

Yes, We Still Need a Concept of Culture

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Comment on Poortinga, Y. (2015). Is "Culture" a Workable Concept for (Cross-)Cultural Psychology? *Online Readings in Psychology and Culture*, 2(1). <http://dx.doi.org/10.9707/2307-0919.1139>

In his paper, Ype Poortinga has raised a series of doubts as to the usefulness of the concept of culture to our field. It seems paradoxical to me that from within his own skeptical gaze he loses sight of the way that skepticism and doubt are some of the key tools of scientific advance. He identifies a series of theories and generalizations that have been found through later research to be formulated in terms that are overly simple. I would say that in each of the areas that he addresses, rather than requiring us to abandon the concept of culture, the failure of earlier theories has led us toward more sophisticated theorising about culture.

Ype suggests that we have been too focused upon finding cultural differences rather than looking for universals. Such a polarisation of choice is not helpful: we increasingly see the need to do both at once. Hofstede's (1980) mapping of cultural differences in terms of national differences provided a framework that helped us toward an empirical classification of the ways in which nations differ in terms of psychological constructs. Ype points to the failures of Oyserman, Coon, and Kimmelmeier (2002) and of Matsumoto (1999) to identify differences in Hofstede's most frequently cited dimension, namely individualism-collectivism. However, these failures were due to the use of severely flawed measurements, as shown elegantly by Schimmack, Oishi, and Diener (2005). There is a multitude of other evidence that national differences in collectivism can be found and measured (Taras, Kirkman, & Steel, 2010), though they have of course been over-interpreted and remain ill-defined. For the moment, let's call them cultural differences and debate definitions later. Whether we must accept Ype's view that they are overgeneralised must depend on how frequently they have proved replicable, especially where representative samples have been drawn from each nation.

Within later approaches such as that of Schwartz (2009) we can compare the variability of more precisely delineated values between individuals and larger cultural entities such as nations, and we find that these contrasts persist, for instance, when testing is done through the representative sampling entailed in the European Values Survey (Schwartz, 2004). Now we also have empirical confirmation that individual-level variability is much greater than nation-level variability (Fischer & Schwartz, 2011). However, that does not mean that nation-level variability is trivial or unimportant. We are increasingly able to use statistical techniques such as hierarchical linear modeling (Raudenbush, Bryk, & Congdon, 2007) so that we can compare the explanatory power of variation at each level. We find for

instance that across 23 European nations, variations in levels of trust are better explained by nation-level value indicators than by personal values (Gheorghiu, Smith, & Vignoles, 2009). In a similar way, across 21 samples from 18 nations, the ways in which individuals seek to accomplish their needs for distinctiveness are explained by sample-level value means, not by individual-level means (Becker et al., 2012). Variations in ways of satisfying motives for self-esteem show a similar pattern of effects (Becker et al., 2014). Note that these last two studies point toward an escape from excessive reliance on nations as definers of cultural difference.

Ype rightly points out that much research into cultural differences is essentially correlational in design, so that we are no wiser as to the ways in which causal relations may be involved. Issues of this kind are increasingly being addressed by cross-cultural psychologists either through the use of experimental priming, or through longitudinal data collection. Priming studies can identify causal effects that *might* occur in the real world, but longitudinal studies can identify effects that can actually be seen to occur. For instance, in the study cited above, Becker et al. (2014) showed that adolescents who gained in self-esteem over time were better predicted by having values that accorded with the values of their sample rather than by their own values, where both sets of values had been measured some months earlier. If this is not a cultural effect, Ype will need another word to describe it.

Ype suggests that the finding by Yamagishi, Hashimoto, and Schug (2008) that Japanese (like Americans) prefer to choose a unique object rather than a common object is a challenge to the concept of collectivism. Since the Japanese preference only became apparent when the experiment was set up in such a way that respondents could choose the unique object without impeding the choices of other experimental participants, this is not a very convincing argument. I would prefer to interpret the results of Yamagishi et al. (2008) in terms of the way in which they illuminate the interplay of individualistic motives and the norms of a relatively collectivistic culture, much as the original authors of this study did. Ype also argues that collectivism theory is challenged by the fact that no differences in reported closeness to one's family and willingness to support them was found between members of two individualist and three collectivist cultures (Fijneman et al., 1996). This argument certainly indicates the need for clearer thinking about individualism and collectivism. Hofstede did not propose that members of individualistic cultures would not feel close to their families. He focused on differences in the *motives* that we have for our involvement in different kinds of groups.

In another section of his paper, Ype finds it important that early assumptions that persons in different cultures experience different emotions has proved false. It is good that we are now able to characterize the range of human emotion more adequately, but doing so in no way requires us to abandon the concept of culture. We have good evidence of wide variations in the extent to which emotions are elicited (Scherer & Brosch, 2009) and the circumstances governing emotional expression in different cultural groups (Matsumoto et al., 2008), and we need to develop and test hypotheses more fully as to why such variations occur. Variations in collectivism gave us first crude approximations, but we can do better if we think more clearly about the types of theories that we require.

Until recently, theorists of culture have failed to provide any substantive historical or dynamic context for their delineation of contrasting cultures. “Culture” has been seen as a fixed, disembodied, and *proximal* cause of all that goes on in and around it. Such formulations are inevitably circular, as Ype would be the first to agree. What we have needed are formulations that identify *distal* factors that predispose the emergence and continuing reconstruction of particular cultural adaptations. Recent formulations of this type include Gelfand and colleagues’ (2011) identification of factors leading toward tight versus loose cultures and van de Vliert’s (2009) theory predicting the combinations of climate and wealth that both limit and foster specific cultural effects. Ype would have us conclude that if national variations in happiness can be predicted from levels of national wealth, then we can dispense with the concept of culture. However, we do better to retain it, because we need among other things to understand where wealth comes from, and socio-cultural theories such as that of van de Vliert begin to address the mutual interactions of climatic challenge and the culturally constructed sets of beliefs that are entailed in the subjective concept of what we now all agree to call “objective” wealth.

There are few concepts in the social sciences that are not the subject of extensive debate and reformulation, so there is little to fear about the current lack of consensus in defining culture. A concept is a tool, and tools can be used for a variety of purposes. Ype tends to pick examples where culture has proved a less than useful tool that are drawn from basic aspects of human experience such as vision, nutrition, and color perception. The concept may be better formulated to address aspects of social behaviours, as he himself has argued in the past (see Poortinga, 1992). So, what sorts of conception of culture can still serve us well? We need first to say where it is useful to consider culture. Compared to the situation a decade or two ago, most authors now see culture as located in the minds of culture-members, not just as a property of nations or other large groupings. However, that need not mean that culture simply becomes another word to describe the individual beliefs, values, or habitual modes of information processing of particular culture-members. We get closer to a useful conceptualization if we consider it as an awareness, implicit or explicit, of the beliefs, values, norms, and effective behavioural procedures operating within one's varying social contexts. We may have been able to tap into these contexts to some extent by sampling nations, but we should be able to do much better by sampling more proximate groupings. National cultures are a jumbled assemblage, for instance, of the cultures of regional, occupational, religious, ethnic, organizational, and gender-based groupings. Earlier definitions of cultures in terms of shared meanings are no longer tenable at the nation level, because meanings are frequently contested at the nation level. At less macroscopic levels, shared meanings are likely to be increasingly frequent. There is now room for debate as to whether we are more guided by our subjective perceptions of prevailing social norms, or by any objectively definable shared meanings, as explored in the special issue of the *Journal of Cross-Cultural Psychology* to be published early in 2016. There is also room for debate and research as to the extent to which cultures are in process of continuous recreation rather than being deeply embedded and resistant to change. However, doing away with the concept altogether would be a reductionist strategy that would leave us ill-

equipped to understand the ways in which the universals of humankind find varying forms of expression in different contexts.

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On Individualism-Collectivism, Generalization, and the Need for “Culture”

A Reply to the Comment by Peter Smith

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Peter Smith (2016) takes a clear position in his comment on my earlier essay in ORPC (Poortinga, 2015). Contrary to what I proposed, Peter argues that culture is a concept that cross-cultural psychology should *not* try to do without. Such a viewpoint by a senior in the field of cross-cultural psychology who has conducted outstanding research cannot be taken lightly.

Recent research by Peter that I am impressed with described terms such as *guanxi* in China, *wasta* in Lebanon, and *jeitinho* in Brazil that denote informal social influence

processes (Smith et al., 2012; Smith, Huang, Harb, & Torres, 2012). My enthusiasm for this work has to do with the, for cross-cultural research, unusually careful design, crossing scenarios of, for each term typical situations with samples, and providing additional checks on the meaning of the scenarios. Levels of endorsement of scenarios were rather similar across societies and largely independent of the country where a scenario came from. I imagine that for effective communication in China it helps to have a detailed understanding of the situations where the term *guanxi* applies and its precise connotative meanings, and similarly in Brazil for *jeitinho*, etc., but the findings make it plausible that outsiders can grasp what these terms mean in the societies where they are used.

There seems to a bit of a gap between this meticulous research and Smith's defense of individualism-collectivism (ind-coll). Overviews in the literature that Peter refers to take a *convergent* approach to the available evidence, and in my opinion make no serious attempt to really challenge the ind-coll dimension; attempts at finding *discriminant* evidence are largely absent. Since the classic article by Campbell and Fiske (1959), we know that construct validity needs both kinds of evidence. Following Peter, I can refer to Schimmack, Oishi, and Diener (2005) who point to dismally low correlations between ind-coll measurements in what they call the "Hofstede tradition" and measurements with ind-coll scales on which the large review of ind-coll by Oyserman, Coon, and Kimmelmeier (2002) was based. Schimmack et al. (2005) see the resolution of this contrast in corrections for response styles. However, this throws doubt on the validity of all findings based on uncorrected data, which make up a substantial part of the overall record on ind-coll. Schimmack et al. (2005) conclude that Hofstede's ind-coll dimension is related to national development and affluence, but this is not sufficient to establish the dimension as a broad *value* dimension. The large overview of Taras, Kirkman, and Steel (2010), also mentioned by Peter, examines the *impact* Hofstede's work has had, but definitely not the validity of Hofstede's instrument. On the other hand, there are explicitly critical views on the validity of ind-coll (e.g., Sinha & Tripathi, 1994; Voronov & Singer, 2002) and on Hofstede's Values Survey Module scales (Spector, Cooper & Sparks, 2001) that tend to be ignored in international survey research. Peter ascribes the inconsistency of past results to poor measures, but we should not forget that these measures have been foundational to the conceptualization of ind-coll. The question needs to be raised what the implications should be of the rather apparent conceptual weakness; a question that has been asked before in various ways by respected scholars in our field, like Jahoda (e.g., 2011) and Segall (1996). Peter appears to agree that more precision is *desirable*: "Construal and measurement of more specific attributes rather than broad inclusive categories such as individualism-collectivism would be more useful" (Smith, Bond, & Kagitcibasi, 2006, p. 268). In my view, more precision is *mandatory*.

In Poortinga (2015), I suggested that affluence (GDP/cap, GNI, etc.) with its consequences for variations in education, social stability, personal security, etc., is the most important antecedent of national differences in social and psychological variables. This also pertains to value dimensions. For example, at the regional level in Europe ($N = 195$ regions) van Herk and Poortinga (2012) found with four antecedent variables the largest effect for GDP on both value dimensions – conservation and self-enhancement – of the Portrait

Values Questionnaire (PVQ; see Schwartz, 1992; Schwartz et al., 2001). On the original, longer scale of Schwartz, the Schwartz Value Survey (SVS, Schwartz, 1992), correlations with GDP are also evident (Fontaine, Poortinga, Delbeke, & Schwartz, 2008). The SVS analyses of internal structure have provided evidence of construct validity (e.g., Fontaine et al., 2008), and the extensive set of value terms that makes up this instrument speaks to its content validity. It may be noted that correction for the confounding of country value scores with estimates of affluence is not simple, since we do not have a good idea how to model such confounding. This implies that van Herk and Poortinga (2012) may have shown a bias towards economic differences, but it could also imply that values are given preference in studies such as those by Becker et al. (2012, 2014) in which Smith was a coauthor. Probably the confounding has become less critical since we now have information on two important points: country differences on the SVS largely correspond (are isomorphic) with individual differences within countries (Fischer, Vauclair, Fontaine, & Schwartz, 2010), and country differences are small relative to these individual differences (Fischer & Schwartz, 2011). I agree with Peter that a small proportion of variance *can* be important, but not by prior assumption; explicit evidence is needed as to why “small” does not amount to “unimportant,” and such evidence has to come from estimates of substantive differences in distributions of causally related variables. For ind-coll it seems questionable whether such evidence is available.

For Peter it is an important argument in emphasizing the need for the concept of culture that there is variance at nation-level which is not explained at individual level, examples being provided by Becker et al. (2012, 2014) and Gheorghiu, Vignoles, and Smith (2009). I certainly agree that there are important differences between countries and also between other identifiable populations, such as the wealthy and the poor or literates and illiterates. The clearest example that Peter mentions is the study on generalized trust by Gheorghiu et al. (2009). To me, this is an instance of a psychological variable where direct relationships with GDP are plausible. A case in point: the annual corruption perceptions index (CPI, <http://www.transparency.org/cpi2015>) is strongly related to national affluence. In many low-income countries, courts, civil servants, and the police often can act with impunity and citizens realize this. Are people in India, Indonesia, or Mexico inherently more suspicious than in Denmark, New Zealand, or Singapore? Looking at the three Likert-scale items used to measure generalized social trust in Gheorghiu et al. (2009), I can well imagine that in the former three countries low scores are pretty realistic: *others can be trusted* (0 = you can't be too careful), *others try to be fair* (0 = most people would try to take advantage of me), and *others are helpful* (0 = people mostly look out for themselves). Other examples of large country differences in mean scores include (aggregated) self-reports of happiness (see Veenhoven, 2015) and Subjective Well-Being (SWB). Diener, Ng, Harter, & Arora (2010) have reported a positive relationship (with an asymptote) between economic prosperity and SWB. To prevent any misunderstanding, GDP is not the only variable that matters; Tov and Diener (2007) also showed that some societies have higher SWB than expected given their GDP, and that social prosperity (a social network and trust) explains part of the variance. Such “irregularities” are secondary to the main trend, even though they are fascinating and deserve further analysis.

As I indicated in Poortinga (2015), much of the argument turns around the level of generalization at which we postulate explanatory psychological concepts in cross-cultural psychology. With broad and inclusive concepts or dimensions, such as ind-coll, we are quite close to the super-inclusive concept of culture that fits more or less every explanation. An alternative orientation is to focus on less generalizable but more transparent relationships. For example, there is a host of findings in our literature suggesting the relevance of situational contingencies, with people being realistic about the “situation” in which they find themselves and their power to deal with it. GDP per capita is one example and trust is another; further examples include the confident reliance of marginalized hunter-gatherers on their sense of direction when walking large distances (e.g., Reuning & Wortley, 1973) and the agreement among various groups in a society about their position in the societal pecking order (e.g., De Ridder & Tripathi, 1992; Galchenko & van de Vijver, 2007).

Peter also refers to the shift in the literature from values to norms (Gelfand et al., 2011). Norms -- including perceived norms -- are more concrete than values, and this can be a step in the right direction, provided no underlying values are inferred, as Peter seems to do in his discussion of the results of the pen experiment of Yamagishi, Hashimoto, and Schug (2008). A move away from broad cross-cultural differences may also advance more precise understanding in other domains, including emotions. There rules for expression (“display rules”) in specific situations may lead to large differences in manifest expression, while the factorial structure of the emotional domain is more or less invariant (see Fontaine, Scherer, & Soriano, 2013).

Peter is right that in the past I have tried to come up with a definition of culture, i.e., “[c]ulture becomes manifest in shared *constraints* that limit the behavior repertoire available to members of a certain group in a way different from individuals belonging to some other group” (Poortinga, 1992, p. 10). Ten years later the notion of constraints was complemented with *affordances* that were defined as the space of alternatives bounded by constraints (Poortinga & Soudijn, 2002; see also Berry, Poortinga, Breugelmans, Chasiotis, & Sam 2011). These contributions to the Babel-like confusion of tongues were seeking to balance invariance and variation, while not specifying any substance or content for culture. As substantive or operational definitions, these formulations are largely untestable, and I am happy to give them up together with the concept of culture.

In conclusion, I have mentioned the work by Peter and colleagues on notions like *guanxi* and *wasta* as an example of research with a rather strict design. In the articles reporting this research, there is little invocation of grand psychological concepts or dimensions, like ind-coll. I submit that precision in conceptualization, methodology, and interpretation will help to move cross-cultural psychology forward. As argued in Poortinga (2015), a self-imposed embargo on the use of the term “culture” in cross-cultural psychological research may help researchers to formulate more precisely what the target variables and populations are in a particular study, making superfluous the use of a concept that has an undetermined range of meanings.

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