the same time, there is an opposite and very valuable skill that continues to distinguish the professional analyst from others: it is the ability to dwell in contradictions and cul-de-sacs without fantasizing about their resolution or their overcoming. Analytical experience here is precisely the capacity to see in these spaces of contradictions the richest of all analytical spaces.

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FOR A CRITICAL ‘SOCIAL’ ANALYSIS

Laura Bear

My argument here is for a critical ‘social’ analysis or an amplification of the disturbing qualities of anthropological knowledge. For the past six months, I have been on the managing team of a research network intended, perhaps overambitiously, to ‘rebuild’ macro-economics. This is an ESRC-funded collaboration led by the National Institute of Economic and Social Research (NISER), a think tank with which Keynes was affiliated and which now offers its predictive models to banks and policy units. The ESRC put out a call in 2016 for a research network because macro-economics had failed in the 2008 financial crisis. The rubric for this initiative suggested that the discipline needed to abandon its rational choice models, rejoin the real world by rethinking ‘policy’, and disrupt its ‘monoculture’. As a result of discussions between policy makers, politicians, macro-economic experts, and anthropologists in the network, it has become clear that there is a legitimacy crisis in our economic institutions. It is far greater than the uncertainty described in the canonical work of Holmes and Marcus (2006), in which the term ‘para-ethnography’ was coined.

In our meetings, experts repeatedly reflect on the distance between their visions of the public good and the concrete social around them. The ‘social’ is no longer simply the ‘anecdotal’ qualitative information that Marcus and Holmes describe for the Federal Reserve. In their account it was possible for anthropologists to share cultures of expertise and collaborate with reflexive technical experts. This cooperation could be constructed through an exploration of, and deferral to, informants’ knowledge practices. In fact, in the case of central banks these unexpectedly contained a common ground. Holmes and Marcus argue that they, just like anthropologists, track social relations in the economy through informalized networks. These findings are a central part of their decision making. But the social is now much more unruly and disturbing than this—it is a ‘problem’ and an ‘uncontrollable’, almost unknowable domain. Macro-economists questions are many: Why is productivity growth not increasing? How come quantitative easing has negative distributional effects? What can we do about public distrust, populism, and inequality? How will our societies

support themselves as climates and resources erode? Their questions about solutions also range from the small to the huge: Do we need new technical models based on complexity theory and new more democratic economic institutions? How could we refound our constitutional settlement from the ground up?

The current unruliness of the social creates the possibility for a new kind of engagement between anthropology and economic experts. However, this cannot simply recapitulate our standard terms of engagement. Macro-economists in the network find many of our disciplines' interventions quite limited and unsurprising. They are puzzled by the restricted vision of ANT and STS of finance and markets because they know that these institutions are not just forms of valuation. They understand them as techniques with distributional effects. They also find entirely unsurprising the argument that economics is a form of moral action rather than experimental science. This is precisely why they care so much about the discipline. Driven by a sense of responsibility, they want to get it ethically and technically 'right'. This has emerged from their involvement in crises stretching back to the Asian financial crash of 1997. They are also searching for exemplars—in particular, a new Keynes who can lead us to a new covenant. They have morals and know that what they do has rebounding consequences.

How, then, can anthropologists engage critically with this explicitly ethical culture of expertise? This is where a critical social analysis becomes highly significant. Economic experts do not have a disciplinary frame that enables them to systematically trace social connections and consequences. The social remains informal, and, as Marcus and Holmes correctly stated (but did not fully explore), it is 'anecdotal'. Macro-economists do not know how to link their informal knowledge of social networks (what they call 'corruption' and 'monocultures') to its effects. They also intuitively grasp a connection between policy—for example, quantitative easing—and inequality, but they do not know how to go beyond intuition. The anthropologist can (and I would say must) take them inside the concrete timescapes of the social and their inequalities. This makes the small space for the social in their analyses a vast, disruptive force. Once taken toward this, the boundaries for creating new forms of the public good are breached. We, as anthropologists, can move them toward a 'social calculus' (Bear 2015). We may even unravel the centrality of calculative models and the dominance of economics as a tool for fixing everything.

Here I am suggesting a more challenging task for us than that of para-ethnography. We would find commonalities with technical experts around the social and then use these openings—or the ways that the social is bracketed and/or encompassed in their framings—as a way of critically generating a new non-economistic public good. This is just one dialogue in which anthropologists can amplify their critical social analysis. Yet given the widespread dominance of market models and *techne* in all of our institutions, from universities to corporations, it has a powerful scope.

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References

- Bear, Laura. 2015. *Navigating Austerity: Currents of Debt along a South Asian River*. Stanford, CA: Stanford University Press.
- Holmes, Douglas R., and George E. Marcus. 2006. "Fast Capitalism: Para-Ethnography and the Rise of the Symbolic Analyst." In *Frontiers of Capital: Ethnographic Reflections on the New Economy*, ed. Melissa S. Fisher and Greg Downey, 33–57. Durham, NC: Duke University Press.

THE POSSIBILITIES AND LIMITS OF ANALYSIS, OR, WHY DOES IT MATTER TO ASK THE QUESTION?

Hannah Knox

Here I skirt the tricky question, what is analysis?, to consider why it might be important to even ask the question. In doing so, I attempt to move the conversation from an analytical mode to a political one. Instead of describing what social analysis is, I take up an external vantage point from which this question can be addressed empirically and pragmatically, rather than philosophically.

The central question I am concerned with is not what analysis is, but what it makes possible. This also takes us to a related question: what does analysis *not* make possible? This is not just an academic question but one that confronts anyone who is trying to stabilize truth claims about the world through analytical methods and who finds those claims being challenged. It seems particularly pertinent to ask these questions at a time when expertise is being undermined, the line between truth and falsity is being blurred, and moral and ethical positions are being pitched against rationality and analysis as criteria that should underpin action. To explore these tensions, I want to think with a group of people to whom I have recently been talking: the climate science community. While at first glance they may not appear to be social analysts, their role as specialists in the description of human/natural relations has in many ways repositioned them as social analysts. However, this raises challenging questions about what climatological analysis should or should not be expected to do. Climate scientists are thus a particularly interesting group of people for thinking with about what analysis does and does not make possible.

The core work of climate scientists is clearly analytical. It entails the collation of traces of atmospheric conditions through monitoring and measurement devices; the organization and structuring of data; the search for patterns in the data through the use of statistical tools; the deployment of this data to construct hypothetical models of global climate futures; and then the validation and testing of those models against new forms of data. By building patterns and testing

models, climate scientists produce provisional and probabilistic facts about the way the world is and the way it might be in the future (Edwards 2010; Oreskes et al. 1994). Analysis in climate science makes possible a vision of a whole earth system undergoing a process of unprecedented transformation. It also produces the capacity to pose questions about the causes and implications of the transformations observed.

But the analytic work of climate scientists produces much more than this. The effects of the analysis of climate systems both intentionally and unintentionally overflow the laboratories and the computers within which they are produced and the journals through which they are communicated. The data diffuse into policy statements, political arguments, and media battles. Analysis itself also leaks beyond the infrastructures of scientific knowledge production, becoming deployed as that which legitimizes descriptions and counter-descriptions. It is analysis that enables the ‘royal science’ of university research institutes to be pitted against the ‘minor science’ of climate skeptics. Indeed, even the term ‘skeptic’ has been wrested from those who speak for the evidence of human-induced climate change and appropriated by those who speak against the existing scientific consensus.

So what about that which analysis does *not* make possible? There is much discussion among climate scientists regarding what to do about a perceived failure to communicate their analysis effectively. Analysis in this framing stops before public communication starts, reinforcing a divide between the act of knowing and the act of instructing. This creates profound concerns for many climate scientists about the implications of being asked to take on the role of public science communicator. Should they really be expected to become responsible for what happens to their findings after they leave the laboratory? Would doing so not ‘dilute’ the science by mixing analysis and politics? Climate change has become for many a proxy for liberal beliefs most clearly embodied in the figure of Al Gore—the perfect instantiation of neutral science gone ideological. Returning to the strictures of analysis protects climate scientists from the charge of political bias. Analysis here works by excluding politics, even as it creates the material out of which new political configurations are constructed.

For some climate scientists, however, an awareness of the inherent politics of their findings has led to a reconsideration of the remit of scientific expertise and an expansion of the fields upon which analysis should be conducted. Some climate scientists have embraced the challenge of being better communicators and vociferous activists. This has required that analysis itself be extended to include not only the analysis of atmospheric systems but also the analysis of media ecologies, public opinion, and the psychology of human reactions to threat, danger, and opportunity.

Finally, when climate scientists become social scientists, then what challenges does this pose to the anthropological versions of social analysis described

in this journal? Climate science and the human/natural relations it describes challenge anthropologists to participate in a conversation as both specialists in social analysis and as political actors, replaying some of the tensions I have described. Moreover, when anthropology is asked to respond to a question posed not by humanism or philosophy but by the natural sciences, then what kind of response to this question do anthropological approaches to analysis make possible, and what might the difficulty of a response tell us about the anthropological practice of analysis and its possibilities and limits?

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References

- Edwards, Paul N. 2010. *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*. Cambridge, MA: MIT Press.
- Oreskes, Naomi, Kristin Shrader-Frechette, and Kenneth Belitz. 1994. "Verification, Validation, and Confirmation of Numerical Models in the Earth." *Science* 263 (5147): 641–646.

FROM THE OUTSIDE IN

Anthropology as a Dialectic of Unsettlement

Bruce Kapferer

The old disciplinary boundaries are being crossed, perhaps dissolved, and there is a new era of creative methodological enthusiasm in social anthropology sparked by the concepts, ideas, and discoveries of other disciplines—especially the biological and physical sciences. Hybridization of perspective and the value of cross-pollination of concept and theory are everywhere evident. It is encouraged and reflected in the all-incorporating, totalizing bureaucratic order of the American Anthropological Association—effectively the imperial hegemon of global anthropology—and in the programming of its annual events. Undoubted positive virtues aside, the methodological significance of certain features of social anthropology's own imaginative intervention, virtually from its start, has become (inclusive that born of self-criticism) lost or smothered in these heady days of concept bidding and theoretical fashion.

We have been asked to address the question of analysis in anthropology. This must always be contingent on situation, issue, and problematic. Rather, I will address what might be called the anthropological attitude, which yields to analysis a particular distinction that might be called anthropological.

Anthropology began during Western imperial expansion, the shame not being in this fact as such but in the failure to fully grasp the nature of anthropology's early conceptual complicity with dominant power. This continues to be the case regardless from where the concepts or orientations are borrowed—such as quantum or theoretical physics. Although Edmund Husserl (1970) was not addressing anthropology but atomistic objectivist psychology given to the rule of science and technology, his book *The Crisis of European Sciences and Transcendental Phenomenology*, written at the time of Nazi Germany's ascendance, has resonance in the current historical moment. Husserl saw a connection between the fascist turn of his time and the worship of science and technology that removed the human dynamics of knowing and its implications from the equation. He stressed the intersubjective dynamics of knowledge and the ontological unity of unavoidable human-centeredness of all knowledge.

The failure to recognize this is a recipe both for an un-science of knowledge and for the disaster of humanity.

In *Democracy Incorporated*, the political philosopher Sheldon Wolin (2008) takes a different line of approach, but with similar implications to that of Husserl, describing a new fascism in thought and practice that he terms ‘inverted totalitarianism’—a kind of atomistic economism of thought that subverts knowledge practices, becoming the ground of their production and presentation and thereby rendering them complicit in the destructions that may be coming. For Wolin, knowledge practices have been increasingly infiltrated by corporate interest, which he sees as the driver of inverted totalitarianism and a motivating force in the generation of a culture of celebrity and consultancy that structures knowledge in the interests of capital, even in its critiques. The enthusiasm for new theoretical fashions and conceptual innovation is not innocent of corporate forces, which are increasingly defining the ground for thought and its recognition. Anthropology is far from being outside such a process.

Short presentations like this one encourage unnecessary brutality of comment that should, in a more relaxed situation, be avoided. But some current anthropological conceptual and theoretical trends such as the anti-constructivism of speculative realism and posthumanism, the flourishes of Latourian assemblage theory and networked systems (that draw much of their legitimacy from science—cybernetics, military-oriented biology, might be noted—and a refurbished objectivism) embed a politics (or else bear an uncanny relation) which I think is connected to a new economic-political formation (rather than political-economic one) emergent from out of the frame of the nation-state that might be labeled the corporate state. This emergence constitutes a reconfiguring of the nature of the social world that we live in as it drives new objectivities of knowledge that are implicated in the very configurations within which they are formed.

Despite its complicities, or perhaps because of them, anthropology at its beginning sought to go outside the everyday (usually imperial) worlds of its practitioners. Anthropologists journeyed to the far horizons, often to confirm the knowledge of the center. But the potential was there to challenge accepted knowledge (scientific or otherwise) by virtue of going outside, becoming external to, the domains of knowledge from which anthropologists came as they were to those to which they traveled. This stress in anthropology of becoming external is a distinction of the discipline whereby anthropologists are enabled to enter within the dynamics of coming to know. Being in the situation of the neophyte, who must learn from the position of no knowledge or a dynamic of ‘unknowing’ (see Mimica 2010), is the vital feature of anthropological work (when anthropologists no longer travel to the far horizons), making it potentially the most critical (and most restless, homeless, and rootless) of the disciplines focusing on the nature of what it is to be human.

In closing, anthropology is given methodologically to challenge all understandings of what human being is or is becoming. Reality is always a construction, and it is the dynamics and implications of constructive processes that anthropology is, in all its analyses, committed to in its work. The going outside and the coming from outside of anthropology (a kind of double externalization) is a vital dimension of anthropology that was intuitively grasped by the early pioneers of the subject. A. L. Kroeber and E. E. Evans-Pritchard were both antagonistic to socio-cultural anthropology's categorization as a social science and what they saw as an overdetermination in theories and ungrounded conceptualization driven, as it was then, in scientific positivism. Anthropology in much of its finest work did not start from theory but moved toward it (see Kapferer 2007). Kroeber and Evans-Pritchard understood the crucial distinction of anthropology born of its externalization of knowledge and the significance of its advocacy of fieldwork immersion in realities that always begins from the position of the neophyte, of one who does not yet know, who must radically unlearn in order to learn and understand. This path is potentially of the most radical kind, an orientation of continual unsettlement that does not begin in theory or the abstract concept but, before Foucault, engaged in an archaeology of their formation and their implications for the understanding of human action. Such is anthropology as a science, a program of work that is enduringly open and disruptive of fashion despite being prone to it and, as a result, possibly losing its vitality. Max Weber wrote of sociology as a discipline that is forever young, always in a situation of openness and wonder. This applies to anthropology most of all and guides its unerringly empirically grounded analyses as always voyages of discovery.

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References

- Husserl, Edmund. 1970. *The Crisis of European Sciences and Transcendental Phenomenology*. Trans. David Carr. Evanston, IL: Northwestern University Press.
- Kapferer, Bruce. 2007. "Anthropology and the Dialectic of Enlightenment: A Discourse on the Definition and Ideals of a Threatened Discipline." *Australian Journal of Anthropology* 18 (1): 72–94.
- Mimica, Jadran. 2010. "Un/knowing and the Practice of Ethnography: A Reflection on Some Western Cosmo-ontological Notions and Their Anthropological Application." *Anthropological Theory* 10 (3): 203–228.
- Wolin, Sheldon S. 2008. *Democracy Incorporated: Managed Democracy and the Specter of Inverted Totalitarianism*. Princeton, NJ: Princeton University Press.