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Powerful Knowledge: applications in a cultural context

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Culture and traditional knowledge are concepts developed and advanced by anthropologists over the past century or so. These have recently been appropriated and used in ways never envisioned by anthropologists, sometimes contravening the data, theory and models used by anthropologists to develop these concepts. It would be fair to say that initially most anthropologists welcomed this attention, with not a few embracing these appropriations. But there is little evidence to support many of the principal applied threads that have developed in 'critical' politics, economic development and conservation. Anthropologists have an opportunity - and an obligation - to clarify and refine both concepts in the context of these (mis-) appropriations, and to clarify them for what they are, anthropological inventions used to define and enhance understanding, not to define movable property or motivate new forms of race (and racism).

In this chapter I will briefly explore some of these ideas and examples in a restricted sense intended to reflect the relationship between culture, knowledge and behaviour in a context of change, in particular with respect to the relationship between explanation and practice, the relationship between applied scientific and cultural knowledge, and applications to economic development projects.

Specifically, I argue that applications of scientific knowledge are not the same as science, and undergo a process that has properties not unlike those described by Ellen and Harris (2000) for 'Indigenous Knowledge' (IK). This process results in knowledge that is not just about the system represented, but is necessary for the system to operate in a contingent world even though it was not originally a subset of the knowledge being applied. This is what I call deontic knowledge, or in more familiar terms, enabling knowledge. Building on Ellen's concept of prehension (Ellen 1986, 1993), I suggest the operative principle in IK has similar properties. Describing or formalising this enabling knowledge permits us to more formally describe what Ellen and Harris suggest is 'tacit, intuitive, experiential, informal, uncodified knowledge'. Using an development example from Pakistan I illustrate how confusing enabling knowledge with 'good practice' can lead to project failure. I conclude with some remarks on a 'relevant' anthropology, and the need for greater cooperation between the different strands of anthropology.

Culture

In many schools of anthropology culture has developed from a useful insight, promoted by Franz Boas and others in the early period of American anthropology, into a symbolic hydra by 1960, finally mutating into the impossible chimera we confront today.

As an anthropologist I think it unlikely that most anthropologists will abandon, or would want to abandon, the culture concept as a central component of anthropological theory, but that does not mean our concept 'culture' can remain undisturbed or that applications of the culture concept emerging within and outside of anthropology be unexamined. Re-examination has been proceeding apace for well over four decades, and although we are no closer to general acceptance of a 'core' to the culture concept, nor have compelling arguments against the culture concept emerged. In the midst of this swirl of reflection, self-reflection and contemplation, culture has actually grown more pervasive as a concept, if not a clearer one. The theme of this volume

is oriented in part to how anthropology contributes to economic development. Given anthropologists' focus on the culture concept, we might expect that a part of this contribution will include use of the culture concept. But can it do the work we demand of it?

IK and Cultural Knowledge

'Indigenous knowledge' (IK) is a term that has emerged over the two decades to describe the knowledge of a group of people local to a given situation, sometimes used interchangeably with 'local' knowledge (Ellen and Harris 2000:1-2), and which I am taking to be instances of cultural knowledge. Many anthropologists have questioned the value of trying to distinguish IK as a 'special' kind of knowledge (ibid.: 25-6). At the core anthropologists and practitioners have very different goals for characterising a peoples' knowledge. Practitioners are interested in knowledge that regardless of source is enactable with respect to their own practice. Anthropologists are more interested in the knowledge itself and its interconnections with other knowledge.

Although Geertz (1966) recast and broadened Wallace's distinction as 'knowledge for' and 'knowledge of' - procedural versus declarative knowledge - there is another more primitive distinction that should be drawn: knowledge that is about the system and knowledge that is a part of the system. Knowledge about a system is knowledge, conscious or non-conscious, that addresses that system and its functioning. Knowledge that is a part of the system is bits of knowledge which must be enacted in order for the system to be, and which need not be in a form similar to the Geertz's more substantive knowledge. I will call this enabling knowledge, though I have generally referred to it elsewhere as deontic, derived from deontic logic, the logic of permissions and obligations (Fischer 1992, 2002). Enabling knowledge relates to how substantive knowledge can interact or inhibit, when to shift approaches to applications, or how to proceed when information is missing. Knowledge of this variety implies an overall system that must be reproduced (in part) using this knowledge (Fischer 1994; Fischer and Finkelstein 1991). As I will amplify in the next section, it is enabling knowledge that represents many of the barriers to applications of any kind of knowledge, IK or not.

Ellen and Harris (2000: 4-5) present a checklist of characteristics that anthropologists and others have associated with 'indigenous knowledge' (IK, although Ellen and Harris settle on 'traditional knowledge' as the best of a unsettling group of terms (ibid:3)). This includes attributions emphasising the empirical, practical, applied, and situated (contextual and geographical) nature of IK, together with aspects such as oral transmission, informality and fragmentary distribution. In their conclusion it is this latter group that forms the prototype:

However, we believe that IK, in the sense of tacit, intuitive, experiential, informal, uncodified knowledge, will always be necessary and will always be generated, since, however much we come to rely on literate knowledge which has authority, the validation of technical experts and is systematically available, there will always be an interface between this kind of expert knowledge and real-world situations. It will always have to be translated and adapted to local situations and will still depend on what individuals know and reconfigure culturally independently of formal and book knowledge. (ibid:28)

Although I agree with the overall sense of their conclusion and, their emphasis on IK being intuitive, informal, uncodified and oral is misplaced. These are probably accurate enough as a description of most instances of what we regard to be IK as they are situation, but Ellen and Harris seem to take these points further as critical to the production and use of IK.

They are fundamentally correct in making the point that IK is complex and rich in its context of application, as, I would argue, is all applied knowledge. They use Richards' (1993) account of knowledge as performance, in which Richards contests a view of knowledge as a simple list of rules and decisions. Richards notes that Hausa farmers in northern Nigeria adapt to drought by making adjustments to their cropping pattern, sowing and resowing until a secure planting is instantiated or they exhaust their resources. However these '[cropping patterns] ... are not the outcome of a prior body of "indigenous technical knowledge" ...' (Richards 1993: 67) instead requiring interactive decision-making within a constantly changing historical context, idiosyncratic for each farmer and where that historical context constrains or directs the appropriate applications. From this Ellen and Harris suggest we should '...recognize knowledge is grounded in multiple domains, logics and epistemologies.' (2000:18) and continuing 'it may be far more productive to move away from the "sterile dichotomy between indigenous and western" [Agrawal (1995:5)] which idealizes and obscures knowledge and practices, disempowering peoples and systems through artificially constrictive systems.' (Ellen and Harris 2000: 18).

They continue by criticising efforts to codify IK, build IK into policies, the politics of IK and the resulting diffusion of agency from these (ibid:18-24). While this is certainly descriptive - most attempts to codify IK have been inadequate, attempts to employ IK in development projects often have mixed results, and the political structures that embed and embody knowledge mirror existing status relations. But this has no bearing on our prospects relating to more formal representations of IK. A better conclusion is that these states of affairs are a result of our generally poor accounts and treatment of characterising human knowledge

Reflecting this off their conclusions, if we are to improve our understanding of IK, we cannot adopt Chomsky's (1965) approach, and just look towards a 'deep structure' of IK, nor can we accept Richards' (1993) approach of simply accepting that each application of IK is an improvisational performance. We certainly should not leave the study of IK and cultural knowledge in general to succumb to our own 'tacit, intuitive, experiential, informal, uncodified' anthropological IK tradition.

Although an 'intuitive' approach might seem attractive to some, we should not follow it for two reasons. Firstly, given that as anthropologists we are mainly interested in knowledge that is shared to a considerable degree, IK is in some manner 'codifiable', though in a more dynamic form than we have as yet developed. It is a mistake to imagine that there is some reified version of 'the knowledge'. It is likewise a mistake to imagine that this knowledge is 'magic', only existing in the ether, or that we cannot make better efforts to avoid some of the issues that emerge from current interpretations of IK. If we can dynamically codify 'instances' of such knowledge that produce results similar to indigenous agents in similar contexts, we establish that this knowledge is codifiable. It was demonstrated twenty-five years ago (Shortliffe 1976) that restricted domains of knowledge could be encoded in an expert system and enacted interactively in new contexts, in Shortliffe's case diagnosis of diseases of the blood. This work has been expanded and refined (including a number of projects by anthropologists including Benefer (1989), Furbee (1989), Behrens (1989) and Read (1989) and Fischer (1985)- see also Fischer 1994b, Ch. 8 for a review and discussion), and producing expert systems for very narrow domains has been a undergraduate-level project in computer science, and at least one anthropology course, for over a decade. The expert system approach has a number of drawbacks (ibid.: ch 8), not least is that it is only descriptive. However, it does demonstrate that it is possible in principle to address in part most of the observations of Ellen and Harris.

Secondly, the same argument related to aggregated vs. individual authority has been taking place in most disciplines concerned with people over the past two decades. Substantial advances in agent-oriented representation and modelling in computer science are beginning to be applied to the social sciences to create 'artificial societies' which the properties of knowledge and its distribution can be investigated (Read 2001, Lyon 2002, Fischer 2002, Bharwani 2002). This work is relatively new, but provides a formidable method for those who are not willing to represent and analyse their data in terms of aggregates or norms (Hobart 1993:19).

Although unfamiliar to most anthropologists, within a few years the technology necessary to work in these terms will be accessible to most anthropologists (Fischer and Read 2001) as the requirements for computational and computer-based skills decreases.

What is needed is an expansion of the conclusions of Ellen and Harris to human knowledge and its uses, not abandonment of this study to our 'intuition'. In particular we need to re-examine the relationship between IK, applications of IK, scientific knowledge and applications based on scientific knowledge.

Scientific knowledge, applied knowledge and IK

Consider the relationship between scientific knowledge, technical applications of scientific knowledge and IK.

Scientific knowledge is derived from two gross kinds of activities. The first is the conscious examination of observed physical phenomena. This itself is comprised of establishing i) a class of phenomena - a classification sufficiently broad such that examples of a class appear more than once - ii) a description of the circumstances or context under which a class of phenomena can be observed, and iii) an account of how aspects of the context interact to create or influence the phenomena.

The second activity is more or less the converse of the first; consciously creating and manipulating a context in order to precipitate an instance consistent with a phenomena class in a replicable manner. Technical applications are derived from this second activity. But they are not science. Whereas doing science requires, in principle at least, a conscious and reflexive knowledge of the relationship between the context and the phenomena class, technical applications do not. These have different goals. Scientific application is oriented towards understanding, technical application towards doing.

Penicillin of a given dosage and frequency works equally well in the same circumstance for allopathic practitioners and *unani tib* practitioners in Lahore, regardless of their basis of understanding or explanation for how it works (Lyon 1991). At the same time, scientific knowledge is important to the 'engineer' as a legitimating device. 'Knowing' there is a good reason for the technology to work is apparently comforting to many practitioners, and much of engineering is involved in advancing the 'ritual' and 'religion' of the explanatory knowledge that underlies practical knowledge (Bourdieu 1990).

This is not to suggest that producing a technical application is simple. Applications are rarely single magic bullets. Instead, applications are created using some combination of techniques that work together for a desired result. The gross combination and sequence is often known for an application type, but detailed implementation usually requires some considerable adjustment in configuring the technology to the specific conditions of the implementation, especially in the

early stages of a technology. For example, in microelectronics it takes one to two decades for a new technical development to make the transition from first implementation to wide application (Fischer 1994b). Part of this delay simply reflects the development and diffusion of knowledge relating to a new technology, but perhaps more important, it is over this time that the technology itself is refined to make it more adapted to a wider range of contexts of application by practitioners who possess less and less knowledge by incorporating accumulated knowledge of these contexts of use into the technology itself. This is similar to the pattern of development of scientific innovations, where initial demonstration of an effect often appears in a very restricted and difficult to produce context, but as the context becomes better understood, so is the effect easier to demonstrate. This process in engineering is a result of gradually describing the many contingencies that make applications difficult, and adapting the technology so that the materials, tools and techniques incorporate knowledge relating to these contingencies and thus tend to work better across the contingent range.

Technology is often a blend of knowledge about how to interact with material systems, knowledge about the interaction and knowledge about what can and can't be done in different circumstances and how to adapt to different circumstances (deontic or enabling knowledge, usually referred to as 'contextual' knowledge, although this usage is descriptive rather than analytic). The latter variety is more often in need of revision than the former two since the kinds of circumstances that can arise change often in contrast to underlying principles. This form of knowledge is necessary to produce results from the former two, and thus must be kept dynamically in 'tune' with contemporary circumstances. But perhaps more significantly, without incorporation of enabling knowledge, we are in fact not importing useful knowledge at all because the powerful things that the knowledge enacts in its origin context are not present.

Development project contexts are often presented as if we are exporting techniques that are based simply on 'true' scientific knowledge. An industry has been made of pointing out that we often do not do so. Most of this discussion has related to not exporting the context within which the knowledge must be embedded to be effective, thus not actually exporting effective knowledge. More specifically, we are not exporting useful, enactable knowledge because important contextual enablements that the exported knowledge interacts with in its origin context are not present.

Some knowledge is seen as being powerful because it is true. Scientific knowledge is often used as a case in point (though science, by definition, is contingent.) But, in fact, much of our knowledge is powerful because it provides access to powerful processes and structures, not because it is, in fact true. The confusion with truth comes from associating too closely philosophical truth with knowledge. For example, a knowledge of spirits cannot be shown to be true based on most empirical knowledge of the world. But a knowledge of spirits can be operative and powerful if it provides access to powerful things, powerful people or powerful social institutions.

Much knowledge that we value is thus not either Geertz/Wallace's *for* or *of*, but is valued because it is enabling. Knowledge of this sort can include knowledge that others hold and relate to - how this exterior knowledge can be enacted or how we can avoid its consequences, how to get knowledge that is suitable for a situation, and even how to simply survive until other knowledge that falls more within our conventional categories can be enacted. Much of what we deal with in the world is contingent, either because it is truly contingent, or simply because it is beyond our power to know and thus we must guess. Enabling contextual knowledge can have many parts that unfold in layers.

I characterise IK (in the universal sense of Ellen and Harris, not just that of 'indigenous' peoples) as incomplete knowledge. Much of IK relates to accessing powerful processes ('natural' and human influenced), structures and people, including the exploitation of environmental resources. Part of this access is due to conventional views of knowledge - 'facts', classificatory systems, relationships and knowledge of processes and contexts. Another part is related to what Ellen 1993:229-234) refers to as prehension, '... those processes which ... give rise to particular classifications, designations and representations.' (ibid:229). In other words those processes that Richards (1993) concludes are situational performance or improvisation. The serious study of this aspect of IK is required to understand how to enable a given body of 'substantive' knowledge for applications. The value of 'substantive' IK should not be underestimated as an export in its own right. But nor should be confused with enabling knowledge, nor should we be surprised if we ignore this component and face difficulties in application. And we can face problems when enabling knowledge is exported if it is inappropriate to the new contingencies within which application is desired, as Dove (2000) suggests. In either case, it is important to be able to identify enabling knowledge, both to understand how IK works in its original context, and how it might be modified in its new context.

An example from Pakistan

In 1981 the Kalam Integrated Development Project (KDIP) was initiated by the Swiss government in cooperation with the Pakistan Agricultural Research Council. This was intended to deal with a unified approach to forestry economy and agriculture in the upper Swat valley, Pakistan. The programme had a very bad start (KDIP nd.) and was forced to close within the first year.

One of the initial projects they had attempted was the introduction of seed potato into Kalam in the mountainous upper Swat Valley in Pakistan for export to the Punjab. The new crop was not well received, in part because it was a new crop and farmers did not have an immediate use for it, did not know about it, resented the insulated intrusion of the project and because the model plots were not consistent in their yield. The geographical focus of the program, the area around and beyond Kalam, has very little level land, and most crops are cultivated on small plots terraced from the mountain sides 2500-2900 metres in elevation, with varying ground cover, increasingly eroded due to high levels of over-depletion of tree cover, and with a very short growing season. Significant areas of arable land are above the level of normal rainfall, requiring irrigation from the melt from glaciers above.

A local historian and religious scholar, Abdul Haq, became interested in the new plant, and set up a number of plots, taking what he took to be the best results, and using these as seed. After three years he had produced several different varieties of potato to grow in a number of different situations that are common in the region (on a level plot, a slanted plot, near trees, mixed with grass). When KDIP reopened in 1983, after consultation with the residents of the area, these were taken up by the development project, and using the descendents of these potatoes eventually went on to wide introduction of successful potato cultivation for local use, as well as plots for seed potato for export. Potatoes are now a staple crop (together with maize - an interesting story for another day) in the area.

Haq did nothing that the development project could not have done, but whereas they had focused on getting people to prepare particular kinds of plots suitable for a particular variety of potatoes, Haq had focused on adapting the potato for the land available. I talked to him about this in 1992 in the village of Bhuiu, and he had been inspired by traditional practices associated

with sheep and goats, whereby small holders would seek to breed their animals with animals of another small holder with a similar type of land access. This was said to result in animals more suited for the land available. Note that this is a broad analogy on his part, since the procedures he actually used were not those of traditional breeding practice.

The other difference was in his criteria for a successful crop. KDIP had focused on how many potatoes were produced for export. Haq was interested in how reliable local crops were, and how quickly they grew, and was rather unconcerned with how big they were or gross numbers. In this process he was not using a scientific approach, though it was empirical, because he was not concerned with understanding the development of the potato or why different contexts altered the potato, but simply towards achieving particular goals. He applied knowledge in a systematic manner, but he was not, at that time at least, interested in the intrinsic validity of that knowledge, but in the application of that knowledge in a way that worked towards his goals. Because his goals were derived from his local, culture-based knowledge, he produced a result that was acceptable and accepted by others in his society as useful (after a time). It also met their criteria, and he had provided an example that made sense to them.

In 1992 KDIP was still mainly focused on production of seed potato, and the overall programme was identified as a success when it was wound up in 1998. They saw the Haq potatoes as a means towards an end of getting people to accept the cultivation of potatoes, potentially one of many. In 1992 they were still oriented to bigger and more potatoes for local use, and had a number of programs to accomplish this goal, rather than examining the prospects for improving the Haq potato. In this they were not being scientific, nor were they very successful from a technical point of view with respect to local staple crops. They were, therefore, employing 'powerful' knowledge, support for local small potatoes, that enables their main goal, the production of seed potato. But they persisted in attempting to change the variety of potato grown locally. They did not see the Haq potato or equivalent as enabling knowledge, but simply as one more part of the development scheme.

I was in Kalam for two weeks before meeting Haq, two more weeks before hearing his story of the potato, and two further weeks following this up with representatives of the development project, who at that time were not even aware of the origins of the current local potato crop (though Haq is acknowledged in older promotional literature for his accomplishment). They were certainly not aware of local criteria for a successful crop, because it had never occurred to them that it could be any different from their own, bigger and more. They had simply accepted the knowledge transmitted to them by their former colleagues in the form of proforma. It had never occurred to Haq to tell them either. He had not really set out to consciously produce potatoes with particular properties, he had set out to produce potatoes that worked, and it was self-evident to him when that had come about. More important, the successful cultivation of local potatoes was critical to making potatoes a cash crop for export. Discussing this issue with KDIP staff I found on the one hand they were facile in the discourse of sensitivity towards local views (doubtless necessary 'powerful knowledge' these days), while still persisting with their views of what constituted an improvement. But a part of this failure on my part was my fault because I was also trying to get them to do too much too soon.

This example is not uncharacteristic for its time. I have seen the residue of other projects in other parts of the world, such as the failed Scanwater project in Cameroon which included components unmaintainable in the local environment, and un-maintained were certain to fail (and they have). Not all organisations take this point of view; the DTZ in particular has a good record of building solutions that suit the local context and employing serious mechanisms that require that

local people select and guide the projects undertaken. And, indeed, these days it is essential to use consultative processes. But effective use of these is often quite another matter because basic conceptions of improvement vary.

Conclusion

For anthropologists to make any impact on these issues, they must be able to express matters of local knowledge and culture in clearer terms, terms that work in the target culture, the development 'experts'. All too often we offer them bigger and more (and more complicated) potatoes, when what they want are small, low-yielding consistent potatoes. These are technical people. They do not want to understand about culture, they want to understand how specific knowledge about local culture can help their work, and exactly how they can acquire the knowledge. They do not want to be told what we want them to know, they will not respond to 'critiques'. They need to be told in terms they already value. Of course we should try to 'educate', but if this is all we do, will we fare any better than they?

Successful applied anthropologists already have made considerable advances in this direction. For their troubles, academic anthropologists often treat their work as at best substandard, more often with contempt. But applied anthropology is not only the 'proof of the pudding', it is the major interface between anthropology and the explicit handling of different knowledge systems. But because many applied anthropologists do not have the resources to analyse their results in a larger context, much of this work goes unnoticed by academic anthropologists. An anthropology that can do no work may be a comfort to the 'old' guard and the 'new' guard of academic anthropology, but is no comfort to me.

This exclusion is probably more related to the 'political economy' of academic anthropology than an indication of the inherent value of a 'working' anthropology. Increasingly many anthropologists are less and less concerned with detail, and more and more concerned with possible problems that might emerge if they were to undertake detailed studies. This is exemplified by many anthropologists, who self-consciously fabricate an impressionistic edifice in favour of a scholarly argument, neatly avoiding any problems that might arise from uncritical acceptance of investigative techniques and interpretation of data by eschewing both.

I think that academic anthropology can benefit greatly from work which will assist the applied anthropologist by directing more attention to 'working' models of culture. That is, to address the means by which a concept like culture can manifest itself in living women and men in a particular locale. How knowledge is distributed through a society, and how access to that knowledge is manifest. How new knowledge is created, disseminated and reproduced. How old knowledge is 'retooled' for the contemporary setting. This is not to say that we should put aside the current work in anthropology. Rather I am suggesting that for anthropology to advance it requires more than simply re-examining the concepts of anthropology with more and more sophisticated language, and occasionally more sophisticated insights. There must come a point where these conceptions are put to the test, evaluating how theoretical knowledge might fit into existing situations, or not.

Not that these concerns that have shaken anthropology over the past two decades were unfounded or unuseful. We had to come to grips with the fact that complex situations are complex, and that a few waves of the hand this way or that was not sufficient to reduce the complexity. We had to understand that all knowledge is contingent, not just scientific knowledge. We had to ad-

dress the implications of much greater interaction between groups of people in both time and space. A focus on individuated knowledge, maintenance of agency and how groups support and reproduce knowledge provides us with an opportunity for the discipline to begin a more unified perspective that permits anthropologists working on different strands of the problem to cooperate. That is, there is the need for detailed contextualised and finely textured studies, systematic fieldwork, development of theory, applied work and the development of new qualitative and quantitative approaches to study these issues. Ultimately knowledge, its distribution and its use is the key to understanding humans and their influence upon the world.

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