

## **Is Interviewing Compatible with the Dual-Process Model of Culture?**

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(Comment on Allison Pugh, "What Good Are Interviews for Thinking About Culture?")

What role should interviews play in the study of culture? The in-depth, semi-structured interview has been the workhorse of cultural sociology since the "cultural turn" of the 1980s (see, e.g., Lamont 1992, 2000; Swidler 2001). In the past few years, however, some cultural sociologists have drawn on cognitive science to criticize this approach, arguing that interviews cannot access the non-discursive cognitive processes that are most consequential for guiding action. One of these critics has gone so far as to claim that "if we want to learn about culture, the last thing we should do is to conduct in-depth interviews" (Martin 2010: 240). But whether in extreme or mild forms, critiques of interview methods have become more and more common in cultural sociology.<sup>1</sup>

Allison Pugh's (2012) recent article pushes back against this trend and seeks to defend the interview as the primary tool of the cultural analyst. She outlines what she sees as the limitations of the critics' assumptions about how culture works and provides a typology of the different kinds of information available in an in-depth interview. As one of the "cognitive culturalist" critics to which Pugh is responding, I welcome this opportunity to engage with her, both because I think we are closer on some of these issues than she thinks and because it gives me a chance to clarify how the cognitive culturalists (or at least this cognitive culturalist!) understand the relationship between theory and method in the study of culture.

### **Cultural Incoherence and the Dual-Process Model**

At the center of this discussion is the undisputed fact of "cultural incoherence"—the tendency for people to give self-contradictory and inadequate accounts of their actions and motives. Pugh claims that the cognitive culturalists (hereafter CCs) have "a certain distaste" for incoherence and want to "obliterate[e]" it (pp. 45, 48). But that description does not seem right to

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<sup>1</sup> Such critiques have come not only from quantitative researchers but also from ethnographers. See, e.g., Jerolmack and Khan (forthcoming) and the response in Vaisey (forthcoming).

me. The role of incoherence in the intellectual history of the cultural turn was not as a generic problem but as a problem *specifically for Weberian-Parsonian theories of action*. It was scholars like Randall Collins (1981: 991) and Ann Swidler (1986: 284) who brought this issue to the fore because they claimed incoherence was fatal to these theories' microfoundations. To simplify greatly, the argument was that if a person's beliefs and values don't *point* in one direction but she *moves* in one direction, it must not be the beliefs and values that are doing the moving. This motivated cultural sociologists' replacement of an ends-focused account of culture with means-based concepts like conversational resources, toolkits, and cultural competences (see DiMaggio 1997; Vaisey 2010).<sup>2</sup>

For CCs, cultural incoherence is more of a puzzle than a problem—how can culture motivate people *even if* they can't explain how? Fortunately, the synthesis of Bourdieu's work with developments in neuroscience and related fields has done wonders for tackling this issue. Though there are differences among scholars, the rough consensus among CCs is that there are two analytically distinct levels of cultural embodiment in persons—a discursive, propositional level and an intuitive, practical level (e.g., Ignatow 2007; Vaisey 2009; Cerulo 2010; Lizardo and Strand 2010; Martin 2010; Vaisey and Lizardo 2010). This dual-process model (DPM) solves the puzzle by locating (Swidlerian) incoherence and (Weberian) motivation on different cognitive levels.

Pugh, however, is not satisfied by this resolution and raises several objections. I discuss her theoretical concerns before turning to a more focused discussion of interviews and their role in cultural sociology.

*Can't we just embrace cultural incoherence rather than trying to "solve" it?*

Although the DPM embraces discursive incoherence, Pugh wants to embrace incoherence at every level (pp. 48, 54). Is this the right approach? There are two ideal-typical ways of dealing with the fact that people are discursively incoherent and yet pursue coherent strategies of action. The first is to posit that the orienting structure is *inside* a person's repertoire such that she will tend to choose more valued, chronically accessible, or salient bits of culture over others. The

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<sup>2</sup> Pugh rightly points out (p. 44, fn. 1) that Swidler's concept of culture is about more than rhetorical justification. She also draws attention to the question of whether people have the cultural skills necessary to navigate particular social environments. See the discussion in Vaisey (2010).

second is to assume that the structure is *outside* a person such that the (social) environment calls forth different bits of culture from her (unstructured) repertoire. Neither of these positions is more "social" than the other; the former emphasizes long-term socialization (or habitus) and the latter emphasizes short-term situational influences.<sup>3</sup>

Most versions of the DPM begin closer to the habitus position. Although Pugh dislikes the idea of "solving" the problem of incoherence, she must nevertheless deal with the puzzle and ends up adopting something closer to the situationalist position. She claims that cultural schemas are "activated under certain circumstances" like "motion-sensor streetlamps" that ultimately "subside into darkness" (p. 49). This is a common position among cultural sociologists, who tend to emphasize the importance of cues from the environment (see e.g., DiMaggio 1997: 274; Swidler 2001: 84) and to be deeply skeptical of dispositional explanations (see Vaisey 2010). There is, however, no empirical reason to privilege a situational account over a more "habitus"-like account. In fact, the literature strongly supports a dynamic relationship between dispositions and social environments, with acquired dispositions playing a key role in both behavior and selection into subsequent environments (see e.g., Vaisey and Lizardo 2010). The point is that incoherence can neither be wished away nor simply celebrated, but must be addressed theoretically.

#### *Does the DPM ignore social context and cultural change?*

Two related concerns Pugh has with the DPM is that it is too individualistic and (thus) incapable with dealing with cultural change. Quoting Swidler, Pugh (p. 47) laments "the fundamental individualism of such models" and enjoins us to recall that "cultural meanings are organized and brought to bear at the collective and social, not the individual, level."

It is important to distinguish, however, between where something is *measured* and what it actually *is*. Class, race, gender, and birth cohort can all be measured at the individual level even though we all understand that they are defined by an extensive network of social meanings and rely on institutional infrastructures. Measuring which cultural meanings are most important to a person does not imply a theoretical commitment to "individualism" any more than does asking him how many years of education he's completed. However, though it is true that cultural

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<sup>3</sup> This is a simplification, of course. See Lizardo and Strand (2010) for a more complex discussion of the relationship between "internal" and "external" cultural elements.

meanings are *organized* collectively, they are not, in fact, always *deployed* collectively. Indeed, one of the things cultural sociologists want to figure out is how individual differences in the ability to deploy cultural capacities influence a person's outcomes (e.g., Rivera 2012).

But what about cultural change? Pugh asks "if the viscera of practical consciousness drive action as unerringly as a caboose down the track, then how might this process incorporate change?" This question makes at least two assumptions foreign to the DPM. The first is that the practical consciousness is unerring. All discussions of the practical consciousness with which I'm familiar discuss it in terms of "disposition," "bias," or "tendency," all of which are probabilistic concepts. Change can happen therefore if a person misreads a situation or if other factors temporarily overcome her dispositions. The second foreign assumption is that the "track" the person wants to go down is always accessible. A person's habitus can certainly be out of sync with the objective state of the social field because not all cultural processes are endogenous. Many things are changing other than a person's culture. When a person can no longer rely on the social and cultural "scaffolding" to which their practical consciousness is well adapted, she must switch into a more deliberative form of processing and initiate some kind of change (Lizardo and Strand 2010).<sup>4</sup> Riders can—and do—train elephants, after all, though it takes time and effort.

#### *Does the DPM neglect emotion?*

I confess that I found Pugh's assertions that emotions are a "missing vector" (p. 48) and "under-elaborated" (pp. 52, 64) in the DPM the most perplexing aspect of her argument. Nearly all of the CC texts Pugh cites have extensive and explicit discussions of the importance of emotion. In my own "Motivation and Justification," I discuss "emotion" six times and "feelings" more than 20 times. Quoting cognitive anthropologist Roy D'Andrade, I argue that "[the] cultural shaping of emotions gives certain cultural representations emotional *force*, in that the individuals experience the truth and rightness of certain ideas as emotions *within* themselves" (Vaisey 2009: 1686, emphasis in original). One of the main points of the DPM in this area is that although people "know" a lot of culture, they only care deeply about (and/or have mastered the use of) a small subset of it. Deeper or more emotionally charged cultural elements influence the way a person deploys "shallower" discursive elements. Situations that call forth deep concern in some

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<sup>4</sup> Pugh summarizes this argument herself in footnote 2, making her claim that this is a problem somewhat puzzling.

people leave others unmoved. I see no contradiction between Pugh's observations on emotions and the role of emotions in the DPM.

### **What Is an Interview Worth?**

The theoretical points discussed above could be considered alone but Pugh's ultimate goal is methodological—to vindicate the in-depth interview. Perhaps the main point of Pugh's argument is that the DPM errs in regarding all "interview talk" as mere post hoc justification. Here I think Pugh is right. It was an oversimplification to claim that interviews "give us little leverage on unconscious cognitive processes" (Vaisey 2009: 1688). Cognitive anthropologists, for example, have made many of the same arguments that CCs have made about dual cognitive processes and yet rely almost exclusively on interview data in their work. Research in this field regularly moves from discourse to an analysis of implicit schemas and emotions (see e.g., Strauss and Quinn 1997; Quinn 2005).

I maintain, however, that interviewing is something like being a "sketch artist." Sketch artists must use their particular skills to translate one kind of information (verbal) to another (visual) just as interviewers must use their skills to move from one kind of content (explicit talk) to another (implicit cultural content). (For the record, no one has likened interviewers to "stenographers" although Pugh defends herself against that characterization twice [pp. 54, 65]). And I think surveys can be something like a "line up" in that they let people agree or disagree with statements that they recognize and understand but might not have been able to produce on their own.

Pugh claims—and I agree—that both of these methods are useful for studying culture. But in what seems to me an overcorrecting move, she claims (with a variant of Churchill's quote about democracy) that interviews are the best way to study culture and that surveys are primarily good for things like "help[ing] us better grasp the prevalence of an honorable schema" (pp. 54, 65). But I can think we can do better at getting interviews and surveys to work together.<sup>5</sup>

First, we can move from interviews to surveys. It would not be too challenging, for example, to take patterns of the sort Pugh recognizes in her work and turn them into survey questions. Many types of information—not just the "honorable"—can be captured by properly

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<sup>5</sup> For reasons of space, I do not discuss ethnography or experimental methods here. These omission should not be interpreted as a claim that they are unimportant to the study of culture.

designed survey questions (see e.g., Beyerlein and Sikkink 2008 for a survey analysis of emotion). The importance of interviews here is it helps us figure out what sorts of questions we should be asking in the first place. My own research, for example, wouldn't have asked about four types of moral cultures without the preexisting interview analysis from *Habits of the Heart*.

Second, we can move from surveys to interviews. In the third wave of the National Study of Youth and Religion, for example, we included a question in the in-depth interviews that asked the respondents to discuss why and how they chose a particular answer to the "how would you decide" survey question. Although my analysis of these data is not complete and space does not permit a discussion of the findings here, I have found this extremely useful. These discussions yielded much more than "justificatory talk"; indeed, they helped clarify the cultural schemas and emotional commitments underlying the survey responses.

### **Is This Only About Interviews?**

Although I am happy with the understanding of the relationship between interviews and surveys I outlined above, I am concerned that Pugh will not be. In the context of her analysis of Sean, one of her informants, she argues that "even if we could design a survey research question that would get Sean to reveal his core understanding[s]," such an approach would be still inadequate because it would "miss the extensive cultural work he does" (p. 58). This example helps get at what I see as a fundamental difference between what Pugh is trying to do and what the CCs are trying to do and I fear the difference is not only methodological.

In their widely cited paper, "A Tale of Two Cultures," Mahoney and Goertz (2006) discuss the different goals and assumptions of qualitative and quantitative research. For this discussion, I think the most important difference is what they call "approaches to explanation." Qualitative researchers, they argue, generally seek to explain outcomes in the context of individual cases, starting from their cases and working backward toward several causes. Quantitative researchers, by contrast, generally seek to estimate the average effect of a particular cause on an outcome for a population of cases.

With this distinction in mind, we can start to see where there is a danger of Pugh and the CCs (most or all of whom are also quantitative researchers) talking past each other. Simplifying somewhat, my primary goal is to identify the most important cultural differences between people or groups and to relate them to systematic differences in some kind of pattern of behavior,

ideally estimating the effect of the former on the latter. My goal is never to explain a particular outcome (e.g., why did *this person* volunteer?) but rather to figure out how one sort of cultural bias *generally* affects some particular outcome (e.g., does individualism reduce volunteering?; see Beyerlein and Vaisey 2013). The DPM helps by providing an interpretation of those effects that does not rely on unrealistic cognitive assumptions.

Because of this orientation toward estimating causal effects, I am not *primarily* interested in what kind of "cultural work" (p. 58) a specific person does, in "document[ing] a wider cultural terrain than simply that which animates action" (p. 64), or in analyzing a person's account that "does not lead to action" (p. 60). This is not because I am an unimaginative misanthrope but because my research question is *what is the average net effect of this particular cultural bias on this kind of conduct?* Everything else is interesting, but given the time and resource limitations, it is not possible to "watch it all" (p. 60). Given my research questions, the main values of interviewing are to find novel patterns in discourse that could generate new measures and to get a closer look at the cognitive mechanisms (e.g., schemas) that underlie the patterns detected in surveys to make sure my interpretations of the data are not implausible.

Pugh (and others) might find this focus misplaced or even frustrating. But I assure you that the feeling is (sometimes) mutual. When I read, for example, the analysis of Sean's discussion of "what we owe each other" (p. 58), I immediately ask myself questions like, "is there systematic variation in whether people see mental illness as a disease or a personal failing?" and "does variation in this classification predict divorce net of other factors?" But Pugh—to my chagrin—does not ask those questions.<sup>6</sup> Even if I started relying exclusively on interview data, I fear we would still end up talking past each other.

## Conclusion

I am grateful for this dialogue because it allows me to say publicly that I am persuaded that interviews are indispensable to cultural sociology. Pugh is correct that it was an oversimplification for me and others to argue that interviews *only* provide access to post hoc justifications (though they do that too). I see them as useful for detecting the cultural schemas

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<sup>6</sup> Similarly, when I first read Swidler's *Talk of Love*, I wanted to immediately figure out how to measure individual differences in the salience of the romantic and prosaic-realist schemas and use them to predict later marital dissolution.

available in a social environment as well as the schemas to which people have emotional attachments. Interviews can also help unearth the mechanisms behind the patterns detected in survey research and suggest new approaches to measuring cultural differences in particular populations.

Though I am glad to agree with Pugh that interviews are valuable, I fear we've come to the same conclusion for different reasons. Perhaps awareness of our different research goals can make us more understanding and less likely to speak in grand pronouncements about "how culture matters." Without asking "matters for what?" such claims can lead to all kinds of confusion. The kind of rich textures that Pugh and others get from interviews certainly "matter" in many senses. But some of them might not matter for my specific research questions. Likewise, systematic variation in the salience of cultural schemas in a population might not matter to some researchers, though, admittedly, that is much harder for me to imagine!

If nothing else, I hope I have shown that—on the level of theory—Pugh's sketch of how culture relates to action and the dual-process approach are not far apart. The DPM can incorporate (and has incorporated) social context, emotion, and cultural change without doing any violence to its basic logic or to the existing evidence. Pugh is right that we have so far paid less attention to the origins of practical consciousness than to its effects (but see Winchester 2008). Nevertheless, the cognitive culturalists have abundant theoretical resources for asking questions about its development (e.g., Lizardo 2007). Such questions (and many others) remain and it will take researchers using many different methods to answer them.



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