Integrating Two Epistemological Goals: Why Shouldn't We Give It Another Chance?

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Abstract

As Beller, Bender, and Medin (in press) pointed out in their target article, in the contemporary study of culture in psychology, anthropology is virtually invisible. In this commentary, I traced this invisibility to a root conflict in epistemological goals of the two disciplines: Whereas anthropologists value rich description of specific cultures, psychologists aspire to achieve theoretical simplicity. To anthropologists, then, to understand culture is to articulate symbolic systems that are at work in a given location at a given time. In contrast, to psychologists, to understand culture amounts to identifying socio-cultural variables that moderate psychological effects. These divergent epistemological goals dictate both theoretical perspectives and empirical approaches in both disciplines. Yet, the two goals are both valid and in fact complementary. A renewed effort toward integration of the two goals may enrich both disciplines.

Keywords: Culture; Psychology; Anthropology; Epistemological goals

1. Anthropology: Important yet deeply irrelevant?

I started out my own career as a cultural psychologist nearly 20 years ago when Hazel Markus and I published a paper on culture and the self (Markus & Kitayama, 1991). During several years preceding this publication, I immersed myself in a number of writings by social scientists, including a host of anthropologists. I believe that this period of incubation was absolutely necessary for my own intellectual development.

Perhaps, it should not come as any surprise that unlike most psychologists, my own theoretical orientation is quite holistic and contextual. I have always hesitated packing everything in the head. I was, and still am, quite skeptical of information processing views, which would posit that the cognitive system can make up very elaborate internal representations

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from an impinging stimulus through a series of cognitive transformations. Instead, I believe that much of what we would care to know as students of culture and social behavior lies in the nature of the social reality that surrounds the social actor. In this sense, I may call myself a ''Gibsonian.''

As a ''Gibsonian'' in this sense, I have preferred to theorize on any given phenomenon, whether it be self-esteem, happiness, or cognitive style, not as purely internal or psychological but as socially or situationally enabled and reinforced. Following Gibson, I coined the term ''cultural affordances'' in an effort to capture the idea that cultural context has the potential to elicit certain behaviors while inhibiting some others (Kitayama & Markus, 1999). For example, we suggested that it is much easier to ''feel great!'' in the United States than in Japan because US cultural contexts afford such ''great feelings'' in ways Japanese cultural contexts rarely do. In other words, ''good feelings'' are *collectively constructed* (Kitayama, Markus, Matsumoto, & Norasakkunit, 1997).

I believe that the symbolic system that surrounds the social actor is ''really real'' in the sense that the system is externalized and embodied in various artifacts, practices, and institutions (Kitayama & Uskul, 2011). Further, this symbolic system is closely attuned to and coordinated with behavioral patterns of others who interact with the person. This complex of the subjective and the objective, or the personal and the social, constitutes the cultural environment (Markus, Mullally, & Kitayama, 1997). This way of theorizing reflects my own interpretation of what I learned from the social science readings during the early years in my own career.

After two decades since then, I still believe that the insight I had gleaned from anthropological writings earlier on is crucial. At the same time, however, I also agree with many of the provocative points Beller, Bender, and Medin (BBM) make to show why and how anthropology—particularly, cultural anthropology—has become increasingly irrelevant in the study of culture in psychology.[1] I now rarely cite anthropological studies when writing up my empirical studies, virtually all of which go to psychology journals and, more recently, to neuroscience journals. Empirical papers I do for, say, *Psychological Science* (e.g., Na & Kitayama, 2011), are too short to cite many, if not *any*, basic or big idea pieces. Although ethnographies are quite useful in informing any research project on culture, they are rarely required, or helpful, in building a theoretical case for the project.

Furthermore, to the psychologist's eyes, anthropologists often seem very critical to the point where their skepticism and criticism is counterproductive. Many of them take pride in this fact by calling their trade ''critical studies.'' Some of them, for example, complain that theories in psychology ignore fundamental power inequalities of the society and how the society seeks to cover them up. By calling their comment critical, they appear to imply that others, say, psychologists, are superficial and uncritical. Some others point out that cultural categories are loose and dynamic. They do so as if they know how to analyze culture without clearly defining such categories. And another one of favorite anthropological criticisms leveled against psychologists is that psychology stuff would never generalize beyond college student samples. This criticism is fair enough. However, isn't it more productive to discuss a matter like this as something that must be decided on some empirical ground? ''In-principle'' arguments, which are all too frequent in critiques by the

''critically minded'' folks, do not seem worthy of careful attention because they ignore the ''real work'' involved in empirically addressing any of the questions they can spill out. They whine—so it appears to the ears of many psychologists—without offering anything positive and constructive.

Although *deconstruction*—an effort to identify component processes underlying a seemingly natural and ordinary category or effect—is useful and can reasonably be placed at the center of ''critical'' approaches to anything, we should be mindful that it is just one step away from *destruction*. Often a lot of mileage can be gained with simplified working assumptions. The practice of deconstructing these assumptions, if exercised with excessive zeal and pride, could easily destroy an important seed for promising empirical work.

Over the years, I have found it very sad to witness my fellow anthropologists having become too critical to the point where they begin denouncing the central construct of their field, that is, the concept of culture. The construct of culture is considered analytically useless (supposedly because it does not explain anything), descriptively inaccurate (supposedly because it gives an impression of homogeneity that does not exist), and politically incorrect (supposedly because it ignores subjectivity of the powerless). It is one thing to point out some drawbacks in any given construct. It is quite another to ''denounce'' and ''abolish'' it. Doing so seems rather dogmatic and political especially if it is motivated, not by any data, but by some sort of a priori principles or political commitments. I cannot help but feeling a déjà vu of what happened on earlier days in psychology: behaviorists—Skinnerians, in particular—denounced and abandoned, under the banner of science, any inquiry into what the discipline was meant to investigate, namely, consciousness and subjectivity. I believe that the current skepticism of culture in anthropology is excessive and, in fact, it is as counterproductive to the study of culture as behaviorism was to the study of human mind.

So in the end, many psychologists would sigh and ask, ''Who cares?'' The irony here is that whereas anthropology, as a way of thinking about humans, was absolutely necessary and instrumental in forming my own scholarly orientation, it no longer seems relevant to my empirical studies. The irrelevance of anthropology is deep and entrenched in multiple factors. It is ingrained not just in each psychologist's way of thinking but also in practices and policies, both formal and informal, of our field. As BBM noted, there are many reasons for this state of affairs. But there is one point that would require further articulation: The two disciplines have very different priorities in their respective intellectual agendas. That is, epistemological goals of the two disciplines are very different (Challenge No. 3 of BBM). In the remainder of this commentary, I would like to focus on this point.

2. Two epistemological goals: Descriptive richness and theoretical simplicity

Nearly 30 years ago, sitting in the office of my adviser, Bob Zajonc, I was explaining my brilliant new ideas to him. After 10–20 minutes, Bob slowly opened his eyes and told me with compassion and sympathy fully expressed on his face, ''Shinobu, that's too complicated.'' I learned many things in graduate school, but this one struck me hard and has since stayed in my heart forever. To make it work, your idea must be simple. This is not to say

that reality is also simple. To the contrary, reality can be, and is in fact, quite complex. But by describing the complex as complex, no real progress can be claimed. At worst, you may just be wasting your time if you ''merely'' describe the complex as such. If you are a scholar of any intellectual merit and prowess, you have to see through the complex and find a simple principle that operates underneath. This Platonic vision or what some might call the essentialist epistemology permeates psychology and, one might add, that's what distinguishes science from everything else (e.g., poetry, literature, etc.) as a way of knowing.

In part because we—psychologists—adopt this cognitive heuristic, our field relies on experimentation in building empirical knowledge. In designing an experiment, one has to think carefully about what will happen as a function of a manipulated variable *if everything else is equal*. Of course, everything will never be equal in real life. So one might wonder if the experiment is artificial and contrived. And, yes, sometimes the experimental manipulation can make the situation less than natural or not sufficiently valid ecologically. But experimentation gives us a great benefit of identifying the causal effect of the manipulated variable on what is being measured as a dependent variable. Through experimental manipulation, as well as through random assignment of participants into the different manipulated conditions, everything else does become equal, enabling the researcher to make strong inferences about causality.

The success of cultural psychology of the last two decades is really not due to the ease of collecting data with questionnaires that can be administered cheaply by colleagues ''over there'' as BBM claim. Although there are many questionnaire studies to be sure, they did always exist even before. The success of cultural psychology in the last two decades may better be attributed to careful experimentation and application of this method to theoretical questions of the interface between culture and the mind. To run a good experiment is not ''cheap.'' It in fact involves substantial effort and working knowledge of the culture in which the study is carried out. Here, careful ethnographic knowledge is indispensable, as I shall argue later.

One key insight that has emerged from this effort is that the same experimental manipulation can have very different effects on a behavior under study. For example, Cohen and colleagues show that when insulted, Southerners of the United States tend to be highly aggressive, as indicated by a variety of behavioral and physiological indicators of aggression (Cohen, Nisbett, Bowdle, & Schwarz, 1996). In contrast, Northerners show no such tendencies as a function of insult. If nothing else, they tend to show an opposite tendency of becoming less aggressive after the insult. Kashima, Halloran, Yuki, and Kashima (2004) found that when a thought of the fatal destruction of one's nation is made salient, Japanese become more collectivistic. In contrast, a thought of the death of one's body makes Australians more individualistic. Heine et al. (2001) have found that whereas Americans work much harder on a task if they have succeeded in a similar (but different) task than if they have failed, Japanese show a diametrically opposite effect, exhibiting a greater motivation to work on the second task after failure than after success.

These findings are powerful because what appears to be the same event (e.g., insult, mortality salience, success or failure) has very different meanings, thus producing correspondingly different psychological consequences. The difference between Southerners and

Northerners in the United States will be very hard to understand without postulating that Southerners have a strong tendency to protect one's honor by responding to an insult with violent aggression. The cultural difference in the effect of mortality salience suggests that the worldviews strongly held among Japanese versus Australians are very different on the individualism/collectivism dimension. And, finally, the cultural difference in the response to success and failure illustrates the underlying difference in achievement-related motivations. Whereas Americans seek to achieve positive goals and, thus, the increased likelihood of success is an important motivator, Japanese typically seek to avoid negative outcomes and, thus, the increased likelihood of failure motivates them more.

By conducting an experiment that carefully manipulates some select variables one can make strong inferences about the nature of the cultural context in which the manipulated variables are embedded and allowed to operate. Often times, participants themselves have no idea what effects any given experimental variable might have on their own behaviors. Only through careful experimentation can one successfully distill the nature of culture, which would otherwise be largely masked and extremely difficult to uncover with methods such as field observations, interviews, and surveys alone. Of course, all these methods are useful. But it is the foregoing feature of experimentation that produced a substantial impact on the study of culture over the last two decades. And the ability or willingness to see through reality and distill conceptual variables that are "hidden" behind it is indispensable for a good experimentalist. If his or her idea is "too complicated" it will probably never work in this business.

Anthropology has a very different set of epistemological goals. As emphasized by Clifford Geertz (1973), "thick description" is at the heart of ethnographic work. All students of anthropology are trained to describe meanings and practices that constitute the lived world of the individual in a specific cultural context such that, once the context is fully understood and articulated from this person's perspective, his or her behavior will come to make better sense and perhaps seem "familiar" even though it might seem strange and senseless at first glance.

One important reason for the epistemological goal of providing a rich and detailed account of cultural meanings and practices may stem from the fact that anthropologists have traditionally studied societies that are very different from Western societies where a vast majority of both anthropologists and readers of their ethnographies live. These societies may be organized by principles that are unheard of in Western societies. Thus, a detailed account of how they work can be quite useful and informative.

But more important, ethnographic work has been crucial in reviving the very notion of culture and providing a detailed account of what it is and what it does. As Shweder (2003) noted, "in the history of twentieth-century anthropological thought, the idea of culture has been variously defined, either behaviorally (as patterns of behavior that are learned and passed on from generation to generation) or symbolically (as the categories, beliefs, and doctrines that organize, rationalize and justify a way of life)" (p. 31). Culture is not purely psychological and internal because it is externalized in overt behavioral patterns. At the same time, culture is also not purely behavioral or external, either: It must be made meaningful in reference to certain cognitive schemas, categories, and folk theories. I would take

it for granted that to understand the nature of this psycho-behavioral complex of culture one will have to look closely into the way in which people behave in a given setting and relate it to a set of meaning-producing categories, folk theories, and other ideas. Descriptions of culture, then, are bound to be highly situation-specific and can be extremely detailed. Necessarily, superb anthropological work would require rich or thick description of the community under study.

3. How can we learn from one another?

The two epistemological goals are quite different indeed. Whereas anthropologists seek richness in description, psychologists aspire to achieve simplicity in explanation. Whereas anthropologists use field observations to enrich their description and make it even richer and thicker, psychologists make heavy use of controlled experimentation to test their hypotheses. These differences entail a host of other differences in practice and even institution. For example, anthropologists value monographs—who can provide any sufficiently rich and thick description of this or that village within the word limit of any scholarly journals? In contrast, their psychological colleagues value peer-reviewed journal articles: If the idea is good, should not it be possible to state it in crystal clear, concise terms and to be tested in an elegant experiment? While monographs are considered important, they typically provide just a compilation of the work already reported in journals. In reality, all this is exacerbated by the tenure system of US higher-education systems, as BBM correctly pointed out.

This division of labor that has thus resulted creates two social entities or belief-practice complexes that would seem mutually incompatible. While anthropologists accuse psychologists of over-simplification, over-generalization, and superficial interpretation, psychologists counter that ethnographies are no more than so-so stories that cannot be tested or, worse, just a set of idiosyncratic, self-congratulatory statements of half-baked political ideology: After all, just because a description is rich or thick would not guarantee that it is also accurate or true. So the antagonism between the two disciplines would appear quite serious and hard to overcome.

But is this antagonism necessary or even inevitable? Isn't it possible to seek some communication while acknowledging both strengths and weaknesses of both sides? Indeed, when the belief-practice complexes of the two disciplines are analyzed to their respective cores (excuse me for this essentialism—I am still a psychologist!), there are two mutually complementary epistemological goals. Indeed, as long as we confine our analysis to these goals, I believe that a good case can be made that both perspectives are equally valuable and necessary to carry out good scholarly work on cultural influences on psychological processes.

Let's consider a research project on culture with the aim of understanding how culture might work to influence psychological processes. The first obvious place to start is to learn something about the cultural context under study. We will have to know what people think and do in daily life. If we are interested in the self, for example, it will be important to know, say: (a) What the self might mean to the people under scrutiny; (b) how that meaning

might differ from the meaning that is more typical in the researcher's own culture; (c) what terms they use in communicating different aspects of the self; and (d) how the self and constructs related to it such as self-esteem and honor might be used to coordinate daily social interactions. Necessarily we will have to start with careful observations of the culture at hand. This is what ethnography does at its best.

However, once a hypothesis is generated, it must be articulated in a concise, testable statement. For example, the self in Asian societies is much more communal or interdependent and, in part because of this, Asians are often self-effacing or self-critical. In order to test a hypothesis like this, one will have to operationally define interdependence and self-effacement or self-criticism. Care must be taken to make sure that the operationalization is relatively easy to implement within a certain conventional psychological task while not compromising its ability to speak to the original observation. This can probably be done in close collaboration between anthropologists who offer relevant ethnographic observations and psychologists who choose and fine-tune the task to measure the construct.

Once the hypothesis is tested and the results are positive, the next step would be to articulate causal mechanisms underlying the phenomenon at issue. Is it really true that self-effacement is a means for interdependence? If so, for example, will the effect disappear once the need for interdependence is minimized by a certain experimental manipulation? Or is it weaker for those with personality traits (e.g., sensational seeking) that supposedly predispose them less toward interdependence? Or how automatic or deliberate is the self-effacement or self-criticism? This list can go on and on. This laborious process of articulating the phenomenon and specifying causal mechanisms underlying it is at the heart of psychological inquiry. Through this work, in conjunction with the original, relatively simple-minded overarching hypothesis, one may hope to be able to establish the internal validity of the phenomenon under study.

This, however, can hardly be the end of the inquiry on ''self-effacement'' in Asia. The causal mechanisms that are established in the laboratory will have to be brought back to the real world so that the researchers can see if these mechanisms might actually work in the ''outside.'' Only through this work of applying the psychological theory or hypothesis to real-world cases will one be able to establish the external validity of the hypothesis. Through this process, one may have to refine the original hypothesis, thereby initiating a new cycle of inquiry on the same phenomenon. It is to be hoped that our understanding of the phenomenon at issue will be deeper and more accurate every time we go through this cycle.

This research cycle is illustrated in Fig. 1. Whereas the ethnographic method of anthropologists is indispensable in hypothesis generation as well as in application of a well-tested hypothesis to real-world cases (which, together, would ensure the external validity), the experimental method of psychologists is instrumental in hypothesis testing as well as in further articulations of causal mechanisms (which would ensure the internal validity). The most important point of this figure, however, is that both approaches are necessary for a fuller understanding of all forms of culture-mind interaction.

This brings me back to BBM's courageous effort to articulate the current issues and barriers against such integration. On many points and issues discussed, I cannot help but fully
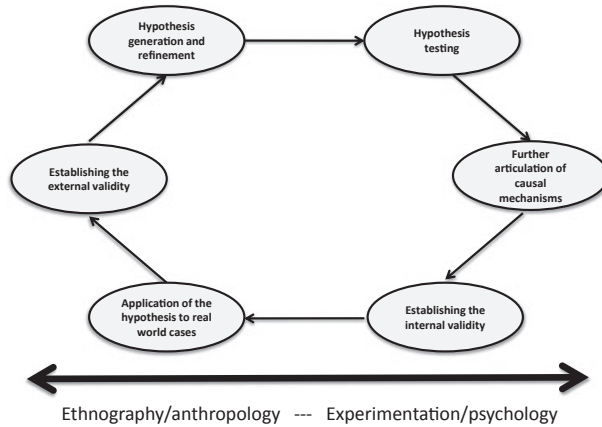
Fig. 1. A full cycle of investigation of the culture-mind interface: Whereas the ethnographic method of anthropologists is indispensable in hypothesis generation, application of a well-tested hypothesis to real-world cases, thereby establishing the external validity, the experimental method of psychologists is instrumental in hypothesis testing as well as in further articulation of causal mechanism, thereby establishing the internal validity. Both approaches are necessary for a fuller understanding of any form of culture-mind interaction.

endorse their arguments and observations. Nevertheless, I also feel compelled to make a vote of dissent and ask, ''Why shouldn't we give it another chance?''—that is, another chance to the effort toward cross-fertilization between the two approaches to the study of culture. This voice of dissent would seem quite appropriate on this particular occasion, in as much as if such cross-fertilization was to be sustainable, it would require an explicitly and intentionally cross-disciplinary intellectual forum—a forum of the sort Cognitive Science is designed to be.

Note

1. Cognitive anthropologists, in contrast, often carry out experiments in the field.

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