3

on fieldwork

(itself)

ONE

Most anthropologists take it for granted—and it indeed sounds obvious—that fieldwork and ethnography are really synonyms: to be an anthropologist is to conduct fieldwork, and to conduct fieldwork is to conduct ethnography.

I am interested in the historicity of this taken-for-grantedness.

When did it become obvious that doing fieldwork meant to do ethnography? And why? What event—or what series of events—led to the self-evident equation of fieldwork with ethnography? Can one write the history of this self-evidence?

If I raise the question concerning the relation between fieldwork and ethnography, it is not least because I am curious about what one could call the possibility of "fieldwork after ethnos" (and after "the human"). I wonder what happens to fieldwork when anthropologists no longer study ethnos or conduct ethnographies? What is the role of fieldwork—if any—when cut loose (liberated) from the study or society and/or culture?[1]

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Differently put, I am interested in the fragility of the equation of fieldwork with ethnography. And my aim is to exploit this fragility.

TWO

Let's begin with conceptual history. Where do the terms "ethnography" and "fieldwork" come from? When do they first appear? Who used them? To what ends?

The term "ethnography" appeared in the late eighteenth century to introduce a new subclass of geographical research. Probably the first to use the term was Gerhard Philipp Heinrich Normann (1753–1837), a German *Staatswissenschaftler* and statistician. Normann suggested that geography came in one of three forms: mathematical geography, physical geography, or political geography. Ethnography he defined as a subclass of political geography.[2]

I thus note that the term "ethnography" was originally introduced to refer to a practice—a curiosity—that belongs to the broad field of natural history.

The term "fieldwork" is of much more recent datum. With respect to anthropology it was first used (in a systematic sense at least) in the early twentieth century, in the writings of British social anthropologists—I notably found the term in the writings of Bronisław Malinowski (1884–1942) and his students—who sought to methodologically ground ethnography.

"Fieldwork" emerged as a qualifier of "ethnography."

What event, though, or what trajectory of events, led Malinowski and his students to argue, with vehemence, that the only proper form of ethnography was fieldwork? And what did they mean by "ethnography," what by "fieldwork"?

My research led me to think that the condition of the possibility for the encounter between ethnography and fieldwork was prepared gradually, in complicated, haphazard, and unintentional ways, in the period between the late 1890s and the early 1920s.

There was nothing straightforwardly obvious about this encounter. The concept of fieldwork-based ethnography wasn't a future anticipated in the past—a vanishing point that teleologically organized the history of anthropological research and that eventually resulted in a breakthrough to the truth. On the contrary, it was an unanticipated—a sweeping—event, one for which many contemporaries were unprepared, one that radically reconfigured what anthropology in theory and practice was about.

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Up until the late nineteenth century, anthropological research came in roughly one of two forms—expeditions to (from a European perspective) faraway lands or the largely home-based reconstruction of various features of the early history of what was then called “mankind.”[3]

Expeditions. In its modern sense as “journey undertaken by a group of people with a scientific or explorative purpose,” the term “expedition” emerged only at the turn from the sixteenth to the seventeenth century.[4] Initially, it referred to voyages undertaken with an interest in discovering unknown lands for economic purposes (think, for example, of the travels that led to the British East India Company).[5] Beginning in the middle of the eighteenth century, the term was increasingly used for scientific explorations, specifically for the cartographical mapping of coastlines and geographical surveys of the flora and fauna of unknown regions of the earth. The exemplary reference here is to Captain Cook’s voyages in the Pacific Ocean (1768–71, 1772–75, and 1776–79).[6] At the turn from the eighteenth to the nineteenth century, European expeditions then shifted focus and targeted primarily South America (think Alexander von Humboldt) and inner Africa (think David Livingston). And in the late nineteenth century, finally, the North and South Pole were explored (recall that Franz Boas, in 1883, went to Baffin Island as a geographer).[7]

What form did anthropological knowledge production take during these expeditions?

Usually the expeditions included young scholars and artists trained as botanists or physicians—figures such as Johann Foster (1729–1798) and Georg Forster (1754–1794), members of Captain Cook’s second voyage, and heroes of von Humboldt and Boas—who became curious about the “savages” and began documenting aspects of their lifestyle. The form this documentation took was the one they, as zoologists and botanists, had available: they sketched (and occasionally also measured) the physiognomies of the people they visited, just as they sketched the plants and birds they encountered; they draw up maps of the architecture and of the villages; they collected, mostly in an unsystematic, random fashion, material goods and myths and songs; and, occasionally, they also captured people and sent them home for further examination and exhibition.[8]

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Reconstruction. Next to the expeditions, there were the armchair reconstructions of the early history of mankind. It is interesting to compare the authors of these reconstructions—for example, Robert Latham (1812–1888), John McLennan (1827–1881), Johann Jakob Bachofen (1815–1887), Friedrich Müller (1834–1898), and Henry Maine (1822–1888)—with the collectors who joined expeditions.

If the latter were overwhelmingly naturalists, trained in botany or zoology (or science more generally), then the former were overwhelmingly philologists, experts in the interpretation of ancient texts and artifacts (or philologically inclined jurists, who studied antiquity to understand the early legal organization of society).

It is not that the philologists didn't travel. However, their professional travels did not lead them to small islands in the Pacific Ocean. As experts on antiquity—in the nineteenth century almost synonymous with "expert on the early history of mankind"—they voyaged to Greece or Rome or Egypt or Persia. And in their efforts to reconstruct various features of the ancient world, from myths to marriage rules (from property rights to political systems), they got interested in the savages as a group of people that would allow them to understand the prehistory that led from a somewhat animal-like existence to antiquity—and thus began to turn to the collections of texts and artifacts provided by the naturalist travelers. Several among the philologists also developed an elaborate correspondence with missionaries and salesmen spread out across the British Empire (one could easily write a media-history of early British anthropology: showing how anthropology was contingent on a tightly knit network of correspondence that unfolded alongside the extraordinary infrastructural expansion of British Royal Mail over the course of the eighteenth and nineteenth century).

Although brief, my sketch of the emergence and development of forms of anthropological knowledge productions allows me to draw two conclusions.

First, beginning in the late eighteenth century, with the success of Captain Cook's voyages, one can see the gradual emergence and consolidation of what was an anthropological curiosity in its own right (as the emergence and quick spread of the term "ethnography" shows).

Second, even though anthropology emerged as a curiosity in its own right, at the end of the nineteenth century, it had still not emerged as an autonomous, distinctive genre of knowledge production; it had not yet differentiated

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itself, either methodologically or conceptually, from natural history on the one hand (zoology, botany, geography), and universal history on the other.

Thus the form anthropology took by the 1890s was ethnography. And the term "ethnography" referred to either the collection of artifacts in the tradition of naturalism or the interpretation of artifacts in the tradition of philology. While the philologists partly drew on the work of the naturalists, the two forms of expertise were largely set apart from one another.

The event that would prepare the possibility for this nineteenth-century conception of anthropology—of ethnography—to break open was yet another expedition: the 1898–99 Cambridge expedition to the Torres Strait.[9]

FOUR

The Cambridge Anthropological Expedition to the Torres Strait was designed and directed by Alfred C. Haddon (1855–1940), a zoologist who specialized in marine life forms. In the late 1880s, Haddon, at that time professor of zoology in Dublin, had joined an expedition that aimed to explore the flora and fauna of the area between New Guinea and Australia referred to as the Torres Strait. His specific task was the study of life around coastal coral reefs. It was while standing knee-deep in the ocean collecting algae that Haddon became curious about anthropology (Haddon et al. 1935, xi):

> I was in close contact with the islanders, especially when dredging and collecting algae. Naturally, when opportunity offered, I spoke with them about their past and soon found that the young men knew extremely little about it and always referred me to the old men. I had previously found that none of the Europeans in the island knew or cared anything about the customs of the natives. . . . I therefore considered it my duty to collect as much as was possible . . . , so I induced the old men to come in the evenings and talk about old times and tell me their folk-tales.

After his return to the United Kingdom, Haddon began writing anthropology papers—and sought to convince colleagues of the necessity of a carefully planned scientific expedition that would systematically study the life and customs of the islanders of the Torres Strait. Haddon was plagued by a sense of urgency: he had no doubt that civilization would destroy the ancient societies forever, and his plan was to collect their folk tales, take pictures of their rituals, record their songs, film their ceremonies, and systematically collect material artifacts.[10]

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In 1898, Haddon finally found a donor, and the expedition took off. Aside from Haddon, the crew was composed of Sydney H. Ray (1858–1939, a philologist) and Charles Seligman (1873–1940, a physician who studied native medicine), as well as William Halse Rivers Rivers (1864–1922), William McDougall (1871–1938), and Charles S. Meyers (1873–1946) (three physicians and psychologists who were charged to study "the mental characteristics of primitive people"; Meyers also documented native instruments and music), and Anthony Wilkin (1871–1901, an archeologist).

On the one hand, the voyage to the Torres Strait was little more than yet another nineteenth-century expedition. Haddon had designed the journey as if it were a botanical or zoological trip: the aim was to survey and to collect—to collect specimens that one could arrange and rearrange back home, thereby carefully reconstructing the early evolution of mankind.

On the other hand, the expedition was a most powerful departure from its predecessors: while almost every other scientific expedition of the nineteenth century was focused on a broad set of naturalistic curiosities, from cartography to astronomy, from botany to ethnography, Haddon had organized his voyage exclusively around anthropological questions: the salvaging of the disappearing culture of the primitives. A hardly ever noticed but retrospectively far-reaching consequence of this exclusive focus was the transfer of a vocabulary initially developed for studies in natural history to anthropology. Indeed, it was only with the Cambridge Anthropological Expedition to the Torres Strait that, however implicitly, the assumption emerged that anthropology was a "field science"—a field science in its own right, independent from natural history.[11]

FIVE

Among the younger members of Haddon's team, the experience of "the field," coupled with the exclusive focus on anthropological questions, led to an awareness of how far off were the speculations of the philologists (and jurists) about primitives. Charles Seligman and Rivers in particular stressed upon their return to England that any future anthropology had to be understood as a field science, that is, it had to be grounded in firsthand empirical research experience.

Differently put, they sought to ground the reconstructive efforts of the philologists and lawyers in the expedition-based knowledge of the field.

As if to prove their point, both Seligman and Rivers, independently from one another, undertook a whole series of further anthropological researches.

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Seligman first worked in New Guinea, as member of the Daniel Ethnographical Expedition (which resulted in his *Melanesians of British New Guinea*, 1910), and then, together with his wife, Brenda Seligman, undertook government-sponsored survey studies of Ceylon (*The Veddas*, 1911) and the Sudan (*The Pagan Tribes of the Nilotic Sudan*, 1932).[12]

While Seligman's work remained much indebted to nineteenth-century conceptions of ethnography—he may have stressed "the field," but the field mattered only insofar as it allowed for the empirically saturated classification of "races" and the comparison of customs—Rivers's continued anthropological research led him to articulate a whole new set of questions.

During the Torres Strait expedition, one of Rivers's tasks had been to study the prevalence of color blindness among the islanders. Curious about hereditary patterns, he asked his interlocutors about their genealogical relations and thus discovered, more by chance than by design, the extraordinary richness of the islanders' kinship vocabulary.[13] Confronted with the suggestion that people had several fathers and mothers, he worked out for himself a genealogical method that allowed him to reconstruct "blood ties" versus "affiliation" and "adoption" (his terms).[14] When in 1901 and 1902 he worked among the Todas (living in the Nilgiri Mountains of Southern India), Rivers, sensitized to and curious about the rich relational vocabulary of non-Western people, recognized the significance of kinship ties for understanding what he called "the system of relations" that seemed to silently organize who was responsible for the different elements of a given ceremony (*The Todas*, 1906).[15]

Could he draw up this system? And thereby explain the structure of the life of the Todas?

Perhaps one has to pause for a moment to appreciate how form-giving Rivers's "discovery" has been: his recognition of the organization of the whole of social life in terms of kinship in 1901–2 is one of the key moments in the history that would eventually lead to the emergence of classical modern ethnography, understood as the fieldwork-based study of a single society (ethnos) and its internal social structure.[16] As long as customs were specimens to be collected and shipped home, an island-hopping expedition was an adequate form of anthropological research. However, once customs were windows onto the lived enactment of the structure that silently organized the living together of individuals, a new, and in the early twentieth century yet undefined and unknown, form of research was needed.

Rivers was acutely aware of the need for methodological innovation. He knew that merely stressing the importance of "the field" was not enough.

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What was needed, in addition, was a new definition of what anthropology was about.

Rivers's most explicit effort to provide such a definition was his contribution to the edition of the *Notes and Queries* issued by the Royal Anthropological Institute in 1912. There he first affirmed that the goal of anthropology is, ultimately, to collect data that would eventually allow one to understand the early history of mankind (Rivers would always hold on to this nineteenth-century conception of anthropology).[17] But he then went ahead and wondered out loud if this overall goal wouldn't require carefully studying single societies and the system of relations that organize them, so that one could then later conduct a comparative study of their structure of organization. Indeed, Rivers not only recommended to future anthropologists that they study one society at a time but also encouraged them to study how the natives who belong to these societies actually see the world.

In a passage that (still) reads like an avant-garde program of experimental research, he wrote (I quote at length),

> Above all, never neglect a statement volunteered by a witness independently. . . . Leave the main path of the inquiry and follow this side track. If the volunteered statement is obscure or even quite unintelligible, so much the better; it may and probably does mean that you have been put on a track which will lead to something absolutely new and unsuspected, while your main path was probably leading to some goal already more or less understood and foreseen. . . . To many it will be repugnant; the person with an "orderly mind" who believes in probing one subject to the bottom before turning elsewhere and cannot suffer interruption in his train of thought, will miss much. He will probably complain bitterly of the difficulty of keeping the people to the point, not recognizing that the native also has a point, probably of far more interest than his own. Further, such information is of very great value as evidence, for it is certain to follow the native categories of thought.[18]

In the early 1920s, two young anthropologists who had worked with Seligman and with Rivers in particular presented the first empirical studies of a single society.

Alfred Reginald Radcliffe-Brown's (1881–1955) *The Andaman Islanders* (1922) was thoroughly indebted to Rivers's systems approach. Radcliffe-Brown presented his study—which was grounded in repeated, expedition-like trips to "the field"—as an almost mathematical inquiry into social organization typi-

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cal of a group at the very beginning of the history of mankind, at a time when neither government nor property were yet known.[19]

Bronisław Malinowski, though much influenced by both Seligman and Rivers, radically broke with the vision of anthropology upheld by his teachers.[20] He presented his *Argonauts of the Western Pacific* (1922) as the beginning of an altogether new kind of anthropology, one that was grounded in what he called "fieldwork" and that aimed to provide a description of the "inner life of a society."[21]

Malinowski styled his work as a sweeping departure from the nineteenth century. He ridiculed the island-hopping of the naturalists and in particular the armchair speculations of the philologists (and lawyers). Their speculations on this myth or that ritual, he explained to his perplexed readers, tell us more about the authors who offer them than about the actual life of the primitives.[22] Instead of speculating, the task of the "ethnographer" was to carefully study the role that a given custom or myth or material artifact played "inside" of a given society. Everything the natives did, Malinowski insisted, had its "function" or "meaning." And the only way to understand this meaning (function) was to conduct "fieldwork," that is, to learn the language, to take part in the everyday life of the natives, living among them, in their midst, "without other white men." The challenge was to immerse oneself in "the imponderabilia of everyday life" and to abstract from them the "underlying ideas" that organize the actions of the primitives.[23]

The significance of Malinowski for the history I try to write is that he—and with him his many students who conducted "fieldwork-based ethnography"—succeeded in decoupling "ethnography" from both evolutionary speculations and speculative reconstructions of the early history of mankind. His alternative was the inseparable correlation of ethnography—defined and practiced by him as the description of the inner life and organization of a society—and fieldwork.

Differently put, Malinowski invented—in a sweeping coup—what became self-evident in the course of the twentieth century: that anthropology is fieldwork is ethnography.[24] Most critical for this becoming self-evident was the first cohort of Malinowski's students, who made ethnography—fieldwork—the state-of-the-art method of anthropology. The reference is to Raymond Firth (1901–2002), Audrey Richard (1899–1984), Hortense Powdermaker (1896–1970), Isaac Schapera (1905–2003), Edward E. Evans-Pritchard (1902–1973), and Meyer Fortes (1906–1983), among others.[25]

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And Radcliffe-Brown?

If Radcliffe-Brown likened anthropology to a natural science busy describing social structures in the abstract, Malinowski likened anthropology to the arts—the challenge was to immerse oneself in the everyday life of a particular group; to discover, by way of attending to their conversations and habits, the "underlying ideas" that structure the natives' lives; and to then learn how to vividly describe, as a novelist describes (as a painter paints) the life of the primitive in such a way that the underlying ideas are rendered visible in the concrete—without rescue into the abstraction.[26]

If Radcliffe-Brown had taken from Rivers the interest in social evolution and systems of relations, then Malinowski took from Rivers the suggestion that "the native also has a point"—and that this point was implicit in his actions. For Malinowski, "the final goal, of which an ethnographer never should lose sight," was "to grasp the native point of view, his relation to life, to realize his vision of his world. We have to study man, and we have to study what concerns him most intimately, that is, the hold which life has on him."[27]

Do I really mean to suggest that anthropologists did not conduct fieldwork prior to the 1920s? No, if by "fieldwork" one merely means that someone has lived for a limited amount of time elsewhere and has written about her observations. Yes, if by "fieldwork" one means the disruptive methodological conception of research that emerged over the first two decades of the twentieth century and that forever changed what anthropology is about.[28]

SIX

What has been the effect of the emergence, since the late 1990s, of anthropologies after ethnos on the conception of "fieldwork as ethnography" as it was first articulated in the 1920s?

Differently put, what new, what other concepts of fieldwork have emerged from, say, studies of immunosuppressants (Lawrence Cohen 2001), Anonymous (Gabriella Coleman 2013), neoliberal city planning (Stephen Collier 2011), marine microbes (Stefan Helmreich 2009), influenza (Celia Lowe 2010; Carlo Caduff 2010, 2015), Chernobyl (Adriana Petryna 2002), open-source code (Christopher Kelty 2009), Matsutake (Anna Tsing 2015), life in vitro (Hannah Landecker 2000, 2002), bioprospecting (Cori Hayden 2003), AIDS (João Biehl 2007), cheese making (Heather Paxson 2012), monkeys (Nicolas Langlitz n.d.), curation (Tarek Elhaik 2016), basic income grants (James Fer-

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guson 2015), cinema (Anand Pandian 2015), and censorship (William Mazzarella 2013)?[29]

While it is difficult, and also unwarranted, to provide a single answer to the question of what fieldwork after ethnos is about—too wild, too untamed, are the still nascent fields of the anthropology after ethnos—it seems that one aspect many of the above-cited studies are concerned with is "difference in time."

In my schematic rendering, if anthropology in its form of classical modern ethnography was interested in difference in space—in distant others, their alternative social structure and cultural logic—then many of the anthropologies after ethnos seem to have replaced this classical interest in spatial difference with an intense curiosity about difference in time.

No longer does the anthropologist after ethnos ask "How is it elsewhere?" Instead she wonders out loud if something new/different has occurred—an event that set in motion a given domain of the real, that changed it, mutated it beyond recognition, an event that challenges the constitution of the real as we know it as well as the conceptions of the human that lay dormant therein.[30]

What is the effect of this interest in "difference in time" on the spatial category of the field? Why and to what ends does one conduct fieldwork when one is no longer conducting ethnography? What notion of the field, what concept of anthropology as a field science—if any—has emerged from the multitudinous, fieldwork-based studies of difference in time?[31]

SEVEN

In my observation, the anthropology interested in difference in time—in "events" and "the emergent"—is as radically a field science as has been the anthropology interested in difference in space. It upholds, like its more classical sister variant, the primacy of the field, the field's particular potential to lead astray, to profoundly derail the research questions one has laid out before one entered the field—and to thereby produce surprise.[32]

Fieldwork, for both classical modern ethnography and the anthropology after ethnos, is a powerful derailment machine, an opportunity to use the accidental as a tool for unanticipated discoveries.

However, where research into that which is only emerging differs from fieldwork as we know it—where it has provoked a powerful mutation of the idea of a field science, of what it is and what it is after—is that derailment is set to radically different use.

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For the more classically oriented anthropologists, that is, for those primarily interested in spatial difference (how people live elsewhere), the concept of derailment (most often) refers to the accumulative experience that one's own presuppositions—including those one did not even know about—are different from those made by the group of people one studies. The methodological significance of this experience is its enabling character—it enables the anthropologist to become (often through serendipitous accidents) aware of the presuppositions others make.

It follows that anthropologists usually assume, first, that a different set of underlying ideas organizes the lives of others (which implies that these "other ideas"—even if they are nowhere explicitly articulated—objectively exist out there, almost in the form of discrete entities to be discovered), and second, that derailment amounts to a kind of rerailment, a being rerailed to precisely those ideas that organize the culture or society (or the social or cultural phenomenon) one studies.

Rivers's above-quoted suggestion to follow "volunteered statements," even if they "lead astray," is a case in point: according to Rivers, this being led "astray" is a sure path to the discovery of "the native's categories of thought" (one could also mention here Malinowski's [1922] talk about "underlying ideas" or Geertz's [1973] suggestion that unarticulated "scripts" organize the native's life).

The significance of an anthropology interested in difference in time—in the conceptual turbulences provoked by the new/different—is that it has radically broken with both these assumptions. To be more precise, it has decoupled derailment—the loss of orientation that results from the recognition that one's presuppositions don't work—from the idea of a rerailment. For where one inquires into that which is only emerging (or not even that yet), into that which comes into existence only at the moment of fieldwork, into that which is such that it escapes the already thought and known, there can be, strictly speaking, nothing one could be rerailed to. Research into the emergent is, quite literally, research into the open (even if the open occurs in very concrete fields and forms).

Like a well, the new/different springs forth, bifurcates in all directions, is explorative, perhaps wild, almost certainly chaotic and incoherent; it likely has not yet given rise to a broad stream in a stable riverbed—and maybe it never will, for it may just as well ooze out and disappear.

The very aim of studying events that open up a difference in time is quite literally to capture the openings, the bifurcations, the troubles, the jumping

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forth, the new causes. At stake is to capture “instances of escape,” that is, situations in which the established breaks open. At stake as well is a continuous derailment by the unexpected ways and forms the new takes. Consequently, fieldworkers interested in the emergent are unlikely to be rerailed—for their goal is to be gripped by instances of derailment.[33]

Would it be a gross exaggeration, then, to suggest that the focus on “difference in time” has cut fieldwork loose from ethnography? And that the unanticipated consequence of this liberation has been that fieldwork has emerged as something in itself?

Today, fieldwork is no longer just a means to get at something that is more or less independent of it—the “underlying ideas” that supposedly structure the native’s life. Instead it is an artful—experimental—technique at the core of which are accidents that have the power to disrupt the taken for granted. To be more precise, that have the power to open up unanticipated, still emergent spaces of marvel and surprise for which no one has words or concepts yet.

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In chapter 1, I used the term “philosophical”—or “philosophically inclined”—anthropology. As I see it, the cutting loose of “fieldwork” from “ethnography” opens up a whole new set of possibilities for encounters between anthropology and art: Would it be saying too much to suggest that the possibilities of an empirically grounded, fieldwork-based philosophy I try to bring into view with this book—its focus on the accidental, its ventures into the irreducibly open, its curiosity about emergent forms, its celebration of movement / in terms of movement, its interest in escapes—offers a multitude of unanticipated interfaces between art and anthropology (between [some] artists and [some] philosophically inclined anthropologists)? Or that, on the level of technologies, vast, yet-to-be-explored venues for conversations and collaborations open up?[34]

EIGHT

There has been the occasional critique that when anthropologists entered labs, clinics, urban planning offices, and advertisement companies, and thus moved closer to cultural studies, science and technology studies, feminism, and theory- and history-inspired modes of inquiry, the power of the field got abandoned and eventually dried up.

I find such an argument utterly misleading. One may certainly argue that the rise to dominance of an anthropology of things modern has outgrown classical conceptions of the field. Who would doubt that? But it would be spectacu-

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larly wrong to assume that the process of outgrowing traditional conceptions of fieldwork as ethnography has implied the end of fieldwork.[35] In fact, I would argue that the exact opposite has been the case: since the 1990s the practice of fieldwork has proliferated in hitherto unknown ways. Anthropologists have transformed countless sites into fields that were once thought to be far beyond the scope of the discipline.

In short, over the last twenty years or so fieldwork has not disappeared—instead anthropologists have spectacularly expanded the possibility of conducting fieldwork. Indeed, in many ways, fieldwork—fieldwork itself—is more alive and well today than ever before.

Perhaps it has never been so exciting and extraordinary, never been so curious and creatively challenging to practice a field science as it is today.

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Do I seriously mean to suggest that when one studies rituals in India, one studies space, but if one studies science in Monterey, one studies time? Am I not, thereby, however implicitly, reintroducing the old nineteenth-century European equations of spatial with temporal difference? Worse, am I not suggesting that those "still" living in or bound by "space" have not yet entered "history," that is, "time?"

I don't think so. The point I seek to make is that many of the anthropological studies that have been published since the early 2000s—whether focused on rituals in India or science in Monterey—did not so much ground in a spatial curiosity (How is it in India? How in California?) as in a temporal curiosity: what kinds of rituals has the Partition introduced to India? What new conceptions of evolution has the microbiology conducted in Monterey opened up?

What defines the focus of the study is less (not primarily) place than—a difference in time.

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assemblages (or how to study difference in time?)

If the analytical vocabulary anthropology provided traditionally to its practitioners was designed to bring into view spatial differences, and if at least some anthropologists have broken with this focus on difference in space and replaced it with an interest in difference in time—then what new kind of analytical tools and concepts have these anthropologists of temporal difference come up with?

How can one bring into view—how can one analytically, through fieldwork, get a hold of—difference in time?

There are obvious candidates for answering this question. For example, over the last two decades anthropologists have in particular used the concept of the "event" and "the emergent" to capture temporal differences.[36] Equally prominent have been the concept of the "contemporary" or of "the recent past and the near future."[37] Here, however, I want to focus on an altogether different concept, "assemblages."[38]

My reference is less to the work of Gilles Deleuze and Félix Guattari (whose use of the term *agencement* in *Mille plateaux* Brian Massumi once translated as "assemblage") than to a fragment from a conversation between Bruno Latour and Michel Serres.[39]

How come, Latour wants to know from Serres, you treat long-dead authors as contemporaries?

"In order to say contemporary," Serres replies, "one must already be thinking of a certain time and thinking of it in a certain way. . . . So, let's put the question differently: What things are contemporary? Consider a late model car. It is a disparate aggregate of scientific and technical solutions dating from different periods. One can date it component by component: this part was invented at the turn of the century, another ten years ago, and Carnot's cycle is almost two hundred years old. . . . The ensemble is only contemporary by assemblage, by its design, its finish."[40]

The ensemble is contemporary only by *assemblage*.

Implicit in this formulation is an entire heuristic vocabulary for thinking about time in general and about difference in time in particular. Serres, however indirectly, suggests that any one moment in time, every instance of a here and now, ought be thought of as a temporal composite—as a *form* composed of different (disparate) elements moving in time. Almost

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as if he were saying, “Imagine the present as a *punctum*, a snapshot of the configuration between independent, freely moving elements at a given moment. Imagine further that each one of these elements has its own history, its own line of flight, its own speed, and that the connections between different elements, in itself perhaps a chance event, structures what is possible to think and say.”

To speak of an assemblage, one thus could say, is to relate to the present—or some tiny part of it—as if it were a form-in-motion composed of a set of different elements (these can be concepts, practices, institutions, machines, technologies or people and other things). One could analyze each element of this form-in-motion separately (where it came from, how it developed). One could trace their individual histories, the speed with which they traveled (some are presumably faster, others slower), the assemblages they were part of in the past (and maybe still are), and the relations they formed. Or one could analyze the history of the relations between elements, that is, how they built up over time, how they changed, how they became denser or looser, or how the arrival of a new element reconfigured all relations and thereby gave the assemblage a new, unanticipated dynamic. One could also analyze the rate(s) at which individual elements—or the entire assemblage—mutate.

Take, for example, avian flu.

What a curious—and deadly—assemblage, composed of a long list of heterogeneous elements: viruses (as such), poultry farms, the history of domestication, biology, birds, migration routes, nesting grounds, rivers and lakes, humans, veterinarians, public health, drug companies, and so on.[41]

Arguably, the migration routes of birds and drug companies have hardly ever been thought of as related, nor have the straightening and regulation of rivers and poultry farms. And yet, the hopping of a virus between different species connected these—and many more elements than I list here—into a closely knit and intricately entangled assemblage that is constitutive of avian flu.

In the case of avian flu, thus, the emergence of an assemblage where before all there was were loosely (if at all) related lines of flight amounts to a massive event.

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Disclaimer.

If I ponder Serres's reflections here, it is not because I think "Michel Serres got it right." Frankly, I don't know—and also don't care—if Serres got it right. What interests me here is not truth but rather possibility: the possibility of abstracting from a chance formulation—the ensemble is only contemporary by assemblage—a heuristically useful analytical vocabulary for an anthropological study of temporal difference.[42]

What follows are three brief (and tentative) entries that elaborate on what I mean by "heuristically useful."

First, the heuristic value of the concept of an assemblage is that it brings the present—understood as a moment in time—into view as composed of con-temporary elements (an assemblage), each with its distinctive moment of origin (some older, some more recent), each moving at a different speed—with different kinds of velocity—in different directions (and each element could be decomposed itself into an assemblage of sorts, made up of different kinds of elements, with different origins, and so on).

One primary effect of this rendering of a given moment in time (the here and now) as composed of moving elements is that it makes available to anthropologists an exuberantly rich vocabulary that is usually associated with the field of art, from music to painting to photography: composition, pace, movement, configuration, line of flight, directionality, dynamics, mutation, and speed.

One can now, for example, listen to Bach's *Art of Fugue* and begin to wonder—qua anthropologist of the here and now, qua de- and recomposer of the movements that make up the present—how one could single out among the many elements (each an instance of movement) one has listed in one's notebook just two in order to show how they ceaselessly circle one another, thereby producing the distinctive rhythm of (one segment of) life today.

Or one can look at the photography Étienne-Jules Marey—each a snapshot of movement, of the unfolding of time captured as motion—and wonder what one can learn, for example, from his rendering of the flight of a seagull for the analysis of different time lines of the elements of an assemblage.

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FIG. 3.1 Johann Sebastian Bach, *Art of Fugue*, 1751. What can one learn, as field-working anthropologist concerned with instances of time composed of lines of flight, from Bach about analyzing assemblages? Could one translate the velocity of the different kinds of elements of an assemblage into a melody? H. and Fr. Rungs Music Archive, Royal Danish Library, Copenhagen.

Or one can look at the paintings of Paul Klee, who continuously described his art as an analysis of the ceaseless, inexhaustible movement and becoming implicit in plants and animals, in humans and things—as the practice of isolating the elements of this movement/becoming and of recomposing them into possible and yet unknown figures, figures that no one had ever seen.

Once one operates, on a heuristic level, with the term assemblage a whole new (and largely unanticipated) set of analytical vocabularies for an analysis of movement/in terms of movement becomes available.

What would it take to come up with a compositional analysis of the present? With a decoding of lines of flight in a tonal analysis? With a typography of possible kinds of movement? With an art history of the forms relations can take?[43]

Second, the concept of assemblage leaves behind the figure of "the human" and along with it what I have called (chapter 2) the "anthropocentric epistemology" on which anthropology had been built.

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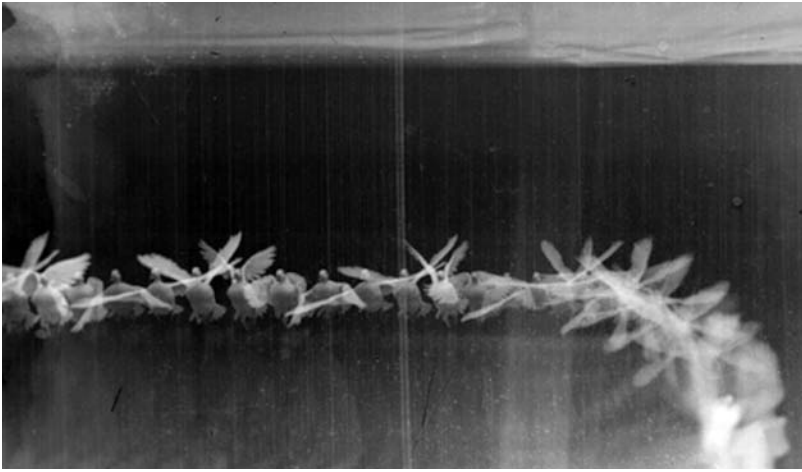

FIG. 3.2 Étienne-Jules Marey, *Analysis of the Flight of a Seagull*, 1887. Could one equally transform the line of flight—the time line—of an element of an assemblage into a linear series of photos—or of letters, words, sentences, paragraphs, texts? Etienne-Jules Marey/Dépot du Collège de France, Musée Marey, Beaune, France.

Take avian flu again: humans, birds, nesting grounds, laboratories, governments, migration routes, rivers, livestock, viruses, culling machines—are these vertical slices of the real, to borrow a phrase from Sandra Hyde (2007), human or nonhuman? Are they natural or cultural?[44]

Or take swine flu (substitute birds with pigs); or MERS (camels); Ebola (bats); or SARS (palm civets and/or raccoon dogs).[45]

Who is the author of these configurations?

Anna Tsing's suggestive answer to this question is that assemblages are by and large chance assemblies that exceed any form of human intentionality. Assemblages, consequently, are marked by "patterns of unintentional coordination." They are "open-ended gatherings" which potentially become "happenings" (as when, for example, a virus hops from birds—or pigs, camels, bats, palm civets—to humans).[46]

The assemblage concept—a bit like Althusser's reinterpretation of historical materialism and Foucault's dispositif—cuts anthropological inquiry loose from the exclusive focus on the figure of "the human" as well as from its exclusive attention to "human world-making."[47]

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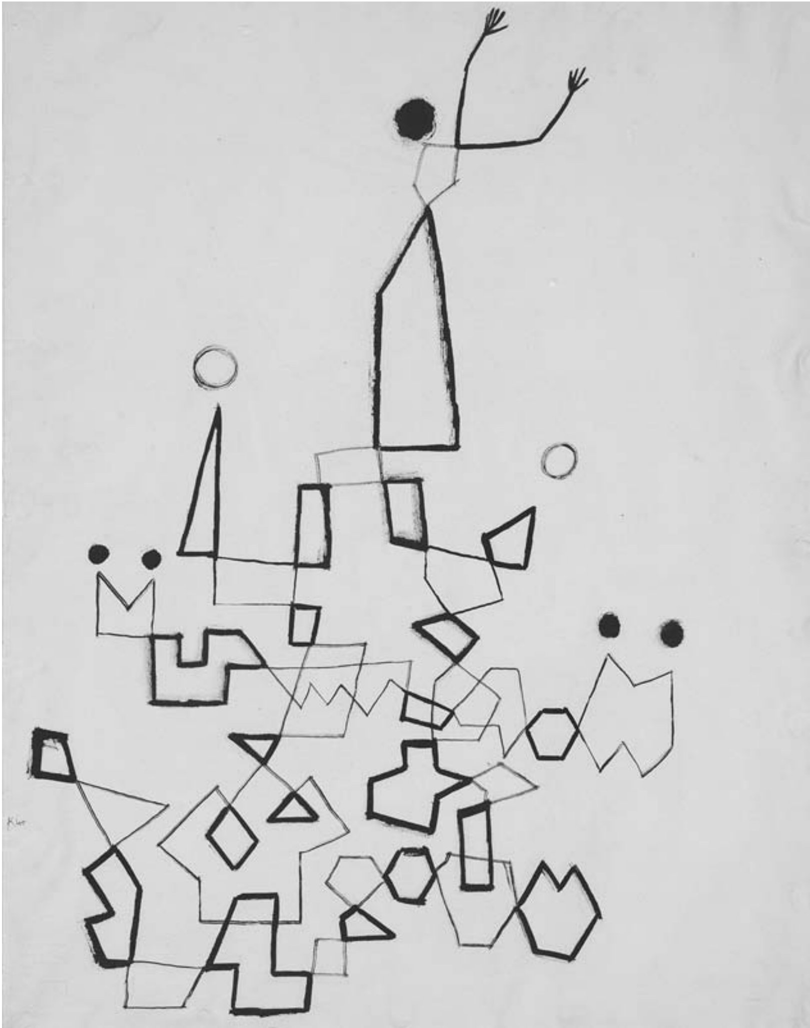

FIG. 3.3 Paul Klee, *Drüber und Empor*, 1931. Brush on paper on cardboard, 61.5 × 48.7 cm. This drawing nicely serves to illustrate the idea of an assemblage as a moving form, composed of different—and differently related—elements, each of which can itself be disassembled and recomposed. *Drüber und Empor*. *Above and Aloft*. Zentrum Paul Klee, Bern, Switzerland.

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Three, the assemblage concept allows one to differentiate an anthropological, fieldwork-based inquiry into temporal difference from the mode of inquiry called history of science.

A study of conceptual movements in the here and now often relies on concepts derived from the history of science, most prominently "rupture" and "discontinuity."[48] While these concepts can no doubt be helpful, more often than not they are a burden to anthropologists for two reasons.

First, the historians of science who invented the concepts "rupture" and "discontinuity"—most famously Gaston Bachelard and Georges Canguilhem—were working on the past. That is, they already knew that the conceptual breaks they studied had accumulated—gradually or suddenly—into major events in the history of science (that is, for the most part, why they studied these breaks/ruptures). The anthropologist who works in the here and now doesn't have the comfort of hindsight. On the contrary, immersed in the chance encounters of fieldwork, there is no way for her to know whether the indices of movement (of change) she carefully captures in her notebooks are eventually going to be significant in a wider, more general sense.

Second, the concepts "rupture" and "discontinuity" were invented by historians and epistemologists to bring into view somewhat totalizing events: a rupture (or a discontinuity) serves to identify a breaking point that allows one to divide a story into a clear before and after. Again, the anthropologist of the here and now, working amid the chaos of fieldwork, can never quite know whether or not the changes (the movements) she seems to have found amount to a full scale rupture (or event).

If thinking in terms of assemblages—if relating to the present in terms of assemblages—is useful, it is not least because it allows to differentiate the study of movement from the study of ruptures (rupture—or discontinuity—is not the only form movement can take); it allows one to bring into analytical focus movements that unfold below the radar of those looking for ruptures—movements that can be interesting and curious for a whole variety of different reasons.

For example, a study of an assemblage might be less interested in a full-blown rupture than in the transfiguration of a single element of an assemblage and the almost undetectable changes in connections between

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elements that silently change what counts as true or that change the dynamic of a whole assemblage without an overt rupture ever occurring.[49]

Or such a study might be interested in decomposing a taken-for-granted truth in an assemblage that never stands still; it might be an effort to document the small lines of mutations that continuously but silently alter the elements of an assemblage that, on the surface of things, seems unchanging and stable. It might as well be an effort to detect how the arrival of a new element reconfigures the pattern of connectivity that seems to have governed an assemblage thus far.

Of course, a rupture (discontinuity) can still be the outcome of anthropological research—but for a study to be successful or interesting, it doesn't have to be.[50]

Take (one last example) the difference global health makes.

From the late 1940s to the late 1990s, world health was organized in the form of international health (IH). IH was grounded in the old eighteenth-century assumption that humanity comes in the form of a family of nations (each nations made up of one society, people, Volk), and that each nation has a government, which is responsible for the well-being of its society. Consequently, IH was composed of national populations, of national governments, and of international institutions, notably the WHO (and, since the 1970s, of emergency NGOs such as MSF).[51] For half a century, IH was a relatively stable assemblage (though arguably none of the defining elements ever stood still). In the late 1990s, however, a whole new set of institutions and actors dedicated to world health emerged and began to speak of global rather than international health: private foundations, philanthropies, NGOs, and private-public partnerships, many of which positioned themselves as alternatives to the WHO. Most famous among them are the Bill and Melinda Gates Foundation, Malaria no More, Label Red, the CUGH, the IHME, and the Clinton Foundation.[52]

Has the emergence of Global Health been a rupture? Is there a clear-cut distinction between a formation called GH and a formation called IH? Have the institutions constitutive of IH been rendered obsolete by the emergence of GH?

Hardly. The assemblage concept allows one to avoid the either/or logic of the rupture concept and to instead focus on how the emergence of new

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elements decentered elements that were formerly central (think of the effect of GH on the WHO) or how they reconfigured the connections between elements that defined what was possible and thereby gave rise to new venues of action (think of the emergence of private-public partnerships).[53]

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not history

Isn't anthropology after ethnos a form of history, then? A history of the here and now?[54]

Precisely not.

A historian, by disciplinary default, will understand the present as a historical moment—even if the here and now is a moment of change, of turbulence. As history is for her the unfolding of time, and as nothing can exist outside of time, everything that is must be thought of as the product of history.

An anthropology after ethnos is not dismissive of history—and yet, it has radically broken with what one could call a historical mode of reasoning (which doesn't mean to say that it cannot make use of this mode).

How so?

Well, insofar as the aim of an anthropology after ethnos is to focus on that which escapes the already established—which escapes the possibilities implicit in the already thought and known—its focus is precisely not on history but on those aspects in the here and now that escape it, that cannot be explained by it.

Differently put, anthropology after ethnos is concerned with the unanticipated spaces of marvel and surprise that an incidental departure from the past opens up (and the reason it is unanticipated is precisely because it conceptually escapes the spaces of possibility that had historically structured the present up until now).

Take, for example, the idea of a "history of the present." Foucault's ambition, when he developed the history of the present, was to show how unlikely our contemporary categories of thought were until very recently, how much needed to happen to make them plausible, and, indeed possible. He was concerned with the present—he wanted to relieve the here and now of the already thought and known—but his intellectual tools where those of a historian: Foucault brought today into view as a product of yesterday.

To the anthropologists after ethnos, things look quite different: where Foucault writes the history of the present, the anthropologist after ethnos / interested in difference in time is interested in precisely those aspects of

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the here and now that escape the present of which Foucault has been writing the history. There is a vast gap, an abyss, between the field of history and the field explored by the anthropology after ethnos.

If one were to equate history with the timely, that is, with that which has existed in time, then the actual is not that which exists outside of time, as if it were an eternal realm, but the untimely. And this is precisely the challenge of the anthropology of the actual: to get at things untimely.

I cannot refrain from pointing out another powerful difference between the history of the present and the anthropology after ethnos: perhaps both seek, in their distinct ways, to liberate the present from the past. But can the historian of the present ever move beyond, well, the present? That is, can she ever depart from the categories of thought of which she writes the history? To ask the question is to answer it: the limit of the history of the present is that it remains part of the formation it problematizes.

Not so with the anthropology after ethnos: as a fieldwork-based mode of inquiry, it is focused on that which escapes the present understood as product of the past; it moves forward, it leaves behind, it explores new, unanticipated spaces of possibility that are still nascent, emergent, not yet—and perhaps never—stabilized.[55]

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epochal (no more)

"You say you are interested in temporal differences. But if you sort time into a before and an after, then isn't it going to amount to a claim of epochal divides?"

The frequency with which I was asked this question, or some version thereof, has been a powerful reminder of just how sensitive anthropologists are to any interest in "difference in time."

How come?

On the one hand, there is the general implausibility of any sharp epochal ruptures—of the suggestion that clear-cut fault lines set apart conceptually coherent epochs from one another.

On the other hand—and for the discipline much more form giving—the reservation against epochal arguments is explained by the critique of the many ways in which anthropology has been contingent on and complicit with philosophies of history that have explained spatial differences (how people live elsewhere) in terms of a linear understanding of human history as progress (they still live in our past). The reference here is to Frantz Fanon (1961), Kathleen Gough (1967), Gerard Leclerc (1972), Talal Asad (1973), Bob Scholte ([1973] 1999), and Diane Lewis (1973); to Edward Said (1978), Johannes Fabian (1983) Fritz Kramer (1977), Dipesh Chakrabarty (2000), and Gayatri Spivak (1987, 1990). Through these critiques an acute political problematization of the articulation of any temporal differences has become a feature of anthropology.

What I have described as fieldwork-based anthropology after ethnos is deeply informed by both of these critiques.

First, if one thinks of the present in terms of assemblages—of contemporaneity—and if one thinks about change in terms of the nonlinear, multifaceted temporal movements (plural) of the various heterogeneous elements that make up any assemblage, then how could one maintain the illusion of the epochal? That is, the assumption that there are clear-cut ruptures that divide the world *au total* into a clear-cut before and after?

The mutations of the possible that an anthropology interested in temporal differences brings into focus have nothing of the grandeur of the epochal. No wave that washes away a face drawn in the sand at the edge of the sea—and then everything is different.

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Second, at stake in a fieldwork-based anthropology after ethnos is hardly the establishment of a philosophy of history—neither in terms of the temporal mediation of spatial difference (the localization of people who live differently from us in our past) nor with respect to the consolidation of a unified schema of the history of humanity (from the state of nature to the present). What is at stake, instead, are instances of temporal difference that disrupt the conceptual presuppositions that have rendered possible given fields of knowledge—not least the field of history (or the history of humanity) itself.

Provocatively put, rather than establishing histories, the anthropology of temporal difference exposes and thereby undermines—as an end in itself—the very condition of possibility of any history. A bit as if the anthropology of temporal difference were a revolt against the conceptualization of temporal difference as history.

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