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In this interesting paper, Istomin and Dwyer address a theoretical debate within anthropology concerning how people from technologically undeveloped cultures (and, presumably, the rest of us) orient themselves while navigating. The debate is cast as a contest between those who advocate a “mental map” and those who advocate “practical mastery.” The authors are right—this is a poorly framed debate for which the answer, on empirical and common-sense grounds, is “both.” Or I suppose you could say it was “neither.” Either way, as it appears it has been framed in anthropology, this is indeed a straw-man debate between two positions that are largely untenable on their own. The practical-mastery theory explains wayfinding as a sense of “knowing the land” that results from experience moving through it. But, cartographic maps aside, how would a person acquire a mental map if not through experience moving through the landscape? And what would it mean to “know the land” if not to form representations of it in the mind? Do proponents of practical mastery think “knowing” is stored in muscles or bones? The practical-mastery position is quite reminiscent of the theory of direct ecological perception expounded by Gibson (1979), whom the authors cite. Although Gibson—or at least his followers—offered an alternative to mental maps, I think almost no one takes direct perception too seriously as a viable model of skills such as orienting to distant landmarks. (An aside: the authors write that “[e]ven Gibson . . . never appeared to insist that his ecological vision was the only basis for explaining spatial behavior among humans. Certain results of the pointing experiments simply excluded this possibility.” I do not believe that J. J. Gibson read papers about cognitive maps or pointing experiments of the kind to which the authors refer, but he and his followers certainly did believe that ecological theory was the explanation for spatial behavior. They did allow for cartographic maps but not cognitive ones.) The important work by Hutchins (1995) on situated cognition deserves consideration here as well. Hutchins made highly relevant observations about the situated nature of navigation and orientation, but as with Gibson’s ideas, few people would accept it as a complete alternative to mental maps.

Questions about the “nonindexical” quality of mental maps, their dimensionality (1 or 2), their geometric sophistication, and the reference systems used in their organization and expression in language are interesting and important issues. In fact, they constitute much of the deepest theoretical debates and fundamental research questions across the disciplines that study spatial cognition. Researchers in geography and human psychology generally do not reserve the term “mental map” or “cognitive map” only for two-dimensional, nonindexical, metric survey knowledge; a mental map is one’s

internal representation of the environment, whatever its form and specific content (Downs and Stea 1973). Yes, there is extensive discussion in the literature about the constructs of “route maps” and “survey maps,” but both are internal representations of the environment that may be referred to metaphorically as internal “maps.” Researchers in biology and animal psychology, in contrast, more or less do reserve the concept of mental maps for two-dimensional, nonindexical survey knowledge. Having said this, I doubt that you could find many proponents of mental maps, including myself, who would claim that they are fully nonindexical, representing all of the Earth surface equivalently and allowing access from any point to any other point with equal speed and accuracy. So if that’s what you think a mental map is, you are right—they do not exist.

I agree with the authors that research on cultural variations is valuable and intriguing, even fascinating, but I wonder how far ethnographic unstructured interviews and sketch mapping can go to adjudicate the theoretical issues the authors discuss. There is no way, for example, to sketch or otherwise make a map from scratch, whether with sticks, paper, or computers, except by employing a sequential process. This in no way proves that the underlying representation is necessarily routelike. On the other hand, being able to sketch a two-dimensional configuration does not prove that one has an underlying survey map. I could turn an internal list of places along a route into an accurate survey map during the drawing process, as long as the list included at least approximate information about metric distances and directions between some of the places. The ability to accurately sketch maps is probably needed if one is to make the case for a completely “nonindexical” survey map, but it in no way suffices to prove the case.

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This is a most welcome continuation of a discussion that tackles the intriguing problem of understanding not only human orientation but also how cognitive processes work more generally. In my first contribution to this topic I had concluded that “both practice theories and mental-map theories fail to account for all skilled and habitual forms of orientation” (Widlok 1997, 328). I am therefore very pleased to see this conclusion replicated by interdisciplinary research based on a very different set of empirical data. It is in the framework of this general appreciation of the work by Istomin and Dwyer that I offer these critical comments for further thought and research.

A weak reading of my earlier conclusion was that practice theory and mental-map theory, each taken by itself, do not explain orientation. However, this does not necessarily imply

that we should call it a futile debate, because it is through this discussion that we were led to make distinctions that went unnoticed previously. Gell and Ingold highlighted that different purposes and backgrounds (e.g., that of a mapmaker, in contrast to a person habitually moving around) also have to be considered when designing our methods. Ingold's emphasis on practice is strategically important because it is a corrective to our dominant default model to treat all knowledge as if it was maplike. Finally, Levinson et al.'s work of different frames of references convinced researchers across disciplinary boundaries that there is more than one human frame of reference for orientation available. In the meantime, we now have not only case studies and positions from the extremes of the spectrum that initially fired the debate but also many more "mixed results" that have emerged, for instance, when frames of reference are combined in the way that one axis is predominantly absolute and the other axis is relative (see Widlok 2007). What I take Istomin and Dwyer to mean is that the combination of what appeared to be exclusive alternatives is probably the normal state of affairs. But does it follow that we should simply subscribe to both models? Do humans switch between two modes of cognition depending on cultural contexts and situations because two of our models (practice and mental maps) each appear to be half-true?

There is also a strong reading of my earlier conclusion that I would like the authors to consider, namely, that both models fail to account for the orientation skills we are dealing with, even when they are taken together as a duality. I am not so much concerned about the general point that an explanation that is based on a single model is in terms of theory to be preferred to one based on a duality of models but rather about unresolved contradictions surrounding the notion of mental representation in the mental-map model and about the notion that we are faced with two clearly separable systems (organism and environment) interacting with one another. I shall refer to these points in reaction to recent developments in neuroscience, another source of information that we as anthropologists need to be able to deal with. These new developments were succinctly summarized in a recent monograph (Fuchs 2008) but are reflected also by other attempts to confront ecological psychology and phenomenological approaches with the neurosciences.

Unless we want to assume that cognitive maps are stored in a completely immaterial and noncorporeal entity called "culture" (which I suggest we should not, for its obvious problems) neuroscientists suggest that cognitive maps are mental representations, nowadays equated with more or less localized patterns of neural activity. In our case of orientation, this would mean that a certain pattern of neural activity (the mental map) is triggered once a certain situational stimulus is received, for example, crossing the river and getting to the country where one knows one's way. Fuchs argues that the notion of representation (or map) in such a context would be flawed. While representations always refer to an image of

something for someone, this subjective someone is being glossed over by most neuroscientists and mental-map modelers. In other words, he argues that environmental information is not stored as a representation in the brain, and mental states are not descriptions of the landscape, but they only participate in the situations from which corporeal agents are able to derive the relevant contents (Fuchs 2008, 146). We, as situated human beings (and not the brain by itself), generate representations based on implicit and incomplete knowledge that "only" provides options for acting in a certain way. And since our particular perspectives as corporeal beings differ because of our experiences and positions in society, there is variation to be found within and between groups. This is not to say that there is no physiological basis for orientation knowledge; rather, it is not the brain alone that creates this basis but a larger system of the organism in its environment. Mental maps therefore do not contain meaningful information, but as patterns of neural activity, they participate in situations where agents create contents and skill. It is not about retrieving memory but about providing "open loops" for experience in the sense of a readiness to act in anticipation of certain situations (like having crossed a certain river).

As long as the implicit or explicit notion of the mental map is as problematic as that of "retrieving" representational contents from the brain, anthropologists should continue looking for a better model to explain orientation.

Reply

We wish to thank the numerous reviewers for providing, throughout the publication process, suggestions and comments that we generally found very useful, insightful, and at times, challenging.

Aporta has four main issues with our paper. First, he disagrees that route-based spatial cognition and mental maps do not conflict, which, therefore, implies that human spatial orientation can be based strictly on either visual memorization of routes or mental maps. He opts for route-based spatial cognition, which despite not "necessar(ily) contesting the existence of mental maps," leaves us wondering about the mysterious process and conditions under which humans would select one method rather than the other?

Second, according to Aporta, our understanding of Ingold's ideas is "limited and . . . misleading," since Ingold "never actually states that humans are incapable of spatial abstraction." To address this comment, we first need to quote a section of Ingold's work.

"For the map-using stranger, making his way in unfamiliar country, 'being here' or 'going there' generally entails the ability to identify one's current or intended future position with a certain spatial or geographic location, defined by the