

Response to Vaisey¹

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This article is a response to Stephen Vaisey's (2021) article, "Welcome to the Real World: Escaping the Sociology of Culture and Cognition."

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INTRODUCTION

We are grateful to Karen Cerulo for inviting us to respond to Stephen Vaisey's always provocative and important work on culture and cognition, specifically, his critique of our recent article (Vaisey 2021). The subset of sociologists interested in culture and cognition, Vaisey says, have made their own world. This is, he argues, typical of sociologists, who would rather have their own one-ring circus than join a bigger one (perhaps because they are worried about being the clowns). Vaisey basically proposes that we in this field shut down, pack up the tents, and start instead rejoin a dialogue with those in the psychological sciences. We entirely and enthusiastically agree. Why is there such serious disagreement between Vaisey and us (as there is)? It is because we believe that Vaisey has not actually left the trampled, beer-stained, grass of the small circus. Rather than go to where the important work is happening, he is urging us to go to a failing circus next door, the one with the sick tiger: the obsolete work of the social psychology of the post-war period.

What Should We Take From Other Disciplines, and How?

Vaisey (2021) seems to have thought that, in our recent critique of the concept of values (Martin and Lembo 2020; henceforward ML2020), we were telling sociologists to "look inward to our own disciplinary traditions" rather than outward for our inspiration. If we misled any readers in this direction, we want to correct this impression with all possible haste. One might be surprised from reading Vaisey's piece to learn that our own arguments draw on articles from *Animal Behavior*, *Annual Review of Psychology*, *Cognitive Neuroscience*, *Consciousness and Cognition*, *Current Opinions in Neurobiology*, *Journal of Cognitive Neuroscience*, *Journal of Experimental Psychology*, *Nature Neuroscience*, *Nature Reviews*, *NeuroImage*, *Philosophical Transactions of*

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the Royal Society Series B—Biological Sciences, Psychological Bulletin, Psychological Science, Social Cognitive and Affective Neuroscience, Social Neuroscience, Psychological Review, Vision Research, as well as a number of journals on educational psychology, and chapters in compilations on primate neuroethology, social cognition, and human development. And, of course, we *cite* a number of social psychology journals, though not always positively. The position that Vaisey attributes to us is precisely the opposite of that which we intended to put forward.

At the same time, it is absolutely true that while we rely on what we believe to be robust *findings* from these disciplines, we do not simply adopt their theoretical approaches *tout court*. This is not surprising; we rely on sciences that would generally be understood as being at a lower level of analysis than sociology. The notion that one science should be compatible with the findings of that below it (biology being compatible with biochemistry, biochemistry with chemistry, chemistry with physical chemistry, physical chemistry with physics) is a very common one. We do not insist that such consistency is necessary for all sociological theory, but this conception of a hierarchy of sciences is conventional in sociology, and Vaisey has not suggested any flaws with this approach.

Instead of trying to construct sociological theories that *build on* theories of cognition at a “lower level,” Vaisey urges sociologists to adopt the theories of a discipline that is basically at a level similar to ours: that of a branch of non-experimental social psychology that we believe is weak and justly in a marginal position in current psychology. Even further, Vaisey proposes to stick with the ideas of Shalom Schwartz. We certainly do not think that it is impossible that the work of someone who got his doctorate in social psychology a half century ago could be of use for social scientists—after all, John Dewey had basically proposed the theory of dual processing a century ago, and the importance of this has only recently been sufficiently appreciated in (post-Parsonian) sociology, to no small degree thanks to the work of Vaisey—but it is hardly obvious on the face of it. Accordingly, our insistence that this research program has fatal weaknesses in its conceptualization need hardly be ascribed to some sort of xenophobic reaction in the face of the wider scientific world, as if we were attacking the theory of the cell. Of course, if we are wrong and Schwartz is right, so be it. But we think that Vaisey has only strengthened the evidence that this research program *is* based on poor conceptualization—both when he agrees with us, and when he disagrees.

Problems With the Conceptualization of “Values”

Our fundamental argument in ML2020 was that the *conceptualization* of the idea of “values” had been extremely weak, even by the generally tolerant standards of the social sciences. Those who are talking about values, no matter how good their work (and we do not deny the quality of some work in this line), like many other social scientists, literally do not (yet) quite know what they are talking about, because they have not been willing to pause in their production of findings to sort out the nature of their theoretical terms. That is, if there is some patterned social phenomenon lying at the heart of whatever processes produce the data that they

analyze, they have not correctly theorized its nature. It is not the worst thing in the world not to know the nature of one's key object of investigation—discovery often precedes conceptualization.⁴ Sooner or later, however, we must recognize that we lack an adequate conceptual grasp on the phenomenon, and be patient enough to attempt to ascertain its nature, before continuing to use it as an explanatory factor, a patience that has not been seen among those studying values.

We see the costs of this impatience in Vaisey's critique. Vaisey agrees with us that positing values as opposed to self-interest is incoherent—given that some of the *examples* of values, and most of the *definitions* of values, provided by values researchers, clearly include hedonic, even downright selfish, orientations as examples of *values*. Yet later, arguing against our recycling of a critique that values are not related to behavior as initially proposed,⁵ Vaisey counters “Moral and evaluative language has been study extensively ‘in the wild’” and there are robust predictive correlations—hereby (if we understand him correctly) making just that equation of *values*, *morality*, and *evaluation* that we thought he accepted as unjustified. Certainly, we never argued against the reality of *moral* or *evaluative* orientations. Instead, we argued that such phenomena were not clarified by appeal to *values*. We are in the position of arguing against the phlogiston theory and being countered by evidence that things in fact *do* burn.

And this was one of our central points—that values researchers were *defining* values in a very broad way (including hedonism), tending to *study* a small fragment of these (that are about abstractions, most of which seem praiseworthy, at least to the sorts of people who write and read this work), and then making *conclusions* about something that is disjoint from either of these first two sets—namely the realm of the moral. This is why we questioned why there is no *Fat* dimension in the conventional values inventory. Our argument was that items querying whether the respondent liked *bacon*, *cheese* and *chorizo* would probably successfully tap a shared dimension, and it would likely predict behavior (those who say they very much these can be expected to eat more fat than others). Does this make *fat* a value? Our argument is that the predictive validity is irrelevant to the conceptual issue of what this tells us about the psychological make-up of the human being. Vaisey goes on to argue against us that there is a convincing logic for Schwartz's particular choice of values coming from “three universal requirements of human existence: needs of individuals as biological organisms, requisites of coordinated social interaction, and survival and welfare requirements of groups.” We certainly have no intention to argue against this master-vision, but it is entirely irrelevant to the question of whether Universalism “is” a value. One could say nearly anything one wanted without it being impossible to put under such a vague umbrella.

Our example, in contrast, is far more concrete—and it appears to us better justified by the very logic to which Vaisey appeals than the Schwartz inventory of values,

⁴ We ourselves hope to contribute to sociological studies of *atmosphere*, and we are not (yet?) quite sure what we intend as the referent of this term.

⁵ We think that here, Vaisey somewhat misreads us—our argument was not that there was *no* evidence of values predicting behavior, but that there was a *split* in the treatment of values between the “elevated” ones that strike most of us as the sort of thing we intend when we use the term *values* but that *don't* predict behavior, and the “submerged” that *do* predict—but don't really seem like *values* as opposed to personality.

since it directly has to do with “needs of individuals as biological organisms.” While it is debatable as to, say, whether *recognition* is necessary for human existence, perhaps the most basic “needs of individuals as biological organisms” are caloric (you can do without protein for quite some time), and fat is concentrated energy. Most evolutionary scientists say that, for this reason, we do tend, in different degrees, to like the taste of fat. The question that we still await values researchers to answer is, is *fat* a value, and if not, why not? The *definitions* that they use, read literally, certainly imply that it is a value, but their actual work, and their interpretation of the significance of their work, implies that it is not. This seems as good a case as can be made that the inventories are arbitrary, or the conceptualization incoherent, as one might wish for. And if one tries to wiggle out of this by answering that *fat* is not a belief (Schwartz defines values as beliefs regarding criteria that can motivate action to goals), the same holds for other things treated as values (such as *security*)—one is always free to transform *fat* into a belief-statement like “fat is delicious.”

The problems of fundamental conceptualization of the notion of *values* should give sociologists pause before they attempt to deploy it—certainly, they cannot treat it as a safely “black boxed” concept, and will need to do their theoretical work (in the way that Hitlin, for one, has). But our argument here is not merely about Schwartz’s approach—we hold that this sort of conceptual weakness has actually been not merely characteristic of, but actually foundational to, the field that Vaisey urges us to take on as our guide—(mostly) non-experimental social psychology.

Naming and Being

We believe that, in his article, Vaisey assumes what is in question—namely, whether the Schwartz scale does measure values, and whether there are any such values to be measured. Certainly, the result of the application of the scale is *called* values. But more than this—a choice of an investigator as to how to term the sum of responses to a set of items—is required before one castigates others for ignoring the reality of the phenomenon (there really *are* values that have been shown to have this or that effect).⁶ We should recall that the great weakness of post-war American social psychology was a wholesale focus on *reliability* (classically, psychometric properties like a scale’s “alpha”) at the expense of *validity*. In practice, much of the theoretical leverage from such scales came from what the researcher chose to *label* them, and the pseudo-scientific ritualization of a science on the defensive led scholars to bury the details of what words they had actually used when querying respondents, and focus instead on the derived numbers.

⁶ Vaisey challenges us: if the Schwartz scale is so flawed, how come it consistently appears as predicted? But no one would deny that there are similarities and differences among people, such that, for analytic purposes, we can often position them in a multidimensional space. Further, it should not be difficult to find items that are disproportionately answered in a positive direction that correspond to different positions in this space, especially less common ones which will, in a spatial model, tend to be away from the center of the space. If one asks the same items or their equivalents, we would indeed not be surprised if a scaling program reproduces a projection of the location of the items in the original space. Nor would one be surprised that checking any box on a survey might have predictive value (especially regarding checking a *different* box on the same survey). None of this has any bearing on the question of what *sort* of conceptual entity is being tapped by the items.

For a wonderful example, in political psychology, part of the measurement of “authoritarianism” involved the complex of “conventionalism”—a mindless obedience to whatever others were doing. Ray (1988) pointed out that what the items actually seemed to capture was more like “old fashioned morality,” and not really *conventionalism* in any plausible sense. (For example, the extremely unconventional view of opposition to premarital intercourse had been coded as “conventional.”) Read one way, the correlation matrix was powerful evidence for an explanation of political attitudes as a result of pathological personality structure. Read another way, it was an unsurprising clustering of a hodgepodge of attitudes. The tendency of researchers in this tradition to make empirically unjustified assumptions to allow for the first, less parsimonious, reading, was a major set-back for social psychology. This is the key thing about an error of conceptualization: it doesn’t matter how many times you replicate the associations, nor what the p-values are—the conclusions remain flawed due to an unjustified labeling of the indices. We argue that an analogous leap to label various indices as “values” has proven a similar setback to serious investigation.

We have, then, argued that such misleading and unjustified labeling of scales has been at the foundation of the research program on values. We pointed out in ML2020 that while in Anglo cultures there does seem to be one leading contender for something that we ourselves confess seems like an abstract moral/ethical principle that can be invoked to force others to change their behavior, *fairness*, this is never included in any values inventories. Vaisey replies by drawing our attention to what Schwartz calls *universalism*, assuming both that there is such a trait and that it is fundamentally equivalent to “fairness,” and cites the following items: does the respondent think that it is important

- “• . . . that the weak and vulnerable in society be protected
- . . . never to think she deserves more than other people
- . . . to be tolerant toward all kinds of people and groups
- . . . to listen to and understand people who are different from her
- . . . that every person in the world have equal opportunities in life
- . . . to accept people even when she disagrees with them”?

We do not doubt that a scale composed of such items might have good predictive power. For example, it might predict voting for Joe Biden as opposed to Donald Trump, as it seems composed of rather conventional liberal pieties. We do not see what justifies calling this *universalism*, in contrast to a scale composed of more or less opposite items, say,

- “• . . . that all people in society be given the same consideration
- . . . to give to all their just desserts
- . . . to hold all to the same standards of behavior and observe the same rules
- . . . to be listened to by others, even those who are different from her

- . . . that every family be allowed to pursue its own way in the world so long as it does not interfere with others
- . . . to treat others as adults responsible for their own views and actions as opposed to patronizing them”?

This seems quite different, but one might imagine a right-leaning researcher calling this a *universalism* scale. And now consider one more. Does the respondent think that it is important

- “• . . . that the rich have their property expropriated by the state
- . . . never to think she can own the means of production
- . . . to support the masses against all of their enemies
- . . . to ignore traditional appeals that support reactionary forces
- . . . that every person in the world have equal outcomes in life
- . . . to denounce those who attack the forces of the masses”?

Interpreting this set in terms of *universalism* may seem even more of a stretch to many readers, but it does have a coherent theoretical grounding that has actually been explicated by past theorists (based on the argument that the proletariat is different from other classes, and is in fact a universal class, so its interests are, by definition, universal interests). Most readers presumably do not accept that the proletariat (should such a thing exist) *is* a universal class, but unless they are willing to defend the even more dubious proposition that the contemporary educated (“Brahmin”) class has such a position of universality, they will be unable to defend counting up the number of YES marks to the first scale as a measure of *universalism*, no matter what Schwartz or anyone else says.

Nor can it be claimed that the first scale is about fairness as the word is used in Anglo countries (we cited work specifically on this issue). *Fairness* is about an interchangeability of persons without reference to abstract principles. Tolerance may be a fine thing, or it may not, but no one would say that *A* is being unfair if *A* does not tolerate some *B* who does not tolerate *her*. Rather, *A* would be in a good position to say that it is *unfair* that she be asked to extend a courtesy that is not reciprocated. But it seems like the “universalism” scale would not count such reciprocation as +1, and so this scale is no more about *fairness* as generally understood than it is about *universalism*.

If the Schwartz set of items is about anything (and as Duncan [1984] forced us to accept, a scale can have fine conventional psychometric properties—properties which have to do with the pattern of co-holdings by respondents—without measuring *anything*), we propose that the content of the items seems to turn on that sort of self-ascribed *noblesse oblige* that characterizes the Brahmin classes. This content, in turn, may itself simply be an indicator of self-understood ideological position, and not interpretable in any literal fashion. What on earth do you think is actually in the mind of the person checking YES that he thinks it important that he “accept people even when [he] disagree[s] with them”? That he admit that they exist? That he not exterminate them? That he invite them over for a barbeque? Are the respondents

actually thinking of *anything at all* concrete enough to consider this as tapping *anything* other than self-placement in an ideological space? If the concept of *values* distracts serious researchers from noticing the slippage between the actual data they gather, and the theoretical constructs they hope to examine, that is as good evidence as one would want that it is a bad concept.

In sum, the success of this paradigm has been its conceptual sloppiness: it is only the conceptual weakness of the notion of *values* that has allowed for this remarkably strong transmogrification. The political ideology of one set of persons—a set of beliefs born in and used for the struggle for political power (even if we think that this is a struggle worth carrying out), to win over one's enemies—has, in the hands of credentialed social scientists, been reinterpreted without further ado as “universalism.” These are not the encouraging hallmarks of a path for sociologists to follow to greater scientific rigor.

What Fields Should We Draw on, and How?

We hope that we have not only defended our claim that the conceptualization of values in the tradition Vaisey discusses is unacceptably poor, but have pointed to a more general problem endemic in the field in which this theory was developed, for it is one that has a long-running and long-recognized set of theoretical and methodological problems. Indeed, it is a field that has, for the past forty or so years, been being pushed (perhaps for bad reasons as well as for good ones) outside of psychology proper and has managed to survive to some degree by relocating itself in business schools.⁷ While there is of course no shame in affiliation with such a school—some of the best sociologists will be found therein—it is not where one would expect cutting edge work in psychology. Indeed, the increasing tendency of professional schools to take mere renown (“digital footprint,” perhaps now even simply “clicks”) as criteria for tenure and promotion suggests that we should expect this to be an area in which the reform of social psychology away from its past tendency to false positives and irreproducible results takes place most slowly.

The orientation to headline-ready findings may be at odds with the sort of psychology that sociologists of cognition need to grapple with. Vaisey suggests that sociologists are insufficiently attentive to the findings of other sciences for reasons of social acceptance, but there seems a much simpler reason (in addition to the fact that very often other fields have explanatory orientations such that much of their work *is* indeed irrelevant to us). It is that many of us went into sociology because we didn't *like* some of the other fields. Nothing terrifies and depresses either of us more than organic chemistry, which combines the need for massive memorization of arbitrary codes with a vocabulary that is nearly pig-Latin and references to laboratory techniques that are unfamiliar to us. Staying up with such work is a slog at best. For example, the gentle introduction to a *review article* we currently are reading informs us that “The resulting ribonucleoprotein complex then surveils the host's cytoplasm

⁷ This is the location of Julie Anne Lee, the first author with Schwartz in the piece most cited by Vaisey; the second author with Schwartz most cited, Jan Cieciuch, is a professor of marketing at a business school.

for DNA and/or RNA sequences that are complementary to the spacer and flanked either by a PAM or a sequence lacking complementarity to the corresponding portion of the crRNA repeat” (Wimmer and Beisel 2020). Comprehending this sends us constantly scurrying back to the dictionary or to Wikipedia.

Struggling through such an article is not always fun, but it can be necessary if we want to understand the implications of cutting-edge work in current sciences for our own theories.⁸ In contrast is the extremely accessible literature to which Vaisey directs the reader: work that may seem very scientific because it uses terms like “evolution,” but may well be located outside any particular discipline that might enforce rigor (of course, some such work is central to its host discipline). There is nothing wrong with evolutionary approaches to culture, and there are undoubtedly excellent scholars in the Cultural Evolution Society. Further, we can imagine ideational inspiration coming from dialogue in such settings. But the question is whether, as Vaisey suggests, such interdisciplinary arenas will be appropriate reference groups—whether they will lead you to conduct your sociological work more rigorously.

We think that the evidence is against this. Although there are interdisciplinary groups that arise because of joint attention to complex problems, these tend to develop into thriving fields (a classic example is cognitive science), and we cease seeing them as “interdisciplinary” after a while. At the other extreme are failed fields that arise when a set of true believers insulates itself from critique as they pursue an *idée fixe*, whether it is Behavioral Genetics, Critical Realism, Creationism, or hardcore Marxism, which—whether or not it turns out to be true—is not convincing to most of those in the relevant subfields (see Aaron Panofsky [2014]). In between these extremes are more common cases of cross-disciplinary conjunctions that stretch too widely to be able to establish criteria for judging arguments other than the conclusions drawn (whether these are political positions, re-statement of assumptions, or what have you). However good their own approaches may be, we do not see why psychologists, primatologists, anthropologists, biologists, human ecologists, developmental psychologists, and of course social psychologists will be better than sociologists at knowing whether or not you have done your sociology well.

What they will of course know is whether you are finding the sorts of answers that they like to find, and, in this case it is important to note that—without casting aspersions upon the quest for elegant and simple models—the love of orthodoxy has long hampered evolutionary approaches in the United States; it was the Darwinians themselves who decided to term the [incorrect] axiom that DNA→protein and never the reverse their “central dogma.” Such a dogma was (perhaps) useful in narrowing down the sorts of evidence that should be examined, but Darwin himself had concluded that such an exclusion of the transmission of inherited traits was premature. From his sixth edition of *Origin* onward, he regretted his early dismissal of other mechanisms that could contribute to evolution. The fact that the mechanisms offered by Lamarck and others were scientific garbage, he realized, did not prove the impossibility of direct environmental influence and the transmission of acquired characteristics.

⁸ In this case, a biologist we talked to corrected an out-of-date idea that we had about the nature of the acquired immune system, and pointed us to current work in immunology.

Darwin was absolutely right. Since the 1980s, the responsiveness of mutation rates to environmental nutrient richness has been shown in bacteria. Even more—it is for this reason that we have been slogging through an article on the immune system in bacteria (yes, they have one, actually, more than one!)—it is not the case that only descendants of jawed fishes have acquired (as opposed to innate) immune systems, as long believed. The T-cell system is only one way that immune systems can learn from experience with disease. The CRISPR array in bacteria works more or less as follows: when a virus makes its way into the bacterium, if it lacks a certain signal present in the bacterium's own DNA, the bacterium takes a sample of the viral DNA and stores it in its own DNA, like a librarian keeping careful track of banned books. This is then passed on to its descendants (hence more or less a Lamarck-type process). This is where current science is (indeed, the 2020 Nobel prize)—not in people who continue to make up stories about why this or that could have, should have, would have evolved. If we are to look to other sciences for inspiration and for orientation, they should be serious and current sciences that have good track records for eliminating bad theories.

Sociologists *should* pay attention to other fields in general, and sociologists interested in cognition *must* stop making a theme-park miniworld of our own version. To do this, we must look to the work of the disciplines whose results might logically set bounds on our own arguments (in the way that cell biologists, or so we imagine, do not propose processes that are flagrantly at odds with chemistry). This requires that we keep up with and understand scientific work being done in cognitive science and in psychology (which is not to say that we should simply regurgitate it: we still must make and examine theoretical arguments that are our own, that go to the particular interests that we as sociologists have). And we can find other inspiration via analogy in cross-disciplinary contact as well. But we need not worry about groups or orientations that are highly speculative, highly simplistic, or, frankly, expired.

CONCLUSION

Above we made the comparison to phlogiston, and this was not a mocking reference. Rather, we think that there is reason to think that in the history of science, early conceptualizations of a phenomenon often invent overly convenient theoretical terms that are constructed to explain phenomena in the form of a latent or virtual pre-existence of the phenomena. Where does the fire come from? It must pre-exist in the wood in some dense, latent form, and be released. In some cases, such theories can turn out to be useful heuristics—for example, the notion of “preferences.”⁹ Values, we have argued, are such a first-guess type of conceptualization—one hopes that there is something on the *other* side of the skull that is just like the verbal productions (or check marks) that we have on *this* side.

⁹ The idea of revealed preferences is therefore very funny, as preferences are really nothing other than non-revealed actions! Few economists have ever been interested in *measuring* preferences as such, in part because such measurements may undermine further application of the theory (for example, they are often intransitive or otherwise sensitive to choice set in a way forbidden by the theory).

Such first-guess types of theories tend to be too strong—they may be immune to disproof (e.g., values are defined as “that which provokes persons to act” and then action is declared to be discovered to be value-laden). In addition, they often turn into individual attributes what are more properly seen as the results of social processes. That does not mean that there cannot be heuristic use to such terms, especially when it comes to formal modeling. But it is unlikely that progress in social science can be made if we resist a reflective critique of the logical and psychological status of the resulting concept(s). It is to such a critique (begun earlier by, in particular, Ann Swidler [1986]) that we have attempted to contribute.

The wonderful thing about this debate—and it is a serious opposition—is that we are all on the same side.¹⁰ We all want a sociology the sits at the grownups’ table, that can be in informed, if critical, dialogue with other fields, as opposed to insulating itself from them, and that holds itself to the highest standards. We believe that Vaisey (and his students) have been and continue to be central contributors to that development—not only in their work on cognition, but their other work on theory and on methodology. However, the debate on values plays out, whether they are rejected or made the center of sociological conceptions of action, if the decision is made on the basis of cumulative, cogent, rigorous and self-critical research, we will all be delighted.

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¹⁰ Indeed, there may be indications of a possible future rapprochement. Vaisey ends the critique by suggesting that intension, attention and extension are part of the phenomenology of valuation (and appropriately cites Joas). Since we always accepted the conceptual coherence of the notion of valuation, it might be that what we are seeing here is actually a convergence—an acceptance that *values* (abstract beliefs—recall this is how Schwartz defines them) are actually the abstract, possibly misleading, tip of an iceberg that has a different nature. Understanding the nature of the iceberg will be crucial for understanding the nature of action and moral reasoning.