Linguistic and cultural variables in the psychology of numeracy

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Abstract: Although anthropologists pride themselves in paying attention to the small details of everyday life, experimental psychologists arguably have an even smaller scale of research - examining variables such as infant staring time and the speed at which words are pronounced. This paper considers the impact of these different approaches to scale and selectivity in research objects, focusing in particular on studies of Pirahã and Chinese numerical cognition. Everyone accepts that cultural factors, such as the use of different counting term sets in different languages, may influence numerical thought. But the tendency of psychologists to restrict scale and eliminate variables, including cultural ones, in order to be able to falsify their claims, sits uncomfortably with the anthropological tendency to incorporate variables, in order to be holistic.

It is sometimes said that anthropologists specialise in looking at human experience in very fine, even 'microscopic', detail. Perhaps this is true in some poetic sense or by comparison with, say, macroeconomists. Anthropologists do, of course, sometimes focus on the tiny details of rituals, language use, everyday life, and so on. But Alfred Gell once observed that the average scale of anthropological analysis is the human lifecycle – which, if you think about it, is not exactly vanishingly small

When I started reading the literature on numerical cognition a few years ago (as background for a project on numeracy and economic agency in China), it struck me that experimental psychologists are the ones who *really* do fine-grained research. For instance, an article by Starkey et al entitled 'Numerical abstraction by human infants' rests largely on evidence about the reactions of 6- to 9-month old infants to displays of either two or three objects. The key question is whether or not their staring time – something measured by two independent observers, and taken as an indication of their level of interest in what they are being shown – will increase when the number of objects is changed from two to three, and back again (Starkey et al 1990). Would it ever occur to an anthropologist to consider *infant staring time* in such meticulous detail? Such things are surely well below our radar. (Note that when psychologists look at a 'small' aspect of human behaviour, such as infant staring time, they typically do so repeatedly – i.e. they seek a very large sample of it. Whereas anthropologists often work with very restricted samples of (relatively) 'large' and multi-faceted human behaviours, such as 'the formation of Chinese identity'.) [1]

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Needless to say, if experimental psychology seems fine-grained and rigorous by comparison with anthropology it is partly because it is experimental. Unlike social and cultural anthropologists, psychologists spend much of their time and energy devising experimental protocols in order – eventually – to gather evidence related to tightly defined hypotheses (e.g. about the ability of human infants, at specific stages of cognitive development, to take in and process numerical information). This lends a precise and exacting nature to their work. But they are also prepared, more prosaically, to consider

human life and thought in terms of manageable chunks. Instead of asking questions such as 'How do Chinese children learn to be good sons and daughters?' or 'What is the impact of globalisation on Chinese conceptions of childhood?', they might ask questions such as 'How do Chinese children learn to count to three?' This scaling down (which does *not* mean that the questions at stake are any less important) makes it possible for them to be more precise about the evidence needed to sustain particular types of psychological claims; or perhaps one should say that the drive to sustain particular types of psychological claims is what leads, in the first place, to the scaling down. (My guess is that the two things go together.) Of course some anthropologists, e.g. linguistic anthropologists, also deal with the relatively micro, whereas some psychologists, e.g. educational psychologists, deal with the relatively macro. But as a general rule, psychologists seem quite happy – for better or worse – to eliminate variables and/or control them out of the analytical/experimental frame in order to have a manageable topic of research. Similarly, as Anthony Good notes (in this collection), legal practitioners typically seek to 'prune away "extraneous" details' in an attempt to get at underlying principles. By contrast, anthropologists seem preternaturally inclined – for better or worse – to try to take 'everything' into account.[2]

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In spite of these marked differences in outlook and approach towards evidence and scale (which, of course, are far from the only differences between anthropologists and psychologists, cf. Knight & Astuti, this collection) there have been a growing number of calls in recent years for increased cooperation between the two disciplines (e.g. Cole 1996, Shore 1996, Bloch 1998, Hirschfeld 2000, Astuti et al 2005). In simple terms, it has been suggested that psychologists can no longer ignore, or gently side-step, the

historical and socio-cultural foundations of human knowledge, while anthropologists must surely now accept (*pace* Durkheim) that many of their most cherished topics of research – emotion, memory, identity, and so on – are intrinsically psychological in nature. Calls for cooperation are presumably a good thing, but will our basic orientations towards research scale and selectivity stand in the way? Are practitioners of either discipline actually going to cede methodological (as opposed to conceptual) ground in order to achieve a rapprochement? Will the kind of evidence routinely collected by anthropologists – our accounts of everything – ever *really* be of interest to psychologists, and vice versa?

Here I want to consider these questions with reference to work on human numeracy. This is an area of individual cognitive development in which, after all, the significance of cultural and linguistic variables is beyond doubt (cf. Butterworth 1999, Dehaene 1999). Lacking expertise in the variety of human cultures and languages, psychologists of numeracy might reasonably turn to anthropologists for help. But the psychology of numeracy is also a field of study in which much of the most relevant data turns out to be very micro indeed, at least when seen from an anthropological perspective . In what follows, I will first discuss a recent case in which a psychologist went to the Amazon in search of "ecologically valid" experimental evidence, collaborating along the way with a highly experienced field linguist – perhaps to his regret. Then I will turn to my own research in China, and to the question of the relationship between micro-features of Chinese language/culture and the numerical skills of people there. In both of these cases, I want to ask how much cultural evidence psychologists are prepared to take on board,

and also to what extent anthropologists are prepared – or even able – to provide psychologists with the types of (stripped down) evidence they want or need. My sense is that practitioners in both disciplines are committed, for the most part without even thinking about it, to their customary scales of research. In turn, this may make it difficult (good intentions notwithstanding) for them to engage seriously with evidence from the other side – which probably seems, respectively, either much too 'big' to be useful or much too 'small' to be interesting. I will suggest, however, that even relatively minor concessions in either direction can pay dividends.

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In a fascinating article in *Science*, the psychologist Peter Gordon has recently discussed experiments he conducted amongst an Amazonian people, the Pirahã, who have an unusually limited vocabulary for numbers (Gordon 2004). To be more precise, they have terms which *can* be used to mean 'one' and 'two', but even these are not used very consistently by them. Beyond 'two', there is simply an expression for 'many'. From the point of view of a psychologist of numeracy like Gordon, the crucial (and very exciting) linguistic/cultural variable in this case is relatively circumscribed and (helpfully) can be stated negatively: the *lack* of Pirahã counting words above 'two'. (Note that from the point of view of most anthropologists, counting terms would likely seem a very micro feature of a whole way of life, the linguistic equivalent of infant staring time.) The key question that follows on from this relatively circumscribed variable also seems straightforward. In the absence of counting words, how well will these people perform

on non-verbal numerical tasks, such as matching up sets of objects? In some respects, it isn't very difficult to find out – although Gordon did have to spend a significant amount of time in Amazonia in order to do so. The results suggest, in brief, that the Pirahã are reasonably good at dealing with tasks involving very small numbers – one, two, sometimes three – whereas beyond this they lose the ability to be precise. However, if imprecision is allowed they can also deal fairly well with larger quantities by drawing, Gordon claims, on the innate human ability to make 'analog magnitude representations' of a rather fuzzy kind. Language, he concludes, is what you need in order to represent large numerical values exactly (2004: 498).

However, Gordon's data are already being used to support very different theories about the role of language in the development of numeracy. Gelman & Butterworth cite the Pirahã case along with material from another Amazonian people, the Mundurukú, to support their claim (contra Gordon) that 'numerical concepts have an ontogenetic origin and a neural basis that are *independent of language'* (Gelman & Butterworth 2005:6, emphasis added; cf. Pica et al 2004; Gelman & Gallistel 2004; Carey 2004). They stress that the peoples in question, in spite of having very restricted number vocabularies, are able to cope surprisingly well with large numerical approximations.

At stake in this debate, of course, are fundamental questions about the role of language and culture in human thought, and about our ability to think, if you like, without words. But in spite of these grand themes it might be noted – and this is not intended as a trivial observation – that the articles in question (specifically Gordon [2004] in *Science*, and

Gelman & Butterworth [2005] in *Trends in cognitive science*) are incredibly succinct by comparison with articles social scientists might find in, say, *Comparative studies in society and history*. For instance, both the crucial experimental evidence (e.g. 'The amazing result was that both groups succeeded on non-verbal number tasks that used displays representing values … as large as 80') and the crucial descriptive evidence (e.g. 'The Pirahã do not even use the words for 1 and 2 consistently') are cited by Gellman & Butterworth in a bracingly stripped down fashion that would rarely be encountered in reading anthropology (2005: 8-9). Of course, this is partly a matter of writing and publishing conventions – after all, how much can one say about language or culture in an article restricted to four or five pages, in which details of experimental protocols and results are meant to be the principal focus? But it is also surely a matter of intellectual priorities. At least on the surface, it seems that, for experimental psychologists, one does not need to know and/or say very much about Pirahã life – even about the bits of their life that relate directly to numeracy and numerical practices – in order to debate their numerical cognition. More specifically, although everyone (including Gordon, Gelman and Butterworth) appears to be in no doubt that culture and language may sometimes matter a great deal, cultural and linguistic *evidence* typically enters these scientific debates in highly circumscribed form.

Of course, the existence of complex – perhaps hard to circumscribe– cultural variables does loom at the margins of the discussion. Gelman & Butterworth suggest, for example, that the lack of number words among the Pirahã and the Mundurukú might have 'a

cultural basis'. In the course of their (succinct) consideration of this, they draw attention to the fact that:

> Mundurukú culture differs from Western culture in innumerable ways, and it certainly uses numbers far less often than we do. It remains possible that one or more of these many differences were responsible for the differences in performance [on numerical tasks], and not just the lack of a counting vocabulary. (2005:9)

Gordon, for his part, provides brief descriptions of everyday numeracy practices among the Pirahã (such as their rather incompetent use of fingers as an enumeration aid), and his experiments were certainly intended to have ecological validity – that is, to take serious account of Pirahã culture and the flow of ordinary life amongst the people with whom he lived for some weeks.

But again: how much scope is there, within psychology, to genuinely incorporate evidence about the 'innumerable differences' between cultures, or indeed non-experimental evidence of any kind?[3] In an online discussion which followed the publication of his *Science* article, Gordon complains about the difficulty of doing precisely this. He says that his original manuscript contained some potentially very important information about Pirahã numeracy, more specifically about their ability/inability to learn numbers in a *different* (i.e. non- Pirahã) language when presented with the opportunity. In brief, another scholar reported to him that a few years ago an

attempt had been made to teach Portuguese numbers to Pirahã villagers. It seems that 'The adults had a horrible time with it, the children had no problems (but were later told not to continue [learning/using the numbers] by adults)'. Gordon points out that he had originally included this information in a footnote to his paper, but was 'rebuked by a reviewer who said that such anecdotal evidence does not belong in the pages of *Science*'.[4] (Note that what is rejected here is not 'macro' evidence, as such, but rather non-experimental evidence. However, as suggested above, the need to design plausible experiments may itself lead to a scaling down towards 'micro' features of human behaviour – the two things go together.)

Equally interesting is Gordon's tetchy response (in the same online discussion) to the suggestion that neighbouring tribes should have served as control groups for his experiments among the Pirahã. Here the costs of ecological validity come crashing in as a justification:

> I think we all need a lesson here in doing research in the jungle. You don't just walk into a tribe and start doing experiments… If you want to contact a tribe as isolated as the Pirahã, you run a very high risk of being killed… [T]o study like this isn't just a matter of holing up in a hotel and driving down to the village, but requires staying for weeks at a time in the tribe, sleeping outside with jaguars roaming around at night, tarantulas dropping onto your hammock, poisonous snakes… The shameful secret of doing actual field research – as opposed to some idealized design that you concoct at your computer in your air-conditioned office

> at MIT – is that you take advantage of what is available and you try not to get yourself killed in the process.[5]

This appeal to the difficulty of 'being there', however overwrought, will have a familiar ring to most anthropologists.

However, given Gordon's reliance, in the end, on standard research protocols, does this background noise about cultural particularities, the perils of real life, etc. really make any difference? He certainly stresses that his conclusions are informed not only by the experiments but also by his direct experience of the Pirahã way of life. Significantly, they are further informed by the 'background of continuous and extensive immersion in the Pirahã culture' of the two scholars who made the project possible in the first place: Daniel and Keren Everett (Gordon 2004: 496). After all, Daniel Everett, a linguistic anthropologist, has been living and working with the tribe for over 20 years. Who better to give the imprimatur of holistic cultural understanding to Gordon's tightly focused experimental work?

But while Gordon must have anticipated criticisms from fellow psychologists, recent comments from Everett – the man who introduced him to the Pirahã – will perhaps have been more surprising.[6] Everett says that Gordon's very general conclusions about Pirahã numeracy are 'likely correct' but he also says, rather confusingly, that he disagrees with them – pointing out that Gordon's experimental design was culturally insensitive, making the Pirahã do precisely the kinds of things they hate to do.[7] Of course, Everett might

have helped the psychologist (his friend and former colleague) avoid this pitfall but he says that 'during the time that he was working on the experiments, I had my own priorities and offered little help'. He adds: 'Following the experiments, Gordon worked alone in interpreting the results, hampered by his lack of knowledge of the Pirahã language or culture, as well as by the fact that neither [Keren Everett] nor I were available to help him by discussing with him the plausibility of his results, or even the relevance of his experimental design to test numerosity in Pirahã'. So much for collaboration!

Perhaps psychologists who disagree with Gordon's linguistic determinism (i.e. with his view that language is a prerequisite of precise numeration) will take pleasure in Everett's comments. But I wonder what they will make of Everett's own recent, and strongly *cultural* determinist, discussion of the Pirahã in *Current Anthropology*? He claims not only that they lack number terms but also that, among other things, they lack colour terms, that they have 'the simplest pronoun inventory known', that they lack creation myths and fiction, that they have 'the simplest kinship system yet documented', that they have no 'individual or collective memory of more than two generations past', that they do not draw or produce art of any kind, and that they have 'one of the simplest material cultures documented' (Everett 2005: 621-34). These extremely surprising features of Pirahã life, Everett suggests, are all the product of one thing: a culture that makes Pirahã talk only about 'nonabstract subjects which fall within the immediate experience of interlocutors' (2005: 620). It is this – according to Everett – that constrains the

development or adoption by them of linguistic or cultural features (such as telling stories about the past or using relative tenses) that are found in most human societies.

Now, even if Everett's claims about the facts of Pirahã life are true, his arguments about the relationship between their culture, language, and thought are certainly open to question (see the comments section at Everett 2005: 635-644). For one thing, a key plank of his argument is the fact that, in spite of over 200 years of contact with outsiders, the Pirahã have strenuously rejected the outside world. He says they ask questions about the outside 'largely for the entertainment value of the answers' (2005: 626). And yet he observes that they routinely trade with non-Pirahã, that they are very keen on buying whisky in particular, that Pirahã women have sexual relations with Brazilians (and sometimes have children by them), and that they specifically asked Everett and his wife to teach them how to count and how to read so that they could become better traders. At the very least, this suggests a degree of ambivalence towards the outside world rather than a total rejection of it – but to say so would weaken Everett's argument for the cultural determination of language.

In any case, I mention all of this because Everett has relied, along the way, on some classic anthropological arguments. Notably, he is a *linguistic* anthropologist and, as his *Current Anthropology* article shows, he is not averse to the micro scale of research typically found in psychology. But like a true anthropologist, he also says that language (including the language of number) must be studied in the context of its use; that one should be extremely cautious about 'testing' people through procedures which are alien

to their way of life; and, perhaps most importantly, that human societies and human thought are highly synthetic – everything is embedded in, and connected to, everything else. He, like Gordon, is intrigued by the 'small' fact of the absence of counting words among the Pirahã. But if you really want to understand Pirahã numeracy, Everett seems to be saying to Gordon, you need to visit them over the course of 20 years and collect evidence about kinship, whisky-buying, trading, sexual relations, etc., i.e. you need to collect evidence about 'everything'. (Indeed, as Good notes in this collection, anthropologists often define both expertise and reliability of data precisely in relation to the investigator's "extent of experience".)Whereas Gordon, of course, simply wanted to know if the Pirahã, lacking number words, could or could not do some simple non-verbal numerical tasks such as matching two objects to two objects. This is an empirical question, and Gordon (to his everlasting credit) went to a great deal of trouble to generate what is, in fact, a rather minimal and tightly focused data set in order to try to answer it. It is presumably this evidence which will be taken up and debated by other psychologists – not the fine details of Pirahã social life, and certainly not Everett's radical holism.

What does this case tell us? Gordon's search for experimental evidence is driven by existing debates in the psychology of numeracy which relate to language and culture but which are nevertheless mostly framed at a level below the anthropological radar. He travels to Amazonia and appears to have gone somewhat anthropological, in the sense that he ends up having to explain to his fellow psychologists how complicated things are out there – that is, the Pirahã way of life is complicated and doing 'real world' research is complicated too. In the end, however, his primary task is to show how Pirahã language

does or does not influence numerical skills. Taking Gordon's conclusions about language and thought as a starting point (some might saw straw man), Everett goes on to suggest that Pirahã culture shapes not only numerical skills, but also grammar, collective memory, kinship, art, material culture and so on. His comprehensive knowledge of Pirahã language and life, the fruit of over twenty years' work, is presumably almost entirely wasted on his psychological colleagues, including (or so it seems) Gordon, for whom it must surely comprise too much information.

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Now let me shift focus to a very different place and a very different context for studies of numerical cognition: China. Not only does the Chinese language – unlike the Pirahã language – have a full complement of counting words, the numeration system has the further advantage (shared by other East Asian languages) of being consistent with base-ten logic. This happens to make it much easier to master than, say, the English one. In English, a child learning to count to twenty starts by learning ten new words ('one', 'two', 'three', etc). Then she learns some *additional* new words – 'eleven' and 'twelve' – which don't, on their own, tell her that she has shifted into a second set of tens, or indeed that there is anything special about 'ten'. It's a confusing business. The Chinese system, by comparison, is a model of clarity, with everything based on combinations of the original ten words. Eleven is simply a combination of 'ten' (*shi*) and 'one' (*yi*): 'ten-one' (*shi-yi*). Twelve is 'ten-two', thirteen is 'ten-three', etc. This (when taken in combination with other features of the system) makes it easier for very young children to

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grasp that eleven is ten plus one and that twelve is ten plus two – and, more importantly, that there is something special about 'ten.'

Now, this difference in counting words may seem a rather minor linguistic phenomenon since children using either the Chinese or English system can perfectly well learn how to count. (And, like infant staring time, it is the kind of thing most anthropologists would scarcely notice.) Reviewing the evidence, however, Geary suggests that children who learn to count in Chinese (and associated East Asian languages) have some significant advantages over those who do not. They appear to '…make fewer counting errors; understand counting and number concepts at an earlier age; make fewer problem-solving errors in arithmetic; and understand basic arithmetical concepts, such as place value and trading, at a much younger age than their American and European peers' (Geary 1994:244; cf. Geary et al 1996; for converging evidence from Korea and Japan see Fuson & Kwon 1992; Miura 1987; Miura et al 1988; Miura et al 1993).

Of course, by the time this happy outcome is achieved, a number of variables other than language – such as schooling and parental pressure – may have intervened, and these too are discussed in the literature. For this reason, it is instructive to compare the Chinese case with the Pirahã one outlined above. For the Pirahã, the relatively simple claim (as formulated by Gordon within the framework of developmental cognitive science) is that they don't have many number words, and this limits their ability to carry out numerical tasks beyond very small numbers. This is shown experimentally. In the Chinese case, the relatively complex claim (as formulated by Geary within the framework of

developmental cognitive science *and* educational psychology) is that the system of number words contributes in positive ways to the development of children's numerical/arithmetical skills. The support for this claim has to be built up from separate bits of evidence (e.g. about children's counting error rates, or about their grasp of basic concepts at different ages, by comparison with non-Chinese children). Even then, because of the conflation of many factors in China which might produce the observed outcomes (ranging from schooling, to abacus training, to parental pressure) the educational psychology claim is undoubtedly harder to sustain – and perhaps in some ways more anthropological? – than the Amazonian one. It is a synthetic claim, based on the aggregation of different studies, and probably more open to refutation.

In any case, because Chinese counting terms (forgetting about schools, abacuses, parents, etc.) provide a powerful illustration of how linguistic/cultural variables shape numerical skills, they have been repeatedly cited and discussed in the psychological literature (e.g. in Fuson & Kwon 1992; Miller et al 1995; Dehaene 1999:91-106; Butterworth 1999:129-133). Here is proof that culture really *does* have an impact on numerical cognition. And I suspect that many psychologists, in their eagerness to use cultural illustrations of this kind, find it hard to resist a form of cultural butterfly collecting – which is not without its dangers.

In neuropsychology, for example, each 'cultural case' becomes – or so it seems to me – the equivalent of cases of patients with brain function impairments of various kinds, and in this sense the more exotic the better. This is for the good reason that highly unusual

constraints (such as severe brain impairments following accidents, or highly atypical counting systems) may reveal a great deal about human thinking. (There's an irony here, given that many anthropologists are attracted to cognitive science precisely because it appears to offer an escape from the endless exoticism and circularity of cultural relativism.) In the index of *The number sense*, written by the neuropsychologist Stephan Dehaene, there are entries for Aborigines, Arabs, Aztecs, Chinese, Dutch, Egyptians, French, Germans, Greeks, etc. The index to Brian Butterworth's *The mathematical brain* (he is also a neuropsychologist) refers to Africans, Amazonians, Arabs, Babylonians, Basques, Chinese, Danish, and so on. Both of these books are fascinating and deeply impressive introductions to the psychology of numeracy. But one potential problem with the accumulation of examples (whether it is being done, as in these cases, for the purposes of illustration in books aimed at popular audiences, or in scholarly research papers) is that only an iconic detail or two about each culture is typically up for discussion (e.g. 'the Pirahã do not have counting words above two'), often by people who do not have direct experience of the language/culture in question. And as these iconic details – which are normally iconic for the good reason that *somebody is able to make evidence-based claims about them* – become the source material for a range of debates, their selective nature may be problematic.

For example, in the literature on numerical cognition only two variables related to the Chinese *language* (as opposed to the Chinese education system, etc.) appear to have come up for serious discussion. The first, as I've just noted, is the system of counting words. The second is the speed at which these words can be pronounced. With respect

to the latter, the basic argument is that because you can (apparently) say strings of Chinese numbers faster than, for example, strings of French numbers, Chinese speakers find it easier than French speakers to recall long number sequences. This, in turn, has an impact on their ability to do mental calculation (cf. Dehaene 1999:102-3; Geary et al 1993; Chen & Stevenson 1988; Stigler et al 1986). As a research object for psychologists, this speed-of-pronunciation variable has the advantage of being micro and quantifiable (like infant staring time), thus making comparisons across languages possible.

But if you consider Chinese number skills with respect to the whole human life-cycle – to go back to that gigantic scale of anthropological analysis – both the number words set and the speed at which they are spoken look like being tiny blips on the radar. It is hard for me, as an anthropologist, to see how data about these two micro-variables can be *genuinely* integrated with the kind of ethnographic evidence I have collected about the flow of everyday life in Chinese villages, or vice versa. Obviously, children there embed number words in (sometimes long and rambling and incoherent) sentences, and even by the time they are twelve or thirteen years old, their 'numerical cognition' is rooted in complex social practices such as learning physics at school or haggling with old ladies when buying vegetables at the local market. Like Everett, I find myself suffering from holism, i.e. from an overload of causal variables and a mountain of highly diffuse ethnographic evidence collected month after month in the Chinese countryside.

But let me return briefly to the question of the memory for numbers – which, as I've just said, the speed-of-pronunciation variable is meant to influence. As it happens, I can think of plenty of other things which might influence the ability of Chinese people to recall numbers, to hold them in mind. I'll restrict myself here, by way of illustration, to one 'small' candidate influence and one 'big' one.

The 'small' candidate influence is the tonality of the language. Along with other tonal languages, Chinese is effectively sung rather than spoken, and number sequences therefore always take on a (potentially memorable) melody. Assuming this does influence memory for numbers, my hunch is that for native speakers this would rarely be an explicit (conscious) phenomenon – but that it would still influence performance. It's interesting, by the way, to think what directions the general discussions of numerical cognition might have taken if tonality, a completely different type of variable, had been somewhere on the agenda. For one thing, tonality provides a kind of musical structure to language, and numeracy itself is, of course, closely connected to the apprehension of structure and pattern in experience (cf. Gallistel & Gelman 2005).

The 'big' candidate influence on number recall (by which I really mean that it is an aggregate of different variables) is the almost generic importance of numbers in China. (While this may not, at first glance, seem like a linguistic variable, I believe that it can be so construed.) In short, the relevance of numerical information – and here I'm using 'relevance' in the sense intended by Sperber and Wilson (1995) – is redundantly *communicated* in China both through the direct use of language and through a wide range

of communicative social practices (Stafford 2003). To give an obvious illustration: Chinese religion and cosmology are very numerical/mathematical in orientation. It is therefore widely assumed that numbers (and the manipulation of numbers) may reveal significant things about the fate of individuals in the flow of time. Following on from this, popular religious practice is often explicitly focused on numerical issues of various kinds (how many offerings to give, how many times to bow, how to read numerical divination signs, etc). The importance of numbers is further heightened, in contemporary China and Taiwan, by their connection to the worlds of money and business – which, in the popular view, are also thought of as highly 'fateful' and therefore not unrelated to cosmology and religion. I could go on with a long list of illustrations, but the basic point is simple: numbers are crucially important and are explicitly seen to be so by ordinary people. This message of relevance is redundantly communicated to them by Chinese culture.

Now, the educational psychology literature does, in fact, make reference to this kind of thing. Geary and others have discussed, for instance, the high value attached to mathematics education in East Asia, and the possible impact of this on the educational achievement of children (Geary 1996; cf. Hatano 1990). But with reference to Japan, Miura says that although social factors such as parental expectations about mathematics achievement 'undoubtedly affect overall performance [in school], it is unlikely that they specifically influence the way in which a child mentally represents number' (Miura 1987). It isn't clear to me why this should be so, and my interest is precisely in the possibility that a generalised cultural valuation of number could itself have cognitive

effects, by redundantly stressing the relevance of numerical information in cognitive environments.

These observations are likely, however, to raise a string of questions from sceptical psychologists – and sceptical anthropologists. What exactly do I mean by saying that in China numbers have an 'almost generic importance'? When does the influence of this variable kick in? During childhood? How does it interact with other variables (such as counting terms or schooling)? Given that the importance of numbers could be expected to vary over historical time in China, and even between different individuals at the same time, what are the risks of treating it as an aggregate, ahistorical variable? And if I want to disaggregate this variable, in order to make it more testable, where should I start? With the importance of numbers in religion, in business, or somewhere else? Finally, how could I prove that this cultural variable actually motivates attention to numbers?

When it comes to collaboration, my guess is that psychologists would appreciate it if anthropologists would come up with ideas like my 'small' candidate influence – tonality. Again, this is partly an issue of scale. It is a relatively restricted point, and one can imagine that it could be tested in some straightforward ways. And yet from an anthropological point of view it is an incredibly small bit of information about Chinese language/culture. How much could I say about it based on my long-term fieldwork in Chinese and Taiwanese villages? Almost nothing. By contrast, I could say a lot about my 'big' candidate influence (the generic importance of numbers in Chinese society and culture); this is the kind of topic that could easily fill an anthropological monograph.

However, I suspect that for most experimental psychologists such a monograph would simply provide interesting background noise, awaiting clarification through a process of disembedding, scaling down and experimentation. It would be like learning that the Pirahã have been trading with the Brazilians for a long time …

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My point in considering these two cases - the Pirahã one and the Chinese one – isn't to highlight the fatal incompatibility of anthropological holism (taking everything into account) and psychological particularism (looking for repeated examples of *one small thing*). Nor do I want to suggest that psychologists, in spite of their interest in cultural/linguistic variables, are never going to be able to cope with the complexities of culture in the real world. Instead, I'd like to conclude this discussion by noting two rather common sins, as I see it, on the anthropological side – sins which relate very directly to the overarching topics of evidence and scale addressed in this collection. The first is the use of anthropological holism as an excuse for avoiding detail, and therefore avoiding saying anything falsifiable. By holding that all things are interconnected, we tend to make falsification of our claims (e.g. via experimentation) more or less impossible. The second is the romanticisation of anthropological research, and more specifically the idea that it has an ecological validity unmatched by other disciplines – this in spite of the fact that fieldwork is, of course, a dramatic intervention in the lives of our informants. Would it really be such an unnatural imposition for anthropologists to examine the micro, as psychologists do, using experimental techniques?

Bearing this in mind, and inspired by my reading in (micro) cognitive psychology, during a recent period of fieldwork in rural Taiwan I carried out a pilot project in which I tried precisely to see whether or not I could quantify the 'big' variable I mentioned above – i.e. the 'generic importance attributed to numbers'. Very briefly: I showed subjects a drawing of a street scene in which certain types of information was embedded: colour (e.g. the colour of a girl's dress), written language (e.g. words on a street sign), explicit number (by which I mean numbers written out, e.g. numbers on a license plate), and implicit number (by which I mean objects that could be counted, e.g. birds in the sky). Subjects were given 15 seconds to look at the drawing, after which it was taken away and they were asked a series of questions about the content – such as 'What colour is the dog?', 'What is the number on the house?', 'How many trees are there?', etc. A sample of respondents from the UK, approximately matched in terms of age and educational level, provided a control group for the research.

My hypothesis was that the Taiwanese subjects – having been enculturated into the Chinese way of thinking of numerical information as having an intrinsic relevance, regardless of context – would be more likely than UK subjects to notice and recall correctly the numbers embedded in the drawing. In fact, the results (based on this very limited pilot project) showed no such thing: the UK subjects were marginally more likely to get the numbers right. Of course, this outcome might be explained in many ways, including the possibility that my research design was completely wrong, or – just as likely – that my hypothesis was an implausible one to start with.

And yet, the simple fact of attempting to prove, through quantification, a general claim I was making about Chinese numeracy was extremely productive for me in a range of ways. One rather simple point is that the drawings proved to be a good prompt for general discussions. The task facing my friends in the village was a completely unexpected one, and it provoked them into saying interesting, sometimes very telling, things about numbers and numerical skills. As well as prompting interesting discussions, the experiment (well, the quasi-experiment) forced me to think – in ways which anthropologists are often *not* forced to think – about exactly what I was trying to say or claim about numbers in China. For instance, the task called on subjects to remember numbers, and from this I would infer whether or not they think that numerical information is, by default, important. But this raises the complex question of whether *remembering* something is the same thing as attributing *relevance* to it. Also, although my hypothesis was that numbers would be shown to have 'intrinsic relevance', a more likely scenario (as I suspected from the outset) is that numerical relevance is highly context-specific. This raises the question of which contextual effects would elicit more attention to numbers, and whether or not these effects, which are very hard to reproduce artificially, could be tested in an ecologically valid way.

In short, simply using the pilot project as a heuristic device had the effect of improving my thinking, as an anthropologist, about Chinese numerical culture and how it is learned and used. This involved scaling down – giving up a little bit on anthropological holism,

and trying to be more precise about how my claims and observations could, or could not, be supported.

I have mentioned, in the course of this discussion, variables of the 'micro' kind around which psychologists sometimes focus their own research on numeracy: infant staring time, small sets of counting terms, the speed at which number words are pronounced. Although anthropologists claim to revel in the small details of everyday life – and so they often do – the fact is that many experimental psychologists look at human life in finer detail than us. They do this not because they are miniaturists, but because they seek to make and support falsifiable claims about human thought and behaviour. The same standard of falsifiability does not apply in anthropology, and this arguably tends to push anthropologists in the opposite direction in terms of research scale. To be an expert in psychology (at least of the kind I have been discussing in this paper) is to convincingly use evidence drawn from the tiny details of life in order to support claims related to very big themes (such as the relation of language to thought). To be an expert in social and cultural anthropology is, for the most part, to possess a kind of encyclopaedic store of evidence – historical, ethnographic, anecdotal – about a particular group of people. The risk for psychologists is that, caught up in the activity of eliminating variables and restricting scale, they might not see the forest for the trees. (For example, they might get obsessed with counting terms and then not see the full variety of ways in which Chinese language and language use influence numerical cognition.) The risk for anthropologists, caught up in the activity of accumulating variables and expanding scale, is that they

might not understand any of the trees very well, and simply wander around the forest making claims that can never be falsified.

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Notes

[1] See also Stephan Ecks's comments, in this collection, about the contrast between evidence-based medicine, which depends on very large samples of patient behaviour, and medical anthropology which, he says, 'usually insists on the soundness of small samples, which can sometimes consist of [evidence from] just one patient or healer'.

[2] As Good observes (this collection), anthropologists 'treat ambiguity and complexity as immanent aspects of all real life situations'. They therefore tend to *include* complicating variables in their accounts.

[3] As Nicola Knight has rightly pointed out to me, the evidence used by Gordon would, in fact, be considered quasi-experimental by most scientists. But it is still of course *more*

experimental (i.e. less descriptive, less anecdotal) than the evidence typically used by most anthropologists most of the time.

[4] Exchange between Daniel Casasanto and Peter Gordon at http://hci.ucsd.edu/cogling/3382.html.

[5] Exchange between Daniel Casasanto and Peter Gordon at http://hci.ucsd.edu/cogling/3382.html.

[6] Interesting background information about the Everetts, and about their relationship with Gordon, can be found in the recent *New Yorker* piece by John Colapinto (2007), published after this article was written.

[7] All of the comments by Everett cited in this paragraph are found in Appendix C of the online material accompanying Everett (2005).

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