What Anthropologists Can Learn from Psychologists, and the Other Way Around

Kara Weisman[1] and T. M. Luhrmann[21]

[1]Department of Psychology and [2]Department of Anthropology, Stanford University

Abstract: The Mind and Spirit Project uses methods from anthropology and psychology to explore the way understandings of what English-speakers call 'the mind' may shape the kinds of events people experience and deem 'spiritual.' In this piece, we step back to reflect on this interdisciplinary approach. We observe that, in some ways, both fields are in parallel states of critical self-reflection around explanation and comparison: anthropology in the wake of the post-modern and post-colonial critique; and psychology in response to a pair of recent crises about the overreliance on Western samples and the reproducibility of psychological research. We suggest that combining our methods may go some way toward giving each field more confidence in its research. Joint fieldwork with specific point-by-point comparison is not common in either anthropology or psychology. We found it fruitful and commend it to others.

***

The Mind and Spirit Project is a comparative, interdisciplinary project based at Stanford University, funded by the Templeton Foundation, and led by T. M. Luhrmann.[2] The project draws on the expertise of anthropologists, psychologists, historians, and philosophers to explore whether different understandings of 'mind,' broadly construed, might shape the ways that people attend to and interpret experiences they deem 'spiritual' or 'supernatural.' We took a mixed method, multiphase approach, combining participant observation, long-form semi-structured interviews, quantitative surveys among the general population and local undergraduates, and psychological experiments with children and adults. We worked in five different countries: China, Ghana, Thailand, the US, and Vanuatu, with some additional

[1] This work reflects joint first authorship.
[2] This paragraph is based on a description drafted collectively by the Mind and Spirit team.

work in the Ecuadorian Amazon. In each country, we included a focus on a charismatic evangelical Christian church in an urban center, with additional work in a rural church, and in another religious setting of local importance (in both urban and rural locations).

The other papers included in this special issue have focused primarily on ethnographic observations from individual field sites (Aulino, this issue; Brahinsky, this issue; Dulin, this issue; Dzokoto, this issue; Ng, this issue; Smith, this issue) and on qualitative comparisons that inform our current sense of what we've learned from the project (Luhrmann, this issue). We consider this the initial "anthropological" take on the Mind and Spirit Project. Future papers will, in turn, introduce the more "psychological" side of the project, reporting the results of (ongoing) quantitative and experimental work that serves to test the hypotheses emerging from our ethnographic observations.

In our day-to-day work on the project, however, there was not such a stark divide between the anthropological and the psychological; both perspectives informed all aspects of our research from the beginning. The influence of psychology can be felt in the ethnographic observations that are the focus of this issue, much as the influence of anthropology will be felt in our experimental reports. In this paper, we—the project's resident psychologist and chief data analyst (Weisman) and the principal investigator and senior anthropologist (Luhrmann)—turn our attention to the way the team borrowed from each of our respective fields over the course of our long collaboration. We think of each field as working with a *mindset*: a mode of asking and answering questions, a way of thinking about how to plan and conduct research, and identify and interpret evidence. Along with a mindset go a set of methods developed to achieve these goals. We've learned a lot from the way that our respective fields think about what counts as evidence, and about how to collect evidence to answer our questions.

This is hardly the first time that anthropologists and psychologists have collaborated (see, e.g., Astuti and Carey 2004; Astuti and Harris 2008; Duncan, Huston and Weisner 2007; Minow, Shweder and Markus 2008; Norenzayan et al 2016; among many others); there are even subfields built out of the

commitment to joint exploration (Psychological Anthropology and Cultural Psychology). There are some psychologists (like Cristine Legare and Suzanne Gaskins, both involved in the Mind and Spirit Project) with extensive experience in anthropological methods and some anthropologists (like Rita Astuti and Pascal Boyer) with extensive experience with psychological methods. Yet systematic collaboration on so large a scale as the Mind and Spirit Project is still relatively rare. Most anthropologists still work as lone wolf researchers. In our view, more of us should seek out such opportunities—not only because such collaborations are fascinating, but because psychology and anthropology are unusually complementary. Both fields are acutely aware of how difficult it is to study and theorize about our fellow humans ethically, responsibly, and carefully, but anthropologists and psychologists have historically chosen to address these challenges quite differently. We suggest that incorporating mindsets and methods from each field can speak to the pressing challenges of the other. Because this journal has a largely anthropological readership, we write here with an eye toward anthropological concerns.

Current challenges in anthropology

Anthropology's most intensive period of self-reflection began several decades ago. At the heart of the postmodern and postcolonial critique lay guilt about replicating colonial dynamics in scholarly practice (see the useful history in Schnegg 2014). Michel Foucault began to dominate the discipline. Scholars shied away from subjecting other people—usually, poorer people with less power—to what they began to see as reductive categories that stripped away their subjectivity. They grew uncomfortable with the idea that friends they made in the field were also data (Behar 1996). They wrote critiques about writing and observing as forms of control (Clifford and Marcus 1986), and they experimented with different forms of representation (for example, Lavie 1990). They shifted their attention—in Joel Robbins' (2013) striking characterization—from the 'savage subject' to the 'suffering subject.' They began to think about anthropology not so much as a field about explaining difference but more as a means

to witness the injustices inflicted by some societies on others. *Anthropology as Cultural Critique* (1986) became a disciplinary bestseller.

The field has learned a great deal from these critiques. Anthropologists have grown acutely conscious of the power asymmetry between the one who conducts the research and the one about whom the research is conducted. They have sought out ways to collaborate with their research participants, from co-authoring to creating a form of fieldwork now known as 'participatory,' in which researchers work as partners with local participants on problems of direct interest to them (Jessee, Collum and Gragg 2015; Ross et al. 2011). All anthropologists have grown more sophisticated about the politics of representation and more sensitive to the risks of latent racism and sexism. They have grown far more conscious of the researcher's unconscious bias.

And yet, these new sensitivities have come at some cost. In particular, anthropologists have become more hesitant of causal explanation and of direct comparison (Borofsky 2019). They rarely compare groups of people or particular cultural contexts explicitly, because they are so aware of the many ways comparison can go astray—for example, by treating one group (often White Americans or Europeans) as the norm or standard from which the 'other' might vary; or by imposing the categories of one context (often, an American or European context) onto the behaviors and experiences of another context. Even in subfields like medical anthropology, where anthropologists sometimes collaborate with medical scientists, direct comparison across groups of people is not the norm.

As a result, these days in the corridors of anthropology departments one can hear rumblings about the field's refusal to ask big, broad questions, like why Europe was so successful in acquiring wealth and power (Diamond 1997), or what social forces equalize income inequality across time and space (Scheidel 2017). In a recent paper, a group of young anthropologists write, "Of all the social and historical sciences, anthropology is perhaps that which is most formally aligned with the very idea of the comparative … Yet in practice, social and cultural anthropology may be one of the least comparative disciplines" (Miller et al.

2019: 284). "Where have all the comparisons gone?" bemoans a more senior group (Borofsky, Nader, Candea and Friedman 2019.) A senior scholar, Marshall Sahlins, goes so far as to suggest that anthropologists have given up the attempt to explain the differences they observe:

> The large increase in the number of North American anthropologists since the 1950s has been matched by their interest in increasingly varied and arcane cultural singularities … the gourmandization of hummus in Israel, the biopolitics of the US war on fat, pyramid schemes in postsocialist Albania, spatiality in Brazilian hip hop and community radio, the occupy movement in Zizek's hometown, and new uses of the honeybee (in Descola 2013: xi-xii).

"Where did anthropology go?" asks Maurice Bloch (2005). There are, he writes, no shared questions, and anthropology graduate students have learned that the very effort to generalize is wrong—that is, that they should not to offer general explanations of anything. Roy D'Andrade (1995) shared that view, and argued that anthropology had come to this point because the postmodern critique led anthropologists towards moral stances—witnessing, giving testimony to pain. Debate disappeared because everyone agreed in the moral stakes; as D'Andrade would say, everyone thinks that oppression is bad.

Such claims, of course, are not wholly fair. The paper on the Occupy movement in Zizek's home town, which Sahlins presents as evidence of contemporary anthropology's interest in the particular rather than the general, was really trying to compare two models of activist democracy in order to consider the way politics might be reimagined. One could make similar points about these other papers. And yet many anthropologists might agree that the discipline has been cautious about explanation and comparison for decades.

Some trends in contemporary anthropology even explicitly reject explanation as a kind of epistemic violence. The leader of the recent 'ontological turn,' Eduardo Vivieros de Castro, argues that the very attempt to explain what people mean in terms that are different from theirs is a kind of ethnocentric imperialism:

> The most Kantian of disciplines, anthropology, is practiced as if its paramount task were to explain how it comes to know (to represent) its object—an object also defined as knowledge (or

> representation). Is it possible to know it? Is it decent to know it? Do we really know it, or do we see it (and ourselves) through a glass, darkly? There is no way out of this maze of mirrors, mire of guilt. Reification or fetishism is our major care and scare: we began by accusing savages of confusing representations with reality; now we accuse ourselves (or, rather, our colleagues). (Vivieros de Castro 2004: 483-484).

The ethnography at issue in this quotation is one in which people make apparently incredible claims: for example, that a child looks at what seems to be her human mother, sees a jaguar's tail between that human's legs, screams and then sees the jaguar (which no longer looks like her mother) bound away into the forest (Vilaça 2005). Vivieros de Castro seems to insist that the claim (the jaguar shape-shifted into a human) should be taken at face value (jaguars are able to make themselves appear to be human) and the attempt to explain it in any other way is morally wrong.

Patricia Greenfield, a partial insider trained as both anthropologist and psychologist, published an essay in 2000 that described what she saw as anthropology's "breast-beating and self-flagellation" (2000: 564). Looking at the discipline's reaction first to Geertz's interpretivism and then to the postmodern critique that followed, she saw a field struggling with sharp critiques that objectivity is impossible, that cultures aren't unitary wholes and so cannot be studied as such, and that observation is always political. She suggested that psychology may have been less daunted by the postmodern critiques of the period because psychologists specified their methods so explicitly and were so clear that their claims were always inferences from limited evidence—and so the acknowledgement of bias was built into structure of the research.

We think that Greenfield has a point. We found that when we adopted a more psychological mindset, focusing in on specific findings produced by methods we knew to be limited, it became easier for us to see how much anthropological methods could accomplish despite their limitations—and that this was liberating both for the psychologists and the anthropologists in the group.

These observations may be timely. In anthropology there are stirrings now towards a new comparativism. There has been great excitement about Philippe Descola's (2013) comparison of the way

social worlds imagine the relations of humans to the natural world. Peter van der Veer (2016) recently published his Lewis Henry Morgan lectures on the value of comparison in anthropology. Matei Candea has produced a text entitled *Comparison in Anthropology: The Impossible Method* which lays out challenges and offers solutions. Candea writes: 'A new wind of epistemological confidence is blowing through the discipline, and comparison is explicitly reclaimed and brandished as the distinctive anthropological method' (2018:1-2). These efforts are indeed chastened by what Sylvia Yanagisako (2007: 1)—in her own essay on the importance of comparison—calls 'the recognition that contemporary local communities, ethnic groups, religions and what we once called 'culture areas' are not autonomous, self-regulating, self-reproducing or self-developing systems.' This new comparativism avoids claims about the way one geographical area or type of person differs from another in general, but instead focuses on claims about the way specific phenomena—the imagined relationship between human and non-human animals, panic disorder, psychosis—differ in different communities, and why. That may enable us to move beyond the limitations of the cautious consensus of recent year without repeating the mistakes of the past.

Here we explain what we found useful from each other's fields, and how our process became (to borrow Levi-Strauss's pleasing phrase) good to think with.

The psychologist's focus on specific findings

Psychologists conduct experiments. They make a single intervention—they pass out a survey, they tell people a story, they ask them to select one out of four pens--and from the participant's response, they draw a conclusion. Compared to anthropologists, they work with a very small amount of evidence. This has led the field to pay close attention to the specifics of what a researcher did and what happened, and to hone in with great skepticism on the relationships between the limited data and the inferences drawn by the researcher. This is the psychological mindset we found useful.

For example, professional talks in psychology departments often focus on just a few experiments. That gives the audience a very clear topic: what did the speaker find out, and are their conclusions reasonable? Perhaps there is a talk in which a speaker presents the results of an experiment suggesting that reading fiction (phenomenon A) causes greater donations to charity (phenomenon B) by virtue of putting readers in a more empathetic frame of mind (mechanism C). An audience of psychologists would likely respond to that talk with attempts to counter the speaker's argument by explicitly proposing 'alternative explanations' for the observations presented. For instance: 'What if it only looks like A causes B because of some additional variable D that you didn't manipulate or measure?'; 'You claim your experiment manipulated Phenomenon A, but you also manipulated Phenomenon E, which could affect B for a totally different reason that's unrelated to A'; 'You claim to be measuring Mechanism C, but I don't think that you did.' The specificity cuts the claim down to size. The question becomes: from these specific observations, what kind of inference makes sense? What is the boldest claim we can make with the data, given how small a slice of the world it really is? In some ideal form (though perhaps not in practice), a psychological experiment produces one clear, single finding—and the job of the scholar is to figure out what it means.

With this comes an expectation that the logic of a psychological explanation and the nature of psychological evidence should be exquisitely clear. Rita Astuti (personal communication) remembers her own surprise when she began reading psychological studies about the young child's developing understanding of biological inheritance. There was an initial paper that presented a claim and the evidence to support it; and then another that questioned the interpretation of the evidence
and presented a new experimental design, producing new evidence that supported a slightly revised claim; and then yet another paper. Astuti remembers that she laid them out one after another in her living room, and that she felt she had never seen anything so structured and cumulative in the recent anthropological

scholarship she had read in graduate school. Her own work has since become a paradigmatic example of clarity for anthropologists.

Anthropologists of course are skeptical and concerned with detail, but the ethnographic method produces so many observations (thousands of pages of notes, hundreds of published pages) that it can be hard to know what to scrutinize or even what method was followed. Once there are many ethnographers working in many sites, one can struggle to locate intellectual footing.

It is hard enough to make sense of a single rich, nuanced fieldsite as it is. To compare across sites without specific points to compare is very difficult. In the back of our minds as we planned our work on the Mind and Spirit Project was the glorious failure of *The People of Rimrock* (Vogt and Albert 1966), one of an earlier generation's ambitious comparisons. The project set out to compare 'values' in five local communities around (as it happened) the Vogt family ranch in New Mexico. The book was ethnographically rich. But the research never defined 'value'—it allowed each researcher to define the term as he or she felt best—and it never defined a clear question about what social features might give rise to different values. As a result, the conclusions of the project—the work of many anthropologists over many years—were disappointingly vague. "Each piece is a gem," the American Anthropologist reviewer said with frustration, "yet the resulting mosaic lacks overall cohesion" (Graves 1967: 751).

In the Mind and Spirit project, the act of focusing in on specific findings produced by methods we knew to be limited helped us to get out of on-going debates about whether comparison was possible. For instance, as the fieldworkers were in the process of making ethnographic observations and conducting semi-structured interviews, the group began to get the impression that participants in Ghana might have had particularly vivid experiences of God's voice. But the experiences and conversations different fieldworkers were having in their different field sites were so rich and nuanced, so attuned to that particular place, that it was difficult to make a direct comparison; indeed, the whole goal of site-by-site comparison seemed dubious. When we added to this mix a specific quantitative finding—that participants

in Ghana tended to say 'yes' to more questions about God's voice than participants in other sites—we found ourselves getting down to work. We had focused discussions about how to understand this *particular* observation, taking into account both concerns about the methodology and the possibility of real differences. This focus gave rise to some very structured discussions about different representations of thoughts and their power, which in turn led to a discussion about the ontological anxiety among the US charismatics, and how it stood out against a different way of handling secular skepticism in China, and against the behavior of the Thai participants who often commented "it depends" when Felicity Aulino tried to pin them down on what they thought was real.

In the beginning, before the team did fieldwork, we had heated arguments about whether it was possible to compare the way people thought about minds. What, after all, was a "mind"? But in order to conduct site-by-site comparisons in the style of psychologists, we had to commit to shared questions in the interviews. When results came back, and the fieldworkers reconvened, we had to ask ourselves whether our findings were informative, or just artifacts of the way we asked those questions. These discussions yielded specific critiques and concrete ways to address them, feeding into a virtuous cycle of gathering observations, critiquing methods, forming interpretations, and testing those interpretations. The struggle about whether we could compare became a puzzle about what we had seen.

We have sought to think like skeptical psychologists—descending on a finding like wolves on a carcass (to use a metaphor offered by another team member, Michael Lifshitz)—in thinking about the central claim we have presented in this volume: that people who conceptualize mind as more 'porous' tend to have more frequent and more vivid spiritual experiences than people who conceptualize the mind as more 'bounded.' The fact that work in a wide range of field sites supports this general interpretation—and the fact that such a diverse group of researchers has come to any consensus about this issue at all—gives us more confidence in the claim.

Still, we continue to challenge ourselves to imagine how our data could be interpreted differently. To give an example: What if it only looks like porosity is related to spiritual experience because people whose social worlds imagine minds as porous are also social worlds in which people interrupt their sleep more often? Research finds that interrupted sleep leads to more frequent and vivid spiritual experiences; on the boundary between sleep and waking, many people report voices and visions and other remarkable events (Ohayon et al 1996). So we included a sleep questionnaire in our survey packets, and we plan to do more work to disentangle the relationship.

In short, when you have specific findings to explain, the work begins to narrow in on the puzzle.

To think of oneself as having *findings to explain* is quite different from setting out to draft the perfect ethnography. The senior author of this paper remembers her own graduate training, and the burden of the idea that she had to write everything down and put it into one book that would explain everything about those people. It was obviously an impossible task. A finding is a specific observation, made in specific circumstances, which may—or may not—be important, offer insight, be useful. You cannot hold the illusion that you are writing the final word on the subject. Instead, presenting specific finding invites a broader discussion about what sense to make of them. When you have findings, you open a conversation.

Methodological tools for promoting specificity and skepticism

Because experiments are so prima facie limited (a small pool of subjects, a single measurement) psychologists are often acutely aware that their methods (and all methods) are limited. They have developed some methodological principles that can help anthropologists who chose to borrow those tools to use them in a way that increases their confidence in what they find.

Gathering converging evidence. The use of more than one method to answer a question can be a powerful approach because the more kinds of evidence that support the claim, the more confident the researcher can be in this claim. Psychologists call this 'converging evidence': observations collected by

different methods that converge on a particular interpretation, each method providing a check on the validity of the other. In the Mind and Spirit Project, we interviewed people in depth about their spiritual experience, in part to determine how frequently people in certain settings experienced particular events. The fact that pen-and-paper surveys with large samples of different participants came up with similar rates helps us to feel more confident in the findings from interviews. When results from these different approaches converge our observations become more believable, both to ourselves and to our audiences in both fields. All methods have flaws. We have found it less anxiety-arousing and more satisfying to have data from multiple methods, each with its own limitations, rather than a single method that would have had to be—but never could have been—perfect (and equally perfect in all field sites).

To be clear, anthropologists have used mixed methods throughout the history of the field. Margaret Mead, for example, used a remarkable array: linguistic probes, drawings, experiments with new technologies like photography and film, and so forth. Still, today mixed methods are relatively rare (Hay 2015). A few decades ago, even methods classes were rare in anthropology doctoral programs. The fieldworker was imagined as a kind of sponge: one became intimately involved in the field and returned to squeeze it all out of the page. In advocating for gathering converging evidence, then, we are harkening back to this more expansive vision of how anthropologists can learn in the field.

Standard protocols: Doing things the same way every time. In a psychological experiment, the gold standard is to write a standard protocol detailing the experiment from start to finish, such that any researcher faced with any participant would conduct the experiment exactly the same way. This often includes 'scripts' to be memorized by every experimenter involved in the study, covering how to greet participants, describe the purpose of the experiment, explain what is expected during the task, and answer any questions they might raise. The protocol might specify aspects of the physical setup of the interaction, such as places where the experiment may or may not be conducted, whether lights should be on or off and doors open or closed, how the experimenter should be positioned relative to the participant, how many

inches should separate the participant's face from a computer screen, the precise location and timing of presenting any 'stimuli' to the participant, and so forth.

The goal of such a protocol is to anticipate common 'low-level' alternative explanations for some set of observations and rule these alternatives out in advance. A standard protocol addresses the following realities of human observation and interaction: (1) Individual researchers differ (e.g., Researcher A is generally friendlier; Researcher B tends to explain things more clearly); (2) Individual participants differ (e.g., Participant A is taller; Participant B is left-handed; Participant C tends to speed through written instructions when bored); and (3) The physical and social context can have large impacts on behavior (e.g., dark rooms make people sleepy; people pay attention to things that are placed directly in front of them; loud and unpredictable noises are distracting; being treated kindly makes people more cooperative). By specifying in advance an ideal version of how an experiment should unfold, experimenters hope to minimize the chances that these factors will skew the results of the study or obscure interesting findings.

We sought to use standard protocols in many aspects of the Mind and Spirit Project. Before fieldworkers left for the field, we discussed in detail how to choose comparable congregations for the charismatic Christian samples. We drew up guidelines to ensure that field workers in different sites would speak to a similar range of interlocutors in each setting within their primary field sites (e.g., setting goals for a mix of ages and genders). We spent months developing, refining, translating, and back-translating an extensive protocol for structured interviews, the goal being for each field worker to ask each of their interlocutors the same set of questions in the same order, using a similar set of strategies across individuals and across field sites to probe interlocutors' understandings of 'the mind' and their spiritual experiences. To the extent that we followed these guidelines and protocols, they are now helping us rule out a variety of alternative explanations for our observations in each field site, as well as the similarities and differences across field sites—e.g., that differences across sites are reducible to differences in the

'charismatic-ness' of different Christian samples, the age or gender of interlocutors in different sites, variations in the wording of our questions across sites; etc.

Of course, the effort to do things in the same way means that, in practice, one needs to adapt them to a particular sample—in other words, to make them superficially different in order to make them deeply similar. If, in California, you ask people about a story in which there is a man you call "John," in Shanghai you should call the man something like "Xiaofeng." A vignette that features a child playing with a pretend "choo choo" won't work in a place without trains. And things can get more complex: How can you ask Buddhists the same questions about religion that you pose to Christians? The introduction tells some part of our own wrestling with these matters. Our solution has been to use some methods that are strictly standardized and others that are more open-ended. In both cases, we sit with the results and try to work through why they came out as they did.

Fixed orders, counterbalancing and randomization: Another strategy we borrowed from psychology that we have found useful is to structure (some of) our interactions with participants in ways that minimize the risk that the results arise from bias in the method itself. Consider a situation in which a researcher wants to know if people in a particular setting tend to prefer A or B (e.g., do people at this church prefer to pray alone or with others?). A common concern among psychologists about such questions, in which a researcher asks a participant to select one of two options, is that the order of presentation of these options matters. If a researcher presents the question verbally—'Do you prefer to pray alone, or with others?'—interlocutors might be drawn to the second option because of the relative stress put on the first vs. second options (try comparing this question to 'Do you prefer to pray with others, or alone?'). Likewise, if a researcher presents the question visually—e.g., showing a picture of praying alone on the left, and a picture of praying with others on the right—interlocutors might tend to point to the picture on the side of their dominant hand (which for most will be the picture on their right). And if a researcher asks this question directly after a series of questions about the importance of

community, interlocutors might be more inclined to say they prefer praying with others than they would if the question about prayer had come first.

One response to this concern is to accept that the way a question is posed will introduce bias and ensure that bias is held constant across all participants by asking the question the same way every time. This is often a wise strategy when response options are naturally ordered (e.g., 'yesterday, today, or tomorrow' makes more sense than 'today, tomorrow, or yesterday'), or when a series of questions flows best in a certain order. In the structured interviews, the standard protocols described in the previous section included a single fixed order in which to ask questions, because the group determined that this would make these detailed, wide-ranging conversations easier to compare than if different interlocutors were asked about their experiences in different orders. The order of the questions surely influenced interlocutors' responses—but by fixing this order, we can be assured that this bias was comparably present in every interview, such that when the interviewer first asked about hearing God's voice (for example) it was in the context of a similar conversation for all interlocutors in all sites.

In other situations, however, researchers might actively try to mitigate such response biases in advance. In the previous example of asking about prayer preferences, psychologists might 'counterbalance' the order of the response options ('alone' vs. 'with others') by deciding to alternate between orders such that every second participant hears the question in the reverse order, or they might 'randomize' the order by flipping a coin to determine which to use for each participant. In the Mind and Spirit Project, we randomized the sequence of questions in many of our pen-and-paper surveys; we counterbalanced the order of surveys, experiments, and other tasks in our extended interactions with a single participant; and we alternated genders of characters in the vignettes that participants were asked to reason about or explain (to give just a few examples). These kinds of strategies ensure that roughly equal numbers of participants heard, saw, or otherwise experience our 'studies' in each of the various ways they

could unfold—helping us to feel that anything that we observe in the aggregate may be true above and beyond what happened in a particular interaction.

Stepping back, we found that using these tools associated with the psychological mindset helped us to structure our conversations and feel more confident about observations.

A parallel set of challenges in psychology

As it happens, these days psychology has its own fraught relationships with comparison and explanation.

Let us turn first to the challenge of comparison. The field of psychology has been built almost exclusively on studies with participants that are Western, educated, industrialized, rich, and democratic ('WEIRD'; Henrich, Heine, and Norenzayan 2010; see also Nielsen et al., 2017). This has remained true despite incisive critiques stretching back to the 1970s (e.g., Cole and Scribner 1974; Fiske et al. 1998; Rogoff 2003; Shweder 1995).

It is not surprising that psychologists gravitate toward what the field calls 'convenience samples' (college undergraduates and the adults and children who live near American and European universities, many of them White and middle class). They are indeed physically convenient: close to hand and relatively easy to recruit. They are also convenient in a deeper sense: The researcher and participant speak the same language, and can read each other's gestures and facial expressions; researchers have strong intuitions about what experimental probes will 'work' for these participants, and are sensitive to what questions might seem too strange, academic, or personal. In a word, psychologists, like most other people, are fluent in their own cultures, and utilize this fluency in their research with participants within those cultures.

Make no mistake: Cultural fluency has undoubtedly improved the quality of psychological research. Without it, researchers can easily ask participants to complete tasks that to them seem incomprehensible or perverse, such as filling out pen-and-paper surveys when they have never done so, or

reasoning about fictional characters in hypothetical scenarios when this kind of speculation is alien, or even discouraged, in their normal social lives (see Greenfield 1997; Heine et al. 2002; and Cohen 2007). Lacking fluency, researchers can easily misunderstand individual participants' responses to these tasks, or misinterpret general tendencies in their sample of interest—often erring in the direction of interpreting variation from the standard established in prior work as a 'deficit' (see, e.g., Foley 1997).

But psychologists are increasingly concerned about the fact that the majority of researchers and participants in the field are fluent in a single dominant culture: what might be called Euro-American middle class culture. For most of the history of experimental psychology, it was assumed—and hardly ever even stated—that people in this context are reasonable representatives of 'human nature' more broadly; that the fundamentals of cognition, development, social relationships, and affective experience are universal and can be studied without being situated in a particular social-cultural context; and that whatever varies across cultural contexts is patina, and not an appropriate subject of psychological inquiry. In recent years, more and more psychologists have publicly disputed these assumptions, and have turned to cross-cultural comparison to shed new light on continuity and variability in the human experience. However, such work has been (often justifiably) criticized for being less than rigorous (e.g., because an experimental method ends up seeming more fluent in one cultural context than another) or difficult to interpret (e.g., because researchers lack the cultural expertise to understand the similarities and differences in the responses of different groups of participants).

The second challenge, often referred to by psychologists as 'the reproducibility crisis,' has been spurred by the troubling revelation that many of the findings published in top psychology journals have been found not to replicate. That is, researchers who attempt to repeat some previously published study often fail to obtain the published result (according to some estimates, up to half of the time; Open Science Collaboration 2015). The goal of modern psychological science is to push the field's collective understanding closer to something like truth by conducting scientific studies that establish some result

and lay the foundation for further studies, in an incremental progression toward an accurate model of the world. The possibility that so many psychological results might not be replicable calls into question how much progress the field has actually made toward this goal.

There have been many suggestions for how to respond to the reproducibility crisis, including identifying and explicitly incentivizing best practices in designing and conducting studies (e.g., through new submission formats and special 'badges' appended to journal articles); making data openly available for external reanalysis and reuse; and adopting better statistical practices (e.g., Simmons, Nelson, and Simonsohn 2011; Frank and Saxe 2012; John, Loewenstein, and Prelec 2012; Simonsohn 2013; Brandt et al. 2014; Cumming 2014; Munafò et al. 2017; O. Klein et al. 2018; Wagenmakers et al. 2018). Nonetheless, the field still struggles to reconcile the goal of using empirical data to support logical, scientific explanations of human behavior with the realization that this is a much more difficult and error-prone process than many experimental psychologists once believed.

This is where a close alliance with anthropology may prove helpful.

An anthropological mindset and tools for promoting cultural fluency

Anthropologists spend a long time—months, years—in one or more cultural settings outside their academic institution during their fieldwork. They develop considerable cultural fluency in that other site, and a rich sense of that different social world rooted in such extensive experience that it is unlikely to rest on fluke observations that are wildly unrepresentative or untrue. Just as we believe that anthropologists would benefit from the psychological mindset described above, we also believe that psychologists would benefit from this anthropological mindset focused on understanding meaning through immersion in other social worlds.

Conducting multi-site research as a built-in check on repeatability. In the simplest sense, doing elsewhere the research that one usually does at home can teach one about the limitations of that research.

Anyone who has attempted to conduct cross-cultural or otherwise comparative research can attest to how much effort it takes to make sure that research is conducted the same way in each site. Preparing to conduct the studies included in the Mind and Spirit Project forced us to make any physical materials easy to re-create; to standardize our interview protocols and experimental procedures; and to be explicit about techniques that might typically be passed on by word-of-mouth (e.g., strategies for recruiting and 'warming up' participants). This provided a built-in check, early on, on which aspects of any given study were easy for a new team to implement from scratch and which might need to be specified in greater detail. Several prominent research groups in psychology have capitalized on this general insight, advocating for studies to be conducted in multiple labs in parallel to assess replicability, estimate effect size, and gauge generalizability (Frank et al. 2017; Klein et al. 2014). Conducting a study in multiple field sites is much like conducting multiple internal replications of that study, thereby improving its repeatability--the probability that another researcher could conduct the same study at a future date (regardless of its outcome). Doing so in sites outside of the US would help to solve the field's overreliance on 'WEIRD' samples.

This of course raises the issue of 'cultural fluency,' described earlier as one of the problems currently facing psychology—but as we have argued throughout this piece, cultural fluency outside of the United States and Europe is a particular specialty of anthropologists, who are uniquely suited to comparing across cultures carefully, conscientiously, and with meticulous attention to noticing and describing cultural variability with nuance. We recommend that when psychologists work abroad, they work with anthropologists who can add the cultural fluency that the project needs.

Including qualitative methods to maximize validity. We believe that psychologists would benefit from incorporating more rigorous qualitative techniques into their research, following in the footsteps of their forbearers (for example, William James and Jean Piaget) and drawing on qualitative research to identify important areas of research, form hypotheses, design 'ecologically valid' studies (i.e., studies that

resemble familiar situations from participants' everyday life), and guide the interpretations of results. The advantage of a highly structured method (for instance, a formal survey) is that the intervention is identical everywhere—at least in principle. Its disadvantage, of course, is that, in practice, the intervention may be interpreted quite differently in different contexts. We chose to combine highly structured methods with qualitative methods that more accurately assessed local meaning. (For discussions about the risks of structured methods without qualitative methods, particularly in international settings, see Gaskins 2018, Greenfield 1997, Hay 2015. Kline, Shamsudhan and Broesch 2018, Tiokhin, Harkman and Hruschka 2018).

Psychologists already collect and interpret qualitative data in private (see Gelman 2018 for recent remarks to this effect from a prominent statistician and critic of psychological research); as they push toward openness and honesty in their methods, data, and analyses, they should also include more of the qualitative observations that help them to formulate their studies and interpret their findings. Psychologists might even incorporate more of the modern qualitative methods developed by anthropologists and others to improve how they go about deciding what to study, how to study it, and what to make of the results. In these ways, including qualitative methods in the modern psychology toolkit could help psychologists move toward theories and conclusions that meet increasingly high standards of replicability and rigor.

Working as a team

In November of 2018—a few months after the Mind and Spirit team members had moved on to their next projects and positions—we reconvened for a two-day retreat to reconnect, update each other on ongoing research and writing, and so forth. One afternoon we spent an hour or so mulling over the questions 'What can psychology learn from anthropology?' and 'What can anthropology learn from psychology?' Many of those lessons appear in this paper.

Our discussion also sparked a shared realization that much of what we as a team have taken away from the Mind and Spirit Project is neither 'from anthropology' nor 'from psychology,' but from the alchemy of combining the two fields. There were many times in this conversation when someone pointed to a habit we had adopted in our weekly conversations, a technique we had used in designing our structured interviews, or a way of describing quantitative results as an example of the influence of one field or the other, just to be rejected by representatives from that field ('That's not psychology—I thought it was anthropology!'; 'I thought *you* were the one who suggested that!').

Of course, there are aspects of this project that we recognize as 'pure' anthropology (e.g., each fieldworker's ethnographic observations of his or her primary field site) or 'pure' psychology (e.g., the administration of pen-and-paper surveys to undergraduates in all sites). As we have argued in this piece and as many others have argued before us, we see value in this use of multiple methods. We would add that an approach that emphasizes *converging* methods hinges on maintaining a certain amount of distance between methods, relying a division of labor between researchers with different areas of expertise and theoretical bents. The primary thesis of the current paper is that anthropologists and psychologists, in particular, have complementary skillsets that, when used together, can improve our research.

Yet not all of our work on this project has been 'multi-disciplinary' in this sense. Some of our primary methods have turned out to be hybrids of anthropological and psychological techniques. For example, the long-form interviews about 'thinking about thinking' and 'spiritual experience' that informed all of the pieces in this special issue were more structured and standardized than most anthropological methods, and more open-ended and less standardized than most psychological methods; they were designed to yield a mix of qualitative and quantitative data. Many of the conversations we have amongst ourselves are conversations that would not occur in any of our home departments. The Mind and Spirit Project has yielded methodological and theoretical insights that we anticipating carrying with us well into the future, both in our ongoing collaborations with each other and in our independent projects.

Many of these insights emerged from working as a team. When we state this bluntly, it seems obvious (even saccharine). It is hard however to convey how differently the intellectual work proceeds when the work is not done at a single person's desk but instead in an on-going group debate. We invested a good deal of time in interaction. For months at Stanford we met in twice-weekly meetings in person and over frequent 'salon'-style dinners at Luhrmann's home. When fieldworkers were scattered around the world we held a weekly video-chat (with some of the team waking up at dawn and others struggling to stay awake past midnight). We hosted workshops with leaders in the fields of anthropology, psychology, religious studies, and philosophy, and went on a weeklong retreat together to the Esalen Institute (where we met up with another team with similar interests, led by Jeffrey Kripal). These experiences cemented our professional relationships into deep intellectual collaborations and meaningful personal connections.

This was not the same as a lab meeting or a writing group, where individuals bring their own projects to a larger group to get 'external feedback.' Rather, we made research plans together, tried to explain to each other what we thought we were seeing even as we were still squinting to see it, sorted through our 'data' together, and pondered at length (and not without heated debate) how best to make sense of the meaning of our 'findings.'

Such an extended, intensive collaboration is a transformative process, rendered particularly powerful when it includes members of other disciplines. This is because when people in other fields encounter each other's work, they ask questions so basic that the researcher might not usually consider: *Does the study measure what it is intended to? Is that question reasonable? How do you know what you have seen?* Of course such questions can come from within one's own field as well. But it can be even more startling and productive to interact with someone from a discipline that thinks differently about evidence—in which what even counts as evidence is different---and to justify one's questions and research to them.

In our view, this is one of the primary benefits of collaborative, comparative work: Each fieldworker's interpretations of observations from his or her primary field site offer a check on the observations and interpretations arising from the other field sites, and each researcher's attempts to study a phenomenon using the mindset and methods from his or her primary discipline offer a counterpoint to the studies emerging from the other disciplines. Reports from each field site and data from each method generate alternative explanations for other sites and methods, leading everyone to reevaluate their assumptions and try out new interpretations of their own observations. This comparative process enables us to refine our descriptions of the nuances of particular places and people, while at the same time identifying points of commonality across cultural contexts and scholarly perspectives—both of which inform our emerging general theory.

Orienting ourselves towards generality and truth

We would like to close by briefly considering the following question: What are we after with our observations, interviews, surveys, and experiments?

For the two authors of this paper, the answer is that we aim to draw conclusions that are both *true* and *general*—'true' in the sense of describing with some degree of accuracy a real phenomenon in the world, and 'general' in the sense of applying not just to one person, or one small group of people, but to humans. At the same time, we consider these goals to be impossible to achieve—not just difficult, but impossible; and not just impossible for a single project, but impossible for an entire research career, even an entire discipline. After all, human observation is limited, while human experience is infinite. "True" is a complicated word. The observations we have made in the Mind and Spirit Project are descriptions of specific individuals, in specific places and times, observed by a group of researchers using a particular set of mindsets and methods. We think that our observations are accurate—within limits. There are surely aspects of our participants' experiences that we have interpreted incorrectly, or failed to see altogether. Such is the nature of human exchange.

In this sense, our claims about theory of mind and spiritual experience are, in some important sense, untrue. Nonetheless, we intend these claims to be *oriented toward* generality and truth—to point in the direction of underexplored aspects of the reality of human experience. For the two of us, at least, this work has affirmed our commitment to tilting at the windmills of comparison and explanation, in hopes that others will take note, inform us of our errors, and push us to do better.

Acknowledgements: Special thanks to Cristine Legare, Nicole Ross-Zehnder, Michael Lifshitz, Ciara Wirth, Hazel Markus, Thomas Weisner, Suzanne Gaskins, and the rest of the Mind and Spirit team, including the authors of the pieces in this special issue and the research coordinators, research assistants, data collectors, translators, and participants in each field site and at our home base at Stanford University. Thanks to Sophie Bridgers, and also to Nick Long and several anonymous reviewers for comments on earlier versions of this paper. In addition to the John Templeton Foundation, Weisman's participation in this project was supported by the National Science Foundation Graduate Research Fellowship Program under Grant DGE-114747 and by a William R. and Sara Hart Kimball Stanford Graduate Fellowship.

References

Astuti, R. and P. Harris. 2008. Understanding mortality and the life of the ancestors in rural Madagascar, *Cognitive Science*, 32, 1, pp. 1-29.

Astuti, R., G.E.A. Solomon and S. Carey. 2004. Constraints on conceptual development: A case study of the acquisition of folkbiological and folksociological knowledge in Madagascar. *Monographs of the Society for Research in Child Development*, 69(3):1-135, vii-viii; discussion 136-61.

Astuti, R., J. Parry and C. Stafford. 2007. *Questions of Anthropology*. (LSE Monographs.) London: Berg.

Behar, R. 1996. *The Vulnerable Observer*. Boston: Beacon.

Bloch, M. 2005. *Essays on Cultural Transmission*. (LSE Monographs.) London: Berg.

Borofsky, R. 2019. *Why a Public Anthropology?* Honolulu: Borofsky.

Borofsky, R., L. Nader, M. Candea and J. Friedman 2019. "Where Have All the Comparisons Gone?." Member Voices, *Fieldsights*, September 10. https://culanth.org/fieldsights/series/comparison

Brandt, M. J., H. Ijzerman, A. Dijksterhuis, et al. 2014. The Replication Recipe: What Makes for a Convincing Replication? *Journal of Experimental Social Psychology* 50, 217–224.

Candea, M. 2018. *Comparison in Anthropology: The Impossible Method*. Cambridge: Cambridge University Press.

Clifford, J. and G. Marcus, eds. *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California.

Cohen, D. 2007. Methods in Cultural Psychology. *Handbook of Cultural Psychology* 196–236.

Cole, M. and S. Scribner 1974. *Culture and Thought: A Psychological Introduction*. Oxford, England: John Wiley and Sons.

Crocker, C. I977a. My Brother the Parrot. In D. Sapir (Ed.), *The Social Use of Metaphor*. Philadelphia: University of Pennsylvania

Cumming, G. 2014. The New Statistics: Why and How. *Psychological Science* 25, 7–29.

Descola, P. 2013. *Beyond Nature and Culture*. Chicago: University of Chicago.

Diamond, J. 1997. *Guns, Germs and Steel*. New York: Norton.

Duncan, G., A. Huston and T. Weisner. 2008. *Higher Ground: New Hope for the Working Poor and their Children*. New York: Russell Sage.

Fiske, A. P., S. Kitayama, H. R. Markus and R. E. Nisbett 1998. The Cultural Matrix of Social Psychology. In D. T. Gilbert, S. T. Fiske and G. Lindzey (Eds.), *Handbook of Social Psychology* , 915–981. (4th Edition). New York, NY: Mcgraw-Hill.

Foley, D. (1997). Deficit Thinking Models Based On Culture: The Anthropological Protest. In R. Valencia (Ed.), In *The Evolution Of Deficit Thinking: Educational Thought And Practice*. Washington, DC: Falmer Press.

Frank, M. C. and R. Saxe 2012. Teaching Replication. *Perspectives On Psychological Science* 7, 600–604.

Frank, M. C., E. Bergelson, C. Bergmann, et al. 2017. A Collaborative Approach to Infant Research: Promoting Reproducibility, Best Practices, and Theory-Building. *Infancy* 22, 1–26.

Gaskins, S., M. Beeghly, K. A. Bard, A. Gernhardt, C. H. Liu, D. M. Teti, R. A. Thompson, T. S. Weisner, and R. D. Yovsi. Meaning and Methods in the Study and Assessment of Attachment. In H. Keller & Bard, K.A. (Eds.), *The Cultural Nature Of Attachment: Contextualizing Relationships and Development*. Cambridge: MIT Press.

Geertz, C. 1973. *The Interpretation of Cultures*. New York: Basic Books.

Gelman, A. 2018. What Is the Role of Qualitative Methods in Addressing Issues of Replicability, Reproducibility, and Rigor? [Blog Post] (Available On-Line:Http://Andrewgelman.Com/2018/06/19/Role-Qualitative-Methods-Addressing-Issues-Replicability-Reproducibility-Rigor/, Accessed 19 June 2018).

Graeber, D. 2015. Radical alterity is just another way of saying 'reality': A reply to Eduardo Viveiros de Castro. *Hau: Journal of Ethnographic Theory* 5 (2): 1–41.

Graves, Theodore. 1967. People of Rimrock, Review. American Anthropologist. 69: 751-2.

Greenfield, P. 1997a. Culture as Process: Empirical Methods for Cultural Psychology. In J. W. Berry, Poortinga, Y. H., and Pandey, J. (Eds.), *Handbook of Cross-Cultural Psychology* (Eds). Boston, MA: Allyn and Bacon.

Greenfield, P. 1997b. You can't take it with you. *American Psychologist*, October 1997, Vol. 52, No. 10, 1115-1124.

Hay, M. C., ed. 2016. *Methods that Matter*. Chicago: University of Chicago Press.

Heine, S. J., D. R. Lehman, K. Peng and J. Greenholtz 2002. What's Wrong With Cross-Cultural Comparisons of Subjective Likert Scales?: The Reference Group Effect. *Journal of Personality and Social Psychology* 82, 903–918.

Henrich, J., S. J. Heine and A. Norenzayan 2010. The Weirdest People in the World? *The Behavioral and Brain Sciences* 33, 61-83; Discussion 83-135.

Hinton, D., V. Pich, L. Marques, A. Nickerson, and M. H. Pollack 2010. Khyaˆl Attacks: A Key Idiom of Distress Among Traumatized Cambodia Refugees. *Culture Medicine and Psychiatry* 34:244–278.

Jessee, N., K. K. Collum, and R. D. Schulterbrandt Gragg 2015. Community-based Participatory Research: Challenging 'Lone Ethnographer' Anthropology in the Community and the Classroom. *Practicing Anthropology*: Fall 2015, Vol. 37, No. 4, pp. 9-13.

John, L. K., G. Loewenstein and D. Prelec 2012. Measuring the Prevalence of Questionable Research Practices With Incentives for Truth Telling. *Psychological Science* 23, 524–532.

Kirmayer, L., R. Lemelson, and C. Cummings, eds. 2015. *Re-Visioning Psychiatry*. Cambridge: Cambridge University Press.

Kline, M. A., Shamsudheen, R., & Broesch, T. (2018). Varia3on is the universal: making cultural evolu3on work in developmental psychology. Phil. Trans. R. Soc. B, 373(1743), 20170059.

Klein, O., T. E. Hardwicke, F. Aust, et al. 2018. A Practical Guide for Transparency in Psychological Science. *Collabra: Psychology*.

Klein, R. A., K. A. Ratliff, M. Vianello, et al. 2014. Investigating Variation in Replicability: A 'Many Labs' Replication Project. *Social Psychology* 45, 142–152.

Laidlaw, J. 2012. Ontologically Challenged: A Review of M. Pederson, Not Quite Shamans. *Anthropology of this Century*, Issue 4.

Lavie, S. 1990. *The Poetics of Military Occupation*. Berkeley: University of California.

Marcus, G and M. Fischer. 1986. *Anthropology as Cultural Critique*. Chicago: University of Chicago.

Matria, A. 2018. Labors of Love: Domestic Violence, Care, and the Family in Kolkata, India. PhD dissertation. Department of Anthropology, Stanford University.

Miller, Daniel, Elisabetta Costa, Laura Haapio-Kirk, Nell Haynes, Jolynna Sinanan, Tom McDonald, Razvan Nicolescu, Juliano Spyer, Shriram Venkatraman and Xinyuan Wang. 2019. Contemporary Comparative Anthropology—the Why We Post Project. Ethnos. 84:2, 283-300

Minow, H., R. Shweder, and H. Markus, eds. 2008. Just Schools: pursuing Equality in Societies of Difference. New York: Russell Sage.

Munafò, M. R., B. A. Nosek, D. V. M. Bishop, et al. 2017. A Manifesto for Reproducible Science. *Nature Human Behaviour* 1, 1–9.

Nielsen, M., D. Haun, J. Kärtner and C. H. Legare 2017. The Persistent Sampling Bias in Developmental Psychology: A Call to Action. *Journal of Experimental Child Psychology* 162, 31–38.

Norenzayan, A., A. F. Shariff, W. M. Gervais, A. Willard, R. McNamara, E. Slingerland, and J. Henrich 2016. Parochial prosocial religions: Historical and contemporary evidence for a cultural evolutionary process. *Behavioral and Brain Sciences* 39: 43-65.

Ohayon, M., R.G. Priest, M. Caulet and C.. Guilleminaut, Hypnagogic and Hypnapompic Hallucinations: Pathological Phenomena? British Journal of Psychiatry. 169: 459-467.

Open Science Collaboration 2015. Estimating the Reproducibility of Psychological Science. *Science* 349.

Robbins, J. 2013. Beyond the Suffering Subject: Toward an Anthropology of the Good. Journal of the Royal Anthropological Association. Volume 19, Issue 3, pages 447–462, September 2013).

Rogoff, B. 2003. *The Cultural Nature of Human Development*. New York, NY: Oxford University Press.

Ross, A., K.P. Sherman, J. Snodgrass, H. D. Delcore, R. Sherman. 2011. *Indigenous People and Collaborative Stewardship of Nature*. Walnut, CA: Left Coast Press.

Scheidel, Walter. 2017. *The Great Leveler: Violence and The History Of Inequality From The Stone Age to the Twenty-First Century*, Princeton University Press: Princeton

Schnegg, M. 2014. Anthropology and Comparison: Methodological Challenges and Tentative Solutions." Zeitschrift fur Ethnologie 139(1): 55-72.

Shweder, R. 1995. Cultural Psychology - What Is It? *The Culture and Psychology Reader*, 41–86.

Simmons, J. P., L. D. Nelson and U. Simonsohn 2011. False-Positive Psychology: Undisclosed Flexibility in Data Collection and Analysis Allows Presenting Anything as Significant. *Psychological Science* 22, 1359–1366.

Simonsohn, U. 2013. Small Telescopes: Detectability and the Evaluation of Replication Results. *Social Science Research Network* 1–31.

Tiokhin, L., Hackman, J., & Hruschka, D. (2018). Why replica3on is not enough: Insights from a cross-cultural study of social discoun3ng. PsyArXiv

Van der Veer, P. 2016. *The Value of Comparison*. Chapel Hill, N.C.: Duke University Press.

Vilaca, A. 2005. Chronically Unstable Bodies: Reflections on Amazonian Corporalities. *Journal of the Royal Anthropological Institute* (N.S.): II: 445-464.

Viveiros de Castro, E. 2004. Exchanging Perspectives: The Transformation of Objects into Subjects in American Ontologies. *Common Knowledge*, Volume 10, Issue 3, Fall 2004, pp. 463-484.

Vogt, E.Z. and E.A. Albert, eds. 1066. *The People of Rimrock*. Cambridge: Harvard University Press.

Wagenmakers, E. J., M. Marsman, T. Jamil, et al. 2018. Bayesian Inference for Psychology. Part I: Theoretical Advantages and Practical Ramifications. *Psychonomic Bulletin and Review* 25, 35–57.

Weisman, K., C. S. Dweck and E. M. Markman 2017. Rethinking People's Conceptions of Mental Life. *Proceedings of the National Academy of Sciences* 114, 11374–11379.

Yanagisako, S. 2007 [unpublished]. Comparative Ambivalence.