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Christophe Heintz has recently finished his Ph. D. with a dissertation titled *Scientific cognition and cultural evolution: theoretical tools for integrating social and cognitive studies of science*.[1] His work was supervised by Dan Sperber at the Institut Jean Nicod (Paris), which develops research on the relations between cognition and culture, something that anthropology is in need for a long time now. Christophe Heintz studied mathematics and philosophy (Master degrees) at the Universities of Paris and Cambridge. He is currently a research fellow at the Konrad Lorenz Institute for Evolution and Cognition. The interview that he generously gave us was conducted by email. The originality and conceptual density of his work justify his inclusion in our gallery of intellectual guests without further ado.

L.Q: Could you please give us a general outline of your work? We know that you are doing something that you would probably describe as "cognitive anthropology of science", but how have you found yourself doing that kind of anthropology, and, especially, what does it means to do it, what are the main consequences for the anthropological tradition (if that is a tradition still manageable) in doing it, what kind of cognitive approach is, in your terms, worthwhile?

C.H: My main work has been to apply theories drawn from cognitive anthropology to the history and sociology of science. I have been showing and illustrating the relevance of Sperber's epidemiology of representation and Hutchins' distributed cognition for understanding scientific development.

I like to say that this work belongs to the nascent field of cognitive anthropology of science. However, my approach remains very philosophical, so the work is best characterised as belonging to the methodology of science

[1] Heintz (2007).

studies: I attempt to find methods and theoretical grounds for integrating cognitive studies to social studies of science. It is by doing philosophy of science that I found myself praising cognitive anthropology of science.

Anthropology has been explicitly applied to the study of the scientific community since the work of Latour and Woolgar in 1979. It is now 'common' to do participant observation in a scientific laboratory. However, one cannot say that there is a well designed sub-field called 'anthropology of science'. While anthropology fought to constitute itself as a discipline, science studies have indistinctly drawn methodologies and theories from the social sciences, picking on pragmatic grounds. This is also what I do with cognitive anthropology, since I apply some of its theories in the history of mathematics. The anthropological tradition is seen as a resource.

The main idea of cognitive approaches in anthropology is that cognitive science should be taken seriously. This implies that explanations of socio-cultural phenomena should not be blatantly contradicting findings in cognitive science, and it suggests that some explanations may include factors that have been specified by cognitive science. There are different trends within cognitive anthropology, depending on the cognitive theories one draws upon, and on the understanding of what culture is. One classical view uses the theories of mental models, schema and scripts, and takes culture as being constituted by the knowledge people must have in order to manage in a community. Another view is represented by Sperber, Boyer, Atran, Hirschfeld and others, who argue that the mind has a rich innate structure that strongly constrains learning and behaviour; Sperber also defines cultural phenomena as relatively stable distributions of representations, mental and public, among a population. It is this latter approach that I have been using.

L.Q: How to overcome the differences between the rationalist bias found in the work of anthropologists such as Lévi-Strauss or, to use a "relevant" instance, Dan Sperber, and the relativistic one associated with the work of people like Clifford Geertz and Paul Rabinow? In another way, how can we reconcile the relativistic stance of many researchers coming from the science studies arena, and the rationalistic bend sustained by most of the cognitive scientists?

C.H: Reconciling different stances is not a goal in itself: if one is true and the other not, then one must adopt the true one and reject the false one. There is much work in science studies that attempt to reconcile the old rationalist position and the current relativist sociology of science. They would say

that the rationalist was not entirely true and that the relativists have gone a little bit too far. The description of the compromise is often limited to philosophical analyses of the positions, and I would argue that none of the compromises have led to a fruitful methodology.

This said, I think some rationalist stances are compatible with some relativist stances. This is because rationalism and relativism are, often without distinction, applied either to the actual working of the mind – asserting the similarities or differences of thinking processes across cultures – or about social norms of thinking – what is said to be reasonable or correctly thought. I am a rationalist in the sense that I think the structure of the mind is largely similar across culture, because it is for a large part species specific, i.e. determined by the human genome. In turns, the innately specified structure of the mind strongly determines what is and can be thought. But I am a relativist in the sense that I think that there is no universal norm of good reasoning that directs the history of ideas and the history of science. So, rational reconstruction in the history of science is nothing but an anachronistic interpretation of events, to which I would like to substitute a causal account (whether the beliefs to be explained are true or not – which could be qualified as a methodological relativism).

L.Q: Remembering the Neurath's boat metaphor, a boat which has to be reconstructed permanently in a tempestuous sea with the resources available, can we consider that the cognitive approach described is a secure basis to rebuild the anthropological way of thinking, a good way to compare practices, scientific or other, in a world tainted by a serious doubt about the possibility of knowing?

C.H: One way to understand Latour's ban on cognitive psychology in science studies is that he does not want to include it in the structure of Neurath's boat.[2] However, social sciences are bound to make some assumptions on the mind as soon as they talk about representations, meaning, and behaviour. The choice is therefore to do with cognitive science, or to do with some naïve post-hoc psychology.

I do not see that doubts about the possibility of knowing have any positive impact on the study of knowledge. It has no consequence on

[2] Ver Latour (1987: 247) e Latour e Woolgar (1986: 280).

methodology. So it remains a purely philosophical topic. Personally, I have no doubt on the possibility of knowing.

L.Q: What is for you the relation between brain, mind, and cognition? These terms are not interchangeable and commensurable, as we all know. How could we trace the mutual implications and correlations between these "conceptual devices"? What is the place of interdisciplinarity or transdisciplinarity in this context?

C.H: Here is how I use the terms:

– Cognition is the flow and transformation of representations, whether it happens within the brain or not (this means that we can talk about cognition being distributed between cognitive tools and human agents).

– The brain is a biological device whose function is to deal with information.

– The mind is what the brain does, as Dennett said. Psychology gives functional explanations of what happens in the brain. These explanations are cognitive to the extent that what the brain does is to deal with information. They constitute an explanatory level that is most appropriate to explain behaviour.

One essential element of the cognitive revolution is its will to investigate on the material realisation of cognitive processes. This first launched research in AI, and this is now understood as requiring interdisciplinary work between brain sciences and psychology. Sperber and Hutchins, by considering that information flows and is transformed also outside of the brain, in the physical and social environment, show the necessity to do interdisciplinary work implicating anthropology and psychology.

L.Q: In your paper "Why There Should Be a Cognitive Anthropology of Science", you say: "In cognitive anthropology of science the issues concern the relationships between folk theories and scientific knowledge and practices".[3] In what sense could we speak of a recursive movement between scientific and folk theories? Quoting Ian Hacking, there is a "looping effect" between the two?[4] How can we describe it?

[3] Heintz (2004: 396).

[4] Hacking (1994).

C.H: In this sentence, I use 'folk theories' with a technical content that has been used in cognitive psychology: folk theories are mental structures that deal with a given domain, such as the physical environment or the behaviour of other people. The issue I mention is whether there is some kind of isomorphy between scientific theories and the mental structures of scientists working with, and thinking about, these theories. If folk theories somewhat change with scientific knowledge, then new scientific theories n+1 emerge out of the mind of scientist who previously had the folk theory n, and the scientific theory n+1, once assimilated produce the folk theory n+1, etc. This would be a looping effect.

My point of view, however, is that folk theories do not change that much with the history of science. Folk theories are to a large extent innately determined and they are framed with our day to day interaction with the world rather than by scientific ideas. I argue that new scientific theories consist in new modes of exploiting unchanging cognitive abilities. These new modes often involves the creation of new distributed cognitive systems, i.e. new tools, symbols or terms are given a role in scientific cognition.

L.Q: If one assumes a general and fundamental discontinuity between the scientific and the folk modes of knowledge production, if a valid description and explanation of the relations between the two is not given, is one not *incorrectly* assuming that there is something like a *set* or *cognitive grid* specific to science, is not one to presume that science is quite different from other forms of knowing? Is science qualitatively distinct from other kinds of knowledge? How do you see, for instance, the relation between science and religion?

C.H: In this question, 'folk modes of knowledge' seems to refer to the usual sense of folk – as belonging to the lay people in general – not to the more specific usage mentioned above – as mental structures.

The question concerning what distinguish scientific beliefs from other forms of beliefs, such as religion, is an old question, which was traditionally answered by philosophical analysis. For instance, K. Popper said (approximately) that what characterises scientific theories from other theories is that there could exist evidence that shows that scientific theories are false, while non-scientific beliefs such as religion, marxism or psychoanalysis cannot be refuted by evidence.

However, finding demarcation criteria is not anymore a central focus of research in science studies. The demarcation criterion was useful for

normative reasons rather than being the result of some description of actual scientific practices. It is now admitted that there need not be one characteristic that distinguish science from, say, religion. Hopefully, knowledge will advance on that topic, but at the moment there is no systematic comparison of practices.

L.Q: As you know, the volume 22/23 of *Antropologia Portuguesa* is basically dedicated to the topic of "violence". In broad lines, is there a way of doing a cognitive anthropology of violence?

C.H: When there is an anthropology of X, then it is always possible to do a cognitive anthropology of X. It is because anthropology is dealing with human behaviour, and this behaviour is determined by the thoughts of the person behaving (there are very rare exceptions). Violent behaviour is included.

Is there a psychological, physiological reward when doing violent acts? If so, how does the reward determine the decision? Is there some deficiency of inhibitory cognitive processes in violent people? – How do people classify certain behaviour as violent? To which extent are fear and disgust involved? Can we find features that would cause people across culture to classify some events as violent? How do people having done a violent act perceive and explain it?

These are questions that pertain to cognitive anthropology of violence: they deal with the cognition involved in decision making, classification, post-hoc rationalisation, perception of social events.

Bibliography

Hacking, I. 1994. The looping effects of human kinds. *In* Sperber, D.; Premack, D.; Premack. A. J. (ed.). *Causal cognition. A multidisciplinary approach*. Oxford, Clarendon Press: 351-383.

Heintz, C. 2004. Introduction: why there should be a cognitive anthropology of science? *Journal of Cognition and Culture*, 4 (3/4): 391-408.

Heintz, C. 2007. *Scientific cognition and cultural evolution: theoretical tools for integrating social and cognitive studies of science*. Ph. D. thesis, Paris, École des Hautes Études en Sciences Sociales. Available at http://christophe.heintz.free.fr/thesis

Latour, B. 1987. *Science in action*. Cambridge, M.A., Harvard University Press.
Latour, B.; Woolgar, S. 1986. *Laboratory life: the construction of scientific facts*. Princeton, N.J., Princeton University Press.

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