

## A Limited Defense of Demographic Cultures

### Abstract

A number of approaches in the social sciences appeal to demographic cultures in their comparative explanations. Though varied, accounts of demographic cultures function both to classify cultural groups and to explain differences between those groups. Yet demographic cultures have long been subject to scrutiny. Here I isolate and respond to a set of arguments I call *demographic scepticism*. This sceptical position denies that demographic cultures can factor into metaphysically plausible and empirically principled research projects. Against this position, I claim that the sceptics overinflate the claims of empirical researchers and rely on a restricted (or possibly, outdated) understanding of metaphysics. Nearby metaphysical positions—notably, relational essentialism—can do the work of classifying different cultural groups and leave open the possibility for multiple ontological operationalizations and causal-mechanical explanations of inter-group differences.

### Introduction

Among other uses, “cultures” or “cultural groups” are used as an explanatory category in comparative approaches. These approaches explore cross-cultural differences in psychology and personality (cross-cultural psychology, cultural psychology, cultural evolution); life history strategies (evolutionary demography, evolutionary anthropology); material assemblages and lifeways (archaeology; anthropology); status, symbolism, and meaning (cultural sociology); ecological, political and religious change (cultural phylogenetics), and; conflict and economic growth (political science, economics). Often identifying cultural groups with nation states or ethnolinguistic populations, these *demographic cultures* are used both to classify—that is, to identify a group as a *cultural* group—as well as to provide explanations for observed variation across such groups.

Worries about the culture concept have long been leveraged to criticize the classificatory and explanatory roles of demographic cultures in comparative work. Because “culture” is a polysemous, ambiguous, and politically freighted concept, demographic cultures are claimed to be unstable, empirically unsuitable, and theoretically pernicious.

The aim of this paper is to defend the use of demographic cultures against a prominent line of argumentation I call *demographic skepticism*. This skeptical position is found across a number of literatures, but is particularly prominent in the cultural evolutionary, anthropological, sociology, and political philosophy literatures.<sup>1</sup> Two general problems motivate their skeptical challenge:

1. *Metaphysics and ontology*: the metaphysics and/or ontology of demographic cultures renders them unable to explain behavior, let alone change in behavior.
2. *Variation and Comparison*: that however construed, demographic cultures will always contain variation, and there is no principled means of identifying a level at which comparison between groups is meaningful.

According to the skeptics, these problems render demographic cultures unfit for purpose: they are committed to ontologically mysterious and metaphysically problematic characterizations of social groups that are unable to accommodate widely recognized features of human communities.

Yet while these general problematics can be stated clearly enough, how these relate to specific positions in contemporary comparative work on culture is often left implicit. Here I provide a partial reconstruction of the skeptical challenge that focusses on the metaphysical and ontological issues. I link these issues to well-known debates in philosophy around the metaphysics of natural kinds, as well as to contemporary comparative work on culture.

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<sup>1</sup> The links to the literature will be made more clearly in what follows, but some of the wellsprings for this line of scepticism include: Tully (1995); Kuper (1999); Sperber (2001); Benhabib (2002); and Hirschfeld (2018).

In making explicit the arguments of the demographic skeptic, I'll be arguing that the metaphysical and ontological problems they point to are real but are vastly overstated. Skeptics overinflate general claims into grand metaphysical commitments that researchers explicitly disavow. And while there may be a reasonable target for such a critique in some latent variable approaches, these methods are not widespread in contemporary comparative work. Not only are these methods complemented by a range of different ontological operationalizations and empirical methods, but the problems of latent variable approaches are well known and manageable.

My arguments show that demographic cultures are both metaphysically plausible and can support empirical endeavors. Still, this paper does not provide a complete defense of contemporary comparative work, nor prescriptions for how such work should progress. I limit myself to making three positive claims. First, comparative cultural researchers recognize cultural change as necessary to their research and reject metaphysical positions and ontological operationalizations incompatible with change. Second, there is at least one metaphysical picture compatible with demographic cultures classificatory efforts. Third, the processes or structures that account for the explanatory usefulness of demographic cultures need not be the same structures or features that account for its classification.

These limited goals go some way to securing the explanatory role of demographic cultures in contemporary comparative work. Yet a more robust defense of demographic cultures would have to both contend with the problem of variation and comparison as well as articulate in more detail the epistemology of cross-culture comparison. Given the limitations of space, I leave explorations of these issues for future work.

### Demographic Cultures

Though comparative logic has a deep history, using “cultures” or “cultural groups” to explain group differences is a relatively recent affair. It was most firmly entrenched in this explanatory role by Franz Boas and his students, especially in the “cultural and personality” work of interregnum and wartime anthropology.<sup>2</sup> The second world war consolidated this role, with efforts like George Murdock’s Human Relations Area Files (“HRAF”) feeding into a massive military mobilization efforts by provisioning organized information on local populations.<sup>3</sup> The HRAF—and the later-developed Ethnographic Atlas and Standard Cross Cultural Sample associated with the HRAF—formed the basis of quantitative cross-cultural work in the middle 20<sup>th</sup> century. Brought online, the eHRAF database now joins a number of related ethnographic databases and international surveying efforts that increasingly underwrite contemporary comparative work on culture.

The eHRAF identified cultural groups with ethnolinguistic populations. Yet other approaches arising in the twentieth century—notably, in business management, economics, political science, and some parts of cross-cultural psychology—identified them with nation states. These two ways of drawing boundaries around cultural groups sit uneasily beside one another, and as I’ll suggest below, the idea of “national cultures” remains contentious.<sup>4</sup> Yet the drawing of boundaries is only loosely linked to questions around the ontology of cultural groups. In fact, multiple ontologies of culture emerged in 20<sup>th</sup> century social science, with groups being identified variously with latent constructs, functionally integrated mental representations, collections of practices, institutional lineages, and more besides.

Rather than representing an impediment to empirical research then, the flexibility that multiple ontological operationalizations afforded to researchers facilitated an explosion of data-gathering

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<sup>2</sup> Evans (2005); Mandler (2013); Jeffers (2013)

<sup>3</sup> In addition to its research role, the HRAF was used to produce pamphlets on the values, practices, and beliefs of local populations to assist occupying military forces. Lemov (2000)

<sup>4</sup> As we’ll see in more detail below—particularly with regards to value consensus models—one prominent line of critique is to point out that there is greater variation within than across putative “national cultures.” (e.g. Taras 2016). Scheffler (2007) defends the value of “national cultures” in a way that aligns with the account provided here.

and analytical techniques during the latter half of the 20<sup>th</sup> century.<sup>5</sup> This explosion also saw the use of comparative methods and “culture talk” move beyond cultural evolution, anthropology, archaeology, and psychology into disciplines like biomedical sciences, philosophy, economics, and political science.<sup>6</sup> And as demographic cultures expanded into new disciplines and domains, it fostered further ontological operationalizations and controversial research practices.

Consider on this point, the comparative work of Nisbett (2003). Based on research conducted over many years, Nisbett claimed that deep cultural differences existed between “Easterners” (broadly, those in East Asian countries, including Japan, Korea, and China) and “Westerners” (broadly, Canada, the United States, Australia, and the United Kingdom). Each “culture” had a distinct, but coherent history with contrasting ontological, conceptual, and linguistic structures. These, Nisbett suggested, were reflected in cross-cultural performances on perceptual and epistemological tasks. Whatever the merits of this research, it represents a substantial and radical expansion in what counts as a cultural group.

This thumbnail sketch of the history, widespread use, and theoretical expansion of demographic cultures is meant to accomplish two things: first, to show that demographic cultures are a widespread explanatory tool; second, to briefly catalog the ontological and methodological variety of comparative cultural work.

That cultural groups are operationalized in diverse ways informs the present piece in two ways. First, because of the sheer variety of work in contemporary comparative work, I cannot address every objection to demographic cultures. Such objections often hinge on specific ontological operationalizations or methodological issues. As noted above, I focus on the general metaphysical and ontological arguments of demographic skeptics. Second, the expansiveness and flexibility of the terms “culture” and “cultural groups” motivates a reasonable skepticism toward demographic cultures. In part *because* of the variety in ontology and methodology—and the radical expansion of its extension— it seems reasonable to question the utility of the concept, and how it has been put to use to study differences across groups.

### The Folk Anthropological Model

We can best understand the arguments against demographic cultures by showing how skeptics align it with what I call the *folk anthropological model* of culture. The general idea of the folk anthropological model is that cultures are *isolates*; geographically-bounded populations with unique ways of life, whose distinctiveness is maintained through boundaries or barriers.<sup>7</sup> The picture is intuitive, grounded in commonplace ideas about what anthropologists are taken to do: they go out into the field and study *a culture*. Yet the folk anthropological model has been widely critiqued.

There are many such critiques. Nonetheless, the perceived problems of the folk anthropological model can be roughly summarized by the following four pernicious commitments:<sup>8</sup>

*Essentialism*: cultures have an essential structure which play a role in identifying and categorizing instances of cultural kinds, as well as explaining the superficial properties of such instances.<sup>9</sup>

*Holistic organization*: cultures are tightly integrated, organized structures of interlocking elements including norms, practices, and values.

*Homogenous communalism*: cultures encompass agents who share values, knowledge, and beliefs in common.

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<sup>5</sup> Lemov (2000); Kashima and Gelfand (2011).

<sup>6</sup> E.g. Kymlicka (1995); Taylor (1995); Laitin (2007); Benjamin (2017)

<sup>7</sup> Henrietta Kuklick (1996) calls this the ‘island model’: that cultures are distinct ‘islands’ of peoples.

<sup>8</sup> Compare Parekh 2000, 77-79; Brightman 1995

<sup>9</sup> Parekh 2000, 77; Benhabib 2002; Hirschfeld 2018, 241-242.

*Strong boundaries*: cultures have geographic, linguistic, commercial, and ethnic boundaries that prevent hybridization and diffusion of cultural elements.

The folk anthropological picture is straightforwardly aligned with demographic cultures. Consider here remarks from Hirschfeld (2018), a demographic skeptic who will serve as one my main interlocutors in this essay:

“[...] the growing relevance of cultural variation in psychology has not extended to a close examination of the notion of culture. Rather, there is wide acceptance that culture refers to a specific kind of social form that is entity-like, bounded, timeless, stable, symbolically fused, and highly shared in terms of the values, practices, and interests embodied in cultural performances and representations” (232)

As Hirschfeld sees it, demographic cultures are committed to all of the pernicious assumptions of the folk anthropological model. It is committed to *essentialism* (‘timeless, stable’ culture), *holistic organization* (‘symbolically fused’), *homogenous communalism* (shared ‘values, practices, and interests’), and *strong boundaries* (‘entity-like, bounded’).

So aligned, demographic skeptics marshal convincing evidence to show that the empirical claims made about populations (*holistic organization*, *homogenous communalism*, and *strong boundaries*) are empirically false. Even deep in history, hominin populations were sites of significant diffusion, change, and hybridization.<sup>10</sup> Leveraging these claims, skeptics argue that the overarching metaphysical picture (*essentialism*) is implausible. Essentialism either renders change impossible, because to be a particular cultural group is to have a particular essence; or ontologically fickle, because changing the essence of a cultural group means bringing a new group into existence.<sup>11</sup>

Yet why call this folk picture *pernicious*? Importantly, skeptics believe that many of these elements persist in the public imagination and in the research methods of cultural researchers. This is so despite widespread efforts at diagnosing the empirical and metaphysical wrongheadedness of the folk anthropological picture. Even though these assumptions and their failings have long been pointed out, they remain entrenched in the assumptions, concepts, and methods of researchers and everyday explanations. Thus, these assumptions are pernicious; hard to root out.

I think there is much truth in the claims, particularly in the idea that the folk anthropological model informs and guides commonsense assumptions about cultural groups. Nonetheless, it is not clear to me that anyone has ever endorsed the folk anthropological model in its entirety. My sense is that if the model persists, it is mostly as a tool used to (often, uncharitably) bludgeon researchers and their research frameworks.<sup>12</sup> That might be the case with demographic cultures. Yet because the model is composed of such neat and distinct elements, it is useful both to illuminate the arguments against demographic cultures, as well as the positive arguments of comparative researchers.

In what follows, my emphasis is exploring the claim that demographic cultures are committed to *essentialism*. What kind of metaphysical commitments are being attributed to comparative cultural work, and why are they so problematic? I turn to this question in the next section.

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<sup>10</sup> Clifford 2013; Voss 2015; Sterelny 2021

<sup>11</sup> See Patten (2014) for a version of this claim, what he calls the “dilemma of essentialism”

<sup>12</sup> Parekh 2000 locates what seem to be the most plausible defenders in Vico, Montesquieu and Herder. Perhaps the closest contemporary researchers come to this model is found in political science and economics. Here, metrics like *ethnolinguistic fractionalization* or *Politically Relevant Ethnic Groups* are used to study how geographically well-bounded and culturally distinct ethnic identities can be linked to indicators like economic growth or rates of conflict (e.g. Alesina et al., 2003; Posner, 2004; Laitin, 2007). Yet for whatever reason, these literatures have not experienced the same level of critiques as other literatures using demographic cultures.

### Metaphysical Problems: A Historical Essentialism and Change

That demographic cultures are problematically essentialist is a widespread charge, found not only in cross-cultural psychology, but also in political philosophy, anthropology, and sociology.<sup>13</sup> Common to all is the claim that demographic cultures are committed to essential, “timeless”, unchanging essences. And though the critics vary in what they take to be the downstream effects of this essentialism, all tend to argue that essentialism underwrites implausible cultural ontologies and thus poor, inaccurate, or unjust empirical research.

At stake in these claims are the essentialist accounts of natural kinds by Kripke (1980) and Putnam (1975). These distinguish between nominal, observable clusters of properties and the underlying real essence or “hidden structure”.<sup>14</sup> Such hidden structures serve as means both to classify entities as well as to explain why members have the nominal clusters of properties that they do. The canonical example in this literature is gold: gold displays a number of observable, nominal properties (being yellow, shiny, malleable, conductive) which are taken to be explained by its underlying atomic structure. The “real essence” of gold is thus its number of protons—its atomic number—which together with other facts from physics and chemistry, explain the nominal properties.

Claims about essentialism in comparative cultural work are, I contend, claims that demographic cultures are committed to hidden structure essentialism. Taking sceptics to be so committed helps to explain their arguments that demographic cultures cannot account for widespread change (because change is incompatible with a hidden structure that generates nominal properties), is ontologically fickle (because it requires positing new cultures with every change to the hidden structure), and ontologically mysterious (because there is no agreement on the nature of the hidden structure). These also complicate the explanatory project. If essences are underspecified, it is hard to see how they could support empirical research and compelling explanations.

Before moving to addressing these claims, however, it is worth stepping back to interrogate the motivations of demographic skeptics. What supports their claims that demographic cultures are best understood as “hidden structure” essentialist kinds?

Again, Hirschfeld (2018) serves as a useful exemplar of the demographic skeptic. Hirschfeld’s reasoning follows a common pattern in the literature, which does not directly object to empirical work of comparative researchers—which should already raise eyebrows—but instead rehearses the disciplinary history of anthropology. This history shows how that field has come to grips with hidden structure essentialism by jettisoning the problematic metaphysics along with any attempt at classifying cultural groups. The narrative is supposed to demonstrate that by getting rid of problematic metaphysics underwriting classification and comparison, anthropologists have been freed to get on with other interesting humanistic and empirical projects.

More generally, this disciplinary history is meant to show the tension between any essentialist characterization of culture and empirical work that deals with entities that vary, change, and hybridize.<sup>15</sup> And for reasons explored in more detail below, this general argument might be correct for hidden structure essentialism. But as it stands, this argument does not show that comparative cultural work is in fact committed to hidden structure essentialism.

Making an even stronger argument, Hirschfeld mobilizes work on evolutionary psychology to argue that *any* classification of social groups will invariably lead to hidden structure essentialism. The argument runs as follows. Comparative researchers of all stripes make descriptive

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<sup>13</sup> Brightman (1995); Tully (1995); Benhabib (2002). For a helpful review of claims around essentialism in the political philosophy literature, see: Patten (2014)

<sup>14</sup> This is often called ‘microstructural essentialism’—but it is clear that whatever culture is, it is not microstructural in the same way that canonical examples from chemistry or physics might be microstructural. I thus stick with the terminology elsewhere used by Putnam of a ‘hidden structure’.

<sup>15</sup> This genealogical strategy is widespread, especially in the literature on multiculturalism. See, for instance Tully (1995) and Parekh (2003).

generalizations and explanatory claims about groups. These groups in turn will have some features that set them apart; they might look a certain way and do things in a different manner. We might even be able to explain these by pointing to features like shared values or practices. But these descriptive and explanatory generalizations feed into cognitive tendencies towards essentializing groups, rendering culture a problematic “universalizing totality” (Hirschfeld 2018, 242; 1998; Morris 2013). Because of our cognitive biases, hidden structure essentialism is the expected outcome of *any* attempt to classify groups as groups of a particular kind, whether these are races, religions, cultures, or linguistic groups. And while not borrowing Hirschfeld’s psychological explanation, most other demographic skeptics agree that *any* classification of culture will be committed to hidden structure essentialism.<sup>16</sup>

I see at least two reasons to be suspicious of this line of thought. Even if one believes that naïve theorizing about groups is guided by universalizing tendencies—for instance, that humans have a tendency to overestimate the similarities between perceived out-group members (Hirschfeld 1998)—this does not seem to be the right place to look for the metaphysical or ontological commitments of scientists. These commitments are often embedded in the assumptions of formal models, empirical research strategies, and other methodological protocols. And while scientists themselves are human and subject to biases, the formalization, operationalization, and rigor of scientific communities suggests that they may be able to avoid if not overcome these biases.

When we look at work from sociology, for instance, like Goldberg and Stein’s (2018) *associative diffusion* model of cultural variation, we find that very simple mechanisms of learning and evaluation (semantic association, “constraint satisfaction”) can generate groups of individuals with similar clusters of traits (a “semantic network”). But the model itself makes no claims about an underlying “hidden structure” anywhere. There is no structure in the social networks nor among the cultural items being transmitted and evaluated. In fact, the kind of universalizing move Hirschfeld points to is explicitly recognized and *disavowed* by researchers in the social sciences who adopt what Emirbayer and Goodwin (1994) have called the “anti-categorical imperative”; the methodological avoidance of explanations that merely appeal to classificatory categories.

Moreover, when researchers are actually interrogated about their commitments, they readily acknowledge the dynamic nature of culture and cultural change. This is true even for the cultural psychologists that form the core target for Hirschfeld’s argument (Kashima and Gelfand 2011). This should lead one to be suspicious of claims that comparative cultural work is somehow incompatible with cultural change and variation. While it is possible that researchers might be confused about their own metaphysical commitments, it would be strange indeed such researchers *identified* cultural change as central to their work, *recognized* this problematic metaphysical picture, and adopted explicit methodological strategies to *avoid* it, yet still remained committed to such a metaphysical picture.

The second reason to be suspicious comes from the evidence used by demographic skeptics. What they identify as problematic are general explanatory claims that appeal to social categories: for example, that “theory of mind develops differently in strongly collectivist cultures” or that “the United States has a distinctive set of individualistic values”.<sup>17</sup> Yet even when such claims make specific attributions to demographic cultures—that they are “tight” rather than “loose”, or that they explain different responses to Gettier cases—it is not clear these claims embed particular metaphysical commitments.

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<sup>16</sup> This is not a particularly well-articulated argument in the literature. Perhaps the clearest alternative is from Benhabib (2002) who argues against what she calls “strong” or “mosaic multiculturalism”. Her motivation, and one seemingly shared by a number of political philosophers, is that any method making empirical claims about cultural groups relies on substance metaphysics—that culture is a *thing*—rather than a processual metaphysics; e.g. that “We should view human cultures as constant creations, recreations, and negotiations of imaginary boundaries between “we” and the “other(s).” (8) Related to this is the claim that cultural groups are created by outside perspectives, “imposed” on a cultural group by the researcher or the state (Brightman 1995).

<sup>17</sup> These claims are paraphrased from the works of Shahaieian et al. (2014) and Muthukrishna et al. (2020) respectively.

Rather than being claims about an essence or common feature of cultural members, one can read such claims as *generics*. Generics are a kind of linguistic construction that can remain true in the face of exceptions; for instance, ‘bread has gluten’ is true, even though there are many gluten-free loaves. Bare plural generics (‘bread has gluten’, ‘tigers have stripes’, ‘cultures are highly integrated’) generate a number of philosophical problems but are unproblematically used in both ordinary and scientific language and reasoning. For instance, researchers use bare plural generics in research and discussion about species (‘tigers have stripes’, ‘platypuses hunt using electrolocation’) even while recognizing that such generics are made about changing populations. The same is true of cultures. It seems equally reasonable to understand claims—including many causal-explanatory claims—about demographic cultures as being bare plural generics.

Taken together these considerations show that the motivations for aligning hidden structure essentialism with demographic cultures are on shaky ground.

Let me now return to consider these metaphysical issues. As mentioned above, demographic skeptics identify change as the key problem with hidden structure essentialism. The problem is both classificatory and explanatory. Essences can’t change without also changing the identity of the entity in question (cultures would be ontologically fickle). And if essences were constantly changing, they would not be able to explain clusters of nominal properties (essences wouldn’t be explanatory). Since we know cultures change, this suggests either that hidden structure essentialism is wrong, or that one must commit oneself to a fickle and empirically suspect picture of cultural groups.

There are two routes to rejecting this argument: one which supports essentialism about demographic cultures and one which doesn’t. The first I’ll consider rejects essentialism—yet the rejection is informative and shows how an amended essentialism might satisfy the problems noted by the sceptic.

First, it’s important to recognize that essentialist metaphysical positions are compatible with significant change. Hidden structure essentialism is a kind of intrinsic essentialism: sets of intrinsic properties (like atomic number) serve as the hidden structure—the “real essence”—of that kind. Such essentialism allows for all non-essential intrinsic properties (“accidental properties”) to vary. Depending on how researchers draw boundaries around the hidden structure, essentialism might be compatible with even substantial cultural change.

Of course, this strategy would only suffice if the essential intrinsic properties could be distinguished from non-essential intrinsic properties. That is, if there were some hidden structure that all and only members of some cultural kind held, and that this hidden structure explains nominal clusters of observable properties. And here I agree with the skeptics who claim that there do not seem to be such hidden structures. From all we know about culture and cultural groups, they are inherently fluid and changing entities, constantly hybridizing, shifting, melding, and combining. All their elements are subject to change. These considerations undercut intrinsic essentialism in any form. Hidden structural essentialism is just not a plausible metaphysical platform for demographic cultures.

Nonetheless, and to take the second route, rejecting hidden structure essentialism does not mean rejecting nearby metaphysical pictures. It is plausible to think that a relational essentialism can underwrite the classificatory needs of comparative researchers, without the baggage of hidden structure essentialism. Relational essentialism identifies kinds with sets of relational properties. Consider for instance, Okasha’s (2002) relational essentialist approach to species. Species are what they are in virtue of their genealogical relationships to other species. For many sexually reproducing fauna, we can roughly characterize this position as saying that species are particular branches in the tree of life. Tracing branches back to the trunk, in effect, is retracing the evolutionary trajectory of that species back through time. On this picture, what it means to be a particular branch—a particular species—is to be related in the right way to the rest of the tree.



What it means to be a platypus is to be a member of a populational lineage that connects up with a separate branch (the echidnas) about 19 million years ago, and so on.

Relational essentialism is compatible with substantial change among intrinsic properties. This is so long as the key relational properties remain intact. Platypuses could change substantially—growing in size, changing their color of their pelt—yet remain platypuses just in case they arise from the right genealogical relationships. Relational essentialism can also accommodate radical similarity between different species. For instance, platypuses have “duck-bills” and “beaver-tails”. But whatever their similarities, ducks and beavers are not platypuses. They have a different set of relational properties and thus are different kinds of critter.

This kind of metaphysical picture underwrites accounts of social kinds like race and gender (Bach 2012; Godman 2020), as well as Alan Patten’s (2014) *social lineage account* of culture. Patten’s account identifies cultural groups as sociohistorical isolates.<sup>18</sup> These persist just when group members *control* the institutions of education and enfranchisement (2014, 47-54). Control over the enfranchising institutions allows already enfranchised members to inculcate new members with the prevailing ideas, values, practices, and norms. Cultural groups, on Patten’s account, are just like species: they can change—and change radically—yet nonetheless remain self-identical so long as genealogical relational properties of institutional control remain intact. As he writes:

At any given moment, its content consists in various beliefs, meanings, and practices, but what makes these the beliefs, meanings, and practices of a shared culture is that the people who hold them share a common social lineage. (Patten 2014, 51)

Taken together, this suggests that skeptics move too quick in aligning demographic cultures with “hidden structure” essentialism. Not only are comparative cultural researchers explicitly opposed to some of the proposed features of essentialism—admitting that cultures can change and hybridize quite radically—there are nearby metaphysical accounts which offer a plausible platform for gathering together the empirically fruitful operationalizations of culture and cultural groups used by empirical researchers. We need not be wedded to relational cultural essentialism, but it provides a plausible grounding for projects that both classify and explain culture.

### Ontological Problems: Concrete Operationalizations of “Culture” and “Cultural Groups”

Often, sceptics raise a related worry about culture: that the term supports a range of ontological and empirical operationalizations, or worse, is never operationalized at all. In either case, there is a lack of ontological specificity and theoretical clarity that can guide researchers who might be engaged in comparative work. As Hirschfeld puts it, there is no excuse for using an “intuitive, unscrutinized notion as the central concept in a causal argument.” (Hirschfeld 2018, 234).

Here it is useful to look at a formulation of the problem by the cognitive anthropologist Roy D’Andrade (1992):

“There is no clear relation between *culture* and *action*. Of course, one can say “people do what they do because their culture makes them do it.” The problem with this formulation is that it does not explain anything.” (D’Andrade 1992, 23)

D’Andrade’s formulation identifies two problems: (i) a lack of *ontological specificity* about the nature of culture and (ii) a clear *causal-explanatory link* between culture and human behavior. As he puts it, “Unless there is some specification of how culture “makes” people do what they do, no explanation has been given.” (*ibid.*) In this section I address whether it is the case that comparative cultural work in fact lack clear ontological operationalizations. In the next, I explore the claims that “culture” is not explanatory. As D’Andrade’s formulation notes, the two issues are linked—yet it is worthwhile treating them separately.

The lack of ontological specificity is sometimes seen as a striking problem. To return to Hirschfeld, he surveys a landmark paper in cultural psychology (Markus and Kitayama 1991) and claims that

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<sup>18</sup> N.B.: not *geographical* isolates as associated with the folk anthropological picture.



the paper “used the concept of culture 105 times without discussion of what it might in fact refer to.” (237) He further notes “their paper is not unusual in this regard.” (237) This underwrites his more expansive and general claim about comparative work, whereby “culture is never identified nor given any material description (beyond the action that is supposedly evidence of it)” (235). Culture is described as something akin to a *force*. This, he thinks, is mysterious, and weakens the case for demographic cultures.<sup>19</sup>

To agree with the demographic skeptic, ontological articulations of culture often share a difficulty articulating their subject matter. But this should be unsurprising. It is hard to provide an exhaustive and detailed description of what culture is, given that it is embedded in a range of material vehicles, behavioral expressions, vocalizations, institutions, and social roles. Yet while it is difficult to provide detailed and encompassing operationalizations of all the ways that culture is instantiated, this doesn’t mean researchers lack ontological operationalizations altogether. Indeed, many researchers seem to arrive at ontological operationalizations by adopting something like a “Quinean” strategy: working back from successful explanations (those that satisfy key desiderata) to discern what ontological commitments are embedded in those explanations.

So for instance, D’Andrade’s identifies a desideratum that cultural explanations be grounded in contemporary cognitive scientific frameworks. Working backwards from successful explanations that appeal to structured “scenarios” or “narratives”, D’Andrade endorses an account whereby culture is identified with a systematically organized “informational pool” embedded in material artefacts, behavior, expressions, goals, values, and environments. The link between culture and action are particular “cultural schema”: mental representations, instantiated in the brain, which facilitate agent abilities to interpret their environment in terms of means-ends relationships (D’Andrade 1992, 31).

While it is true there is no consensus on cultural ontology, it is hyperbolic to say that it is never given a material description. The *informational* operationalization favored by D’Andrade (1981; 1992) can also be found elsewhere; in cultural evolution (Henrich 2015); sociology (Axelrod 1997; Goldberg and Stein 2018); and archaeology (O’Brien and Lyman 2002). Beyond this, philosophers have identified a range of candidate ontologies for culture in their surveys of the anthropological literature (Risjord 2016), with practice-based ontologies finding adherents in the sociological (Bourdieu 1977), anthropological (Ingold 2011), and social ontology literature (Rouse 2015).

Of course, the ontology of “culture” is not the same as the ontology of “cultural groups” as a social category; and there is room for significant divergence of opinion in how the two are linked. Those who identify culture with “information” tend to identify cultural groups with a particular “informational pool”. This is particularly prevalent in areas of sociology, and the evolutionary human sciences. In evolutionary anthropology, economics, and political science, cultural groups are frequently identified with ethnolinguistic populations—that is, informational accounts of culture are complemented by boundaries drawn around populations with shared biological ancestry. And as noted above, in philosophy, Patten’s (2014) social lineage account identifies cultural groups with lineages of institutional control.

“Culture” and “cultural groups” may not always be clearly operationalized terms. Some researchers may lack clear operationalizations altogether. But we must not confuse such researchers as being representative of those who employ comparative approaches to culture. Many of these have clear and well-defined operationalizations for both “culture” and “cultural groups”. Though these evince different ontological commitments, philosophers of science have long noted how complex concepts support different and empirically fruitful ontological operationalizations. Culture does not stand out as distinct in this regard.

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<sup>19</sup> Related comments disparaging operationalizations of “culture” can be found in Kuper (1999) and Mesoudi (2011)

## Explanatory Concerns Latent Variables and Value Consensus Models

There is one final component to the skeptics worry about metaphysics and ontology. This links the ontology of culture to explanatory concerns. Again, it is helpful to return to Hirschfeld:

“[...] the overwhelmingly dominant view in psychology is that the conjunction of features — spatially bounded, territorialized, convergent and shared systems of belief and practice derived from key symbols, key values, etc. — typifies a culture and as a result of which cultures are seen to possess a *causal force*. The Japanese rear their children as they do because of culture values for *communitas* and veneration to senior generations.” (Hirschfeld 2018, 245)

It is the idea that culture is a *causal force* that is so problematic.

“Anthropologists and cultural psychologists *should be* interested in accounting for how universal phenomena are culturally inflected, treated in cultural discourses, and come to have the cultural profiles they enjoy. But they *shouldn't* say that cultural inflection, discourse, and profile are *caused* by some cultural force that inhabits, as Strauss & Quinn (1997) so nicely put it, a cloud over Cincinnati.” (Hirschfeld 2018, 246)

Alberto Acerbi (2020, 217) comes to a similar conclusion: “we should not think of culture as an entity and, especially, as an entity with causal powers.” Culture is an *explanandum* rather than an *explanans*. It should neither be used to classify groups as cultural, nor to explain the behavior of group members.

There is a legitimate target being identified here; one that does indeed rely on problematic ideas about essentialism, ontology, and explanation. It is a problem that is most conspicuous in latent variable approaches to culture—and more specifically, when these approaches bootstrap the latent variables they identify into an ontological claim about the nature or “hidden structure” of cultural groups. Where I'll disagree with the demographic skeptic is in thinking this approach represents the “overwhelmingly dominant view” in psychology, or indeed, the wider breadth of disciplines that study culture, cultural groups, and cultural change.

Speaking generally, latent variables (sometimes also called latent constructs) are unobservable variables whose existence and values are inferred from observable (“indicator” or “manifest”) variables. Scientists and researchers employ any number of methods to extract such latent variables (factor analysis, principal components analysis), which function both to decrease the dimensionality of data and in some case represent candidate hypotheses about causal relationships. Familiar latent variables include Spearman's *g* (the “general intelligence factor”) and the “Big Five” personality traits.

This kind of approach has a long history in comparative cultural work. Anthropologists like Margaret Mead, Ruth Benedict, and Geoffrey Gorer—researchers associated with the “culture and personality” school, mentioned above—often made quite grandiose claims about the shared values and personalities of different cultural groups. These explanations were psychological, often psychoanalytical in nature, and often more than a little speculative. Drawing on evidence from literature, limited sociological data, and interviews with immigrants, these researchers linked observable features to deep “personality” constructs that structured group behavior. Sometimes, this approach veered into caricature. The most ridiculed of the culture and personality studies was Geoffrey Gorer and John Rickman's study linking Russian obsequiousness (this was the era of Stalin, after all) to protracted periods of childhood swaddling—a claim quickly disparaged as speculative ‘diaperology’.<sup>20</sup>

One ancestor to the “culture and personality” school is the shared-values approach in cross-cultural psychology. These explore how some set of shared values—for instance, how “tight” or

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<sup>20</sup> Mandler (2013)

“individualistic” a culture is—might correlate with an explain a host of other behaviors (e.g. Triandis 1995; Gelfland 2018). My concern is not with the shared-values approach in general, but with a specific variant of it: what Morris (2014) calls *value consensus models*. These models assume that a shared set of consensus values inform all members of a population.

Such value consensus models have most recently and memorably been defended by Schwartz (2014). He identifies a “hidden structure” of values; a “latent, normative value system, external to the individual, which underlies and justifies the functioning of societal institutions.” (6) This value system is “manifested” in meanings, beliefs, and practices prevalent in a society, “but it is not located in their minds. It is an aspect of the context in which people live.” (*Ibid.*) The value system, in other words, exists on a “separate ontological plane” (Morris 2013, 19) and is used both to classify particular cultures (each culture has its own latent value system) and explain observable differences between them (the manifestations of that value system in beliefs, practices, and meanings).

This is a “hidden structure” account of culture if there ever was one: the “latent, normative value system” has been bootstrapped into an organizing structure that is manifested across the society. Yet it isn’t promising for reasons mentioned above. It runs headlong into the metaphysical problems identified by the skeptic, notably, that the consensus set of values is incompatible with change. In fact, the failings of the value consensus model should give pause to those that adopt the “Quinean” approach articulated above. Ontologizing latent variables can be fraught.

Just as suspect is the methodology used to arrive at the consensus values in the first place. As the sociologists Amin Ghaziani and Delia Baldassarri (2011) suggest of latent variable approaches like these:

“Scholars were generally not concerned with construct specification because they presumed culture was a latent variable that they could study using any number of indicators. This produced a loose conceptualization of the concept as a people’s way of life with a corresponding group-level unit of analysis [...]. This analytic and methodological approach led to the conclusion that culture is shared, consensual, and coherent. The goal of documenting cohesion led scholars to exclude inconsistent elements, a process we recognize today as a softer version of sampling on the dependent variable” (181).

Most problematically, value consensus models fail empirical tests. It just isn’t valid. Research has consistently shown that there is more variability in values within groups—especially nation states—than across them (Oyserman et al. 2002; Lenartowicz et al. 2003; Fisher and Schwartz 2011; Taras 2016). This means that comparisons based on the presumption of a shared and consensus set of values isn’t supported.

We should separate the value consensus model from latent variable approaches more generally. The latter can still be valuable when treated with caution. Morris (2014, 21) for instance, argues that latent variables can be understood as “summary indicator[s] of the extent to which these various causal forces on behavior will be pushing in a given direction” (Morris 2014, 21). That is, rather than taken latent variables to be hypothesis about a deep and persisting structures, they could be seen as aggregate measures capturing general trends that would require grounding in causal-explanatory mechanisms.

Let’s not lose sight of the larger point. Value consensus models are not reflective of all comparative approaches to classifying and explaining culture. They exists alongside sophisticated informational or practice-based ontologies, and methods of network analyses (e.g. Goldberg and Stein 2018); database-driven approaches to sociocultural history (Turchin et al. 2015; Slingerland et al. 2020); cultural group selection models (Richerson et al. 2015); and other psychological and sociological methods. Moreover, the methodological and empirical failings of these models are well-known. These failings inform the deflationary, aggregative approach of

latent variable methods suggested by Morris (2014). And this approach too is just one approach, among many others, aimed at explaining the origins and dynamic features of cultural groups.<sup>21</sup>

### Against Cultural Forces: Demographic Cultures and Mechanism Sketches

I've argued above that demographic skeptics have a genuine target in value consensus models of cross-cultural psychology. At the same time, I've suggested that these are not representative of comparative work on culture. Might there be more to sceptic's invocation of *cultural force* that might speak to this broader set of ontologies and methods? I think there might be. To see why requires engaging with some of the positive arguments employed by demographic skeptics.

Objecting to the idea of a cultural force, Hirschfeld and Acerbi express their commitment to an *epidemiological* framework inspired by the work of Dan Sperber (1996). This framework, broadly, explains the spread of mental representations across individuals by appeal to geographic and psychological causes. Such mental representations can be more or less *cultural*, but they are not, just by dint of being socially transmitted, a cultural *thing*. This is an ontological commitment: culture is a scalar quality. Mental representations that are more or less pervasive in a population are more or less cultural.

These populations can be arbitrary. Because the epidemiological framework denies that culture is a kind of entity, it also denies there are cultural groups determinable by links to such entities. As they take it, there are merely assemblages of actors, perhaps in specific contexts, that exist within and create conditions for traits to spread.<sup>22</sup> It just so happens that those traits that do spread are more cultural—and, often, more empirically interesting—than those that don't. Picking up on a widespread example in the cultural epidemiological literature, Hirschfeld gives the example of the story of Little Red Riding Hood:

“What makes Little Red Riding Hood cultural is the convergence of constraints on transformations inherent in communication, the evocation of a “catchy” narrative structure, and a suite of memory and relevance functions. The cultural environment defined by Little Red Riding Hood is not itself a community; rather it defines a set of representational dispositions identified with a population in which a genre of children's folk tales is widely distributed and has stabilized.” (Hirschfeld 248)

Let's call this picture *cultural pervasiveness*, since whether something counts as being cultural hinges on its pervasiveness in space and time within some arbitrary assemblage of agents. For demographic skeptics—especially those influenced by Sperber—this pervasiveness is in turn explained by concrete mechanisms.<sup>23</sup> These explain the differential diffusion or distribution of traits in a population over time.

With this we can restate Hirschfeld and Acerbi's arguments around *cultural force*. Culture does not perpetuate and spread by itself. Explanations appealing to culture require clear mechanistic accounts that explain their distribution in populations. Value consensus models—and maybe other cross-cultural comparative methods—fail insofar as they lack clear mechanistic explanations.

I think this broader demand is reasonable. Researchers should demand that clear causal accounts—mechanistic or otherwise—ground explanations using demographic cultures. These would show how differences between groups can be explained in terms of mechanisms (perhaps institutional ones) that lead to systematic variation between those groups. Against the sceptics,

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<sup>21</sup> For surveys of the cultural sociological methods, see Mohr et al. (2020); for a similar survey of the cultural evolutionary literature, see Henrich (2015).

<sup>22</sup> Again, Hirschfeld (2018) provides a remarkably clear articulator of this position: “The notion of community (or cultural environment) as I am using it makes no assumptions about the nature of a community beyond its epidemiological properties (again, to evoke Sperber's notion of epidemiology of representations). What is of concern is whether and how ideas, practices, and institutions become catchy, or not.” (249)

<sup>23</sup> Especially for Hirschfeld and Sperber, these explanations appeal to innate and universal cognitive mechanisms. (Sperber 1996; Sperber and Hirschfeld 2004, 2007. Cf. Buskell 2019).

however, I think that there is a wealth of comparative work that satisfies this desideratum. Bad comparative work might merely appeal to culture as a force, but there are plenty that put forward compelling causal-mechanistic explanations.

Consider as an exemplar the ambitious claims of Joseph Henrich (2020) on the deep origins of WEIRD psychology.<sup>24</sup> Over the last few years, Henrich and his colleagues have argued that WEIRD individuals are statistical outliers on a range of metrics: they are more individualistic and meritocratic, inordinately trusting of strangers, motivated by fairness, and “analytic” in reasoning (Henrich et al. 2010; Henrich 2020; Muthukrishna et al. 2020). Extrapolating from WEIRD cultures, then, is fraught—because they are statistical outliers—and the article has galvanized opinion about the need for more cross-cultural research and diversity in research participants, even if such research has been slow to appear (Barrett 2019).

More to the point, Henrich has recently argued that the suite of values and motivations associated with WEIRD psychology are proportionate with a metric he and his colleagues call the Kinship Intensity Index (“KII”). This is an aggregate measure that draws from Ethnographic Atlas—another cross-cultural survey associated with George Murdock and the HRAF—that combines a range of indicator variables: on cousin marriage, the structure of families, residence patterns, marriage structures, and marriage restrictions (particularly around endogamy). In a nutshell, the KII tracks the intensity of kin-based structures (around kinship and marriage) and the extent that they influence everyday lives.

These kin-based structures are pervasive within groups, structuring everyday activities: who one sits next to (not *that* cousin), who counts as a father or uncle (not from *that* side of the family), who can be married (not from *that* clan). Kin-based structures also influence psychology—and this is where the KII meets WEIRD psychology.

On Henrich’s account, cultures that rank high on the KII will be those where there is greater “conformity to peers, deference to traditional authorities, sensitivity to shame, and an orientation toward the collective (e.g., the clan) over oneself.” (Henrich 2020, 198) In populations that rank lower on the KII—where there is rampant exogamy, neolocality, and importantly, a lack of cousin marriage—agents live in “individual-centered worlds” with “greater independence, less deference to authority, more guilt, and more concern with personal achievement.” (*Ibid.*) The broad causal claim Henrich makes is that WEIRD populations are outliers compared to most other cultures because they have “fewer and weaker” relational bonds, as measured by the KII.

This account is ambitious, as noted above, and is bolted onto an even more ambitious historical narrative. Henrich claims that links lower KII scores seen in Western Europe were driven by a package of ecclesiastical and legal decisions by the Catholic church which simultaneously outlawed cousin marriage to an unusual degree and boosted a package of ideas supporting the individual accumulation of wealth. The historical merits of that case are contentious, but not obviously wrong. Whatever its merits, Henrich’s account provides a clear and unambiguous case of demographic cultures featuring into a powerful cross-cultural comparative account, grounded in concrete mechanisms of kinship.

Hirschfeld and Acerbi are right that cultural pervasiveness is an interesting explanandum. But demographic cultures can function as an explanans when further grounded in mechanisms and causal explanations.

## Conclusion

Henrich’s work raises an important consideration. He takes demographic cultures mostly for granted, merely showing that multiple measures using national cultures or ethnolinguistic

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<sup>24</sup> “Western, Educated, Industrial, Rich, and Democratic”. The extension of the term WEIRD is not without controversy. Though it points to a persistent comparative pattern among (predominantly) the *white people* in WEIRD nation states and other populations, the label itself excludes and marginalizes non-white populations that also reside into those nation states (Clancy & David 2019).

populations as cultural groups offer supporting evidence. But nowhere does Henrich defend a metaphysics of cultural groups. And as the ambivalence around national cultures and ethnolinguistic population suggests—he is open to multiple ontologies to boot. Does this suggest that empirical work can progress largely separate from overarching metaphysical commitments?

In large part, I think this is so. This is a point also raised by Okasha (2002). While researchers rely on notions of ancestry to ground the essence of species—placing them in appropriate genealogical relationships—a great deal of work in the life sciences looks at sets of facts (ecological relationships, geographical distribution, genome structure, dynamics of gene flow) that make little contact with genealogical ones. Relational essences might classify species, but much of the empirical action is focused on other clusters of facts.

The same might be true for demographic cultures. While relational essentialism might be important to *classifying* cultures, the *explanation* of salient features of those groups might appeal to different sets of facts and standards of application. Institutions like kinship structures, for instance, might help explain why one sees the clustering of particular norms, beliefs, and practices. As the case of species shows, this separation of explanatory and classificatory function shouldn't trouble us.

Overall, I think this paper has shown why demographic cultures are not troubled by the metaphysical, ontological, and explanatory claims of the skeptic. Intrinsic “hidden structure” essentialism may be incompatible with empirical research on cultural groups, but relational essentialism can ground the classificatory work of comparative work on culture. This metaphysical picture is in turn compatible with a wide range of operationalizations of “culture” and “cultural groups”. Though these operationalizations might have competing ontological commitments, this in itself does not render projects employing demographic cultures (or culture more generally) empirically bankrupt. If anything, it supports a varied and flourishing field of empirical research across the humanities, natural, and social sciences.

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