THE PERILS OF PARSIMONY

“National culture” as red herring?

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Abstract. This paper discusses the ways in which Hofstede’s model of ‘dimensions of (“national”) culture’ – and similar models developed in a functionalist paradigm – are problematically used to classify people. It briefly surveys critiques of Hofstede’s research method, but focusses on the dangers of attempting to develop models of culture within a functionalist paradigm. Although such models may be parsimonious and rapidly applied, I argue that they are a poor fit for CATaC investigations of the dynamic and reciprocal interactions between human cultures and technology. Instead, I contend, we must abandon this paradigm, and embrace methodologies that permit meaningfully explorations of the multiple and dynamic conditions influencing the field of cultural practices in human societies. I discuss the merits of ‘articulation’ as theory and method, and offer Hacking’s theory of “dynamic nominalism” as one example.

Résumé. Ce travail décrit comment le modèle des ‘dimensions de culture nationale’ de Hofstede (ainsi que d’autres modèles similaires dans un paradigme fonctionnaliste) est utilisé de façon problématique pour classifier les gens. J’examine brièvement les critiques de la méthodologie de Hofstede, mais je me focalise sur les dangers de développer des modèles de culture dans un paradigme fonctionnaliste. Bien que ces modèles peuvent être parciornieux et appliqués rapidement, je soutiens qu’ils ne marchent pas pour les investigations CATaC des interactions dynamiques et réciproques entre les cultures humaines et la technologie. Au contraire, j’affirme que nous devons abandonner le paradigme fonctionnaliste et considérer des méthodologies nouvelles qui nous permettront d’exéplorer les conditions dynamiques et multiples qui influencent les pratiques culturelles dans les sociétés humaines. Je discute les mérites de l’articulation comme théorie et méthode, et j’offre la théorie du «nominalisme dynamique» de Hacking comme exemple.
1. Introduction: The Problem with Classifying People

I worry about classifying people. The nature of my worry is illuminated by Hacking (2006), who distinguishes usefully between ‘kinds of people’ and ‘ways of being a person’.

Hacking (2006) describes how we tend to think about ‘kinds of people’ as “definite classes” with “definite properties” (p. 2), especially when such classes of people are the objects of scientific inquiry: the obese, the poor, suicides...the Japanese, the Australian. Worse, Hacking explains that the “engines of discovery” (p. 3) that we routinely employ in the human sciences (ranging from sociology to medicine) are actually responsible for ‘making up people’ – they literally create the kinds of people that we then claim to have identified, and that did not previously exist. What are these engines of discovery? Statistical analysis is primary: we count and quantify, we determine norms and assess spectra of deviations, and we calculate correlations in order to objectively divide humanity into ‘kinds’. Medicine, biology and, more recently, genetics, are employed to try to uncover root causes and explain underlying defects that manifest as ‘kinds of people’: the autistic child, the homosexual. We use our engines of discovery to make up kinds of people...and then, too often, these new forms of classification become fixed in our collective understanding as universal and essential, descriptive of unchanging differences between peoples. (Indeed, there exists a school of thought in cognitive sciences that maintains that essentialist thinking is innate to the human mind, that we are unable to avoid it).

This is not to argue that different groupings of people throughout history have not shared or do not share common features; but it is an attempt to flag the dangers of relying on such “static nominalism” (Hacking, 2006, p. 3). Conceptualizing people in “species mode” (ibid., p. 13) depersonalizes them, turning them into objects for scientific study, bureaucratic management, treatment, or control. Methodologically, by adopting such thinking we fall into the trap of believing that naming a problem, or group, or phenomenon, effectively solves it – the nomothetic fallacy.

Although beyond the scope of this paper, Hacking’s extensive work that has detailed the making up of different kinds of people over historical time convincingly supports his argument that “there is no clear and distinct class of human kinds, and no vague class either” (2006, p.6). Most significantly (see below), Hacking highlights the complex ways in which classifications interact with the people classified – a phenomenon he calls “looping”. “Our investigations interact with the targets themselves, and change them,” he argues (ibid, p. 2), making them not quite the people they were before. Groups of people are therefore, he notes, “moving targets”, in dynamic relationship with the names we give them and the ways we (embedded in our network of power relations) define them.

By differentiating between ‘kinds of people’ and ‘ways of being a person’, Hacking allows us to conceptualize in a different way what is signified by the classifications of human kinds that our discovery engines produce. Accepting that ‘real kinds’ of humans do not exist, it becomes meaningless to investigate whether such and such a kind of people (Italians? homosexuals?) existed in the past, whether such kinds will exist in the future, or whether we can identify universals that comprise their essential nature. Equally, it is meaningless to consider such a grouping as a static and homogenous cohort of ‘cultural dopes’ that interacts with the world as a pinball
interacts with the bells and paddles of a pinball machine: moved around but fundamentally unchanged.

Instead, a focus on ‘ways of being a person’ allows us to examine the specific and situational conditions and social forces that have brought such a grouping into being, and with which it continues to interact. It becomes possible to say:

Before time $t$, X was not a way to be a person, people did not experience themselves in this way...but after time $t^*$, this was a way to be a person, to experience oneself, to live in society (ibid., p. 13)

In the realm of culture, this perspective recalls Street’s insistence that “culture is a verb” (1993), an active and dynamic process of interaction. It demands that we examine the ways in which social, institutional, technical, economic and political forces

...at a certain moment, yield intelligible meanings, enter the circuits of culture - the field of cultural practices - that shape the understandings and conceptions of the world of men and women in their ordinary everyday social calculations, construct them as potential social subjects, and have the effect of organizing the ways in which they come to or form consciousness of the world. (Hall, 1989, p. 49)

2. CATaC’s Search for (a) Theory of Culture

How ironic, then, that in the history of CATaC’s existence we have devoted such a lot of time and ink to theoretical models of culture whose main thrust is the classification of people (and thus, it is proposed, their interaction with information and communication technologies). These include models developed by Schwartz (1992), Trompenaars & Hampden-Turner (1997) and the Chinese Culture Connection (1987). But foremost among them is the ‘dimensions of culture’ model developed by Hofstede (1980).

Hofstede, a Professor of Organizational Anthropology and International Business, developed his model by undertaking a large empirical study in which he examined a pre-existing set of attitude surveys completed in 1967 and 1973 by marketing and sales employees of IBM in 66 countries. Hofstede determined that some of the questions that had been asked in these surveys might give answers that revealed respondents values, which, he argues, are the “core element in culture” (Hofstede, 1991, p. 35). Moreover, because he assumed that the variables of ‘occupational culture’ and ‘organizational culture’ could be considered equivalent for all of these IBM employee groups, he concluded that “the only thing that can account for systematic and consistent differences...is nationality itself” (1991, p. 252). From statistical analysis of the survey data, Hofstede elucidated four main bi-polar dimensions of culture: power distance, uncertainty avoidance, individualism versus collectivism and masculinity versus femininity. Furthermore, he calculated that 40 of the 66 countries assessed could be assigned comparative scores on each of these dimensions. (Hofstede later added a fifth dimension, long- versus short-term orientation, based on analysis of data from a Chinese values survey (Chinese Culture Connection, 1987).

Hofstede’s model of culture has been cited in 18% of CATaC papers from 1998-2006, and indeed the frequency of citation of this model has been increasing: in 2006, 27% of CATaC papers cited Hofstede’s model (Rogers, 2008).
More widely, Hofstede’s model has been embraced by investigators across the human sciences, as well as by intercultural educators around the world. A Google search for “Geert Hofstede” on March 1st 2008 revealed 76,600 hits; Williamson (2002) notes that Hofstede’s model “is probably the dominant explanation of behaviour differences between nations” (p. 1392), and Hofstede himself (rather self-importantly, I suggest) has argued that his work had “made a paradigm shift in cross-cultural studies”, becoming part of ‘normal science’ (2002, p. 1355).

3. Why Should We Worry?

If Hofstede’s model is so widely accepted, and, indeed, replicated and corroborated in subsequent empirical studies (see studies reviewed by Sondergaard (1994), and Chanchani and MacGregor (1999)) why should I be so concerned about the application of this model in CATaC work? Williamson’s (2002) careful differentiation between ‘methodologies’ and ‘research methods’ offer a useful framework for articulating my concerns.

3.1 CONCERNS ABOUT RESEARCH METHOD

Research method refers, of course, to the techniques used by researchers in gathering and analyzing data. In his extensive critique of Hofstede’s work, McSweeney (2002) first takes aim at Hofstede’s research methods. Although the total number of questionnaires used in Hofstede’s study was 117,000 (Hofstede, 1980), McSweeney notes that this large number in itself does not guarantee representativeness (Bryman 1988). Moreover, although the initial data covered 66 countries, only data from 40 countries was ultimately used in development of the ‘dimensions of (national) culture’. In reality, he notes, the total number of respondents per country was small or even tiny: the total number of respondents was only greater than 1000 in six countries; in 15 countries the total number of respondents was less than 200; and in some it was less than 100 (McSweeney, 2002, p. 94). McSweeney argues that Hofstede’s analysis simply cannot meet the conditions for reliability and validity of standard survey research because these assume that the surveyed population is representatively homogeneous with regard to the criteria under study.

McSweeney (2002) also points to the narrowness of the population surveyed: very specific kinds of employees in a single multinational company. In addition, he critiques Hofstede’s underlying assumptions that organizational and occupational cultures can be assumed to be the same across the 66 countries studied.

Lastly, McSweeney (2002) raises concerns about the ways in which the initial survey data was collected. He notes that survey questions focussed exclusively on workplace issues; that they were administered by the employing company itself (IBM), in the workplace; that some questionnaires were completed within groups and not individually; and that respondents “had foreknowledge that ‘managers were expected to develop strategies for corrective actions which the survey showed to be necessary” (Hofstede, 1984, p. 46). Not without cause does McSweeney wonder whether ‘gaming’ might have influenced respondent answers.

Williamson (2002), and, of course, Hofstede himself (2002) rebut McSweeney’s criticisms of method, pointing to corroborating empirical studies.
3.2 METHODOLOGY: THE REAL CONCERN

We may quibble over details of method, but the heart of the matter, in reality, is the choice of an appropriate research paradigm for cultural and intercultural studies. Williamson (2002) reminds us that it is methodology that underlies our choice and justification of research methods. And methodology is dependent on the values and beliefs of the researcher, including assumptions and beliefs about epistemology, ontology and human nature (Burrell & Morgan, 1979). Selection of a methodology is therefore essentially political in nature (Llewellyn, 1992); it cannot and should not be adopted for simple pragmatic reasons.

My real difficulty with Hofstede’s model (and similar models), and, indeed, McSweeney’s difficulty with Hofstede’s work, is the paradigm within which it sits. Hofstede’s work is firmly situated in what Burrell & Morgan (1979) have called a ‘functionalist’ paradigm. This has been, they note, the primary paradigm for organizational studies. It is deeply rooted in sociological positivism, and assumes that human interaction can be identified and quantified scientifically through statistical measurement. It presumes that the researcher is an objective observer, and that research is a process of testing hypotheses in order to explain causality in interrelationships and develop formal laws that can be generalized as statements of regularities among objective properties (Packer, 1999). As Williamson (2002) points out, Hofstede’s a priori assumptions about the existence of ‘national culture,’ and his selection of research method, are clearly consistent with his functionalist stance.

It may be that models of culture founded on functionalist assumptions about ‘kinds of people’ have a certain utilitarian value. It may be that “quantification of national cultures opens up what is otherwise a black box of cultural factors” (Williamson, 2002, p. 1391) in a way that is sufficiently meaningful for organizational and business studies. I contend most strongly, however, that a functionalist approach and functionalist models of culture are not at all appropriate or useful for our collective study of dynamic human cultures and their reciprocal and evolving relationships with technology (itself in a constant state of development and change). To be fair, even Hofstede himself noted that “cultural dimensions were never intended to provide a complete basis for analyzing a culture” (Hofstede & Peterson, 2000, p. 404).

3.3 THE PROBLEM WITH THE PARADIGM

McSweeney (2002) questions many of the assumptions underpinning Hofstede’s model, and these are worthy of careful consideration (a full survey is beyond the scope of this paper). Williamson (2002) points out that many of McSweeney’s criticisms are simply critiques of Hofstede’s paradigm and the resultant (and internally coherent) choice of research method...but this does not reduce their significance, in my opinion. Three difficulties of such models are worthy of elaboration here, and support, I believe, my proposition that the functionalist paradigm is a poor choice for our work.

Primary among these difficulties is the assumption that culture is a ‘national’ phenomenon that simply distinguishes the members of one nation from another. This classification of people into territorial groups is, of course, a division that rests entirely on the (relatively recent) history and vagaries of the modern political State, and ignores both internal cultural diversity within political States (for example, between the nations of Great Britain) and ‘national’ cultures that span multiple political States. States “may fissure, coalesce, combine, be combined, expand, or contract” (Connor, 1978) and yet
none of these changes can be accommodated into the models of culture projected by Hofstede and others. As McSweeney (2002) points out, the evident instability of political states means that they are poorly matched with a cultural model that claims to have “achieved measurement precision” (p. 111).

A second difficulty of such models is their static and essentializing nature, especially when they are used to try to predict an individual’s values or behaviour. In Hacking’s language, such models seek to classify ‘kinds of people’, and they permit non-alert investigators to view individuals as ‘cultural dopes’, lacking agency, and carrying uniform cultural attributes. Hofstede’s model emerged from data collected 40 years ago. Can we really convince ourselves that such data is relevant now, for meaningful investigations of contemporary cultural attitudes to technology and communication?

A third significant difficulty of using such models is their inherent reductionism and determinism. Geneticists would say, wisely, “you get what you select for” (Shuman & Silhavy, 2003); that is, if you create conditions to test for an expected difference, that difference is the only difference you will see. In the context of culture, over-reliance on functionalist models of national culture fools us into identifying and studying only a small number of a priori cultural dimensions as causal, and blinds us to the myriad cultural and non-cultural conditions that influence values and behaviour in specific places and times, as well as to emic conditions that are specific only to an individual culture. In an era in which we are increasingly acknowledging that societies can be best understood as a dynamic formation of competing truth regimes, rather than a mythical unity (Hall in Grossberg, 1986, p. 136), I would argue that is a tragic weakness.

Others (Bhimani, 1999; Harrison & McKinnon, 1999; Krug, 1993; Redding, 1994) have offered additional warnings about an over-dependence on functionalist paradigms in analysis of culture and on Hofstede’s model in particular.

4. Popularity: The Perils of Parsimony

Given these evident difficulties with functionalist models of culture, why do we continue to invoke them so frequently? Williamson (2002) suggests, and I agree, that the appeal of Hofstede’s model (and others within the functionalist paradigm) is their parsimony. A parsimonious theory “is relatively easy to explain, communicate and apply” (p. 1387) and its “resemblance to parsimonious theories of physical science may enhance plausibility” (Kuhn, 1996), especially in a context in which positivist assumptions about culture are valued. In a research context in which time and money is increasingly scarce, investigators have an “increasing interest in finding out – and finding out quickly – how to ‘do’ a cultural study” (Slack, 1996, p. 113), giving parsimonious theories an increased appeal. Hall calls this a “thrust towards codification” (in Grossberg, 1986, p. 149). He also points to a further positivist assumption that may underlie our search for a/the theory of culture upon which to establish our work: “the assumption that theory consists of a series of closed paradigms” (in Grossberg, 1986, p. 19). Closed paradigms may be seductive in their ease of application and their apparent existence as objective, value-free tools. But by definition they will make new phenomena very difficult to interpret because these will depend on new historical conditions. Such theories and models simply “let you off the hook, providing answers which are always known in advance” (ibid.).
I propose, then, that it is critical for us, individually and collectively, to examine and make overt the assumptions of our methodologies; to create, seek out and embrace counter-methodologies or counter-paradigms for our work, and to understand theorizing as “an open horizon, moving within the magnetic field of some basic concepts, but constantly being applied afresh to what is genuinely original and novel in new forms of cultural practice” (Hall in Grossberg, 1986, p. 138).

5. The Need to Abandon the Paradigms

Our work in the realm of culture, technology and communication seeks to explore, I believe, the differential (and often unequal) participation by different groups of people around the world in the so-called “information society”. As successor to the “industrial society”, the information society (sometimes called the “information economy”) is driven by forces of globalization, computer networking and Internet connectivity. An information society, says Wikipedia (2008)

…is a society in which the creation, distribution, diffusion, use, and manipulation of information is a significant economic, political, and cultural activity. The knowledge economy is its economic counterpart whereby wealth is created through the economic exploitation of understanding.

Our work asks: which values, practices, belief systems, forces and structures of different social groups influence the ways in which they design, perceive, adopt or utilise information and communication technologies...if they have access to it at all? And, how do these conditions constrain the ways in which societies are able to participate in contemporary systems of knowledge and wealth generation?

If we seek to meaningfully explore this digital divide – “the gap between those people with effective access to digital and information technology and the knowledge economy, and those without access to it” (Wikipedia, 2008) – it is critical that we do not abstract ‘culture’ “from its material, technical and economic conditions of existence”. For “how could culture, on its own, transcend the social, political and economic terrain on which it operates?” (Hall in Grossberg, 1996, p. 139).

This means that we must adopt methodologies that will allow us to examine the dynamic relations of power which structure the many social worlds we study, in pursuit of contextualized understandings of individual behaviour. Such methodologies call for investigation and elaboration of specific cases (rather than a search for universals), and assume that individuals are positioned within “intersubjective social fields which structure and constrain their activity, and that these same subjects are also actively involved in the reproduction of these fields, emphasizing the role of culture as a contributing force within the wider context” (Packer, 1999). Importantly, we must seek out methodologies that acknowledge our subjective position as participants in our research. What are the assumptions underpinning our methodological approach and choice of research methods? Which goals or knowledge systems are shaping our interpretations?

Do theories or models even exist that might allow us to structure our research and approach examination of culture and technology in new ways – one which emphasizes process, dynamic interaction, and context?
5.1 ‘ARTICULATION’ AS THEORY AND METHOD

Cultural studies has encountered the impasse of essentialist and reductionist thinking before. By the 1970s, cultural theorists were actively seeking ways to move beyond the limits of classical Marxist theory, because it simply could not fully account for all of the “mechanisms of domination and subordination in late capitalism”, or for the actual variations in the conditions of existence of different social groups (Slack, 1996, p. 116). Similarly, the essentialism of class that flowed from orthodox Marxism was unable to account for the non-revolutionary nature of the working class, for what individuals believed about their living conditions, or for the ways that other conditions of existence (for example, gender, race, or subculture) “entered into what looked like far more complex relations of dominance and subordination” (ibid., p. 116). Similarly, I have argued above that reductionist and essentialist theories of culture fail to account for differences in individual and group encounters with technology, and fail to illuminate how individuals and groups are, in turn, changed by these encounters.

Cultural studies avoided “falling into twin traps of reductionism and essentialism” (Slack, 1996, p. 112) by adopting the notion of ‘artication’ as a new approach to characterizing a given social formation. I suggest that articulation may help us avoid these same traps.

In simple terms, a theory of articulation asks us to critically investigate how, and if, different sorts of things are contextually connected to each other. In analysis of a social formation, it asks us to examine how some individuals or groups that have specific interests and values try to connect to other people, groups or economic arrangements, ideas or property, to carry out their interests. For example, how do disparate social forces, under certain conditions, come to cohere within a discourse? (Hall in Grossberg, 1986, p. 141). What is important is to understand that this form of analysis examines which critical assumptions of unity exist in a discourse, and how linkages came to be. Linkages are not necessary, determine, absolute and essential for all time it argues. Instead, the “so-called unity of a discourse is really the articulation of different, distinct elements which…have no necessary ‘belongingness’.” In the realm of individuals and communities,

the theory of articulation asks us how an ideology discovers its subject rather than how the subject thinks the necessary and inevitable thoughts which belong to it. (Hall in Grossberg, 1986, p. 142)

It becomes important to investigate not simply ‘how things are’, but to engage in tracing the multi-directional and complex relations of power, desire and knowledge for any particular context (Deleuze & Guattari, 1987). Moreover, context

…is not something out there within which practices occur, or which influence the development of practices. Rather, identities, practices and effects generally constitute the very context within which they are practices, identities or effects (Slack, 1996, p. 125)

Articulation insists that we examine societies as a dynamic field of interacting forces.

Slack notes that ‘articulation’ can also be understood as a method of cultural analysis, in that it can suggest strategies for undertaking a cultural analysis. Investigation of the articulated structure of a culture or society “requires an examination of the ways in which the ‘relatively autonomous’ social, institutional, technical, economic and political forces are organized into unities” (Slack, 1996, p. 124). Moreover, articulation as method makes clear that the linkages are recorded in an
“arbitrary moment of closure” (ibid., p. 114) – a snapshot recording a moment in a dynamic system – and makes no claim that the momentary social formation is universal or eternal.

Perhaps of particular interest to the CATaC community is the proposition that articulation permits us to go beyond theory and method to the levels of politics and strategy. Politically, says Slack (1996), articulation allows us to illuminate the “structure and play of power” (p. 112) that determine inequalities in our societies of interest. Strategically, articulation allows us to more meaningfully shape an intervention within a particular social or cultural context.

5.1 APPLYING ARTICULATION: DYNAMIC NOMINALISM?

Although Hacking (2006) does not explicitly reference a theory of articulation as the grounding for his work, the model of dynamic nominalism that he proposes for considering ‘ways of being a person’ might be considered as one example of how articulation might be applied.

In his 2006 paper, Hacking offers us an interactive ‘framework for analysis’ of the phenomenon of dynamic nominalism, which he contends makes all kinds of people ‘moving targets’. A full understanding of how we comprehend a group of people in a given moment in time involves considering

a) Names – or classifications.

But these are only one part of the dynamic in the continuous looping process of ‘making up people’, Hacking insists. In addition, we need to consider:

b) The people so classified (and how they respond to and interact with the classification)

c) The institutions involved in the processes of study, regulation or control of the people classified.

d) Existing knowledge about the people classified – both popular knowledge, and the “presumptions that are taught, disseminated, refined, within the contexts of the institutions” (Hacking, 2006, p. 5)

e) The experts or professionals who generate this knowledge, judge its validity and use it in their practice (these experts, of course, “work within the c) institutions that guarantee their legitimacy, authenticity and status as experts. They study…the b) people who are a) classified of a given kind.” (ibid., p. 5)

Hacking emphasizes the nature of this list as ‘banal’, and discusses the ideas of other theorists that might be usefully incorporated into alternate versions of the framework. He also notes that for each case, the roles and weights of the elements will be different. This is not a prescription.

Why is this framework useful? It continually reminds us to move beyond our attachment to old classifications, or names (the Japanese, the Australian) of groups of people. It is, moreover, “a nicely positivist list” (p. 5) which might appeal to our continuing need for speed and closed paradigms. While being relatively ‘implementable’, it forces us to keep our eyes on the dynamic horizon. We can ‘find out about people’, to some extent, using our engines of discovery. But we are forced to acknowledge that the “target” – the location of these people in time and social formation – is where it is because of the ongoing interaction of all five elements in the framework (and more).
6. Conclusions: The Importance of a Commitment to Practice

Certainly, by giving up on functionalist models of culture, we lose the speed and neatness they provide. We lose the credibility currently bestowed upon (apparently) ‘objective’ methods and ‘scientific’ approaches. And – nothing to sneeze at – we lose the easy communicability and applicability of parsimonious models.

I argue, however, that meaningful investigation of societies and cultures (and, within this, of cultural attitudes to technology and communication) requires a dramatic shift out of the functionalist research paradigm and the abandonment of static, reductionist and essentialist theories of culture.

We must not embrace, uncritically, closed theoretical paradigms that simply ‘let us off the hook’ of doing serious critical work. “Theory must be constantly challenged and revised”, argues Slack (1996, p. 113), highlighting Hall’s insistence that the “only theory that is worth having is that which you have to fight off, not that which you speak with profound fluency” (Hall, 1992).

Similarly, while we tend to think of ‘method’ as implying the application of “rigid templates or practical techniques to organize research” (Slack, 1996, p. 114), I wish to advocate most strongly for a perspective of ‘method as practice’ – invoking definitions of method both as ‘research techniques’ as well as the activity of “practising or trying out” (ibid., p. 114).

In our continuing work – research, political action, strategic intervention – I propose that we take for guidance Hall’s strenuous rejection of closed theoretical paradigms. "I am not interested in Theory,” he insisted “I am interested in going on theorizing" (in Grossberg, 1986, p. 150). And we must be, too.

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