
'THE ORIGINS OF MONSTERS' BOOK CLUB

The International Cognition and Culture Institute

January-February 2016

From mid-January to mid-February 2016, the ICCI website has hosted a Book Club devoted to David Wengrow's book, 'The Origins of Monsters', and organised by Olivier Morin. This ebook brings together a Précis of the book by David Wengrow, and commentaries by Alberto Acerbi, Maurice Bloch, Pascal Boyer, Mathieu Charbonneau, Olivier Morin, Karolina Prochownik, Erhard Schüttelz, Thom Scott-Phillips, Jeremy Tanner and Denis Tatone.



2017 © cognitionandculture.net

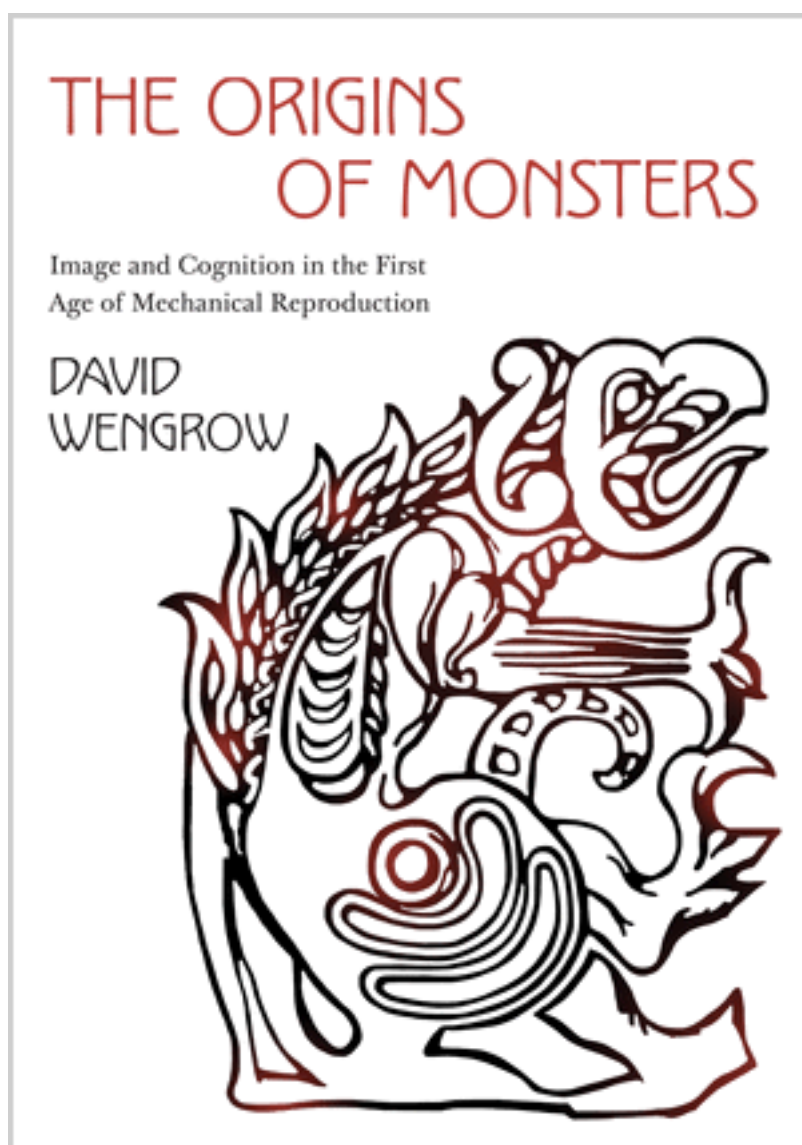
This content is licensed under a Creative Commons Attribution-NonCommercial-NoDerivatives 4.0 International License.

TABLE OF CONTENTS

<i>THE ORIGINS OF MONSTERS: A PRÉCIS</i> <i>David Wengrow</i>	<i>page 2</i>
<i>A COMMENT ON THE ORIGINS OF MONSTERS</i> <i>Maurice Bloch</i>	<i>page 18</i>
<i>COMMENT ON DAVID WENGROW'S THE ORIGINS OF MONSTERS</i> <i>Jeremy Tanner</i>	<i>page 23</i>
<i>THE SPECULATIVE ORIGINS OF MONSTERS</i> <i>Alberto Acerbi</i>	<i>page 31</i>
<i>CHIMAERAS AS ATTRACTORS: EPIDEMIOLOGY AND CULTURAL VARIATION</i> <i>Pascal Boyer</i>	<i>page 36</i>
<i>THE STAMPED, SEALED AND DELIVERED RIDDLE OF THE SPHINX</i> <i>Erhard Schüttpelz</i>	<i>page 44</i>
<i>YOUR VERY OWN MONSTER CREATION KIT</i> <i>Mathieu Charbonneau</i>	<i>page 49</i>
<i>AN IMPORTANT CONTRIBUTION – BUT NOT AN AMENDMENT – TO CULTURAL EPIDEMIOLOGY</i> <i>Thom Scott-Phillips</i>	<i>page 66</i>
<i>CAN CULTURAL EPIDEMIOLOGY EXPLAIN THE CULTURAL EVOLUTION OF MONSTERS?</i> <i>Karolina Prochownik</i>	<i>page 70</i>
<i>THE TALE OF THE THREE-HEADED SNAIL</i> <i>Olivier Morin</i>	<i>page 83</i>
<i>THE SCOPE AND FLAVOURS OF CULTURAL ATTRACTION THEORY</i> <i>Denis Tatone</i>	<i>page 92</i>

THE ORIGINS OF MONSTERS: A PRÉCIS

By David Wengrow



"The Origins of Monsters: Image and Cognition in the First Age of Mechanical Reproduction" Princeton University Press, 2013 (184 pages).

The origins of *The Origins of Monsters* lie in an interview I conducted more than a decade ago with Maurice Bloch (Wengrow 2003). We were talking about the work of Dan Sperber and Pascal Boyer, and more specifically the problem of how to explain striking recurrences in the content of human culture, especially when these relate to such apparently arbitrary things as the form taken by mythological or otherwise imaginary beings.

I had in mind some specific examples, which have intrigued art historians and archaeologists for more than a century. They concern the transfer of images depicting composite beings such as griffins, sphinxes and so on. Images like these have “epidemic” features. They spread with amazing bravado from one society to another, often in periods of major social change. The whole phenomenon begins at a suggestive time – about 6000 years ago, when the world’s first states and urban societies were taking shape in North Africa and the Middle East. Another famous and much studied monster-epidemic occurs in the Iron Age, when imaginary beasts from the East invade the image worlds of archaic Greece and Etruria along with a variety of other social and technological innovations, as part of what is sometimes called the “orientalising” of the Mediterranean.

There are plenty more examples in between, which I try to document in the book.

I had no idea at the time that Rostovtzeff, best known for his (1926) *Social and Economic History of the Roman Empire*, had himself written a series of lectures on the transfer of images, and in particular the fantastic ‘animal style’ of the Central Asian steppe. I have the 2010 Eyjafjallajökull eruption to thank for this discovery. It left me stranded at Princeton University, where the library has a copy of the original lectures. Never mind that most of Rostovtzeff’s ideas were wrong: their existence offered the prospect of academic credibility for what must otherwise have seemed an unlikely project.

Around that time, and for reasons initially unconnected with my research, I was reading an increasing amount of work in experimental psychology, especially about mental modularity, symbolic expression, and theory of mind. A few prehistorians were already exploring the implications of this kind of work for their own material, but in a way which seemed problematic to me (see pp. 4-5 of the book in particular). Still this seemed better than the default position of most culture historians, which is simply to ignore what is going on in fields such as neuroscience and cognitive psychology.

Susanne Küchler (2005) has made similar observations about anthropology, where approaches to human cognition – if made explicit at all – seem torn between incompatible notions of what the mind is and how it works. Is it a highly malleable organ, constantly re-trained through its encounters with a culture-laden world? Or is each of us, by contrast, carrying within ourselves a pristine hunter-gatherer brain that fights its primeval battles through a modern world of its own mysterious making? In their own ways, both alternatives are highly romantic and appealing; but my book joins many others in arguing that the reality is somewhere in between.

I conceived of the book as a test case for what Dan Sperber calls the ‘epidemiology of culture’, and am absolutely thrilled that it has been taken up for discussion by researchers who are serious about exploring the relationship between cognition and culture. But I am also aware that my subject matter raises some immediate problems. I half anticipate that the “epidemiologists” will take the opportunity to explain that I have simply misunderstood what they are trying to do. But I have read their work closely, and taken them at their word when they say that they are interested in understanding how cognition might underpin the spread of culture (culture, in all its manifestations, as opposed merely to language).

The main difficulty with epidemiological approaches to culture, it seems to me, is the mismatch between theory and data. The theory is all about diffusion, popularity, distributions of culture, spreads of representations. But the data used to test this hypothesis seem so far to derive either from experimental (laboratory-type) conditions or from the ethnographic record. This would have been fine a century ago, when mapping out distributions of culture on a large scale was precisely what a lot of anthropologists did. But that kind of work is rarely done nowadays. What we have instead are snippets of culture, isolated examples uprooted from their histories of circulation, and arguably unsuited to an empirical study of how things spread.

Most archaeologists, by contrast, have never lost their love of map-making and the study of distributions. It's arguably what we're best at. So it seemed to me that, in this case, the anthropologists and evolutionary psychologists had proposed a great hypothesis that could not really be tested from within their own material, but which at the same time offers a wonderful invitation for archaeologists to get involved. A second problem is that the definition of 'culture' in evolutionary psychology often seems heavily weighted towards spoken discourse. It has relatively little to say so far about either images or objects (Alfred Gell's [1998] work surely addresses some of this, but Gell was rarely explicit about the kind of cognitive models he was using).

An exception is Sperber and Hirschfeld's (2004) piece on 'The cognitive foundations of cultural stability and diversity'. They present a suggestive discussion of how artifacts such as outlandish masks, caricatures, and cosmetics may stimulate – yet at the same time violate – specific mental modules for face recognition. Such things are intuitively recognised as both face-like and un-face-like, in a way that makes us pay special attention to them. And according to the theory, this may enhance their chances of transmission. It makes them culturally catchy. Pascal Boyer, if I understand him correctly, argues on similar lines that some such cognitive balancing act is necessary or at least desirable if a given concept of the supernatural is going to catch on, and become embedded in some wider cultural milieu:

In any cultural environment, indefinitely many representations of religious entities are constantly created and communicated. Only some of them, however, have the potential to support both imaginative scenarios and intuitive references. These are the ones that combine a rich intuitive base, with all its inferential potential, and a limited series of violations of intuitive theories, which are attention-demanding. Because of these characteristics, such assumptions are more likely than others to be easily acquired, memorized, and transmitted than other assumptions. It should not be surprising, therefore, that they constitute the most recurrent aspects of religious systems.

(Boyer 1994: 122)

Can this kind of theory be legitimately extended from spoken discourse about supernatural beings to the world of images? I don't see why not, and two particular sources of inspiration here were the work of Barbara Stafford (especially her 2007, *Echo Objects: The Cognitive Work of Images*) and *La fabrique des images*, an exhibition hosted at the Musée du quai Branly in 2010-11. Philippe Descola and Anne-Christine Taylor, its creators and organisers, were kind enough to show me around on its final day. The timing could not have been better for me, and it will already be obvious that – like its subject matter – my book is very much a composite of influences, thrown together more or less by chance.

From *La fabrique des images* I took two things. The first was a way of approaching modes of figuration as types of visual experimentation, at once anatomical and theoretical. The exposition showed how such experiments offer points of entry to more general principles, by which people try to organize the world around them in some sort of systemic relationship with other worlds “beyond” – the imagined worlds of the supernatural. The second thing I took away was a more technical point about the production of composite figures, which Descola associates with an ontological stance called “analogism”. It’s the same point made by Da Vinci in his notebook entry on *How to Make and Imaginary Animal Look Natural*: that precisely because of their fictional character, the creation of visually compelling composites requires enhanced empiricism and accuracy in the depiction of individual body parts, each of which should be independently identifiable as belong to a certain kind of species (Arcimboldo’s 16th century faces take the same principle to another kind of extreme). I also found it fascinating that nearly all the examples of analogism derive from large-scale, hierarchical societies such as Han China, medieval Europe, or the historical kingdoms of Benin. I discussed this point with Philippe and Anne-Christine, but I suspect this is roughly where our interests begin to diverge.

In Barbara Stafford’s work I found a scaffold that bridged research on mind and image. It emboldened me to expose data of an archaeological and art historical kind to an “epidemiological” approach. Stafford uses the term “compressive compositions” for images that ostentatiously combine elements from different species, and she suggests that such images draw attention to otherwise unconscious systems of visual processing. For example, experimental studies show that human cognitive processing of animal forms is highly sensitized to part-whole relations (Davidoff and Roberson 2002). A total presence can be inferred from quite limited visual cues (horns, tails, feet and so on). This inferential capacity we owe to an intuitive repertory of biological information that is hard-wired, and is part of our evolutionary make-up (New et al. 2007). Pictures of animals – even when jumbled, distorted, or incomplete – may thus activate neural pathways attuned to the recognition and classification of living kinds.

In violating some limited part of this intuitive biology, composite figures nevertheless affirm many of its underlying structural principles. Even such fantastic beings as a dragon, mermaid, centaur, minotaur, unicorn, sphinx, or whatever, will have legs correctly positioned for walking, eyes for seeing, wings for flying, horns for gorging, fins for propulsion, and so on, allowing us to infer (often extraordinary) properties of movement and vitality for such figures. They are, in short, the visual counterparts to Sperber and Boyer’s “minimally counter-intuitive representations”. And if the latter are right, then such representations might be expected to exhibit special “epidemic” properties of transmission, of a kind that should leave traces in the record of human image-making. Or at least that was the premise from which I began my own experiment in tracking distributions of composite figures.

Of course this was not entirely a “blue skies” experiment. I already knew that, from at least around 4000 BC, “monster epidemics” were a striking and recurrent feature of the visual record. I diligently began to compile evidence for the earliest widespread appearance of composite beings in the art of Egypt, Mesopotamia, Pakistan, Greece, and so on, and to map out the likely relationships between

them. In nearly all cases the appearance of monsters on the scene correlates with a number of other historical features, which seem non-random and important in terms of modes of transmission. Not all of them will apply to every case, but for western Eurasia and the Mediterranean world they can be summarized under five broad headings:

1. Mechanical image-making devices: the use of stamps, moulds, and seals (remote ancestors of the modern printing press) to replicate and promulgate officially sanctioned images in standard formats and on a wide scale. This took place mostly through the administrative offices of temples, palaces, and other urban institutions.
2. Centralisation: composite figures seem most at home in (what was then) the newly emergent social world of the city, whether the ancient city-state or some other kind of large-scale territorial polity, such as the Bronze Age kingdoms of North-East Africa, or the Neo-Assyrian Empire. Monsters are urbanites, and highly cosmopolitan ones at that.
3. Social transformation: composite figures typically appear in times of major social and economic restructuring – the phases that archaeologists love to call ‘formative’, ‘archaic’ or ‘proto’ (as in Archaic Greece or Protodynastic Egypt) because they precede and foreshadow the emergence of states with their official canons of cultural representation.
4. Conservatism: these composite figures are rarely original or local flights of imagination. More commonly – as with the serpent-necked felines on the ceremonial cosmetic palette of King Narmer – they are imports from an exotic and distant source (and this they have in common with those much later “marvels of the East”, famously discussed by Wittkower and later Mitter, whose movements from India to Europe can be traced in images from late antiquity to the age of the modern printing press).
5. Apotropaism: where composite figures can be associated with rich iconographic repertoires and/or ancient written sources, these frequently implicate them in the use of magic and medicine to avert misfortune, including protection from the spread of harmful diseases. The Neo-Assyrian empire furnishes wonderfully detailed examples, such as the corpus of cuneiform texts from the House of the Exorcist at Assur, with their pedantic instruction-manuals for the making and placement of protective images, and injunctions to send sickness-bearing demons “3600 miles” away from their intended human targets.

Based on these five factors it might be reasonably argued that composite figures do, in fact, exhibit (almost literally) epidemiological features in the record of human image making, far outstripping most other kinds of imagery in their scale of distribution (and this includes, of course, other types of image transmitted by mechanical reproduction). In which case we would have discovered a good empirical demonstration of how cognitive dispositions works in practice to create spreads or distributions of culture. The difficulty, of course, is that all my examples derive from the last 6000 or so years of human

history – a relatively shallow period of time in evolutionary terms. If the striking distribution of imaginary composites in the visual record is to be explained in terms of innate pre-dispositions, then what about the many preceding millennia of human image making. Where, in short, are all the prehistoric monsters?

Here we enter slightly murky waters. Some of the initial reactions to my argument were along the lines: but what about the famous ‘Sorcerer’ of Les Trois-Freres Cave, which dates back to the Upper Palaeolithic? Or the ‘Bison-Man’ of Chauvet and the ‘Lion-Man’ of Hohlenstein-Stadel, which at 30,000 BC are twice as old still? But even if we accept the identification of these and some tens perhaps of other very early prehistoric figures as composites (and this is not uncontroversial – as pointed out by Dale Guthrie, for instance, Lion-Man may simply be “standing bear”), they remain strikingly isolated within the much wider corpus of images that survive from Palaeolithic and Neolithic times. The majority of those images, as I discuss in chapter 3, are more or less schematic representations of animals and other subjects that could actually be seen in the worlds of Ice Age hunter-gatherers and of the first farmers.

The question is not whether early humans were capable of producing images of fantastic, composite beings: they undoubtedly were, and they undoubtedly did. The question (of my book at least) is whether such images ever caught on, becoming stable and widely transmitted features of prehistoric culture. And while there may be limited exceptions, the general answer, it seems, is an emphatic: ‘no’. As Andre Leroi-Gourhan pointed out half a century ago, ‘Palaeolithic art offers very few examples of what might be construed as flights of the imagination. Its monsters can be counted on the fingers of one hand’ (or today, perhaps, two hands and a foot).

Modern scholars may love to disseminate and talk about images of prehistoric bull-men and so on. They open the door to discussions of early religion and the imaginary worlds of our hunter-gatherer ancestors. But the ancestors themselves just don’t seem to have shared this fascination. So rare, in fact, are these kinds of images that one almost suspects a policy of avoidance or prohibition. Such a policy would in fact be quite familiar to more recently documented groups of hunter-gatherers or small-scale cultivators, who follow the ontological precepts of ‘animism’ and ‘totemism’ (in Descola’s sort of sense). Consult the catalogue of the quai Branly exhibition and you see that the kind of anatomical reshuffling required to produce composites is generally at odds with the plastic and visual arts of such groups.

It is, by contrast, a pervasive feature in their performance arts, where ritual actors take on attributes of animal and other non-human bodies, and vice-versa. These are the image worlds of masks and short-lived effigies, destined to vanish in spectacular rites of expiation. For reasons of prudence, among the San or Inuit for example, such effigies were not traditionally rendered as permanent images. More often, crossing the boundaries between species also meant having to navigate a safe return – as epitomized in the figure of the trickster, shaman, or shape-shifter – or in the multi-layered

masks of the Kwakiutl that flicker open and shut in ritual performances, affording glimpses of a human face lodged in an animal body, but never more than a glimpse.

To depict such states of mediation or mid-transformation in durable form may invite danger, by leaving open an extended trace of a relationship (between human and “other”) that should be properly circumscribed by rites of passage. Images of composite beings, rigid and unchanging, thus evoke the principles of metamorphosis and liminality, only to subvert them. They fix transformations in stable media that render them capable of being replicated and disseminated, over and over again, in canonical forms. Composites thus typically belong to the image-worlds of cities and hierarchical states: mechanized, modularized, standardized, and centralized. The basic point was made long ago by Elias Canetti, in those sections of his (2000 [1960]) *Crowds and Power* that deal with the topic of transformation. Over the decades related points were developed by, among others, Victor Turner, Ruth Benedict, Fredrik Barth, and more recently Harvey Whitehouse (2004) with his distinction between ‘imagistic’ and ‘doctrinal’ modes of religious transmission.

In some other respects too, the ‘cultural ecology’ of cities seems especially well suited to the production of composites. Not just in imagery but also in other spheres of technical production. In the central chapter of the book I try to show how principles of modularity and standardisation – all basic to composite figuration – can in fact be found at work more generally in styles of craft and industry that developed in the early urban centres of western Asia, north Africa, and the Mediterranean. From the fourth millennium BC onwards, new levels of uniformity and precision are evident across a whole range of activities from the ceramic arts to techniques of making furniture and buildings, and also in modes of depiction. Similar points have been made for the bronze-working systems of Shang China and their associated forms of ornamentation. There, from around 1500 BC, images of anatomical composites (like the taotie figures on metal vessels) proliferated within an urban industrial complex that allowed manufacturers ‘to assemble countless combinations from a limited repertoire of motifs and compartments’ (Ledderose 2000: 25).

Of course, generating signs from standard components is also a favourite pastime of bureaucrats and administrators the world over. Some of the earliest evidence for this kind of activity can be traced back to ancient Mesopotamia and Elam – today’s Syria, Iraq, and parts of western Iran. There the first known forms of literate administration were concerned with classifying, archiving, and monitoring the flow of resources – both raw materials and finished goods – as they passed through large urban institutions: so many hundred jars of beer or oil, so many thousand sheep, so much grain or textiles. Such institutions combined temple and factory functions, forming the religious and economic hubs of cities such as Uruk and Susa – this was bookkeeping for the gods. The surviving inscriptions (written in scripts known as proto-cuneiform and proto-Elamite) also show that resident scribes sometimes engaged in what Robert Englund calls “fanciful paradigmatic name-generating exercises”, producing long lists of signs, many of which seem never to have been used for any practical purpose whatsoever.

A further function of temple administration was to guarantee the authenticity of finished goods by applying seals to them, or rather to their containers. The miniature impressions, thus applied, offer one of our main sources of evidence on the contemporaneous development of pictorial design, and often show lively arrangements of animals and people engaged in a variety of activities and postures. Such markings also testify to a close relationship between skilled depiction (in intaglio carving) and bureaucracy as linked domains of urban activity. The entire system of marking and recording was predicated on the constant generation of new visual signs that could fulfil their designated purposes, as discrete signifiers within an expanding system of administration. Under such conditions it is easy to see why the “bureaucratic eye” was drawn to the possibilities of composite figuration which – quite apart from being fun and memorable – would have greatly multiplied the range of possible subjects for depiction (i.e. instead of just ‘cow’, one suddenly has the possibility of ‘body of cow + bird head + wings’, or any other number of possible combinations).

This precis is now wandering a bit too far from its initial questions concerning the relationship between culture and cognition, which is supposed to be the main concern of this forum. So let me take a step back and return to where I started, posing again the question: can the origins of monsters (or, more precisely, of composite figuration) be taken as a test case for an “epidemiological” approach to the evolution of culture? And, if so, then what are its main conclusions? On this point I fully expect a lack of consensus from the readers, to whom I am deeply grateful for their attention to my book. My own conclusions are roughly as follows:

As minimally “counter-factual” or “counter-intuitive” representations, images of composite figures do indeed have “epidemic” qualities, which enhance their chances of transmission within and between populations. How else would we account for the cultural survival, in our own modern imaginaries, of such arbitrary Bronze Age creations as the griffin or unicorn, and for the apparently endless capacity of such figures to acquire rich new meanings and associations across the ages? But I would also argue that these epidemic qualities were unleashed only under certain sets of conditions, which are much more limited in range than the universal cognitive capacities that underpin them. Such conditions have been prevalent only in certain parts of the world, and only for what is – on an evolutionary time-scale – a relatively short episode in the history of our species.

Another way of putting this would be to say that the “intuitive” basis for the cognitive reception of monsters has not one, but two foundations – it is a sort of “double intuitive”. On the one hand, it rests upon the mind’s innate and evolved tendency to compensate for gaps in the visible world, conjuring organic-seeming wholes out of ill-fitting parts, and ascribing them properties of living beings. This inferential capacity has no doubt been with us since the origins of our species. But at another level, the distinct epidemiology of monsters is a product of institutional environments existing only since the appearance of the first cities, and with them (as Jim Scott might have it) our first systematic attempts to “see the world like a state”: as a realm of standard and divisible subjects, each comprising a multitude of separable and combinable parts.

The latter and much more recent development is, I argue, what lay foundations for the enhanced orders of “monster-reception” that would be predicted by an epidemiological approach, and that are so evident a feature in the record of human image-making from the Bronze Age onwards. More generally, I suggest, it is in reconstructing such emergent properties of cognition over historical timescales that an archaeological – rather than purely lab-based or ethnographic – approach remains important for understanding cultural transmission and cultural evolution.

References

- Boyer, P. 1994. *The Naturalness of Religious Ideas: A Cognitive Theory of Religion*. Berkeley, CA and London: University of California Press.
- Canetti E (2000 [1960]) *Crowds and Power*. New York: Farrar, Straus and Giroux.
- Davidoff, J. and D. Roberson. 2002. Development of animal recognition: a difference between parts and wholes. *Journal of Experimental Child Psychology* 81: 217-34.
- Descola, P. ed. 2010. *La fabrique des images: visions du monde et forms de la representation*. Paris: Somogy/Éditions D’art.
- Englund, R. K. 1998. Texts from the Late Uruk period. In *Mesopotamien: Späturuk-Zeit und Frühdynastische Zeit, Annäherungen 1*, ed. P. Attinger and M. Wäffler, pp. 15-233. Fribourg: Universitätsverlag.
- Gell, A. 1998. *Art and Agency: an Anthropological Theory*. Oxford: Oxford University Press.
- Guthrie, R. D. 2005. *The Nature of Paleolithic Art*. Chicago and London: University of Chicago Press.
- Küchler, S. 2005. Materiality and cognition: the changing face of things. In *Materiality*, ed. D. Miller, pp. 206-30. Durham and London: Duke.
- Ledderose, L. 2000. *Ten Thousand Things: Module and Mass Production in Chinese Art*. Princeton: Princeton University Press.
- Mitter, P. 1992. *Much Maligned Monsters: A History of European Reactions to Indian Art*. Chicago: University of Chicago Press.
- New, J., L. Cosmides, and J. Tooby. 2007. Category-specific attention for animals reflects ancestral priorities, not expertise. *Proceedings of the National Academy of Sciences of the United States of America* 104: 16598-16603.
- Rostovtzeff, M.I. 1926. *The Social and Economic History of the Roman Empire*. Oxford: Clarendon Press.

– 1929. *The Animal Style in South Russia and China, being the Material of a Course of Lectures delivered in August 1925 at Princeton University under the auspices of the Harvard-Princeton Fine Arts Club*. Princeton Monographs in Art and Archaeology 14. Princeton; London: Princeton University Press; H. Milford, Oxford University Press.

Sperber, D. and L. A. Hirschfeld. 2004. The cognitive foundations of cultural stability and diversity. *Trends in Cognitive Sciences* 8: 40-46.

Stafford, B. M. 2007. *Echo Objects. The Cognitive Work of Images*. Chicago and London: University of Chicago Press.

Wengrow, D. 2003. Machiavellian moments: an interview with Maurice Bloch. *Journal of Social Archaeology* 3: 299-311.

Whitehouse, H. 2004. *Modes of Religiosity: A Cognitive Theory of Religious Transmission*. Walnut Creek, CA: Altamira.

Wittkower, R. 1942. Marvels of the East: A Study in the History of Monsters. *Journal of the Warburg and Courtauld Institutes* 5: 159-197.

Comments

Olivier Morin

Two questions fell from the last draft of my comment post. Here they are, in no particular order. As a non-specialist, I expect to be wrong about many particulars, so take these as curiosity questions rather than objections.

1. The *Origins of Monsters (TOM)* is keen to focus on iconography, as opposed to live performances (and the masks that often accompany them), or verbally recounted tales. I understand that, as an archaeologist, you want to focus on what fossilises, so to speak. Yet *TOM* gives the impression of dismissing the evidence for cultural beliefs and practices involving hybrid animals in tales or performances as not worth considering, your investigation being about images and images alone. This might be problematic. The “MCI monsters” theory that informs your account was developed to account, first of all, for beliefs (and the tales and legends reflecting them)—not for images alone. Besides, taking tales and performances into account might reveal that (contrary to *TOM*’s apparent claim), representations of hybrid animals are very common in non-state societies.
2. Speaking of which, *TOM* seems to ignore a line of evidence that might be relevant. First, iconographic material from contemporary (i.e., from the 19th c. onwards) small-scale societies depicts hybrid animals. (I guess you might reply that these societies could have adopted this imagery from surrounding states, but you also claim that state institutions determine a society’s readiness to borrow composite imagery, and not just to invent it.) Relatedly, *TOM* addresses prehistoric cave paintings in a very illuminating way—but, even if your interpretation of it is right, can we turn absence of evidence into evidence of absence? After all, normal animals are (it seems to me) more numerous than composites in many visual cultures, including some that are truly fond of monsters. Taphonomic biases may well have erased most prehistoric monsters (except for a few items).

David Wengrow

I think I deal with the relationship between images and other media a bit more carefully than you seem to give me credit for (for instance, pp. 28-32 of chapter 2, and then also in chapter 3, 'The hidden shaman'). You will also find there some discussion of toponymy, and the importance of considering what has not survived.

As you say most cognitively based studies of cultural transmission have so far taken language and verbal expression as their main source of evidence. I think it's high time to reconsider the 'uses of images', as Ernst Gombrich put it, but with a more up-to-date psychology.

You ask how my argument relates to the visual products of recent hunter-gatherers and small-scale cultivators. This is something I would certainly like to discuss more. But perhaps we should await the contributions of Susanne Kuechler and Erhard Schüttzel? I wonder, in the meantime, what you make of the Descola exhibition, and my thoughts on it?

Your questions raise a couple of other points that I want to try to clarify.

From the point of view of cognition, I think it would be an analytical dead-end to collapse all cultural representations into a single category or genre. Images, verbal expressions, and ritual enactments – even if based on shared beliefs – are not the same thing. They have different properties of transmission, and the differences are in my view important.

I think this is the problem with some current applications of experimental psychology in the social and historical sciences – they treat external (public) representations as direct reflections of internal (mental) representations, without attending to how different media and different genres of expression organise thought into patterns that are historically distinctive.

From the standpoint of cultural transmission, I wonder whether the whole issue of "belief" (which you raise) may be slightly beside the point, or at least not central. I doubt whether many people today actually believe in the existence of "monsters" (or "witches", or whatever), but their cultural epidemiology is surely more rampant than at any time in the past.

I'm not sure I actually share your view of archaeologists as being attracted to "what fossilises". Most of the ones I know (including me) would love nothing better than to access living performances and stories from the remote past. But unless we base our interpretations on something empirical, we are engaging in pure speculation. I think it unlikely that the known corpus of Palaeolithic parietal art is statistically trivial as you suggest. There is just too much of it, and I recommend Dale Guthrie's (2005) 'The Nature of Palaeolithic Art' for a survey.

What is striking, I think, is the degree of emphasis that has been placed on a tiny handful of surviving figures from the Upper Palaeolithic that do appear to depict composite or imaginary body-forms. On the one hand this says a lot about archaeologists' determination to access the intangible dimensions of prehistoric experience. But on the other it gives a misleading impression of the frequency of such figures in prehistoric art, which is in fact remarkably low. This to me is the more interesting point.

No doubt there were costumed performances and such like, mixing body parts from different species into "ritual composites". We even catch a glimpse of these in prehistoric burial customs (pp. 37-38 of the book). But an important part of my argument, picking up from Canetti and others, concerns the issue of transformation, and the crucial difference between performance and image in this regard. A fixed and static image of a composite figure is not the same thing as an enacted transition between bodily states (and then back again) – the latter represents a genuine transformation, the former a refusal to transform.

Olivier Morin

Thank you for this illuminating answer. My first two questions were extremely clumsy and masked a key point: Your treatment of the representations, practices and performances surrounding composite animal imageries is admirable, especially in the book's chapter 3.

The book is making a very ambitious claim—hybrid animals thrive in state societies with a complex technical and commercial infrastructure, not, or much less, elsewhere—and this requires you to explain away a lot of evidence pertaining to the mental representations (“belief” is, I agree, an awkward term in this context, to be avoided) and practices surrounding hybrid animals in societies that did not leave monsters set in stone (or carved in clay). TOM's level of engagement with the ethnographic, art-historical, and psychological literature is quite exemplary. What I wonder is this: Does TOM give definite proof that mental representations of composite animals are much more prevalent in state societies with modular technologies and mechanical reproduction of images, than elsewhere? I am not quite sold to this claim yet, and perhaps neither are you.

The same claim is much easier to defend, however, if we restrict its scope to images set in stone, as opposed to representations, tales, performances, and practices that leave no solid trace. I read TOM as making precisely that move.

For instance, p. 37: “The significant point, to which I will return, is that if such beings [composite animals] did populate the collective imagery of early hunter-gatherers, then their presence was made manifest predominantly through ephemeral modes of display such as psychotropic visions and masked performances that have left few durable traces.” Earlier (pp. 33 sq.), TOM makes a similar point about contemporary hunter-gatherer societies.

This is important because, later in the book, you use the claims above to criticise the view that representations of monsters might be universally appealing. The reason you can use these claims in such a way is because TOM carefully defined away the representations and practices involving monsters in hunter-gatherer societies, on the grounds that they usually leave no solid traces (at least that is my interpretation). Yet narratives and performances concerning monsters possess most of the cognitive properties that are also possessed by composites carved in stone or impressed on clay—if anything, in narratives and performances involving metamorphoses, men morphing into animals and back again are more counter-intuitive than simple composites.

I agree that Guthrie's *Nature of Paleolithic Art* does much to dispel the view that monsters were common in prehistoric cave art, and the way he explains away many so-called monsters as clumsy attempts by children to make realistic animal images feels quite compelling. On the whole, it gives great support to your thesis.

I would also read his book as containing some evidence that Paleolithic hunter-gatherers had a complete and vivid understanding of the modular nature of vertebrate anatomy: bodies made of interchangeable parts, following a broadly universal body plan. Cave paintings of headless or siamese animals (cf. p. 393 of Guthrie's book, for instance) seem to show as much (and in a way, this is to be expected from a society where so much time was devoted to butchering and processing carcasses).

Martin Fortier

Thank you for this very interesting and thought-provoking piece. A series of questions/objections came to my mind through this reading. Hopefully I don't misconstrue the points you have made, but please correct me where I'm missing what you actually meant.

1. I quite like the idea of expanding the epidemiological approach (EA) beyond the restricted scope of discourse. The tricky question, however, is whether such a move is legitimate (i.e., supported by experimental evidence). As you notice, so far the EA has been dealing almost exclusively with verbal representations. For example, when Boyer & Ramble (2001) show that a specific kind of counterintuitivity makes representations particularly catchy they are talking about verbal representations.

Your proposal is to apply the EA to public **material** representations. The problem is that the strand of evidence traditionally used by the EA literature cannot support the move to material representations. Indeed, while a study such as Boyer & Ramble (2001) tells us something about the catchiness of verbal representations it doesn't say anything about the catchiness of material (artistic, artefactual) representations.

In your book you adduce two main experimental studies in favour of your move towards an EA of material representations (Davidoff & Roberson, 2002; New, Cosmides & Tooby, 2007). First, it seems to me that these studies should be taken cautiously. To my knowledge, Davidoff & Roberson's study hasn't been tested cross-culturally; now it seems that at least in some cultures (e.g., Diamond & Bishop, 1999; Medin & Bang, 2014) contextual cues do play a crucial role in the recognition of animals; it is hence not clear which of the three components (parts, wholes and context/situation) plays the main role in animal detection and how these components relate to each other. Regarding New et al.'s study, I think the importance of domain-specificity (e.g., distinguishing between animals and inanimate objects) in vision has definitely some grain of truth, but still things are more complicated than what is often suggested by the proponents of strong modular views (VanRullen & Thorpe, 2001). However, for the sake of the argument, let's assume that these studies can be taken at face value. Well, in my view, it still remains that these studies don't support the move from verbal EA to material EA. Indeed, in order to support the EA, it is not sufficient to demonstrate that **recognition** is based on parts detection rather than wholes or that **change detection** is faster and more accurate with animals rather than with inanimate objects; what needs to be shown is that some material representations are more **catchy** – are more easily **recalled** – than others. As far as I know, you never adduce experimental evidence demonstrating this very last point.

A recent development of Dan Sperber's work sheds an interesting light of this question. Let's put it this way: lately, Sperber has been trying to import the EA from the realm of verbal representations into the realm of (ritual) action. As previously, one may wonder: is such a move legitimate? I think in this case, it is indeed perfectly legitimate. And the reason for this is that there is clear evidence that patterns found in opaque actions (e.g., Gergely & Csibra, 2006) or learned conventionally – as opposed to instrumentally – (e.g., Legare & Nielsen, 2015) are particularly catchy. Not only is the EA of ritual actions experimentally supported; but it can furthermore be very elegantly applied to ethnographic data – e.g., to cargo cults, in which causal opacity plays a key role (Umbres & Sperber, Forthcoming).

To conclude on this and to put my point in even more general theoretical terms, let's say that one can think of at least four versions of the EA: (i) one applied to verbal representations; (ii) one applied to material representations; (iii) one applied to experience; and finally, (iv) one applied to action. On the one hand, it seems to me that there is to a certain extent clear evidence in favour of versions (i) (e.g., narratives about spirits) and (iv) (e.g., cargo cults) of the EA. On the other hand, I remain unconvinced that the EA (that is, the key concepts of counterintuitivity, opacity, quasi-propositional format, etc.) can be legitimately and fruitfully applied to material representations (e.g., an engraving representing extraordinary monsters) or to experiences (e.g., a mystical experience).

2. I find the thesis that there is a blatant scarcity of material representations of chimeric creatures in “stateless” or hunter-gatherer societies quite disputable. Arguably, Amazonian societies (to restrict myself to the region I know best) provide numerous examples averring the fact that chimeric representations are indeed widespread among hunter-gatherer (or, to be more precise: in hunter-horticulturist) societies. This is so much the case that very recently, the confidential world of French americanist anthropology has been stirred up by a heated and vehement debate between Dimitri Karadimas and Carlo Severi. The moot point precisely concerned the status of chimeric representations; interestingly enough, many of the ethnographic data on which the discussion was based were drawn from Amazonian societies.

If I understood you well, in the book you do acknowledge that some chimeric representations can be found in hunter-gatherer

societies, but then, you point out that such representations are strictly restricted to masks and performance. Yet, many Amazonian examples of chimeric representations, and notably those discussed by Karadimas (2003, 2007, 2008, 2015a, 2015b) and by Severi (2011, 2015 [2007]), consist not only of masks or of performance clothing, but they also consist of baskets, pottery, more or less lasting body paintings, clubs, etc. It seems to me that this strand of ethnographic evidence somewhat challenges the thesis you are putting forth.

Detailing Karadimas' and Severi's respective take on the question of chimeric representation would lead me beyond the scope of my comment. But to put it in a succinct and rough way, Karadimas has it that a chimera is nothing but ethnobiological knowledge about ordinary animals gone extraordinary (i.e., gone chimerical) through the distorting mechanisms of analogical inference and knowledge transmission whereas the author of **The Chimera Principle** considers that chimeric arrangements stem from basic cognitive mechanisms related to memory and imagination.

I don't want here to adjudicate between the two authors, but suffice it to say that, arguably, each thesis defended respectively by Karadimas and by Severi proves to be incompatible with (and hence challenging to) the main thesis of **The Origins of Monsters**. If Karadimas is right, then there is no genuine chimeric representation, for every chimera can be boiled down to some cryptic ethnobiological knowledge; the trouble for you is that in his view this is true not only for Amazonian chimeric representations but also for any kind of such representation. Indeed Karadimas has applied his theory not only to Amazonian material but also to material pertaining to Mesoamerica (Karadimas, 2014) and to Europe (Karadimas, 2010) (see next point). His view is that the analogical inferences through which chimeras are created is virtually universal. If, on the other hand, Severi is right, then chimeric representations are genuinely such; but again, this is quite challenging for **The Origins of Monsters** for Severi's claim is that the basic cognitive mechanism he dubs "chimera principle" is widespread in "stateless" societies. It thus appears that Severi's thesis is, as it were, the opposite of the one advocated in **The Origins of Monsters**.

So my general question is: how do you deal with this kind of data coming from Amazonian ethnography? And how would you position yourself with respect to Karadimas' and Severi's respective theory of chimeric representations?

3. Philippe Descola's grand theory of ontological schemas is broad-ranging and very stimulating indeed; but probably precisely because of its breadth it is also very doubtful. I have spent hours thoroughly reading and reading again **Beyond nature and culture**, and now I tend to think that numerous claims of this book remain (conceptually, ethnographically and cognitively) ill-supported. It would be way too long to mention all the problems of Descola's theory. Let me simply mention one which is directly related to the topic of your book. One central claim of **Beyond nature and culture** is that a systematic coherence can be found between modes of identification (i.e., the way internal and physical properties are ascribed to surrounding entities) on the one hand and modes of figuration (e.g., whether one represents chimeras or not) or mode of production (e.g., whether one domesticates animals or not) on the other. According to Descola, being an animist (i.e., recognising similar interiorities and dissimilar physicalities between entities) has very specific implications as to how figuration and production will be performed and implemented. Inter alia, Descola states that within the animistic ontology, it is impossible to produce works of art where composite beings are represented or to indulge in herding. Descola claims that his framework is useful and fruitful to the extent that it can explain, for example, why Amazonian people have always been so reluctant to breed animals or to depict supernatural beings in the way Incas or Aztec would do so. The strength of the four ontological schemas would be that they allow us to make very specific predictions: given a specific ontological schema we can confidently infer a set of practices compatible with such a schema and a set of other practices which are incompatible with it.

So far so good. The problem, however, is that there are plenty of cases which seem to be at odd with Descola's predictions. Here is one example. In **The Origins of Monsters** (chap. 2, fn 24 and 25), you refer to Karadimas' work on composite beings (Karadimas, 2010). In his chapter, Karadimas mainly uses European material. Strikingly enough, his ideas nicely fit with Descola's overall framework (Descola (ed), 2010): Karadimas shows how widespread composite beings were in the iconography of the Middle Ages and of the Renaissance (i.e., in a time where, in Descola's terms, the analogical ontology was prominent). And yet, as explained before, Karadimas' work deals only partly with European iconography; most of his work concentrates on the Miraña people of Colombian Amazonia. Now it is particularly interesting to notice that Miraña people, who, by all Desco-

lian standards possess an animistic mode of identification, are nonetheless prone to represent composite beings through various mediums. We can then wonder: how is it that one can be an animist (i.e., interact with spirits before hunting expeditions, transform herself into an animal, and furthermore be very reluctant to breed any animal) and yet represent chimeras while Descola clearly predicts that such representations are only compatible with analogism?

A way to solve the problem would be to say: in fact, Miraña people are analogists and not animists. But this would sound way too ad hoc. Moreover, it wouldn't solve anything for we would then have to explain how is it that one can be an analogist and still be reluctant to breed animals while Descola's framework predicts that the two are not incompatible whatsoever. To be sure, the framework developed in **Beyond nature and culture** is very elegant; but, it seems to me, it is replete with numerous pitfalls similar to the one just mentioned. If you choose to endorse this wobbly framework, as you seem to do in **The Origins of Monsters**, you then have to explain, among other things, why is it that composite beings can be encountered in the Amazon just as easily as they can be encountered in the iconography of the Middle Ages.

4. My final question concerns the specific scope of your thesis that "composites typically belong to the image-worlds of cities and hierarchical states". What do you exactly have in mind when you speak of "cities and hierarchical states"?

Agriculture, urban centres, hierarchy and State-structure are often lumped together, as if they were all tightly co-dependent, but ethnographic and ethnohistorical data suggest otherwise: there are cases in which these four components are not co-present. For example, the Calusa were famous for being a hierarchical and yet a non-agricultural society (Goggin & Sturtevant, 1964; Widmer, 1998).

So, which component(s), among the four above-mentioned, is (are?) definitely required for representations of chimeric creatures to appear and to spread around? The reason why I would like to know which of the four components you deem to be crucial is that it could help us clarify what the situation is in the Amazon. At first sight, no agriculture, no urban centres no hierarchy and not state is to be found in Amazonia. But here are some subtleties and nuances. First, pre-Columbian and pre-historic Amazonia looked very different from what it is now. The forest was traversed by plenty of roads. Populous towns had been settled in many places and especially in the regions contiguous to the rivers. The mode of production and the economy as well were very different from what they are now. Even if we consider present-day Amazonia, things are a bit more complicated than what appears at first glance. Some (relatively weak) forms of social stratification can be encountered in groups of the north-west amazon while it is extremely difficult to find similar patterns among the Pano groups. All these points have been abundantly documented and discussed in the last years (Balée & Erickson, 2006; Denevan, 1991; Hill, 1996; Hornborg, 2005; Neves, 1999; Santos-Granero 1992, 2002). As I suggested above, the existence of composite beings in Amazonian iconography is problematic; given the very diversity which can be encountered within the Amazon, things turn out to be even more complicated (e.g., Arawak societies are different from Pano societies). And, on the top of that, the various heterogeneous historical layers which define contemporary Amazonia make things even more difficult.

To conclude, I want to say that I have mainly addressed ideas of your book I found disputable and I haven't say a word about those which I found utterly convincing, but I want to make clear that, by and large, I did find your ideas utterly convincing!

David Wengrow

I am working towards a fuller response with regard to Carlo Severi's book and how it relates to mine. The 'Origins of Monsters' and the 'Chimera Principle' tackle much the same problem from different angles, and this makes the comparison exciting. Thank you for pushing the discussion in that direction.

For the moment I just want your help in clarifying a few things. I am no expert on Amazonian societies, but I have read Severi carefully, admittedly in the new English translation. It seems to me that what you describe as 'material representations of chimeric creatures' actually have a very specific character and place in his argument, and that he himself is keen to differentiate them from the kind of composite figures discussed in my book.

This distinction seems crucial for Severi, and he goes into it in some detail: see for instance the careful contrast he draws between the Greek chimera and the Hopi snake-lightning-bird figure (on p. 67 ff. of ‘The Chimera Principle’), in terms of their distinct principles of integration. The difference, as I understand him, resides in the relationship between image and memory.

Old World composites, of the kind I’m concerned with in my book, are constructed in a way that leaves very few gaps on the visual plane – they are full and often compelling visual representations of animals that don’t exist, and as such they could serve among other things as illustrations to sequential narratives.

The ‘chimerical representations’ of the New World that Severi writes about cannot possibly be represented in such a simple and unambiguous way on the visual plane, at least not as he describes them. They are not illustrations to narratives, but part of a far more complex memory apparatus that is activated in shamanic journeys between the visible and the invisible worlds.

If I understand Severi right, the ‘chimera principle’ in Amazonian art – and other American examples he gives – works through incompleteness, using visible images as indirect cues for mental images that have no tangible existence. Take for instance his discussion of the celestial jaguar, whose oscillation between bird and jaguar is in fact conveyed mainly on the auditory plane, and only very obliquely through visible images.

This seems completely consistent with the idea that stable and tangible images of mid-transformation are generally to be avoided in such contexts, because the shaman’s capacity as a heroic interlocutor with the invisible is based on remaining in a state of perpetual transformation. Whether the shaman happens to be a hunter-gatherer, horticulturalist, or city-dweller is a secondary point.

As Severi himself puts it, the Kuna shaman is trapped in a series of unremitting metamorphoses, while his celestial foe – the bird-jaguar – cannot be represented in images other than those that ‘appear either through dazzling sunlight or else as dream-images that can only be seen by closed eyes’.

That is not to say there are no images of hybrid figures in indigenous American art, but rather that they have an indexical role in relation to language, ritual, and memory quite distinct from that of my Old World composites. As Severi points out this is also reflected in their distinct morphology, which has more in common with the prehistoric hybrid figures discussed in my Chapter 3 (‘The hidden shaman’; e.g. Fig 3.7). These are not composites. Their ambiguous forms result instead from a ‘thoughtful play on continuities and resemblances in the appearance of various species’, just like the Siberian toggle pin discussed early on in Severi’s book (his Fig. 13), which can be wolf or seal depending on the perspective we bring to it, and also like so much of the indigenous American art he discusses.

The comparison between Severi’s book and mine could be extended in other ways that I hope to get around to. But on these particular points I would see them as mutually reinforcing, rather than contrary arguments. Or have I misunderstood what his chimera principle is really about?

A COMMENT ON THE ORIGINS OF MONSTERS

By Maurice Bloch



The beauty of the epidemiological approach came from its theoretical strength. Its weakness came from the difficulty of demonstrating occurrence in actual, non-historically trivial cases. David Wen-grow's book is a serious attempt to do this, it is probably the only such serious attempt. Whether he is successful in this enterprise seems to me less important than the courage of such an attempt in an anthropology which is becoming ever more pusillanimous. We can only move forward by taking risks. In the case of this book the totally admirable risk taking also involves mastering and discussing completely different types of disciplines. This is, after all, what Darwin did when he combined ideas coming from people such as pigeon fanciers with those of his more scientific work.

The theoretical strength of Dan Sperber's epidemiological approach was that it recognised the necessity of taking into account species-wide human characteristics without running the risk, so evident in previous approaches of this kind, of self-fulfilling reductionism such as we find, for example, in Malinowski's need theory or in Freudian psychology. The approach also avoids the common mistake of ignoring the obvious fact that representations are more usually borrowed than the fruit of individual creation. The whole point of the epidemiology of representations is that it predicted recurrences within populations without invoking over-strong explanations of specific cases. These could only be ex-

plained by the coincidence of general human predispositions and specific historical circumstances. David Wengrow attempts to do this in *The Origins of Monsters* with a fascinating wealth of examples. Wengrow therefore explains a coincidence. That is the coincidence of cognitively equipped humans with a specific development occurring very late in the history of mankind and only in certain places. These places are principally Mesopotamia, Egypt, and China. Only the first two geographical locations are discussed in the book in any detail.

Since we are talking of the coincidence of two types of very different phenomena it is important to be clear what these two sides, so to speak, are. I would like Wengrow to tell me if I am correct in what I take these to be.

On one side the author accepts the existence of certain general human characteristics from the work of a number of writers such as Scott Atran, Dan Sperber, Pascal Boyer and, more recently, developmental psychologists such as Susan Carey and Liz Spelke. The problem which much of this recent literature considers and which is most difficult for the anthropologist wanting to take this work into account is how far very young children's predispositions are modified, changed or lost through development—especially development in specific historical contexts. (This second aspect is raised by the points made in the citations by Tomasello which form the basis of the conclusion.) (Clearly, if these predispositions were to get obliterated through ontogenic development in specific historical contexts they would have no explanatory value for the kind of things considered in Wengrow's book.).

These general human characteristics taken on board by Wengrow are three. Firstly, there is the proposal that infants display a privileged interest in human as well as non-human animals. This privileged interest is especially focussed on self-generated movements, eyes, and certain facial features. There is by now a mass of evidence for this which, even if I was competent, I will not review.

Secondly, Wengrow takes on, mainly from Atran, the idea that there is a general human tendency to classify the natural world in essentialist ways, thereby creating a non-Darwinian understanding of categorical differences between species.

Thirdly, Wengrow takes on from Sperber, Boyer and others the suggestion that certain systematic limited contradictions of how the world is intuitively conceived become interesting and, as a result, super catchy. It is obvious that representations of the composite creatures discussed in the book would be candidates for this kind of catchiness.

(I will say nothing here of subsequent work which has questioned the specific modular view of the mind proposed by the writers on which Wengrow relies.)

Now for the other side of things. According to Wengrow the historically specific occurrences which stimulate and encourage the type of composites discussed by Wengrow are, above all, the growth of urbanisation and/or states (surprisingly, given Wengrow's subsequent work, these two factors are not much differentiated here).

Associated with the growth of towns and state three further factors are emphasised by Wengrow: standardisation, mechanical reproduction (mainly through seals) and cultural exchange.

The coincidence of these two sides is what (Wengrow argues) causes the salience of composites. This is a bold and fascinating thesis. The composites from dynastic Egypt and Mesopotamian states cannot but unsettle and interrogate any visitor to the great Museums of the world. The reasons for their existence here proposed are much more powerful than the alternatives reviewed by Wengrow.

Obviously there may well be some particularist objections to the argument such as whether there is something truly distinctive about the composites discussed. Are these truly distinct, as Wengrow argues, from other famous examples of the presence of composites such as those from the west coast of north America or Island Oceania? There is also the question whether exclusively concentrating on material culture, as the archaeologist must, while ignoring, for example, mythology, is theoretically legitimate. (Discussing these niggly points does not seem appropriate in a forum such as this.)

Putting these problems aside, I am left with the central question which the book raises. Is the coincidence that the book argues for a compelling explanation for the occurrence of the types of composites Wengrow discusses?

Some elements seem to me unproblematic. It seems right and very important to stress the connection between the state, standardisation of all sorts, and mechanical reproduction.

More challenging is the proposed connection, or affinity, between composites and urban states. As a first step we can accept with James Scott (cited on page 110) that a characteristic aspect of the development of states is the fact that they impose incompleteness on the much more coherent constitutive communities they absorb.

However, accepting this point is still a long way from seeing a causative connection between this political fact and the presence of relatively standardised types of images or carvings of composites occurring in these states. Further elements along the causation trail connecting states/towns and composites are proposed in chapter 6. There Wengrow talks of three modes: transformative, integrative and protective. I do not, however, see how these connect up with the general theories of the epidemiologists Wengrow discusses. Am I missing something there? Probably, but, if that is so, I would like Wengrow to spell this out in further exchanges in the forum.

Comments

David Wengrow

It is good to begin with Maurice's thoughts on the book, since as I note in my précis the whole thing originates in a conversation with him. In that context I should also mention my UCL colleague David Napier, with whom I began discussing these topics some years ago, and whose work (especially 'Masks, Transformation and Paradox' [1987], 'Foreign Bodies' [1990] and 'The Age of Immunology' [2003]) is highly relevant to the kind of issues we are discussing. It would be great if David could also be lured into the forum.

Back to Maurice, who stresses the exploratory – and potentially risky – terrain we have entered. He notes, for instance, that my whole argument about image transfer is based on a specific modular view of cognition that may yet be refuted (all the more reason, then, for putting its assumptions to the test). I think this generally risky state of play with regard to culture and cognition has been well summarised by David Graeber, in his recent introduction to the English edition of Carlo Severi's 'Le Principe de la chimère', which should enter our discussion at some point.

The dilemma, as Graeber puts it, is that cognitive science has by now demolished the analytical basis for many classic studies of thought, symbolism, and communication (Lévi-Strauss, Vernant, Goody, etc.), without yet providing tools from which to construct new studies of comparable ambition or sophistication. The epidemiology of representations probably represents the most concerted effort at building such a tool-kit. Partly what we are doing in this forum is a "health-check", assessing its current fitness for purpose, in relation to what is hopefully an appropriate dataset.

Maurice characterises the 'two sides' of the dataset in a way that is completely consistent with the aims and arguments of the book, as I tried to develop them. Providing they fulfil certain minimal common-sense requirements, we are innately predisposed to perceive disjunctive anatomical forms (like the coquettish unicorn decorating his forum post) as being nevertheless organic and animate, in ways that are special and arresting. This is an evolved mental capacity, sporadically attested in prehistoric art dating back at least to Upper Palaeolithic times, and no doubt earlier examples will one day be forthcoming. But the epidemic properties of the composite figure as image – and on my limited definition of such – were unleashed only through a combination of factors that came into play much more recently; factors institutional and technological, coinciding broadly with the emergence of large-scale, centrally organised societies around six thousand years ago.

If my book really is, as Maurice suggests, the first serious attempt at a case study in the epidemiology of culture (sensu Sperber), then that's both exciting and gratifying. I'm not sure it is. Others will no doubt have their own ideas of what constitutes a serious attempt, or in fact what constitutes a truly epidemiological approach. More specifically he asks how my three suggested 'modes' of image transfer – described in chapter 6 as 'transformative', 'integrative', and 'protective' – relate to the general principles of such an approach.

I can best try to answer this with reference to a passage from Maurice's own study (with Dan Sperber) on the mother's brother controversy:

'... the task of the epidemiology of representations is not to describe in any detail the actual causal chains that stabilize (or destabilize) a particular cultural representation (although in some cases it is of great historical interest to be able to do so) but to identify factors and processes that help explain the existence and effect of these causal chains. For instance, showing that a particular folk-tale has an optimal structure for human memory and that there are recurring social situations in a given society in which people

are motivated to tell it or to have it told helps explain why the tale is told again and again with little or no distortion of content in that society.’ (Bloch & Sperber 2002, ‘Kinship and Evolved Psychological Dispositions’, in *Current Anthropology* 43[5]: 727).

My ‘modes’ are stepping stones towards the ‘recurring social situations’ of this formulation. Neither macro- nor micro-scale phenomena, they attempt to describe something in between: processes such as state formation and the expansion of trade networks, unfolding over generations, that seem to recurrently frame and stimulate the kind of visual experimentation I’m concerned with. My colleague Stephen Shennan suggested a possible parallel here with the ‘cycles of contingency’ discussed by developmental systems theorists like Russell Gray: ‘contingent cycles of interaction among a varied set of developmental resources, no one of which controls the process’ (see Oyama, S. et al. 2001, ‘Cycles of Contingency: Developmental Systems and Evolution’. Cambridge, MA, and Boston: MIT Press).

I would like to think a bit more about how these middle-range phenomena relate both to the larger and smaller scales of analysis required by an epidemiological approach. My last comment for now concerns one of Maurice’s “niggly” points, because I don’t think it’s all that niggly. That’s the one about different types of figuration, and whether the composite images discussed in my book are truly distinct from other famous examples, such as those of the American North West Coast or Island Oceania?

Other contributors to the forum are better placed to answer this, but the general point is quite central to my argument. Hard as they sometimes are to pin down, I don’t think these kinds of contrasts in the way images are put together are necessarily vague or trivial. As I see it, the Descola/’Fabrique des images’ exhibition aimed precisely to show how different modes of figuration – different ways of building images – imply wider differences in concepts of relationality, linking up with other areas of practical activity, associative reasoning, social learning, and speculative thought. Barbara Stafford’s work, also referred to in my précis, does this in a different way.

My thanks to Maurice for comments that give me a lot to think about – I will carry on thinking about them in light of the unfolding discussion.

COMMENT ON DAVID WENGROW'S THE ORIGINS OF MONSTERS

By Jeremy Tanner



The attempt to engage with recent developments in evolutionary psychology and neuropsychology has been one of the more distinctive theoretical trends in recent histories and archaeologies of art, increasingly dissatisfied with purely cultural modes of interpretation, whether derived from the German critical tradition or more recent structuralist and post-structuralist art histories. I think in particular of Orians' and Heerwagen's (1992) account of aspects of landscape gardening and painting in terms of evolved dispositions to respond favourably to certain features characteristic of the savannah landscapes within which modern humans evolved; or of David Freedberg's recent essay (2007), co-written with the neuropsychologist Vittorio Gallese, which explores how the functioning of mirror-neurons may inform our response to depictions of action, from the abstract expressionism of Jackson Pollock to Doubting Thomas' thumb, pressed into the wound in Christ's side, in a painting by Caravaggio. Whilst interesting, such studies have had little resonance within mainstream art history, in part because of their rather ahistorical character. Although they may address how certain features of evolved human psychology may inform the character of and responses to some forms of artistic representation, they do little to explain why such forms manifest themselves in some places and periods rather than others.

David Wengrow's account of the origins of monsters in the iconography of early Bronze Age Western Asia takes the debate to a new level by integrating evolutionary psychology, and the epidemiology of representations, with a strong archaeological emphasis on the material technologies of visual representation, and a sophisticated account of art as an institution, embedded in social and political structures, and articulated with trade networks permitting intercivilisational exchanges. The surprising stability of composite iconography in cross-cultural transfers supports Wengrow's claim that the composite character of this 'monster' iconography combines the right combination of aberration and appeal to taxonomic common sense to have the same kind of broad cross-cultural appeal as certain themes of religious symbolism famously discussed by Pascal Boyer (2002). The relative infrequency of such representations before the Bronze Age and their extraordinary efflorescence and widespread transmission thereafter are then explained in terms of the changing affinity between the socio-cultural affordances of such representations and key features of cultural ontology grounded in the rather differing social and political structures of Bronze Age, Palaeolithic and Neolithic civilisations.

These arguments seem to me largely persuasive, and certainly much more compelling than competing explanations, not least since most of them, as Wengrow points out, focus only on one part of the larger picture he describes, the reception of composites in Bronze Age Crete, for example, or in early Iron Age Greece. A particularly attractive aspect of Wengrow's argument is the role played by comparisons across time and space both to justify his claim about the status of composites as 'minimally counterintuitive images' and to unpack the different kinds of mechanism – cognitive, social, and political – which inform the genesis and the chronological and spatial distribution of the images. It was here, however, that I found myself on the one hand questioning some of the claims made by Wengrow for the mechanisms operating to explain the epidemiology of composites, and on the other simply wanting more, in particular an extension of the analysis beyond the West Asian/East Mediterranean focus of the bulk of his discussion. It is against this background that I would like to raise a series of questions which I hope he may be able to address in order to clarify, and perhaps elaborate, his arguments.

Both the title of the book, and Wengrow's analysis of composites, lay particular emphasis on the mechanical replication of composite images, primarily through the medium of sealstones and their impressions. Wengrow makes a persuasive case for a structural affinity between modularity in the construction of composites, and the standardization, through modular principles, of material culture and social life in the increasingly bureaucratically organised societies of early Mesopotamian city-states, transformations in which seals and sealing played an integral role. "Composites thus encapsulated in striking visual forms the bureaucratic imperative to confront the world not as we ordinarily encounter it – made up of unique and sentient totalities – but as an imaginary realm made up of divisible subjects, each comprising a multitude of fissionable, commensurable and recombinable parts" (p. 71). It is this affinity (if I understand Wengrow correctly) which explains the universal affordances of composites, not exploited in earlier Neolithic etc. civilisations, being taken up and exploited so intensively in the early Bronze Age. Yet in neither the archaic Greek case – where most representations of composites are singular representations (on vase-paintings or sculptures) rather than media of mechanical replication – nor the Shang Chinese (as discussed by Wengrow himself pp. 85-6) is mechanical reproduc-

tion particularly central. What does this imply about the causal significance of technologies of mechanical replication in the genesis and distribution of composites? Is the relation a necessary and internal one – no mechanical replication, no composites—or simply a contingent and facilitative one, first in the genesis of composite iconographies, and second in their transmission beyond their context of origin? As regards the issue of their genesis, how might comparison with other cases – particularly independent ones like the first New World cities and states, and perhaps also those of South Asia – clarify (or complicate) the issue?

A parallel set of questions might be asked about the importance of the role of bureaucratic states. Wengrow makes a compelling case for core structural parallelisms between early Bronze Age states in terms of key features of social organisation, and the cultural practices associated with them, in particular bureaucratization and standardisation in practices of state administration and in the organisation of commercial life within “the large urban institutions, which acted as the religious and economic hubs of the earliest cities” (69). None of these features is really characteristic of the emergent poleis of early iron-age Greece which—in striking contrast to their Bronze Age counterparts—lacked any elaborate bureaucratic organisation of political life or commercial enterprise. Nevertheless, as Wengrow discusses, early Iron Age Greek artists and their patrons were enthusiastic adopters of composites. What does this imply about the variable causal weight of the different factors identified by Wengrow in different contexts? For example, would it make sense to argue that once generated, the psychological stickiness of composites of counterintuitive images is sufficient to explain their transmission independently of any affinity between their composite character and the character of the receptive society? How far is it possible to disentangle the role of the strategies of local elites, emulating the practices of peer-polities, from the intrinsic potency of composites as minimally counterintuitive images? There are, after all, many other cultural practices, and features of artistic style and iconography, which are transmitted between the Near East and Greece in the same period, for which one cannot invoke the kind of evolved psychological mechanism relevant to composites: iconographies of lion hunting, animal friezes and the like. Ockham’s razor might be taken to imply that we could explain the transmission of the composites also simply in terms of sociological processes of elites emulating their Near Eastern counterparts and appropriating a range of exotic visual images to legitimate the new positions they were carving out for themselves in emergent Greek states, without needing to invoke the evolved psychological mechanisms which may be associated with composites. These two lines of criticism are of course mutually contradictory, but they do at least open up some issues of the logic of causal explanation which it would be good to see clarified.

A final set of questions addresses the passing comment that Wengrow makes about the transmission of composite iconography as occurring most intensively in ‘proto-’ or ‘archaic’ periods, before the coalescence of the officially sanctioned styles sponsored by the ruling elites of emergent state-level civilisations, for example of Dynastic Egypt and Classical Greece. This is intriguing, and surely requires further exploration. What does Wengrow see as the relationship between the intercultural character of composite iconography – sponsored by state-building elites according to his model – and the forms

of social and cultural closure characteristic of the civilizational styles developed by the same elites (cf. Baines and Yoffee 1998)? How far does the more bounded character of these civilizational styles suggest that the kind of epidemiological model, linked to evolutionary psychology, which informs Wengrow's account of composites, is applicable only in rather special cases, rather than being a model which may be of general relevance to the analysis of ancient visual art? How far does the Chinese case fit this model, since the dominant artistic style of the Shang elites and the composites of Bronze Age China seem to develop together, indeed in internal relationship to each other, on ritual bronzes?

It perhaps seems churlish, in raising these questions, to ask for even more wide ranging comparison from one of the few studies – at a time when there is so much empty talk of 'World Art Studies' – that is genuinely cross cultural and comparative in its approach. Doubtless a full answer would require another book, maybe even a series of books, since *The Origins of Monsters* offers not just an intriguing set of case studies but an entire research programme which deserves much further elaboration if the fruits of Wengrow's approach are to be fully realised.

References

Baines, J. and Yoffee, N. 1998. "Order, legitimacy and wealth in Ancient Egypt and Mesopotamia", 199-260 in G.M. Feinman and J. Marcus eds. *Archaic States*. Santa Fe: School of American Research Press.

Boyer, P. 2000. "Functional origins of religious concepts: ontological and strategic selection in evolved minds". *Journal of the Royal Anthropological Institute* 6 (2): 195-214.

Freedberg, D and Gallese, V. 2007 "Motion, emotion and empathy in aesthetic experience". *Trends in Cognitive Sciences* 11: 197-203

Orians, G.H. and Heerwagen, J.H. 1992. "Evolved responses to landscapes", 555-579 in J.H. Barkow et. al eds. *The Adapted Mind*. Oxford: Oxford University Press.

Comments

David Wengrow

I'm delighted to have a view from Jeremy Tanner, who is able to put my book in the context of recent developments in the history of art. Many of the people to whom I first presented these ideas were in fact historians, especially of ancient art. What they forced me to do, among other things, is work towards a kind of technical precision in my description and classification of forms and images that was otherwise lacking, so when I talk of "composites" it has some internal consistency across time and space. I think that sort of precision is essential in a study of transmission. If the subject matter is permitted to broaden out towards such loose and

general categories as “chimerical representations” (see the earlier post by Martin Fortier) the whole discussion loses focus, and the comparisons become highly subjective.

Art historians – or at least some of them – can be usefully pedantic “scientists of culture”. Archaeologists are similarly and rightly insistent about tight control over the spatial and chronological parameters of cultural analysis. I don’t want to dip my ladle too deeply into the porridge of disciplinary stereotypes, and it’s wrong to generalise, but I do wonder sometimes whether socio-cultural anthropology has moved too far from the kind of classificatory rigour it once had. Hence my initial interest in epidemiological approaches, and their willingness to revisit intriguing and unresolved problems such as the ‘mother’s brother controversy’ in kinship theory.

In responding to Jeremy’s substantive points and questions, I find it necessary to invoke a three-headed “David-Monster” (myself, Napier, Graeber); but before revealing this unholy trinity, some thoughts on mechanical reproduction. Taking the counter-examples of archaic Greece and Shang China, Jeremy asks me to clarify my views as to the causal significance of mechanical replication (e.g. using moulds or seal impressions) for the genesis and spread of composite figures in the Bronze Age. I should say at the outset I think the whole issue of mechanical reproduction – and its impact on society and visual design – has been horribly underplayed for these early periods, and here it is perhaps the art historians, and their insistence on treating such images as singular art objects, that is mostly to blame. The topic merits an entire book in itself.

Jeremy’s question about causality takes me back to a point in my research (around six years ago) when I was pursuing a particular critique of the epidemiological approach. I subsequently abandoned this critique because it was over-simplistic, and based on an out-of-date and unsupported psychology. Something of this original train of thought, and its rejection, is preserved in chapter 5 of the book. But I agree it is worth clarifying.

In *The Sense of Order*, Ernst Gombrich (1984: 256) proposes that pictures of imaginary composite animals – far from being cognitively infectious – confront unusual obstacles to transmission: “there is nothing to hold on to, nothing fixed, the deformitas is hard to “code” and harder still to remember, for everything is in flux”. This now seems completely wrong to me on almost every level, but seduced by Gombrich’s marvellous prose (see p. 82 of my book for the whole passage) – and at this early stage in my research – I wondered whether we had an explanation here for the apparent relationship to mechanical reproduction.

Could it be that the transmission of counterfactual images on a spatially and temporally extended scale poses cognitive challenges, and needs support from some kind of extra-cognitive (i.e. technological) scaffolding, such as mechanical reproduction? This would provide an economical and attractive explanation for the sparseness of composite figures in prehistoric art, and tallies reasonably well with their later spread in the Bronze Age of western Asia and the Mediterranean. But I abandoned the idea of a strong causal link between mechanical dissemination and the spread of composites, for two reasons: Gombrich, and China.

The issues with Gombrich are already clear – he offered no supporting evidence for his psychological assertions. China, and more specially the case of Chinese ritual bronzes, was brought in to my study partly because it was central to Rostovtzeff’s earlier work on the subject, and partly because debates over the significance of composite figures are perhaps nowhere more central to the reconstruction of Bronze Age society than in China (see, for example, the many works by K.C. Chang and their subsequent critiques and modifications; e.g. L. von Falkenhausen [2006], *Chinese Society in the Age of Confucius*. Los Angeles: Cotsen Institute). The interpretation of early Chinese bronzes and their ornamentation is highly controversial, and consulting with some of the leading art historians, epigraphers, and archaeologists of this material made me intensely nervous about venturing into it.

One thing, however, struck me as both relatively uncontroversial, but also fascinating from a comparative perspective. Unlike their counterparts in western Asia and the Mediterranean, the Bronze Age craft workers of China seem to have systematically avoided mechanical techniques for replicating complex visual designs, despite being aware of their possibilities. Bronze manufacture

was intensely modular, but resolutely non-mechanical (the classic study is L. Ledderose's [2000] *Ten Thousand Things: Module and Mass Production in Chinese Art*. Princeton: Princeton UP).

This raises all kind of fascinating questions about the ritual circulation of images and how that might relate to things like courtly shamanism, which are far beyond my competence to discuss (but see M. Puett's [2002] *To Become a God*. Harvard-Yenching Institute). It also offers proof that mechanical reproduction is in no way necessary for the dissemination of standard templates for the visualisation of imaginary beings (see p. 87 of mine). In western Asia and the Mediterranean, mechanical reproduction formed an important environmental factor in the "epidemiology" of images throughout the Bronze Age, so it has a correspondingly important place in my study, which focuses on these regions. But as the case of China shows, it cannot by itself be accorded any strong causal or independent value for the cognition of images, or at least not the sorts of images I'm concerned with.

What about early Iron Age Greece, with its competitive city-state system and enthusiasm for composites? Jeremy points out that when discussing the genesis of composite figuration in Mesopotamia, I ascribe causal value to the development of complex bureaucratic systems based on modular forms of reasoning. Yet these features are only weakly developed in Archaic Greece (or Etruria), which are nevertheless enthusiastic adopters of composites from larger civilisations and empires of the east. This would indeed weaken my argument, were I proposing a uniform set of environmental expectations for the popularity of composites. But I think this neglects the spatial and temporal dimensions of argument (especially chapter 6), which not only allow for but also explicitly define a variety of institutional contexts for the take-up of composite figuration.

The most relevant ones here are my 'transformative' and 'protective' modes, which try to address spatial and cultural dynamics between large, centralised polities and the smaller societies on their edges. The underlying model of state formation here is familiar to experts on South Asia as Stanley Tambiah's (1973) 'galactic polity', recently reproduced in the pages of Hau (2013). It relates partially to the spread of 'new ideas and concepts', along with various material resources, and how these are mobilized on a local scale to establish and demarcate new types of political horizon – a process involving selective adoption as well as calculated rejection of certain traits between expanding economic-political centres and their margins.

It is precisely here that I see the recurrent links between exotic imagery and those formative periods called 'proto-' or 'archaic', when the self-image and public persona of elites is taking form, and is also therefore at its most vulnerable (and see also my earlier comment on 'modes of transfer' and 'cycles of contingency' in response to Maurice). The relevance of this model for archaic Greece has been discussed in much greater detail by David Napier (the second component of my David-monster), with specific reference to the Perseus-Gorgon story and its associated imagery of self, non-self, and supernatural protection derived from heroic conquest of the foreign ('Greek Art and Greek Anthropology', in his [1990] book of essays called 'Foreign Bodies').

Napier stresses that the foreign encompassed here is not the proximate foreign of expanding commercial interactions, but a more distant and exotic one: 'the most foreign of all imaginable places'. So he argues, more controversially perhaps, that the Greek iconography of the gorgon contains traces not only of Near Eastern but of still more remote (South Asian) influences. It is worth noting, in this context that – in terms of remoteness – the relationship between Mesopotamia and Egypt in the fourth millennium BC, and between Egypt and Crete in the early second, was probably roughly equivalent to that between Greece and India in the Iron Age. This is of course a function of "ancient globalisation", as wonderfully described in Philippe Baujard's (2012) *Les mondes de l'océan Indien* (Paris: Armand Colin).

In such contexts the focus on composites for the host society has much to do with their provenance and established functions combined – as always – with their innate appeal and cognitively rooted capacity to absorb and attract new (e.g. ritual, mythological) associations and inferences. General things often have specific functions, and I see no reason why it should be otherwise with images. Napier's suggestion for archaic Greece is that the extreme foreign becomes a 'medium for making social change permanent'

(or at least relatively stable) in Greek society, leading to the kind of ‘canon formation’ – and symbolic closure to the outside world – that Tanner asks about, and which characterises classical Greece as much as pharaonic Egypt or dynastic Mesopotamia.

As I note in the book, Napier’s model of cultural transmission is ‘immunological’ rather than ‘epidemiological’. It addresses the ‘tactical use of the foreign in manipulating cultural canons’ – the modification of cultural content through ‘assimilation of what one takes to be foreign’ (Napier 1990: 108); roughly parallel to the biological action of antibodies in identifying and neutralising pathogens. This to mind offers an alternative way of approaching the relationship between individual psychology and cultural identity, which picks up on various points made by Rodney Needham and others.

An intriguing slant on these issues is provided by David Graeber – the third head of the David-beast under construction – in his recent (2013) Strathern Lecture, ‘Culture as creative refusal’ (Cambridge Anthropology 31: 1-19). There is a degree of recursiveness here, because his argument is partly based on my characterisation of the relationship between ‘bureaucratic’ and ‘heroic’ societies – the latter as originally defined by Hector Munro Chadwick (1926. ‘The Heroic Age’. Cambridge UP). Heroic societies, like those of archaic Greece, are ‘drawn into the trading orbit of commercial-bureaucratic civilisations, and thus accumulating vast quantities of new material goods, while at the same time rejecting the ultimate values of the societies with which they were in contact’ (Graeber 2013: 5; and see also ch. 6 of my [2010] ‘What Makes Civilisation’, Oxford: OUP).

Under such conditions, the situation described by Jeremy Tanner for archaic Greece is just as might be expected: the uptake of exotic iconography in a heroic society, and simultaneous inversion its functions as first established in the more bureaucratically oriented world of the Neo-Assyrian Empire. This is not, by the way, a simple or binary contrast between ritual and administrative functions, or between heroic and bureaucratic values. The differences are subtler. Neo-Assyrian bureaucrats were often ritual specialists of another kind (more ‘guru’ than ‘conjurer’, in Fredrik Barth’s famous terms), and they too had their heroes. But it is, nevertheless, about the movement of an established repertory of images between different kinds of ritual milieu, and between societies organised on quite different scales and principles of integration.

A final observation, more archaeological, brings the discussion back to issues of transmission, memory, and replication. Jeremy notes, rightly of course, that most surviving representations of composite figures in archaic Greece are ‘singular representations’ on things like vase-paintings or sculptures, rather than images repeated through media of mechanical replication. That fits the contrast between ‘heroic’ and ‘bureaucratic’ contexts of image use in more ways than one. The point could be expanded if we consider the likelihood that another medium – i.e. complex figured textiles – played a central role in the transmission both of images and stories.

The problem of course with ancient textiles is that they hardly survive in the archaeological record. But as Elizabeth Barber points out in her brilliant ‘Prehistoric Textiles’ (1991, Princeton UP) we can nevertheless infer something of their appearance in archaic Greece from the content and organisation of painted designs on pottery, which often depict people wearing and using textiles, and also replicate something of the textiles’ decorative syntax in the division of register lines and in the distribution of figural and geometric motifs around a vessel.

‘Could it be’, asks Barber (1991: 365), ‘that old, figured textiles with mytho-histories were the sources from which the less-destructible arts – vases, terracottas, and later sculptured reliefs – were deriving their forms and concepts in archaic Greece?’. Links between weaving, storytelling, and heroic values, as well as being near-universals, are also clear enough in the Greek case. The Iliad (125-7) describes Helen at her loom as she ‘wove in the many struggles of the horse-taming Trojans and bronze-armored Achaens’. So Barber concludes, in terms I find suggestive for our wider discussion, that:

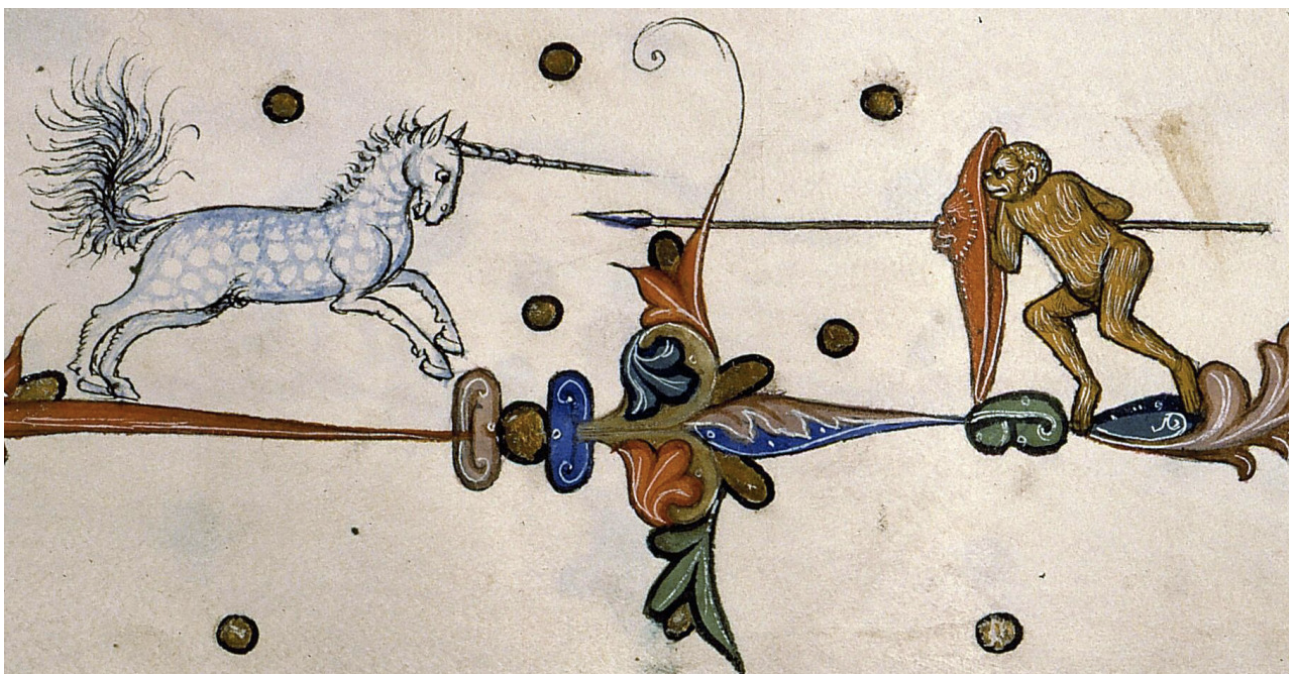
‘... heirloom “tapestries” recording the earlier mytho-history of the Greeks may have survived from Mycenaean times through the Dark Age into the Archaic Greek period when Homer lived. Such a survival might help to explain the astonishing tenacity and detailed-ness of some of the Greek traditions. That is, Homer and the other bards may have had considerable help in remembering

the content of their epics. Could they indeed have glanced up at the patron's palace walls and seen hanging there a graphic reminder of the episodes in their songs, among the scenes on precious heirloom tapestries – of Troy – Helen – and Penelope'. (Barber 1991: 382)

The image evoked by Barber would, I think, also be my counter-response to anyone who suggests we can usefully study cultural transmission by separating out the verbal, haptic, material, and visual aspects of human cognition.

THE SPECULATIVE ORIGINS OF MONSTERS

By Alberto Acerbi



A researcher in the field of cultural evolution – whom I never met in person and who would be probably very surprised of this wildly out-of-context mention – twitted, few weeks ago, that “Implementation is the hard part, not the idea. [...] I have five ideas in the shower every morning. That’s the easy part.” My showers are, alas, far from being that exciting, but, for some reason, the musing resonated with me when I first saw it, and it continued to resonate through the reading of *The Origins of Monsters*.

There is much to like in David Wengrow’s book. *The Origins of Monsters* inspects the diffusion of a very specific cultural item (first plus) making use of Dan Sperber’s epidemiology of representation (second plus), and exploring how both universal cognitive factors and local, socio-economical, ones contribute to the item’s success (third plus).

Let me unpack this sentence. Wengrow examines the reasons of the success of images of monsters, or, better, “composites”, i.e. images of fictional beings, composed using combinations of anatomical parts of real beings: the chimaera in the Greek mythology is a well-known example of such images. According to Sperber and other cognitive anthropologists, these composite figures explicitly violate

our intuitive, domain-specific, expectations (lions do not usually have a snake as a tail) and, in the same time, conform to them (heads are where heads are supposed to be, etc., and composites are readily recognised as “living kinds”). It is this combination, in jargon the fact that they are minimally counter-intuitive (MCI), that makes supernatural beings in general, and composites in this particular case, cognitively appealing.

Wengrow endorses this hypothesis, but also notes that the distribution of such composites may follow a peculiar pattern in space and time, difficult to explain if “MCI” were the only reason of their success. Such images appear to become more common “in the first age of mechanical reproduction”, that is, with the emergence of the earliest urban societies, together with their commercial network and social elites, whereas they are relatively rare in the figurative art of Palaeolithic and Neolithic.

Why is it so? “The Origins of Monsters” advances an interesting hypothesis: composites, Wengrow writes, “imply within their own structures certain principles of integration that were weakly developed in prehistoric societies, becoming prominent only with the emergence of urban life (p. 59)”. It is only in the first cities that the physical and social world became divided “into standard and interchangeable subunits” (p. 7) and the success of composite images, themselves representing beings built with “interchangeable subunits”, is interpreted as a reflection of these new circumstances.

While surely fascinating, how convincing is this hypothesis? Obviously, its plausibility chiefly depends on the main assumption of the relative absence of images of composites in prehistory, and of their later success. I am far (very far) from being an expert, but I would have liked to see a more systematic – quantitative – analysis of the empirical support. Wengrow dismisses well-known prehistoric “monsters” such as the Löwenmensch (lion-man) figurine or the Breuil’s “sorcerer” as isolated and rare exceptions, and takes later examples of composites (say the Mesopotamian lion-faced Humbaba) as representative for the period. I would be curious to know what art experts and historians have to say about that. A different line of reasoning would involve considering contemporary hunter-gatherer cultures. If the “interchangeable subunits” hypothesis is true, composites should not be particularly successful there, as individuals in these cultures do not experience the social and physical subdivision typical of urban societies. Wengrow quickly mentions that, in fact, composites are present in hunter-gatherers performances, but less in their plastic and visual art, substantiating this statement with a couple of references to Ingold and Descola (p. 30). It seems that there is here ample space for additional ethnographic and comparative studies here.

In addition, differently from the MCI explanation, it is difficult to understand exactly what kind of cognitive mechanisms would enhance the salience of representations of composites, just because “interchangeable subunits” become more prominent in the social environment. The MCI explanation is supported by psychological results: what about the “interchangeable subunits” hypothesis? One could imagine to assess it by testing how appealing images of “monsters” are for children, and the prediction should be that they would become appealing only when children are able to appreciate the modular nature of their social environment. I am unaware of studies of this kind, and nothing similar is discussed in the book.

In sum, as a minimum the “interchangeable subunits” hypothesis cries for support from ethnography, as well as comparative and developmental psychology. That is why Wengrow lost me in his conclusion, claiming the superiority of its “archaeological – rather than laboratory-based or ethnographic – approach” (p. 112). I really would like to see in our field more ideas like the ones presented in *The Origins of Monsters*, but the book left me with the feeling that the “hard part” is yet to be done.

Comments

David Wengrow

Alberto Acerbi asks what art experts and ancient historians make of my overall characterisation of the visual record. Over the past five years or so I have presented it to many audiences of this kind, as well as to archaeologists and anthropologists. Of course I can’t summarise the many reactions, but so far none of them obliged me to rethink the general patterns of distribution for composite figures, as presented in the book. Whether these patterns can be generalised to regions and periods not treated there (such as the prehistoric Balkans or modern-day Amazonia) is another matter. Mine is a middle-range study of cultural transmission, dealing with significant chunks of time and space. It does not assume or require a universal model and if other, significantly different trajectories of image making exist it would be fascinating to compare them, and try to account for the differences. For published views by art historians and archaeologists, I direct Alberto (and others) to Jeremy Tanner’s comments in this forum, and also to a review of the book in *Times Literary Supplement* (September 12th, 2014) by Christina Riggs.

Responding to the rest of Alberto’s response is difficult because he distils my argument down to such a simplistic formulation that I can barely recognise it, let alone try to defend it on those terms. He is right of course that my argument stresses the relative infrequency of imaginary composites in Old World prehistoric art, as against their relative popularity in early urban and literate societies of Eurasia and North Africa, and then it tries to explain this phenomenon. But nowhere do I propose a single prime mover, let alone an “inter-changeable subunits hypothesis” of the sort that could be tested under laboratory conditions.

My conclusion, which Alberto does not like (but seems also to misread), emphasises how formative elements of human cognition – using Tomasello’s terms – may result from ‘complex conjunctures of social, technological, and moral processes’, which the book seeks to unravel. No mono-causal hypothesis here. Nor do I assert that archaeology is a superior method of reconstruction – only that it is a necessary supplement to laboratory-based and ethnographic work if we are to avoid false assumptions and blind alleys in the study of human cognition.

Early on in the book (p.4) I give an example of what, in my opinion, is one such blind alley. There I discuss experimental work done in the 1990s by cognitive psychologists. They were trying to establish if spontaneously representing fantastical beings (of the kind that exist only in the mind) might itself involve a distinct neuro-cognitive mechanism. Children on the autistic spectrum formed part of the scientific control group, on the assumption that they find ‘spontaneous and fantastical acts of imagination’ challenging to a greater degree than neuro-typical children (and see, more recently, Low et al. 2009. ‘Generativity and imagination in autism spectrum disorder: evidence from individual differences in children’s impossible entity drawings’, in the *British Journal of Developmental Psychology* 27: 425-44).

The experiment used a standard protocol for drawing “impossible” beings, designed by Annette Karmiloff-Smith (1990. ‘Constraints on representational change: Evidence from children’s drawings’. *Cognition*, 34, 57–83). I find the line of questioning here

fascinating. But as with so much experimental work of this kind, it appears to be largely assumed that physical images offer faithful reflections of evolved mental representations, projected mirror-like onto the material world. Sensory-motor skills, developed in relation to particular tools and materials, were little considered; nor were socially learned and historically patterned expectations about what can and cannot be seen in the world.

My own, very different kind of study draws attention to the centrality of such environmental and historical factors in shaping what we might otherwise (and wrongly, in my view) consider to be purely free and spontaneous acts of human perception and imagination. As Jack Goody frequently pointed out the visual and tactile environments we inhabit – and which frame our experience of social learning – are to a significant degree a product of the Bronze Age. We use writing to communicate, we live and work in brick-built cities, we sit on chairs and sleep on beds, eat our food with metal cutlery, teach our children abstract rules of mathematics, give them Lego to play with, and fill their heads with mechanically (or now digitally) reproduced images of all kinds of fantastic beings. I don't think all this enculturation could possibly be unworked through some magic of the laboratory.

If work by archaeologists, anthropologists, and other culture historians prompts experimental psychologists to pay more attention to such factors, then it is surely doing them a service. Incidentally I don't feel it's necessary, in such cases, to ask whether one method of approaching human cognition is superior to another; but simply to acknowledge that archaeological, ethnographic, and laboratory based approaches offer different and complementary paths of access to any given cultural phenomenon. None of these fields, in my opinion, can validate the others to a greater or lesser degree. They are distinct and mutually informative ways of making valid inferences about human cognition; and they should be given roughly equal weighting.

Why, as Alberto seems to imply, must the path of inference always lead from the laboratory to the cultural-historical record? Surely it can just as well go the other way too? I think this would also be my response to Martin Fortier's earlier comments on developing an epidemiology of material representations. My thanks both to Alberto and Martin for their stimulating thoughts on the book. Perhaps we somehow need to try and link together the various threads of the discussion so far?

Alberto Acerbi

Overall, I think that, despite some differences in tone, we agree on the main point. Nowhere, in my comment, I wrote or implied that “inference always lead from the laboratory to the cultural-historical record” (incidentally, this would be against my own research!), but that it is an excellent opportunity, when an hypothesis developed to fit archeological data can be tested in the lab, or in ethnographic contest, to do it. Just to be sure: I would say exactly the same if the hypothesis would have been formulated starting from psychological experiments, or ethnographic/comparative analysis. David agrees – I subscribe word-by-word the penultimate paragraph of his answer – so there is not much to say here, but I still wonder whether this message transpires as clearly in *The Origins of Monsters*.

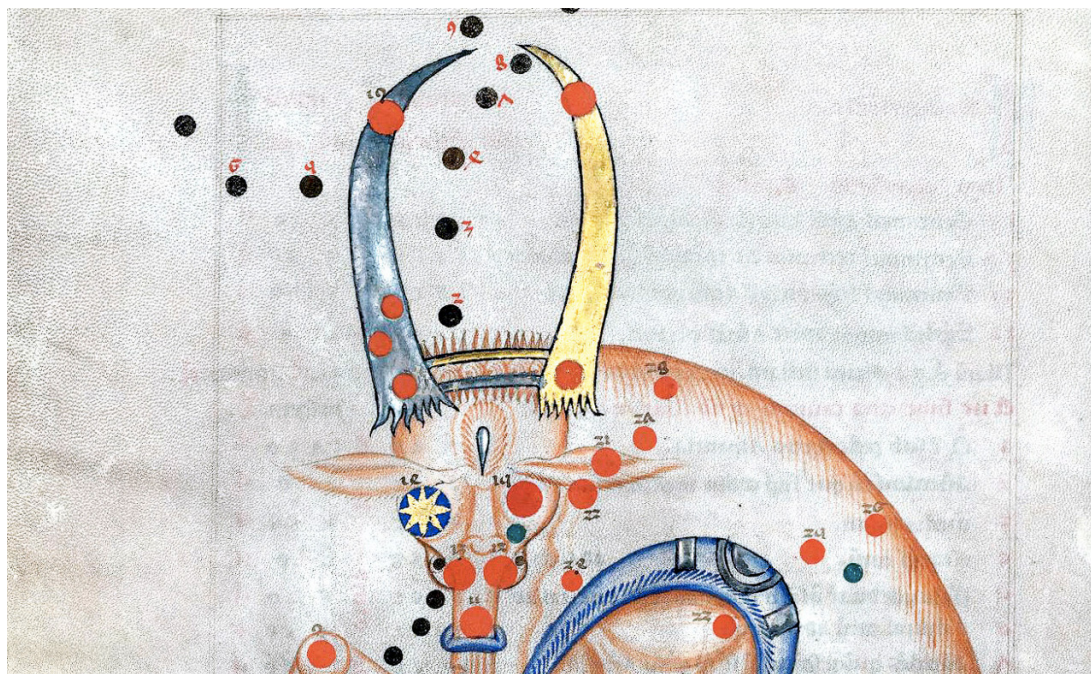
Finally I would like to spend again some words on my interpretation of David's hypothesis. I wrote in my comment that it can be described as an explanation of the success of “very specific cultural items” (images of composites in early urban societies) due to the combination of universal cognitive factors (the success of minimally counterintuitive representations) and local, socio-economical ones (which I called, probably clumsy, the “interchangeable subunits” hypothesis). David thinks that this interpretation is “simplistic” and, of course, if he thinks so, this is it. To me, however, this hypothesis, exactly because in this form, is very interesting, clear, and can be quite straightforwardly used to generate several testable predictions. It is exceedingly rare to find hypothesis of this kind in social sciences, and especially in anthropology: in fact, as I wrote in my comment (“I really would like to see in our field more ideas like the ones presented in *The Origins of Monsters*...”), the main strength of David's excellent book – from my very personal point of view, of course – lays mainly here.

James Waddington

This is a really interersting and productive conversation. The investigation of monsters, reticulated clades, is one of the few areas of present cultural evolution analysis which seems to me to deal in units, lions' heads, dragons' claws, well defined enough and discrete enough to be examinable in terms of variation, transposition, selection and transmission. Clearly it would be presumptuous to comment further without reading the book, which I now hope to do.

CHIMAERAS AS ATTRACTORS: EPIDEMIOLOGY AND CULTURAL VARIATION

By Pascal Boyer



David Wengrow's brilliant *Origins of Monsters* is a rare example of an archaeological study that addresses an important "middle-level" causal question (in this case, Why the proliferation of chimerical images in the Bronze Age?) from the standpoint of a scientifically sophisticated model of cultural evolution. The transmission of a specific iconography is of course a locus classicus in both history of art and archaeology, but it has been generally addressed in purely formal terms, without much consideration of the cognitive processes required to process and recreate visual information, with brilliant exceptions. So Wengrow takes over where distinguished predecessors like Aby Warburg left off, with of course the benefit of a much more precise psychology.

Before discussing Wengrow's rich material and fascinating discussion, it may be of help to do some conceptual cleanup. In particular, some confusion about the underlying assumptions of evolutionary models, and specifically of an epidemiological framework, may result either from Wengrow's own formulations, or more likely from the way we discuss his hypotheses in the course of these exchanges.

For example, Wengrow at various places mentions the “limits” of epidemiological approaches. He also suggests that change or variation are not expected in such models. But it would be a misunderstanding to consider that evolutionary psychology can only explain cultural universals. This fits with a common understanding of ‘genes’ providing immutable features of organisms and ‘environments’ their variation. But that is of course misleading (Sperber, 2005). Indeed, some of the best examples of evolutionary models explain how evolved systems are designed to modulate responses as a function of external information. For instance, some young women mature and reproduce early, in their teens, while others delay reproduction. One of the main factors involved is the presence of fathers in their households during early childhood, which triggers an unconscious estimate of the extent of paternal investment in their social environment (Ellis et al., 2003; Ellis, Figueredo, Brumbach, & Schlomer, 2009). In other words, the evolved reproductive system is designed to motivate different behaviors, contingent on specific environmental cues. Or, people in the same town adjust their level of cooperation and trustworthiness, depending on a largely unconscious perception of uncertainty in their environment (Nettle, 2010; Nettle, Colléony, & Cockerill, 2011). So a single life-history strategy process, as a result of natural selection, results in either a ‘fast and furious’ or a prudent and moderate approach to life’s choices (Sheskin, Chevallier, Lambert, & Baumard, 2014). There are many more examples. In fact, a central lesson of evolutionary biology is that most instincts are conditional, not of the “do x if y” form, but rather “do x if y, given conditions c1, c2, ... cn”.

Related to this is the fact that neither an epidemiology of culture, nor the broader evolutionary psychology framework it is a part of, can make use of such a vacuous distinction as ‘nature’ vs. ‘nurture’. (Wengrow’s use of these terms, page 82, is the only blemish on a magnificent book). The terms are simply meaningless. If young girls in a poor social class react to their jailed father’s absence with earlier menarche and earlier interest in sex, is this ‘nature’ or ‘nurture’? The terms have no place in a causal explanation of human behavior.

Enough jeremiads and quibbles. The central question of the book, and the hypotheses presented by Wengrow, are of much greater interest.

So, why this proliferation of composites? As Wengrow points out, the question is more complex than this terse verbal formulation may suggest. The first specific question is, Why this (roughly accurate) cultural transmission of visual representations constructed on the same principle, of combining parts of distinct animals in a single body? This Wengrow addresses in terms of intuitive biology, of the expectations we spontaneously develop as regards invariances in living species. Because of the intuitive connection between species identification and apparent Bauplan, composites constitute a salient violation of our domain-specific expectations for animals, which makes them more attention-grabbing than standard representations.

Wengrow’s explanation shows how a cognitive evolutionary framework, not only answers old questions (e.g., Why combine parts of several animals?), but also highlights features that in other frameworks, as in the classical study of iconography, are not explained because they are simply not considered. In this case, why do people use accurate representations of each body part that is used in a

chimaera? Also, why are these fantastical creations ‘anatomically correct’? That is, when adding fins to a lion’s body, why do the creators of chimeras place them in the ‘right’ place on the back? In the standard description of chimeras as fantastical, we could predict imaginary body parts as well as real ones, and inappropriate positioning of real ones. The odder, the better. By contrast, the cognitive interpretation suggests that the effect of incongruous, counter-intuitive chimeric combinations is stronger if all parts can be quickly identified and associated with their species of origin, as Wengrow points out.

This, by the way, is an example of what epidemiologists would call a cultural attractor, a combination of representations whose probability of occurrence at a generation g increases if either that particular combination, or other specific ones, are frequent at $g-1$ (Claidière, Scott-Phillips, & Sperber, 2014; Sperber & Hirschfeld, 2004). To simplify, the implicit notion that the lion’s fin must be in the middle of its back, would be reinforced by ‘incorrect’ exemplars that place it on the lion’s paws. This kind of hypothesis can be tested, either by studying the actual occurrences, if one is lucky enough to have such a corpus, or by experimenting with cultural transmission in the lab.

David Wengrow also addresses the question, Why this proliferation of composites there and then rather than before or elsewhere? but tells us, rather depressingly, that cultural epidemiology has “no way of explaining why these images become stable and widespread only with the onset of urban life and state formation” (page 88). I found the statement baffling, as the various hypotheses Wengrow puts forward in the following pages strike me as perfectly fine epidemiological conjectures. (Unless of course one assumes that epidemiology and more generally evolutionary psychology are only about cultural universals... but see above).

So, for instance, Wengrow points out that there may be a causal connection in the coincidence between the appearance of composites and the spread of mechanical reproduction (page 82). If I follow his reasoning, the link may be that the onset of urban life and the development of intensive trade between distinct polities resulted in cosmopolitan exoticizing, so to speak, of which Wengrow describes three distinct modes, transformative (exotic goods upset traditional conventions), integrative (different conventions are blended in an international style) and protective (imagery is construed as a barrier to foreign conventions), respectively (pp. 91ff).

My main request to David Wengrow would be to clarify the differences between these modes, to speculate on what specific psychological motivations and processes underpin each of them, and explain to what extent they are mutually exclusive. This is important, as these distinctions about models of transmission constitute the rudiments of a genuinely epidemiological model of this extraordinary cultural development. Obviously, the constraints of taphonomy mean that specific answers are bound to remain speculative. However, such speculation, if made psychologically precise, could be supported or challenged by relevant evidence from other cultural trends or from laboratory experiments. For instance, under what conditions would a ‘protective’ mode, where imagery is used as a threat against

foreign ways and people, favor the creation of fantastic composites rather than simply terrifying images?

It is of course unfair to ask an author to provide a second book that would answer all the questions raised by the one being discussed. But that is what happens when you engage in great scholarship. Anthropologists and other scientists should be grateful to Wengrow for such a precious contribution to the epidemiology of culture.

Bibliography

Claidière, N., Scott-Phillips, T. C., & Sperber, D. (2014). How Darwinian is cultural evolution? *Philosophical Transactions of the Royal Society of London B: Biological Sciences*, 369(1642). doi: 10.1098/rstb.2013.0368

Ellis, B. J., Bates, J. E., Dodge, K. A., Fergusson, D. M., Horwood, L. J., Pettit, G. S., & Woodward, L. (2003). Does father absence place daughters at special risk for early sexual activity and teenage pregnancy? *Child Development*, 74(3), 801–821.

Ellis, B. J., Figueredo, A. J., Brumbach, B. H., & Schlomer, G. L. (2009). Fundamental dimensions of environmental risk: The impact of harsh versus unpredictable environments on the evolution and development of life history strategies. *Human Nature*, 20(2), 204–268. doi: 10.1007/s12110-009-9063-7

Nettle, D. (2010). Dying young and living fast: Variation in life history across English neighborhoods. *Behavioral Ecology*, 21(2), 387–395.

Nettle, D., Colléony, A., & Cockerill, M. (2011). Variation in cooperative behaviour within a single city. *PLoS One*, 6(10).

Sheskin, M., Chevallier, C., Lambert, S., & Baumard, N. (2014). Life-history theory explains childhood moral development. *Trends in Cognitive Sciences*, 18(12), 613–615. doi: <http://dx.doi.org/10.1016/j.tics.2014.08.004>

Sperber, D. (2005). Modularity and relevance: How can a massively modular mind be flexible and context-sensitive? In S. L. Peter Carruthers (Ed.), *The Innate Mind: Structure and Content*.

Sperber, D., & Hirschfeld, L. A. (2004). The cognitive foundations of cultural stability and diversity. *Trends in Cognitive Sciences*, 8(1), 40–46.

Comments

Olivier Morin

This is a comment not just on Pascal's post, but on two other participations to the book club, one by Carmen Granito & Thom Scott-Phillips, and one by Karolina Prochownik (both to be published later this month). All three, as I read them, make more or less the same point: Wengrow is wrong to think that his work underscores the limits of cultural epidemiology; it is in fact a fine addition to it. The things that drive cultural change (aka the factors of attraction) can be local, contingent, and dependent on political contingencies—we never denied it.

That's always a good point to make, but here's what I worry about. Studying cultural diffusion, and studying it in a way that is highly sensitive to local social and political contexts, is something archaeologists have been doing for a long time now. They should all be decorated for their contribution to cultural epidemiology, but... well, I fear they won't feel as honoured as we'd like them to be. More likely to say, "Thank you for the medal, very grateful"—and never wear the trinket again. The real question (that David Wengrow might be too polite to ask) is: What does cultural epidemiology contribute to this kind of research, that other approaches don't?

Local factors will always be best studied by local specialists; what cultural epidemiologists bring that others don't is a set of hypotheses very general in scope, inspired by findings from psychology, including various brands of developmental and evolutionary psychology. These hypotheses are universalist in the important sense that they claim to describe biological and cognitive mecha-

nisms that are at work in the vast majority of humans. As Pascal notes, this does not, of course, commit us to the view that people behave the same everywhere: identical mechanisms can produce variable outcomes in different contexts; but such reasoning is still beyond the pale in many anthropological discussions where claims of universal validity are regarded with suspicion, to put it mildly.

General psychological hypotheses, not social or political particulars, are our bread and butter. Wengrow and his colleagues are absolutely right to regard such hypotheses as the key contribution of our approach, and quite entitled to fault cultural epidemiology if they fail.

As I understand it, the hypothesis that Wengrow starts his book with (but ends up questioning the relevance of) is that images of composite animals are "MCI" and are more readily invented and diffused than other animal representations (all else being equal and barring local perturbations), by virtue of a very general proclivity of the human mind that makes us favour all things MCI. If this is indeed what epidemiologists should predict (would you agree, Pascal?), and if Wengrow is right that images of composite animals hardly ever emerge without a variety of factors that includes an organised state, the mechanised reproduction of images, and extensive trade networks, then the hypothesis is indeed in trouble, and so is cultural epidemiology's original and specific contribution to this particular problem.

David Wengrow

An objection I faced when initially presenting this research – mainly to archaeologists, ancient historians, and art historians – concerned the images themselves, and their status in relation to human cognition. It was suggested on more than one occasion that images 'combining parts of distinct animals in a single body', as Pascal Boyer puts it in his response, might not really be 'minimally counter-intuitive' at all, at least not in the sense intended by Boyer. It is gratifying now to have it "from the horse's mouth" that we are indeed talking about roughly the same thing.

It is true that Pascal has not written specifically on images. But, rather than be put off by such objections, I tried instead to adapt the principle of minimal counter-intuitiveness to the domain of visual cognition, as supported by psychological data. Now a second objection has been raised by Martin Fortier and others, in earlier posts: the experimental work on animal recognition, on which I rely for my hypothesis, does not relate specifically to the cognition of images. Surely, they ask, I cannot be simply assuming that principles governing innate biological classification (as discussed by Scott Atran for instance) are the same as those governing the perception and transmission of images? The experimental work on images, they suggest, has not yet been done.

But I would argue this is not in fact so. In one case (Davidoff/Roberson [2002] ‘Development of animal recognition’, in *Journal of Experimental Child Psychology* 81) subjects were presented with pictures of animals, or bits of animals – not real ones. And in the other (New/Cosmides/Tooby [2007] ‘Category-specific attention for animals reflects ancestral priorities, not expertise’. *PNAS* 104) the stimuli used were ‘colour photographs of natural complex scenes’; again, not actually living animals.

Of course you wouldn’t know this from the titles and empirical claims of these articles, and that is exactly what I mean about the problem of ‘genre-blindness’ (or ‘culture-blindness’) in at least some experimental psychology. Perhaps it makes no difference whether the animals are depicted or real, but whatever the case these particular experiments are in fact more securely tied to the perceptual world of human-made images than to the perceptual world of living kinds.

The same is true of some other quite recent experimental work on the role of contours and edge alignments in animal detection. With its theoretical roots in the Gestalt psychology of Kurt Koffka, research of this kind now uses software like the Berkeley Segmentation Dataset, rather than hand drawings or mechanically reproduced images on cards, as was once the case. BSD carves up images into meaningful regions and groupings – a wonderful way of generating animal forms on a digital screen for experimental subjects to look at, but very far from placing them in a moving landscape full of living, breathing, snorting, howling wildlife (see for instance Edler/Velisavljević [2009] ‘Cue dynamics underlying rapid detection of animals in natural scenes’. *Journal of Vision* 9; and Martin/Fowlkes/Malik [2004] ‘Learning to detect natural image boundaries using local brightness, colour and texture cues’. *IEEE Transactions on Pattern Analysis and Machine Intelligence* 26].

All this experimental work, in my view, constitutes an unheralded contribution to the study of images, and I feel comfortable using it as such.

Like Maurice Bloch, Pascal asks for clarification on my three modes of image transfer: ‘transformative’, ‘integrative’, ‘protective’, for which he provides an elegant summary. I’ve tried to clarify this in my reply to Maurice, but will go a bit further, because Boyer asks whether I can specify the different psychological underpinnings of the various modes. I suspect what he has in mind here is something like Harvey Whitehouse’s distinction between ‘doctrinal’ and ‘imagistic’ modes of religious experience, which activate different parts of the human memory system (semantic versus episodic). Could my distinctions have a similarly clear neurological basis?

The short I answer I think is ‘no’. They are not to be taken the same way as Whitehouse’s ‘modes’, and I am beginning to regret using the term ‘mode’ at all, because I can see how it might give a slightly false impression. In the book I quite deliberately kept the boundaries between the three modes fuzzy and stress how they may shade into one another, or indeed be aspects of the same historical process (as discussed for ‘protective’ and ‘transformative’ modes in my reply to Jeremy Tanner).

My ‘modes’ frame contexts of transmission at the scale of institutional change (e.g. processes of state formation, colonial encounters), rather than the scale of individual psychology. They do not meet the epidemiological requirement that macro-level distributions of cultural facts are to be explained in terms of micro-level cognitive processes. But they were never intended to fulfil this function. The micro-level comes in elsewhere: in the cognitive and perceptual machinery that – under certain identifiable conditions – makes composite figures into hyper-effective vehicles of cultural transmission and political transformation.

Olivier Morin, Martin Fortier, and Alberto Acerbi have since raised other questions, specific and general, that I have yet to answer satisfactorily. What about ethnographic cases of hunter-gatherers and other non-state societies possessing a rich and stable imagery of composites? How exactly and how far does my study engage with the epidemiological agenda? Do I come as an archaeologist (shovel in hand) to bury or to praise it? Neither, in fact – but this post is probably already overlong, so I will end here for the moment, and offer my very sincere thanks to Pascal Boyer for his close engagement with and critical appreciation of the book.

Hugo Mercier

Olivier, you say that “if Wengrow is right that images of composite animals hardly ever emerge without a variety of factors that includes an organised state, the mechanised reproduction of images, and extensive trade networks,” then not only is the hypothesis “that images of composite animals are “MCI” and are more readily invented and diffused than other animal representations (all else being equal and barring local perturbations), by virtue of a very general proclivity of the human mind that makes us favour all things MCI” false (trivially), but also “cultural epidemiology’s original and specific contribution to this particular problem.”

Having just reread your book, I think I see where you’re coming from, but I still think this is too categorical. Why couldn’t cultural epidemiology make an original contribution by helping explain some features of the phenomenon in question rather than the whole thing? In the case at hand, it might help explain why the images that spread thanks to the factors highlighted by Wengrow take the form they take. Similarly, you see your work on writing as part of an epidemiological approach (right?), even though writing requires criteria similar to Wengrow’s to emerge. In your terms, the factors that motivate the production of some representations would be chiefly determined by local social factors, but attraction would still play a role.

Olivier Morin

Thank you for the clarification.

One of my answers applies only to this case: I don’t think the view that monsters are MCI is true at all. I explain why in my post next week... stay tuned!

More generally, this debate might be, in part, a matter of epistemological tastes. Some people like their theories to have many degrees of freedom: to explain a wide variety of phenomena with a model of many variables, each of which might be overridden by the others. Such a multi-causal approach is clearly the one favoured by David Wengrow. These models are excellent from a descriptive point of view, but their predictive power is quite low. A model with many parameters (of undefined weight) can accommodate a wide range of results. It does not say anything false, but it doesn’t take many risks.

Others, of a more reductionist bent perhaps, like models that give them a proportionate bang for their buck, so to speak: a good causal hypothesis makes surprising predictions, it has informative value in addition to its descriptive value. My tastes carry me towards this second option. The risk of over-fitting (or retro-fitting), i.e., of helping oneself to ad hoc variables to accommodate any and all exceptions, is just too great. So I would make a plea to keep epidemiological hypotheses as simple as possible (though not more). The risk of embracing multi-causal models where fundamental psychological mechanisms are often (or always) overwhelmed by local and contingent factor, is quite simple. The psychological hypothesis ends up not doing any explanatory work. This, I suspect, is what ends up happening to MCI theory in David Wengrow’s model (and I am not immune from the danger myself).

Incidentally, this difference of taste between people who prefer descriptive models (at the risk of over-fitting) and those who prefer informative models (at the risk of reductionism) might unlock some misunderstandings between David Wengrow and some of the debaters here. David Wengrow claims that monsters should be fostered by the mechanical reproduction of images—except when they aren’t, as in China. Monsters should thrive in big, centralised states—except when they don’t, as in Iron Age Greece.

Composite monsters should be appealing—except that in most of human history, they weren't. Alberto, or Mathieu (like me) see this flexibility as a problem, or at least as a sign that the model is incomplete, while David Wengrow presents it as a strength. Is he wrong? I don't know. I can understand why, when one values accuracy, descriptive sophistication, and erudition, above other things (as archaeologists do and perhaps should), one can prefer such models to simplified and risky ones. Perhaps we simply need to acknowledge a conflict of scientific tastes here.

THE STAMPED, SEALED AND DELIVERED RIDDLE OF THE SPHINX

By Erhard Schüttpelz



In the perspective of a deep media history, one of the most exciting points in David Wengrow's great book about the genealogy of composite monsters is the evidence of a link between mechanical reproduction, long chains of external trade, and what has recently been called "certification" by the scholar of modern standards and food chains, Lawrence Busch (in a book called "Standards", 2011). Then and now, mechanical reproduction, in this case: the culture of stamp seals and cylinder seals, gives rise to imitations and forgeries, so the people in charge of brands and seals have to make sure they own the monopoly of producing and identifying the authentic tokens of their seals. Thus, "certification" becomes a necessity: the authority to delegate the authority of issuing the authentic tokens of brands and seals. The ownership of matrices and patrices of seals becomes the obvious focal point; and the use of force against any infringement along the chains of their distribution. But why should many of those seals and certificates be guarded and vivified by monsters, or "composites", as David Wengrow calls them? Why do these composite monsters travel along the paths of "certified" elites? Somehow, the development of a bureaucratic meta-authority—the authority to decide about the

authenticity of producing and issuing seals and brands—seems to be linked to the meta-monsters made out of the incompatible anatomical limbs of different animals and humanimals. As David Wengrow demonstrates, it is not modularity itself that lets these composite monsters emerge, but the mechanical reproduction of modular beings, and the bureaucratic gate-keeper position of their media. The emergence of an administrative media monopoly and of a purely media-made monster—a monster built from the pictorial representations of anatomical details, but sticking to the surface of their representations—seem to be linked. In more general terms: In the Bronze Age, a newly found ‘Media Immanence’ takes its turn against the world, unleashing bureaucratic force, physical violence and anatomical surrealism.

Looking at the illustrations in David Wengrow’s book and comparing them with the modular designs both from the Pacific North West Coast and from Archaic China, I find it striking how “shock-frozen” most of these composite monsters appear, and that this quality seems to be part of the horror—or of the synthetic wilderness—they incarnate. Whereas on the North West Coast animals and monsters alike remain in a smooth flow of curvilinear designs of encompassing parts and divisible wholes, duck-rabbit-wise popping in and out of each incorporation like an endless fountain of images and creatures, at first glance, the composite monsters from the Near East seem to aim at a similar aesthetics of abundance. But more often than not – even in their non-mechanical versions – they arrest the flow of the gaze in dead ends, crammed corners and unhappy angles. It may be only an association, but the gate-keeper function of these media and their elites seems to have sunk deep into their aesthetic sensibility: the ability to let goods and people pass or to arrest their flow, or to arrest and confiscate people as well as goods. The Shock-Frozen— epitomized in Medusa’s stare—, the Stamped Sealed & Delivered, and the Gate-Keeper function, they seem to go hand in hand.

Franz Baermann Steiner famously writes in his note on the “Process of Civilization” from 1944: “What was once outside society, what was later inside society will, when this society triumphs, one day be within the individual. That is the process. The process of civilization is the conquest of man by the natural forces, the demons. It is the march of danger into the heart of creation.” (“On the Process of Civilization”, in: *Selected Writings*, vol. II, p. 128). When *The Origins of Monsters* was published, I expected this Big Point to be discussed. But then I had to acknowledge the wisdom of avoiding the evolutionist bias in Steiner’s proposition. Looking sideways, f.i. at the ebb and tide of witchcraft accusations in colonial and post-colonial times, there is no law governing an evolution of the “march of danger into the heart of creation”. All civilizations and all societies possess the means to make this march happen and to protect against it. Still, the “epidemiology of composite monsters” is a prominent historical trajectory for the long-lasting march of the demons into the heart of the cities and city-states of Mesopotamia, the Near East and Greece... and into our hearts. And we can imagine the feelings of disgust and horror experienced by hunters and gatherers, but also by pastoralists (f.i. as visitors or captives) encountering the pictures and public media of composite monsters in those city-states, of an iconography turning (as David Wengrow spells out at the beginning of his book) the most intimate horror of sorcery into a public proclamation of sovereign power.

There remains a striking reversal of authority and practice to be unravelled by comparing composite monsters with others. F.B. Steiner writes: “The Eskimo considers those magicians or ‘sorcerers’ as

dangerous persons who know how to cast a spell on the spirits from the danger zone which are willing to do their bidding and can be controlled within the sphere of social relations.” (p. 126) People and objects dealing with physical and spiritual danger remain ambiguous beings, and become a locus of danger themselves. Inverting this relationship, the composite monsters may transform the locus of obvious danger into ambiguity, and ambiguity into a knowledge of “how to cast a spell on the spirits from the danger zone”. Within the public and private practices dealing with composite monsters, protection could be provided by public authority (especially by divination), and by rituals locating the demons on the thresholds separating the places of intimacy from the public sphere. Thus, the composite demons found their protective function in symbolizing and stopping the “march of danger into the heart of creation”, by sealing the domestic location of fertility, health and well-being. The “conquest of man by the natural forces, the demons” happens all the time, and it might turn into a collective enterprise in times of publicly acknowledged crises. The only way to stop this march seems to lie in the ability to particularize or even ‘privatize’ its danger, turning horror into protection (like Taweret; like the shield of the Gorgo, and its relationship with the Evil Eye ever since).

“What was once outside society, what was later inside society will, when this society triumphs, one day be within the individual.” How old is this particular individual? Is it only the modern psychological self in which the march of the demons triumphs? Or is the modern triumph of the demonic individual as old as the composite monster, and emerged within its ancient epidemiology? Oedipus meeting the Sphinx, this scene has obviously become one of the best-known encounters connecting the modern psychological selves with the composite monsters of ancient civilizations. We know Claude Lévi-Strauss’s astonishing analysis of chthonic traits and foot handicaps in the family tree of Oedipus (in his essay on “The Structure of Myth”); and artists like Francis Bacon have been eager to show their version of swollen feet. But how would we read – or depict – the oedipal encounter with the Sphinx after reading David Wengrow’s treatment? In fact, Oedipus and the Sphinx are an interesting test for spelling out some of the historical implications hidden in *The Origins of Monsters*. The kingship of rulers finds its power base in social relationships beyond kinship, in the mobilities of bureaucratic and military offices, and that is why the king time and again is symbolized by creatures of the wilderness, and especially by predators. The bronze age elites controlling the long chains of trade networks and other monopolies of power, obviously extended their power bases beyond kinship too, and they found a striking symbolism in synthetic creatures going against the grain of animal kinship – and against the very idea of kinship -, arbitrarily combining the beasts (and predator traits) of air-, land- and sea-borne creatures. Because these new beasts “beyond kith and kin” demonstrated the power of an elite, its potential of violence and the impersonal chains of bureaucracy (or ‘certification’), these hybrid beasts indicated a new scale and scope of protective beings. One of these creatures was a goddess of protection indeed, protecting the nucleus of the family, or of child-birth: Taweret. But all of the composite monsters may have developed this potential of defining the intimate sphere of elites, or the core of their kinship, of “what-to-protect” against the volatility of the outside world.

When Oedipus, the legitimate king of his city-state, meets the Sphinx at the threshold to his city, he encounters his future disaster, by answering her question and destroying a gate-keeper that threa-

tens to kill all passengers. But this composite monster turns out to be Oedipus' last protection against his own monstrous fate, arresting his tragic movement for the last time. And the riddle he solves makes him a composite monster too, four-legged, two-legged and three-legged, the baby he was, the grown-up he is and the old man he will be. In the oedipal answer that kills the Sphinx, I am inclined to hear an echo of the artists of composite monsters at work, taking the extremities of different beasts and defining the impossibilities of kinship from without and from within. Only now, it's the one beast no one had thought of, and the murderous potential that was once outside society will from now on, for the next millennia, become implanted into the individual heirs and family affairs of royal dynasties and their kith and kin.

Comments

David Wengrow

Many thanks to Erhard for his rich and eclectic response, which brings to mind studies by mid-twentieth century scholars that are central to the themes of the book, but unfortunately featured little in its pages. I think they all relate to the 'epidemiology of culture', and specifically to the question of whether morality has any place in it – this seems an important question in terms of defining the scope of the epidemiological approach in relation to more established anthropological approaches. In responding to Erhard I will try to draw out these connections, and some related questions. I agree we should look back to go forward.

First, images: Erhard is struck by the "shock-frozen" character of Bronze/Iron Age composites in the Mediterranean and Near East: how they "arrest the gaze" in a way consistent with their often explicit functions as gatekeepers for people, buildings, and goods. He sees a contrast with the imaginary compositions of North West Coast and early Chinese art, the affinities of which were famously discussed by Lévi-Strauss. I am also reminded of Bob Bagley's points about the visual "slipperiness" of figural designs on Shang bronzes, notably the ubiquitous taotie or 'monster mask' element, which shrinks back from the eye into larger patterns of interlaced lines and projections, and how different these designs are to the canonical dragons of later Han art, with their clearly projected and bounded limbs (see p.85 of mine, and R.W. Bagley [2006] 'Ornament, representation, and imaginary animals in Bronze Age China. *Ars Asiaticus* 61).

The Han period dragons – well known to us from placemats in Chinese restaurants – are true composites, in the sense that I use that term. Their genesis lies arguably in the "transformative" (again in my specific sense) encounter between lowland China and nomadic peoples of the Central Asian steppe, carrying with them crafts and aesthetic formulae from as far afield as Persia. This period of trans-cultural encounters, from around the eighth century BC onwards, is what mainly interested Rostovtzeff in his early study of the 'animal style', and seems also to line up quite well with the introduction of mechanical replication techniques for the manufacture of ornamented bronzes in China (again, I stress there may be no strict causal relationship here in terms of visual cognition, but rather a bundle of interrelated innovations; and see my response to Tanner).

Erhard's observations also bring to mind one of the boldest twentieth century attempts to address the cognitive foundations of ancient art: Henriette Groenewegen-Frankfort's brilliant but neglected (1951) *Arrest and Movement: An Essay on Space and Time in the Representational Art of the Ancient Near East*. As an interesting side-note Groenewegen-Frankfort's final project, which she had barely started before her passing in 1982, was to be a study on *Mischwesen* ("monsters") in ancient Near Eastern and early

Aegean art. But then on the other hand Erhard's allusions to witchcraft epidemics and 'synthetic images' take me in another direction: is this a veiled reference to Rodney Needham's (1978) essay of that name, which I now suddenly want to re-read and perhaps come back to at a later point in the discussion? Too much to think about!

Erhard's more direct references are to social anthropologist F.B. Steiner, and in particular his posthumously published essay on civilisation. There is another aspect of Steiner's writing that I think poses quite direct questions for the epidemiological approach to culture, and I find it most clearly expressed in the collection 'Taboo, Truth, and Religion' (Adler/Fardon eds. 1999). I'm thinking about his treatment of 'positive' and 'negative' transmission in the essay 'Taboo and contagion'. There he discusses avoidance behaviour, contagion, evil eye, amulets, and other non-neutral forms of cultural transmission, where the act of passing something on or repelling it has clear moral connotations (being either higher or lower, positive or negative).

The idea of transmission (or repulsion) as a moral act has very deep roots in social anthropology, not least through Frazer's work on sympathetic magic, and then Mauss's on gift exchange and (with Hubert) on sacrifice. Mary Douglas, following Steiner, famously argued that the transmission of disease through contagion or otherwise is itself a fundamental source of 'natural symbols', always carrying moral as well as biological connotations. And the idea of a "moral epidemiology of culture" is something David Napier has been explicitly arguing for lately.

My book eschews moral definitions of 'monsters' (p.24 ff), such as that offered by Arnheim in his psychology of art, because they seem too cumbersome (and too strictly normative) for any kind of general-comparative analysis. I suppose that's OK as far as it goes. Clearly a great deal of human cultural transmission takes place without any strong moral content. Alex Bentley's work on 'random copying', 'neutral transmission', and 'cultural drift' (whether in baby-names or prehistoric pottery styles) is about exactly that (http://www.alex-bentley.com/culture_evolution). But clearly sometimes, in some specifically human types of situation – such as ritual, sacrifice to the gods, or witchcraft accusations – the act of cultural transmission does have that sort of special moral density to it.

My three modes of transmission are all in some way connected to these more difficult contexts for cultural transfer: the kind of transfers that provoke an explicit interrogation of established social roles and structures, rather than their simple affirmation, mimicry, or reproduction. Perhaps that is why I find it tricky, as Bloch and Boyer note, to link them in any formal way with current epidemiological models. Might anyone then like to comment on the significance of morality as a species-specific feature of human interaction, and its place in the epidemiological approach?

I should add, as a final point, that I have thought about the evolutionary principle of 'costly signalling' in relation to these issues, but find it to be a) hopelessly reductionist when applied to human beings as opposed to chimpanzees, male peacocks etc.; and b) quite difficult to apply to the study of history and change, which seems odd for a theory developed on evolutionist principles.

YOUR VERY OWN MONSTER CREATION KIT

By Mathieu Charbonneau



In *The Origins of Monsters*, David Wengrow tackles a very interesting historical phenomenon: a sudden surge in images of fantastic animals accompanies the rise of urban life in Mesopotamia. In contrast, such images are excessively rare in pre-urban, prehistoric art. Wengrow contends that the reasons behind this phenomenon are cognitive, socio-economical, institutional, and to some degree technological.

First, let us be clear as to what sorts of images Wengrow is interested in. Images of fantastic animals – or composites, as Wengrow calls them – are composed by the joining together of body parts of different species of animals. Consider the following image.



Black and white crop of full plate scan, from Austen Henry Layard's 'Monuments of Nineveh, Second Series' plate 19/83, London, J. Murray, 1853 (no copyrights)

The creature is an example of a composite: a bird's body, together with bird wings and a bird tail, but with lion forelimbs, a lion head, and bovine horns. Composites act as a modular system of representations because their different body parts are interchangeable between species. Although many sculptures depict such imaginary creatures (think of the Great Sphinx of Giza), Wengrow is more specifically interested in images engraved or printed on portable objects such as vessels or weapons.

Following the work of cultural epidemiologists such as Dan Sperber, Lawrence A. Hirschfeld, and Pascal Boyer, Wengrow argues that the success of composites relies in part on the violation of our expectations of what real animals look like. However, even though composites shock us by their fantasy, they retain an 'anatomical correctness' that allow us to understand them as animal-like creatures rather than inarticulate blobs of body parts. The expectations we have about what a proper animal looks like, how to classify animals, and how we intuitively reason about them is part of our innate psychology, what is commonly referred to as our folk biology. Wengrow agrees with cultural epidemiologists that it is the shock-value of such representations – how they are slightly off our folk-biological expectations – that makes them so memorable and popular: "On the expectations of this model, images of composite animals, with one foot in anatomical reality and the other in fantasy, should provide good materials for an "epidemiological" study. We might reasonably hypothesize that, as minimally counterintuitive images, they constitute robust points of reference for ideas about the supernatural, capable of crossing cultural boundaries and acting as ready vehicles for a multiplicity of ritual, theological, and mythological discourses." (p. 24)

However, our folk biological cognitive dispositions are not sufficient to explain the success of composites. Wengrow rightly argues that prehistoric people had the same folk biology as urban Mesopotamians. Nevertheless, very few images of the kind were produced in pre-urban societies. It is at this point in Wengrow's argument that socio-economical and institutional factors come in. With the rise of

urban societies, the increase of demographic density, the proliferation and networking of economic goods, and the expansion in maritime commerce, adopting means to indicate the origins of products and their ownership became a necessity. If I understand Wengrow's argument correctly, composites spread because they were recruited to serve as brand names and as the basis of a system of heraldry (secular and religious). For instance, a vessel on which some composite was printed – such as the one depicted below – could be traced back to the producer of the vessel (and its reputation) because only that specific producer was authorized to use, let's say, a human-headed griffin. In contrast, another producer might use a snake-headed griffin, etc.



Cylinder Seal with Human-Headed Griffin Attacking a Horse, between 1400 and 1200 BC (Middle Assyrian)
(Photography by Walters Art Museum)

If this is right, then it is unclear why Wengrow bothers at all with the shock-value of the counter-intuitiveness of composites. In fact, cognition per se seems to play no distinctive explanatory role in the argument: the success of composites in new urban societies seems to be due to their socio-economical usefulness as a modular system of representation for an increasingly institutionalized world. This usefulness, in turns, is based on their flexibility as symbols – their modular nature allows an indefinite number of animal part combinations – and their expressivity – they can be used to arbitrarily represent many different things (p.71–73).

In what follows, I suggest that cognition did in fact have an important role in the rise of composite imagery, but that contrary to Wengrow's thesis, this role is not one of shock-value. In fact, I argue exactly the opposite: composite only rarely shock our folk-biological intuitions. The naturalness of composites is the rule rather than the exception. The very modularity of composites – the fact that they are flexible symbols – and the centrality of “anatomical correctness” for imaginary creatures – the fact that they are flexible, yet constrained in this flexibility – points to the underlying cognitive processes involved in the “assembling” of composites. I suggest that these cognitive processes have an important role in explaining the distribution of different composite images, more specifically in explaining why

specific variants of composite images – specifically natural, well-formed fictive animals – are persistently reconstructed whereas others, less-intuitive forms are less readily reconstructed.

Shock-value or flexibility?

As mentioned above, composites represent fantastical creatures, but creatures that nevertheless maintain a certain “coherence on the anatomical plane”. Composites are “anatomically correct”, even though they do not represent any real species of animal with which their correctness can be compared. Wengrow explains this idea by claiming that “n violating some limited part of intuitive biology, composites thus typically affirm many of its underlying structural principles. Legs are still positioned for walking, eyes for seeing, wings for flying, [...]” (p.28) Wengrow suggests that the “structuring principles” are part of our folk biological intuitions. I concur. However, Wengrow fails to offer any detailed account of these “structuring principles” and how they are used to assess the correctness of a composite’s anatomy.

This lacuna becomes especially salient – and confusing – when we ask what is supposed to be shocking in an imaginary creature if it is nevertheless “anatomically correct”. Wengrow claim that composites are shocking because they have one foot in reality and the other in the supernatural, this being due to their fictive character (see the first quote above). However, nowhere does he explain what in the mixture of fiction and composition is supposed to lead to the supernatural. In fact, most composites dealt with by Wengrow could be part of Earth’s natural history, at least intuitively.

Consider that some existing animals may well be described as composites. That would not contradict their naturalness. The platypus is basically a muskrat’s body, a beaver’s tail, and a duck’s bill. Here, although we could represent the animal by joining together the parts of the different species, it does not make the platypus a supernatural creature, nor even a particularly shocking being for that matter. Contrast now with the hippocampus, a Greek and Phoenician mythological creature composed of a fish’s body and a horse’s head and neck. The composite was likely a fiction then, and according to Wengrow, its composite nature is supposed to shock our intuitions and evoke the supernatural. However, later on it was discovered that hippocampi or sea horses – fishes with horse-like heads and necks – do in fact exist. What these two cases show is that composition and fiction do not make an image shocking and unnatural. At the very least they are not sufficient conditions for producing intuition-shockers.



Section of mosaic floor from the Roman Baths at Bath, representing a sea horse.
(Photography by Andrew Dunn)



The Black-Sea seahorse (Photography Florin Dumitrescu)

Contrary to Wengrow, I believe that early-urban composites are in fact rarely if ever shocking. On the contrary, I believe that they tend to maximally satisfy our folk biological intuitions. Only in specific circumstances do they fail to do so. I suggest that this is because there is a “proper” way to build composites, one deeply based on our cognition, but not one based on shock-value. Rather, I argue that the flexibility of composite is due to their modular structure, and that this modular structure is in fact a product of our folk biology. Thus, when the Mesopotamians start exploiting the flexibility and expressivity of composites for socio-economic reasons, they in fact exploit their cognitive makeup to serve new cultural functions, namely to assemble modular interchangeable animal parts according to a schematic body plan structure. The remainder of this critical comment aims to offer a basic skeleton of what role cognition may in fact play in the advent of composites.

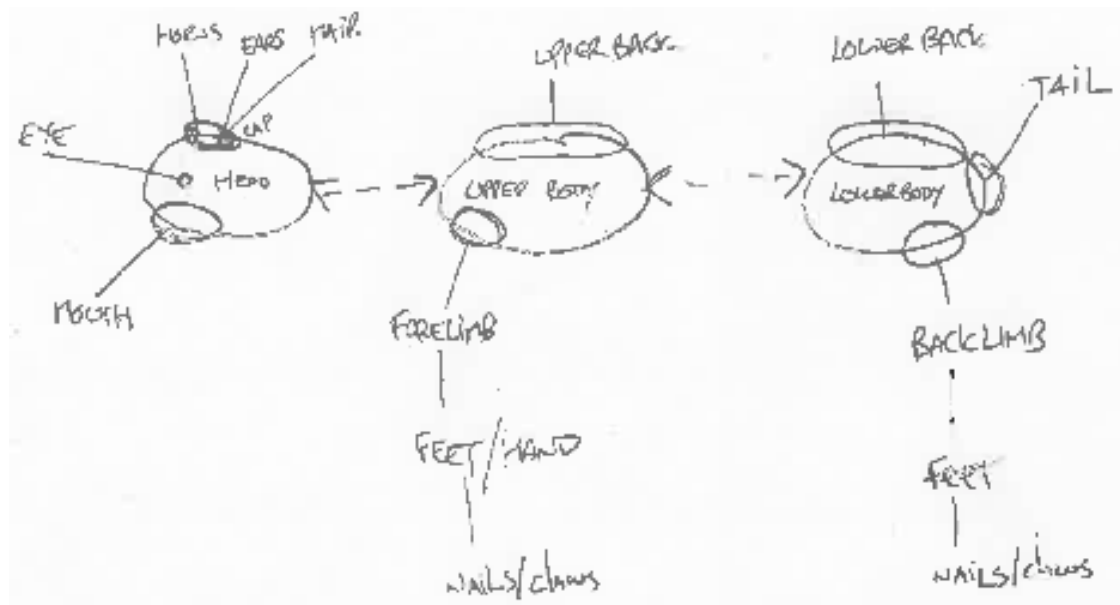
The modular structure of composite body plans

The imaginary creatures discussed by Wengrow exhibit a very strict uniformity in body plan. Their anatomical variation seems highly constrained. In fact, they share important body-plan properties that could have been altered but are not, or rarely so. Composites usually represent imaginary animals with bilateral symmetry and with an anterior-posterior axis. Composites always have sensory and locomotory organs, etc.

In contrast, the specific body parts they are made of greatly vary, but in systematic ways. First, the interchangeable body parts all serve a distinct function. We find full paws or full eyes, but no half-eyes or finger-less paws. Second, the varying body parts usually serve the same function in the composite as they did in the original animal. Heads are used as heads, torsos as torsos, limbs as limbs, tails as tails, etc. We do not encounter composite using their horns to walk, their legs to eat or bite, their tail to see, etc. The organ's specific function is preserved. Third, the general anatomical relationship between functional parts are maintained. We do not see (or rarely do so, see below) composites with mouths located where we would expect eyes, with tails where we would expect a mouth, or torsos where we would expect legs, etc. The "location" of the body part in the general economy of a typical animal body plan is generally preserved in composites. Fourth, not every body parts are used as detachable modules for creating composites. Variation in composites seems to concentrate around the joints of a general body plan structure, with specific functional organs serving as interchangeable modules. Legs and hands/paws are used interchangeably, but not knees nor calves, for instance. So not all salient organs serve as modules for composition.

All together, these four regularities in composite variation suggests to me that the interchangeable parts – the building blocks of composite imagery – seem to be individuated in terms of their folk biological, relevant functions. Head parts (eyes, mouths, horns, etc.) and appendages (limbs, claws, wings, etc.) tend to be important when it comes to identify and interact with an animal, such as a predator or a prey. They also tend to be used as interchangeable modules in the production of composites. Other body parts do not seem to vary in composites. A composite's legs (considered independently from the feet/paw) is usually borrowed as a full leg – of a human or of an animal – including a thigh, a knee and a calf. We do not encounter composites with lion thighs, human knees and fox calves. This is because calves and thighs do not have their own unitary function, at least, not in folk biological terms. They participate to the legs unitary function: walking, jumping, and running.

What these observations suggest is that composites are built according to a basic body plan. This schematic animal body plan appears to be organized in terms of a hierarchical organization of functionally differentiated morphological parts. Some features are subordinate to others, such as eyes always figuring on heads but never the opposite. Others can be understood as being set at the same level, such as eyes and mouths both figuring on heads, or heads and torsos serving as two superordinate body parts (see drawing below). Creating composites seems to obey the following general rule: when using a body part of an animal to assemble a composite, the location of the part in the composite must be the same as in the original animal's body plan. Heads are accordingly exchanged with heads, forelimbs with forelimbs, etc. Superordinate parts such as heads typically bring with them the transfer of their subordinate parts (e.g. eyes). However, subordinate parts can be transferred independently of their superordinate system (e.g., you can exchange feet for paws while keeping human legs, as in the two first composites depicted above).



Wengrow does not tell us where the modular structure of a body plan comes from, or how it is articulated. He does suggest that it is only when the techniques used to represent animals get more precise and subtle that the animal image becomes properly modular: “Precisely because of their fictive character, the creation of visually compelling composites requires enhanced accuracy in the depiction of individual body parts, each of which should be rendered at a common scale and should be clearly identifiable, in and of itself, as belonging to a certain kinds of species” (p. 26; emphasis in original).

Of course, in order to systematically produce composite images, it is necessary that each body part be representable independently from their original animal. However, artisanal techniques and social norms are insufficient to explain the modularity of composites. First, they do not explain how the body parts were individuated in the first place, nor why there is a preference to exchange body parts with similar functions instead of some other relations. In contrast, assuming a folk biological origin for the modular body plan of animal representations answers both problems. Second, even when accurately depicted and following social norms of representations, not all such body parts are used in a modular way. Consider the very well defined and impressive standardization of human calves in Mesopotamian relief carving (see figure below). We do not encounter composites with lion thighs followed by human calves and then lion paws. Although visually salient and following strict norms of representation, human calves simply do not appear to be a composite’s module in Mesopotamian imagery.



Part of 'Relief with Winged Genius'. This relief decorated the interior wall of the northwest palace of King Ashurnasirpal II at Nimrud, which is situated in present-day Iraq. between 883 and 859 BC (Neo-Assyrian)(no copyrights)

Analogical reasoning and coherence maximization

So far, our folk biological expectations about the basic scheme of an animal body plan and our intuitions about the biological function of body parts offers an explanation of the modular “joints” of composites in Mesopotamian fantastic imagery. Now I turn to the rules apparently used for creating composites, which I also suggest originated in our cognitive faculties. More specifically, I argue that composites are assembled by reasoning analogically over the functional structure of a generic animal body plan (such as the one drawn above).

Cognitive theories and experimentation on analogical reasoning typically agree on three key points. First, analogical reasoning is a structure-mapping process where two abstract structures have their different parts mapped onto one another (Gentner 1983). An analogy is not based on superficial similarities between two objects such as shared material properties. Planet Mars is not analogous to a tomato because it is also red and round. Analogies also do not concern specific relations between two objects. Planet Mars is not analogous to a tomato because they both have water under their surface. Rather, analogies concern abstract relationships, or relations of relations, such that sound propagation and electromagnetic radiation are analogous because they both can be described through a wave function. A body part in one species is analogous to another if, for instance, they share a common biological function. So lion limbs and human legs are analogous because they are both used for walking, running, jumping, and standing.

Second, analogical reasoning consists in inferring something new on the basis of the matching of two abstract structures. In the specific case of composite imagery, the novelty is the composite itself, with the “sources” of the analogy being the different animals the parts of which have been borrowed.

Third, the “quality” of an analogy relies on satisfying several cognitive constraints, such as structural consistency and semantic similarity, which should be optimized to increase the strength of the analogy (Holyoak & Thagard 1989). In the present context, the maximization of structural consistency can

be understood as the maximal conservation of anatomical relationships in the composite, such that the borrowed animal body parts will tend to maximally respect the structural schema of the animal body plan, as drawn above. The maximization of semantic similarity means here that the animal body parts borrowed to assemble the composite will tend to serve the same biological function they did in the original animal.

I suggest that the construction of composites is a result of analogical reasoning and that composites will generally tend to optimize the structural match between similar functional parts of the animals used as sources for the composite. It is by assessing the structural consistency and the semantic similarity of a composite with that of the source animals' body-plans that we measure anatomical correctness in fantastic creatures. The less a composite satisfies these two factors, the more aberrant it will be. I will use 'coherence' when referring to structural consistency and semantic similarity together.

If the hypothesis suggested here is roughly correct, we should expect two kinds of observations. First, we should observe that maximally coherent composites constitute most of the repertoire of composite imagery found in the archaeological record. A corollary to this observation is that more aberrant forms do exist, but that they will be less frequent. Second, composites made from source animal parts having conflicting locations and/or functions will tend to maximize either their structural consistency or their semantic similarity. When this is not possible in any principled way, composites made of the very same conflicting parts will tend to solve the tension in different ways, thus leading to more anatomical variability in these problematic cases. The first hypothesis is straightforward. Most composites should not be very aberrant, and they are not. However, this also means that most composites have very little shock value as they typically are well-organized creatures that could exist in the natural world (remember the hippocampus). The second hypothesis is less obvious, and requires some further elaboration.

Imagine that we want to create a composite with two source animals that differ in some of their body parts such that one of the two animals has a body part with a specific function that the other animal lacks. For instance, imagine you are producing a composite with a bull and a lion as source animals. Bulls have horns, lions don't. The semantic similarity condition cannot be perfectly satisfied here because lions have no body parts functionally analogous to the bull's horns. There are two solutions to this problem. The first one is simply not to use horns in building the composite: the tension is resolved through omission. The second solution would be to use the horns but to use them such that structural consistency remains maximized. In the latter solution, the problematic part (the horns) can be joined to the composite so as to reflect their anatomical location in the original animal's body plan (the bull). Bulls' horns are located on their forehead, lions have forehead, then the natural location for bull horns on a composite with a lion head would be to fix them on the composite's forehead. Following the same principle, a fish dorsal fin goes on the back of an antelope (see figure below).



Ritual Knife with rows of animals on both sides of the handle (close-up), ca. 3300-3100 B.C.E.
 Flint, elephant ivory, 2 1/16 x 9 3/16 in. (5.3 x 23.4 cm). Courtesy of Brooklyn Museum,
 Charles Edwin Wilbour Fund, 09.889.118. Creative Commons-BY; modified for close-up.

A second kind of problematic composite appears when two body parts of different functions conflict for a same location on the composite's body plan. Consider the cases of composites with non-bird (e.g., land mammalian) body plans that nevertheless possess wings. No land-based mammalian possess clear functional analogues to wings. In the case of the lion with the bull horns, we could simply add the supplementary part where it was "missing". However, wings and mammalian forelimbs conflict as they both occupy the same structural location on an animal body plan – they are forelimbs, see body plan scheme above –, but they do not have the same function.

Is there such a thing as an anatomically correct location to put wings on a land-based animal composite? Contrary to the case of the lion with horns, there is no obvious answer or clear rule to follow as to what would maximize the coherence of such composites. One solution would be to replace the forelimbs with wings. Another would be to start the wings under the arm pits so that they are both located in the same place. Inversely, we could put the wings on the shoulders or on the upper back of the composite. Then again why not on the lower back, the head or even the feet? There is no necessity to solve the problem in any of those ways since they all seem to offer some coherence to the composite without any solution being obviously better than the others. Perhaps it is more satisfying to have the wings close to the shoulders – with the shoulders serving as an attractor point – but then again, there is no clear rules as to how to choose the "best" solution.

The lack of principled solutions to these sorts of problems means that we are likely to observe many different solutions for a same set of conflicting parts. This in turn will result in more variation in composites with the conflicting parts – i.e., in the location of wings on land animals' body plan (e.g., wings on heads, wings on upper back, wings on arm-pits, wings on lower-back, wings on feet, wings instead of upper limbs, etc.) – than we would with composites with no conflicting parts (e.g., composites with an head usually all have their head where we would expect it). At the same time, among this variation, we should expect to find wings more often around the upper limbs of the composite, such as replacing the arms, on the upper back or under the arm pits, since it is in that structural region that wings are located on birds. Wings elsewhere would be less intuitive, but not dramatically so. This prediction seems to be supported the sample of composites Wengrow uses in his book. The table below shows a count of such winged composites found in the figures used in *The Origins of Monsters*. Obviously the data set is not large enough to make any conclusive demonstration; I can send the list of figures on demand.

Wings under arm pits	6 composite images
Wings under arm pits	7 composite images
Wings on upper back/shoulders	4 composite images
Wings on head	3 composite images
Wings on middle and lower back (perhaps should be divided as two variants)	5 composite images

Note: as far as I could find, no winged land animal composite clearly had their wings closed on their side, whereas there are representations of birds with their wings closed on their side.

Finally, even among profound composite aberrations there seems to be some rules of maximizing coherence. An example of composites failing to maximize their coherence are those that break the rule of an anterior-posterior axis. For instance, the figure below is engraved with a composite breaking the rule of an anterior-posterior axis by having two heads and no posterior (limbs have been replaced by the symmetric repetition of the upper body). The modular structure of the body plan identified above shows why the anterior-posterior axis is generally respected: it is implicit in the fact that anterior and posterior appendages do not have the same functional role and structural organization. However, in this special case, the aberration might be better explained by its role in the general decorative economy of the object. The two-headed aberration is centrally located so that its symmetry axis is aligned with the symmetry axis of the object on which it is engraved. Aberrations of this kind might be better understood as satisfying esthetic desiderata, but this is an empirical investigation in which I will not commit myself here. Moreover, notice how the body plan remains highly coherent on each side of the symmetry. The symmetric freak does not disintegrate into a chaotic mush of body parts arbitrarily sown together. As developmental biologists know very well, even monsters follow strict rules (Gould 1977).



Egyptian apotropaic wand (or “knife”) made from hippopotamus ivory, 19th–17th centuries BC, in the British Museum (no copyrights)

Conclusion

The lack of a clear theory of what the modular structure of composites consists of belies the fact that Wengrow does not really develop on the cognitive factors (vs. social-economic and technological factors) of the production of composite imagery. This is surprising given that cognition might serve as a unitary basis for the multiple historical origins and convergences of composite imagery, such as in China, Meso-America, the Pacific North West, etc. In fact, Wengrow even acknowledges that his theory is limited to the Mesopotamian origins, arguing that the case for China does not depend on mechanical reproduction (pp. 83–87).

Here, I have argued that shock-value has very little to do with the composition and cultural success of composites. Rather, it is their flexibility and their expressivity that makes them so useful, and both flexibility and expressivity rely on our folk biological intuitions about anatomical correctness and our generative capacity to assemble arbitrary forms through analogical reasoning. Finally, analogical reasoning is based on a logic that seeks to maximize the coherence of the composites by maximizing their coherence. Contrary to Wengrow, I feel that non-aberrant, natural (but fictive) monsters are more popular than intuition-shocking, supernatural aberrations.



Modern impression on clay of Achaemenid cylinder seal, 5th. cent. BCE. A winged solar disc legitimises the Persian king who subdues two rampant Mesopotamian lamassu figures.

(Credit: Hjaltland Collection. CC BY-SA 3.)

References

- Gentner D (1983) Structure Mapping: A theoretical framework for analogy. *Cognitive Science*, 7:155–170
- Gould, S. J. (1977). The return of hopeful monsters. *Natural history*, 86(6), 22–30.
- Holyoak KJ, Thagard P (1989) Analogical Mapping by Constraint Satisfaction. *Cognitive Science*, 13:295–355.

Comments

David Wengrow

I am extremely grateful to Mathieu Charbonneau for his response to the book, and for putting together such a detailed counter-argument. In one important respect though I feel he has set up something of a straw man, so perhaps best to get this out of the way. Mathieu you repeatedly suggest that my hypothesis relies on images of composite animals being in some way “shocking”, and you spend quite some time refuting this point, which you think I have made. But nowhere in the book do I suggest this. I only suggest that these images are perceptually interesting, attention-worthy, and attractive in the same kind of broad sense as other “minimally counter-intuitive” representations; i.e. they are effective “sponges” for a whole variety of inferences and associations. Some of these may be to do with the supernatural, but certainly not all of them, and nowhere do I claim this either.

We are talking in many cases about mechanically reproduced images (like the modern impression of an ancient cylinder seal in your second image) that would have been viewed tens, hundreds, or even thousands of times. I find the idea that such images were inherently shocking as implausible as the idea that anyone might be shocked on walking into a Chinese restaurant and finding dragons on the placemats. I think it’s likely that standardised and widely reproduced images of composite figures actually served to disrupt the authority of established rituals, which would have been based on the less predictable (and potentially shocking) effects of actual performances. Images can do this by standardising the outcomes of transformation, by transferring such experiences from ritual into everyday contexts (like the handling of commodities), and by generally taming rituals in a way that reflects the centralising interests of elites. But that’s a different issue.

Moving on to ‘folk’/‘innate’/‘intuitive’ expectations for living kinds, and how these relate to fictive anatomy: if I understand correctly, Mathieu, you seem to imply that if a type of animal (your example is a platypus) is real (which you define as being ‘part of Earth’s natural history’), then it cannot be considered unnatural or anomalous from the perspective of innate biological expectations. If so, then I think you are confusing Linnaean and non-Linnaean systems of taxonomy. Innate systems of biological classification in humans do not have to be comprehensive in order to be innate. Like all classificatory systems they generate anomalies, as with Mary Douglas’ celebrated (1966) discussion of the Lele pangolin cult in ‘Purity and Danger’, or the many examples in Dan Sperber’s (1974) ‘Rethinking Symbolism’. This only goes to prove that these are indeed cognitive systems rather than complete or perfect accounts of empirical reality.

Again if I understand rightly, you consider it obvious that ‘most composites have very little shock value as they typically are well-organized creatures that could exist in the natural world’. As I’ve said, I don’t consider ‘shock-value’ to be important in this context, but notwithstanding I still find this a very surprising statement, and one that can be easily refuted by your chosen example:

because so far as I know, when European scientists first encountered the platypus most of them initially found it completely shocking, refused to believe it was a real animal at all, and even claimed it was an artificial composite. We may have learned to accommodate the platypus and the seahorse as part of ‘nature’, but the basic point is surely that innate biological expectations and Darwinian biology don’t always go hand-in-hand, and conflating them leads to confusion.

Now onto your points about the modular structure of composite body plans. I think what you say about the basic body scheme behind composite animals – and how this mostly conforms both to common-sense principles of anatomy and to analogical reasoning – is completely in line with what I say in the book (pp. 26-28), and strengthens it. It also seems in line with the definition of ‘analogism’ in the Descola/Fabrique des images exhibition, which everybody (except me) seems to be carefully avoiding any discussion of, so far at least. Your detailed ruminations on how best to attach wings to a land-based animal are, I imagine, quite close to the kind of discussions engaged in by the ritual specialists, bureaucrats, and image makers of ancient Egypt and Mesopotamia, thousands of years ago (see pp. 101-3 of the book).

So, turning to your conclusion, where exactly are we in agreement, or disagreement? I agree that ‘shock-value has very little to do with the composition and cultural success of composites’, but I don’t feel this undermines my argument in any way, because I haven’t claimed otherwise: almost the opposite in fact. You suggest that my theory is limited to Mesopotamia and surrounding areas, because it is there we find the spread of composite imagery in connection with mechanical modes of image production. But what I actually say is, again, quite the opposite. Since this keeps coming up, I’ll quote the relevant passage from pp. 88-9: ‘In the western Old World high levels of co-variation exist between figures of this type and mechanical modes of image making, but, for reasons explored in the previous chapter [i.e. the example of Bronze Age China], a direct causal relationship between the two phenomena can be safely ruled out’.

Your own explanation for the popularity of imaginary composites in various parts of the world, if I read you right, would be based on an innate human capacity ‘to assemble arbitrary forms through analogical reasoning’. But how would this account for the empirical distribution of these images in the archaeological record of the areas I write about – including such striking features as their relatively scarcity in prehistoric art; their relative popularity in large-scale and literate societies; and the stable transmission of specific imaginary figures across extraordinary vistas of time and space? Those are the main problems addressed in the book. Analogical reasoning, like mechanical reproduction, is not in my view sufficient to answer them. The problem is more complex and, I feel, more interesting as a result.

Mathieu Charbonneau

I believe that we are mostly in agreement, but that this did not come through in my commentary. In fact, I even think that my commentary builds on your theory and somewhat complement it.

First, let us get terminology out of the way. I used the expression “shock value” to refer to what you refer to as “minimally counter-intuitive”. I see them as synonyms, or at least was intending to. The confusion is strictly my fault as you indeed do not use such expression. Nevertheless, even with this confusion clarified, I do not think that it changes much to the point you make in your reply. I want to address them now.

In general, I find the argument of the book persuasive. I find very compelling your explanation of the distribution and diffusion of composites in early Mesopotamian city state (and then further west) in terms of socio-economical and institutional processes. I also completely agree with you that analogical reasoning does not explain the popularity of composites, and says little about how they were diffused. As the title of my response shows, I was discussing how to make monsters, not how to sell them (which is what you are interested in).

My commentary is mainly intended to add up to the thesis of the book by showing how cognition can play a larger role in your argument. The key criticism I made in my response concerns the role you ascribe to cognition. Well, in fact, it is unclear to me what is really going on with cognition in your argument. Given your reply, I find it even more difficult to understand why folk biology and intuitions are referred to at all. You wrote that “I only suggest that these images are perceptually interesting, attention-worthy, and attractive in the same kind of broad sense as other “minimally counter-intuitive” representations; i.e. they are effective “sponges” for a whole variety of inferences and associations.” I simply do not understand what is “minimally counter-intuitive” about them and in which way such counter-intuitiveness has anything to do with the diffusion of composites. An important part of my response is to argue that on the contrary composites are not counter-intuitive at all, most of the time.

In TOM, nowhere do we find an explanation of what minimal counter-intuitiveness is supposed to amount to when dealing with composites. We are told that composites are minimally counter-intuitive, but we are not explained what makes them so. The closest explanation is found in the brief mention of the supernatural and how it fires our imagination (segment that I quoted from the book in the response). But again, rereading the passage, it is unclear why having “one foot in anatomical reality and the other in fantasy” should lead to minimal counter-intuitiveness. I mentioned the platypus as an example of a composite with no foot (or paw) in fantasy but that still could be represented as a composite. You may be right that platypuses (and not platypi according to my word corrector) are minimally counter-intuitive and somewhat incredible, but this just shows that fantasy has nothing to do with the story. Then if not fiction or the supernatural, what makes a composite minimally counter-intuitive?

In addition, we would need an explanation of what “minimally” means here. With most composites (at least those illustrated in the book), we are very far from the minimal counter-intuitiveness of ghost going through walls or of man-eating trees. A strong expectation – of folk physics in the former, and folk botany in the latter — is violated in each case, but the other properties of the imaginary creature generally satisfies our intuitions. What are the principles at work in composites, and how can they be more or less respected?

As no such explanations are offered in the book, I found that cognition had in fact a very little, if any real role at all in your argument, despite what you suggest in it (especially in the chapter on the epidemiology of culture). My reaction was that cognition does have something important to say about composites, but perhaps not directly about their popularity and the patterns of their transmission. My reaction was to elaborate some more on an issue that your theory does not address in much details.

In contrast to what we can find in TOM, the model I sketch does offer avenues to answer the question of what minimal counter-intuitiveness means for composites, and how to measure the degrees of such counter-intuitiveness. In terms of our folk-biological intuitions, a minimally counter-intuitive composite is one that fails to fully satisfy our expectations about a typical animal body plan based on structural consistency – are there body parts where we expect them? – and semantic similarity – are the body parts where they should be? Seven-headed hydras, scary women with snakes instead of hair, a giant with a central eye (here it is the centrality of the one eye that really matters), etc; these are counter-intuitive as some body parts are not what or where we would expect them to be. They “shock” our intuitions but minimally, as the rest of their morphology is relatively natural (intuitively).

In the end, the commentary I wrote is mainly concerned with how composites are made, what they are made of, and how we might recognize one as somewhat counter-intuitive. I do not directly address their transmission, it’s true, and it is also true that transmission is mainly what your book is about. Nevertheless, the sketch model I developed does suggest some patterns in the transmission and reproduction of composites, specifically about their variability (e.g., the winged land-mammal example discussed in the response). I would be very interested to see further work on the variation of composites to see how the model and its corollary hypotheses will fare in the face of the archaeological record.

So where do we disagree? I would say mainly (1) about the importance and specific role of cognition in your theory, and (2) that composites are, at large, or most of the time, minimally counter-intuitive (a claim I disagree with). Certainly, you do pay lip ser-

vice to cultural epidemiology and the importance of cognition, but no real (or strong perhaps?) explanatory role is ascribed to it. I felt it could and should, and I suggested one way to reintegrate cognition in your stimulating project.

David Wengrow

I find nothing to disagree with in how you characterise the cognitive underpinnings of modular perception (or analogical reasoning). As you show, convincingly in my view, the treatment of vertebrate anatomy in images – and specifically the phenomenon of composite figuration – offers a window onto what is, for sure, an innate mental capacity for perception and reasoning of this particular sort.

The way you develop this point seems excellent to me, and is more detailed than anything I supply in the book. But it seems a bit extreme to state that nowhere do I deal with this issue, or try to explain how it relates to the concept of minimal counter-intuitiveness. Chapter 2 of the book (‘Materials for an epidemiology of culture’) is devoted almost entirely to this point, and builds on a large corpus of work (by, among others, Atran, Boyer, Stafford) to define what I see as the balance of intuitive and counter-intuitive factors in the imagery of composite creatures.

I also point out – following Descola and Karadimas – that composites built on analogical or modular principles are just one specific type of mixed body-form. Towards the end of chapter 3, I discuss another kind of hybrid body-type that we find, for instance, in the art of predynastic Egypt – a mixed body-type constructed on principles of affinity, continuity, and resemblance, rather than through the assemblage of strict anatomical contrasts on modular (part-whole) principles.

The next question, from an archaeological (and, I would have thought also an epidemiological) point of view is why modular principles of perception and representation – which, as we agree, have been cognitively available all along – come to the fore only at a given juncture in the history of these societies, why they seem to largely replace an earlier set of principles for the construction of images, and what this might imply in terms of wider patterns of social change. Those are the questions I set out to answer in the chapters that follow, through a wider interrogation of the visual and archaeological records.

Where we seem to be getting hung up is on the thorny issue of how cognitively counter-intuitive a given image has to be before it qualifies as counter-intuitive at all. For you, as I understand it, this would have to involve some striking violation of our innate cognitive *bau-plan* for vertebrate anatomy (one eye in the middle of head, etc.). For me the criteria are rooted more in the work of Atran and others on folk biology, which provides detailed evidence to suggest – as Maurice puts it – that there is ‘a general human tendency to classify the natural world in essentialist ways, thereby creating a non-Darwinian understanding of categorical differences between species’; and thus also, by implication, allowing for cognitively salient violations of the same (a horse with wings = counterintuitive; wings placed in an appropriate location for lift-off = intuitive; the resulting mixture = minimally counter-intuitive).

I wonder if it is even possible, in experimental terms, to devise a laboratory protocol that would reliably measure for degrees of counter-intuitiveness in such precise terms? I doubt it, and by way of sharpening definitions I feel we might do better by discussing concrete examples of non-modular (or non-analogical) modes of figuration, and how these interface with wider systems of memory, reasoning, and thought as developed in particular historical, institutional, and technological contexts.

Further comparison of ethnographic, archaeological, and art historical data on these lines could yield wonderful results. The potential is certainly there, and writers such as Carlo Severi and Dimitri Karadimas have made a flying start. I would see my book as hopefully contributing to these kinds of discussions, and would be intrigued to see some wider development of the debate on those lines.

Olivier Morin

One thing I don't quite understand in (Maurice Bloch's reconstruction of) Scott Atran's argument is the part where folkbiology is described as essentialist and anti-Darwinian. I don't exactly know what essentialism is. In fact, I subscribe to the view that "essentialism" referred, in the early 20th century, to a range of interesting critical ideas that Ernst Mayr later distorted into a polemical (and incoherent) straw man (here is a great paper that tells the story). Perhaps fixism, the view that species can never change, is what we are talking about here. So, let's assume that fixism is intuitive and widespread. What are the consequences for composite animals? They are not very clear, to me at least. Any reasonable fixist would accept that one animal species can look like some other species in various ways. Fixist biological classifications do not need to be more rigid or more absolute than evolutionary ones. They can (and they did) allow for the existence of "hybrid" species that look a bit like others: birds, but also bats and flying squirrels; fishes, but also whales; etc. If zebras (horses + stripes) or boars (pigs+tusks) are intuitive (and who would deny that?), then I don't see why unicorns (horses+horns) could fail to be intuitive too. In short, fixism need not make monsters counter-intuitive. I know that this question has been extensively written about, and that Mary Douglas' *Purity and Danger* is the locus classicus for this discussion, but I must confess that I always found her treatment of this problem a bit vague: we are not precisely told what makes some animals violate folk classifications **in the right way** to become impure. If having some features from another species were all it took, then all animals would be somewhat monstrous; yet real animals (even pangolins or platypi) are clearly neither monstrous, nor treated as such. There has to be something else—but what?

AN IMPORTANT CONTRIBUTION – BUT NOT AN AMENDMENT – TO CULTURAL EPIDEMIOLOGY

By Thom Scott-Phillips



In his excellent book, Wengrow argues that animal composites spread because of both cognitive and social-historical factors. The cognitive factors include human preferences for minimally counterintuitive images, and the way in which these composite images are especially well fit to the mode of perception of the world as made of divisible, recombining parts (rather than unique totalities) fostered by the state-like systems of organisation. The social-historical factors include among them urbanisation, state growth, display of cosmopolitan prestige by élites, and intense cultural exchange brought about by trading activity. As non-experts on these empirical matters, we found Wengrow's data fascinating and his arguments largely persuasive. For these reasons alone *The Origins of Monsters (TOM)* is a welcome contribution to cultural epidemiology.

In several passages (e.g. p. 7, 110–111), Wengrow suggests that his findings and conclusions not only contribute to, but also amend and extend the project of an epidemiology of representations, as developed by Dan Sperber, Pascal Boyer, and others. For instance, “the distribution of composite figures in the visual record offers fertile testing ground for an “epidemiological” approach to culture, and ultimately forces a revision of some of its central assumptions” (p. 7).

Wengrow believes this because, on his understanding, the project of an epidemiology of representations is “a school of evolutionary psychology” (p. 20) aiming at explaining culture in terms of largely innate cognitive dispositions, whereas his data show that such factors are only part of the story. For instance:

I suggest we can still accept, with the evolutionary psychologists, that the transmission of cultural imaginaries within a given population is grounded in frameworks of inference that are intuitively shared by most of its members. But it is only in view of the unique plasticity of human cognition – its embedding within forms of practical reason; its ability to shape and be shaped by institutional environments constructed over historical rather than genetic timescales – that the value of this insight becomes clear. (TOM p. 111).

This interpretation of the epidemiology of representations is not an uncommon one, but it is, we are sure, an unfortunate misunderstanding, comprised of two parts, as we describe below. The upshot of this is that, rather than revising the epidemiology of representations, TOM in fact subscribes to its orthodox version, and in fact makes an important empirical contribution to it.

First, factors of attraction are not limited to psychological factors, such as those that Wengrow draws attention to, but also include ecological factors. On the one hand, the local environment selects the members of a population, provides the inputs for individuals to process, and puts temporal, spatial and material constraints to the formation, transmission, and stability of representations. These are all ecological factors. Demography is a good example, as too are some of the factors that Wengrow draws attention to in this case, such as urbanisation and state growth. On the other hand, the mind and its equipment determine which available inputs are processed and how, and what information influences behaviour. These are psychological factors. From its inception, the idea of cultural attraction included both psychological and ecological factors as key components of the explanation of the distribution of cultural representations (Sperber, 1996, p. 113). Their equal importance is illustrated by, for instance, the title of the relevant section in Sperber’s *Explaining Culture* (1996): “Ecological and Psychological factors of attraction”. It is the same in later work, where ecological factors are listed beside random forces, natural selection, and psychological factors as different aspects of cultural attraction (Sperber & Claidière, 2008). Psychological factors are “properties of the members of a population: psychological and biological susceptibilities”, whereas ecological factors are “properties of the environment of the population” (Claidière et al., 2014, p. 7).

Second, psychological factors of attraction do not include only the genetically-determined cognitive structures which are adaptations to the ancestral environment produced on the time-scale of biological evolution, but also those psychological aspects that are the output of cultural cognitive development on the historical time-scale (Claidière et al., 2014; Claidière & Sperber, 2007; Sperber & Claidière, 2008; Sperber, 1996). Again, Wengrow points to several such factors. So too, incidentally, can ecological factors originate in both the natural environment and the culturally modified environment, which includes the pool of public cultural representations available to population members, and

which can cause individuals to interpret inputs in locally converging ways (Sperber & Claidière, 2008; Sperber, 1996).

Having said all of that, Wengrow's interpretation of the project of an epidemiology of representations, as more narrow than its architects intended, is not an uncommon one. Henrich & Boyd (2002), for example, criticised Sperber, Atran, and Boyer for a supposed exclusive interest in the role of the innate structures of human mind in shaping cultural evolution, and for paying insufficient attention to social factors involved in the transmission of cultural representations, and also population dynamic models of cultural change (Henrich & Boyd, 2002, p. 87). In their reply to that article, Claidière & Sperber (2007) do mention the existence of different kinds of factors of attraction: "At the most general level, [the factors of attraction] may have to do with psychological dispositions or with environmental constraints and affordances", and underlined that "contrary to what Henrich and Boyd suggest, it has never been part of the theory that factors of attraction should be exclusively cognitive" (p. 92).

However, since this reply was principally focused on other matters (in particular the role of selection and cognitive transformation in cultural change), they did not dwell on the point. Perhaps this was a missed opportunity for further clarification: the same misunderstanding recurs elsewhere (see Acerbi & Mesoudi, 2015 for some recent discussion).

Going back to *The Origins of Monsters*, it seems to us that the epidemiological approach already offers W. all the conceptual tools that he needs for his explanation of the massive spread of composite images in the Bronze Age. Wengrow's analysis of the "cultural ecology" of composites is entirely in line with the complete version of the epidemiology of representations, and is actually, we believe, a brave and impressive application of the theory to the archaeological record.

References

Acerbi, A., & Mesoudi, A. (2015). If we are all cultural Darwinians what's the fuss about? Clarifying recent disagreements in the field of cultural evolution. *Biology & Philosophy*, 30(4), 481–503.

<http://doi.org/10.1007/s10539-015-9490-2>

Claidière, N., Scott-phillips, T. C., & Sperber, D. (2014). How Darwinian is cultural evolution? *Philosophical Transactions of the Royal Society B: Biological Sciences*, 369, 20130368.

<http://doi.org/http://dx.doi.org/10.1098/rstb.2013.0368>

Claidière, N., & Sperber, D. (2007). The role of attraction in cultural evolution. *Journal of Cognition and Culture*, 7(1), 89–111. <http://doi.org/10.1163/156853707X171829>

Henrich, J., & Boyd, R. (2002). On Modeling Cognition and Culture representations. *Journal of Cognition and Culture*, 2(2), 87–112. <http://doi.org/10.1163/156853702320281836>

Sperber, D. (1996). *Explaining Culture. A Naturalistic Approach*. Oxford: Blackwell.

Sperber, D., & Claidière, N. (2008). Defining and explaining culture (comments on Richerson and Boyd, Not by genes alone). *Biology & Philosophy*, 23(2), 283–292.

<http://doi.org/10.1007/s10539-005-9012-8>

Wengrow, D. (2013). *The Origins of Monsters: Image and Cognition in the First Age of Mechanical Reproduction*. Princeton: Princeton University Press.

CAN CULTURAL EPIDEMIOLOGY EXPLAIN THE CULTURAL EVOLUTION OF MONSTERS?

By Karolina Prochownik



This book deals with a fascinating topic and, despite its modest size, the scope of archeological and historical records it examines is impressive. One of its biggest advantages in my eyes is that it combines different perspectives—cognitive science, archeology and history—in the search for an explanation of the cultural evolution of particular cultural representations: monsters (also known as composites). I believe that in doing so it sets a standard for high-quality interdisciplinary studies in the area of cultural evolution.

My commentary will have two parts. In the first part I will examine the extent to which cultural epidemiology can account for the cultural evolution of monsters. In particular, I will discuss some claims that Wengrow has put forward in regard to cultural epidemiology as a cognitive approach to culture in general, and the way he has adapted this approach to the particular study of composites. My reservations will concern the way that the author has reconstructed some basic assumptions of cultural epidemiology and the way they have been applied to his subject of study.

In the second part I will propose, for the sake of discussion, some hypothetical extensions of the account proposed by the author from the epidemiological perspective. None of my remarks are intended to be strongly critical; my aim is rather to make a few clarifications and discuss some potential directions of development for the account that has been proposed.

Wengrow characterizes cultural epidemiology as the study of how evolved cognitive dispositions and constraints account for the stability and distribution of cultural representations in a population (TOM, p. 3, p. 20). In particular, the author focuses on the ways in which the epidemiology of representations explains how macro-level cultural phenomena are informed by modular cognitive systems (TOM, p. 21). Wengrow points out that composites (beings that combine body parts from different species; TOM, p. 1, p. 26) are minimally counterintuitive representations (i.e., they violate some of the expectations generated by our intuitive biology, while preserving others). Based on standard assumptions of cultural epidemiology, he then claims that such concepts should be universally catchy to human minds and should enjoy a cognitive advantage in the process of cultural transmission (TOM, p. 50). However, one of the main points that Wengrow makes in his book is that composites were quite rare in Paleolithic and Neolithic art, and that they became culturally stable and cross-culturally successful only around 6000 years ago (and later), in the “first age of mechanical reproduction” (the Bronze Age and the Iron Age). The author proposes that various non-cognitive factors (such as the invention of a technology of mechanical reproduction, along with specific cultural, institutional and political components of the first states) may have been relevant for the cultural spread of composites in this period.

Wengrow points out that a role for such factors in the cultural success of composites is not something one would expect under the assumption that the human mind is naturally receptive to counterfactual creatures, and that this therefore poses a puzzle for cultural epidemiology. As he writes: “It implies a strong element of redundancy—a kind of superfluous cultural prosthesis to cognitive predispositions that are already biased towards the reception of such images” (TOM, p. 80). A full-blown version of this argument is also put forward elsewhere in the book: “The distribution of composite figures in the visual record raises a number of intriguing problems for the study of cultural transmission, for which only partial and unsatisfactory solutions have so far been offered. Their impressive transmission across cultural boundaries [...] is consistent with the expectations of an “epidemiological” approach to the spread of culture, which would accord them a special kind of cognitive catchiness. But this approach, in its current form, offers no way of explaining why such images become stable and widespread only with the onset of urban life and state formation, beginning little more than six thousand years ago—a mere blink of the eye, on the timescale of biological evolution [...]” (TOM, p. 88).

Can the epidemiological approach provide only partial and unsatisfactory explanation of the trajectories of cultural evolution of monsters? Is it true that the study of the cultural distribution of composites through the ages “ultimately forces a revision of some of its central assumptions”? (TOM, p. 7; cf. p. 24). There are two points that require more in-depth addressing and clarification before deciding the question of whether the case of monsters really poses a puzzle for cultural epidemiology. The first point concerns the role of minimal counterintuitiveness as a cognitive factor of attraction in the cultu-

ral transmission of monsters. (I use the notion of “attraction” to refer to all factors which bias cultural transmission to converge on certain themes or types; I perceive it to be an equivalent of “catchiness”, as used in the book.) Specifically, the minimal counterintuitiveness (or the counterfactuality) of monsters is the only cognitive factor identified and considered in the proposed epidemiological account of the cultural evolution of monsters (e.g., TOM, p. 23, p. 24, p. 28, p. 50, p. 51, p. 78, p. 80, p. 82, p. 111). Although the property of being counterintuitive has been recognized as a recurrent feature of cross-culturally successful representations, it is far from being clear whether being counterintuitive is a sufficient condition of cultural stability and cultural success for such representations. A more detailed discussion of other frequent aspects of highly cross-culturally successful representations such as religious beliefs could shed some light on this issue. My second point is clarificatory. Wengrow characterizes the epidemiology of culture as an approach aimed at explaining culture by examining the evolved modular cognitive dispositions and biases that inform and constrain cultural transmission (TOM, pp. 19-24). The epidemiological notion of attraction is understood exclusively in terms of cognitive attraction (TOM, p. 88, pp. 50–51). These, however, are not the views of cultural epidemiologists.

With regards to the first point, some influential research has clearly shown that minimally counterintuitive concepts are more memorable and attention-grabbing and thus have an advantage in cultural transmission (e.g., Boyer & Ramble, 2001; Barret & Nyhof, 2001). Also, minimal counterintuitiveness has been recognized as a typical feature of culturally successful representations such as supernatural beliefs (e.g., Boyer, 2001; Sperber, 2012). Nonetheless, being minimally counterintuitive is not the only cognitive component that matters in the cultural propagation of representations over time. People create and communicate various minimally counterintuitive concepts, but not all of them become culturally stable or widespread. Supernatural beliefs constitute a paradigmatic example of cross-culturally successful minimally counterintuitive beliefs. What is special about religion, in addition to being counterintuitive? Pascal Boyer proposed that “it activates inference systems that are of vital importance to us: those that govern our most intense emotions, shape our interaction with other people, give us moral feelings, and organize social groups.” (Boyer, 2001, p. 135). Elsewhere he made this point even more explicit: “[...] religion as we know it is not just a matter of counterintuitive concepts. Religion is not just about flying mountains, talking trees and biological monsters but also about agents whose mental states matter a lot, about connections with predation and death, about links with morality and misfortune” (ibid., p. 324). Hence I believe that what we can learn about cross-culturally successful representations from studying supernatural beliefs is that they often activate many different mental systems and generate rich inferences (cf. Sperber & Hirschfeld, 2004, p. 45; Boyer, 2003; Boyer, 2001, p. 164). In other words, representations which are culturally widespread are more cognitively relevant than others: they produce more inferences for less cognitive effort (Boyer, 2000, p. 210).

Coming back to biological monsters, such counterfactual representations may be relatively easy to remember, reason about, and communicate to others, but their overall inferential potential may be relatively poor comparing to other counterintuitive concepts, unless some additional characteristics ap-

ply. In other words, a degree of relevance associated with the property of biological counterfactuality may not be sufficient to drive cultural transmission in the direction of cultural stability and cross-cultural distribution. Perhaps a change in composites' degree of relevance through the ages is another factor worth considering in accounting for the patterns in the cultural evolution of monsters that Wengrow reported. In particular, it may be that composites of the "first age of mechanical reproduction" were built in such ways that they activated more mental systems and produced richer inferences. I will come back to this hypothesis in the second part of my commentary.

Even leaving this aside (and turning now to the second point mentioned above), the epidemiology of representations is more than the study of cognitive factors that inform and bias cultural transmission, or of general patterns of cognitive attraction. One of the discipline's core assumptions is that two different factors of attraction—psychological and ecological—matter for the explanation of the cultural transmission and cultural stability of particular representations (Sperber, 1996, pp. 113–118). Psychological factors of attraction are individual-level processes, which direct the way people reconstruct and transmit cultural information to converge on certain themes or types (e.g., evolved mental modules, or idiosyncratic psychological properties). Cognitive and affective/emotional factors of attraction belong to this category. Ecological factors are all the features of physical or cultural environments that affect the transmission of cultural information in such a way as to converge on certain themes or types (cf. Sperber, 1996, p. 113; Claidière, Scott-Phillips, & Sperber, 2014, p. 4). Various factors that qualify as "environmental" in a broad sense—natural, economic or cultural—can fall under this label. Importantly, according to the epidemiological approach, it is the interaction between psychological and ecological factors that accounts for most historical changes in the distribution of cultural representations (Sperber, 1996, p. 115). In other words, to explain why a particular type of representation constitutes a cultural attractor in a specific historical context, one must identify all the important factors of attraction for this type of representation, in that context. Because the balance of particular ecological and psychological factors is changing, cultural attractors are not stable and also change over time. As Dan Sperber pointed out: "Cultural attractors emerge, wane, or move, some rapidly, others slowly, some suddenly, over historical time" (1996, p. 115). In a similar vein, Claidière and Sperber noted that "attractors themselves can and do change over time as an effect of the factors that explain them [...]" (2007, p. 92).

This clarification concerning the epidemiology of representations has implications for some specific points made in the book. First, there is no contradiction between the notion of attraction and historical or institutional factors having an impact on cultural transmission (e.g., TOM, p. 51, p. 82, p. 89; pp. 111–112), as the latter belong to ecological factors of attraction. Actually, Wengrow himself frequently speaks of "cultural ecology" in his book (TOM, p. 51, p. 67, p. 106), and is remarkably successful in identifying a range of ecological factors of attraction for images of composites in the "first age of mechanical reproduction". I will come back to this point at the end of my commentary.

Second, attraction and fittingness are not alternative accounts of the cultural propagation of monsters. If I understand correctly, Wengrow speaks of fittingness in terms of congruence between general esthetic and constructive canons developed in the urbanized world of the first civilized states, and the particular esthetic principles on which composites were made (TOM, p. 59, p. 89). He places this among the factors that facilitated cultural spread of the composites in this historical context. But if so, then fittingness can be rephrased in terms of a cultural factor of attraction. Apparently, two of the types of esthetic principles that Wengrow mentions constitute an instance of two cultural traits that mutually reinforce each other's cultural transmission (cf. Claidière et al., 2014, p. 4).

Moreover, the author's observation concerning the plasticity of human cognition, and its openness to being (to some extent) shaped by institutional factors and historical circumstances (TOM, pp. 111-112) is also compatible with cultural epidemiology. As said, the epidemiological approach envisages that psychological and ecological factors are in constant interaction and together create the cultural space in which cultural representations converge on certain types. For example, Baumard, Hyafil, Morris, & Boyer (2015) proposed that an increase in energy capture per capita between 500–300 BCE affected people's motivational systems in the direction of a preference for long-term versus short-term strategies, which in turn contributed to the cultural propagation of moralizing religions during the Axial Age. Finally, Wengrow's observation that images of monsters are not universally but selectively culturally attractive in particular circumstances (TOM, p. 89) is reconcilable with the epidemiological perspective's emphasis that cultural attractors are context-sensitive and change over historical time.

What I believe follows from these two points is that the changes in the cultural distribution of monsters through the ages, as observed by Wengrow, are not strictly speaking a puzzle for cultural epidemiology, in that the latter always envisages various psychological and ecological factors as important for explaining trends in the cultural transmission of representations. Yet although not necessarily such a puzzle, the cultural evolution of monsters still poses something of a challenge for cultural epidemiology. That biological monsters became cultural attractors in the "first age of mechanical reproduction" requires an explanation in terms of identifying certain forces of psychological and ecological attraction: i.e., particular micro-processes which biased cultural transmission in the direction of a macro-level increase in the frequency of these images during this period.

Therefore, I will next propose two stories that may shed some light on what might have happened in the course of the cultural evolution of monsters to transform them from rare images in the Paleolithic and the Neolithic eras to cultural attractors in the "first age of mechanical reproduction". The first story concerns possible changes in the way composites were represented, and the second story concerns changes in the environment in which they were culturally transmitted. These stories are not mutually exclusive, and should be considered jointly. Some elements of these stories were already presented by Wengrow in his book, while others represent an attempt at elaborating the ideas that have been put forward

Changes in the mode of representation of composites

My first hypothesis is that something about the way monsters were mentally and publicly represented changed during the “first age of mechanical reproduction”, and that this can account for the observed increase in their cultural distribution in this period.

The main problem with evaluating this hypothesis is obvious: we cannot be certain about the way people were imagining composites in the Paleolithic and Neolithic periods (this is not easy even for later periods), hence it is difficult to assess which features of composites were distinctive of the “first age of mechanical reproduction”. My proposal below will be constrained to tracing some common properties of composites in the Bronze Age and the Iron Age, when they already were cross-culturally successful. This is under the assumption that some of these traits may at least partially account for their cultural success in this period. I leave it to the specialists to assess whether these features really distinguish composites of the Bronze Age and the Iron Age from the composites of the Paleolithic Age and the Neolithic Age.

I suggested above that being minimally counterintuitive per se might not be enough to cause the cultural stability and cross-cultural success of a representation. In particular, minimal counterintuitiveness in composites may not have provided them with enough relevance to guide their cultural transmission in the direction of cultural attraction in the Paleolithic and Neolithic times. Perhaps composites of the “first age of mechanical reproduction” were constructed in such ways that they engaged more mental systems and produced richer inferences than their predecessors. This could explain—at least to some extent—why monsters became culturally stable and widespread around six thousand years ago (and later).

There are several possible scenarios concerning how the relevance of composites could have been altered. They entail either the granting of some new properties onto counterfactual biological creatures, or their increased association with important domains of human life. Again, these scenarios are not mutually exclusive, and could have occurred in various combinations.

One such scenario envisages that around the transition from the Neolithic to the Bronze Age visual components of composites drifted in the direction of the prevalence of body parts of predatory animals. Counterintuitive creatures resembling predatory animals might have effectively triggered mental systems responsible for the detection and avoidance of predatory agents. This in turn might have increased the cognitive relevance of counterfactual representations of this type and given them an advantage in cultural transmission.

Taking this perspective, it is not surprising that the composites in the “first age of mechanical reproduction” were largely made of body parts of predatory animals such as lions, snakes, crocodiles, scorpions, and eagles (or at least, this picture emerges from the sample of composites provided in the book: e.g., TOM, p. 10, p. 17, p. 57, p. 62, p. 84, p. 93, p. 97, pp. 101-102, p. 104). Even if main body parts of some of the composites in this period did not belong to predatory animals, they fre-

quently manifested features potentially dangerous to humans (e.g., horns, claws, fangs, and beaks), and sometimes in exaggerated forms (e.g., TOM, p. 10). Perhaps this relatively high frequency of predatory composites in the “first age of mechanical reproduction” was not random: cultural images of composites which triggered thoughts of dangerous predators might have been more cognitively attractive than composites made of physical parts of other animals. The visual effects of these images might have been reinforced by mythological narratives which frequently presented monsters as dangerous or even life-threatening intentional agents (e.g., Greek myths about the Sphinx, the Gorgon, the Minotaur, or Cerberus). Additionally, successful cross-cultural transmission of such images and stories might have been due to people’s general tendency to be more credulous toward information concerning hazards (e.g., Fessler, Pisor, & Navarrete, 2014). Finally, if monstrous biological creatures invoked concerns of dangerous predators, this might shed light on the reason why some other properties were frequently ascribed to them too. In particular, imagining monsters as powerful protectors (TOM, p. 89, pp. 99-107) could be related or even secondary to perceiving them as a danger (i.e., if you did not believe that monsters were threatening in the first place, how could you ever believe they could protect you?).

There are many other possible scenarios which envisage that composites were associated with various domains of life that were of general importance, and that this would have had an impact on their cultural propagation in the Bronze Age and the Iron Age. For instance, counterfactual monsters might have been associated with moral matters (Wengrow loosely refers to connections between composites and moral processes at the end of the book; TOM, p. 112). It seems that such associations might at least to some extent be traced back in mythology and material culture of the period. For instance, in ancient Babylonia the cultural hero / fish-man monster Oannes (Adapa) was connected to law-making (e.g., TOM: introduction); and a snake-dragon composite was an emblematic animal of Marduk, a god associated with justice and supernatural punishment (e.g., Leick, 2002, p. 116). Maat—the ancient Egyptian winged goddess of law and order—the jackal-headed Anubis, the ibis-headed Thot and the falcon-headed Horus were all engaged in the process of judging the dead in the underworld; whereas a part-lion, part-hippopotamus and part-crocodile monster Ammit whose name translates as “Devourer” was waiting to eat the hearts of sinners (e.g., Meeks & Favard-Meeks, 1996, Figure 16). Also some versions of Greek myths about the Sphinx and the Minotaur present them as punishments that gods sent for misdeeds committed by people.

Besides this association with morality, monsters might have been associated with other aspects of social interactions. For instance, the fact that the new social elites utilized foreign objects and motifs such as composites as their group markers during the period of transformations (e.g., TOM, p. 91) seems to point at a connection between the composites and social group identification. Other possibilities worth considering include associations with human concerns about misfortune—some composites, e.g., Lamashtu, were believed to cause illness and misfortune and/or were invoked in defense against them (TOM, p. 102, p. 89) and with concerns about purity—figures of composites were used in purification rituals (TOM, p. 99) and in defense against pollution (TOM, p. 100). Overall, representations of counterfactual creatures which activated mental systems governing morality, social interac-

tions and contagion might have had an advantage in cultural transmission compared to representations devoid of such features (Boyer, 2003).

Moreover, some properties of monsters might have been altered in response to changes in human subsistence conditions in the “first age of mechanical reproduction”. For instance, in the very beginning of the book we are presented with the story of Oannes (from Berossos’s History of Babylonia), who taught people all sorts of knowledge, including writing, mathematics, building cities and temples, and making laws. Because the subsistence of the first civilized states was highly dependent on these kinds of knowledge, composites of this type could have been granted more relevance than mere biologically counterfactual creatures. I believe that this case could be extended to a variety of monsters, as mythology frequently presented them as guardians of highly esteemed goods (e.g., knowledge, powerful objects, and treasure).

In the final analysis, what may be worth considering in its own right is the potential increase in the degree to which composites were associated with religious beliefs and organized worship. Many examples of such connections have been provided above. TOM mentions fantastic animals that had religious significance (p. 17), demonic agents imagined as composites (p. 56), gods and goddesses presented as composites (e.g., Taweret; p. 63), figures of composites utilized in cleaning rites that invoked gods (p. 100), composites with a role in the ideology of sacred kingship (p. 62), and others. Expanded connections with religious systems might have been an additional booster for the cultural propagation of monsters.

In any case, this list is an initial proposal and offers only a few speculative scenarios of how changes in modes of representation of composites might have altered their relevance, and how this might have contributed to their cultural stability and increased distribution in the “first age of mechanical reproduction”. Experts will have more to say on the value of these insights; perhaps they will propose some alternatives.

Changes in the environments in which representations of composites were culturally transmitted

The second story entails that some changes occurred in the environment (widely understood as including natural, economic, political, institutional and cultural conditions) in which composites were culturally transmitted, and that this contributed to their higher cultural distribution and cultural stability around 6000 years ago (and later).

With regards to this point I do not have much to offer, as in his book Wengrow has identified and examined in detail a range of ecological factors of attraction for composites that he argues were specific to the “first age of mechanical reproduction”. Some of the most important factors are:

- Urbanization (TOM, p. 2, p. 59, p. 67, p. 88)
 - Expansion of political networks and formation of new states (TOM, p. 2, p. 16, p. 74, p. 88)
 - Expansion of commercial networks (TOM, p. 2, p. 8, p. 12, p. 16, p. 59, p. 60, p. 94)
 - Emergence of new social elites (TOM, p. 2, p. 8, p. 61, p. 91; pp. 90-94)
 - Use of composites as a part of the lifestyle of these new elites (TOM, p. 81, p. 91, pp. 90-94)
 - Invention of technology of mechanical reproduction (TOM, p. 74-87)
 - Establishment of general canons of esthetics and construction oriented so as to combine individual parts and part-whole relations (TOM, p. 49, pp. 54-55, p. 69, p. 89)
 - Emergence of an international esthetic style (TOM, pp. 94-99)
 - Environments of higher risk and uncertainty (e.g., societal, political and diplomatic instabilities; TOM, pp. 106-107)

As pointed above, Wengrow shares with the epidemiological approach the view that environmental factors of different types should be considered jointly and in interaction with psychological factors (e.g., minimal counterintuitiveness) in formulating a fully-fledged account of the cultural evolution of composites. It might be worth exploring the exact roles that these different factors played in the cultural transmission of monsters, and scrutinizing in detail some of the main pathways of their interaction. For instance, some of the environmental conditions under examination seem to be crucial as they conditioned all the others (in particular, urbanization). Others might have been mutually reinforcing, such as the new principles of esthetics and construction based on ideas of modularity and the urbanized forms of life (e.g., TOM, p. 59, p. 106). Finally, the ways that psychological and environmental factors interacted might account for why composites spread cross-culturally and why some of their particular types were more popular than others. For instance, some fears and anxieties might have been psychologically more salient among members of groups that were living under constant threat of political disintegration and social instability (TOM, pp. 106–107). This in turn might have biased the cultural transmission of composites to converge on themes of protectors (the intensification of societal fears and representing biological monsters as predatory agents might both have contributed to the cultural attraction of protective images of monsters).

In summary, I want to repeat that I found Wengrow's book to be an excellent epidemiologically oriented case study of the cultural evolution of monsters. I am eagerly looking forward to future developments of his account. Identification of all the important causal forces standing behind the cultural dissemination of composites—despite gaps in knowledge and all the messiness of historical and cultural processes—is open to further elaboration. Perhaps this might be an issue for a research project that could engage the close collaboration of archeologists, historians, and cognitive scientists?

References

- Barrett, J. L., & Nyhof, M. A. (2001). Spreading non-natural concepts: The role of intuitive conceptual structures in memory and transmission of cultural materials. *Journal of Cognition and Culture*, 1(1), 69-100.
- Baumard, N., Hyafil, A., Morris, I., & Boyer, P. (2015). Increased affluence explains the emergence of ascetic wisdoms and moralizing religions. *Current Biology*, 25(1), 10-15.
- Boyer, P. (2000). Functional origins of religious concepts: Ontological and strategic selection in evolved minds. *Journal of the Royal Anthropological Institute*, 195-214.
- Boyer, P. (2001). *Religion explained: The evolutionary origins of religious thought* (No. 170). Basic Books.
- Boyer, P., & Ramble, C. (2001). Cognitive templates for religious concepts: Cross-cultural evidence for recall of counter-intuitive representations. *Cognitive Science*, 25(4), 535-564.
- Boyer, P. (2003). Religious thought and behaviour as by-products of brain function. *Trends in cognitive sciences*, 7(3), 119-124.
- Claidière, N., & Sperber, D. (2007). The role of attraction in cultural evolution. *Journal of Cognition and Culture*, 7(1), 89-111.
- Claidière, N., Scott-Phillips, T. C., & Sperber, D. (2014). How Darwinian is cultural evolution?. *Philosophical Transactions of the Royal Society B: Biological Sciences*, 369(1642), 20130368.
- Fessler, D. M., Pisor, A. C., & Navarrete, C. D. (2014). Negatively-biased credulity and the cultural evolution of beliefs. *PloS one*, 9(4), e95167.
- Leick, G. (2002). *A dictionary of ancient Near Eastern mythology*. Routledge.
- Meeks, D., & Favard-Meeks, C. (1996). *Daily life of the Egyptian gods*. Cornell University Press.
- Sperber, D., & Hirschfeld, L. A. (2004). The cognitive foundations of cultural stability and diversity. *Trends in cognitive sciences*, 8(1), 40-46.
- Sperber, D. (1996). *Explaining culture*. Oxford: Blackwell Publishers.
- Sperber, D. (2012). Cultural Attractors. In John Brockman (ed.) *This Will Make You Smarter*, 180-183.

Comments

David Wengrow

Carmen, Thom, and Karolina's comments are concerned mainly to show that my book affirms the principles of the epidemiological approach to culture, and provides an example of its explanatory power. In general their responses are so similar that it makes sense, in this case, to give them a joint reply, and I hope this is acceptable.

Carmen and Thom in fact go slightly further, suggesting that ‘all the conceptual tools needed’ for my analysis are contained within the epidemiological approach. That would be quite a conceptual coup, given that epidemiologists have so far had little to say about the transmission of images, or material culture, or the archaeological record in general. I take them to mean that the conceptual tools I use are compatible with an epidemiological approach.

All three are also concerned to show, in the politest way possible, that I have not fully understood my own contribution to this exciting and emerging field of cultural-cognitive research. Specifically, I should have realised the epidemiology of culture does not assign causal priority to evolved cognitive dispositions in explaining the attractiveness of some cultural traits over others. It places equal weight on environmental factors, which potentially cover the whole range of institutional and technological features unfolding over timescales much shorter than those of biological evolution.

I am not alone in this misconception, and they mention other researchers who have similarly underrated this aspect of the epidemiological approach, to which we now may add Carlo Severi's *Le Principe de la chimère*, which deals with related (but by no means identical) material. ‘We have noticed’, writes Severi (2015: 239), ‘that, in order to construct a theory appropriate for cultural propagation, it is not sufficient to refer, as Sperber and Boyer do, to a general psychology of memory; it is also necessary to take into account the pragmatic aspect, that is to say, the specific conditions in which the cultural propagation of representations is taking place’.

Why then do researchers like me, who are clearly sympathetic to the aims of the epidemiological approach, keep misreading it in this way? Less generous respondents than those writing on this forum might point out that academics stand to make quicker (but not necessarily greater) reputational gains by bringing paradigms down than by following or applying them. Obviously I can't speak for Severi or anyone else. I take Dan Sperber seriously when he writes that the epidemiological agenda is a work in progress, and that the validity of its theoretical ambitions should be weighed against its ability to resolve concrete problems in the study of cultural transmission.

As I've already indicated, this for me meant supplementing the existing toolkit in various ways. It meant broadening the definition of key concepts, such as “minimal counter-intuitiveness”, to areas of cognition – specifically the area of visual perception and the cognition of images – so far untouched by the epidemiological approach. Some of the contributors to this forum (Bloch, Boyer) feel I have succeeded fairly well in this, while others (Morin, Acerbi) are less convinced. It also meant adapting the whole approach to the archaeological record, which it was not designed for, and which is no easy task.

On the negative side that presented issues of interpretation – especially concerning scales of analysis and taphonomy – that anthropologists and psychologists are not used to dealing with. Some of the more tricky exchanges so far on this forum can be attributed at least partly to these difficulties of translating ideas between disciplines. On the positive side, the archaeological record is replete with evidence for spreads of cultural data in time and space, of just the kind that epidemiological approaches focus on and seek to

explain. In some respects, as I've suggested, archaeology may even be better suited to this kind of analysis than the ethnographic record in its current state.

It would be odd, in a way, if all this reshaping of the approach to a new kind of data-set led simply to an affirmation of the initial theory, or to its simple refutation. I think what I've actually come up with is something in between. Granito, Scott-Philipps, and Prochownik all focus on aspects of my book that can be seen to affirm the theory and method of the epidemiological approach. They do a much better job of this than me, and I am grateful to them for these clarifications. Pascal Boyer makes some of the same points in his earlier post. But even once we've ironed out the differences caused simply by disciplinary conventions, some questions nevertheless remain, for me at least.

One way to formulate these questions, to which I hope the contributors on this forum can supply some answers, would be to focus on the issue of cultural attraction. As I understand it, the epidemiological approach to culture describes transmission mainly in terms of attraction, or what we might call positive transmission (and see my earlier response to Erhard, concerning Franz Steiner's early speculations on this). The theory postulates that cognitive attractors coupled with environmental attractors produce the most successful distributions of cultural facts. The comments by Carmen, Thom, and Karolina confirm this in quite strong terms. Logically, this seems to make perfect sense. But I wonder how adequate it is for explaining the kinds of phenomena treated in my book?

The reason I ask is that, more than once when writing it, I found myself pulled away from the notion of attraction towards another mode of transmission, which I think has been best described by David Napier through his notion of an 'immunological' paradigm for culture (also discussed in my earlier posts). This too is a naturalistic model of culture, but one that takes as its starting point the workings of the immune system as an analogy for cultural process. In that sense it sets up a mirror image to the epidemiological approach. Something like this other approach seems necessary to understand the phenomena described in my book (and see especially Chapter 6). The focus would be on negative as well as positive transmission, on what we might call 'cultural deflectors', as well as attractors.

Following Napier's analogy, deflectors in cultural systems would work rather like antibodies in biological systems. The composite, otherworldly anatomies I consider in my book are in some ways culture's prototypical "anti-bodies", and their social functions as gatekeepers, apotropaic devices, and so on clearly involve deflection and repulsion, as well as attraction (or, more accurately, attraction of what is harmful, in order to repel it). In my view the most sophisticated discussion to date of how exactly such devices work – both at the micro (cognitive) and macro (social) levels – is that provided by Alfred Gell in his (1998) *Art and Agency*. It's a pity Gell wasn't more explicit about the psychological models he was using.

What I find useful in this other kind of approach, but currently lacking in the epidemiological programme, is that it accounts for modes of cultural transmission more complex than attraction. The processes I am trying to get to grips with in the book are of this more complex sort. At the simplest level, what triggers them is the encounter between a given social formation (read 'host organism') and an encroaching foreign agent. As with the effect of antibodies on pathogens, the encounter sets in motion a process of cultural hyper-creativity (or efflorescence) in the host, which is designed specifically to forestall further interactions.

In such cases the function of copying, or transmission, is to neutralise, deflect, and ultimately to repel. Over time such a process – if successful – leads, not to the blurring of 'self' and 'non-self', but to cultural closure and canon formation; not to the convergence of cultural forms upon a common denominator, but to their divergence or 'schismogenesis', to use Bateson's term. It is linked therefore to non-trivial aspects of culture change, such as ethno-genesis and identity formation. That is why the episodes of transmission described in my book align with those 'proto' or 'archaic' periods, which precede the formation of cultural canons (and see also Jeremy Tanner's earlier post, and my response).

Some such process is what actually links the micro- and macro-scales of my analysis most closely. My ‘protective mode’ of transmission for composite figures is not simply about attraction. It is about the making of images as a direct response to threats against household and person, in which (again as with antibodies) we see pre-emptive attacks being launched on the demonic carriers of illness and misfortune. They are recognised and depicted, in order to be neutralised (and cf. Bruce Kapferer’s discussion of exorcism and the aesthetics of healing in his [1983] *A Celebration of Demons*). My ‘transformative mode’ is basically the same sort of process writ large. Status accrues to those groups in society who become “expert immunologists”, claiming to maintain stable relations with an encroaching outside world that is ultimately presented as chaotic and threatening.

The epidemiological approach, I feel, has helped to account for the forms taken by these responses and their remarkable stability over time and space. But I don’t feel that it yet provides all the conceptual tools needed to describe or account for their modes of transmission, or their cultural effects. The reason for this, I suggest, is precisely that the epidemiological approach has so far focused almost exclusively on processes and forces of cultural attraction. If so, and however neat the balance of psychological and environmental factors involved, then I stand by my statement that, to fulfil its aim of providing a naturalistic model of culture, the approach requires some modification.

THE TALE OF THE THREE-HEADED SNAIL

By Olivier Morin



David Wengrow opens his fascinating book with a conjecture that he attributes to cultural epidemiologists (not without good reasons): composite animals are “minimally counter-intuitive” and thus, some monsters enjoy a supplement of cultural success by virtue of being composites (and not because they are, say, big animals, or predators). By the last pages of *The Origins of Monsters*, though, this hypothesis is all but jettisoned. There might be something universally appealing about composites, but that ingredient, by itself, is utterly insufficient to explain the vogue of Bronze Age monsters that he documents (and the absence of such a vogue in earlier periods). I agree, and I would go even further: it is entirely possible that composites are nothing special, and that the psychological hypothesis Wengrow started with is not just insufficient, but false. This does not mean we should despair of psychological explanations, though: it just means we had the wrong hypothesis in mind.

More specifically, I'll discuss two ideas that loom large in *The Origins of Monsters*:

(i) Composites are appealing. Representations that fuse together different animal parts into one single animal shape, like the chimaera or the griffin, are cognitively special, and culturally successful for that reason. The odds of successful diffusion are better for composites than they would be for most other representations of animals (monstrous or not).

(ii) Composites are “minimally counter-intuitive.” What makes composites appealing is “minimal counter-intuitiveness,” as theorised in particular by Pascal Boyer. Composite animals are weird, but they also respect some basic and widespread intuitions about animal shapes. Because of this, composites are rapidly and easily perceived, memorised, or (if needed) reconstructed; but composites also thwart these same intuitions in a way that makes them seem informative and worth paying attention to.

This comment will explain why I doubt both views, starting with the second: there may not be anything “minimally counter-intuitive” about animals combining features of different species. I take the phrase “minimally counter-intuitive” in its usual technical sense: a belief is counter-intuitive if it violates one (not two) elements of an early-developing, implicit, cross-culturally robust naive theory. In the case at hand, a counter-intuitive animal would violate one element of “folkbiology.”

We should not too readily assume that monsters are counter-intuitive in that sense. After all, babies are not born expecting to see horses, cows and eagles in the world, as opposed to unicorns and griffins. Few people would say a complete knowledge of the particular species or taxa that exist on Earth today is part of our species’ early-developing, cross-culturally robust “folkbiology.” Neither is our intuitive zoology made of implicit intuitions, based on deep, unconscious principles we cannot easily spell out. Quite the contrary.

If this is true, then many composite monsters aren’t counter-intuitive in the proper technical sense. Weird, unusual, unheimlich, yes—but not in a way that we can use to link monsters with the cognitive anthropology literature on religious beliefs. More generally, folkbiology (or what we know of it) is not terribly useful in predicting the appeal of imaginary animals. Violations of folkbiology don’t usually make good monsters. Composite animals do not, for instance, contravene the inheritance principle, which states that any creature bequeathes an invisible and powerful “essence” to their offspring, and few if any successful monsters do so. Lastly, there are much better ways to explain what draws our minds to monsters (composite or not), starting with the cognitive appeal of big, dangerous animals.

Counter-intuitive, or just weird?

As an excellent paper by Benjamin Purzycki and Aiyana Willard (2015) recently showed, “minimal counter-intuitiveness” is becoming a murky concept. Originally, a counter-intuitive belief or concept had to violate a “deep” (early developing, cross-culturally robust, implicit) intuition about a particular ontological domain. Merely surprising or unusual beliefs would not do. Why?

Before the “minimal counter-intuitiveness” idea took hold, earlier authors had tried to show that weird, atypical or uncommon ideas are better remembered or communicated—the “bizarreness bias.”

It seems clear from Purzycki and Willard's account that human memory does not, in fact, have a "bizarreness bias": weird content is not better remembered than prototypical content. In the aesthetic sphere, I don't know that there is a body of work showing that "minimally weird images" are better remembered and more likely to be transmitted, or that specifies the optimal dose of weirdness in a clear, analytic fashion. There is intriguing work (in robotics, for instance) on the Freudian "feeling of the uncanny" that a certain degree of admixture between familiar and unfamiliar features elicits. Yet (as far as I know), no one claims that uncanny images are particularly successful (the opposite would be just as likely).

The "minimally counter-intuitive beliefs" research program is successful precisely because it proposes something different—a theory grounded in cutting-edge developmental psychology and comparative ethnography. Minimally counter-intuitive beliefs or concepts are supposed to contradict one and only one of these fundamental (i.e., cross-culturally robust, implicit, and often early developing) intuitions. Yet, as Purzycki notes, it can be hard to be consistent with this commitment, and the view that "counter-intuitive" simply means "weird" sometimes creeps back. Monsters seem to be a case in point.

Folkbiology is not against composites (or monsters)

A counter-intuitive belief should not merely challenge common expectations—like the belief that pigs can't fly. It should go against deeper intuitions. What could these intuitions be in the case of animals? Intuitive "folkbiology" has three principles that could be relevant here: inheritance (dogs don't breed cats), a belief in more or less isolated and fixed species (sometimes deemed essentialist), expectations regarding a few dangerous taxa (like spiders and snakes), and (possibly) a sharp distinction between humans and other animals.

Inheritance. Young children, as we know since Frank Keil and Susan Gelman's work (Keil 1992) have strong implicit intuitions regarding biological inheritance: Dogs don't breed cats. Real and imaginary animals may change shape (of course), but they bequeath a constant, species-specific biological legacy to their offspring. This legacy is understood as a constant and immutable gift, even though it cannot be perceived to the naked eye: a raccoon's offspring is a raccoon, even when disguised as a cat. Do composites and other monsters challenge these intuitions? Arguably not. Unicorns breed unicorns (but horses don't), and dragons breed dragons. Composite monsters either don't have parents, or they have composite parents, or their parents each belong to one of the species that make up the composite (like the Minotaur, descended from a bull and a human mother). The only exception that I can think of is monsters that can't reproduce but have to be "created" in some way (like vampires)—but they don't really contradict the inheritance principle (since they don't reproduce).

Species (possibly essentialised). As Douglas Medin and Scott Atran have shown, the classification of living things into species-like categories appears universal (Medin and Atran 1999). Folkbiological categories are species-like, because they include rules like the inheritance principle. Some have ar-

gued that part of people's intuitions about species is that they cannot change through time (a belief that would explain many a form of resistance to Darwinism). Whatever the case may be, composites do not in any way challenge these intuitions. Folk biology does not, of course, say that species X cannot possess any feature also possessed by species Y—otherwise the fact that horses have legs like humans, or that bats have bird-like wings, would be deeply counter-intuitive; endless songs and tales would be composed about lichens; platypi, marsupial dogs, mole-rats, would be box office regulars on a par with sharks and dinosaurs.

Fear of snakes and spiders. Folkbiology is not entirely incompetent about the shape of animals, and even of specific species. For instance, we share with other primates a vivid and partly innate repulsion for snakes and spiders-like shapes: aversion to these things is more readily learnt. How taxonomically precise these expectations are is still quite unclear. Here again, most cultures have not come up with monsters that go against this deep intuition—no sweet snakes, no cuddly spiders.

Human uniqueness. Some could argue that a “human exception” is another cross-cultural constant of folkbiology: humans are thought to occupy a distinct ontological niche. One consequence is that properties possessed by most other animals will not be projected on to humans without unusual and specific difficulties (Carey 1987). Michael Kelly and Frank Keil's remarkable analysis of metamorphoses in Western folk tales (Kelly and Keil 1985) seems to confirm that humans occupy a distinct ontological domains there. Human-animal composites (like Taweret, the hippopotamus-headed protector) clearly challenge this boundary, as Wengrow does not fail to note. It would seem, thus, that some (but not all) composites do challenge one deep folk-biological intuition.

I see two reasons to disagree. First, it is doubtful whether human uniqueness is a universal intuition. Bang, Medin and Atran (2007) found that Carey's result, obtained with urban USA children, did not generalise to Menominee children. (I suspect that Wengrow, who is clearly influenced by recent anthropological trends coming from Descola or Viveiros de Castro's work, would not claim that a belief in human uniqueness enters into a universal folk biology.) Second, even if we accept that all humans share an intuitive belief in their own uniqueness, the frequency of human/animal composites could be explained in many other ways. It is true that humans are more likely than (say) ants to enter into composite creatures, but so are horses or lions. The human form is interesting for many reasons, and we expect human bodies to be extremely prevalent in all kinds of visual cultures (unless actively banned). Monsters should be no exception.

We have to conclude that, of all the principles of folkbiology that we can plausibly identify, composite animals violate only the most dubious one (and only some composites do so). Overall, composite animals do not counter deep folk-biological intuitions (and neither, by the way, do giants, pygmies or mutant animals). It is no coincidence, then, that Wengrow's account is most persuasive when he stresses all the intuitive features of composites—most strikingly the conservation of the vertebrate body plan. This phenomenon had been shown in a psychological study by Thomas Ward (1994), but to see it fleshed out in impeccable archaeological detail is a delight.

Are composite creatures particularly appealing—and as compared to what?

If many composites are not counter-intuitive, then why do composites spread? Well, in fact, the view that composites as such enjoy a strong and specific cultural advantage is hard to assess. The composite creatures that Wengrow shows flooding Eurasian visual culture in the Bronze Age could, in fact, owe their success to many different factors, and we won't know which ones exactly without comparing the success of several types of cultural items, ideally in a systematic and quantitative way. Cultural success can't be properly assessed meaningfully without a baseline: What should we compare representations of composite animals to? Representations of plant or non-animals? Of normal animals? Of mutants? Of outliers? Of truly counter-intuitive creatures? I don't know. A wide range of options are on the table and have been defended, including the view that prototypical animals are just as cognitively attractive as monsters (Sperber 1975).

This, in fact, is what *The Origins of Monsters* ends up demonstrating, and quite convincingly. His main conclusion seems to be that the success of composites is quite contingent on particular social contexts that have little to do with humans' evolved psychology, which plays the role of an enabling factor, at best. One thing that makes composites attractive is the variety of forms they permit, which comes in handy when one needs to generate a great variety of animal forms, for heraldry or for marketing, in what Wengrow called elsewhere "cultures of commodity branding." Nothing seems to connect this particular coincidence to a universal and specific bias for composites, and I don't really know what psychological theory could predict such a bias.

What would it take to show that composites, as such, enjoy some added intrinsic appeal? Ideally, a perfect dataset should show composites to be successful, not only compared non-animal shapes (rocks, plants, etc.), but also compared to other animal shapes, and possibly to other kinds of imaginary animals, of which there are many. Some, like botched reconstructions of dinosaurs, dragons, or ogres, are only partly imaginary, being based on (more or less badly misinterpreted) fossil evidence. They do not clearly qualify as composites. Animal outliers, too, are the stuff of legends, without being composites (the tiniest dog in the world, the tallest man, the biggest boa, the Gévaudan beast). Mutants (cyclops, eight-armed humans, etc.) are a hit too. So are over-sized or miniaturised animals (e.g., giant spiders). (Giant spiders, interestingly, are physically impossible but perfectly intuitive.) I would not bet that composites are more successful than other imaginary creatures (T-Rex still beat griffins in the toy store). Specifically, it seems plausible that the appeal of composites, most of which also happen to be big, predatory animals, cannot be distinguished from the appeal of other big predators—at least not with the kind of data we are dealing with here.

Two simple (yet untested) ideas

I closed David Wengrow's book with the feeling that the cultural epidemiology of animal iconography now appears to be a promising subfield, thanks to this book's contribution; but also that we haven't

even started to prove the simplest things in the area. I'll take as examples two simple conjectures that would, I think, seem obvious to many people on this site.

The appeal of animal shapes. We have some psychological reasons to expect animals (as opposed to plants or minerals) to enjoy a strong cognitive appeal, which should translate in higher cultural prevalence. Joshua New's experimental work suggests as much (New, Cosmides, and Tooby 2007). A cursory glance at patterns of visual culture worldwide seems to confirm this; even the exceptions are revealing: most Islamic visual arts ban animal representations—but, precisely, they do so only because of an explicit ban—and one that is not easily forced upon the public. (As is well known, Islamic aniconism was not always respected, and most other monotheistic traditions found ways to tweak the Second Commandment in a way that allowed them to ban the Golden Calf and still carve it.)

How nice it would be, though, to have this intuition confirmed in a systematic, comparative and quantitative fashion! It would allow us, for a start, to address the doubts of most anthropologists (some of whom would probably be dismissive of the notion of a universal and evolved preference for animal shapes). It would also address one nagging doubt that I had while reading Wengrow's book: Perhaps the success of composite monsters could simply be explained by the fact that composite animals are attractive as animals, their composite character being quite irrelevant.

The appeal of predators and dangerous animals. As Karolina Prochownik rightly stresses in her comment, there is at least one obvious alternative to the “minimally counter-intuitive composites” account. Most successful monsters (composite or not) are also predators, and most composites are likely to include snakes, hippopotamus, lions, scorpions, etc., not only in Wengrow's books but also in the myths and visual imagery of many cultures. (Do you know the tale of the three-headed snail? Precisely.)

Evolutionary psychologists have much to say about the hold that predators have on our imagination (Barrett 2015), and from this point of view, it would seem that an epidemiology of monsters won't be doing its job unless it explains the obvious connection between monsters and fangs, horns, stings, and claws.

Yet, here again, the simplest things remain to be proven. First of all, we'd need a baseline to compare the prevalence of predators to: lions and hippos make good monsters, but so do horses and crows. Next, we'd need to go quantitative (unless we get stuck in endless counterexample-mongering). Lastly, we'd need to rule out some alternatives to the evolutionary account. As Wengrow stresses when discussing the apotropaic role of monsters (like Taweret or Medusa), predatory features may owe their success to the perceived need to ward off evil spirits with scary animals. More generally, as Erhard Schüttzel notes, we should not discount the terrifying effects of some monstrous imagery on some of its human spectators (laughable though it may seem to the more blasé consumers of horrible images that we are).

Whatever else makes composites and other monsters appealing, “minimal counter-intuitiveness” seems to play a small part at best. Should this lead us to give up the search for a psychologically grounded epidemiology of monsters? Not so fast. Numerous appealing hypotheses—the appeal of predators, the appeal of animal forms—have not even been tried yet. I take *The Origins of Monsters* as an invitation to get to work, hand in hand with archaeologists, anthropologists and art historians.

References

- Bang, Megan, Douglas L. Medin, and Scott Atran. 2007. “Cultural Mosaics and Mental Models of Nature.” *Proceedings of the National Academy of Sciences* 104 (35): 13868–74. doi:10.1073/pnas.0706627104.
- Barrett, H. Clark. 2015. “Adaptations to Predators and Prey.” In *The Handbook of Evolutionary Psychology*, edited by David M. Buss, 200–223. John Wiley & Sons, Inc. <http://onlinelibrary.wiley.com/doi/10.1002/9780470939376.ch7/summary>.
- Carey, Susan. 1987. *Conceptual Change In Childhood (Learning, Development, and Conceptual Change)*. MIT Press.
- Keil, Frank C. 1992. *Concepts, Kinds, and Cognitive Development*. Cambridge, Mass.: A Bradford Book.
- Kelly, Michael H., and Frank C. Keil. 1985. “The More Things Change...: Metamorphoses and Conceptual Structure*.” *Cognitive Science* 9 (4): 403–16. doi:10.1207/s15516709cog0904_2.
- Medin, Douglas L., and Scott Atran, eds. 1999. *Folkbiology*. Cambridge, Mass.: The MIT Press.
- New, Joshua, Leda Cosmides, and John Tooby. 2007. “Category-Specific Attention for Animals Reflects Ancestral Priorities, Not Expertise.” *PNAS* 104 (42): 16598–603. doi:10.1073/pnas.0703913104.
- Purzycki, Benjamin Grant, and Aiyana K. Willard. 2015. “MCI Theory: A Critical Discussion.” *Religion, Brain & Behavior* 0 (0): 1–42. doi:10.1080/2153599X.2015.1024915.
- Sperber, Dan. 1996 (1975). “Why Are Perfect Animals, Hybrids, and Monsters Food for Symbolic Thought?” *Method & Theory in the Study of Religion* 8 (2): 143–69. (1996 translation of “Pourquoi les animaux parfaits, les hybrides et les monstres sont-ils bons à penser symboliquement?” *L’Homme*, XV (2) (1975) 5-24.)
- Ward, T. B. 1994. “Structured Imagination: The Role of Category Structure in Exemplar Generation.” *Cognitive Psychology* 27 (1): 1–40. doi:10.1006/cogp.1994.1010.

Comments

David Wengrow

Regarding the use of quantitative methods to analyze images in ancient art, very few people have tried this in a serious way. One of the few I'm aware of is my UCL colleague Peter Schauer (for instance: 'Quantifying the importance of motifs on Attic figure-painted pottery', in E. Slingerhand and M. Collard (eds.) 2012. 'Creating consilience: integrating the sciences and humanities'. OUP). I'm all in favor of it as a way of sharpening the debate, but am also conscious that the resulting statistics should not be over-reified as a complete (or even near-complete) picture of cultural transmission, when clearly some of the most important media of transmission, like textiles and metalwork, are mostly lost (and see my comments on Jeremy Tanner's post).

It's funny how the term "folk" biology has come to mean commonsense, when the knowledge of the "folk" in general used to be seen as quite the opposite.

So: unicorns beget unicorns, and dragons beget dragons? Maybe, Olivier. But in whose folklore?

Mine is not a book about comparative mythology, and this sort of information is simply not available for about 95% of the material I cover. But were we to go down that path the data would immediately become so much richer and more complex than this. My impression is that fantastic beasts have for the most part equally fantastic genealogies (the Chimera is sometimes a sibling of Cerebus, and parent of Sphinx). Their kinship relations, as Erhard discussed, are often highly counter-intuitive, and hence aligned with the origins of gods, stranger kings, and other archetypal violators of human inheritance patterns.

There is so much work by classicists, ancient historians, and anthropologists on that sort of thing. It is absolutely right, of course, to say that much of this existing work is based on outmoded psychological assumptions. But then it also has to be pointed out that, in their comparative scope and rigorous control over cultural data, the structural analysts of myth (Detienne, Vernant, Lévi-Strauss, etc.) and those who followed them set an empirical benchmark, which should surely also apply to the new generation of cultural-cognitive studies.

To move forward the debate on culture and cognition I think we should try and stick reasonably close to cultural materials like these – i.e. cultural facts that are ubiquitous, well documented, and have historical distributions that can be mapped and compared with some degree of accuracy.

As Olivier points out, the fate of the three-headed snail is immediately to be squished. I'm afraid it will be the same with his dragons and unicorns ...

Olivier Morin

My (biased) impression is that you singled out one blunder at the expense of the broader question that was asked in this post (and in Mathieu's too): What is it exactly that makes composites counter-intuitive? We agree that monsters have fantastical properties over and above the fact that they are composites—of course. But this does not show how the fact of being a composite animal is, in and of itself, minimally counter-intuitive—which to me is the big issue.

David Wengrow

My reply is the same as it's always been: it's the arrangement of blatantly incongruous body parts in an anatomically correct fashion that makes images of composites (minimally) counter-intuitive on the plane of visual perception. The images used as headers for posts in this forum are all brilliant examples, and everyone I show the site to immediately comments on how striking and interesting they are. I would very much like to discuss with Mathieu the broader points he raises, but I can't see how that's possible if we are reading Severi's book in such apparently different ways. That's why I've asked him for clarification on specific points about the Amazonian material and Severi's interpretation of it, and am still hoping for a response.

THE SCOPE AND FLAVOURS OF CULTURAL ATTRACTION THEORY

By Danis Tatone



Empirical tests of theories of cultural evolution are (relatively) rare. Those using rigorous archeological datasets, even rarer. These reasons alone suffice to make *The Origins of Monsters* a must-read for anyone interested in exploring the interface between cognition, societal infrastructures, and the spread (and design) of cultural items.

But let's get straight to the heart of the matter. David Wengrow's main argument rests on two basic assumptions:

1. Composites (i.e., fictional beings produced by recombining the anatomical subunits of taxonomically different beings) eloquently typify the balance of intuitive and counter-intuitive elements, which, following Boyer and Sperber should make cultural items attention-arresting and memorable;
2. The epidemiological model should attribute to cultural items exhibiting such properties a selective advantage in transmission and diffusion, supporting the prediction that they should be, according to Boyer (2000) "both relatively stable within a group and recurrent among different groups".

To simplify, according to Wengrow, if composites do indeed exhibit the right combination of “rich intuitive base” and “limited series of violation of intuitive theories” necessary for the successful transmission of a cultural item (Boyer, 1994), we may expect a (more or less) uniform distribution of composites across geographical areas and ages, from the earliest combinatorial experimentations in human visual culture.

Gathering an impressive wealth of archeological data, Wengrow argues that it is far from being the case. Composites are in fact virtually non-existent before 4000 BC, a period when the institutional foundations of early urban societies were finally being laid down. Hence, Wengrow’s question: “if the popularity of minimally counterintuitive images is to be explained by their core cultural content and its appeal to universal cognitive biases, then why did composite figures fail so spectacularly to “catch on” across the many millennia of innovation in visual culture that precede the onset of urban life?” (p. 50).

This apparent paradox exists only insofar as assumptions (1) and (2) are satisfied. As for the first assumption, Mathieu Charbonneau already forcefully argued that this may in fact not hold ground: composites, to put it simply, may not have the “shock value” required to make them cognitive attractors, in the sense epitomized by Boyer’s religious entities.

As for the second assumption, on the other hand, it seems to entail that, for an attractor to qualify as such, it should be uniformly spread in the archeological record (this being a signature of the selective advantage in diffusion that the “attractive” cultural item enjoyed). This strikes us as an excessively demanding diagnostic criterion of attraction, for reasons that we shall unpack below.

Recent developments in cultural attraction theory have distinguished, amongst others, two basic types of attractors: cognitive and motivational. As defined in Olivier Morin’s book:

“A tradition has cognitive appeal when it fits our information-processing capacities. This makes it easy to store and reproduce. It is motivationally appealing when it taps into emotional or decisional mechanisms that make us want to use or transmit it. The first kind of appeal has to do with the ease of communicating, recalling and reproducing it. The second kind bears on whether or not we want to do all these things.” (How Traditions Live and Die, p. 148)

Importantly, both kinds of attraction may be more or less limited in scope: linguistic rules (an instance of cognitive attraction), which today’s speaker find intuitive and easy to learn, may not have appeared so to previous generations; similarly, traditions which successfully spread through a population due to being promoted by political authorities would have no particular force among people outside the influence area of those authorities.

In sum, leaving aside whether composites qualify as instances of cognitive or motivational attractors, there is nothing inconsistent in principle in these cultural items making a late and geographically sketchy appearance in the archeological record. The association between mechanical techniques of

image production and the spread of composites, which Wengrow considers “puzzling”, insofar as it would imply “a superfluous cultural prosthesis to cognitive predispositions that are already biased towards the reception of such images” (p. 80), is in fact fully compatible with the possibility of composites being local attractors.

This, however, still leaves open the question as to what type of focal points for cultural transmission the emergence of large-scale societies (henceforth, LSS) provided.

We shall first entertain the hypothesis that LSS supported the proliferation of composites by making them cognitively attractive. One may argue that the transition to LSS societies, which — as Wengrow suggests (p. 68) — was accompanied by the cultivation of new technologies based on modular principles of assembly, could have raised the salience of those cultural items more fittingly reflecting these compositional principles. In this sense, composites would become cognitively attractive (i.e., easier to remember and reproduce), because particularly suited to means of artefact production based on compositionality.

It is important to emphasize that in this scenario LSS does not provide a template for the ideation of composites, but rather for their realization. This clarification is essential to safeguard our (speculative) account from far more radical interpretations of the role that LSS played in explaining the sudden appearance of composites. Wengrow himself seems to vacillate in several occasions throughout the book (as well as in the *précis*) between a lean account, akin to the one we just sketched, where LSS “merely” supplied the institutional and technological means for the proliferation of composites, and a thicker one, according to which the institutions of early urban life stimulated a genuinely new type of intuitive knowledge within which the counterfactual properties of composites were grounded. To put it with Wengrow, the early urban life:

“fostered the cultivation of an otherwise latent mode of perception that confronts the world not as we usually encounter it — composed of unique and sentient totalities — but as a realm of divisible subjects, each comprising a multitude of fissionable and re-combinable parts.” (p. 110)

While Wengrow is careful not to suggest that navigating the institutional environs of early LSS may have directly honed in the combinatorial skills required for the production of composites (an even more far-fetched claim, no doubt), the statement above nonetheless suggest that the “urban experience” may have awakened a dormant compositional cognitive style, which he find typified in James Scott’s idea of “seeing like a state”. Behind this evocative simile, however, we fail to understand which specific relational schemata or conceptual frame the gradual familiarisation with the centralised bureaucracy typical of LSS could have supplied.

Long predating the advent of modes of large-scale social organisations, the technological life of pre-state societies in fact already presented a number of adaptive challenges that may have solicited the

emergence of compositional abilities suspiciously akin to the “latent mode of perception” above described, as the production of composite tools documented in several hunter-gatherer groups attests. A proponent of the above account would then be hard-pressed to define just how specific, and how different from the cognitive routines of ‘basic’ analogical and compositional reasoning, was the representational mode that LSS made accessible.

This is especially the case, if we consider that (a) not all LSS were similarly characterized by the adoption of composites, and (b) performative arts — which may have been a feature of prehistoric performances (a possibility that Wengrow hints at in the précis) — prominently feature body-wear that is strongly reminiscent of the principles of anatomical re-shuffling underpinning composites.

These two considerations alone suffice to doubt the claim that LSS solicited the adoption of a new conceptual schema only as a function of some (underspecified) conceptual matching between this “latent mode of perception” and the principles of inter-relatedness, embodied in the institutional settings of LSS, that are supposed to reflect it. These two signature limits of composite distribution, on the other hand, are perfectly compatible with the possibility that LSS may have provided instead a set of political and socio-economical motives for the transmission of these cultural items.

According to this second account, based on the idea of motivational attraction, LSS boosted the attractiveness of composites via the provision of an increased motivation to ‘use’ them. Consistently with this possibility, Wengrow charted out three modes of transmission (transformative, integrative, and protective) intended to explain the diffusion of composites on the basis of the societal and political function that these should have served. Each of these modes, Wengrow suggests, is associated with environments of heightened risk where failure to properly negotiate boundaries would be often catastrophic:

“Within the transformative mode, status accrues to those groups within society who can establish stable relations with an encroaching outside world. The integrative mode is associated with the tense theatre of court diplomacy, with its fragile alliances and fateful transgressions. And the protective mode [...] is a direct response to threats against the household.” (p. 106)

Regardless of how accurately this tripartite classification cuts at the joints of the functional spectrum of composite use, and how legitimate is to equate “modes of transmission” to functionalist descriptions (since the former may encompass mechanisms having nothing to do with the ascribed function of a target cultural item), this account seems, unlike the previous, immune to the perils of radicalising the role of LSS. Far from providing “modes of practical and abstract reasoning”, LSS here supply a suite of social and political reasons (among which we would tentatively include the “branding” of manufacture sources for products destined to long-distance trade) that should promote the adoption and diffusion of composites in large-scale formations.

To conclude, despite Wengrow’s book represents a fresh and rigorous attempt to put the epidemiological framework to test with archeological evidence, we fear that the author’s efforts may have been

partly vitiated due to a set of assumptions about cultural attraction (such as its general scope and exclusively cognitive nature), which we argued as being unwarranted. As emphasized already, cultural attractors are in fact compatible with (a) local, historical phenomena, and (b) strictly motivational factors. Admittedly, however, the lack of clear-cut diagnostic criteria for cultural attraction, such as the uniform diachronic distribution of cultural items, may run the risk of making this notion dangerously close to unfalsifiable on the basis of archeological data alone.

Comments

Erhard Schüttpelz

(1.) After reading the whole series of the debate and trying to make sense of it, I cannot help but wonder what the most obvious gap in this debate means, and how it can be filled, if filled it should be. “Cognition” seems to be about universal cognitive schemata, and if their epistemology has the potential to explain an “epidemiology”, then it would have to be a uniform epidemiology of some sorts. On the other hand, “Culture” is nothing but history, and if we follow historical epidemiologies of (seemingly) basic cognitive schemata, then we have to refer to contingent cultural and historical factors, as David has done in his book. And all in all, his version of epidemiology (or “diffusion”) has not been challenged, not even by counter-examples, because f.i. the counter-examples from the Amazon and the Americas turn out to be different kinds of monsters, unlike the “composite monsters” that his book is about, and unlike the trajectories of Eurasian composite monsters. Thus, all in all, there remains a huge gap between the arguments that have been raised from the “cognitive” side, and the historical trajectories from the “cultural” (or historical) side. This gap, of course, will be interpreted and has been interpreted differently from both sides: that cognitive schemata alone are not enough, that they remain “underdetermined” in historical respects; vs. that historical trajectories are only offer poor and insufficient evidence for universal cognitive schemata, not proving much, because the historical and especially the archeological sources are so contingent and incomplete (f.i., as David writes: 95% of all images cannot be linked to any written oder linguistic corpus, and to no known mythology or ritual tradition). Thus, the exchange of cognitive hypotheses and their experimental evidence on the one hand, and of historical and archeological data and their interpretation on the other, gives rise to mutual frustration: what is there to learn, exactly?

(2.) Speculative as it may sound, I would nevertheless like to propose that there is a third way of dealing with the gap between what the “cognition” camp claims to be cognitive, and what historians and archeologists claim to be “cultural” and culturally contingent. The gap itself may be significant to describe what is aesthetically irritating or appealing about the object in the middle, i.e. the “composite monster”. This time, let me quote one of David’s sources, the sinologist Bagley writing about the big difference between archaic Chinese monsters, and the “composite monsters” that were adopted during the Warring States era:

“In Egypt and the Near East, imaginary animals – chimaeras and sphinxes and lion-griffins – are composites of real animals. ... If we look up the word ‘sphinx’ in a dictionary, we will read that a sphinx is a lion with a woman’s head and a bird’s wings; that is essentially a recipe for making a sphinx. Similarly if we look up ‘griffin’ or ‘chimaera’ or ‘unicorn’ we will find a description that enables us to visualize the creature. But what would a dictionary entry for the Erligang dragon or taotie say? It could not give us a description that would enable us to visualize one.” (R.W. Bagley, Ornament, representation, aand imaginary animals in Bronze Age China, *Arts Asiatiques* 61/2006, p. 21).

Comparing the composite monsters with their predecessors and alternatives, it's this possible verbal reduction that makes the composites a "recipe", and their creation only the variation of an all-encompassing recipe that has been exhaustively analyzed by Mathieu Charbonneaux in his Book Club contribution. Indeed, there is nothing else in these monsters but a "minimally counter-intuitive" or maybe even a most convincingly "intuitive" building plan, and the modular combinations of limbs and extremities. The part-whole principle – the one and only principle – of this building plan is so simple that children at an early age may comprehend it, and that – let's face it – would be a very good reason for the grown-up artisans and artists of many and most cultures to avoid the "slavish" application of that principle. I.e. they would use that principle, but only in combinations with other and more imaginative principles of imagining and representing monsters. (As both Carlo Severi and Dimitri Karadimas have shown for the Amazon and the Americas, i.e. there are some substitutions of the "composite" kind and some of the monsters may look similar to Eurasian composites, but there are no reductions to their principle and all kinds of idiosyncratic gestalt-switches involved, and thus, the Amerindian principles of "thinking through monsters" are much more sophisticated than any possible reduction to a composite monster with its restricted vocabulary of parts and substitutions. And especially in the writings of D. Karadimas, we find some really mind-boggling exercises of imagining and imaging Amazonian monsters.)

(3.) This leaves us, I hope to demonstrate, with a third option for the gap between the universal cognitive scheme (of intuitive or explicit "folk biology", if that is the best term), and the contingent Eurasian trajectories (with their highly specific settings of power relationships and technical innovations). Yes, the cognitive scheme underlying "composite monsters" may be universal and "intuitively" grasped, from an early age on, but that (exactly that) would be the reason to avoid any aesthetic reduction to that scheme and its equally verbalist and realist bias. After all, f.i. from an Amazonian or Melanesian or Archaic Chinese point of view such a reduction would be "childish", or it would be "barbarian", or it would be aesthetically silly and reductive: imagination stripped of nearly everything that imagination has to offer, and made even more realistic in its reduction – adding aesthetic injury to aesthetic insult, so to speak. And because this reduction did indeed happen in China and in the West, it could mean that the reduction was a "barbarian" thing indeed, i.e. meant to represent a "barbarian" outside world, a world of "terribles simplifacteurs" or rather, "terribles simplifications". After all, it happened in China during the Warring States period, with references to the peoples of the Steppe, and adopting patterns from the Western Barbarians (cf. Bagley); and it happened in the Near East in the elites of violence and trading and urbanization, adopting patterns from the outside elites of far-distance trading and violence. It may sound polemical, but that's not my intention:

(A.) Yes, a universal cognitive scheme is something so easily recognized that it may not be aesthetically pleasing in its "pure form"; and if it is applied in its "pure form" only,

(B.) it will be recognized as an explicit "reduction". If that aesthetic reduction takes hold and makes sense to a stable group of artists and their patrons, then this possibility is likely to emerge because parts of the experienced world have been grasped as some sort of powerful "reduction", i.e. manifesting the reduction to a cognitive scheme reveals the potential of representing a powerful "barbarian" reduction,

(C.) which gives this emerging art genre the big chance to integrate the experience of figurative "reduction" and transform it into something new, in this case into the possibilities of terrible and apotropaic beings, and thus, by having crossed the uncanny gap between cognition and culture, into a new form of sophistication.