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## **Dimensions do not exist: A reply to Brendan McSweeney**

*Geert Hofstede*

In January 2001 *Human Relations* invited me to write a response to an article by Brendan McSweeney which was a critical examination of my 1980 book *Culture's consequences*, to coincide with the forthcoming publication of the books' second edition. I reacted enthusiastically, but my enthusiasm quickly faded away when I saw McSweeney's diatribe. I pointed out that the appearance of a re-written and updated edition of my 1980 book would make many of McSweeney's comments obsolete. Also, I reacted to his style, which I found unnecessarily abrasive.

*Human Relations* decided to publish McSweeney's article anyway, in a somewhat mollified version. My response to his comments follow below.

The second edition of *Culture's consequences* contains a section: 'Support and Criticisms of the Approach Followed' which reads as follows (endnotes omitted):

The first edition of this book's disrespect for academic borderlines paid off in a multidisciplinary readership. It also caused very mixed reviews: Some enthusiastic (e.g. Eysenck, 1981; Triandis, 1982; Sorge, 1983), some irritated, condescending, or ridiculing (e.g. Cooper, 1982; Roberts & Boyacigiller, 1984). I had made a paradigm shift in cross-cultural studies, and as Kuhn (1970) has shown, paradigm shifts in any science meet with strong initial resistance.

### *Editor's Note*

*This exchange has been prompted by interest in and response to the original McSweeney article in Vol. 55, No. 1 (January 2002) of the journal. The Editors regard this exchange as now closed.*

Five standard criticisms of my approach were:

1. Surveys are not a suitable way of measuring cultural differences (my answer: They should not be the only way).
2. Nations are not the best units for studying cultures (my answer: True, but they are usually the only kind of units available for comparison and better than nothing).
3. A study of the subsidiaries of one company cannot provide information about entire national cultures (my answer: What was measured were *differences between* national cultures. Any set of functionally equivalent samples from national populations can supply information about such differences. The IBM set consisted of unusually well matched samples for an unusually large number of countries. The extensive validation in the following chapters will show that the country scores obtained correlated highly with all kinds of other data, including results obtained from representative samples of entire national populations).
4. The IBM data are old and therefore obsolete (my answer: The dimensions found are assumed to have centuries-old roots; only data which remained stable across two subsequent surveys were maintained; and they have since been validated against all kinds of external measurements; recent replications show no loss of validity).
5. Four or five dimensions are not enough (my answer: Additional dimensions should be both conceptually and statistically independent from the five dimensions already defined and they should be validated by significant correlations with conceptually related external measures; candidates are welcome to apply).

Since the later 1980s the idea of dimensions of national cultures has become part of what Kuhn called “normal science.” The message of the first edition of this book has been integrated into the state of the art in various disciplines dealing with culture. The four or five dimensions I introduced have become part of intercultural training programs and of textbooks and readers in cross-cultural psychology, organizational psychology and sociology, management and communications. They have also been used in a number of other areas and disciplines; these will emerge in the following chapters, and Chapter 10 will summarize some of the more surprising applications.

In fact, this extensive use has its disadvantages. Some people have tried to imitate my approach cheaply for commercial purposes. Some carry

the concepts further than I consider wise. At times my supporters worry me more than my critics. But fortunately the message has also reached serious academics and practitioners who carry on with research and experimentation in intercultural cooperation, to meet the crying need for integration of human efforts in a shrinking world.

(Hofstede, 2001: Ch. 2, p. 73)

McSweeney's article reiterates some of the old comments, mostly from the categories 1, 3, and 4. He focuses very much on details of the analysis of the IBM database, but does not write a word about the validation of the country differences in the IBM study on other data. It all started at IMEDE, where I taught on a leave of absence from IBM. IMEDE (now IMD) is an international business school in Lausanne, Switzerland. The 1980 edition of *Culture's consequences* contains the following text (which McSweeney could have read):

I taught courses in organizational behavior at IMEDE from 1971 to 1973. By that time it had become clear that certain questions in the HERMES questionnaire which could be expected to express values produced stable and predictable differences in answer patterns among countries. I included in my IMEDE courses the administration of a 17-item "Questionnaire on Work Goals and Preferences" which contained questions . . . of the HERMES questionnaire; I used the results as teaching material in the course itself. Answers on this questionnaire were obtained from 362 managers from about 30 different countries and from a variety of private and public organizations unrelated to the HERMES Corporation. As will be shown in Chapters 3 and 5, the major country differences found in HERMES are also visible in the IMEDE sample. The latter is based on a different population, and all respondents reacted to the English version of the questionnaire, whereas in HERMES every nationality received its own language version. The similarity between HERMES and IMEDE data therefore also ruled out the hypothesis that the differences found among countries could be due to the translation of the questionnaire.

(Hofstede, 1980: Ch. 2, p. 68)

'HERMES' was the nickname for IBM used in the early versions of my work, when IBM had not yet agreed to make its identity public. The IMEDE experience was the first external validation of the country differences already identified in IBM. It provided statistical proof that a significant part of the differences in answers on the IBM questions were due to the nationalities of

the respondents. To me this was the starting point of an exploration of other cross-national differences that might be expected to relate to the IBM scores. Most of the 1980 book is devoted to these validations – how could anyone, including McSweeney, possibly claim to have read this book without noticing it? They consist of cross-national survey and test data from other studies, including a number of representative samples of entire national populations, and of indicators measured at the country level, such as GNP per capita, income inequality and percentage of the national budget of wealthy countries spent on development assistance to poorer countries. All validations are summarized on pages 326–31 of the 1980 book, a total of some 90 significant and independent correlations.

Precisely these validations were the reason why so many academics in different disciplines felt stimulated by my work; these people added more validations, and contributed to the overall picture. They did so because of its possibilities to further their analysis, not because of some kind of faith, as McSweeney suggests. Their work has been reviewed for the 2001 second edition. More than half of the over 1500 sources in the 2001 reference list were published after the first edition appeared. The count of significant and independent correlations has grown to more than 400. The validations are now in Appendix 6. Besides, there have been more straight replications of the IBM surveys, using the same questions on different populations (as was done at IMEDE). My 2001 book describes four large-scale replications covering between 15 and 32 countries, on country elites, employees of other organizations, airline pilots and consumers. After the completion of the 2001 edition, new large-scale replications were published on civil servants (Mouritzen & Svava, 2002) and on employees of a multinational bank (van Nimwegen, 2002). Replications usually confirm most, but not all of the dimensions, but different replications confirm different dimensions.

This leads me to McSweeney's allegations of my supposed rigidity, such as holding 'the notion of a mono-causal link between national cultures and actions within nations' (McSweeney, 2002: 109). But this rigidity is in the eye of the beholder. In my 1980 book, where I introduced the term 'mental programs' to include both 'values' and 'culture', I wrote:

It is possible that our mental programs are physically determined by states of our brain cells. Nevertheless, we cannot directly observe mental programs. What we can observe is only behavior: Words or deeds. When we observe behavior, we infer from it the presence of stable mental software. This type of inference is not unique to the social sciences; it exists, for example, in physics, where the intangible concept of "forces" is inferred from its manifestations in the movement of

objects. Like “forces” in physics, “mental programs” are intangibles, and the terms we use to describe them are *constructs*. A construct is a product of our imagination, supposed to help our understanding. Constructs do not “exist” in an absolute sense: We define them into existence.

(Hofstede, 1980: Ch. 1, p. 14)

In the first session of a new student class, I used to write big: CULTURE DOESN'T EXIST. In the same way values don't exist, dimensions don't exist. They are constructs, which have to prove their usefulness by their ability to explain and predict behavior. The moment they stop doing that we should be prepared to drop them, or trade them for something better. I never claim that culture is the only thing we should pay attention to. In many practical cases it is redundant, and economic, political or institutional factors provide better explanations. But sometimes they don't, and then we need the construct of culture.

Also, the validations of my dimension scores do not imply assumptions about causality: validations can point to causes, effects, or association based on circular causation or on hidden third factors. Circular causation applies to the relationship between national culture and national institutions, illustrated in the diagram on page 27 of the 1980 book.

McSweeney misses the point completely about our research on organizational cultures. This was a separate large-scale project carried out in the 1980s across 20 organizational units in Denmark and the Netherlands. The full report was published in an article in *Administrative Science Quarterly* (Hofstede et al., 1990), which McSweeney lists in his references but does not refer to in the corresponding section of his text (pp. 96–7). (For unclear reasons he lists, but does not use, a number of my other publications as well.) I did not ‘begin to belatedly acknowledge that there is cultural variety within and between units of the same organization’ (p. 96). We had planned this research for years – it was the logical sequel to the cross-national study. And, what if I had acknowledged it belatedly? On page 90, McSweeney reproaches me with never having changed my mind.

The organizational culture study tried to identify the values component that differentiated organizations within the same country rather than similar organizations across nations. Contrary to our original hypothesis we found only a weak values component, but strong differences in what we labeled as ‘practices’. For a description of what we meant by that, McSweeney should read the article. If he wants to define practices differently, fine, but then we are talking about something else. The practical consequences of the fact that the national culture component relates primarily to values, the organizational

component to practices, are far-reaching. Values (as we measured them) are hardly changeable (they change but not according to anybody's intentions), whereas practices can be modified – given sufficient management attention. This explains why a multinational like IBM could function at all, in spite of the considerable differences in values, which my research revealed. What holds a successful multinational together are shared practices, not, as the 'corporate culture' hype of the early 1980s wanted it, shared values.

McSweeney's criticism of my interpretation of survey data (his pages 100–6), if correct, applies to all survey and test-based cross-cultural studies, including those of Schwartz, Triandis, market researchers, sociologists and political scientists around the world. All of these draw conclusions from central tendencies calculated from individual survey answers. There is no creative accounting in the way I treated my data, I followed common practice and moreover in the 1980 and 2001 books provided all the data by which others can verify my findings. What we social scientists all do is called statistical inference, but McSweeney is obviously unfamiliar with it.

To conclude, let me cite from a review of my work on culture by Malcolm Chapman, British like McSweeney, but an anthropologist, not an accountant:

... Hofstede's work became a dominant influence and set a fruitful agenda. There is perhaps no other contemporary framework in the general field of "culture and business" that is so general, so broad, so alluring, and so inviting to argument and fruitful disagreement. . . . Second, although Hofstede's work invites criticism on many levels, one often finds that Hofstede, in self-criticism, has been there first. Third, although Hofstede's work is based on a questionnaire drawn from social psychology that was not expressly designed for the purpose to which it was later put, Hofstede brings to his discussion such a wealth of expertise and erudition from outside the questionnaire that many criticisms of "narrowness" are withered on the tongue.

Hofstede's work is used and admired at a very high level of generalization. Those who take country scores in the various dimensions as given realities, informing or confirming other research, do not typically inquire into the detail of the procedures through which specific empirical data were transmuted into generalization. Hofstede, of course, provides all the background one could wish for about these procedures, and that is another reason for admiring his work.

(Chapman, 1997: 18–19)

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