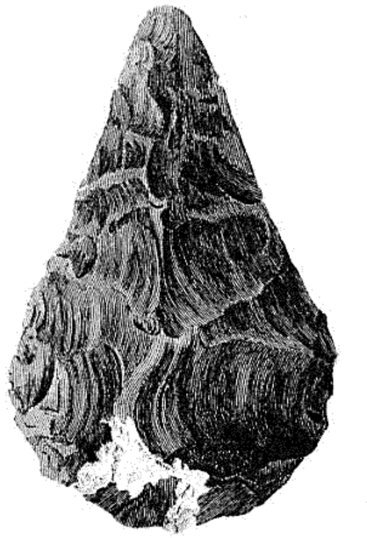
Evidence, Inference and Human Evolution: Essays in the Philosophy of Cognitive Archaeology



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A thesis submitted for the degree of Doctor of Philosophy of The Australian National University, 28th of February 2022.

STATEMENT

The Introduction, Chapter 1, Chapter 5, Chapter 6 and the Conclusion are solely the work of the author of this thesis.

Chapter 2 and chapter 3 were co-authored with Anton Killin. The author of this thesis is responsible for 50% of the research involved in these chapters (a statement of contribution can be found at the end of these chapters).

Chapter 4 was co-authored with Rachael L. Brown. The author of this thesis is responsible for 60% of the research involved in this chapter (a statement of contribution can be found at the end of this chapter).

No part of this thesis has previously been submitted for any degree, or is currently being submitted for any other degree. To the best of my knowledge, any help received in preparing this thesis, and all sources used, have been duly acknowledged.

Ross Pain
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Title page image: The first published picture of a hand axe, drawn by John Frere and published in Archaeologia, Volume 13, in the year 1800 (image released to the public domain).

ABSTRACT

The promise of cognitive archaeology is considerable: the discipline can potentially outline a chronology of human cognitive evolution, and offer insights into the dynamics involved. However, the field faces two major methodological hurdles. First, it is a historical science; one that requires running inferences from the archaeological record to the cognitive capacities of our hominin ancestors. Second, it requires synthesising work from a disparate range of disciplines, including archaeology, cognitive science, and evolutionary biology.

The overall goal of this thesis is to demonstrate how philosophers of biology can help meet these challenges using the philosophical tools of *analysis* and *synthesis*. Chapters 1-3 analyse various inferential strategies employed by cognitive archaeologists, and identify problems for those strategies. These problems relate to theoretical diversity, cultural variation, and the explanation of historical phenomena with multiple causal inputs. Chapters 1-3 also propose solutions to these problems. Together, they contribute to the growing literature aimed at developing a reliable inferential framework for cognitive archaeology. Chapters 4-6 synthesise work from archaeology, cognitive science and evolutionary biology in order to produce first-order claims about the evolution of particular cognitive capacities. These capacities include technical cognition, language, and theory of mind. Chapters 4-6 align with a growing trend amongst philosophers of biology for unifying theoretical work across disciplinary boundaries.

Producing a robust science of human evolution is a daunting challenge. In this thesis, I aim to highlight the role both cognitive archaeology and philosophy of biology can play in addressing this challenge. I demonstrate that interdisciplinary work is essential to understanding our evolutionary origins.

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PART 2: SYNTHESIS

A note on the layout of the thesis:

This thesis is by publication. Chapters are hence formatted as they were published, and are individually referenced. A list of references is found at the end of each chapter.

INTRODUCTION

Crossing the Creek and Hanging Together

"The aim of philosophy, abstractly formulated, is to understand how things in the broadest possible sense of the term hang together in the broadest possible sense of the term."

- Wilfrid Sellars (1962, 35)

1. Prelude

I have two tasks in this introduction. The first is to outline the approach to philosophical work that guided my PhD project (section 2). The second is to provide an overview of each of the chapters that make up the thesis (section 3).

Regarding the first task, I will use a discussion of Sellars' take on the aim of philosophy, tersely surmised above, to outline a metaphilosophy; that is, a methodology for approaching philosophical research. That methodology also draws on Sellars: to paraphrase, we achieve the above aim by keeping one eye on the scientific detail and one eye on the big philosophical picture.[1] To some, it might be confusing why a philosopher would pay close attention to scientific detail. When describing our work, philosophers of biology—especially those of us at the more applied end of the discipline—are often met with some version of the following question: "Ok, that all sounds great… But why is it *philosophy*?"[2] And this puzzlement is fair enough. To those familiar with more traditional analytic philosophy, it might not be immediately obvious what philosophers might have to offer on questions concerning, say, the evolution of technical cognition (Chapter 4) or language (Chapter 6), or perhaps even methodological concerns in archaeology (Chapters 1, 2 and 3). Here I will explain what it is that we philosophers of biology do, why we do it the way we do, and why it is that what we do is philosophy. In particular, the metaphilosophical view I present aims to capture the approach

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[1] The full reference is as follows: "It is therefore, the "eye on the whole" which distinguishes the philosophical enterprise. Otherwise, there is little to distinguish the philosopher from the persistently reflective specialist." (Sellars 1991, 8).

[2] Brown (2015, 63) makes a similar point.

to philosophy of biology practiced at the ANU.[3] That approach is first and foremost characterised by a deep engagement with the biological, psychological and social sciences.

At the broadest level, I am interested in what it means to be human. More specifically, I am interested in understanding the genetic and cultural evolutionary forces that build the biological parts that make us human. But how exactly do we keep one eye on the scientific detail and one eye on this big philosophical picture? My answer to this question—exemplified in the body of this thesis—is *analysis* and *synthesis*. That is, we analyse the inferential strategies employed by cognitive archaeologists, and synthesise work from a range of disciplines in developing a big picture account of human evolution.

As a brief illustration, consider our species' advanced technological abilities. There is broad disagreement between psychologists and anthropologists regarding the source of these abilities. Psychologists—unsurprisingly—say they are a product of our minds; anthropologists—equally unsurprisingly—say they are more a product of high-level cultural processes. The most plausible answer—again, unsurprisingly—combines elements of both these two positions. Philosophers of biology are well-placed to see beyond disciplinary silos, and are hence well-placed to draw out this answer. For instance, we can bring together work from animal cognition studies and palaeoarchaeology to undermine claims that causal understanding is a uniquely human psychological capacity. On the other hand, we can point to work in experimental psychology linking causal reasoning to the performance of toolmaking tasks to undermine claims that social cognition, or cultural processes more broadly, are the dominant force driving human technological capacity. And we can then situate these claims within various evolutionary frameworks, and attempt to identify which is the most plausible (this, roughly, is the work of Chapter 4). Consequently, by employing the philosophical tools of analysis and synthesis, we can keep one eye on the scientific detail and one eye on the philosophical big picture. This looks like our best chance of meeting the challenge of understanding how it all hangs together. Indeed, I will argue that it helps overcome one of the main problems that Sellars himself outlined as an impediment to meeting this challenge: the so-called "clash" of the manifest and scientific images.

My second task is to situate each of the chapters of the thesis within the metaphilosophical view outlined in section 2. I spend some time going through the content of each chapter, and link their themes specifically to the broader goals that ground the ANU

[3] This approach is not *unique* to the ANU; it is also practiced elsewhere. More on this shortly.

approach to philosophy of biology. This will, I hope, give the reader a sense of how the argument in each individual chapter links to a more general research programme.

2. Philosophy of Biology at the ANU

Sullivan's creek runs north-south through the middle of the ANU campus. The science buildings tend to be clustered on the west side of the creek, and the humanities to the east. As a result, "crossing the creek" has become a euphemism for interdisciplinary work between the humanities and the sciences. In the late 1990's, Kim Sterelny began crossing the creek to attend seminars in the Research School of Biology, with Lindell Bromham crossing in the other direction, and so began a tradition of interdisciplinary research that continues to this day. Sterelny joined a more global movement in philosophy of biology and cognitive science that prioritised robust engagement with the empirical sciences: other creek-crossers include Paul and Patricia Churchland, Jerry Fodor, Stephen Stitch, Daniel Dennett, Bill Wimsatt, Eliot Sober, and Samir Okasha. More locally, in Australia, this movement includes Philip Gerrans, Paul Griffiths, Peter Godfrey-Smith, Toby Handfield, and John Sutton. It turns out that when philosophers and scientists cross the creek and hang together, the task of figuring out how things (broadly construed) hang together (broadly construed) becomes a lot less daunting. Or, to use Brian Keeley's slightly darker turn of phrase (borrowing from Benjamin Franklin), "…if we don't hang together, we shall hang separately, intellectually speaking" (Keeley 2005, 27).

Research in philosophy of biology at the ANU is characterised by a robust engagement with the biological, cognitive, archaeological and anthropological sciences. This engagement goes far beyond the traditional motivating question of philosophy of science—roughly, "what is science?" Rather, much of the research conducted falls under a project that has been called by Peter Godfrey-Smith *philosophy of nature* (2014, 4). The basic distinction is as follows. The topic of inquiry in philosophy of science is the practice and outputs of science itself; whereas the topic of inquiry in philosophy of nature is the natural world, and, ultimately, ourselves, understood as a product of that world. This brings me back to Sellars.

2.1 Sellars on science and philosophy

I was a latecomer to philosophy; most of my undergraduate degree was in archaeology. The philosopher whose work first really engaged me was Sellars, and, in particular, his *Philosophy and the Scientific Image of Man*. In this paper, Sellars proposed that we have two allencompassing, and competing, frameworks for understanding ourselves and the world—the *scientific image* and the *manifest image*. I am framing my thesis through the lens of Sellars' notion that philosophy is an attempt to understand how things (broadly construed) hang together (broadly construed); my claim is that we can use analysis and synthesis to keep one eye on the scientific detail and one eye on the philosophical big picture. In this section, I want to suggest that pursuing this project helps us resolve the tension between the two images.

Sellars argued that there are a range of phenomena in the world—in particular relating to minds; e.g. sensory states, meaning, thought, normativity and intentional action—that are difficult to square with a robust philosophical naturalism. He framed this problem in terms of a "clash" between the two rival images of humanity and the world. The manifest image is a conceptual framework in which objects and persons are the basic ontological constituents. It contains cups, pens, colours and shapes; as well as people, who think and act according to reasons and norms. Making sense of this image and its constituents has been the goal of traditional philosophy. Roughly speaking, the manifest image is the framework of common sense folk psychology. This does not mean it is a pre-reflective, unsophisticated framework. Rather, Sellars emphasises that it has undergone frequent empirical and categorical refinement. One mode of reasoning, however, that is off-limits to the manifest image ("by stipulation") is the postulation of unobservable entities to explain observable entities (1991, 12). On the other hand, the scientific image describes and explains the world in terms of the theoretical postulates of the sciences. In the case of physics, for instance, this might include subatomic particles, or collapsing probability waves. Or, in the case of psychology, it might include neuro-computational frameworks, such as predictive processing (see Chapter 6).[4]

But this leads to a potential conflict. If there are entities in one image, but not in another, what should we say about that entity? And what should we say about the images themselves? Is one right and the other wrong? For instance, when we look to the manifest image we find that there are people who act and think according to beliefs, desires and reasons. But when we look to the scientific image we find only subatomic particles (physics), or, say, neural networks embodying Bayesian inference (psychology/neuroscience). Entities like beliefs, desires, reasons and persons seem absent. What should we do in this situation? Sellars outlines three options: (1) despite first inspection, there is an identity relation between manifest objects and scientific objects; (2) manifest objects are what really exist; scientific objects are just abstract

[4] That there are many different sciences likewise suggests there are many different images. More on this shortly.

or instrumental ways of understanding them; (3) scientific objects are what really exist; manifest objects are just "appearances" to human minds (1991, 27). Of the three options, Sellars takes (3) to be the most palatable. However, he wants to pursue this line in a way that does not simply dismiss manifest objects. Those norms, sensations and persons may be appearances, but they are not *mere* appearances; appearances, it turns out, are important.

Sellars' commitment to the primacy of the scientific image is summed up in his famous (to some, infamous) *scientia mensura*:

> … in the dimension of describing and explaining the world, science is the measure of all things, of what is that it is, and of what is not that it is not. (Sellars 1997)

The *scientia mensura* states a principle that is essential to Sellars' solution to the clash of the images. In more colloquial terms, I read it as follows: if you want to know what's in the world, consult the sciences. To extend that reading a bit further, we might say that the sciences introduce a constraint on the raw materials we have to work with when it comes to philosophical explanation: namely, those that we find posited by our current best theories. A commitment to some form of this principle has motivated a range of prominent philosophers of mind in the latter half of the 20th century, including Daniel Dennett, Paul Churchland, Patricia Churchland and Ruth Millikan.

Sellars' solution, then, is to argue there are ways of approaching the clash of the images which maintain the primacy of a naturalised ontology, yet do not involve straight-forwardly denying the existence of the problematic phenomena. In other words, there is a route to a comprehensive philosophical naturalism that does not entail eliminitivism; what James O'Shea has called Sellars' "naturalism with a normative turn" (2007). In more colloquial terms: you can have your cake and eat it too.

But in accepting Sellars' solution, we are required to accept the terms of the problem he is addressing; namely, that there is a clash between two mutually inconsistent explanatory frameworks. That there is a tension here is in turn driven by the claim that there is a clear distinction between two competing images. In what follows, I will suggest that recent work in philosophy of biology undermines this claim *and* that this work meshes well with Sellars' own methodological principles. I will argue that, when philosophers and scientists cross the creek and hang together, the contrast between the two images fades. We can hence ease the problem by rejecting its terms.

Sellars has a few methodological suggestions for tackling the task of seeing how it all hangs together; suggestions which square nicely with my own strategy of analysis and synthesis. First, we need to "know our way around":

> To achieve success in philosophy would be, to use a contemporary turn of phrase, to "know one's way around" […] in that reflective way which means that no intellectual holds are barred. (Sellars 1991, 7)

So we have to know our way around with no intellectual holds barred. Roughly, I take this to mean that the philosopher must know their way around the philosophical and scientific landscapes, in a way that encompasses both *what it is that we know* and *how we think we know it*. For Sellars, the "no intellectual holds barred" clause means that philosophy is an exercise in *knowing how* rather than *knowing that*. To borrow his example, knowing how to find a mathematical proof presupposes knowing that each step in the proof follows from the previous step; but knowing that each step in a given proof follows from the previous steps, and even understanding how it follows, does not mean you know how to find mathematical proofs.

The two parts of this thesis—representing analysis and synthesis respectively—reflect Sellars' suggestion. The first part is concerned with methodological issues in cognitive archaeology; it makes claims regarding *how* we think we know about human cognitive evolution. The second part is concerned with first-order issues; it makes claims about what actually happened in human cognitive evolution.

We now have a general idea of Sellars' philosophical project, and of how this project aligns with the project of this thesis. My task now is to show why crossing the creek and hanging together dissolves the clash of the images. This claim is based on two criticisms with Sellars' dichotomy: first, the distinction between the manifest image and the scientific image is not in kind, but by degree[5]; second, there is not *a* scientific image, but *many* scientific images.

2.1.1 Distinguishing the two images

We saw previously that a distinguishing feature of the scientific image is a particular mode of explanatory reasoning. Namely, the practice of postulating unobservable entities to explain observable entities. Hence we use subatomic particles to explain the behaviour of macroscopic objects, or explain human behaviour by positing that neural systems are Bayesian

[5] Bass van Fraassen (1999) makes a similar general critique, though his line of argument and conclusions are different.

learning engines. But what should we say about folk psychology? On one hand, it looks to belong firmly in the scientific image. I explain your going to the fridge to get a beer by positing unobservable entities; namely, your desire for a beer and your belief that there is one in the fridge. So this looks to be a straightforward case of the reasoning that distinguishes the scientific image. On the other hand, belief/desire folk psychology is central to traditional philosophical and everyday discourse, and presumably these belong squarely in the manifest image.

What should we say about this issue? Sellars himself suggests there are ways to cash out the link between publicly observable behaviour and mental states such that folk psychology might belong to *either* the scientific image or the manifest image. One suggestion concerns whether or not the evidential connections between mental states and behaviours are "pre-existent", or whether they are "correlations between constructs which it introduces..." (Sellars 1991, 25). But the difference between "pre-existing" and "introduced" is surely not relevant here; after all, pre-existing constructs were introduced at *some* point in human history. Perhaps he has in mind the *explicit* introduction of constructs for the explanation of behaviour. The idea would be that blind cultural processes gave rise to folk psychology, whereas properly scientific psychology requires the intentional postulation of constructs, and perhaps a system of institutionalised testing. But if this is true, the key difference between the manifest and scientific image is not a mode of explanation that posits unobservable entities; rather it is how that mode of explanation is propagated. Moreover, the difference will be one of degree, given the actual history of science is an incremental and partially evolutionary process. The crux of the issue is this: if we take Sellars at face value, the key difference is supposed to be whether or not the constructs are *observable*, and on that metric the posits of folk psychology are unambiguously part of the scientific image.

Another suggestion concerns a distinction between "perceptible and introspectable events…" on one hand and the postulation of "imperceptible objects and events…" on the other (Sellars 1991, 22). This gets us back to the distinction between observables and unobservables, but a lot then turns on whether or not the posits of folk psychology are introspectably observable. Can we introspect beliefs? There is a long debate over such issues. I will not go into that debate here, but suffice to say there is a lot of work to be done if the introspectable/imperceptible distinction is going to do the work of distinguishing the manifest and scientific images.

Some researchers argue that folk psychology does postulate unobservable entities, and that it should be classified as part of the scientific image (e.g. Churchland 1981; Fodor 1987). I am more sympathetic to this view, but note that it has two significant implications. The first is well-known, and has been ably defended by Patricia and Paul Churchland: if folk psychology is a scientific theory, then it should be replaced by better scientific theories. Consequently, if the mind sciences produce successful theories that shed the terms of folk psychology, then, for instance, we should replace a commitment to beliefs with a commitment to brain states. This view is controversial, but not one that I find particularly unpalatable. However, the important point for our purposes is that this line has another important implication: if folk psychology is a scientific theory, then a large section of what Sellars calls the manifest image is in fact part of the scientific image. Indeed, such a large portion—basically that to do with persons—that it seems unclear what of interest is left to the manifest image. Certainly, much of traditional philosophy (apart, perhaps, from the metaphysics of objects other than persons) and science would now be classified together. But that, of course, does not get at the distinction Sellars wants.

So far I have focussed on the question of how Sellars' distinction might deal with folk psychology. But it's also important to note that the slippage goes both ways: many scientific explanations do appeal to unobservables. For instance, explaining the extinction of small Australian marsupials because of changes in fire regimes and fox predation does not involve positing hidden entities.

The lesson in all of this is that the criterion of postulating unobservables is not able to do the work Sellars wants it to do. And, I would suggest, nor is any one criterion. Rather, the difference between the manifest and scientific image is only a messy difference by degrees; there are aspects of everyday/philosophical reasoning at play in scientific discourse, and there are aspects of scientific reasoning at play in everyday/philosophical discourse. For example, the concept of a species of animal is a folk concept that has been taken over and refined by science. Sellars is to some extent aware of this issue, and he is careful to emphasise that the images are "idealisations" (Sellars 1991, 22). But if the distinction between the images can only be maintained if they are treated as idealisations, and becomes significantly less distinct as we look at actual practice, then the problem of fusing the images diminishes considerably. Idealisations are useful up to a point, but they can also be used to create artificial philosophical dichotomies (van Fraassen 1999). One way of dissolving these dichotomies is for philosophers and scientists to become more familiar with each other's discourse. Crossing the creek and hanging together, then, is a good way of keeping one eye on the scientific detail and one eye on the philosophical big picture. In turn, this is our best chance of seeing how it all hangs together.

2.1.2 The scientific image or the scientific images?

We have so far talked of "the" scientific image. But, as Sellars notes, there are many scientific disciplines, from particle physics to behavioural economics, and there is no common set of postulates among them (Sellars 1991, 22-23). So should we not then be talking of the scientific image*s*? Sellars thinks we should not, and presents an extended argument designed to show that the postulates of "behaviouristics" are able to be reduced—or at least "telescoped"—to the postulates of neurophysiology, biochemistry and, eventually, physics (Sellars 1991, 23-27). In other words, Sellars works extremely hard to maintain the notion of there being one image, rather than many images. I want to suggest that more recent work in the philosophy of biology and the philosophy of modelling indicates that the application of methods from the physical sciences to biology faces deep problems. In turn, this calls into question Sellars' reference to *the* scientific image, and pushes us toward accepting a more pluralistic understanding of scientific practice.

Sellars' argument is designed to show that, despite the fact that neurophysiology and behaviouristics are different sciences, the latter is just as committed to the idea that humans are complex physical systems as the former. The reason for this effort, it seems, is to show that the scientific image does not contain radically incongruent ontological posits, such that we would be forced to conclude that there are in fact many scientific images. Now, I have no issue with the claim that humans are complex physical systems. But I do not think that a commitment to this claim likewise commits you to the claim that there is a single scientific image.

The reason for this stems from Levins' (1966; see also Weisberg 2006) work on model building strategies in population biology. Levins highlighted the extreme complexity of population dynamics, and noted the number of relevant variables involved may render it impossible to ever capture all the relevant detail in a model, even in comparatively small populations. This forces modellers to make choice between what Levins called the *brute force approach* or the *idealisation* approach. On the brute force approach, the goal is to get as much detail in to a model as we possibly can. On the idealisation approach, we make strategic idealisations, deliberately omitting certain aspects of the modelled population.

Levins was particularly critical of the brute force approach. But this leads to an important theoretical choice: what aspects of a system should we idealise away from, and what aspects should we attempt to capture in detail? Levins' work focussed on a trade-off between generality, realism and precision. In particular, he thought that maximising the generality of a model will require sacrifices in terms of realism and/or precision. Realism, or accuracy, is typically understood in terms of the amount of causal structure that a model represents. Consequently, the more target systems a model encompasses (i.e. the more general it is) the less accurately it represents them. Precision is understood in statistical terms, as the closeness of repeated measurements of some quantity. Consequently, as a model's parameters become more finely specified, the number of systems which lie outside those parameters increases (i.e. the less general it is). So, roughly, we have a trade-off between the *number* of systems a model can represent and the accuracy with which it can represent. Levins' work is normally thought to deliver a pragmatic lesson: we cannot produce one model to rule them all, so which trade-off strategy you take should be relativised to your aims. For instance, models that score highly on realism—and thus capture a lot of the causal structure of a system—will be better for predicting the effects of some intervention. Meanwhile more general models will allow us to say things about more systems, and aid the production of high-level theory.

Levin's work has since been expanded, and is now thought to apply to any biological system; from ecosystems to brains. The point, then, when it comes to Sellars' reduction argument, is that different ways of modelling biological entities like persons will be good for different reasons. Some, such as those from the neurosciences, will idealise away from whole organisms and maximise accuracy relating to brains; others, such as those from the social and behavioural sciences, will idealise away from brains and maximise accuracy with respect to persons as rational decision-makers. The pragmatic lesson from Levins' work pushes us toward model pluralism; to get a good handle on complex biological systems, we need lots of models at lots of different levels of grain.

All this undermines Sellars' attempts to motivate the case that there is *a* scientific image. Rather, any comprehensive scientific understanding of humans in the world is going to need to employ multiple different models, and the posits of these models will range from desires and beliefs to neurons and synapses. This undermines the dichotomy between a scientific *image* of micro-physical particles and a manifest image populated by persons. Instead, we have multiple scientific *images*, and the posits of some of the images will cross over with those from the manifest image.

Now to sum up. I take the lesson from the above discussion to be as follows: once we turn our attention away from idealisations, and look to actual scientific and philosophical practice, the contrast between the two images starts to dissolve. In other words, by crossing the creek and hanging together, we find that the problem of the clash of the images is reduced.

Next I turn to the notion of keeping one eye on the scientific detail and one eye on the philosophical big picture, and my own strategies for achieving this aim; namely analysis and synthesis.

2.2 A deeply philosophical question

Here is a question that has engaged philosophers over the ages: what does it mean to be human? There was a time when this question was exclusively the domain of philosophy, but that has changed with the rise of evolutionary biology, scientific psychology, neuroscience, and the social sciences. We now recognise that understanding what it means to be human requires understanding both the biological parts we are made of, and the genetic and cultural forces that build those parts.

Given all this, the role philosophy plays in addressing this question changes considerably. Traditional, a priori theorising must take a backseat, and we should work toward avoiding a dichotomy summed up in a quote often attributed to Konrad Lorenz:[6]

> Philosophers are people who know less and less about more and more, until they know nothing about everything. Scientists are people who know more and more about less and less, until they know everything about nothing.

This sentiment should be taken as it was probably intended: tongue in cheek. However, the dichotomy Lorenz outlines presents a distinct risk for the kind of project I am engaged in (indeed the dichotomy here bears striking similarity to the Levins trade-off outlined above). However, if we employ Sellars' notion of the philosophical project—that is, if we take the sciences seriously and know our way around the detail, yet do not lose sight of the big picture—we can avoid the Lorenz's dichotomy.

2.3 Analysis and synthesis

[6] See 'A Quote by Konrad Lorenz' (2022). Though this general sentiment was certainly around a long time before Lorenz gave it his particular gloss.

But how do we do this precisely? What can the armchair bound philosopher offer the empirical sciences engaged with the question of what it means to be human if not a priori theorising? My answer to this question is that philosophers can provide analysis and synthesis. With regard to the former, the idea is that philosophy can offer an analysis of the logical structure of arguments, and more general critiques of the epistemic claims made by the sciences. With regard to the latter, the idea is that philosophy can bring together different ideas from a broad range of different fields, and investigate the inferential relationships between them.

And, when it comes to cognitive archaeology, the benefits of analysis and synthesis are particularly acute. All academic pursuits are difficult, but cognitive archaeology presents an especially unique challenge. First, of course, you need to know a lot about archaeology. Second, you need to know a lot about cognitive science (which encompasses neuroscience, psychology, and philosophy of mind). Third, you need to know a lot about evolutionary biology. Fourth, as a historical science, you are confronted with deep methodological challenges: how can we go about understanding events and processes that happened millions of years ago?

Philosophy can help with the first three of these questions through synthesis. Unlike archaeologists, philosophers do not need to coordinate excavations, catalogue artefacts or collate datasets. Rather, most of our time is spent reading, thinking and writing (usually in that order). Philosophers of biology who take it upon themselves to be well-informed enough about archaeology, cognitive science and evolutionary biology are thus well placed to bring together various ideas therein, and attempt to see how things might hang together.

Addressing the last of these questions requires analysis. The historical sciences face a key methodological problem: they have no direct empirical or experimental access to the phenomena under investigation. To illustrate, consider the challenges faced by researchers working on the evolution of language (see chapter 5). We cannot simply observe, say, a population of Neanderthals to assess their linguistic capacity. Rather, all we have are the patchy, fragmented remains found in the archaeological and paleoanthropological record; and reliable inferences from that evidence to claims about linguistic capacities are difficult to construct. Moreover, we cannot run any experiments on Neanderthal linguistic capacities. All we can do is run experiments on modern day humans, and then produce an argument by homology. And again, such inferences face significant methodological challenges. So, to address our question, we need to assess the key differences between the historical sciences and the more traditional sciences. This requires identifying inferential limitations in the former, and devising strategies for minimising or countering those limitations.

Developing a comprehensive account of language requires coming to grips with the biological and cultural forces that shaped its evolution. In Sellarsian terms, this means: (i) "knowing our way around" the landscape of linguistics, neuroscience, psychology, archaeology, and evolutionary biology, such that we can draw together various concepts from those disciplines, make them clear, and map relationships between them; and (ii) doing so in a "no intellectual holds barred" manner, such that we can identify and address their inferential and epistemic limitations. In a nutshell, it requires keeping one eye on the scientific detail and one eye on the philosophical big picture.

All of the following chapters are an exercise in analysis, or synthesis, or both. They hence align with Sellars' general metaphilosophical framework. My remaining task is to provide an overview of each of these chapters, and contextualise them within this framework.

But before I turn to that, I want to emphasise one final point. I do not think that those with formal philosophical training—that is, philosophers—are the only people able to do the above work. Indeed, I think there is very little light between the scientifically informed philosopher and the philosophically informed scientist. So, just as properly trained philosophers are able to do methodological or theoretical work in the sciences, so scientists with philosophical inclinations can—and frequently do—provide analyses and syntheses to great effect (e.g. Thomas Wynn 2002; Coolidge and Wynn 2016; Botha 2016; Garofoli and Haidle 2013). In my opinion philosophers frequently underestimate the philosophical sophistication found in methodological reflection in the sciences; certainly, in any case, this is true of cognitive archaeology.

3. Chapter summary

The thesis itself is made up of two parts. The first part (chapters 1-3) fall under the scope of analysis. The second part (chapters 4-6) fall under the heading of synthesis. Each of the chapters uses either the analysis of inference or the synthesis theories from archaeology, psychology and biology as their primary methodology (and some papers employ both). In this section I will summarise the key claims and arguments in each of the chapters.

Part 1: Analysis

3.1 Chapter 1: What can the lithic record tell us about the evolution of hominin cognition?

Archaeologists, and particularly those interested in cognitive and social features of past societies, have long worried about inference. Hawkes (1954) provides a now classic analysis, while more modern treatments in the cognitive archaeological context are found in Coolidge and Wynn (2016), Wynn (2002), Garofoli and Haidle (2013) and Botha (2006; 2016). However, there has been much less interest from philosophers of science regarding these issues.

In the last 20 years, Alison Wylie (2002; Chapman and Wylie 2014; 2016) has engaged specifically with philosophical issues related to archaeological inference. Yet her work is squarely in traditional archaeology, and does not provide a treatment of cognitive archaeology. More recently, Adrian Currie and Anton Killin (2019) have outlined and characterised some of the inferential strategies used by cognitive archaeologists.

Chapter 1 builds on this work. I give myself two main tasks. First, I map out an inferential strategy used by cognitive archaeologists; one that has not previously been characterised. Second, I argue that this inferential strategy commits us to a particular *type* of explanation for transitions in the archaeological record.

Regarding the first task, I begin with Currie and Killin's (2019) characterisation of a *minimal capacity inference*. A minimal capacity inference takes some artefact from the archaeological record and asks: what were the minimal cognitive prerequisites required to produce this tool? The general idea is that this gives us a baseline reading of the capacities of the population that produced that artefact. A different inferential strategy used in cognitive archaeology begins identifying a technological transition in the archaeological record. We then run a minimal capacity inference on either side of the transition, and identify some capacity that is the best explanation for the later technology. Finally, we infer that the capacity was absent during the period of the earlier technology, and that the later technology signals the emergence of the new capacity. I call this strategy a *cognitive transition inference*.

Cognitive transition inferences are a useful tool, but they have an important limitation: their reliance on minimal capacity inference mean they are committed to cognitive explanations for transitions in the archaeological record. The problem here is that, in many cases, there are multiple competing explanations for transitions in the archaeological record. Moreover, the ethnographic record shows wide variation in technology with no difference in cognitive capacity. I use Coolidge and Wynn (2018) as a case study. In that book they use a cognitive transition inference to argue that the European Upper Palaeolithic was the product of a neural transition that allowed for greatly expanded working memory in *Homo sapiens*. However, there are those who argue that the European Upper Palaeolithic is best explained by demographic changes (Powell, Shennan, and Thomas 2009), or by environmental pressures (Stiner et al. 1999; Stiner, Munro, and Surovell 2000).

This leaves us with a difficult question: how do we tease apart competing explanations for some event, when it is very likely that *each* of the causal factors named in those explanations played *some* role in the production of the event? In the final section of the chapter, I provide some thoughts on how we might approach this problem. The first is to use multiple lines of independent evidence to zero in on the dominant cause. The second, more speculative thought is that the philosophical literature on causality can help; in particular, Kenneth Waters' (2007) work on difference-making causes.

The chapter hence uses philosophical analysis to clarify inferential strategies in cognitive archaeology, identifies a problem, and offers some potential solutions.

3.2 Chapter 2: Cognitive archaeology and the minimum necessary competence problem (co-authored with Anton Killin)

Archaeologists have long engaged with the issue of *minimum necessary competence*. The problem is that we tend to assume that the material remains of a culture represent the peak of their technological/cognitive capacity. Yet it is always possible that the most sophisticated behaviours produced by those people left no archaeological signature. So we should only ever take the record to signal the lower-bounds of cognitive capacity.

The issue of minimum necessary competence presents a particularly acute challenge for cognitive archaeologists. The goal of the discipline is to use the archaeological record to outline a chronology of human cognitive evolution; but if we assume that all the record signals is the lower cognitive bounds of the hominins involved, that chronology will be very coarse-grained. In response to this "maddening" challenge, Wynn and McGrew famously pushed back against the thinking behind minimum necessary competence worries: "… the point of scientific archaeology is to use evidence, not make assumptions" (Wynn and McGrew 1989, 384). This produced *Wynn's Mantra*: "Make no more assumptions than are necessary for explaining a phenomenon." (Overmann and Coolidge 2019, 3).

However, if we are to satisfy Wynn's mantra, we need to be able to identify the base-level capacities required to produce some technology. This becomes a problem for cognitive archaeologists given that different cognitive models will produce different minimum necessary capacity readings. In particular, models coming from the 4E (embodied, enactive, extended and embedded) paradigm will produce lower readings of the capacities required to produce a tool, while those coming from representational paradigms will produce higher readings. This generates important methodological questions: which principles should cognitive archaeologists follow in selecting their cognitive model, given that the results of their study will depend to some degree on the background commitments of the particular theory they choose?

We outline a range of cases where this problem is evident in the literature, and propose two solutions, in the form of heuristics for theory choice. The first heuristic is that cognitive archaeologists should select cognitive models that produce readings corroborated by multiple lines of independent evidence. The basic idea is that, all things being equal, if the minimal capacities a particular cognitive model produces are supported by other inferential strategies, that model is to be preferred over one that is not. The second solution we offer is more speculative. Using modelling work produced by Zollman (2009), we argue that theoretical diversity is a good thing for cognitive archaeology. Roughly speaking, cognitive archaeologists should not be in the business of picking winners in cognitive science, as this may lead to the discipline neglecting models that turn out to be important. Rather, cognitive archaeology should be attempting to test a range of different models. So in this sense, theoretical diversity turns out to be a good thing. The lesson here echoes the lesson from Levins' trade-off outlined in 2.1.2: we should be model pluralists.

So Chapter 2 follows the same model as Chapter 1: we analyse inferential strategies in cognitive archaeology, identify a problem, and propose some solutions.

3.3 Chapter 3: How WEIRD is cognitive archaeology: engaging with the challenge of cultural variation and sample diversity (co-authored with Anton Killin)

In their landmark paper, Henrich, Heine and Norenzayan (2010) argued that the cognitive and behavioural sciences face a big methodological problem. The problem begins with an observation: most of the research in these disciplines use subjects from Western, Educated, Industrialised, Rich and Democratic (WEIRD) societies. This is not a problem, if one of two conditions is met: (i) culture has no effect on the cognitive trait you are studying; (ii) WEIRD people are generally representative of the species as a whole. However, Henrich and colleagues show that often neither of these conditions is met. Culture does matter to cognition, and WEIRD people are frequently outliers with respect to the rest of the population. The lesson for the behavioural and cognitive sciences is clear: if you have not sampled cross-culturally in your study, generalise with care.

The vast majority of the discussion following the publication of this article focussed on the implications of these results for research on extant human populations. In this chapter, we extend that analysis to encompass inference to populations of ancient humans, their hominin ancestors, and their cousin lineages.

As we have seen, the main methodology in cognitive archaeology involves taking some model from the cognitive sciences and analysing a tool technology through that lens. We then run an inference, via the model, to the cognitive capacities of the hominin/s that made the tool. But how is this inference affected if that cognitive model has not been subject to cross-cultural sampling? We show that such inferences do not generalise. For example, suppose you use a cognitive model to show that Neanderthals had long-term working memory. We show that the inference is provisionally justified regarding the localised cultural group that produced the tool, but that a claim regarding the capacities of the Neanderthal species is unjustified.

We then use a series of case-studies to show that a large amount of work in cognitive archaeology does rely on models that are not cross-culturally verified, and that cognitive archaeologists are typically very interested in making population-level claims. We do so both with particular inferences regarding specific cognitive capacities, such as language and long-term working memory, and also with broader research programs, such as affordances research and neuroarchaeology. We address some cases where the use of models tested on limited cultural samples might be justified, but find these cases to be very limited. The upshot is that, if we want to generalise out cognitive archaeological inferences, then we need to use models corroborated by cross-cultural data.

Chapter 3 again follows the methodological strategy of analysis. We isolate a particular inferential problem, and then offer a solution to that problem.

Part 2: Synthesis

3.4 Chapter 4: Mind the gap: a more evolutionarily plausible role for technical reasoning in cumulative technological culture (co-authored with Rachael Brown)

Human populations are capable of maintaining technologies that are too sophisticated for any one individual to produce in isolation. This feature of human societies is called *cumulative technological culture*. Theories regarding the origin and propagation of cumulative technological culture have tended to focus on the evolution of social cognitive capacities; in particular teaching, imitation and innovation, metacognition and theory of mind. This is for good reason: cumulative technological culture is clearly a deeply social phenomenon.

However, Osiurak and Reynaud (2020) reject this emphasis. Instead they argue that a type of cognition grounded in the ability to reason about physical objects, called *technical reasoning*, is the key driver of cumulative technological culture. In their view, social cognitive capacities are necessary for cumulative technological culture, but they are not sufficient. Whilst we are sympathetic to the view that non-social, causal cognitive capacities have not been given their due in the literature, we are nonetheless sceptical of Osiurak and Reynaud's proposal. We focus on two aspects of their account.

First, they commit themselves to the claim that technical reasoning is a species-specific property of *Homo sapiens*. We show that there is considerable empirical support for the claim that both cumulative technological culture and technical reasoning are found in non-human animals. Second, as technical reasoning is understood as a necessary condition for cumulative technological culture, the causal arrow goes one way. This cognition-first account forces a difficult theoretical choice. On one hand, they can posit a "leap"; the sudden appearance of technical reasoning in humans. This is unappealing, as the sudden generation of a domain-specific complex cognitive mechanism via undirected means is highly unlikely. On the other hand, they could adopt a more gradualist story, and hold that technical reasoning evolved piecemeal over a long period of time. However, this requires abandoning their cognition-first commitment because, as previously mentioned, there is strong evidence that cumulative technological culture is found well beyond our species.

We argue that a more evolutionarily plausible account of technical reasoning jettisons *both* the species-specific and the cognition-first commitments. Such an account takes a co-evolutionary relationship between toolmaking and technical reasoning to be central: in colloquial terms, you have to be smart to make tools, but, luckily, making tools also makes you smarter. We sketch a general picture of this account, which maintains an important role for technical reasoning, but which emphasises the role of social learning in maintaining sophisticated technologies. This developing technological niche selects for more advanced social and technical cognition capacities, which in turn drive more sophisticated technological cultures.

Chapter 4 brings together works from archaeology, psychology, animal cognition research and evolutionary biology in order to make a first-order claim about the evolution of a cognitive capacity. It thus demonstrates how philosophers of biology can use the tool of synthesis in aiding cognitive archaeology.

3.5 Chapter 5: Stone tools, predictive processing and the evolution of language

A recent development in archaeology is the sub-field of experimental archaeology. Experimental archaeologists perform experiments in the present and use them to project inferences to the past. In the discipline of cognitive archaeology, researchers use various neuroimaging techniques in an attempt to find out which areas of the brain are co-opted by different toolmaking tasks. One result from this method—dubbed *neuroarchaeology*—is that there is considerable neural overlap between the areas of the brain co-opted by Early Stone Age toolmaking tasks and those recruited during the production of language (e.g. Stout et al. 2008; Stout and Chaminade 2012; Stout et al. 2021; Putt et al. 2017). This has motivated a resurgence of interest in tool-language co-evolutionary theories.

My starting point in this chapter is work by Dietrich Stout and colleagues (2008; 2012) that demonstrates this overlap. I note that another way to bolster tool-language co-evolutionary theories is to identify a corresponding computational overlap; that is, some computational mechanism that is common to both toolmaking and language production. I use an error minimisation model to do this. I show that, when applied to the action tasks required to produce Late Acheulean tools, the error minimisation framework shows that complex structured representations—that is, representations with nested part-whole structure—were present in the Early Stone Age. As representations of this kind are typically thought to be necessary for the production of syntax, it is plausible that toolmaking facilitated the early development of cognitive structures that were co-opted and refined in the evolution of language.

In the second part of the paper, I situate the error minimisation approach within broader debates regarding the evolution of language. *Saltationists* (e.g. Berwick and Chomsky 2016) argue that language appeared suddenly, as the result of random genetic mutation. *Gradualists* (e.g. Planer and Sterelny 2021) argue that language evolved via incremental steps over a long period of time. The error minimisation approach supports gradualism, as it identifies a plausible evolutionary precursor to the more complex cognitive capacities underlying the full-blown syntactical abilities of modern humans. I then argue that the error minimisation approach occupies a unique place in this literature. Typically, it is thought that the transition between non-structured and structured representations required a corresponding transition between sequential and hierarchical processing. Driven by evolutionary continuity concerns, this has led some theorists (e.g. Frank, Bod, and Christiansen 2012) to deny that language processing requires hierarchical cognition. Others have attempted to provide a gradual account of the sequential-hierarchical transition.

The error minimisation account offers a different take on this issue. In particular, it allows for the evolution of structured representations *without* needing to posit a corresponding transition between sequential and hierarchical processing. Rather, it is error minimisation all the way down. In this regard, it differs from other gradualist theories in the literature (e.g. Planer and Sterelny 2021)

This chapter is a piece of first-order cognitive archaeology. It takes some technology from the archaeological record and analyses the behaviours required to produce that technology using a model from the cognitive sciences. It demonstrates the way that philosophers of biology can synthesise theories from various disciplines, and assess the features and bugs against other evolutionary proposals.

3.6 Chapter 6: Cognitive archaeology meets cultural evolutionary psychology

In recent years, Cecilia Heyes has produced a novel framework for understanding the evolution of the human mind. Heyes claims that the dominant force shaping our cognitive capacities—both at ontogenetic and phylogenetic levels—is cultural evolution, not biological evolution. According to *cultural evolutionary psychology*, the genetic differences between ourselves and the chimpanzee do not go far toward explaining the cognitive and behavioural differences between the two species. Rather, what matters is social learning. Heyes' big idea is that a range of distinctively human cognitive capacities—including selective social learning, imitation, theory of mind and language—are transmitted via cultural processes.

In labelling these capacities *gadgets*, Heyes draws a direct analogy with the material artefacts studied by archaeologists. Both gadgets and artefacts are the products of cultural transmission. In this paper, I bring together the discipline of cognitive archaeology and the framework of cultural evolutionary psychology. In particular, I look at what doing cognitive archaeology in a cultural evolutionary psychology framework involves, and how research in cognitive archaeology can inform research on gadgets.

Regarding the latter, I examine, but ultimately reject, one seemingly promising line of thought. The idea is that cultural evolutionary psychology, if true, would mitigate somewhat the central inferential challenge facing cognitive archaeology. That challenge is: how do we infer the cognitive capacities of past hominins from their patchy material remains? One might think that inferring culturally transmitted cognitive gadgets from culturally transmitted cultural artefacts is an easier task than inferring biologically transmitted instincts from culturally transmitted artefacts, because the inference does not bridge the cultural/biological divide. The problem here is that cultural evolution is messy, and more volatile than biological evolution. As a rough rule of thumb then, if the hominin evolutionary trajectory is more the result of cultural evolution, that does not make sound cognitive archaeological inferences easier to produce. I then follow Heyes in examining how we might interpret the Palaeolithic record using cultural evolutionary psychology. I criticise her suggestion that the record signals a sudden, and relatively recent—that is, less than 100 thousand years ago—appearance of gadgets. Rather, I suggest that we should be sceptical of major transition accounts of the archaeological record. Consequently, cultural evolutionary psychologists need to develop more gradualist accounts of the evolution of gadgets.

I then look at how research in cognitive archaeology can inform cultural evolutionary psychology. I look at Heyes' controversial proposal that language is a gadget, and argue that there is considerable evidence, both from traditional cognitive archaeology and from neuroarchaeology, that supports her case with respect to the early evolution of language. But an important question concerns the genetic assimilation of gadgets. Why, if language was adaptive, was it not assimilated? Heyes argues the cultural targets that gadgets track are too transient for assimilation to occur. I show that whether or not language is an instinct or a gadget in modern humans turns on this claim.

In this chapter, I integrate research in cognitive archaeology with a new evolutionary framework developed in the psychological sciences. I show that bringing the two together produces important lines of further research. Cultural evolutionary psychology is a new but burgeoning field of research, and an engagement with cognitive archaeology promises to be productive for both disciplines.

4. Concluding remarks

Let's take stock. My goal here was to outline a metaphilosophical view, and situate the chapters of the thesis within that view. I began by outlining Sellars' conception of the goal of philosophy: to know how things in the broadest possible sense hang together in the broadest possible sense of the term. I then aligned Sellars' strategy for achieving this goal—keep one eye on the scientific detail and the other on the philosophical big picture—with the methodology of analysis and synthesis. Next, I argued that Sellars' adopting this methodology undermines the clash between the manifest and the scientific images. Rather, when philosophers and scientists engage, we find there is considerable crossover between the two images. Finally, I situated each of my chapters within this general metaphilosophical strategy, and illustrated how each exemplifies the use of analysis or synthesis.

The lesson in all of this is that by crossing the creek and hanging together, we can develop a more comprehensive understanding of our evolutionary origins.

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PART 1

ANALYSIS

CHAPTER 1

What Can the Lithic Record Tell Us About the Evolution of Hominin Cognition?

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Abstract: This article examines the inferential framework employed by Palaeolithic cognitive archaeologists, using the work of Wynn and Coolidge as a case study. I begin by distinguishing minimal-capacity inferences from cognitive-transition inferences. Minimal-capacity inferences attempt to infer the cognitive prerequisites required for the production of a technology. Cognitive-transition inferences use transitions in technological complexity to infer transitions in cognitive evolution. I argue that cognitive archaeology has typically used cognitive-transition inferences informed by minimal-capacity inferences, and that this reflects a tendency to favour cognitive explanations for transitions in technological complexity. Next I look at two alternative explanations for transitions in technological complexity: the demographic hypothesis and the environmental hypothesis. This presents us with a dilemma: either reject these alternative explanations or reject traditional cognitive-transition inferences. Rejecting the former is unappealing as there is strong evidence that demographic and environmental influences play *some* causal role in technological transitions. Rejecting the latter is unappealing as it means abandoning the idea that technological transitions tell us anything about transitions in hominin cognitive evolution. I finish by briefly outlining some conceptual tools from the philosophical literature that might help shed some light on the problem.

1. Introduction

Archaeology attempts to infer the behaviour of past societies from their material remains. Cognitive archaeology attempts to infer the cognitive capacities of individuals in past societies from their material remains. For instance, one might infer that the production of prepared-core technologies requires long-term working memory (Wynn and Coolidge, 2004), or that Acheulean toolmakers require apprentice learning systems managed by expert individuals

(Hiscock, 2014), or that the symmetry displayed by hand-axes from the middle Palaeolithic indicate their makers were capable of practicing 3rd-order intentionality (Cole, 2016). Archaeology runs an inference from material remains to behaviour. Cognitive archaeology extends that inference to the cognitive capacities required to perform that behaviour.

If inferences of this general character can be supported, then the Palaeolithic record offers an important line of evidence regarding the evolution of hominin cognition. And given that signals of this story are few and noisy, it is important that this evidence is fully exploited. However, cognitive archaeology has faced sceptical criticisms regarding the reliability of its inferential framework, and, indeed, the viability of the project in general.[7] Identifying and addressing the target of these criticisms is thus an important project. The overall aim of this paper is to outline a problem of this kind – one which I believe has not yet been articulated.

The paper turns on a distinction between two different projects we might pursue in cognitive archaeology. The first project takes a particular tool-type, or technology, and asks "what kind of cognitive capacities were required to produce this technology?" Questions of this type are typically addressed using *minimal-capacity inferences*. The second project takes a transition between two technologies and asks "what can this transition tell us about the evolution of hominin cognitive capacities?" Questions of this type are typically addressed using *cognitive-transition inferences*. The first goal of the paper is to distinguish these two types of inference. The second goal is to show that combining these two inferential strategies commits us to a view about the cause of transitions in the tool record. The third goal of the paper is to show that this leads to a dilemma. I use the work of Wynn and Coolidge (2004; 2018) as a case-study throughout the paper.

I proceed as follows. Recent philosophical work has started teasing apart some of the different inferential strategies used by cognitive archaeologists (Currie, 2018; Currie and Killin, 2019). In section 2 I follow Currie and Killin (2019: 265) in developing an account of minimal-capacity inferences. Minimal-capacity inferences attempt to infer the cognitive prerequisites required for the production of a technology. In section 3 I distinguish minimal-capacity inferences from cognitive-transition inferences. This latter type of inference uses transitions in technological complexity to infer transitions in cognitive evolution. I show that cognitive-transition inferences are often informed by minimal-capacity inferences, and argue

7 See in particular Lewontin (1998) and Smith and Wood (2017). For a general summary (though Currie is an optimist) see Currie (2018: 16-18).

that this reflects an implicit commitment to the view that transitions in the complexity of the tool record are caused by transitions in the sophistication of innate hominin cognitive capacities. In section 5 I outline two alternative explanations for transitions in technological complexity, namely the environmental hypothesis and the demographic hypothesis. This produces a dilemma with respect to traditional cognitive-transition inferences: either defend a flat-footed cognitivist explanation for transitions in technological complexity (despite evidence favouring environmental and demographic explanations); or abandon traditional cognitive-transition inferences and lose this line of evidence to our cognitive past. This dilemma is substantive, and not one I could hope to address here. Rather, in section 6, I briefly outline some conceptual tools from the philosophical literature which might be of use in responding to the problem. Section 7 concludes.

2. Minimal-capacity inferences

Figure 1 depicts an Acheulean biface, dating from 500-300 kya. Here is one question a cognitive archaeologist might ask: what kind of cognitive capacities would be required to produce this hand-axe? The background assumption is that the cognitive capacities of the hominin that produced this tool played a causal role in its production, and this in turn licences an inference from tool to cognitive capacity. But we must be careful not to infer beyond what is suggested by the evidence. Consequently, attempts to address questions of this kind aim to identify *baseline* cognitive capacities. We ask: what are the minimal cognitive prerequisites needed to produce a tool? [8]

8 Another important background assumption here is that the tool is the product of a standing ability, and not of blind luck. In other words, it is a technology that its maker can reproduce with some reliability. Thanks to Kim Sterelny for helpful discussion here.

Figure 1. Acheulean biface from Cintegabelle, Haute-Garonne, France (500-300 thousand years old). Source: Didier Descouens; reproducible under the terms of the Creative Commons Attribution-Share Alike 4.0 International License. URL= https://upload.wikimedia. org/wikipedia/commons/8/87/Biface_Cintegabelle_MHNT_PRE_2009.0.201.1_V2.jpg. Accessed 20 August 2019

We cannot address this question *de novo*. Rather, we need some model of cognition through which we can analyse both the tool and the behaviours required to produce it. So cognitive archaeologists must look to the cognitive sciences. Only then can we reach the conclusion that the tool in figure 1 indicates the presence of a particular cognitive capacity; for instance a protolanguage (Planer, 2017) or 3rd order intentionality (Cole, 2016).

This general mode of inference has been described by philosophers as an example of *trace-based reasoning* (Currie and Killin, 2019; Currie, 2018; Jeffares, 2008; Kosso, 2001). Trace-based reasoning allows us to infer a past state of affairs from a current state of affairs. We do this by linking a physical trace – that is, some aspect of the contemporary world – with a theory about the processes that produced that trace (a 'mid-range' theory). In the case of cognitive archaeology, the past states of affairs are the cognitive capacities of ancient hominins, the physical traces are the tools we find in the Palaeolithic record, and the mid-range theory is the model of cognition we use to assess those tools.

When this general schema is used to produce a claim about the minimal cognitive requirements for the production of a technology, we have a minimal-capacity inference.

2.1 Wynn and Coolidge on Neanderthal cognition

Let's now look at an example of a minimum-capacity inference in action. I will use Wynn and Coolidge's (2004) paper on Neanderthal cognition. The overall schema of the argument is relatively straight-forward. The central claim is that the production of prepared-core technologies such as the Levallois requires long-term working memory. The archaeological record indicates that Neanderthals were using the Levallois for thousands of years. Consequently, Wynn and Coolidge conclude that Neanderthals possessed long-term working memory capacities.

The detail of Wynn and Coolidge's argument is illustrative, particularly with respect to their construction of a mid-range theory. But let's begin with the Levallois technique itself (see figure 2). The technique is classified as "prepared-core" because significant time is spent refining the shape of the core from which the final flake is removed. In particular, a flattened platform is constructed which, if struck correctly, will produce a flake with a distinctive plano-convex profile (i.e. one spherical surface and one flat surface). This process can be broken down into three steps. First, a large, flat striking surface is prepared from which the final flake will be struck. The shape of the final flake can be refined by trimming the edges of this surface. Second, a smaller, more convex surface is prepared, which becomes the striking platform. Third, the final flake is removed by applying hard-hammer percussion to the striking platform (see A in figure 2). The core can then be re-touched to produce further flakes (see C in figure 2). This latter procedure, called *recurrent Levallois*, requires a particularly high level of skill, as the difficulty of the task increases as the size of the core decreases.

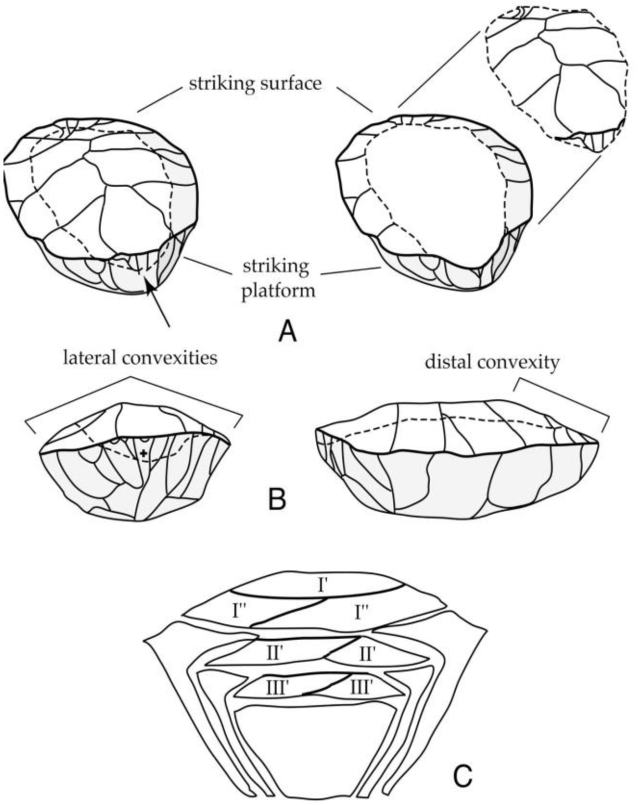

Figure 2. Stages in Levallois production. Source: Ambrose 2001, reproduced here with the permission of the American Association for the Advancement of Science.

There are a variety of techniques that can be employed to mitigate this problem. These require fine-grained motor control and the ability to fluidly adjust one's strategy as the task progresses.

As Wynn and Coolidge note, the complexity of this task – and the forward-planning involved – invite a cognitive interpretation (Wynn and Coolidge, 2004: 474). This requires a mid-range theory. Wynn and Coolidge appeal to two theories from the cognitive sciences, one from cognitive neuropsychology and one from cognitive anthropology. In regard to the former, they use Ericsson and colleagues' (Ericsson and Kintsch, 1995; Ericsson and Delaney, 1999; Ericsson et al., 2000) cue-structure model for long-term working memory. In regard to the later, they use Keller and Keller's (Keller and Keller, 1996) phenomenological/action theory of skill-learning. By combining these two theories, Wynn and Coolidge create a powerful argument for their conclusion; one that links a specific cognitive module with a broader, behavioural account of skilled action.

Ericsson and colleagues develop their model of long-term working memory to account for a particular aspect of skilled performance. One feature of standard working memory is that information contained there can be lost very quickly. This effect is particularly strong in cases where the focus of a subject's attention changes. For example, if we are distracted while attempting to perform a working memory task then we find it very difficult to return to the task, as the relevant information is lost. Yet this is not the case with experts. The repetition of a task over time allows a subject to access and process information relevant to that task quickly and effectively, even when distracted. As the information is not lost, it would appear that the type of memory involved is long-term. However, this ability is restricted to the task that a subject has practiced. All this suggests a particular type of memory: one that is rapid, accurate, long-term and domain-specific (Wynn and Coolidge, 2004: 471).

The model Ericsson and colleagues use to account for long-term working memory appeals to *retrieval cues* and *retrieval structures*. Retrieval cues are items of knowledge that are capable of reinstating the conditions of its encoding, via association. A retrieval structure is a reliable, stable set of retrieval cues that an expert develops and maintains over time via task repetition. The idea is that, in virtue of this cue/structure model, the expert is able to very quickly access and use the relevant stored information required to perform a task.

Wynn and Coolidge, following (Ericsson and Kintsch, 1995), use the example of chess to illustrate this. Suppose you say the words 'Sicilian defence' to me. I am not a chess expert, and as a result all that comes to my mind is 'something to do with chess'. On hearing the same words, however, a chess expert will be able to immediately bring to mind the various positions of 32 pieces on a chess board. The idea is that the retrieval cue – in this case hearing the words 'Sicilian defence' – very quickly activates an encoded item of knowledge, such that the expert is not required to further encode aspects of a situation – in this case, the location of each piece on the board in front of them (Wynn and Coolidge, 2004: 471). The breadth of retrieval cues means that a retrieval structure can be employed in a wide variety of contexts, relative to the area of expertise. The result is a "powerful and flexible kind of thinking", that is nonetheless "limited to a narrow domain" (Wynn and Coolidge, 2004: 471).

The cue-structure model for long-term working memory applies broadly across all kinds of expertise (chess, music, etc.). But it remains to be seen how this model is supposed to apply in the specific case of technical craft expertise, such as that required to produce the Levallois. In particular, Wynn and Coolidge want to link the cue-structure model to a first-person account of technical skill. They appeal to Keller and Keller's behavioural account of skilled action to perform this bridging task. Keller and Keller's account attempts to describe the phenomenological character associated with performing a technical skill, and they use black-smithing as their example. The account uses three concepts to do this, which roughly correlate to knowledge, planning, and action/perception feedback.

The first concept is called the *stock of knowledge*. This consists mainly in a store of sensory information relating to performance of a task - the desired result and the procedures and materials required to attain that result. Wynn and Coolidge posit that this information is stored in long-term working memory. Keller and Keller call the second concept the *umbrella plan*. An umbrella plan consists of a mental representation of the overall result and the various steps required to attain that result. This plan is then executed through an action/perception feedback-loop, called a *constellation*. A constellation is composed of mental representations, sensory information, and indeed the tools and materials themselves (Keller and Keller, 1996: 91). Constellations are actively updated through the performance of a task, depending on the success or failure of each step. The idea is that, in cases of expertise, very little active reflection is required for constellations to be revised and updated.

The general picture Wynn and Coolidge suggest is that a particular task activates a retrieval structure stored in long-term working memory (the stock of knowledge). Further knowledge related to the task is initiated via retrieval cues. This as a whole makes up an umbrella plan. Finally, constellations link the umbrella plan/retrieval structure to tools and materials via a perception/action feedback loop. Importantly, Keller and Keller's model shows how mental images might act as retrieval cues, rather than the semantic cues that Ericsson and colleagues focus on. For Wynn and Coolidge, this is significant, as much of the stimulus for action in technical craft is non-semantic (Wynn and Coolidge, 2004: 473).

Wynn and Coolidge then apply this combined mid-range theory to the Levallois. Keller and Keller's account looks to map on to the process required to produce prepared-core technologies nicely. In particular, the process requires completing a three-stage process – constructing a striking surface, constructing a striking platform, and applying hard-hammer percussion – with the overall goal of producing a flake. In addition, both smithing and prepared-core technologies require that the craftsperson flexibly choose between a number of different strategies in order to attain their end goal, so there is no reason to think something radically different is going on across the two tasks. Consequently, Wynn and Coolidge conclude that the concepts of stock of knowledge, umbrella plan, and constellation apply to the production of the Levallois.

Finally, as we have seen, these concepts can be understood in terms of Ericsson's cue-structure model for long-term working memory. A knapper capable of producing prepared-core technologies organises a broad range of retrieval cues into retrieval structures capable of being deployed as the construction of the tool unfolds. Subtle cues such as the shape and angle of the striking face and platform, pressure ridges, and existing features of the specific stone in question activate information stored in long-term working memory. This allows the knapper to flexibly negotiate the sequence of procedures required to produce a usable flake. So, given we have good reason to think that the Neanderthals produced prepared-core technologies, Wynn and Coolidge conclude that we have good reason to think that Neanderthals possessed long-term working memory capacities.

2.2 Minimal-capacity inferences characterised

In the terminology of traced-based reasoning, the physical trace here is the Levallois tools associated with Neanderthals, the past state of affairs is the cognitive capacity of Neanderthals, and the mid-range theory is Ericsson and colleague's cue-structure model of long-term working memory combined with Keller and Keller's phenomenological/action account of technical skill.

Minimal-capacity inferences are an example of inference to the best explanation. The general schema is as follows: the best explanation of the presence of a trace, given the mid-range theory employed, is the presence of some past state of affairs. Put in the terms of Wynn and Coolidge's framework: the best explanation for the presence of prepared-core technologies at Neanderthal sites, given the cue-structure model of long-term working memory and the phenomenological/action account of technical skill, is that Neanderthal's possessed long term working memory capacities.[9]

It is worth flagging an interesting issue that emerges here. I previously noted that we must be careful not to infer beyond what the evidence suggests. This is why we must aim to

9 Here I divert somewhat from Currie and Killin's account, which builds a necessity clause into minimal-capacity inferences (Currie and Killin, 2019: 266/272). This suggests they are thinking of them as deductive claims. I am here characterising them as inductive, but I don't think too much hangs on this issue.

identify the *baseline* or *minimum* cognitive capacities required to produce a technology. This then leads to the question: what are the minimum cognitive capacities required to produce a technology?

In appealing to the cue-structure model, Wynn and Coolidge claim that long-term working memory is a baseline capacity with respect to the Levallois. But there are cognitive archaeologists who have argued that explanations such as these are too cognitively or computationally rich. These theorists emphasise enactive and embodied accounts of cognition, which attempt to account for skilled action without appeal to computational theories of cognition such as Ericsson's model. For these theorists, the best explanation for the production of prepared-core technologies is not long-term working memory. This speaks to a long-standing debate in cognitive archaeology – one that has spilled over from the cognitive sciences – between those committed to a computational theory of mind (Cole, 2016; Coolidge and Wynn, 2018; Mithen, 1996; Wynn, 1985, 2002) and those who argue for enactive, embodied, or Gibsonian frameworks (Davidson and Noble, 1989, 1993; Davidson, 2019; Malafouris, 2016; Overmann, 2016; Tomlinson, 2015). As a result, it would appear that our background theoretical commitments in the cognitive sciences are going to influence our assessment of the baseline capacities required to produce a tool.

I will not attempt to address this issue here. My goal now is to characterise a different kind of inference employed by cognitive archaeologists. This mode of inference is not based on a technology, but rather on a transition between two technologies.

3. Cognitive transition inferences

So far we have seen how cognitive archaeologists attempt to identify the cognitive prerequisites required to produce a technology. Here is a different question we might ask in cognitive archaeology: what can transitions in the Palaeolithic record tell us about the evolution of hominin cognition? Attempts to answer this kind of question involve the use of cognitive-transition inferences.

The Palaeolithic record is striking in so far as it is characterised by long periods of stasis, punctuated by periods of rapid change.[10] For instance, it is typically thought that the

10 The structure of the record as one characterised by stasis and change has been challenged (Kuhn, 2019). I agree with the general critique that periods traditionally described as 'static' - in particular, the Oldowan and the

Oldowan lasts for at least 0.9 million years (2.6-1.7 mya), before the rapid onset of the Acheulean. And although there are some changes in this period, in general the Oldowan exhibits remarkable conformity. The Acheulean begins at 1.7 mya and continues until the beginning of the Middle Palaeolithic at around 300 kya. There are significant changes in complexity over this time, but nonetheless the Acheulean likewise displays striking conformity. It seems natural to interpret this pattern in terms of changes in innate hominin cognitive sophistication. Very roughly put, the idea is that long periods of conformity signal periods of evolutionary stasis in hominin cognitive capacities, whereas transitional periods signal significant up-scales in those capacities. In section 2 I noted that, when attempting to identify the cognitive prerequisites required to produce a technology, we assume that those capacities played a causal role in the production of the technology. This then licences the inference from technology to cognitive capacity. The same applies here. In other words, the assumption is that there is a causal connection between a change in the sophistication of innate hominin cognitive capacities and a transition in the tool-record. In turn, this licenses an inference from the material record to a claim about a transition in the evolution of hominin cognition.

Like minimal-capacity inferences, cognitive transition inferences are an example of trace-based reasoning. However, they are somewhat more complicated. The trace we are using is a transition between two technologies, rather than a particular technology. Likewise, the past state of affairs we are targeting is a change in innate hominin cognitive capacities, not a particular cognitive capacity. Our mid-range theory will again be a theory from the cognitive sciences.

Cognitive-transition inferences build on minimal-capacity inferences. A cognitive-transition inference typically takes the following form:

1. Run a minimal-capacity inference on a technology, *Y*.
2. Run a minimal-capacity inference on a technology *X*, where the appearance of *X* predates the appearance of *Y* in the record.
3. Identify a capacity, *C*, that is the best explanation for *Y* but not *X*.
4. Infer that:
   a) *C* was **absent** during the period *X* was produced; and

Acheulean - in fact show significant variation and increasing complexity over time. Nonetheless, these periods do display striking standardisation of form.

b) *Y* **signals** the emergence of *C*.

The basic idea is that we identify two baseline capacities required to produce the technologies on either side of a transition, assume that the upscale in cognitive capacity caused the technological transition, and then infer that there has been an upscale in hominin cognitive capacity.

Recall that minimum-capacity inferences yield only a baseline reading of cognitive capacity. Cognitive-transition inferences give us significantly more. In particular, they produce a claim about *when* a capacity emerged.

3.1 Coolidge and Wynn on behavioural modernity

Let's now look at an example of a cognitive-transition inference in action. Coolidge and Wynn (2018) present a broad account of the evolution of hominin cognition. They argue that the archaeological and fossil record suggest three major transitions in this process (Coolidge and Wynn, 2018: 5). The first transition is signalled by the dispersal of hominins from woodland zones into a variety of different habitats. This is particularly associated with *Homo erectus*. Coolidge and Wynn claim this event demonstrates increased social cognition and spatial awareness capacities, which may have been driven by the move from arboreal sleep to ground sleep. The second transition is signalled by the dramatically increased encephalisation found in *Homo heidelbergensis*. This is associated with the emergence of big-game hunting.

The third transition Coolidge and Wynn propose will be our focus. The claim is that the transition to behavioural modernity occurs with the development of enhanced working memory. The hallmarks of human cognition, such as abstract reasoning, symbolic thinking and language processing are thought to be a product of this cognitive development. The event is marked in the archaeological record by the African Middle Stone Age and the European Upper Palaeolithic. Coolidge and Wynn's thesis is bold, and, if correct, a major advance in our understanding of the evolution of hominin cognition. In the remainder of this section I characterise the inferential strategy they use to derive their conclusion.

Coolidge and Wynn adopt Baddeley's model for working memory (Baddeley and Hitch, 1974; Baddeley, 2000, 2011). Baddeley's work is complex and has generated a huge secondary literature, but a basic overview will suffice for our purposes. Baddeley first developed his model for working memory in response to the short-comings of the existing, two-component model of memory (Baddeley and Hitch, 1974). This prevailing account split memory into two kinds: short-term memory and long-term memory. In particular, Baddeley was interested in developing a model that explained how short-term memory interacts with long-term memory. The model is composed of four parts: a central system, called the *central executive*, and three subsystems, called the *phonological loop,* the *visuo-spatial sketchpad* and the *episodic buffer*. The central executive controls the flow of information to and from the subsystems, and integrates and updates that information. It does this via functions such as attention, inhibition or impulse control, decision making and planning. The central executive also acts as the primary interface with long-term working memory. The phonological loop is a language module, the primary function of which is the temporary storage and manipulation of auditory verbal information. The visuo-spatial sketchpad allows for the temporary storage and manipulation of visuo-spatial information. The episodic buffer was added by Baddeley in 2000 (Baddeley, 2000). It integrates information from the phonological loop and the visuo-spatial sketchpad into a single episodic representation for use by the central executive.

As noted above, Coolidge and Wynn posit that the hallmarks of properly modern thinking – abstract and analogical reasoning, grammatical languages, symbolic thinking and autonoetic awareness – are dependent on the development of enhanced working memory. Working memory is 'enhanced' in so far as the system has increased capacity (Coolidge and Wynn, 2018: 234). But what caused this increase in capacity? Coolidge and Wynn suggest that "an additive genetic mutation or an epigenetic event that affected the neural organisation of the brain" produced enhanced working memory (Coolidge and Wynn, 2018: 233). In other words, they posit a change in innate hominin cognitive capacities. The task now is to identify when this change occurred. To do this, Coolidge and Wynn adopt a "more explicit archaeological approach" (Coolidge and Wynn, 2018: 240).

Figure 3. The Hohlenstein-Stadel figurine, 39-41 thousand years old. Source: Dagmar Hollmann; reproducible under the terms of the Creative Commons Attribution-Share Alike
4.0 International License. URL= https://upload.wikimedia.org/wikipedia/commons/4/ 4c/Loewenmensch1.jpg Accessed 20 August 2019.

In particular, they aim to identify particular behaviours that require enhanced working memory, and link these behaviours to traces found in the archaeological record. Their inferential strategy is complex, and they survey a wide range of archaeological evidence. Here I will focus on one strand of their overall argument – one that takes a particularly striking archaeological trace as its focus.

Consider figure 3. This is the famous Hohlenstein-Stadel figurine, often claimed to be the earliest known example of figurative art. For Coolidge and Wynn, the production of this artefact would have required the kind of abstract/analogical reasoning that is the hallmark of enhanced working memory. The figure is 30cm in height, and dates from between 39-41 kya. It depicts a lion-headed human, and so combines the concepts of 'lion' and 'human' into a single, abstract concept. This abstract concept must have been one that the maker conceived themselves, or perhaps shared with a broader community. Either way, producing this abstract concept required the use of analogical reasoning, which on Coolidge and Wynn's view requires enhanced working memory (Coolidge and Wynn, 2018: 250). The claim is that the concepts of 'human' and 'lion' reside in an evolutionarily old ability to quickly distinguish and classify aspects of the natural world (typically called *folk biology*). A 'lion-human' would not have been part of this conceptual repertoire. Rather, someone had to take the two concepts, hold them in attention, and then combine them. This task requires enhanced working memory, and in particular the development of a sophisticated central executive module. Coolidge and Wynn thus conclude that the Hohlenstein-Stadel figurine is a strong signal of the presence of enhanced working memory.

Is there evidence of this kind of thinking that pre-dates the HohlensteinStadel figurine? Coolidge and Wynn note that shell-beads discovered by Henshilwood at Blombos cave in South Africa were probably used to symbolise social status (Henshilwood et al., 2002; Henshilwood and Dubreuil, 2011). The claim is that the beads might have been used to mark membership of a particular group within in a social hierarchy. If so, this suggests that a distinctly modern type of thinking, namely symbolic thinking, had emerged as far back as 77 kya. However, Coolidge and Wynn are reluctant to attribute enhanced working memory capacities to the makers of these artefacts. They do not see representing membership of a particular group, or social-strata, as requiring the same level of abstract concept manipulation as that required to produce the Hohlenstein-Stadel figurine. In particular, symbolic representation of this kind does not combine or subordinate concepts or categories in a way that is implied by the Hohlenstein-Stadel figurine. Furthermore, Coolidge and Wynn argue that we can give an account of this kind of symbol use that appeals only to associational learning (Coolidge and Wynn, 2018: 262-263). They conclude that we should withhold ascribing enhanced executive function to the inhabitants of Blombos cave.

This yields a striking conclusion:

> The archaeological evidence for modern working memory capacity presents us with a surprisingly shallow time depth; indeed nothing convincing (cognitively valid and archaeologically credible) clearly antedates the 40,000-years-ago date of the Hohlenstein-Stadel [figurine]... The parsimonious interpretation is that modern executive functions *did not emerge much earlier* than 40,000 years ago. (Coolidge and Wynn, 2018: 260, my emphasis)

The important elements of Coolidge and Wynn's view are summed up succinctly in the following passage:

> Our third proposed leap in cognition [enhanced working memory] led to completely modern thinking... The archaeological record for this leap is dramatic... To *explain this*

leap, we propose that a *neural event* occurred that led to a reorganisation of the brain to *enable fully modern thinking*. (Coolidge and Wynn, 2018: 5; my emphasis)

So the claim is that a change in innate hominin cognitive capacities caused a transition in the complexity of the Palaeolithic record. This then licences an inference from technological transition to a claim about cognitive evolution.

Now, recall that our schema for cognitive transition inferences is as follows:

1. Run a minimal-capacity inference on a technology, *Y*.
2. Run a minimal-capacity inference on a technology *X*, where the appearance of *X* predates the appearance of *Y* in the record.
3. Identify a capacity, *C*, that is the best explanation for *Y* but not *X*.
4. Infer that:
   a) *C* was **absent** during the period *X* was produced; and
   b) *Y* **signals** the emergence of *C*.

In the case of Coolidge and Wynn's inference, we would fill this out as follows:

1. Enhanced working memory is required to produce the Hohlenstein-Stadel figurine.
2. Enhanced working memory is not required to produce the shell-beads found at Blombos cave.
3. Enhanced working memory is the best explanation for the Holhenstein-Stadel figurine but not the shell-beads.
4. Consequently:
   a) Enhanced working memory was **absent** during the period the shell-beads were produced; and
   b) The Hohlenstein-Stadel figurine **signals** the emergence of enhanced working memory.

This inferential strategy carries an implicit theoretical commitment, which I will call the *innate-cognition first view*. According to this view, the best explanation for transitions in the complexity of the Palaeolithic record are upgrades in innate hominin cognitive sophistication.[11] This is, in part, a product of the fact that cognitive-transition inferences build on minimal-

11 Another implicit commitment embedded in Coolidge and Wynn's inference is something like the following: given a large enough time-scale and a diverse range of environments, a capacity will show up in the material record. In other words, adaptation pressures are such that we can reasonably expect to see a signal of a cognitive trait if it exists. Thanks to Kim Sterelny for helpful discussion here.

capacity inferences. In Coolidge and Wynn's case, the idea is that a genetic event led to a neural reorganisation, which brought about enhanced working memory. The presence of enhanced working memory then produced the diversity in material culture we see in the European Upper Palaeolithic. The innate-cognition first hypothesis is nicely summed up by Kuhn:

> The story of technological evolution during the Palaeolithic is typically told as a tale of gradually increasing complexity of artefacts and production processes. This trend is assumed to be a *direct function* of the cognitive sophistication of human ancestors. Hominins got smarter over time, which enabled them to make more complex, and presumably more effective artefacts. (Kuhn, 2019)

However, we can follow Kuhn in asking: is this the right story to tell about technological transitions? The following two sections will address this issue. Section 4 will look at evidence that does not appear consistent with the innate-cognition first hypothesis. Section 5 will look at two alternative hypotheses which can account for this evidence.

4. Evidence against innate-cognition first views

The question facing us now is as follows: are there reasons to be sceptical of the innate-cognition first hypothesis? I think that there are.

The first reason is a well-known one. A puzzling feature of the Palaeolithic record is that periods of transition and conformity do not appear to correlate with hominin speciation and, in particular, increased encepahalisation. If the innate-cognition first hypothesis were true, we would expect to see some kind of rough pattern emerging between hominin morphological evolution (particularly with respect to the neocortex) and transitions in the complexity of the tool-record. But this does not seem to be the case. For instance, the emergence of prepared-core technologies in the middle Palaeolithic does not coincide with any morphological change, and nor does the African Middle Stone Age. The same holds for the European Upper Palaeolithic – very large-brained hominins had existed for at least 150,000 years prior to this event. There does appear to be a rough correlation between the emergence of *Homo erectus* and the onset of the Acheulean. But apart from this exception, the pattern we see between hominin morphological change and transitions in the complexity of the tool record is not one that lends support to the innate-cognition first hypothesis.

And indeed, a more fine-grained assessment of the Palaeolithic record raises further problems. Technologies display significant geographic variation at similar time periods, with no correlation between hominin taxa with different degrees of encephalisation. Another problem is the appearance and disappearance of various technologies; including fire, microliths and material symbols. Another is that new technologies do not appear to emerge in spatiotemporal hotspots; rather they emerge – and sometimes disappear – with no apparent relation to morphological change in the hominin lineage. Again, none of these patterns are what we would expect if technological change were indeed a 'direct function' of the neural power of bigger-brained species of hominins.

A particularly worrying case for the innate-cognition first hypothesis are situations in which we see a backwards trend in the complexity of a technology (Jagher, 2016; Saragusti et al., 1998). Jagher's (2016) findings at Nadaouiyeh Aïn Askar in Northern Syria is a particularly striking example of this phenomenon. Jagher undertakes an analysis of hand-axes at the site over an occupation period of 225,000 years (550 kya to 325 kya). Here the oldest occupation of the site presents the most complex, sophisticated examples of hand-axes, which then become less standardised, less elaborate and more irregular over time. Furthermore, flaking accidents become more common. Jagher notes that it is unlikely that these later, less complex examples are the result of re-working more sophisticated hand-axes; rather, they appear to have been conceived as we find them from the beginning of their production. What we have here then is a long-term backwards trend in the complexity of a technology. Again, this is not a result we would expect were it the case that the primary driver of technological complexity was innate hominin cognitive capacity.

5. Alternative explanations for technological transitions

So it looks as though there are good reasons to be generally sceptical of the innate-cognition first hypothesis. Our question now is: are there other explanations for transitions in the Palaeolithic record that can account for this evidence? In particular, we are interested in whether there are explanations for the Upper Palaeolithic other than Coolidge and Wynn's innate-cognition first hypothesis. Here I outline two such explanations.

5.1 Demographic explanations

There is a growing body of research that supports the idea that change in the complexity of material culture is heavily influenced by demographics (Henrich, 2004; Powell et al., 2009; Premo and Kuhn, 2010; Richerson and Boyd, 2013; Sterelny, 2007; Sterelny and Hiscock, 2014). According to this view, population size can both inhibit and accelerate the rate of technological change.

Premo and Kuhn's 2010 paper gives a nice example of the former. The question they begin with is the following: why do early technologies (particularly the Oldowan and Acheulean) persist without change for so long? According to the innate-cognition first hypothesis, this would be explained by the fact that the innate cognitive capacity of the makers of these technologies remained relatively static. One suggestion along this line is that Oldowan and Acheulean tool-makers were unable to innovate (Klein, 2000; Wynn and Coolidge, 2004). Premo and Kuhn reject cognitive explanations, and instead produce modelling that suggests the stability found in early technologies might be a product of high rates of extinction in local sub-populations. This makes it difficult for populations to maintain their informational resources across generations. Roughly put, the less heads you have in which to store knowledge, the more likely you are to lose that knowledge. Consequently, it is plausible that "... the remarkable stability of Middle Palaeolithic and earlier "cultures" is not to be found in their makers' capacities for innovation and change, but rather in the demographic fragility of the small social groups in which they lived." (Premo and Kuhn, 2010: 8). So according to Premo and Kuhn, demographic factors can play a role in explaining the rate of technological change. If this story turns out to be right, then the conformity we see in earlier technologies has little to do with innate hominin cognitive capacities. Consequently, we should be wary of running any inferences from technological complexity to cognitive sophistication.

It has also been argued that socio-demographic factors can explain accelerated rates of change in material culture. In particular, it has been suggested that demographic changes might explain the expansion of diversity in material culture from 100 kya that is commonly associated with behavioural modernity (the African Middle Stone Age and the European Upper Palaeolithic). As we have seen, innate-cognition first explanations of this data face a problem: anatomically modern humans predate this expansion by some 150,000 years, and very large-brained hominins existed for at least 400,000 years prior. So it looks as if an upgrade in innate cognitive capacity does not explain this transition in the complexity of material culture. So what does? One suggestion is that larger populations enabled hominids to store and transmit their informational resources with more stability, such that these resources were no longer susceptible to attrition through extinction (Sterelny and Hiscock, 2014). If this is right, then the increased rate of change in technological complexity from 100 kya was caused by demographic factors, not by increased cognitive capacity. Again, this creates problems for inferences that run directly from technological change to cognitive sophistication: what looks like a signal of increased cognitive capacity turns out to be a signal of increased population.

Powell and colleagues have suggested an even more direct link between population size and technological complexity (Powell et al., 2009).[12] According to the *social-scale hypothesis*, the complexity of technology that a population can maintain is a function of its size. Powell and colleagues produce modelling to show that skill levels act as a constraint on technological complexity. In turn, skill levels are constrained by cultural learning. Effective cultural learning depends on two factors: (i) how noisy the information flow from expert to novice is; and (ii) how the spread of aptitude among experts and novices. As the spread of aptitude among experts and novices emerges from normal variation within a population, aptitude is sensitive to social-scale. Consequently, as a technology becomes more complex, the minimal social-scale at which that complexity can be maintained over successive generations goes up. So there is a very direct relationship between population size and technological complexity. Powell and colleagues think that the social-scale hypothesis can be used to explain those aspects of the Palaeolithic record linked to behavioural modernity: "estimates of regional population size over time show that densities in early Upper Palaeolithic Europe were similar to those in sub-Saharan Africa when modern behaviour first appeared. Demographic factors can thus explain geographic variation in the timing of the first appearance of modern behaviour without invoking increased cognitive capacity." (Powell et al., 2009: 1298) So, contra Coolidge and Wynn's innate-cognition first hypothesis, Powell and colleagues argue that demography accounts for the transition into the European Upper Palaeolithic.

Are demographic explanations for technological transitions better able to explain the evidence that counts against the innate-cognition first hypothesis? To begin with, if demographic accounts are on-track then the lack of correlation between hominin encephalisation and transitions in technological complexity is not mysterious. Rather, periods of long-term conformity and rapid change in the tool-record are explained as a product of population size. The demographic hypothesis also handles the phenomena of disappearing and reappearing technologies better than the innate-cognition first hypothesis. We would expect to

12 See also Henrich (2004).

see complex technologies such as fire and microliths disappear and reappear as population sizes fluctuate. And if the demographic hypothesis is true, then we *do* see spatio-temporal hotspots; it's just that these are correlated to population size rather than speciation and encephalisation. Finally, Jagher's findings at Nadaouiyeh Aïn Askar can be explained in terms of a decreasing population. Long-term backwards trends in complexity would simple indicate that a declining population was unable to maintain the baseline complexity of its technology over time. So it looks as though demographic hypotheses can account for the evidence that creates problems for the innate-cognition first hypothesis.[13]

5.2 Environmental explanations

It has become increasingly clear over the past 50 years that environmental factors exert a strong effect on the complexity of a tool-kit. Consequently, another challenge to the innate-cognition first hypothesis is the claim that transitions in technological complexity are caused by pressure to adapt to environmental conditions. Perhaps the strongest example of this effect is the latitudinal gradient in technological complexity (Collard et al., 2005; Torrence, 1983, 1989, 2001). Torrence's work in particular has shown that there is a positive and significant correlation between latitude and the complexity of hunter-gatherer tool-kits. Her initial explanation of this data was time-stress. She suggested that, given the number of edible plants decreases as latitude increases, populations at higher latitudes would rely more heavily on animal resources for food. As hunting is a more time-consuming exercise than plant gathering, these populations would be under greater time-stress. There would thus be pressure to produce more specialised tools, in order to engage more efficiently with their environmental circumstances. Consequently, their tool-kit would become more complex. Torrence later revised this hypothesis to focus on risk (Torrence, 1989, 2001). The idea is that populations in higher latitudes face greater consequences if food resources are not secured. Populations that experience high resource failure risk will produce more specialised tool-kits in order to mitigate that risk. Conversely, populations that do not experience high resource failure risk (i.e. those in lower latitudes) will require a much less specialised tool-kit. So the complexity and diversity of tool-kits is heavily influenced by environmental conditions.

The important point here is that we see large variation in the complexity of material culture that is not explained by innate cognitive capacities. Torrence's sample populations were

13 It should be noted that it is very difficult to produce estimations of population sizes in the distant past. As a result, there are very real issues when it comes to testing demographic hypotheses.

extant hunter gatherer groups, so there was no difference in terms of genetic potential for cognitive sophistication. Any variation in technological complexity appears to be the result of expediency given environmental circumstances. This has implications for the innate-cognition first hypothesis: if environmental conditions can influence the diversity and complexity of a population's tool-kit, then the move from transitions in material culture to transitions in cognitive sophistication is far less straightforward than cognitive-transition inferences would suggest. More specifically, if we take it that the cause of transitions in technological complexity is innate cognitive capacity, then we might be tempted to infer a transition in cognitive sophistication from a transition in the complexity of material remains, when in fact the explanation for that transition is purely environmental.

Can the environmental hypothesis account for the evidence that creates problems for the innate-cognition first hypothesis? It certainly seems so. If environmental factors exert as much influence on the complexity of tool-kits as proponents think, then we might not expect to see much correlation between encephalisation and technological transitions. In addition, disappearing and reappearing technologies might simply be understood as adaptations to changing environments. Likewise there would be less reason to expect to see spatio-tempral hotspots that relate to speciation in the hominin lineage. Rather the hotspots we do see might relate to environmental fluctuations. Finally, we might explain long-term backwards trends in technological complexity as a function of changing environments. Conditions at Nadaouiyeh Aïn Askar may have originally been such that resource-risk was mitigated by producing more elaborate, standardised hand-axes. Over time, these conditions may have changed such that more roughly created versions would suffice. So it seems that the environmental hypothesis can be used to explain the evidence that creates problems for the innate-cognition first hypothesis.

It is perhaps less clear how the environmental hypothesis might account for the changes in the record we see in the Upper Palaeolithic. Artefacts such as the Hohlenstein-Stadel figurine and the beads at Blombos cave are not tools, and this lack of utilitarian function might be thought to exclude them from the environmental hypothesis. However, I think there are ways in which we might understand the Upper Palaeolithic as a product of environmental changes. One thing abstract and symbolic artefacts do plausibly signal is increased social complexity, and this is often associated with increased group size (Carneiro, 1967; Kosse, 1990). It has been argued that increased group size is correlated with a population's ability to access a greater variety of food resources (Flannery et al., 1969; Stiner, 2001). Stiner (Stiner et al., 1999, 2000) in particular has argued that variations in small game hunting track population pulses in the Late Middle and Early Upper Palaeolithic. Specifically, it looks as though populations during this period relied less on slow moving prey (in particular, tortoises) and more on faster moving prey (for instance, hares).

But it remains to be seen whether increased population size caused these changes in diet, or whether the changes in diet caused an increase in population size. If the latter turns out to be true, then this provides one route to understanding how environmental conditions might bring about the circumstances required to produce items like the Hohlenstein-Stadel figurine and the Blombos cave beads. The idea is that an environmentally adaptive (dietary) change brought about an increase in population size, which produced the social complexity required to bring about the production of such artefacts. If this kind of hypothesis is on track, then enhanced working-memory may significantly pre-date the Hohlenstein-Stadel figurine. So it looks as though the environmental hypothesis can account for the data that Coolidge and Wynn explain using the innate-cognition first hypothesis.

5.3 A dilemma

All this leads to a dilemma. On one horn of the dilemma, we continue to use cognitive-transition inferences and attempt to defend the innate-cognition first hypothesis against the counter-evidence. This is unappealing, as it seems implausible that neither environmental nor demographic factors play *any* causal role in technological transitions. On the other horn of the dilemma, we abandon cognitive-transition inferences. This is unappealing, as it seems transitions in cognitive sophistication must play at least *some* role in producing transitions in technological complexity. More generally speaking, it looks as if there is a trade-off here between generating a testable hypothesis and recognising the causal complexity involved in the production of a phenomenon. Furthermore, cognitive-transition inferences are an important part of the cognitive archaeologist's inferential toolkit. They offer something that minimal-capacity inferences alone cannot: namely an estimation of *when* a cognitive trait emerged. So: can we rescue cognitive-transition inferences? This is a difficult question, and not one I could hope to address here. Rather, in the remainder of the paper, I outline some conceptual tools from the philosophical literature which may help shed some light on the issue.

6. Discussion

The problem at hand is as follows. It is plausible that there were significant transitions in the evolution of hominin cognition, and transitions in the complexity of the tool-record seem an important line of evidence about this story. But the demographic and environmental hypotheses suggest that precisely what transitions in the complexity of the tool-record carry information about is unclear: they might signal a population increase or an environmental change, as well as a change in cognitive capacity. In other words, cognitive-transition inferences might in fact be demographic or environmental-transition inferences. One approach we might take here would be to accept this result. This is an interesting line, and it would be productive to consider how precisely one might analyse the tool-record using demographic or environmental models as one's mid-range theory. Most of the work done on the demographic and environmental hypotheses has been in the service of explaining periods of conformity or change in the tool-record. If these hypotheses are on track, then we might start to think more actively about using the record to infer claims about fluctuations in population size or environmental conditions.

However, taking this line would sideline the cognitive aspect of inferences generated from technological transitions; and this is not particularly satisfying for those of us interested in telling a story of the evolution of hominin cognition. If we want to keep cognitive-transition inferences cognitive, then we had better come up with a different response. As mentioned previously, a proper response to this problem is beyond the scope of this paper. Instead, in the next two subsections I briefly outline some conceptual tools that may be used to help develop such a response. The first looks to the notion of multiple lines of evidence. Depending on how the data pans out, such a move could bolster cognitive-transition inferences of the kind made by Coolidge and Wynn. The second looks to the philosophical literature on causality. I outline a set of concepts that can help us to think about situations in which there are multiple causal factors influencing a particular phenomenon. This at least gives us a theoretical framework via which to assess the problem.

6.1 Multiple lines of evidence and causal-association inferences

Currie and Killin suggest one important way of responding to this problem (Currie and Killin, 2019: 266). They distinguish between minimal-capacity inferences and *causal-association inferences*. Causal-association inferences use multiple lines of evidence in order to get at their target. Currie and Killin use the claim that adornment technologies are a signal of increasing social complexity (Kuhn, 2014) as an example. Kuhn argues that the increased levels of ochre and pigment and more complex grave goods signal increasingly complicated social dynamics.

Killin (2018) adds musical instruments to this picture. The idea is that by broadening the range of artefacts that serve to evidence Kuhn's proposal, we increase its plausibility.

Importantly, our multiple lines of evidence do not have to come from the archaeological record. An excellent example of this is the work of Stout and colleagues (Stout and Chaminade, 2007; Stout et al., 2008; Stout, 2011; Stout and Chaminade, 2012). In building a co-evolutionary hypothesis linking the emergence of language and tool-making, Stout and colleagues appeal to data from neuroscience, neuroanatomy, fMRI studies, experimental archaeology, evolutionary biology and ecology, in addition to traditional cognitive archaeology. The point here is a straightforward one: when we have multiple potential causal factors influencing the emergence of some phenomenon, then the more lines of evidence we can appeal to the better. In particular, multiple lines of evidence can give us reason to favour one hypothesis – cognitive, demographic or environmental – over another.

Causal-association inferences are common practice in cognitive archaeology. Indeed, Coolidge and Wynn appeal to factors such as the heritability of enhanced working memory to bolster their case (Coolidge and Wynn, 2018: 233). It would be interesting, however, to see causal-association inferences used in an attempt to rule-out two of these three competing hypotheses in favour of a third. In particular, if we could develop a strong causal-associative case for Coolidge and Wynn's inference, and at the same time argue that the causal-associative cases for the alternative hypotheses are weaker, then we would have good reason to favour their innate-cognition first approach. Recall that we use two minimal-capacity inferences on either side of a technological transition to pick out an upgrade in cognitive capacity. The idea here is that we can use multiple lines of evidence to strengthen the case that the upgrade was the cause of the transition.

Of course, whether or not this vindicates the use of a cognitive-transition inference with respect to a particular technological transition will depend on whether or not the other lines of evidence vindicate the inference. However, if it is plausible that at least some technological transitions are influenced by increased hominin cognitive sophistication, then multiple lines of evidence is one way of adjudicating such situations.

6.2 Difference-making causes

The general schema of the problem facing us is an old one: how do we pick out the primary cause of some phenomenon if there are multiple causal factors contributing to the production of that phenomenon? If we are attempting the task of identifying which cause – cognitive, demographic, or environmental – was primary in producing a technological transition, we must have some kind of conceptual framework to assess the situation. To begin with, we need to get clear about what is meant by ‘primary cause’? One way to think about this is to use the notion of a *difference-making cause*. Waters (2007) outlines some useful concepts for distinguishing types of difference-making causes. In the remainder of the paper I apply these concepts to the problem at hand.

Waters illustrates a general philosophical puzzle by asking us to consider striking a match (Waters, 2007: 551-552). In this situation, we are inclined to say that the cause of the match being lit was my striking the match. The reasoning for this is counter-factual: had I not struck the match, then the match would not have lit. Consequently, we conclude that striking caused the match to light. The puzzle emerges when we consider that the same reasoning applies to a range of other factors influencing the lighting of the match. For instance, were there no oxygen in the room, the match would not have lit. Likewise, were there no rough surface on which to strike the match, the match would not have lit. So it appears oxygen and a rough surface were also causes of the match being lit. Nonetheless, we intuitively identify my striking the match as the cause that made a difference. The challenge is to come up with a way of vindicating this intuition.

In addressing this challenge, Waters introduces the concepts of *a potential difference-making cause*, *the actual difference-making cause*, and *an actual difference-making cause*. A *potential* difference-making cause is a cause in so far as changing the value of one variable is associated with changes in the value of another. For instance, my striking or not striking the match is associated with the match being lit or remaining unlit. Likewise, the presence of oxygen in the room or the lack of oxygen in the room is associated with the match being lit or remaining unlit. To be a potential difference-maker, then, is just to satisfy the type of counter-factual reasoning outlined above. As a result, counter-factual reasoning is limited to picking out potential difference makers, and it “... does not matter whether the causal variable actually varies in any actual population and whether this variation brings about actual differences” (Waters, 2007: 568). If we are to address our puzzle then, we need something stronger.

For this job, Waters introduces the notion of *the* actual difference-making cause. To be the actual difference-making cause “... the value of the variable must actually differ, and this variation must bring about the actual difference...” (Waters, 2007: 568). When I strike the match, there is no change in oxygen levels in the room, nor is there a change in the number of rough surfaces in the room. The variable that has changed is whether or not the match has been struck. So striking the match, oxygen levels and rough surfaces are all potential difference-makers, but the actual difference-maker in this case was striking the match. The concept of the actual difference-maker thus allows us to pick out the cause that actually made a difference in a way that counter-factual reasoning cannot.

We can then start to use these concepts to think about the problem at hand. For instance, cognitive, demographic and environmental factors are all potential difference-makers when it comes to transitions in the Palaeolithic record. For defenders of the innate-cognition first hypothesis, changes in the sophistication of innate hominin cognitive capacities are *the* difference-making cause for technological transitions. On this view, upgrades in cognition are analogous to striking the match, and the match being lit is analogous to technological transitions. Meanwhile, environmental and demographic factors are background conditions – analogous to the quantity of oxygen and rough surfaces in the room. For defenders of demography or environment first hypotheses, however, changes in population size or environmental conditions are the difference-making cause for technological transitions. On their view, these factors are analogous to match-striking and upgrades in cognitive capacity are a background condition.

However, it is of course plausible that *all* of these factors have an influence on transitions in technological complexity. Here too Waters' framework can help. In cases where there is no single actual-difference maker, but instead a set of actual difference-makers, Waters introduces the concept of *an* actual difference-maker. For instance, a plausible view is that cognitive, demographic and environmental changes are all associated with transitions in technological complexity. On this view, these are all actual difference-makers. Each one can be described as *an* actual difference-maker. To draw the analogy here, we have to adjust the match-lighting scenario. Suppose there was a change in oxygen levels in the room (from levels that would not sustain a match to levels that would), and that there was a change in the number of rough surfaces in the room (from none to one), and finally that the match was struck. In this case, oxygen levels, number of rough surfaces and match-striking are all difference-making causes. Likewise, in the scenario where changes in cognition, demography and environment all effect a technological transition, then all would be difference-making causes.

Waters' framework gives us a way of delineating these different views, and of getting clear about what we mean when we talk about 'primary' cause/s. The task from here is to begin assessing specific transitions in the tool-record according to this framework, using it to clarify competing hypotheses.

7. Conclusion

My goals in this paper have been threefold: to characterise cognitive-transition inferences; to show that they lead to a dilemma; and to make some suggestions as to how we might begin to address that dilemma. I started by distinguishing two projects we might pursue in cognitive archaeology. The first takes a particular technology and attempts to identify the cognitive capacities required to produce that technology. The second takes a transition in the tool-record and attempts to derive a conclusion regarding a transition in the evolution of hominin cognition. The former project employs minimal-capacity inferences, while the latter employs cognitive-transition inferences. I then argued that cognitive-transition inferences build on minimal-capacity inferences, and that this reflects an implicit commitment to the innate-cognition first hypothesis. Next I surveyed a range of evidence that undermined the innate-cognition first hypothesis, and outlined two alternative hypotheses that might account for that evidence: the demographic hypothesis and the environmental hypothesis. I argued that taking this evidence seriously means either rejecting cognitive-transition inferences or defending the innate-cognition first hypothesis. Finally, I outlined some conceptual tools from the philosophical literature which might be of use in developing a response to this problem: multiple lines of evidence and the notion of difference-making causes. The challenge from here is figuring out how we can apply these tools to the empirical data in order to tease apart the competing hypotheses. Hopefully this will allow at least some inferences from technological transitions to remain 'cognitive'.

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Statement of Contribution

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CHAPTER 2

Cognitive Archaeology & the Minimum Necessary Competence Problem

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Abstract: Cognitive archaeologists attempt to infer the cognitive and cultural features of past hominins and their societies from the material record. This task faces the problem of *minimum necessary competence*: as the most sophisticated thinking of ancient hominins may have been in domains that leave no archaeological signature, it is safest to assume that tool production and use reflects only the lower boundary of cognitive capacities. Cognitive archaeology involves selecting a model from the cognitive sciences and then assessing some aspect of the material record through that lens. We give examples to show that background theoretical commitments in cognitive science which inform those models lead to different minimum necessary competence results. This raises an important question: what principles should guide us in selecting a model from the cognitive sciences? We outline two complementary responses to this question. The first involves using *independent* lines of evidence to converge on a particular capacity. This can then influence model choice. The second is a broader suggestion. Theoretical diversity is a good thing in science; but is only beneficial over a limited amount of time. According to recent modelling work, one way of limiting diversity is to introduce extreme priors. We argue that having a broad spectrum of views in the philosophy of cognitive science may actually help cognitive archaeologists address the problem of minimum necessary competence.

1. Introduction: taking on the sceptics

Around six or seven million years ago (mya) the human and chimpanzee lineages diverged from a common ancestor. And then something remarkable happened: in an incredibly short period of (evolutionary) time, the hominin lineage inhabited almost every ecological niche on the planet, produced increasingly advanced material culture, and developed a range of complex cognitive and behavioural traits. To understand the details of this story is to understand the forces that shaped the human mind. However, understanding those details requires making inferences into the deep past, and there are limited lines of evidence by which we can do this.

One line of evidence we can appeal to is the stone-tool record. This is the core business of evolutionary cognitive archaeology: using stone tools to infer aspects of hominin cognitive and social evolution.[14] Cognitive archaeology involves taking models from the cognitive and social sciences and analysing the stone-tool record through that lens. While we think that hominin cognition and sociality co-evolved and should not be artificially separated, in this article we focus more on the cognitive dimension of this research program. Classic examples include Noble and Davidson's appeal to Gibsonian ecological psychology in developing an argument for the evolution of language (Noble and Davidson 1996), Mithen's appeal to Fodorian modularity in developing a general account of the evolution of hominin cognition (Mithen 1996), and Coolidge and Wynn's appeal to Ericsson and colleagues' model of long-term working memory in accounting for behavioural modernity (Coolidge and Wynn 2011).

Cognitive archaeology has had prominent sceptics, according to whom the evolution of cognition is not an adequately testable scientific endeavour. Richard Lewontin provides a succinct example of this sentiment:

> … the best lesson our readers can learn is to give up the childish notion that everything that is interesting about nature can be understood. History, and evolution is a form of history, simply does not leave sufficient traces… It might be interesting to know how cognition (whatever that is) arose and spread and changed, but we cannot know. Tough luck. (Lewontin 1998, p. 130)[15]

Responding to sceptics like Lewontin requires developing a robust, reliable inferential framework through which to practice evolutionary cognitive archaeology. This requires identifying and addressing the problems that this task confronts. The goal of this article is to examine one such problem.

[14] Of course, it is not the only task of evolutionary cognitive archaeology. But there is a stone tool bias in the material record, and consequently in archaeological discussions.

[15] See also Smith and Wood (2017); Bolhuis and Wynne (2009). For a general summary (though Currie is an optimist) see Currie (2018, pp. 16-18), and for further discussion see Currie and Killin (2019).

This problem begins with an old challenge facing archaeological practice in general - that of *minimum necessary competence*. According to Wynn and McGrew, "[in] prehistory, one can assess only the minimal abilities needed to produce the archaeological patterns uncovered." (Wynn and McGrew 1989, p. 384). This proposition follows from a plausible assumption: given that the most sophisticated thinking/behaviours of a population may not have been the kind of activity that produces lasting material traces, researchers can only ever infer a baseline reading of that thinking/behavior. In other words, we must assume that the archaeological record signals only the lower boundary of hominin cognitive capacity (Wynn 1993, 2002; Toth and Schick 1994).

However, as cognitive archaeology involves selecting a model from the cognitive sciences, researchers must endorse, at least implicitly, certain background commitments in the philosophy of cognitive science. Here we show that the different background commitments of competing models will produce different results regarding the hypothesised minimal capacities required to produce a technology. As a preliminary example, consider the difference between cognitive theories drawing on computational, information-processing models, and those drawing on embodied, enactive, embedded and extended (4E) models. According to the former, the kind of skilled action required to produce a technology (such as Acheulean stone handaxes or cleavers) might require cognitive capacities such as mental models or templates, long-term working memory, cognitive control, language, or theory of mind (e.g. Cole 2016; Planer 2017). According to the latter, the same actions might be accounted for using purely embodied, anticipatory processes or enactive perception/cognition via associational learning (e.g. Malafouris 2013; Ingold 2013; Tomlinson 2015). And it is typically thought that the former explanations invoke more cognitively sophisticated capacities than the latter. So different theoretical paradigms in cognitive science will return different results regarding the minimum necessary processes required to produce the traces we find in the archaeological record.

This generates important methodological questions: which principles should cognitive archaeologists follow in selecting their cognitive model, given that the results of their study will depend to some degree on the background commitments of the particular theory they choose? And what should cognitive archaeologists take the minimum necessary capacities required to produce some artefact to be, given that this will influence theory choice? We believe the best approach to these ouroboric questions involves deploying multiple complementary responses. Here we offer two. The first is to appeal to multiple, independent lines of evidence in order to converge on a particular capacity. If independent lines of evidence converge on a capacity best described by (say) one set of theories, then all else being equal, we should use that convergence as a basis for inference to the best explanation and provisionally endorse those theories. The second is to embrace methodological pluralism. This point is most obvious in cases where there is yet no convergence: as researchers debate over theories it can be fruitful to follow up many options. And there has been some modelling to show that the introduction of extreme priors can aid researchers to converge on the truth (Zollman 2010). Interestingly, this is so even when there is some convergence. As the history of science shows us, scientific consensus sometimes 'locks onto' falsehoods all too easily. As such, it may turn out that inheriting a diversity of background theoretical commitments from the cognitive sciences will ultimately benefit the program of cognitive archaeology as it progresses towards a narrowing of the hypothesis space.

We proceed as follows. Section 2 outlines this problem in more detail. Section 3 provides some examples of the problem in action. In Section 4 we outline our two responses. First we provide a case study (Acheulean stone tool production) detailing how multiple lines of evidence, including the use of 'Kon-Tiki experiments' (Novick et al. 2020), can in principle converge on some capacity. Then we provide our argument for methodological pluralism and discuss its implications for cognitive archaeological research. Section 5 concludes.

2. The problem outlined

In their classic 1989 paper, Wynn and McGrew argue that the producers of the then earliest known stone tools - the Oldowan, which dates back some 2.6 million years - were more ape-like in their cognitive capacity than had previously been thought (Wynn and McGrew 1989).[16] They did this by analysing various characteristics of the Oldowan, and comparing those to key features of tool-use in modern chimpanzees.[17] However, their analysis was subject to the minimum necessary competence problem. That is, an important objection to their view was that the makers of the Oldowan may well have exercised their most sophisticated thinking in ways that are archaeologically invisible, whereas researchers typically take tool production and

[16] The recent discovery of the even earlier stone tool industry (dated to 3.3 mya, designated the 'Lomekwian') was not published until 2015.
[17] This built on important earlier work by Wynn (e.g. 1981). McGrew, Wynn and collaborators have recently expanded this assessment to include some monkeys; in particular capuchins and macaques (McGrew et al. 2019).

usage to represent the upper range of chimpanzee cognitive abilities. As Wynn and McGrew put it, this problem "... is especially maddening for the Oldowan, because, as we seek to show in this article, the minimum competence needed to produce Oldowan tools and sites is that of an ape adaptive grade" (Wynn and McGrew 1989, p. 384).

Wynn and McGrew preempt this objection in order to push back against it. In doing so, they created a principle that has exerted a lasting influence on evolutionary cognitive archaeology. Their response in worth quoting in full:

> One solution to this problem is to assume the existence of behaviour that would have required a more human-like competence, but which is not evident in the archaeological record. We see no reason to assume the existence of such behavioural patterns, as the point of scientific archaeology is to use evidence, not make assumptions. Thus, for the purposes of this exercise, we accept minimal competence as being a reliable description of Oldowan behaviour. (Wynn and McGrew 1989, p. 384)

This response requires some unpacking. Wynn and McGrew begin by suggesting that one approach would be to simply assume the existence of more complex cognitive capacities (compared to those of a great ape baseline), despite the fact that, in their view, these are not evidenced in the archaeological record. This is certainly a live option for those who wish, for whatever reason, to preserve the notion that the hominins who produced the Oldowan were more cognitively sophisticated than extant nonhuman great apes. Wynn and McGrew push back against this move. For them, it goes against "the point of scientific archaeology" to posit the existence of a cognitive capacity in the deep past for which no evidence is known.

It is worth noting that this is probably a bit fast. For instance, one may simply admit that the evidence from the Oldowan *underdetermines* claims about the cognitive abilities of its makers. Doing so does not commit one to the existence of more sophisticated abilities during the early Pleistocene. Rather, one may simply acknowledge that hominin cognitive capacity at that time remains an open question.

Not for Wynn and McGrew, however. In their view, not only should these assumptions be rejected, but researchers should take the evidence provided by the archaeological record to be a "reliable indicator" of the behaviour of the makers of the Oldowan. It is perhaps puzzling that Wynn and McGrew invoke the notion of *reliability* here. After all, their opponents are not claiming that the evidence is *un*reliable. Rather, their opponents accept that the Oldowan is a reliable indicator of the *lower* bounds of cognition of its makers, but worry about expanding this to a claim about *general* cognitive capacity. The worry then concerns not the reliability of the evidence, but the scope of the inference that we can draw from that evidence: Wynn and McGrew think the evidence licenses a comparison between the cognitive capacities of ancient hominins and extant great apes, whereas their opponents call this into question.

We think a charitable interpretation of Wynn and McGrew's point here is the following. The Oldowan lasts for some 0.9 million years (2.6-1.7 mya). Given this time-scale, adaptive pressures are such that *if* a more sophisticated cognitive capacity were in existence at the time *then* we should reasonably expect to see a signal of it in the record. Call this the *time-depth condition*. Given Wynn and McGrew's argument that ape-grade cognition is a plausible competent cause of the Oldowan, and due to the lack of direct signals of more sophisticated hominin cognition over that time period, the time-depth condition provides them with provisional epistemic license to interpret the record as being a 'reliable description' of the cognitive capacities of its makers. (An open question of course is whether they are right that ape-grade cognition is a plausible competent cause of the Oldowan; we'll return to this thought later.) Wynn clarifies his position in a 2000 publication:

> It is not always necessary to kowtow to the catchy phrase 'absence of evidence is not evidence of absence'. Sometimes absence of evidence is a very persuasive argument for absence. (Wynn 2000, p. 114)

The more general thought here may be summed up as follows: if we accept the time-depth condition, then we can take the lack of evidence for some capacity to be a plausible indication of its absence. The Oldowan persisted for some 0.9 million years with very little change in production technique and form. Consequently, Wynn concludes that the absence of evidence for any more sophisticated cognitive capacities in this case is indeed evidence of their absence.

This commitment has since been canonised as 'Wynn's mantra':

> **Wynn's mantra:** Make no more assumptions than are necessary for explaining a phenomenon. (Overmann and Coolidge 2019, p. 3)

Wynn's mantra is one way in which to address the minimum necessary competence problem. The claim is that because the time-depth condition is satisfied in many cognitive archaeological contexts, the problem simply does not arise. There are some aspects of Wynn's mantra that are worth noting.

First, Wynn looks to be leveraging some form of parsimony reasoning. That is, all things being equal, we should prefer simpler explanations of empirical phenomena than more complicated explanations (Mizrahi 2016; Huemer 2009; Bradley 2018). And by 'simpler' we mean explanations that carry fewer presumptions unexplained by empirical data. So if we can explain the presence of the Oldowan by appeal to ape-grade intelligence (which is our empirical baseline), then, given we have comparative evidence that those hominins had at least those capacities, and a lack of direct evidence of more sophisticated cognition in the record of that time period, that is what we should do. In this sense Wynn's mantra is a very direct response to one dimension of the minimum necessary competence problem: it suggests that to accept its terms is to violate an important theoretical principle. (It does not, however, address the second dimension we have identified: the problem of minimum necessary competence will still depend to a large extent on the cognitive model we choose to employ.)

Second, Wynn's mantra can be read as quite specific to evolutionary cognitive archaeology. Investigating the deep hominin past allows the time-depth condition to be satisfied, but this is not the case for archaeological investigations focusing on recent periods. Recall that for Wynn and McGrew the point of scientific archaeology is to *use evidence*, not make assumptions. Yet to use a toy example: if we do not see trace evidence of, say, the use of symbols in a Bronze Age village in Crete, then clearly we should be very wary of inferring from this a *general reading* of the cognitive abilities of the inhabitants. Rather it is far more likely - given that symbolic cognition was widespread at this time - that the inhabitants of the village had the ability, but for whatever reason it is archaeologically invisible. In a case like this, the threat that the record is underselling the cognitive capacities of its makers is much more imminent, and the minimum necessary competence problem looms.

There is another reading of how Wynn's mantra applies to this case, however. Notice that the inference that these Bronze Age villagers *were* symbolically cognizant is based on widespread, synchronous evidence of the symbolism of the larger population of Holocene *sapiens*, as well as deeper-time (evolutionary) evidence of the Late Pleistocene lineage from which this population descended. And notice now that the time-depth condition *is* invoked to evidence a general reading of cognitive capacity over a broader time scale. Admittedly, when the time-depth condition is satisfied in cases of the Oldowan (or Acheulean), however, researchers infer not from a record of a single population or lineage, but from samples of quite different populations of hominids. This renders those inferences more difficult to make, and thus Wynn's mantra supplies a theoretical heuristic useful there, but read differently here. The inference that our hypothetical Bronze Agers were symbolic cognizers, despite an absence of direct evidence from their specific site, draws on a robust evidential record from elsewhere and elsewhen to converge on a general reading of capacities of the population in question.[18] Thus, a reply to our example might be that to *deny* those inhabitants symbolic cognition would be to posit a *very heavy assumption* indeed - some hidden cause leading to the *loss of cognitive capacit*y already well established in other human societies at that time as well as much earlier times. On this reading, Wynn's mantra is surely consistent with ascribing symbolism to our population of Bronze Agers despite the lack of direct evidence from that site. (That is, assuming that loss of symbolism is far more unlikely - and thus more of an assumption - than the presence of it despite it being archaeologically invisible at that site.) But in that case, the time-depth condition is met, and the mantra alone (no more assumptions than needed!) does not speak directly to the problem of minimum necessary competence, which must then be addressed.

Third, Wynn's mantra is, in both intent and effect, similar to Morgan's canon (though we have not the space for an extended discussion). In the early days of comparative psychology Morgan (1894) observed that researchers were prone to *anthropomorphise* the behaviour of animals: that is, to attribute human-ish cognition in order to explain the actions of animals. In order to combat this tendency, Morgan asserted that:

> In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale. (Morgan 1894, p. 53)

Morgan's canon places a constraint on theorising about cognition from observed behaviours. The general thought is that, if a phenomenon can be explained equally well in terms of 'lower' and 'higher' cognitive faculties, the former explanation is to be preferred.[19] An exception is

[18] As a commentator has pointed out, if one thought that Neanderthals and *sapiens* were one 'larger population', then one could use the preceding line of reasoning to infer Neanderthal symbolic cognition from *sapiens* evidence. We're hesitant to endorse the inference (although the conclusion might be right for all that is known) given its antecedent. Although the two could interbreed, the individuals who did are themselves representatives of distinct lineages which diverged half a million years or so before contact and interbreeding in Upper Palaeolithic Europe. The Bronze Agers in our example are not a lineage distinct (at the same level of analysis) from other relevant *sapiens* populations.

[19] By 'placing a constraint' we do not mean advocating *the simplest theory* per se, which is probably naive anthropomorphism. Morgan himself states "the simplicity of an explanation is no necessary criterion of its truth" (Morgan 1894, p. 54). Allen-Hermanson (2005) interprets Morgan's canon as a supervenience claim rather than one about theoretical simplicity, and in our view, this interpretation is plausible. Further analysis is beyond the scope of this article.

where there is independent evidence of the animal's use of the relevant 'higher' faculties elsewhere (see Morgan 1894, p. 59).[20]

Morgan is not alone. The philosopher Daniel Dennett has defended a similar principle, since dubbed 'Dennett's canon': "One should attribute to an organism as little intelligence or consciousness or rationality or mind as will suffice to account for its behavior" (Dennett 1976, p. 182).[21] Dennett, Morgan and Wynn are all concerned with the cognitive processes underlying behaviours. The difference between Morgan and Wynn, though, is that comparative psychologists can assess those behaviours more directly, whereas cognitive archaeologists only have the material traces of behaviours found in the stone-tool record. And that is a crucial difference.

It is time to draw some of the above threads together. In order to conform to Wynn's mantra, we must not assume more than is necessary to explain the appearance of a technology in the record. We are also told we must take the stone-tool record to provide an accurate reflection of the *general capacities* of the producers and users of those tools, given the time-depth condition. But, in order to satisfy these requirements, we must be able to identify the most plausible *minimal* competent causes of the production of the technologies in question. (That is, we must ask if Wynn and McGrew's reading of the Oldowan is persuasive.) And the answers we give will depend at least in part on the cognitive model we use to interpret the record. As we previously noted, there are well known theoretical divisions in cognitive science, such as between those who appeal to computationally rich representational models, and those who appeal to enactivist and other approaches. Theorists from these different paradigms will disagree about the minimum necessary requirements for a wide range of cognitive processes.

Indeed, it seems to us that this problem is amplified in the particular case of cognitive archaeology. In cognitive science generally, representationalism has, and continues to be, the dominant paradigm. However, in cognitive archaeology there is a more even split between researchers across the two paradigms. This is probably due to a 'founder effect'. Modern cognitive archaeology has been heavily influenced by two archaeologist/psychologist duos: Wynn and Coolidge, on the one hand, and Davidson and Noble on the other. The former employ a more traditional representational paradigm, whereas the latter employ a Gibsonian,

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[20] Thanks to Colin Klein for discussion here.

[21] This explicit methodological principle appears to be absent from Dennett's later work on the intentional stance.

ecological approach to cognition. Add to this founder effect the fact that other prominent, influential cognitive archaeologists have been interested in synthesising aspects of 4E cognition with more traditional cognitive theorising (see, e.g., Renfrew 2008, ch 6). So the consideration of non- and extra-representational approaches to the mind is no fringe issue in cognitive archaeology, as it may well be in other circles in cognitive science.

In the next section, we consider a range of examples in which we see this problem arising. Then in the following section, we consider two ways in which to address the problem. The first involves appealing to multiple lines of evidence in order to constrain the hypothesis space. The second looks at the merits of methodological pluralism. Briefly put, our overall strategy is as follows: we can satisfy Wynn's mantra, in part, by casting our evidential nets widely, and by embracing a research program in which researchers not only engage more thoroughly with cognitive science (as Wynn 2019 calls for), but are explicitly operating with a diversity of plausible theoretical commitments.

3. The problem in action

We have seen that, according to Wynn's mantra, we should make no more assumptions than are necessary for explaining a phenomenon. And we have seen how this principle figures in Wynn's assessment of the Oldowan. Let's now compare this with a quick glimpse at the vast work on human behavioural and psychological modernity, including Wynn's. We select this topic for it is, plausibly, an 'easy case' of the problem at hand. There has been much evolution-oriented discussion about the evidence for various modern human cognitive capacities: forward planning and mental time-travel; fourth- and fifth-order theory of mind; advanced working memory, focus, and impulse control; fine-tuned manual dexterity and the executive control required; task specialisation and the expertise underwriting such specialisations. Until recently, much of this discussion has been centred on the so-called 'creative explosion' or 'great leap forward' once thought to be evidenced in the art and artefacts comprising the European Upper Palaeolithic record. It is now reasonably uncontroversial that behavioural/psychological modernity has a longer, incremental trajectory than previously supposed (McBrearty and Brooks 2000; d'Errico et al. 2003; Sterelny 2011; Davies 2019).

To take a well-known example (figure 1), the 40,000 year old Löwenmensch figurine of the Hohlenstein-Stadel, according to Wynn and others, "should be considered strong evidence for fully symbolic communication and cultural modernity"; these researchers assert that the figurine "firmly establish[es] modern executive functions and working memory" (Coolidge and Wynn 2011, p. 174; see also Conard and Bolus 2003). In other words, according to Coolidge and Wynn, the Löwenmensch figurine signifies the expression of distinctively modern human cognitive capacities. Of interest here is how this inference stacks up against Wynn's mantra. That is, how does it navigate the minimum necessary competence problem?

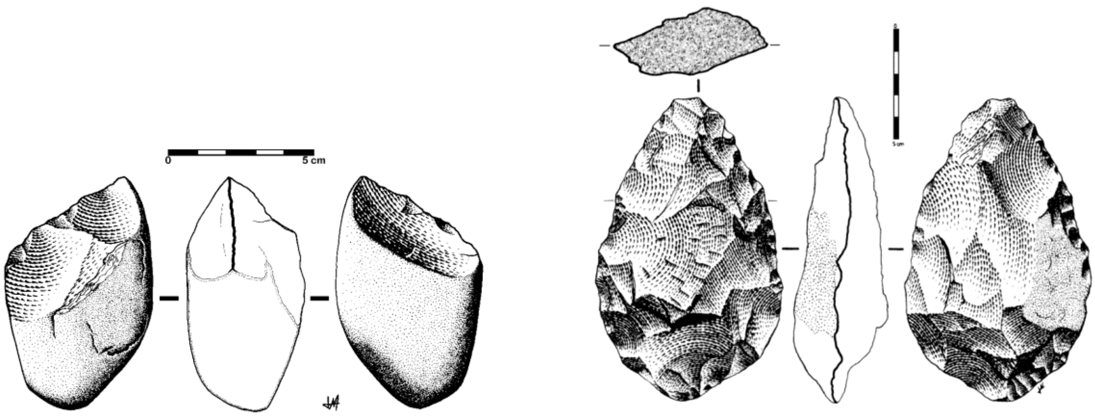
Figure 1. The Hohlenstein-Stadel Löwenmensch figurine. Source: Dagmar Hollmann/Wikimedia Commons; reproducible under the terms of the Creative Commons Attribution-Share Alike 4.0 International License. URL https://upload.wikimedia.org/wikipedia/commons/4/4c/Loewenmensch1.jpg

Researchers such as Coolidge and Wynn infer the existence of 'higher' cognitive functions by way of *minimal capacity inference* (Currie and Killin 2019; Pain 2019). Schematically, the basic line of reasoning goes as follows. Since it is implausible that ancient humans without modern executive functions and working memory could create some artefact *A* (say, the Löwenmensch figurine), according to some plausible cognitive model *C*, those capacities must be among the necessary capacities for its production. The inference takes us from a physical trace to the existence of cognitive capacities via a cognitive theory which speaks to the capacities required for the production of the artefact in question. Since the hypothesis that those capacities are required is theoretically and empirically constrained (for *C* should have independent evidential support), we have at least provisional license to make the ontological commitment that the minimal capacity inference permits. In doing so, Coolidge and Wynn do not fall foul of Wynn's mantra by positing advancements in cognitive capacity as compared to that of a great ape cognitive baseline in order to explain the intriguing Löwenmensch figurine. Indeed, as they demonstrate, it is *positive evidence of capacity*.[22]

That's the Upper Palaeolithic, though. Pushing back the dates for the evolution of these distinctive cognitive capacities into the deep past is far more controversial than evidencing and arguing for them at a mere 40 thousand years ago (kya). As far as the Oldowan lithic industry is concerned, we have already seen that Wynn is content to ascribe little more to ancient hominin tool-makers than ape-grade capacities (Wynn 1981, 1993, 2002; Wynn and McGrew 1989; McGrew et al. 2019).[23] We are sceptical, as will become clear, but let us simply grant this assumption for the moment. What about that of the Acheulean?

Acheulean stone handaxes (~1.76 mya to 150 kya) are typically bifacial, tear-dropped shaped, more-or-less symmetrical large cutting tools (figure 2). There is much debate about the capacities one might infer from these artefacts. In line with the above picture of Oldowan stone-tool production, a picture that does not posit advances beyond the range of variation of great ape cognition, Tomlinson (2015) posits a cognitively conservative picture of our forebears. Tomlinson agrees that the Oldowan tradition was perpetuated by simple imitation and individual experience, and argues that little more if anything is required for the production of Acheulean handaxes: that they were the result of fully embodied, unplanned, opportunistically-enacted actions. On this view, the properties of the handaxes (bilateral symmetry, tear-drop shape, etc.) were not pre-planned or intended - they were merely byproducts of the embodied, enacted sequence of gestures to which their makers 'entrained', a standardised sequence which results in the production of a multifunctional large cutting tool.

[22] See Pain (2019) for further discussion of this example.
[23] Thus on Oldowan tool-making, Wynn states "at this point in hominid evolution it appears to have been merely a variant on the basic ape adaptive pattern, with no obvious leap in intellectual ability required" (2002, p. 394).



(a) (b)

Figure 2. (a) Oldowan chopping tool; (b) Acheulean handaxe. Source: José-Manuel Benito Álvarez/Wikimedia Commons. Image (a) is reproducible under the terms of the Creative Commons Attribution-Share Alike 2.5 Generic license; (b) has been released into the public domain.

URL (a) https://en.wikipedia.org/wiki/Stone_tool#/media/File:Chopping_tool.gif

URL (b) https://en.wikipedia.org/wiki/Stone_tool#/media/File:Hand_axe_spanish.gif

Tomlinson is not alone. According to Malafouris: "The topography of the knapping activity and the accurate aiming of a powerful blow are neither pre-planned nor recollected; they are embodied, and therefore they must be *discovered* in action" (2013, p. 174; emphasis in original). On this view, the cognitive explanans is not in the head but in the act; indeed, for Malafouris, the knapper's 'intentions' and 'decisions' are "not even processed internally" (p. 173), but are literally constituted in part by the stone. The 'knapping' (the act) is the "true agent and controller" (p. 175) - agency and intentionality being the properties of material engagement - on this view.

Moreover, according to Ingold (2013), Acheulean handaxe production is no more or less than an "expression of an instinct" (p. 36). Birds build nests and beavers build dams in more or less invariant ways, distinctive of their species, though responsive to the environment and availability of raw materials. For Ingold, it wasn't any different for *Homo erectus* knappers. Ingold rejects 'hylomorphic' tendencies to view Acheulean lithic production as imposing an intentional mental form onto raw materials. Handaxe knapping may have been a deliberate activity, on Ingold's account, but not one that implies intentions of design or assembly plans.

According to Ingold, Acheulean knappers 'surrendered' to the raw material and 'followed where it led' (borrowing from Deleuze and Guattari 2004).

On many other views, however, the properties exhibited in Acheulean lithics are too technically demanding to be the mere byproducts of unplanned, unintended sequences of gestures (or of deliberate activity expressing mere instinct), evidencing more sophisticated operational skills and cognitive wherewithal than assumed of Oldowan hominins (Wynn 1989, 1993; Toth and Schick 1994; Hiscock 2014; Killin 2016, 2017; Shipton et al. 2021). Indeed, to some it would seem to be miraculous that the material record contains the axes it does, with the properties they have, if they were generated by such cognitively light processes. Thus John Gowlet claims "*Homo erectus*… had a geometrically accurate sense of proportion and could impose this on stone in the external world" (Gowlett 1984, p. 185); likewise, according to Brian Fagan, handaxe knappers "had to envisage the shape of the artefact, which was to be produced from a mere lump of stone" (Fagan 1989, p. 138). Finally, as Peter Hiscock has more recently put it:

> [Knapping] is intrinsically a complex process that requires competency at a number of levels simultaneously: bio-mechanical capacity to strike accurately and forcefully; the capacity to anticipate and identify emerging problems in the specimen morphology and to apply an effective action from a repertoire of potential responses; the capacity to plan ahead, which involves mental projections of both future actions and predicted outcomes (Hiscock 2014, p. 34).

These different positions exemplify our problem at hand.[24] The background assumptions that underpin models in the cognitive sciences produce different minimum necessary competence results. How then are we to satisfy Wynn's mantra? We'll unpack two complementary responses. We don't pretend that these are exhaustive, or sufficient for straightforwardly settling current debates (which require case-by-case treatment); though we certainly believe they are steps in the right direction, and present them as such.

[24] Other views are possible too. An anonymous referee points out that Garofoli (2019), for example, does not deny intentionality and operational planning to ancient hominins yet acknowledges the importance of embodied meaning allegedly inherent in the stone tools themselves.

4. Two approaches to the problem

4.1 Multiple lines of evidence

Many debates in evolutionary cognitive archaeology are over the capacities required to produce the objects populating the archaeological record. How should we settle such debates? How might we come to know what the minimal capacity for the production of a certain object is? One step in the right direction is to gather and synthesise *multiple lines of evidence*. As Bateson and Martin remark in a different context: "Historical explanations are by their very nature difficult to test… but multiple lines of inference can provide the basis for robust conclusions" (2013, p. 45). The guiding thought here is that by broadening the evidential resources bearing on the question at hand, we increase the plausibility of the answer upon which they converge (see also Sterelny 2003, ch. 6; Wylie 1989).

This idea - that progress can be made on open questions of interest to archaeologists by consilience - is not a novel point, though it is one well worth stressing here.[25] Recall the debate about cognitively lighter and richer explanations of Acheulean handaxe production. Stout (2011) notes that Acheulean tool-making action-sequence analysis has yet to be fully integrated with lines of evidence from other disciplines, though as we will see shortly, Stout is among those leading the charge.

This solution to the problem in practice benefits from a species of experiment which Novick et al. (2020) have called 'Kon-Tiki experiments'. This label references Thor Heyerdahl's famous voyage from Peru to Rapa Nui on a balsa raft, demonstrating the capacity of those rafts to make the voyage (in support of his hypothesis about westward Polynesian migration). A Kon-Tiki experiment in the historical sciences, then, aims to demonstrate the capacity of some cause to produce some effect, typically in order to project that cause's operation outside of the experimental set up (to an explanatory target in the deep past, for example).[26] The experiment, in other words, aims to turn a 'how possibly' conjecture into a 'how plausibly' explanation by corroborating a mid-range theory or testing assumptions underlying a hypothesis. The reconstruction of ancient tools to test cut marks on bones and

[25] As Alison Wylie has long argued, archaeology is a 'trading zone' (Wylie 1999, 2000; Chapman and Wylie 2016). It comprises practitioners with expertise in diverse approaches and methods who draw lines of evidence from different nearby fields, requiring integration. For a recent discussion concerning the case of archaeogenetics, see Downes (2019).
[26] Heyerdahl's Kon-Tiki voyage, then, succeeded in demonstrating capacity, but failed in its projection. See Novick et al. (2020) for discussion.

compare them with carnivore damage (see Jeffares 2008) is one example of Kon-Tiki experimentation; experimental taphonomy (Briggs 1995; Raff et al. 2006) is another; research testing the hypothesis that the Divje babe I bone is a Neanderthal ‘flute’ is a third (see Killin 2019).

The paradigmatic epistemic situation calling for a Kon-Tiki experiment is the following: “1. An inquirer desires to know the cause(s) responsible for producing some particular effect, and 2. at least one candidate cause is known to exist (and some evidence may suggest that this cause was responsible for producing the effect); however, 3. it is unknown whether the cause is competent to produce the effect” (Novick et al. 2020, p. 217). This is the situation researchers facing the minimum necessary competence problem are often in. Cognitive archaeologists are interested in the causes that produced the lithic record, and at least one candidate cause is known to have existed: that of the standard range of variation of great ape cognition. However, although chimpanzees and bonobos in the wild are known to pick up a stone for use as a hammer/nutcracker (Boesch and Boesch 1990; Inoue-Nakamura and Matsuzawa 1997), and evidence for this behaviour dates back over four thousand years (Mercader et al. 2007), they are not known to fashion stone tools (McGrew et al. 2019). But could they? Naturally, this calls for a Kon-Tiki experiment.

Fortunately, such an ‘experiment’ has been attempted already.[27] Endeavours to train bonobos in simple (Oldowan-style) knapping show that ape-grade cognition is *possibly* a competent cause of Oldowan tools (Toth et al. 1993, 2006; Schick et al. 1999; Savage-Rumbaugh and Fields 2006; Toth and Schick 2009). Possibly, but not probably the cause, in our view. Trained bonobos succeeded in stone breaking and flake production, to be sure, but also struggled in certain respects: “most pieces lacked the attributes of controlled flaking evident in the earliest hominin assemblages” (Moore 2019, p. 185). The bonobos failed to adopt freehand percussion techniques and the behaviours they were able to produce were the product of constant encouragement and an artificial reward structure set up by researchers. Moreover, the cognitive demands of the broader behavioural context of ancient Oldowan knapping (selection and transport of raw materials; effective use of tool post-production; acquisition of relevant skills and know-how, etc.) are not at all clear (Stout 2010). For example, since an assumption of many researchers is that observation and copying were sufficient for Oldowan

[27] We are speaking loosely here. As an anonymous referee points out, these endeavours were to refine frames of reference for characterizing variation, not test how something might have been done in the past. Here we are reframing general observations/outcomes for our purposes.

tool-making skill transmission, it would be good to know whether observation of Oldowan-style knapping activates any 'higher-level' areas of the human brain. Consistent with Wynn's picture, however, although fMRI studies of modern humans observing tool-making demonstrations (and making technical judgements) have indicated significant activation of the prefrontal cortex in Acheulean-style demonstration-observation conditions, this did not occur in Oldowan-style conditions (Stout et al. 2011, 2015).

In any case, with due caution, let us simply grant for the sake of argument that ape-grade cognition is a *possible* competent cause of simple Oldowan tool production.[28] The question, then, is whether that cognitive package is also competent to produce *complex* stone tools of the kind that comprise the Acheulean record. This has not been borne out by experiments with trained apes, whatever we think of the Oldowan-style knapping experiments. But perhaps turning to other lines of evidence will shed light.

4.1.1. Neural correlates of Acheulean lithic production

Dietrich Stout et al. (2008) tested neural correlates in contemporary humans of Oldowan-style and Acheulean-style stone tool production via a positron emission tomography (PET) study of expert knappers, and a comparison with a previous PET study of novice Oldowan-style knapping (Stout and Chaminade 2007). Stout and colleagues infer from this neuroimaging work that although expert competence at Oldowan-style flake production requires a significant practice period, and although experts documented greater activation of brain areas for visuomotor coordination and organisation of hierarchical action sequencing than novices (facilitating more effective flake detachment), Oldowan-style knapping was not observed to utilise advanced (prefrontal cortex) neural functions for forward planning and executive control. This is in contrast to expert Acheulean-style knapping which appears to utilise prefrontal systems for action planning and execution (e.g. right inferior frontal gyrus). And this is consistent with the fossil record of hominin cranium evolution: the pre-*Homo* and habiline makers of the Lomekwian and Oldowan industries had considerably smaller brains (450cm^3) than the erectines and descendent hominins of the Early Acheulean (850cm^3) and Late Acheulean (1200cm^3) (Stout et al. 2008; Klein 1999). Habiline fossil evidence is consistent with an expansion of the posterior parietal lobes (from a great ape baseline, enhancing sensorimotor cognitive capacity) but still rudimentary prefrontal lobes (Holloway et al. 2004).

[28] It may be a possible competent cause but we suspect it will fail projection (as did the Kon-Tiki hypothesis) not least for the issues extant apes face with knapping even in the most favourable of learning environments.

While it is not possible to directly infer the internal neural structuring of ancient hominin brains from endocasts, palaeoanthropologists' best guess is that erectines, though not habilines, evolved expanded prefrontal cortices (Stout 2010; Holloway et al. 2004).

An alternative reading of Stout's results questions whether the observed activation of increased neural function reflects more advanced cognitive planning, and not merely the physical demands of handling/manipulating raw materials into distinct products: one simple, one complex. Subsequent research by Stout and colleagues addresses this concern (Faisal et al. 2010). It suggests that the above-reported differences in brain activation in expert Oldowan-versus Acheulean-style knapping indeed reflect not 'lower-level' manual differences in terms of handling/manipulating raw materials, but 'higher-level' executive organisation of behaviour. These researchers used a data glove to ascertain manual joint and abduction angles of the fingers of the nondominant (core-holding) hand in expert knapping in both Oldowan and Acheulean styles. The rationale for this was to clarify the relationship between the left (nondominant) hand in right-handed knapping and the activation of contralateral brain areas: does the previously observed increases in right hemisphere activation during Acheulean- vs. Oldowan-style knapping reflect increased contralateral grasp control, a distinctive right hemisphere contribution to executive action-sequence control, or both? Data glove recording enables the quantification and mathematical analysis of grip complexity and diversity. The results of the study demonstrate that Oldowan and Acheulean knapping techniques are indistinguishable with respect to manual manipulative complexity, though both were clearly higher in complexity than control tasks. If robust, these findings would undermine the alternative reading that the differences in brain activation are due to 'low-level' differences between the tasks, corroborating Stout's (and some others') view that Acheulean-style knapping relies on advanced cognitive wherewithal beyond the standard range of variation of extant nonhuman great apes.

Let's reframe the above as a messy, multifaceted 'Kon-Tiki experiment'. Stout's research suggests that while ape-grade cognition might suffice to explain Oldowan flake production, the same is called into question with regards to Acheulean handaxes. It so far has not corroborated the hypothesis that ape-grade cognition is competent to produce the effect in question: Acheulean tools. It suggests that functions enabled by an expanded prefrontal cortex are (at least partial) competent causes. It remains to be seen how this finding could be projected into the deep past. Neuroscientific research does not directly reveal the neural organisation or cognitive capacity of ancient hominins.[29] That said, it can clarify the relative demands of behaviours sure to be evolutionarily significant. Combined with other complementary lines of evidence, then, this research can constrain the hypothesis space and thus provide avenues for responding to the minimal competence problem. The next step? Divide and conquer. We'll demonstrate with the example of symmetry, commonly (though not universally) exemplified in Acheulean handaxes. Contra Tomlinson, Ingold, and others, it appears that there is good reason to infer that Acheulean knappers consciously imposed symmetrical form onto the raw material they were shaping.

4.1.2. Intentional imposition of symmetry in Acheulean toolmaking

Some theorists question whether the symmetrical form of many Acheulean bifaces was a deliberate goal of ancient knappers. On one set of views, the symmetry is epiphenomenal, due to a visual perceptual bias in the brains of Acheulean tool producers (Deręgowski 2002; Hodgson 2009); on another, it is an unintended byproduct of the creation and maintenance of a bifacially edged stone tool (Hayden and Villeneuve 2009; McPherron 2000, 2013). Against these views, Ceri Shipton and colleagues (Shipton et al. 2018) argue that Acheulean bifaces were deliberately made symmetrical. We'll be silent here on the underlying reasons for the symmetry: whether these were utilitarian - for example, a symmetrical handaxe is a balanced handaxe (a core is easier to hold stead if the knapper shapes it symmetrically, that is, for improved balance) - and/or non-utilitarian, for instance, aesthetic. Shipton and colleagues synthesise three lines of evidence. The first tests symmetry bias in modern humans (under the assumption that modern humans should have an even stronger symmetry perception bias than the makers of Acheulean handaxes) via a series of transmission chain experiments, in which participants copied stylised drawings of Acheulean bifaces, symmetrical or otherwise, to be passed on to the next participant to copy, and so on. While drawings of asymmetrical axes tended to become more symmetrical, the symmetrical axes tended to become less so, suggesting that imposition of symmetry is *not automatic* (at least, in modern humans). If it is not automatic, it is under some level of intentional/executive control. Since there appears to be no evidence for this capacity in extant great apes, presumably it was absent in the Pan/Homo last common ancestor.

[29] However, while the brain studies of living humans might not be direct evidence relevant to projection, the failure of apes is direct evidence: it is evidence that the standard range of variation of ape cognition is not enough to produce Oldowan let alone Acheulean tools. Hence our scepticism.

The results of this experiment are then combined with two further lines of evidence, bolstering the plausibility of the deliberate symmetry thesis. These lines tested whether symmetry is simply the inevitable consequence of producing and maintaining tools with bifacial edges, via (a) analyses of the symmetry of the bifaces themselves, and (b) analyses of Acheulean cleaver reduction sequences. First, Shipton and colleagues analyse the FlipTest[30] scores of a variety of Palaeolithic stone tool types and compared the results with that of a variety of Acheulean bifaces. According to their data, the degree of symmetry of a tool is *independent* both of its degree of bifaciality and its degree of reduction intensity. Thus symmetry is not a mere byproduct of bifacial edge technology. Second, by analysing Acheulean cleavers (figure 3), these researchers argue that Acheulean tool-makers often achieved symmetry where unifacial flaking is involved too. In other words, bifacial flaking is neither sufficient nor necessary for the co-instantiation of symmetry in the Achulean record.

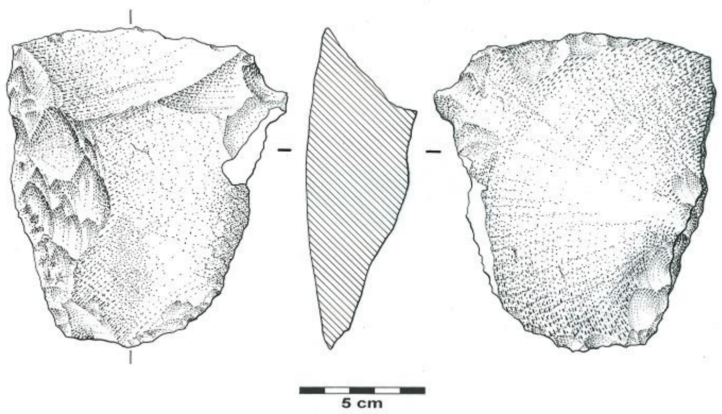


Figure 3. Acheulean cleaver. Source: NMI-ICHTO/Wikimedia Commons; reproducible under the terms of the Creative Commons Attribution-Share Alike 4.0 International license.
URL https://en.wikipedia.org/wiki/File:Cleaver_from_Shiwatoo.jpg

In sum, according to Shipton and colleagues it does not look like the symmetry of Acheulean bifacial handaxes is a mere byproduct of enacted production sequences, or an instinctive following of the reduction path that the raw material leads one on; it does not look

[30] FlipTest is a computer program for measuring how symmetrical two halves of an electronic image are, producing numerical values of overall asymmetry in pixels (Hardaker and Dunn 2005).

like an unintended side-effect of bifacial technology, or of hominin visual perceptual biases favouring symmetry. These researchers are explicit in their conclusion: “In conjunction, these analyses show that symmetry was deliberately imposed on Acheulean bifaces” (Shipton et al. 2018, p. 74).

Each line of evidence is of course separately defeasible. Yet taken together they support the proposition that symmetry was an intended property of Acheulean tools. Again, the purpose of the discussion is not to claim the debate has been settled, but to demonstrate the merits of the approach. Following Shipton and colleagues, it appears we have good reason to take seriously the suggestion that an intentional sensitivity to symmetry is a minimal capacity for the production of the Acheulean technocomplex. That epistemic warrant comes from inference to the best explanation based on convergence of multiple independent lines of evidence. New evidence might shake things up: human evolution research is a ‘trading zone’ after all. Nonetheless, piece by piece, line by line, we can construct a clearer idea of our cognitive past.

4.1.3. Summary

The aim of the preceding discussion was to demonstrate how, in a particular case study (Acheulean lithics), cognitive archaeologists have responded, at least implicitly, to one aspect of the minimum necessary competence problem by marshalling evidence from multiple lines of inquiry. The discussion demonstrates the value of this approach.

As we saw earlier, some researchers hypothesise only cognitively-light explanations of Acheulean handaxe production. However, as our discussion shows, multiple lines of evidence converge on a picture of scepticism concerning those cognitively-light hypotheses. Still, the nature of the cognitive capacities to be posited as competent causes of Acheulean lithics remains up for debate and a topic for future research. For now, lines of evidence seem to converge on the capacity to impose a mental template on raw material, however crude that template might be, and an intentional sensitivity to symmetry.

In the following section we turn to the second aspect of the problem: theory choice. We argue cognitive archaeology would do well to embrace a plurality of theories. Although we do not advocate for methodological anarchy (the ‘anything goes’ of Feyerabend 1975), theoretical diversity is a good thing in science in general. According to recent modelling work, one way of limiting diversity is to introduce extreme priors. Embracing a broad spectrum of plausible views in the philosophy of cognitive science may therefore assist cognitive archaeology in addressing the problem of minimum necessary competence.

4.2 Methodological pluralism: embracing theoretical diversity

Recall that cognitive archaeologists interpret the material record through the lens of different cognitive theories. The early works we've cited by Thomas Wynn[31] utilise Piaget's theories; sceptics of that framework were quick to criticise (Noble and Davidson 1991, 1993). Moreover, Mithen (1996) appeals to Fodorian modularity; Noble and Davidson (1996) to Gibsonian ecological psychology; Malafouris (2013) to embodied, enactive, embedded and extended theories of mind. Each has their critics, not least because debate over theories within cognitive science is widespread and lively, let alone debate concerning their archaeological application. This is not the place to adjudicate between specific rivals or single out a most-plausible framework. Rather, we suggest cognitive archaeology would do well, in the end, were it to embrace a diversity of plausible cognitive theories, including but not limited to theory from cognitive neuroscience, developmental psychology, information processing, social cognition, symbolic theory, and approaches drawing on one or more of the four Es. While some theories are compatible with others, of course some are in tension. (And we have our own prognostications concerning the prospects of various theoretical frameworks.) Nonetheless, after discarding theories satisfactorily shown to be incoherent, internally inconsistent, without empirical or theoretical credentials, and/or hopelessly outdated, a range of serious contenders would remain. Hence the state of live debate. We recommend a pluralism ranging over that diversity of plausible theoretical approaches, a dynamic range which shifts as necessitated by progress in the field.

The distribution of workers and resources across rival scientific research programs has long been discussed in philosophy of science (Kitcher 1990, 1993; Strevens 2003; Godfrey-Smith 2003; Muldoon 2013). To our minds, it has been firmly established that methodological pluralism is both advisable and desirable. While sometimes scientific research is propped by a specific theoretical framework given some law or law-like generalisation to which researchers must submit in practice (at least, until that framework is overthrown) - relativity, in some physics research programs, for example - when it comes to selecting theory from cognitive science, there are a plethora of live approaches for archaeologists to consider. Cognitive

[31] See Wynn (2016), pp. 8-10, for retrospective discussion of his Piagetian days.

archaeology (like any other) should not put all its eggs into one theoretical basket since there is clearly the possibility that some rival theoretical framework now or in the future would enable its researchers to better solve a given puzzle. That's a straightforward lesson of scientific fallibilism. Moreover, it helps to promote exploration and creativity - which are good things in science - when workers within a field have a diversity of theoretical priors. When they don't, researchers will converge on theoretical presuppositions, and given expected payoffs, no one will opt to do the exploratory work (a free-riding problem; see Zollman 2010, pp. 26-27).

To this end, in a recent article Zollman (2010) argues that *transient diversity* benefits scientific heterogeneity and we echo this argument here. Zollman uses the case of peptic ulcer disease as a case study. Since 1875, there have been two hypotheses regarding the cause of the disease: (i) bacteria; and (ii) excess acid. Until the 1950s both hypotheses enjoyed support, but in 1954 Palmer produced a study that appeared to show that bacteria are not capable of colonising the stomach of a human. All previous evidence to the contrary was attributed to the contamination of samples (Zollman 2010, pp. 20). Such was the uptake of Palmer's study, the bacterial hypothesis was virtually abandoned until the late 1970s. By this point, deficiencies in treatments based on the excess acid hypothesis were being increasingly noticed; in particular, they did not cure peptic ulcers, but merely mitigated the symptoms. In 1984, Warren and Marshall published results observing Helicobacters - in particular, *Helicobacter pylori* - in a human stomach, though these results were initially dismissed. In 2002, after repeatedly failing to convince the research community of the causal relationship between *H. pylori* and peptic ulcer disease, Marshall took matters into his own hands. He drank a solution containing *H. pylori*, contracted peptic ulcer disease, and was able to cure himself using antibiotics. Marshall's results were replicated, and eventually he and Warren received the 2005 Nobel Prize in Physiology or Medicine.

The lesson here is as follows. The research community does not seem to have done anything wrong: there was no reason to think that Palmer had engaged in any misconduct; rather, researchers were convinced by a well-conducted, careful study. But something had gone awry - after all, the correct hypothesis was given almost no credence for three decades. What prevented researchers from converging on the bacteria hypothesis sooner was the general acceptance of Palmer's study. This indicates that pursuing a range of solutions to a problem is an important aspect of scientific inquiry. Achieving scientific consensus - so that it tracks truth - should be 'slowed down' to ensure sufficient investigation has occurred (Muldoon 2013).

However, we do not want that to continue indefinitely. Ideally, we want the community to eventually converge on the truth (as they presumably have done in the *H. pylori* case).

This boils down to the following dilemma: scientists are typically confronted with a range of competing ideas (hypotheses, theoretical frameworks, etc.), where for each issue at stake, it is not yet known which ideas if any are correct (or approximately correct). On the one hand, it's not desirable to test false theories, not least given current publication biases and competition for funding; on the other, the only way to ascertain the best theories is to test a range of theories. In communities of scientists where results are shared, this motivates free-riding: I get access to the information you produce without taking on the cost of testing a potentially false theory. This means that exploration is disincentivised. The dilemma is only aggravated once we take into account the resource and opportunity constraints that are a fact of real-world scientific practice. Researchers working on peptic ulcer disease were in this situation; so are those in cognitive archaeology, who are applying to the archaeological record the theories developed and tested within cognitive science. So, what strategy should we employ here?

Zollman's (2010) agent-based modelling work provides some clues. One result this modelling suggests is that *limiting* the flow of information between researchers can have the effect of *increasing* the probability of success. This reflects the incentive to free-ride, such that the community will tend to converge on a promising theory *before* the optimum amount of exploration work has been done. Another result the modelling suggests is that endowing researchers with extreme priors (supporting a plurality of background theoretical commitments) will promote exploration. This is simply to say that, if we are less likely to abandon our theories, diversity is maintained and the community is more likely to converge on the truth. However, Zollman warns that when information is limited *and* we introduce extreme priors researchers will fail to converge on the truth. This implies that "diversity is not an independent virtue which ought to be maintained at all costs, but instead a derivative one that is only beneficial for a short time" (p. 19).[32] Methodological pluralism is a good thing, but not indefinitely so.[33]

[32] This is just one example from the burgeoning field of social epistemology of science. It is beyond the scope of this article to provide a full review; see e.g. Zollman (2013), Muldoon (2013).

[33] Rosenstock et al. (2017) criticise Zollman (2010), showing his results are sound but only in a smaller proportion of parameter space than he initially claimed. But these criticisms do not detract from the take home messages presented here - not least because cognitive archaeology satisfies Rosenstock et al.'s conditions for a 'difficult' inquiry: there are (relatively) few practitioners; (relatively) small batches of information are acquired

Thus, in responding to the minimum necessary competence problem, cognitive archaeology would stand to benefit from embracing a plurality of theories from the cognitive sciences.[34] The case for diversity is strengthened when we take into account the infancy of the discipline; the science is at a stage where exploration is important.[35] This will facilitate a diversity of approaches to puzzles within the field. However, that diversity need only be transient: Zollman's modelling work suggests that on a given problem, methodological diversity "should be around long enough so that individuals do not discard theories too quickly, but also not stay around so long as to hinder the convergence to one action" (pp. 32-33). As progress is made within cognitive science, some cognitive theories will be rejected, others corroborated, and new ones developed. Novel puzzles will arise in archaeology. Along both dimensions, the need for periods of transient diversity within areas of cognitive archaeology will ebb and flow.

5. Conclusion

Back in 1954, the eminent Kenneth Oakley told us that "Even the crudest Palaeolithic artifacts indicate considerable forethought… Using a hammerstone to make a hand-axe, and striking a stone flake to use in shaping a wooden spear, are activities which epitomize the mental characteristics of man" (1954, p. 15). In the ensuing decades, much scepticism has been launched against this proposition. But also research programs have been developed in order to probe and test it (Stout et al. 2015). Even so, which 'mental characteristics' are we talking about here? What can we safely ('minimally') infer about the mental lives of ancient hominins from Oldowan and Acheulean stone tools? And through which cognitive models should we look at the archaeological record?

Although these remain open questions, we have argued - against prominent pessimism (Lewontin 1998) - that progress can be made. Cognitive archaeology can respond to the minimum necessary competence problem in at least two ways - by seeking convergence on

at any given time; differences stemming from competing midrange theories are often (relatively) minor or difficult to test. It is not our intention to weigh in further on such debates here.

[34] Again, this is not the place to adjudicate between rival approaches or determine which approaches are plausible. The point is a coarse-grained methodological one.

[35] Indeed, as an anonymous reviewer points out, this diversity would encompass current debates between proponents of the extended evolutionary synthesis and those advocate the standard evolutionary theory (Laland et al. 2014). We agree that working through this line of thought is important, but is beyond the scope of the current paper.

capacity by pursuing and integrating multiple independent lines of evidence, and by embracing a plurality of plausible models from the cognitive sciences. Thus we are optimistic that some traction is achievable in addressing a puzzle deemed by some to be largely intractable. Our optimism is not constrained to these two responses: they almost certainly do not exhaust the possibility space. Other prospects for gaining traction (and their integration with those discussed here) are thus an important avenue for future research.

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Statement of Contribution

This thesis is submitted as a Thesis by Compilation in accordance with https://policies.anu.edu.au/ppl/document/ANUP_003405

I declare that the research presented in this Thesis represents original work that I carried out during my candidature at the Australian National University, except for contributions to multi-author papers incorporated in the Thesis where my contributions are specified in this Statement of Contribution.

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CHAPTER 3

How WEIRD is Cognitive Archaeology? Engaging with the Challenge of Cultural Variation and Sample Diversity

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Abstract: In their landmark 2010 paper, "The weirdest people in the world?", Henrich, Heine, and Norenzayan outlined a serious methodological problem for the psychological and behavioural sciences. Most of the studies produced in the field use people from Western, Educated, Industrialised, Rich and Democratic (WEIRD) societies, yet inferences are often drawn to the species as a whole. In drawing such inferences, researchers implicitly assume that either there is little variation across human populations, or that WEIRD populations are generally representative of the species. Yet neither of these assumptions is justified. In many psychological and behavioural domains, cultural variation begets cognitive variation, and WEIRD samples are recurrently shown to be outliers. In the years since the article was published, attention has focused on the implications this has for research on extant human populations. Here we extend those implications to the study of ancient *H. sapiens*, their hominin forebears, and cousin lineages. We assess a range of characteristic arguments and key studies in the cognitive archaeology literature, identifying issues stemming from the problem of sample diversity. We then look at how worrying the problem is, and consider some conditions under which inferences to ancient populations via cognitive models might be provisionally justified.

1. Prelude

In 2010 Henrich and colleagues argued that the behavioural sciences face a serious methodological issue: most of the results in the field are produced using participants from

WEIRD populations, yet these results often fail to replicate in cross-cultural studies (Henrich et al. 2010). Moreover, cross-cultural research suggests that WEIRD people are often outliers with respect to many cognitive and behavioural traits. So it seems that inferences from culturally localised samples to species-wide psychological claims are unjustified. The take home lesson is that, when it comes to behavioural psychology, culture matters; and often matters profoundly. There is no prior guarantee that an effect will be reproduced beyond the group it is found in, nor that it is an inlier with respect to the broader population.[36] Therefore, generalise with care.

So far, so worrying. But we think there is more to be worried about. The bulk of scholarly attention in the wake of Henrich and colleagues' article has focused on the issue of synchronous generalisation; that is, inferring from WEIRD populations to the (extant) human species at large ('generalisations'). However, as we will show, in the field of human cognitive evolution we often make diachronic inferences; that is, inferences from modern WEIRD populations to populations of ancient humans, their hominin forebears, or cousin lineages ('past projections').[37] And this too should be worrying—after all, ancient humans were certainly non-WEIRD.

Our focus is the discipline of cognitive archaeology. Cognitive archaeologists attempt to reconstruct the cognitive and cultural lives of ancient humans, Neanderthals, and their hominin ancestors by studying their material traces. One aim of the discipline is to shed some light on the evolution of human cognition. However, artefacts alone are insufficient to link information about behaviour to underlying cognitive capacity: a mid-range theory is required (Currie 2018; Binford 1972). So cognitive archaeologists typically appeal—either implicitly or explicitly—to a theory or model from the cognitive/psychological/behavioural (henceforth 'cognitive') sciences; one which is thought to be independently plausible. Using this model, inferences are made from artefacts to cognitive capacity.

[36] Indeed, if much of the mind's machinery is installed through cultural learning—whether the product of cultural evolution (Heyes 2018) or gene-culture coevolution (Laland 2017; Sterelny 2012)—then naturally that machinery will vary across cultures.
[37] For ease of expression, throughout this article we restrict the term 'human' to *H. sapiens*, and use 'hominin' for all *Homo* species, usually in reference to ancestral or cousin lineages of *H. sapiens*.

We begin by introducing the challenge to cognitive archaeology in a little more detail, and provide some motivating examples (Section 2). We then outline four case studies which exemplify the issue of cross-cultural sample diversity in cognitive archaeology (Section 3). Next we look at how worrying the issue is, and briefly consider some conditions under which inferences from contemporary samples—most often WEIRD humans—to ancient populations might be provisionally justified (Section 4). Throughout our mantra will be that further cross-cultural testing of cognitive models stands to strengthen or undermine the cognitive archaeological inference provisioned by those models. Of necessity, the discussion in section 4 is schematic and suggestive of further lines of inquiry; our primary goal in this paper is to foreground the problem at hand. Finally, Section 5 summarises, and outlines some future directions for research.

2. Introducing the problem

Cognitive archaeology is a pluralistic and interdisciplinary research cluster with no single method. Rather, cognitive archaeologists employ a range of inferential strategies in order to reconstruct cognitive and cultural aspects of our evolutionary past in the general manner described above (see also Currie and Killin 2019; Currie 2018; Pain 2019; Malafouris 2020). A fairly common mode of inference, however, is *minimum-capacity* inference. This strategy attempts to identify the minimum cognitive prerequisites required for the production of some artefact by applying a model from the cognitive sciences to an artefact's construction.[38] For instance, we might infer that the production of Acheulean or Levallois technologies requires operational thought (Wynn 1979) or long-term working memory (Wynn and Coolidge 2004). Cognitive archaeologists running this mode of inference have often utilised cognitive models based on WEIRD samples, and this is potentially problematic.[39] In many domains relevant to

[38] There are many other strategies. For example, Hiscock (2014) infers that Acheulean toolmakers maintained apprentice learning systems managed by expert individuals based not on an explicit model from the cognitive sciences but a task analysis of Acheulean technology. Moreover, in addition to cognitive models, cognitive archaeologists may also leverage comparative, palaeoneurological, and other data (Wynn 2016; Currie and Killin 2019; Killin and Pain 2021). Here we are focused on the epistemic licence for an inference from archaeological artefact to cognitive capacity provisioned by a cognitive model.

[39] We say 'potentially', for not all cognitive domains might be sensitive to the WEIRD problem (we discuss this in Section 4.2); neither is it inevitable that any differences will entail effect sizes large enough to skew conclusions drawn by cognitive archaeologists via WEIRD data; nor is it known how many inferences will be undermined by the use of WEIRD data. So we do not claim that by using WEIRD data, cognitive archaeology is or has been wrongheaded. Our claim is that the use of cognitive models corroborated by more diverse samples stands to strengthen or undermine particular cognitive archaeological inferences, and, as we will argue, this is a point worth reflecting upon. Likewise, we note that it is for very good reasons that many cognitive/psychological/behavioural experiments are conducted on exclusively WEIRD subjects. The extent to which a study's sample is 'narrow' or 'diverse' is not a measure for distinguishing 'bad' from 'good' studies. The issue at hand is with inference beyond

the interdisciplinary study of evolution and human behaviour, individuals from WEIRD societies are not representative of human beings in toto (Henrich et al. 2010; Henrich 2020; Apicella et al. 2020). And while extant foragers are not Pleistocene hominins, and do not form a unified, monolithic whole (Astuti and Bloch 2010), their socioecology plausibly resembles more closely that of the ancient *H. sapiens* foragers from whom all humans today are descended. For these reasons, cognitive archaeology would do well to seek out mid-range theories corroborated by evidence from more diverse samples than the standard WEIRD range, and where possible to provide details of the sample so that inferences from WEIRD or otherwise narrow samples can be seen for what they are.

We do not have to look far for motivating cases. In their contribution to a special issue on the archaeology of children and childcare, Lew-Levy et al. (2020) argue that the results of developmental psychology experiments researching the innovation of children in WEIRD societies—results which suggest that children are poor innovators (e.g. Lister et al. 2020)—are not generalisable to children of small-scale, forager societies. And seemingly with good reason. Sterelny (2021) sums it thus:

> "...forager children are likely to innovate much more than WEIRD children, given that they grow up in an environment in which they have a lot of autonomy to play, to experiment and to explore the affordances of the material substrates to which they have access. In many forager cultures, experimental learning for oneself is positively encouraged. Furthermore, they routinely engage in peer-peer learning. While adults support social learning, they do relatively little directive teaching. Instead, they scaffold learning with equipment and raw materials, they provide occasional advice and they allow children to involve themselves with adult activities. Finally, children, even quite young children, do not just imitate in play adult economic and social activities, they practice those activities. They engage in subsistence activities, often in distinctive ways." (Sterelny 2021, p. 5)

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the sample. When generalisation is at issue, over-reliance on WEIRD participants is problematic for all the reasons detailed by Henrich et al. (2010), and much the same can be said of projection to the past. We thank an anonymous referee for pushing us on this.

The general concern should be clear: if the results of innovation studies do not generalise beyond WEIRD populations in extant humans, then we have reason to question their applicability to ancient humans. Furthermore, if we think that the learning environments of ancient populations were more akin to those of small-scale forager societies, then we should be prepared to treat samples from WEIRD populations as outliers.

Of course, innovation is typically assumed to be heavily influenced by socio-cultural processes. So the preceding example is perhaps not particularly striking. It might be thought, however, that more 'base-level' processes—such as those involved in visual perception—would not vary much across contemporary human populations. Henrich et al. (2010) provide a motivating case that undermines this line of thinking—the Müller-Lyer illusion—which is now a classic example for establishing cross-cultural variability (Segal et al. 1966).[40] The two lines in the illusion are of equal length, though (to us and our colleagues, at least) they do not appear to be equal (see Fig. 1). Line A appears to us to be shorter. Researchers have tested the strength of the illusion by asking subjects how much longer Line A needs to be than Line B in order for the two lines to appear equal (see Fig. 2). This quantity varies widely across societies, along a continuum. At one end, WEIRD undergraduates require Line A to be about 20% longer than Line B; at the other end, the difference in line length San foragers required was indistinguishable from zero. For the San, there is no illusion. Much has been suggested about the causal role one's environment plays in bringing the illusion into effect (carpentered corners being ubiquitous in WEIRD societies and absent in the Kalahari; see McCauley and Henrich 2006; Henrich 2008).[41] However, a causal explanation of such variation is not what is at stake here. Rather, the case illustrates that even base-level visual processing can be subject to cultural variation. And, if even visual perception can vary across populations, the range of cognitive/psychological processes that are sensitive to culture is plausibly very broad. Moreover, the fact that the magnitude of the effect is strongest in WEIRD subjects is of concern, as this demonstrates that WEIRD people are outliers. We cannot thus argue that despite cultural variation WEIRD subjects are generally representative of the population. And

[40] For review, see Phillips (2011). There is some debate about the replicability of the original results and the extent to which the cultural variation exists. For example, Deręgowski (2013) argues that only Müller-Lyer figures with interfin angles between 90° and 180° range are appropriate for testing cultural variation. Modified versions of the experiment continue to demonstrate cultural variation (e.g. Nestor 2017), however. While we acknowledge that the strength of the variation may be less than initially thought, we follow Henrich et al. (2010) in utilising this phenomenon as a motivating example, one which still demonstrates cultural variation in what we think is a surprising domain.

[41] See Nisbett (2003) for a broader take on cultural effects on perception.

if the inference from WEIRD people to San peoples is not justified, then we should be likewise concerned about any inferences to ancient populations, for, plausibly, the socioecology of the San more closely resembles that of ancient humans than that of WEIRD populations.

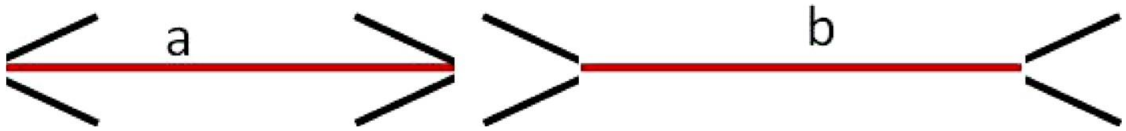

Figure 1: The Müller-Lyer illusion. Source: Henrich et al. (2010), reproduced here with the permission of Cambridge University Press.

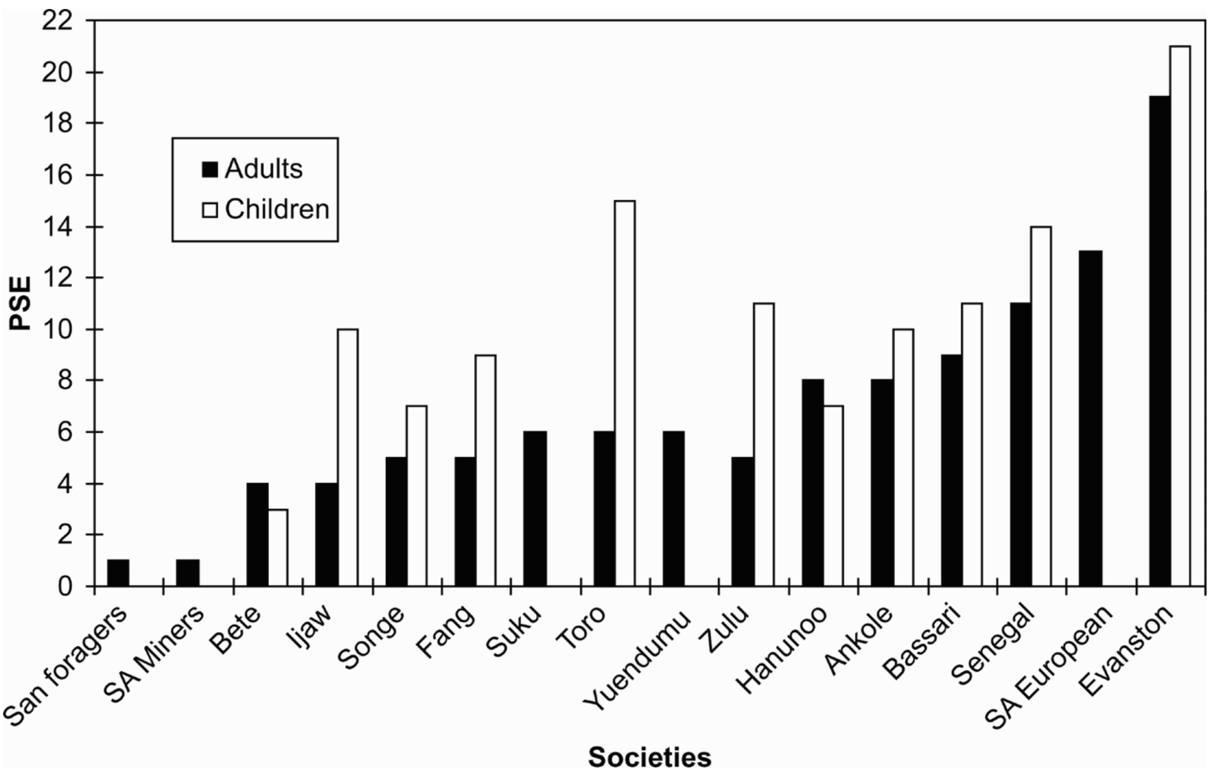

Figure 2: Results from Segal et al. (1966). The point of subjective equality (PSE) indicates the percentage increase required for Line A to be judged as equal to Line B. Source: Henrich et al. (2010), reproduced here with the permission of Cambridge University Press.

There are other examples, not involving illusions. For instance, researchers once thought, based on their studies of university students at their home institutions, that humans exhibit a right-hemisphere bias in face-recognition processing (see Henrich 2020, pp. 3-7). However, we now know that the process of becoming literate rewires our neural circuitry. One effect of this is that face-recognition processing is shifted to the right hemisphere (Dehaene et al. 2010, 2015). So the generalisation from the all-literate sample to *H. sapiens* is undermined, as it appears that people from non-literate societies do not exhibit such a bias (and, as it happens, are better facial-recognisers than literate people). Given that literate societies are relatively new, from an evolutionary perspective it would be an obvious mistake to infer on the grounds of the initial studies that ancient humans exhibited a right-hemisphere bias in face-recognition processing.

The purpose of this article is to apply this general line of critique to cognitive archaeology. We aim to draw out some of the issues raised by the challenge of cultural variation and sample diversity within the context of the historical sciences.

3. Case studies

In this section we outline four examples from work in cognitive archaeology where WEIRD sampling issues are at play. In the first two, we target specific inferences concerning the evolution of spatial cognition and long-term working memory. We then expand our scope to look at research programs as a whole; namely the affordances framework and neuroarchaeology.

3.1 Case Study 1: Wynn & Piagetian theory

Thomas Wynn is a cognitive archaeology trailblazer,[42] and one of the most cited representatives of the discipline (as of the time of writing: just shy of 6,500 citations according to GoogleScholar). Whether one agrees with his arguments or not, Wynn has been incredibly influential in the development of the field. For this reason, we pay particular attention to his work. Of course, we must be highly selective in an article of this length; but it is not from some fringe corner of the field that we select our examples. Indeed, Wynn's application of Piagetian psychological theory to the stone tool record is one of the earliest examples of a cognitive archaeological argument. This demonstrates that the problem of sample diversity was inherited (and indeed acknowledged) by cognitive archaeology from the outset; Piagtian theory's failure to generalise is one of the reasons Wynn discarded it in later work (see Wynn 2016). Our other examples demonstrate that the problem persists in various guises in the contemporary literature.

[42] As is well known to the fields' initiates: see, e.g., contributions to Overmann and Coolidge (2019).

Before beginning, we note that our comments are not intended to be an indictment of Wynn's work in general; there are many areas of his work that are not subject to WEIRD concerns. Wynn and McGrew (1989), for instance, utilise comparative data on non-human primates, and in other work, Wynn considers the implications of gross brain morphology as gained from palaeoneurological data. Our analysis merely draws attention to specific instances from Wynn's oeuvre where sample diversity issues are at play.[43]

The work of twentieth century Swiss psychologist Jean Piaget has been incredibly influential in developmental psychology and beyond. Piaget proposed a sequential stage model for individual intellectual development known as "genetic epistemology" (Piaget 1972). The stages are invariant, each necessary for the next, and based on a structuralism that conceives of intelligence as governed by a set of operational principles.[44] In 1979's seminal "The intelligence of later Acheulean hominids", Wynn applied Piaget's genetic epistemology model to the evolution of hominin intelligence. Specifically, Wynn's goal was to identify the spatial cognition capacities of ancient hominins by interpreting the 300,000 year old stone tools from Isimila Prehistoric Site, Tanzania, in terms of the final stage of Piaget's model, 'operational thought'.

Each stage of Piaget's model presents a set of operational principles—characterising development from birth until adolescence—which regulate thought. The final stage of the model sees individuals between 11 and 16 years old develop abstract reasoning, logical thought, and metacognitive reasoning resources. Moreover, according to the theory, *reversibility* and *conservation* are the two fundamental regulatory operators of these cognitive principles. Reasoning with the principle of transitivity exemplifies conservation. Schematically, if $x=z$ because $x=y$ and $y=z$ then it is due to the conservation of some property; an inference from $x=y$ and $y=z$ to the conclusion $x=z$ is based on the same principle. Reversibility is the operation that takes one, through inversion, back to the starting point (e.g., $0+n-n=0$) or, through reciprocity, to an equivalence (e.g., $p \geq q$ and $p \leq q$ lead to $p=q$). Although the lines of reasoning here can be formalised with symbols, they are intended to capture operations which, at least most of the time, we employ informally in casual settings. (For

[43] Thanks to an anonymous referee for pressing this.
[44] Although abstractions, the principles are conceived as reducible to (physical) brain activity.

further examples of these operations—including their relevance to classifications of kinship systems—see Wynn's paper.)

Wynn's interpretation of the stone tools from Isimila identified the targets of these psychological concepts in the tools' production. In doing so, he ran a minimum-capacity inference:

> "In order to manufacture all but the most rudimentary stone tools, however, flake removals must be related to one another in a fashion yielding the appropriate configuration or pattern. If a stone artefact presents a pattern of flake removals that could only have been organised by means of reversibility and/or conservation, then it must be concluded that the maker possessed operational intelligence. I will show that the later Acheulean artefacts from the Isimila Prehistoric Site present such patterns." (Wynn 1979, p. 374)

His argument ran as follows. The reduction sequence from raw material to finished tool required the toolmaker to apply four kinds of operational spatial constructs: whole-part relations, qualitative displacement, spatio-temporal substitution and symmetry. Because each of these operations requires conservation and reversibility, the final stage of Piaget's model can be located in the makers of these tools. The bulk of Wynn's paper, then, is dedicated to explaining how one can infer the four spatial constructs from the knapping method identified in the Isimila bifaces, and how these relate to Piaget's two core ingredients of operational thought. (Although the details are interesting and informative, we must skip over them here; see Wynn 1979; also Wynn 1985 for discussion.)

Wynn's specific conclusions aside, our concern is with the use of Piaget's model as mid-range theory. Did it provide epistemic licence to Wynn's projection to the past? There are many reasons that Piagetian theory has gone out of fashion (Bjorklund 1997, 2018). According to Parker and Gibson (1979, p. 400) its evolutionary application is "Lamarckian and vitalistic"; and for other researchers, its reliance on the principle of recapitulation is "dangerous" (Renfrew 1982, p. 14). Importantly for our purposes though, Piaget's developmental model has long been criticised for its limited sample size: it was based on the observations of middle-class European children, and yet proposed as a general model of human cognitive development. Furthermore, as argued by Lancy (2010) and Shweder (2010), the general applicability of Piaget's model has been thoroughly undermined by cross-cultural research. Mead (1932) demonstrated that Piaget's developmental model (in its 1929 form) fails to generalise given her studies of an Admiralty Islands small-scale society, and Luria (1976) described alternative patterns of reasoning in Uzbek peasants in Central Asia. Wynn himself notes that a desiderata of a mid-range theory is that it is cross-culturally confirmed (Wynn 1985, p. 33), yet Piaget's theory fails this test. And if the theory cannot generalise from middle-class European children to children from the Admiralty Islands, then we should be skeptical of inferences made via the theory regarding the makers of the tools found at Ismilia. Moreover, we should be even more skeptical of any generalisations from individual tool producers to other contemporaneous hominins, which Piaget's theory—and the scope of his conception of ontogeny as a singular, invariant process—appears to allow (cf. Wynn 1979, p. 383ff). It's no surprise that in later work Wynn discontinued this line of reasoning, eschewing the commitment to Piaget. It is important to note that this does not mean that the individuals who produced the Ismilia tools *did not* have the spatial constructs Wynn identifies; just that Piaget's cognitive model isn't the way to licence that inference. Wynn himself recognised this, and in later work he turned to comparative and palaeoneurological data, as well as a broad range of cognitive theories and frameworks via his subsequent teaming with psychologist Frederick Coolidge.

Wynn's early work is lauded by many cognitive archaeologists (e.g., Mithen 1996; Stade and Gamble 2019; McGrew et al. 2019; Davidson 2019), even though its hardline Piagetian justification would have few—if any—advocates today, as it helped to pave the way for a more mature discipline (and see Wynn 2016, pp. 8-10, for retroactive reflection on his Piagetian days). The case demonstrates that the problem of sample diversity in psychology was present at the beginnings of the cognitive archaeological project, and that its negative effects were acknowledged. The next subsection details a much more promising inference to the long-term working memory capacities of Neanderthals (Wynn and Coolidge 2004) by way of contemporary psychological and cognitive anthropological models.

3.2. Case study 2: Neanderthals and long-term working memory

Recall that a minimum-capacity inference takes an artefact or tool industry—say, the Levallois technocomplex, associated with Neanderthals—and, via a cognitive theory, reaches a claim about the production of the trace—say, that the producers of those prepared-core technologies had the capacity for advanced (essentially, modern or very near-modern) long-term working memory. This is the claim defended by Wynn and Coolidge (2004).[45]

Levallois reduction is a task comprising multiple steps: “The first prepares a core with two distinct but related surfaces, one, a more convex platform surface that will include the striking platform, and a second flatter production surface from which the blank or blanks will be removed. The second step prepares the striking platform itself in relation to the axis of the intended blank. The third step is the removal, by hard hammer, of the blank or blanks.” (Wynn and Coolidge 2004, pp. 473-474; see Figures 3 and 4 for a schematic representation and a photograph). When one blank is prepared, the method is called *preferential* and when two or more are prepared it is called *recurrent.* The recurrent method contains unidirectional, bidirectional and centripedal variants, each requiring different knapping techniques and platform preparation. Consequently, the ‘end products’ (e.g. Levallois points) are the result of not a single action-sequence but a plurality of operational schemas/methods (Boëda 1995).

[45] See Pain (2019) for further analysis of this line of reasoning. The conception of working memory comes from the work of Baddeley (e.g. Baddeley and Hitch 1974; Baddeley and Logie 1999; Baddeley 2001). Wynn and Coolidge also discuss how other aspects of the Neanderthal record complement this inference, so their argument is not solely based on Levallois.

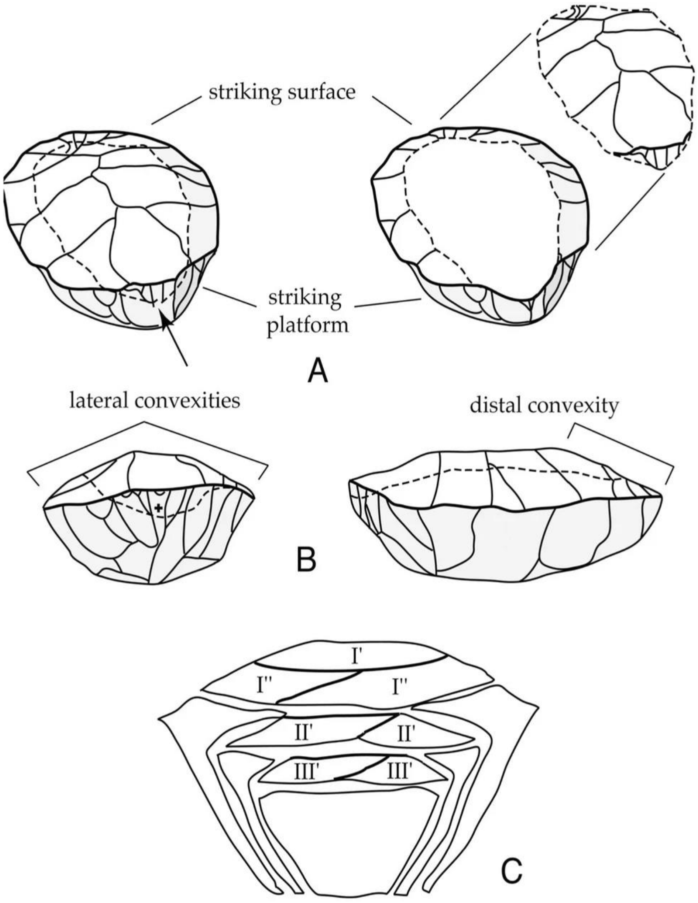

Figure 3: Stages in prepared-core tool production. Source: Ambrose (2001), reproduced here with the permission of the American Association for the Advancement of Science.

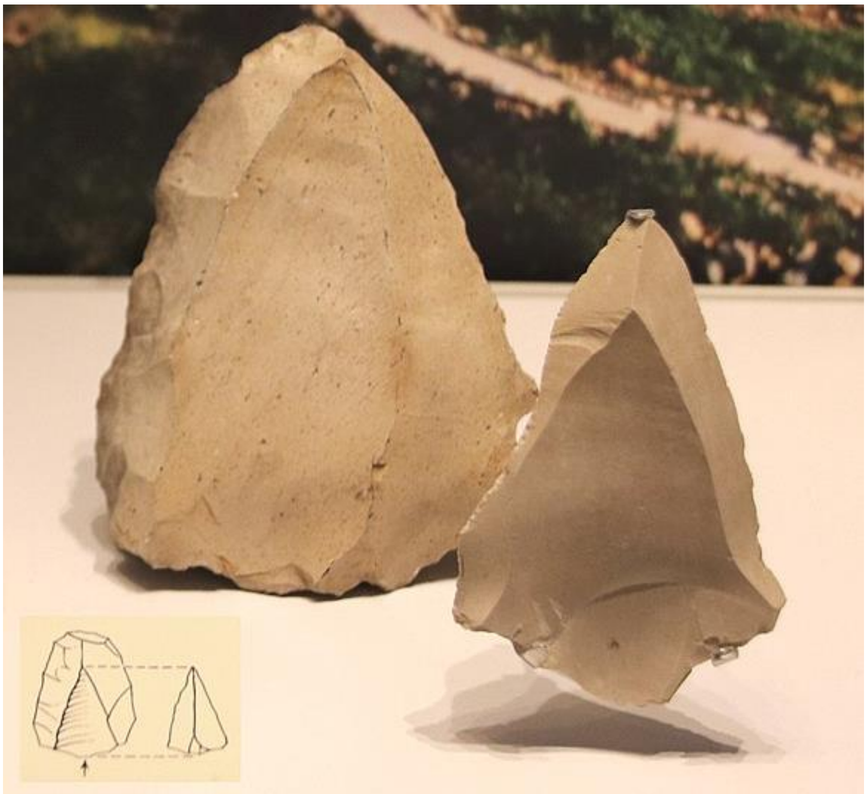

Figure 4: Levallois stone tools from Tabun Cave (Mousterian culture) 50-250 kya. Reproducible under the Creative Commons CC0 1.0 Universal Public Domain Dedication. URL: https://commons.wikimedia.org/wiki/File:Production_of_points_%26_spearheads_from_a_flint_stone_core,_Levallois_technique,_Mousterian_Culture,_Tabun_Cave,_250,000-50,000_BP_(detail).jpg

Now, what of the mid-range theory through which the cognitive claim above is epistemically licenced? In this case, Wynn and Coolidge appeal to two cognitive models, one from cognitive neuropsychology (Ericsson and Kintsch 1995; Ericsson and Delaney 1999; Ericsson et al. 2000) and the other from cognitive anthropology (Keller and Keller 1996). Here we will focus on the latter.

Keller and Keller use the practice of smithing as their primary case study. According to their model, there are three crucial aspects to skilled expertise. First, "the stock of knowledge", which is the information pool a smith acquires, builds, and maintains over many years of experience. This would include semantic/symbolic information, but also sensory information: visual, sonic, and tactile 'images' of procedures, materials, and so on. Second, an 'umbrella plan'; a mental model or representation of the end product intended as well as the tasks and subtasks required for its production. Again, this would include both semantic/symbolic information and visual, sonic, and tactile information. Umbrella plans are similar to the concept of retrieval structures employed in cognitive neuropsychology: a given task recalls the relevant retrieval structure from long term memory, containing cues that facilitate encoding and retrieving associatively linked knowledge. Third, 'constellations', which are the requisite ideas and mental images, as well as the materials and tools, that enable each step of the process to be successfully accomplished or begun anew. Depending on the tasks involved, constellations can be deployed more or less automatically, or with full conscious attention. Active feedback between the smith and the constellation shapes the smith's actions and decisions in deploying the constellation.

Wynn and Coolidge claim that, in light of this model, Levallois reduction (and perhaps other Neanderthal technologies) demonstrates that the producers had long-term working memory capacities. They argue that, like smithing, prepared-core technology involves successfully completing a complex sequence of tasks requiring a stock of knowledge, an umbrella plan, and constellation, and that these concepts can be "directly applied" (p. 474). They say:

> "The sequence of actions that can be reconstructed for Levallois reduction resembles the sequence of action documented by the Kellers for blacksmithing: a sequential task with definable steps during which the artisan makes choices among a variety of specific techniques and procedures in order to complete each step and, ultimately, produce a finished product" (Wynn and Coolidge 2004, p. 474).

As is further outlined by Pain (2019), Wynn and Coolidge provide good reason to think the models' concepts map nicely onto aspects of the target phenomenon (the Levallois), so the conceptual framework is at least plausible.[46] Nonetheless, smithing may well be more culturally variable than the Kellers and Wynn and Coolidge implicitly assume, potentially restricting the scope of the model. Smithing, of course, is not an exclusively WEIRD activity. So cross-cultural research stands to strengthen or undermine the plausibility of the model's general application. As such, the cognitive capacities involved in the production of Levallois technologies may be distinct from that of modern smithing, undermining the analogy. This

[46] What's less plausible is Wynn and Coolidge's subsequent species-wide generalisation from the individual producers of the Levallois to Neanderthals broadly, e.g., "We believe that Neandertals regularly employed L-TWM [long-term working memory] that was as effective as any documented for modern humans" (2004, p. 473). It is important to distinguish between a past projection and a species-wide generalisation from that projection. For even if some Neanderthals demonstrated a capacity (a producer of some tool) that does not imply that the capacity is distributed universally through the population/species.

raises an important question: are all cases of artisan expertise organised in broadly similar ways, cognitively speaking? The concepts might appear to 'fit', but that fitting might yet overstate the long-term working memory of those ancient knappers (some interpretations from ecological psychology, and more broadly anti-representational approaches, would be consistent with this line of reasoning). So, even if the Keller's model is applicable to skilled expertise in particular domains, it not only might fail to generalise, but also might not be suitable for projection to the deep past (after all, the Levallois dates as far back as roughly 300,000 years ago, and the last common ancestor of Neanderthals and *H. sapiens* lived roughly half a million years ago). Wynn and Coolidge admittedly are aware of this problem: "But are the cognitive underpinnings the same, that is, did Neandertal stone knappers have stocks of knowledge, make umbrella plans, and use constellations?" (p. 474). In their efforts to epistemically licence their inference, Wynn and Coolidge turn to modern skilled knappers, and they cite the work of Boëda (1995) and Baumler (1995). But these papers only describe the techniques involved in Levallois reduction, not cognitive underpinnings. Of course, it is relevant to take into account tool morphology, assemblage analysis and the constraints of knapping processes.[47] But testing the cognitive model, ideally utilising a diverse sample of participants, is also required.

Of course, all too often cognitive models based on culturally diverse samples are simply not available—precisely because of the bias towards WEIRD samples in the cognitive and behavioural sciences. Furthermore, for the most part, it is not the role of cognitive archaeologists to develop such models. Cognitive archaeologists are typically consumers, not producers of such models. But clearly, all things being equal, models licenced by data from culturally diverse samples should be preferred over those that are not. And where such models are not yet available, this could be acknowledged. The same point can be made about anthropological data: it is likely to be anthropologists, not cognitive archaeologists, who would provide rich descriptions, for example, of African indigenous smithing. Nonetheless, where

[47] For all we have said, Wynn and Coolidge's conclusion may be right. Our point here is that it is too quick to assume as much based on their argument. Smithing may evince cultural variation in cognition; skilled expertise may be more cognitively heterogenous (within and/or across cultures) than assumed here—or may even necessitate a more representation-light explanation than the Kellers' model allows. These possibilities are empirically tractable and testable.

available, such data stands to strengthen or undermine the epistemic licence for inferences such as Wynn and Coolidge's above.[48]

This example, like the Piagetian one preceding it, demonstrates that, in relying on models from the cognitive sciences, minimum-capacity inferences are at risk of inheriting the problem of cultural variation and sample diversity. However, perhaps unlike the Piagetian one, here we have an example of an argument that looks prima facie plausible (Pain 2019) before considering that challenge. In sum: further testing of the cognitive model using culturally diverse samples stands to strengthen or undermine the projection to the past.

3.3 Case study 3: Affordances

The previous two examples targeted specific inferences in cognitive archaeology, focusing on the work of Wynn. In the following two case studies, we broaden our scope to look at the challenge of cross-cultural variation and sample diversity to research programs more generally.

The dominant theoretical paradigm in contemporary cognitive science is representationalism/computationalism. Over the last 30 or so years, this dominance has come under increasing pressure from advocates of so-called 4E (embodied, enactive, extended and embedded) approaches. However, the situation in cognitive archaeology is somewhat different. This is partly due to a "founder effect" (Mayr 1942). Two archaeologist/psychologist duos were particularly influential in the development of cognitive archaeology: Coolidge and Wynn (as previously discussed), and Noble and Davidson (see e.g. 1996). And while the former duo work primarily in a representational framework, Noble and Davidson appeal to Gibsonian ecological psychology (see Davidson 2019 for a retrospective on the debate between these two). So frameworks that reject internalist, representation-focused accounts of cognition have been, and continue to be, a mainstay of cognitive archaeology (e.g., Malafouris 2020; Overmann 2017; see papers in DeMarrais et al. 2004; indeed, even Wynn (2020) has begun to engage with affordance theory).

So far our discussion has focused on Wynn's early- and mid-career work, which means it has been confined to representational/computational approaches to cognitive archaeology. In this section we want to broaden it to 4E approaches. We will focus on affordance theory, as

[48] Thanks to an anonymous referee for pressing these points.

this was Noble and Davidson's chosen framework. We will not target specific cognitive archaeological inferences here.[49] Rather, we want to track some work in the development of affordance theory, and show that the concept in general is likely to target phenomena which are culturally influenced. Consequently, care must be taken when inferring from WEIRD samples to ancient populations when appealing to the framework.

Affordances are understood as products of interactions between an organism and its environment. As different organisms have different body types, the same environment will produce different affordances for different organisms. For instance, to use an oft-quoted example: the wall in my office does not offer me the affordance of walking (I cannot walk on walls).[50] This is not the case for the spider that lives in the corner of my office (spiders can walk on walls). Walls (and ceilings) offer spiders the affordance of walking. Gibson (1979) argued that organisms perceive affordances directly, and the theory has subsequently become popular with those attempting to challenge the representational paradigm (e.g. Chemero 2009). Initial experimental work with human subjects focused on biomechanical models, where affordances were treated as a ratio between some aspect of a subject's body scale and some feature of the environment; for instance, between leg height and the height of a stair, or between eye height and the width of a gap (see e.g. Mark 1987; Burton 1992, 1994; Mark et al. 1999). A range of experiments appeared to generate reliable results. Most famously, Warren found a consistent ratio between leg height and stair height with regards to the boundary between stairs deemed "climbable" and stairs deemed "unclimbable" (Warren 1984). Similar effects have been found in the case of eye height and the width of a gap deemed "crossable" (Jiang and Mark 1994). This work appears to lend weight to the body-scale account of affordances, and a body-scale account is one that would plausibly generalise across human populations.

However, as Chemero (2009, p. 143) notes, the property of organisms we are really interested in when it comes to affordances is their *ability* to perform some action. Aspects of body scale, such as leg height, are just a proxy for our ability to climb stairs that is easily quantifiable. This idea was tested by Cesari et al. (2003). Warren's original stair climbing experiment used US college students as subjects, so the participants were mainly younger

[49] Though see, e.g., Silva-Gago et al. (2021), for a recent example of cognitive archaeological research via affordance theory.
[50] Shifting to single-author pronouns here for ease of expression.

adults. By contrast, Cesari et al. used subjects with a range of different ages, including elderly people. They found that the ratio between leg height and stair height when it comes to the transition between climbable and unclimbable stairs varied considerably according to age. For elderly people, this ratio was considerably lower than the rest of the participants. Rather, flexibility seemed to also be playing an important role. Similar results have been produced in the case of gap crossing tasks (Chemero 2009, p. 145). Other work has contrasted subjects' *judgments* of their ability to cross a gap and gap length with leg length and gap length (Chemero et al. 2003). The former ratio was found to be much more highly correlated with the maximum gap judged crossable than the latter. Taken together, these results suggest that it is ability, not body scale, which is relevant in the perception of affordances.

All this is important for our purposes when we take into consideration two points. First, body-scale is a biological category, whereas ability is influenced by a range of biological and cultural factors. Stair climbing ability is a product of properties like leg height and flexibility, but it is also a learnt skill, and hence, to some extent, a product of culture. Furthermore, stair climbing ability will be to some extent determined by the prevalence of stairs in the environment—again, a cultural property. So populations that do not inhabit built environments will have different stair climbing abilities to those that do. The bottom line is that, if affordances are abilities, then they are influenced by culture. The situation here is roughly analogous to the case of the Müller-Lyer illusion (see section 2). There a 'base-level' perceptual effect was shown to be a product of architectural environments. Something similar is true of the trajectory of experimental work in affordances: an effect once thought to be more strictly confined to the biological domain is now understood as, to some extent, culturally determined. Second, the cited research above was carried out, not merely on WEIRD subjects, but for the most part, US college students.[51] And US colleges tend to be stair-rich environments.[52] Together, these two points suggest that any kind of generalised claim about affordances (beyond, perhaps, their existence) will need to be based on cross-cultural samples. And this is no different in the case of cognitive archaeology's use of affordance theory.

[51] Details are as follows. Warren (1984): 54 male US college students; Cesari et al. (2003): 39 US citizens; Jiang and Mark (1994): 56 US college students; Chemero et al. (2010): 26 US college students.

[52] Of course, there are many non-WEIRD cultures with stair-rich environments. Though this is much less true of small-scale forager societies.

3.4 Case study 4: Experimental neuroarchaeology

Recently, cognitive archaeologists have developed a new strategy for producing inferences to the past. As we have seen, traditional strategies—such as minimum-capacity inferences—rely on interpreting the archaeological record using a model from the cognitive sciences. In contrast, experimental neuroarchaeology takes modern human subjects and investigates their brain activity during knapping tasks using neuroimaging techniques. This technique inherits a different set of theoretical and methodological problems. First and foremost among these is that modern human subjects are not, for instance, *H. erectus*; so any argument produced by this strategy is one by homology. This means that the strength of the inference involved depends on the neural similarity between modern *H. sapiens* and (again, for example) *H. erectus*, and the value of that ratio is very difficult to ascertain.

We will put these concerns aside here. Instead, we want to focus on issues of sample diversity in this research. In many ways, neuroarchaeology suffers similar problems to those we have identified in more traditional cognitive archaeology. But there is an upside here: the experimental aspect of neuroarchaeology allows us to more directly test cultural impacts on cognitive and behavioural capacities. In turn, this further illustrates the importance of sample diversity.

The majority of work in neuroarchaeology has so far focused on tool-language co-evolutionary hypotheses. In particular, research has tried to identify if there is any neural overlap between toolmaking and language production (e.g. Stout et al. 2008; Putt 2019). For instance, in their 2008 study Stout and colleagues took three expert tool knappers and used fluorodeoxyglucose positron emission tomography to assess the areas of the brain co-opted by both Oldowan and late Acheulean tool production tasks (Stout et al. 2008). They found that, compared with Oldowan toolmaking, late Acheulean toolmaking produced increased brain activity in areas of the brain associated with language production (in particular, Broca's area). More recently, Shelby Putt and colleagues have expanded the scope of this work using functional near-infrared spectroscopy (Putt et al. 2017; see Putt 2019 for an overview). Putt and colleagues were particularly interested in investigating whether the mode of learning—either verbal or non-verbal—had any effect on the brain regions co-opted by Oldowan and late Acheulean tasks. Participants in the experiment were taught to knap using either spoken language or via visual aids alone. Their results indicate that the mode of learning has a significant impact on parts of the brain co-opted during Acheulean-style toolmaking. This suggests that modern day knappers may be rehearsing the verbal instructions they were exposed to during learning.

Importantly though, the participants in these experiments were all WEIRD, and indeed either university professors or college students from the United States.[53] And this is worrying, as the need for sample diversity does not stop at Henrich et al.'s level of explanation (behavioural sciences) but is also a priority for neuroscience (Chiao and Chen 2010). According to Chiao (2009), 90% of peer-reviewed neuroimaging research comes from Western countries. Yet even between Westerners and East Asians there appear to be neuroscientific-level instances of cultural variation:

> "Westerners engage brain regions associated with object processing to a greater extent relative to East Asians, who are less likely to focus on objects within a complex visual scene (Gutchess et al. 2006). Westerners show differences in medial prefrontal activity when thinking about themselves relative to close others, but East Asians do not (Zhu et al. 2007). Activations in frontal and parietal regions associated with attentional control show greater response when Westerners and East Asians are engaged in culturally preferred judgments (Hedden et al. 2008). Even evolutionarily ancient limbic regions, such as the human amygdala, respond preferentially to fearful faces of one's own cultural group (Chiao et al. 2008, [...]). Taken together, these findings show cultural differences in brain functioning across a wide variety of psychological domains and demonstrate the importance of comparing, rather than generalizing, between Westerners and East Asians at a neural level." (Chaio and Cheon 2010, p. 89).

Moreover, there is a dearth of neuroimaging work utilising individuals from small-scale populations. While fMRI machines are not easily transportable, EEG methods (for instance) are non-invasive and the technology far more mobile. Chiao and Cheon's call for more effort to investigate neuroscientific research questions via sampling more diverse populations thus

[53] Details are as follows. Stout et al. (2008): 3 professional archaeologists; Putt et al. (2017): 31 participants, recruited via posted flyers (presumably around the campus of Indiana University, Bloomington).

looks well justified.[54] Meanwhile, in the case of neuroarchaeology, it is important that inferences licenced via culturally localised samples are treated with caution.

Indeed, even the trajectory of research from Stout to Putt illustrates this. Stout purported to show neural overlap between toolmaking and language. Putt observed that Stout's experiments did not control for the method of learning of the participants. In testing the difference between visually taught participants and verbally taught participants Putt demonstrated that a cultural force—learning—could influence the results of neuroscientific testing. This trajectory serves as a cautionary tale, yet also illustrates an important opportunity. As an experimental research program, neuroarchaeology can actually test cultural variables, even when operating with limited sample diversity—Putt's work shows that the way in which a skill is learnt affects the neural substrates it co-opts. This is not an option for traditional cognitive archaeology, which operates by interpreting the archaeological record through a cognitive model.

4. How worried should we be?

In the previous section, we outlined examples in which cognitive archaeologists use models, frameworks, or experimental research from the cognitive sciences which have been built using limited samples or are otherwise susceptible (or potentially susceptible) to the challenge from cultural variation. In this section, we look in more detail at how worrying this situation is. We begin with some conceptual work clarifying the issue of generalisation and sample diversity in the context of inference to the deep past. Then, drawing on Henrich et al. (2010), we look at some conditions under which generalisations from limited sample sizes might be justified.

4.1 Sample diversity, generalisation, and inference to the past

Recall the issue of sample diversity as it faces the cognitive sciences. The problem is that, by generalising from WEIRD samples, researchers assume that either there is no cross-cultural variation, or that WEIRD people are generally representative of the species. However, cross-

[54] Suppose this was done and the results were broadly similar: Acheulean-style but not Oldowan-style knapping engages language-processing areas of the brain across a very broad sample. This corroboration would add licence to past projection, in our view, rendering it more plausible than we'd otherwise grant based on a small and narrow sample.

cultural research has shown that in many cases these assumptions do not hold. So sample diversity is required to make reliable claims at the larger population- or species-level.

Now, take the case in which we use a cognitive model derived from WEIRD samples alone, and then apply that model to some artefact from the stone tool record using a minimum-capacity inference. What precisely is the worry here? In the first instance, notice that this does not *necessarily* undermine the inference to the cognitive capacity of the maker of the artefact. If the minimum-capacity inference holds—and that is a big 'if'—then we should be confident that the maker of the artefact at least had the relevant capacity. Remember, the lesson from Henrich et al. (2010) is *generalise* with care; at this stage in the process, no generalisation has been made. Though perhaps unlikely given the differences in lifeways between WEIRD people and past peoples, it is at least logically possible that we are successfully inferring from one culturally localised sample to another. A distinct problem arises, however, when we generalise from that minimum-capacity inference to a population-wide or species-wide claim in the deep past. That generalisation would be justified if we had reason to believe that there was no cultural variation in the population (which is perhaps only plausible for pre-*Homo* populations) or that WEIRD people were generally representative of the species and diachronically so, but of course this is not the case. If, however, the cognitive model at hand has been corroborated by cross-cultural research, then, all other things being equal, researchers have greater epistemic licence to extend the inference to that of the population- or species-level.

To illustrate this point, consider Wynn and Coolidge's minimum-capacity inference concerning the long-term working memory abilities of Neanderthals (section 3.2). The inference here used Keller and Keller's cognitive anthropological model of skilled expertise, based on their observations of smithing. In the absence of cross-cultural testing of that model, the minimum-capacity inference may well licence claims regarding the long-term working memory capacities of those individuals, and perhaps their wider cultural group, found in situ with Levallois technology (that is, if we are satisfied that the past projection via that cognitive model is provisionally licenced—we have discussed above some challenges for this). In other words, we may be satisfied that the inference takes us from the technology, via the cognitive model, to the cognitive capacity of the individuals who produced the technology. But even then we would not be justified in making species-wide claims regarding Neanderthals—that Neanderthals in general had modern or near-modern long-term working memory—as Wynn and Coolidge do (and then go on to draw additional inferences based on that generalisation, see 2004, pp. 478ff). On the other hand, if Keller and Keller's model was corroborated by cross-cultural research, all other things being equal, the inference would more reliably allow species-wide generalisation. At the very least, such inferences would gain credibility. The lesson is this: sample diversity allows us to make more plausible generalisations in the present, and this is important insofar as it provides greater epistemic licence for generalisation in the past.

4.2 Are there cases where generalisations from WEIRD samples are 'safe'?

We have thus far argued that lack of sample diversity is inferentially problematic for cognitive archaeology, insofar as it utilises cognitive models licenced by data on WEIRD subjects, and outlined a range of cases—both specific and more general. In this section, we want to examine some cases where traditional minimum-capacity inferences might be more plausible. Henrich et al. (2010) suggest a range of conditions under which generalising from WEIRD samples to contemporary humans might be provisionally justified:

1. Effects found in "[...] cognitive domains related to attention, memory and perception…".
2. Effects "... measured at a physiological or genetic level".
3. "... generalisations from one well-studied universal phenomenon to another similar phenomenon".
4. Effects demonstrated "...in other species, such as rats or pigeons…".
5. Effects "...which are evident among infants".
6. Effects in brain regions "...less responsive to experience". (Henrich et al. 2010, p. 79)

We think that this list can be reduced to two conditions: [a] where an effect is reliably thought to be a product of biology; [b] where an effect is reliably thought to be a product of a cultural universal. 2 is a way of reaching the conclusion in [a], while 4, 5 and 6 are methods of eliminating culture as a potential cause of an effect, thereby increasing the likelihood that it is biological. 3 is one way of reaching the conclusion in [b]. Finally, the intuition driving 1 looks to be related to [a], but Henrich et al. note that the work of their paper "...does not bolster this intuition" (Henrich et al. 2010, p. 79). Simply put, there are two reasons why we might think a cognitive effect might be universal—it is either part of human biology or it is, for whatever reason, common to all human cultures. Are there areas of research to which cognitive archaeology contributes that satisfy these conditions? We consider cases from research on language and theory of mind, but begin with two general observations.

First, a distinct possibility is that satisfying [a] in the present may be insufficient for the production of reliable inferences to our deep past. This will occur as the morphology of hominin body shapes changes through phylogenetic space. Thus effects that generalise across modern day human populations due to their being measured at a physiological level will not apply as we move further back in time.

Second, it might be thought that long periods of cultural uniformity, such as the Acheulean, signal that either [a] or [b] has been satisfied.[55] This would only apply to those who commit to *cognitive* explanations of uniformity—those who posit demographic or environmental causes might not consider either to be satisfied (see Pain 2019 for an overview).

As a result of Chomsky's influence, the view that our ability to produce language is innate and domain specific (e.g. Berwick and Chomsky 2015; Chomsky 2007) is widespread. If we thought this view was true, then we might think that [a] would be satisfied. The evolution of language was a key focus of early cognitive archaeological studies (e.g., Gibson and Ingold 1993; Mithen 1996; Noble and Davidson 1996), so perhaps much of this work is immune from sample diversity concerns. However, there is increasing acknowledgement amongst researchers regarding the causal importance of culture in the evolution of language. This includes gene-culture coevolutionary accounts (e.g., Laland 2017; Tomasello 2005, 2010), and more radical accounts that deny biologically produced language-specific capacities in human ontogeny (e.g., Christiansen and Chater 2016; Heyes 2018). If these theories are on the right track for language, [a] would either not be satisfied at all, or look much more difficult to satisfy. In addition, recent tool-language co-evolutionary hypotheses look to develop accounts where the evolution of syntactical features of language involves co-opting existing capacities evolved to support toolmaking (e.g., Stout et al. 2008; Stout and Chaminade 2012; Planer and Sterelny 2021). Syntactical capacities are sometimes thought to be one of—if not *the*—biologically-endowed, domain-specific language capacity. These accounts, however, suggest that the

[55] There are those who question notions of uniformity during the Acheulean (e.g. Stout 2011). Naturally, people of this persuasion will not be particularly persuaded by this point.

phylogenetic ancestry of that capacity was heavily driven by culture. This again raises questions about the ability to satisfy [a].

‘Theory of mind’ or ‘mindreading’ refers to our ability to infer, understand, or simulate the mental states of another individual. For instance, one of us might infer from the yawns of an audience that participants in the lecture are bored. Our theory of mind capacities are embedded in a broader framework of orders of intentionality. These orders begin with the awareness of our own mental states, and progress from there. For instance: a lecturer intends (1); that their audience understand (2); that Stout believes (3); that Chomsky disagrees (4); with Tomasello’s commitment to domain-general processes in theorising about language (5); and so on. Theory of mind is thus located in the second order of intentionality and beyond. Recently, Cole (e.g., 2016, 2019) has produced a range of studies attempting to correlate theory of mind capacities and orders of intentionality with the lithic record (see also Planer 2017). Now, one might think that the ability to interpret other people’s mental states is a culturally produced capacity (Heyes 2018) and is perhaps also *distinctive* of human beings. Theorists of this persuasion may well think that theory of mind is something approaching a cultural universal, and hence that it satisfies [b]. However, Cole (2019) has argued that the record suggests more variation in orders of intentionality capacities than universal models indicate, which would undermine this conclusion.

We have run, very briefly, through two cases where one might have reason to think that [a] or [b] is satisfied—however, recent work tells against this conclusion in both cases. Of necessity we have given these research programs short shrift; a full analysis is beyond the scope of this article and is an avenue for future research.

5. Conclusion and Future Directions

Henrich et al. (2010) identified an important problem for the behavioural and cognitive sciences: the tension between the empirical reality of cultural variation and the narrow cultural representativeness of most study samples. Cultural variation in many domains engenders cognitive variation, and WEIRD participants are often not representative of the human species. We have argued that cognitive archaeology inherits this problem insofar as it uses models (and frameworks, experimental research, etc.) from the cognitive sciences to provide epistemic licence for its inferences to the past. Our examples demonstrate the breadth of the challenge to cognitive archaeology: it is not restricted to any specialisation or theoretical paradigm, and has historical roots.

We have claimed that rather than being methodologically flawed, inferences to cognitive capacity from a physical trace via a cognitive model stand to be strengthened or called into question by further testing of the cognitive model against more culturally diverse samples. Corroboration is possible, so the situation is not totally dire; there is cause for *optimism* (Currie 2018). We have also considered that there may be some cases where the force of the problem is mitigated—hypothetically, some minimum-capacity inferences from particular cognitive models might be more or less plausible in light of *independent* reason to think an inference can be applied outside the sample, e.g., when an effect is reliably thought to be the product of biology, or when an effect is reliably thought to be the product of a cultural universal. That said, our brief analysis suggests that these conditions look difficult to satisfy. A full analysis is an important avenue of future research.[56]

Finally, as we have stressed throughout, theoretical resources from the cognitive sciences are an important part of the cognitive archaeologist's inferential toolkit. Yet cognitive science is in a state of live debate. This poses a problem for cognitive archaeologists: which principles should guide cognitive archaeologists when selecting a cognitive model for use as mid-range theory? Killin and Pain (2021) have recently proposed two complementary, though non-exhaustive solutions: theory choice should be guided by consilience (convergence from multiple independent lines of evidence) and methodological pluralism. This article proposes a further, compatible principle. That is, all other things being equal, cognitive archaeologists should prefer models tested and corroborated by diverse samples; or, at the very least, where effort has been undertaken to assess how well the results (very often from WEIRD samples) might project to the deep past. Since cultural effects produce cognitive and behavioural variations even in contemporary populations, due caution must be taken when projecting inferences to ancient hominins, and it would be no bad thing to heed this caution in model selection. After all, the above considerations suggest that past projections require a

[56] Another route to optimism utilises causal-association inferences, not discussed in this paper. See Currie and Killin (2019) and Pain (2019) for analysis; and Killin and Pain (2021) for the role of multiple independent lines of evidence converging on capacity in cognitive archaeological theorising.

developmental cognitive mid-range theory, one that takes into account the effects of culture on cognition. In turn, this implies that reconstructing ancient minds requires, to some degree at least, reconstructing their cultures.[57] This challenge too is suggestive of future research.

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Statement of Contribution

This thesis is submitted as a Thesis by Compilation in accordance with https://policies.anu.edu.au/ppl/document/ANUP_003405

I declare that the research presented in this Thesis represents original work that I carried out during my candidature at the Australian National University, except for contributions to multi-author papers incorporated in the Thesis where my contributions are specified in this Statement of Contribution.

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PART 2

SYNTHESIS

CHAPTER 4

Mind the gap: A more evolutionarily plausible role for technical reasoning in cumulative technological culture

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Abstract: How do technologies that are too complex for any one individual to produce ("cumulative technological culture") arise and persist in human populations? Contra prevailing views focusing on social learning, Osiurak & Reynaud (2020) argue that the primary driver for cumulative technological culture is our ability for technical reasoning. Whilst sympathetic to their overall position, we argue that two specific aspects of their account are implausible: first, that technical reasoning is unique to humans; and second, that technical reasoning is a necessary condition for the production of cumulative technological culture. We then present our own view, which keeps technical reasoning at the forefront whilst jettisoning these conditions. This produces an account of cumulative technological culture that maintains an important role for technical reasoning, but that is more evolutionarily plausible.

1. Introduction

A striking feature of human technological culture is our ability to produce technologies that are too sophisticated for any one individual to invent alone. This phenomenon has been termed *cumulative technological culture* (Boyd and Richerson 1996; Boyd, Richerson, and Henrich 2011; Richerson and Boyd 2008; Tomasello 1999; Tomasello, Kruger, and Ratner 1993).

Over the past 30 years, attempts to understand the cognitive mechanisms driving cumulative technological culture have focused on social cognition. Consequently, socially-orientated types of cognition have been typically thought to be the key driver of the apparent gap between tool use in humans and other species of (Boyd, Richerson, and Henrich 2011;

Tennie, Call, and Tomasello 2009). This assumption has produced a lot of theoretical work on teaching, innovation and imitation, metacognition and theory of mind. Central to this focus is the idea that *only* the type of high fidelity cultural inheritance made possible by human social cognition is sufficient to generate the rapid spread and persistence of beneficial technologies within populations required for *cumulative* technological culture. According to this view, social cognition is the key innovation in human evolutionary history that explains the prodigious complexity of our technological culture.

In a recent paper, Osiurak & Reynaud (2020) offer a compelling alternative hypothesis.[58] They propose that *technical reasoning*—a type of non-social, technical cognition grounded in the ability to reason about physical objects—is the primary cognitive mechanism driving human cumulative technological culture. In short, they claim that, whilst high fidelity social cognition is necessary for cumulative technological culture, it is not sufficient. Rather, technical reasoning is the proper explanation for complex human technological culture and the key evolutionary innovation that sets our culture aside from that of other animals. In this paper, we critically assess Osiurak & Reynaud's proposal. Although we agree that technical reasoning has been under-theorised in the study of cumulative technological culture, in our view, Osiurak & Reynaud's account over-emphasises human uniqueness. Instead the evidence favours a more gradualist, co-evolutionary hypothesis.

Our critique targets two particular aspects of Osiurak & Reynaud's account. First, we think that their account is unduly *species-specific*. Osiurak & Raynaud claim that technical reasoning is both unique to *Homo sapiens* and a necessary condition for the production of cumulative technological culture. Thus, in their view, a clear gap exists between humans and non-humans in terms of the type of culture they display and the cognition under-writing it. The evidence does not, however, support such a strong claim. Rather, we think it is more indicative of differences of degree rather than differences of kind. First, there is empirical support for the presence of cumulative technological culture in non-human animals (Yamamoto, Humle, and Tanaka 2013; Vale et al. 2017; Hunt and Gray 2003; Hunt 2014) and in hominin assemblages from the lower Palaeolithic (Stout 2010; 2011; 2019). There is also compelling evidence of non-human causal and technical reasoning which Osiurak & Reynaud downplay in presenting their species-specific account. For example, in experimental problem solving tasks New

[58] Here we focus primarily on Osiurak & Reynaud (2020), as it is their most recent and systematic treatment of the issues; but see also De Oliveira, Reynaud, and Osiurak (2019); Osiurak and Badets (2016); Osiurak et al. (2016); Reynaud et al. (2016); and Zwirner and Thornton (2015) for important earlier work on the role of technical reasoning in cumulative technological culture.

Caledonian Crows (*Corvus menuloides)* demonstrate analogical reasoning (A. H. Taylor et al. 2009; A. Taylor et al. 2009), meta-tool use (Alex H. Taylor et al. 2007), causal pre-planning (Bugnyar 2019; Gruber et al. 2019) and the ability to understand unobservable causal properties (A. H. Taylor, Miller, and Gray 2012). This evidence aside, the historical failure of claims to human uniqueness should be enough to warn us that relying on it in our theorising is a risky strategy (Boyd 2017, 11). The intuitively appealing idea of a great gap between us and other animals has led many theorists astray, and we worry that Osiurak and Reynaud are in danger of also falling into this trap. The evidence from cognitive archaeology and comparative psychology supports a much more gradualist account of cumulative technological culture, where humans are outliers on a continuum rather than islands.

Our second concern with Osiurak & Reynaud's account is their commitment to a *cognition-first* narrative regarding the emergence of cumulative technological culture. In their view, technical reasoning is both a domain-specific cognitive mechanism and a necessary condition for the production of cumulative technological culture. They are thus committed to a particular causal story—one in which the causal arrow runs from technical reasoning to cumulative technological culture. This commitment leads to an important theoretical choice.

On the one hand, Osiurak & Reynaud could posit a cognitive "leap" in the evolution of cumulative technological culture. If this is the case, then they are forced to posit the sudden appearance of technical reasoning in humans. This is unappealing, as evolutionary theory tells us that the *de-novo* generation of an entirely new domain-specific complex cognitive mechanism via undirected means is at best highly unlikely, and at worst implausible (Fisher and Bennett 1999). If Osiurak & Reynaud adopt this type of saltationist, one-shot picture then the burden of proof is on them to provide a plausible account of the evolutionary origins of the mechanisms underwriting technical reasoning, beyond mere reference to adaptive value. What mechanisms predate technical reasoning? If there was indeed a leap in the evolution of human cognition that accounts for our capacity for cumulative technological culture, what gradual genetic and developmental changes led to that leap? Such concerns make this option an unattractive theoretical move.

On the other hand, Osiruak & Reynaud could adopt a more gradualist account of the emergence of technical reasoning. But then this would undermine the species-specific aspect of their view. Their claim that tool use and construction is the product of distinct cognitive mechanisms across non-humans and humans looks strained; especially given the aforementioned evidence of cumulative technological culture and causal reasoning in non-human animals. More plausibly, both technical reasoning and cumulative technological culture are found in a variety of species at different levels of sophistication. Furthermore, we argue that evidence of cumulative technological culture in non-human animals renders the species-specific and cognition-first aspects mutually exclusive.

We think that a more evolutionarily plausible account of technical reasoning must abandon Osiurak & Reynaud's species-specific and cognition-first commitments and instead focus instead on co-evolution, positive feedback loops and gradualism. In slogan form: you need to be smart to make tools, but making tools also makes you smarter. The story we tell aims to properly incorporate this second clause. In our view, the cognitive capacity for technical reasoning (whether it be socially learned, domain-general or domain-specific) is central to the production of cumulative technological culture, but predates the emergence of *Homo sapiens*. This initial rudimentary technological reasoning capacity was then amplified in the Hominin lineage as the use and production of tools became more complex and there was, in turn, selection for more sophisticated types of technical reasoning. Rather than arising *de-novo* and in a relatively isolated manner, there were important feedback loops in both the evolution and development of cumulative technological culture and technical reasoning which resulted in a gradual ratcheting of technological capacity over time. Ultimately, our theory maintains a central role for technological reasoning in the production of cumulative technological culture, whilst avoiding the problems associated with the species-specificity and cognition-first aspects of Osiurak & Reynaud's approach.

We begin with an outline of traditional, social-cognition based accounts of cumulative technological culture, and contrast these with Osiurak & Reynaud's view (Section 2). We then argue against the human-specificity and cognition-first aspects of their account (Section 3). Finally, we present our own view and discuss the broader methodological benefits of co-evolutionary hypotheses (Section 4).

2. Social vs non-social accounts of cumulative technological culture

In this section we give a more detailed outline of Osiurak & Raynaud's reasons for claiming that technical reasoning is the driver of cumulative technological culture. We highlight the species-specific and cognition-first aspects of their approach. We begin by giving an overview of the prevailing accounts of cumulative technological culture, which focus on socially-orientated types of cognition. We then use these as a foil to Osiurak & Reynaud's alternative view, so that the points at which they disagree, and the justification for these disagreements, is made clear.

2.1 Tomasello and Boyd & Richerson's accounts of cumulative technological culture

There is no doubt that the production, spread, maintenance and accumulation of human technological knowledge within populations is a deeply social phenomenon. It is thus unsurprising that attempts to understand the cognitive drivers of cumulative technological culture have focussed on social cognition. While these include teaching, innovation and imitation, metacognition and theory of mind, much of this work has targeted imitation. Here we briefly survey two of the most prominent accounts in this tradition; Tomasello's ratchet effect (Tennie, Call, and Tomasello 2009; Tomasello 1999; Tomasello, Kruger, and Ratner 1993) and Boyd's cultural niche hypothesis (Boyd and Richerson 1988; Boyd, Richerson, and Henrich 2011)..

Tomasello's account begins with the observation that there is significant divergence between the cognitive and behavioural skills we see in human populations and those we see in great ape populations. Furthermore, this divergence happened over a remarkably short period of (evolutionary) time: at most 8 million years (the upper estimate of the period to the last common ancestor with our closest primate relatives, *Pan troglodytes* and *Pan paniscus*); and more likely 2 million years (since the genus *Homo* branched from *Australopithecine*). According to Tomasello, the key to explaining this divergence lies in a uniquely human ability to reliably transmit cultural information across generations. This ability, which underpins the accumulation of culture, produces a *ratchet effect*. The idea is that the high-fidelity transmission of culture (imitation) facilitates the evolution of complex culture within populations by allowing the propagation of successful innovations. When a successful technological innovation occurs, the dispersal of that new technological knowledge within a population via imitation both increases the number of possible individuals that can build upon that technology in the future and protects the innovation from being lost to the group over time. Repeated iterations of this process allows a steady "ratcheting up" of human technological culture. According to Tomasello, in populations without such high fidelity social transmission, successful innovations that did arise would be at best local ephemera, present for a short period but lost over time and not built upon in any significant way.

In Tomasello's view, high-fidelity transmission is made possible by a range of socially-orientated cognitive skills, including shared attention and theory of mind. It is these abilities which enable novices to imitate the motor sequences of conspecifics, which in turn allows for the reliable transfer of techniques for tool construction and use over generations. According to Tomasello then, social cognition is the key driver of cumulative technological culture.

Boyd & Richerson's cultural niche hypothesis has its origins in a different observation. They note that humans occupy the largest geographical and ecological range of any terrestrial vertebrate, which indicates that the species is remarkably adaptable. Furthermore, human expansion into this diverse range of habitats occurred in an incredibly short period of time: between 70 kya and 30 kya behaviourally modern humans had spread from the arid grasslands of Africa to the tundras of the Arctic Ocean. What, they ask, explains the extraordinary adaptability of our species? Clearly, cumulative technological culture plays an important role here, as our ability to adapt to a broad range of environments is dependent on our ability to develop and maintain a broad range of tools. But what explains our ability to produce those tools? One influential answer is that we are more intelligent than other species. The morphology of our brains and bodies allow for greater information processing power, and as a result we are able to produce more complex technologies (Barrett, Cosmides, and Tooby 2007; Pinker 2010).[59] This view has come to be known as the *cognitive* niche hypothesis. Boyd & Richerson, however, argue that intelligence itself is not sufficient to explain the adaptive success of our species. Instead, they suggest we inhabit a *cultural* niche; a socially mediated and informationally rich cultural environment that allows for the effective transmission of the information required to survive across a range of habitats. A key point here is that social learning offers an important alternative to asocial learning. Species that are restricted to asocial learning must rely on that mode even when it is costly and inaccurate, whereas species capable of social learning can afford to choose between two sources of information. Importantly, on Boyd & Richerson's view, the imitation that drives the transmission of cultural information is not cognitively demanding. Rather, simple learning heuristics, such as "copy successful individuals" are enough to produce cumulative technological culture.

[59] Osiurak & Reynaud's view is similar to these accounts in so far as they stress the importance of technical and causal reasoning. Importantly though, on the cognitive niche hypothesis these capacities are thought to be domain general, while on Osiurak & Reynaud's account they are domain specific.

For our purposes what is important here is that, according to both of these influential accounts, cumulative technological culture is the product of distinctly social cognitive mechanisms. Osiurak and Reynaud, however, offer an alternative hypothesis.

2.2 Osiurak & Reynaud's account of cumulative technological culture

The accounts of cumulative technological culture just described begin with the observation that humans are able to produce technologies that are too sophisticated for any one individual to produce. As humans are dependent on technology for survival, it follows that "We owe our success to our uniquely developed ability to learn from others." (Boyd, Richerson, and Henrich 2011, 10918) Osiurak & Reynaud begin with a slightly revised version of this observation:

> None of us is nearly smart enough to acquire *all* the information necessary to survive in any single habitat, but all of us are smart enough to acquire *each* piece of information – as well as to produce any kind of innovation – necessary to survive in any single habitat. (Osiurak and Reynaud 2020, 4-5).

This revised formulation is designed to realign our focus on individual, non-social cognitive capacities, thereby emphasising that the production of cumulative technological culture is an individual, as well as a collective, achievement. For Osiurak & Reynaud this change of perspective motivates the claim that humans possess a non-social cognitive structure—technical reasoning—that can acquire and generate the technical information required to produce cumulative technological culture.

In the simplest terms, technical reasoning is the ability to reason about the properties of physical objects. So on Osiurak & Reynaud's view, while imitation facilitates the transfer of this technical information, the underlying driver of cumulative technological culture is technical reasoning. And while there are both non-cognitive (non-social and social learning) and cognitive (motivation, aptitude, etc.) influences on an individual's ability to absorb technical information, it is the fact that humans possess a cognitive structure that can absorb this information that is central (Osiurak and Reynaud 2020, 5).

Importantly, Osiurak & Reynaud claim that technical reasoning is species-specific. They argue for this claim by emphasising the differences between tool-use in humans and other species. They then conclude that the best explanation for this data is that tool-use across humans and non-humans stems from different cognitive processes. We will return to this point in section 3.

Osiurak & Reynaud characterise technical reasoning according to five key features. First, it involves the ability to both predict the effects of an action on an object, and generalise from that prediction to other similar situations. This is to say it is both *causal* and *analogical*. For example, I might predict that striking one stone with another will cause the first stone to flake. I might then make the same prediction when using a stone to strike bone. Second, technical reasoning is based on a kind of non-declarative knowledge of physical and mechanical principles. This is an intuitive form of know-how gained through experience; one which most humans develop through their everyday interactions with the environment. For instance, most people realise that a stone is a useful tool to use when trying to flake another stone, whereas a leaf is not. Importantly, this is not a specialised or expert type of knowledge such as a craftsperson or artist might have. Third, technical reasoning involves being able to draw on knowledge of particular physical or mechanical principles and apply them to a specific situation. This requires being able to mentally simulate the effects of applying those physical/mechanical principles to the task at hand. To illustrate, we are able to conclude that a stone is useful for the task of flaking another stone because we can simulate the effects of applying the mechanical principle STRIKE and the physical principle HARDNESS to the task of flaking a stone. When we simulate the same effects using a leaf (HARDNESS, SOFTNESS and STRIKE) we do not get the same results. Fourth, technical reasoning applies to interactions between a tool and an object; it does not apply to the process of identifying and controlling the motor actions required to perform a task. The later process is performed by the motor-control system. The mental simulations produced by technical reasoning will influence the operation of the motor-control system, but there is no specific mapping between the use of a tool and the selection of a motor-action. Fifth, technical reasoning applies to any situation in which mechanical and/or physical principles apply. It is thus not constrained to the construction and use of familiar tools, but is employed to solve a broad range of everyday problems.

In Osiurak & Reynaud's view, it is this uniquely human capacity to reason about physical objects that best explains the phenomenon of cumulative technological culture. As they say, this view represents an "epistemological break with the state of the art" (Osiurak and Reynaud 2020, 14); the shift of focus from social cognition to technical cognition generates some interesting results.

To begin with, the technical reasoning hypothesis might explain why it is that we find only minimal cumulative technological culture in non-human animals. If the ability to store and develop the technical information is necessary for the production of anything more than rudimentary cumulative technological culture, and non-human animals lack that ability, then this is precisely what we would expect to see. In Osiurak & Reynaud's terms, humans possess "dormant" technical potential, which is only operationalised given certain environmental cues. Non-human animals lack this potential. We will return to this cognition-first aspect of the account in section 3.

One of the most striking features of Osiurak & Reynaud's view is that the two capacities traditionally thought to be the drivers of cumulative technological culture—imitation and innovation—are unified under one (cognitive) framework. Typically, we think of imitation as the product of social learning, and innovation as the product of asocial learning. However, the technical reasoning hypothesis suggests that both imitation and innovation have a common origin in the ability to reason about the properties of physical objects. As a result, according to Osiurak and Reynaud, the social aspects of cumulative technological culture are underwritten by a non-social cognitive capacity. In their view, technical reasoning plays an integrative role between asocial learning/social learning and imitation/innovation. It allows us to draw technical content from social situations (imitation) and then develop and improve this content in non-social situations (innovation). Furthermore, despite the presence of innovation in non-human animals, there are aspects of human tool-use that suggest they are the product of technical reasoning. One example is combinatory innovation, such as the ability to use one tool to create another tool. Osiurak & Reynaud suggest this ability is indicative of the kind of analogical thinking that characterises technical reasoning (Osiurak and Reynaud 2020, 17).

To sum up, the technical reasoning hypothesis represents a significant break with the accounts of cumulative technological culture outlined in section 2.2. It presents a unified cognitive framework in which to understand imitation and innovation, one which is based on non-social cognitive capacities. It is also species-specific and cognition-first. In the next section we look in detail at these last two aspects of the account.

3. Two problems for the account

In this section we outline why the species-specific and cognition-first aspects of Osiurak & Reynaud's account are problematic. With respect to the former, we critically engage with Osiurak & Reynaud's own arguments, and then survey the empirical evidence for technical reasoning in animals. With respect to the latter, we draw on evolutionary theory to argue that positing a leap in cognitive evolution is implausible. We then show that evidence of cumulative culture in non-human animals means that the species-specific constraint and cognition-first hypothesis are mutually exclusive.

3.1 Is technical reasoning unique to humans?

To answer this question, we begin by critically assessing Osiurak & Reynaud's argument that technical reasoning is species-specific, using the empirical data they cite (*3.1.1*). We then review further empirical data that suggests animals are capable of technical reasoning *(3.1.2)*.

3.1.1 Osiurak & Reynaud's case for species-specificity

As we have seen, Osiurak & Reynaud argue that technical reasoning is unique to *Homo sapiens*; it is species-specific. What is their argument for this claim? The general schema is to point to differences between tool construction and use across human and non-human animals, and then conclude that this indicates a difference in the type of cognitive structure at play. Osiurak & Reynaud appeal to a range of data points in supporting their case (Osiurak and Reynaud 2020, 6-7; 16). We believe there are problems with some of these claims, but we will address these in the next subsection. For now we will take these claims at face-value, and assess whether or not they license the conclusion Osiurak & Reynaud draw.

Osiruak and Reynaud support species-specificity using the following key claims:

1. Only humans are capable of producing tools that qualitatively transform our motor actions. For instance, a knife turns a repetitive back-and-forth arm movement into cutting, a pencil turns fine-grained finger movements into writing. Other tool-using species, by contrast, tend to use tools to amplify existing motor actions. For instance, a New Caledonian Crow may use a stick to extend the grasping range of its beak, or a Chimpanzee may use a stone to increase striking force when cracking a nut.
2. Only humans exploit natural forces (wind, water, fire, etc), animals do not.
3. Only humans actively test the functionally relevant properties of an object before using it as a tool. Tests with expert tool-using chimpanzees suggest that this behaviour is limited to humans (Povinelli and Frey 2016).
4. Humans are capable of transferring the skills learned in one context to another context. Thus using a stone to crack a nut can be transferred to using a stone to flake another stone. As we have seen, Osiurak & Reynaud attribute this ability to the analogical

aspect of technical reasoning, and hold that we see limited evidence of this in non-human animals.

5. Only humans can interpret the world in terms of unobservable causal properties and forces; a level of causal understanding that Osiurak & Reynaud see little evidence of in non-human animals.

On the basis of 1-5, Osiurak & Reynaud reach a tentative conclusion: “These findings *indicate* that tool use and tool making—as well as construction behaviour—*might* be based on distinct cognitive processes in non-humans and humans.” (Osiurak and Reynaud 2020, 7; our italics).

In our view, however, the inference from this evidence to Osiurak & Reynaud’s conclusion is problematic. Of particular concern is that the evidence is equally compatible with the conclusion that technological reasoning is present across both animals and humans, but at different stages of sophistication. More is needed in order to justify the species-specific hypothesis. Our challenge is nicely illustrated by a 2016 study in which Povinelli & Frey analysed the ability of chimpanzees to test prospective tools for task-specific functional properties (Povinelli and Frey 2016). Subjects were presented with two visually identical rakes. One of these was functional for the task of retrieving food, while the base of the other was spring-loaded, and thus non-functional. Over a series of 6 separate tasks Povinelli & Frey tested whether the chimpanzees would test the rake for the property of RIGIDITY, but found no evidence of such behaviour. One assessment of this evidence is that chimpanzees lack a cognitive ability—technical reasoning—that humans possess, and this ability is necessary for the production of functional-testing behaviour. Another is that both chimpanzees and humans possess the capacity for technical reasoning, but it is more highly developed in the human case. From this evidence, it is not clear that we have any reason to prefer one explanation over the other.

This worry can be translated into Osiurak & Reynaud’s terminology of structure and content. Their claim is that humans possess a unique cognitive structure, not found in other animals, that explains cumulative technological culture. Our claim is that there is a common structure across all tool-using species—technical reasoning—that is necessary for the production and use of tools; however, the *content* that this structure contains differs across species. In particular, in the human case, the content of our technical reasoning structure has been amplified greatly by those cognitive processes directed toward the social dimension of cumulative technological culture; namely teaching, innovation and imitation, metacognition and theory of mind (we will develop this idea further in section 4). On this view, the evidence provided by Povinelli & Frey's tests show that, while both chimpanzees and humans are capable of reasoning about physical objects, only humans are able to extract a certain kind of content from the situation: namely, that testing the rake for its rigidity will aid in the task of retrieving food.

Now, it might be thought that we are being unfair to Osiurak & Reynaud. As mentioned, their conclusion is deliberately tentative. Furthermore, their hypothesis that technical reasoning is uniquely human is a *working* hypothesis, and is clearly intended to facilitate exploring the connection between technical reasoning and cumulative technological culture. Nonetheless, working hypotheses can be criticised, especially if there is evidence that counts against them. In this subsection we have suggested that the evidence Osiurak & Reynaud provide is consistent with an alternative hypothesis: namely, that technical reasoning is a cognitive structure common to all tool-using species, and the variation we see across animals and humans is due to differences in the content of that cognitive structure. In the next subsection we will explore evidence in favour of this alternative hypothesis.

3.1.2 The empirical case for technical reasoning in animals

Osiurak and Reynaud make a number of quite strong empirical claims regarding the nature of non-human tool-use and cognition (1-5 above) in order to justify their inference to species-specificity. In this section of the paper we challenge the evidential support for the strength of these claims, focusing mainly on evidence from the New Caledonian Crow. Specifically, we show that their claims regarding the uniqueness of human technical competence are overstated. Whilst a wide ranging and more inclusive survey of the relevant literature would be valuable, our motivation for focusing on New Caledonian Crows here is threefold. First, there is insufficient space in this forum to do such a survey justice and, moreover, it is unnecessary, as evidence of corvid technical reasoning alone is sufficient to undermine the strength of Osiurak and Reynaud's claims (especially given their own reference to work in this species). Second, there is a significant amount of detailed work on New Caledonian Crow tool use and causal reasoning skills upon which some reasonably firm conclusions can be drawn. Third, their phylogenetic distance from humans illustrates the potential for at least rudimentary or simple forms of technical reasoning to be widespread in the animal kingdom, as opposed to the capacity being entirely specific to our own species or the primate lineages.

We deal with each of Osiurak and Reynaud's claims in turn.

1. *Only humans are capable of producing tools that qualitatively transform our motor actions.* This claim rests on how one decides whether a tool qualitatively transforms a motor action. The New Caledonian crow uses their stick and leaf tools for (amongst other things) hooking grubs out of their holes in tree branches. The tool they use is carefully produced so as to be hooked at the end and is used in a precise "fishing" manner rather than merely being poked into the grub or used as a straightforward probe as would be typical with the beak (Hunt 1996; Hunt and Gray 2003). It is thus plausible to describe this specific fine-grained manipulation of the hooked tool as a qualitatively transformed motor action rather than just an extension of natural probing behaviour. Indeed, even the apparently simple case of a Chimpanzee using a stone to crack a nut (Osiurak and Reynaud 2020, 6-7) is ambiguous. Chimpanzees do not use their hands as hammers, so it is not the case that this behaviour amplifies a typical motor action.[60] Of course the contrast here is not as stark as a human using a pencil or a knife, but then we should not expect it to be. The mere capacity to modify motor actions to perform novel tasks involving tools in some form or another is what is relevant.
2. *Only humans exploit natural forces – wind, water, fire, etc. Animals do not*. Compelling evidence against this claim comes from the use of fire by three species of so-called "firehawk raptors" in Australia. Both collectively and alone, Black Kites (*Milvus migrans*), Whistling Kites (*Haliastur sphenurus*) and Brown Falcons (*Falcon berigora*) spread fire by wielding burning sticks in their talons and beaks (Bonta et al. 2017). They pick up smoldering sticks from active fires and carry them to unburned areas nearby. The new fires they start flush prey species out from the vegetation, providing a ready food source. The cognition involved here is unstudied but the behaviour offers a clear example of a non-human species successfully exploiting a natural force. New Caledonian crows also demonstrate the capacity to exploit natural forces, specifically wind. In particular, experimental work suggests the birds are able to use pre-existing knowledge of how differently weighted objects move in the wind to make inferences about the weight of novel objects and use that knowledge to guide action (Jelbert et al. 2019). This behaviour is deliberate, flexible and cognitively demanding. Again, the species here is exploiting a natural force (wind) to their own benefit.
3. *Only humans actively test the functionally relevant properties of an object before using it as a tool.* Again, New Caledonian crows offer us a counterexample. The evidence

[60] Thanks to Kim Sterelny for alerting us to this point.

above of their use of the information regarding the weight of tools is suggestive, but much better evidence comes from their use of different objects in a series of experiments involving the Aesop's Fable paradigm (Alex H. Taylor et al. 2011; Jelbert et al. 2019; Logan et al. 2014). The Aesop's fable paradigm is a problem solving task involving a plastic tube that is partially filled with water. Floating in the water is a food reward. The subject must drop stones or other objects provided into the tube to raise the water level and reach the reward. In the case of New Caledonian crows, the use of stones as tools to reach the food is not spontaneous but, once learned, the birds are able to discriminate between stones with different functional properties such as large/small and heavy/light. They have been observed picking up novel stones and then discarding them *before dropping them in the tube*, on the basis of their now apparent functional properties. In short, they actively explore the functionally relevant properties of the stones before using them (Alex H. Taylor et al. 2011). Other investigations of the flexibility and spontaneity of New Caledonian Crow tool use and manufacture support this inference concerning their cognition regarding the functional properties of objects (Miller et al. 2020; Knaebe et al. 2017).

4. *Only Humans are capable of transferring the skills learned in one context to another context.* This kind of behaviour is based on analogical reasoning, and various experiments strongly suggest that New Caledonian crows can reason analogically (Alex H. Taylor et al. 2007). One of the more compelling of these, the "trap-table task", involves crows learning to avoid a trap in order to obtain food from a tube—"the trap-tube task"—and then being presented with a visually distinct (but causally analogous) problem—the "trap-table task". Birds able to solve the trap-tube task were able to immediately solve the analogous trap-table task suggesting that they were reasoning using causal analogy (A. Taylor et al. 2009).
5. *Only humans can interpret the world in terms of unobservable causal properties and forces.* The aforementioned case of New Caledonian crows using the effect of wind on objects to determine their functional properties could be considered evidence against this claim. More stark evidence comes in studies of New Caledonian crows' reasoning about hidden causal agents (A. H. Taylor, Miller, and Gray 2012). In these experiments subjects were presented with two sets of scenarios in which a stick moved. In the first of these, a human entered the arena prior to the stick moving and then left. In the second scenario, the stick moved without the human entering or leaving the arena. When a possible cause of the stick moving was not apparent the birds spent longer exploring

the arena than when a causal agent (the human) was observed. This suggests that crows can reason about hidden causal agents.

The New Caledonian Crow demonstrates, at very least, a level of technical competence and causal reasoning that is on a par with infant children. This challenges Osiurak and Reynaud's claims regarding the species-specificity of technical reasoning. Whilst it is clear that there is a difference in degree between human and non-human technical reasoning, this evidence challenges the claim that there is a difference in kind.

3.2 Is technical reasoning a necessary condition for the production of cumulative technological culture?

We turn now to the cognition-first hypothesis. Osiurak & Reynaud claim that technical reasoning is "the *necessary* cognitive structure that enables humans to constantly acquire and develop new techniques" (2020, p.5; our italics), and "the *difference-maker* from which CTC (cumulative technological culture) develops" (2020, p.7; our italics). They are thus committed to a particular causal story—one in which the causal arrow runs from technical reasoning to cumulative technological culture (See Figure 1).

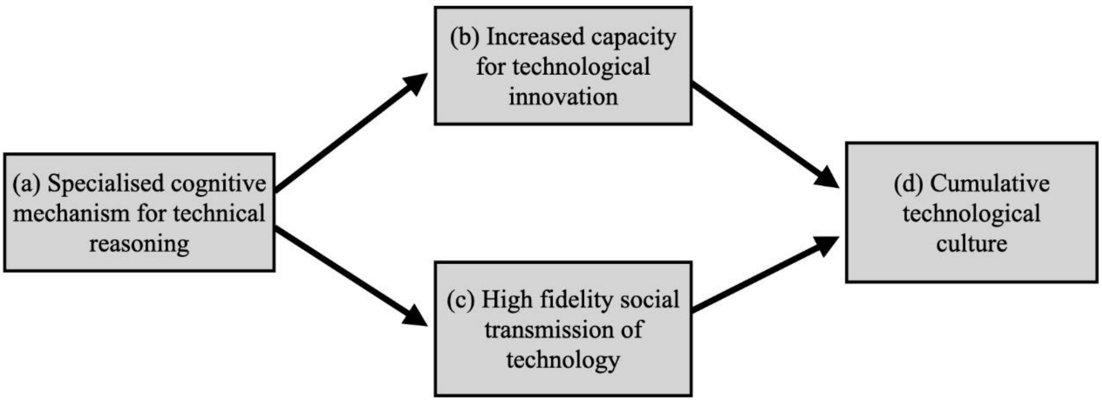

Figure 1. Osiurak & Reynaud's model of the evolution of cumulative technological culture. The evolution of a specialised cognitive mechanism for technical reasoning (a) drives the evolution of (d) cumulative technological culture by increasing the capacity for both (b) technological innovation and (c) the high fidelity social transmission of technology.

We claim that this aspect of their account is evolutionarily implausible. Furthermore, we argue that a more evolutionarily plausible causal story requires abandoning both the cognition-first hypothesis and species-specificity.

As we have seen, Osiurak & Reynaud argue that tool use across human and non-human animals is based on distinct cognitive mechanisms, and thus that technical reasoning is species-specific. This raises the following question: what were the circumstances in which technical reasoning emerged in *Homo sapiens*? What sort of cognitive apparatus existed before the origin of technical reasoning (see Fig. 1 (a))? And, what is the relationship between that pre-existing cognitive apparatus and technical reasoning? There look to be two options here: (i) technical reasoning developed *de novo* in *Homo sapiens* entirely independent of prior technical reasoning capacities; (ii) technical reasoning developed in *Homo sapiens* by piggy-backing on existing cognitive capacities—presumably those capacities that explain tool use in our great ape relatives (though this need not be the case). In what follows, we argue that (i) is evolutionarily implausible. This leaves (ii). We see this as the better option, but also think that a gradualist account undermines both species-specificity and the claim that technical reasoning is a necessary condition for the emergence of cumulative technological culture. Consequently, the cognition-first aspect should be abandoned. We begin with (i).

Basic evolutionary theory tells us that the *de-novo* generation of an entirely new domain-specific complex cognitive mechanism, such as technical reasoning, via random mutation alone is, at best, highly unlikely, at worst, implausible (Fisher and Bennett 1999). Very simply, this is because it would require adaptive mutations of large effect—the evolutionary equivalent of unicorns. For Osiurak and Reynaud to plausibly claim the novel origination of technical reasoning in humans they must give an account of the underlying mechanisms of innovation that made this possible. Once we start attempting to do this, we see that only a gradualist picture of some sort is plausible.

Technical reasoning must have evolved from the repurposing or retooling of some existing cognitive apparatus. Whilst it is possible for that apparatus to be independent of earlier forms of rudimentary causal reasoning, given (a) our evidence of non-human technical competence and (b) parsimony, it is more plausible to assume that human technical reasoning evolved from the elaboration of pre-existing cognitive mechanisms for technical competence (i.e. option (ii)). The question now lies in whether this evolutionary retooling occurred via in a shift in an existing structure which resulted in an entirely new mechanism (i.e. a difference of kind), as Osiurak and Reynaud claim, or an elaboration of modification to the same mechanism (i.e. a difference of degree), as we suspect is the case.[61]

A gradualist story is of course entirely consistent with both these possibilities. According to the framework that Osiurak & Reynaud propose, technical reasoning evolved in humans, and humans alone, from the elaboration of more rudimentary cognitive capacities found in ancestral species. By combining cognition-first and species-specific commitments in their account of technical reasoning, however, they commit themselves to an important empirical prediction: cumulative technological culture should be absent in non-human animals. If we *do* see evidence of this, then there are three options available:

1. *The species-specific constraint could be relaxed.* Technical reasoning can still be a necessary condition for the emergence of cumulative technological culture, but it cannot be unique to humans.
2. *The cognition-first condition could be abandoned.* Technical reasoning can be unique to humans, but it cannot be a necessary condition for the emergence of cumulative technological culture.
3. *Both the species-specific and cognition-first conditions could be abandoned.* This produces a coevolutionary story, in which technical reasoning and cumulative technological culture mutually develop.

To our minds, 3 is the best option given the evidence. In what follows, we defend this claim. We outline evidence for cumulative technological culture in a range of non-human animals and hominins, ordering our analysis in terms of phylogenetic distance. We begin with evidence from New Caledonian Crows, then look at Chimpanzees, and finally assess the hominin cultures of the lower Palaeolithic. Given this evidence it appears that, as per option 1 and 2

[61] Before moving on, we feel it is important to draw attention to some serious metaphysical and empirical questions sitting in the background here surrounding the individuation of traits and what counts as a novelty or innovation in the evolutionary context. These issues are particularly challenging when we look at something functionally defined like a cognitive mechanism (see Brown 2014). Short of solving these challenges (a project in itself), we assume here that a difference of kind of the type Osiurak and Reynaud advocate requires a novel mechanism to arise via some mechanism of innovation. There are many candidate ways this could happen, such as the duplication of cognitive architecture and then divergence (akin to serial homologues in anatomy), or by significant changes to an existing mechanism such as that it has a very different functional profile (such as we might see in the evolution of a flipper from a terrestrial limb). For our purposes, what is important is that there is a functional gap between what comes before and what comes after this innovative process.

above, the species-specificity and cognition-first conditions are mutually exclusive. Ultimately though, we argue that 3 is the most plausible story.

3.2.1 New Caledonian Crows

As we saw previously (Section 3.1.2) New Caledonian Crows manufacture and use complex hook tools in order to "fish" grubs from their holes in tree branches (Hunt 1996; Hunt and Gray 2003; Hunt 2014). One type of manufacturing process involves tearing strips from the barbed leaves of the *Pandanus* species of tree ("pandanus tools"). Three different types of pandanus tool have been identified: wide, narrow and stepped. These vary in the complexity of manufacture required. Reconstructing the chaîne opératoire suggests that wide tool construction is the most rudimentary, which involves tearing a strip from the pandus leaf. Producing narrow tools requires thinning a wide tool by tearing an additional strip from the tool. Stepped tools involve creating steps in the tool in order to narrow the working end (the "fishing" end) and widen the non-working end (the "grasping" end). This apparent improved functionality is thought to be a case of cumulative technological culture, rather than independent invention (Hunt and Gray 2003). Evidence of geographic clines in tool manufacture and technology across New Caledonia also supports this conclusion (Hunt and Gray 2003; 2007). More recent work suggests further diversity may exist within these tool types, such that there are different types of narrow tools constructed for different foraging goals (Hunt 2014).

3.2.2 Chimpanzees

Wild chimpanzees have been observed employing a variety of culturally inherited tool technologies including twigs for "termite fishing" and probing ant nests, leaves as sponges for accessing drinking water, and stones as hammers for extractive foraging (A. Whiten et al. 1999; Andrew Whiten 2011). As with New Caledonian Crows, these tool technologies vary in their complexity and by geographic region (Andrew Whiten and van Schaik 2007). Despite some earlier evidence to the contrary (Dean et al. 2012; Marshall-pescini and Whiten 2008), recent work examining the inheritance and cultural evolution of twig probing tools in chimps gives us reason to attribute to them cumulative technological culture akin to that seen in New Caledonian Crows.

First, the type of background cognitive capacities required to drive cumulative technological culture are employed in the transmission and maintenance of twig probe tools within chimpanzee populations. The twig probe tool form as well as the techniques for using and manufacturing them are inherited via high fidelity social learning (Andrew Whiten, Horner, and Waal 2005; Horner et al. 2006; Andrew Whiten et al. 2007; C. Sanz, Call, and Morgan 2009; C. M. Sanz, Schöning, and Morgan 2010), including via teaching (Musgrave et al. 2016). In addition, chimpanzees are sensitive to the efficiency of the tools and techniques being used by their conspecifics and this influences their social learning. Such sensitivity in social learning makes the cultural evolution of progressively more efficient tool technologies within populations much more likely. For example, captive chimpanzees, already proficient at using a straw to access juice in a container with a small hole via "dipping", upgraded their technique to a superior, more efficient "sucking" technique when it was demonstrated by a conspecific (Yamamoto, Humle, and Tanaka 2013).

In addition to this evidence of the requisites of cumulative culture, Vale et al. 2017 used a modification of the juice extraction task described above to demonstrate the capacity for groups of captive chimpanzees to accumulate tool modifications that build upon each other to increase efficiency (i.e. cumulative technological culture). Although this doesn't show cumulative technological culture in wild populations, this research suggests that such processes could be behind some of the more complex tool technologies seen outside the lab (such as the use and manufacture of brush fishing tools (C. Sanz, Call, and Morgan 2009)).

In their study, Vale et al. (2017) presented a naive group of chimpanzees with a container via which juice could only be accessed through a small hole, and multiple tools that could be used to extract the juice. As a reflection of their latent wild behaviour, there were absorbent materials that could be used to make sponges for sponging, and straws of various lengths that could be used for dipping or sucking. Some of the straws could be modified to increase efficiency; by straightening them (longer tools reach more juice and maintain their utility as the juice level in the container drops); or by removing a valve in one end (which makes it possible to employ the more efficient "sucking" rather than "dipping" technique). Constructing the most efficient possible tool available (a long, valveless straw) with the most efficient technique (sucking) required individuals to learn a number of pieces of information regarding the possible modifications of tools available, the efficiency impacts of the different modifications, and the different techniques that could be used with the tools. Moreover, these required innovations were cumulative in nature, the sucking innovation was improved upon by using the tool without the valve (over the shorter valveless tool). The sucking and valve removal innovations were improved upon by also straightening the tool and getting the longest possible tool available. As expected given Yamamoto, Humle, and Tanaka (2013), when seeded with individuals that had been taught modifications on the basic tools, groups would rapidly adopt more efficient techniques. Significantly, however, one group of entirely naive individuals learned the sequence of modifications to access the most efficient possible tool along with the most efficient extraction technique. Tests with control individuals suggest that it is highly unlikely that one individual could have produced the entire sequence of modifications alone. This means that we must invoke cultural factors (along with individual innovation) to explain how this group reached the most efficient solution to the extraction task. As with New Caledonian Crows, individual invention is insufficient to explain the complexity of the technology observed in chimpanzees; hence cumulative technological evolution is required.

3.2.3 The lower Palaeolithic

The lower Palaeolithic runs from roughly 3 million years ago (mya) to around 300 thousand years ago (kya). It is typified by two stone tool industries: the Oldowan (2.6mya - 1.7mya), associated with late *Australopithecus* and early *Homo* (particularly *Homo habilis*), and the Acheulean (1.7mya - 300kya), associated with *Homo erectus* and *Homo heidelbergensis*. It has typically been thought that these industries were relatively static. Coupled with their long lifespans (at least 700 thousand years for the Oldowan; well over 1 million years for the Acheulean), this has led researchers to conclude that they do not show signs of cumulative technological culture (Andrew Whiten, Horner, and Marshall-Pescini 2003). However, recent thinking regarding the Acheulean has challenged the notion that the industry was technologically static (Kuhn 2020; Wynn and Gowlett 2018). In addition, it has been suggested that this apparent stasis may be the product of focussing overly on the form of artefacts, rather than the action sequences used to produce them (Stout 2011). In this study, the action sequences required to produce Oldowan, early Acheulean and late Acheulean tools are analysed hierarchically. The results indicate that rates of technological change in the lower Palaeolithic are continuous with that found in the middle and upper Palaeolithic, suggesting that the Acheulean may indeed be an example of cumulative technological culture. Further work indicates that this conclusion may extend to the Oldowan (Stout et al. 2010; 2019; de la Torre et al. 2003). In particular, inter-assemblage analysis suggests that intersite technological variation is the result of relatively high-fidelity copying of tool construction behaviours (Stout et al. 2019). It seems then that even the earliest hominins were able to sustain some form of cumulative technological culture.

3.2.4 Summary

No one piece of evidence we have reviewed here is, on its own, conclusive. Taken together, however, they suggest an incremental upscaling of cumulative technological culture over time and across phylogenetic space. This is clearly inconsistent with the claim that technical reasoning is both a necessary condition for the production of cumulative technological culture and unique to humans. Consequently, a consistent account must at least abandon *either* species-specificity or the cognition-first hypothesis (options 1 or 2 above). Alternatively, *both* might be abandoned (option 3 above). In the remainder of the paper we focus on this latter option.

4. An alternative account

In this section we sketch an alternative account of cumulative technological culture; one that sees technical reasoning as central, but that jettisons the species-specific and cognition-first aspects of Osiurak & Reynaud's account. The resulting account is gradualist and co-evolutionary. We begin by looking at the methodological benefits of co-evolutionary hypotheses, and link our proposal to others in the literature. We then outline our view, and suggest some implications of the view for further research.

4.1 Why co-evolutionary hypotheses are important

In evolutionary biology, and particularly in evolutionary anthropology, we often come up against phenomena that appear to require saltationist explanations. One form these explanations take involves positing random genetic mutation as a mechanism to account for that phenomena. Take, for instance, the European Upper Palaeolithic. At around 40,000 kya in Europe we see a rapid increase in symbolism and art in the archaeological record. This is often thought to be a signal of *behavioural modernity*, which is associated with cognitive traits such as abstract reasoning, symbolic thought and language. What then explains this rapid change? One influential hypothesis suggests that a random mutation led to a neural reorganisation of the brain; one which supported the emergence of enhanced working memory. This facilitated the development of abstract reasoning and symbolic cognition, which in turn explains the sudden changes we see in the European Upper Palaeolithic (Coolidge and Wynn 2018: 260). Or take the evolution of language. It has recently been forcefully argued that gradualist explanations for the evolution of language fail, as syntax cannot be reduced beyond the "merge" operation (Berwick and Chomsky 2016). The capacity for language is thus understood as the product of a recent, novel and species-specific mutation. Saltationist explanations such as these inherit the kinds of problems we have outlined in sections 1 and 3.2.

Co-evolutionary and gradualist proposals, on the other hand, avoid these problems. Here the aim is to identify a pre-existing, more phylogenetically diverse trait and use this as a platform for understanding the development of a new trait. Take for instance gesture-tool-language co-evolutionary hypotheses (Sterelny 2012b; Stout et al. 2008; Stout and Chaminade 2012). It has been shown that there is significant neural overlap between the processing used to produce language and that used to produce the tools of the lower Palaeolithic (Stout et al. 2008). In particular, Broca's area, traditionally thought to be specifically related to language, in fact supports a range of hierarchically organised motor-tasks; including tool-making. This suggests that the behaviours supporting language production had the opportunity to co-opt existing computational architectures, pointing the way to a gradualist understanding of language evolution. It has also been suggested that the adaptive platform for language was likely great ape gesture, as opposed to great ape vocalisation (Sterelny 2012b). This is because great ape vocalisation is typically stimulus driven, reflex-like and automated, whereas great ape vocalisation is context-dependent and requires motor-control governed by top-down processing. In addition, the relationship between vocalisations and their referents are arbitrary, and so require inference to be understood. Gestures, on the other hand, can be structured and iconic. Selective pressure for the more finely grained motor-control required for tool-construction and for cooperation (due to changes in diet and ecology) creates an adaptive base for the development of language. Again, this points the way to a gradualist, co-evolutionary account of the evolution of language that avoids saltationist pitfalls.

Here we take a similar methodological approach. Our aim is to sketch an account of the development of technical reasoning in humans that sees the trait as both phylogenetically ancient and phylogenetically diverse. The question for us becomes: what were the conditions by which technical reasoning became so amplified in humans, such that we are able to sustain such highly sophisticated cumulative technological culture?

4.2 Our view

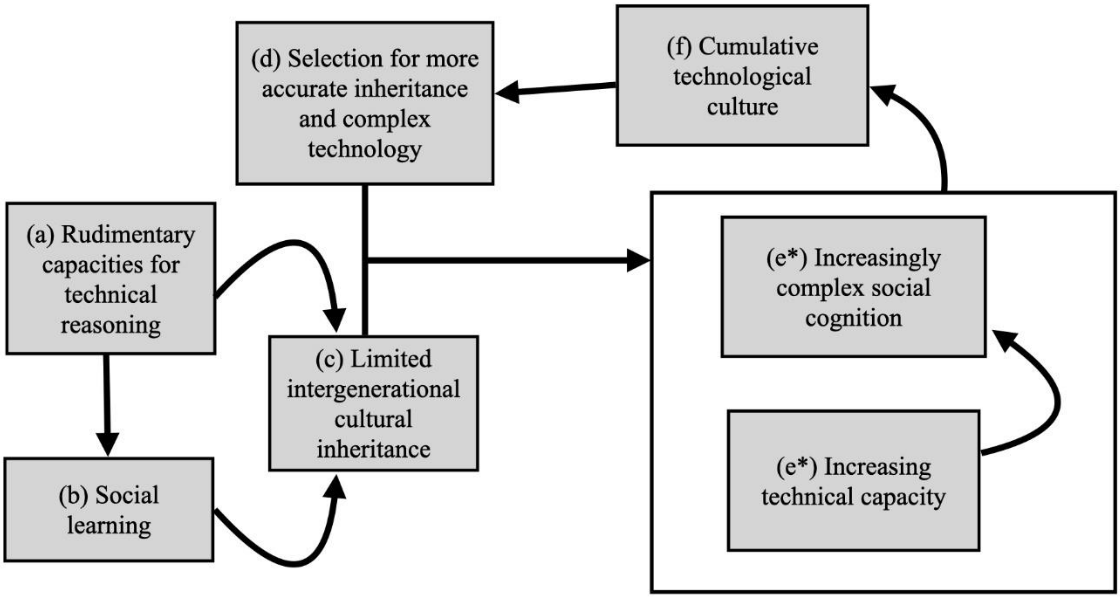

Figure 2. Our evolutionary model assumes a relatively limited capacity for cultural inheritance seen in non-human animals as a starting point (a)-(c). This is consistent with evidence of widespread social learning in animals and animal traditions (Andrew Whiten, Horner, and Marshall-Pescini 2003). We postulate that under the right selective regime (d), this limited capacity for cultural inheritance could be "ratcheted" up via increasingly complex social and technological cognition (e*) to the type of cumulative technological capacity we see in humans. Importantly, we take this ratcheting process to be iterative: the increases in social and technological competence build capacity for cumulative culture which, in turn, results in selection for more and more accurate inheritance of cultural information and complex technology. This feeds back into generating the evolution of ever more complex social and technological capacities, and so on. Whilst we make no strong assumptions about the nature of the social and technological cognition which evolves (e*), it is most plausibly the product of gradual accumulation of adaptations upon the simple base at (a) and (b) rather than specialist modules that arise entirely separately or specialist modules that arise via large step-wise changes to existing mechanisms (i.e. differences of degree rather than differences of kind).

Figure 2 illustrates and outlines our revised model of the relationship between technical reasoning and cumulative technological culture. We will finish by adding a little detail to the framework sketched there, providing an outline of the selective pressures we think were important in producing technical reasoning-cumulative technological culture co-evolutionary feedback loops.

In our view, all tool-using animals possess at least some rudimentary form of technical reasoning. Plausibly, the last common ancestor with the Chimpanzee was an animal with such capacities, which coupled with social learning allows for limited intergenerational cultural inheritance—(a)-(c) above. This phylogenetic story is supported by the aforementioned evidence of the dependence of Chimpanzee cultures on both social learning and technical reasoning (Section 3.2.2).

Turning more closely to the hominin lineage, the appearance of the Australopithecines at around 4 mya marks a morphological change toward increasing bipedalism and a related ecological move to grassland (as opposed to woodland) habitats (Mace 2000). The emergence and subsequent speciation of the Homo genus at some 2.5 mya signals increasingly high-quality diets, resulting in a reduction in tooth, jaw and gut mass, and an increase in brain size and life expectancy (Kaplan et al. 2000). From 2.3 mya, we also see an increasingly upward (though staggered) trend in the complexity of stone-tool technology. Finally, across this period there is a general (though again, staggered) trend towards increased group size and cooperation (Sterelny 2011; 2012a; 2016; Andrew Whiten and Erdal 2012). This all points toward the following two-pronged conclusion: (i) the evolutionary trajectory of the late Australopithecine and Homo lineages is one characterised by increasing reliance on high-quality, difficult to extract resources (*extractive foraging*); (ii) the ability to access these resources was made possible by increasingly sophisticated cooperation and increasingly complex tools. As such, the adaptive benefits of cooperation and tool-use produced increased selection effects for technical reasoning and social cognition. Thus we would expect to see increasingly robust iterative causal feedback between (d), (e*) and (f): upgrades in these cognitive capacities facilitated increasingly cumulative technological culture, which produced selection effects favouring more accurate cultural inheritance and increasingly complex technologies, which feeds back into the development of increasingly complex forms of social cognition and technical reasoning, and so on. In our view then, the highly sophisticated cumulative technological culture we find in humans is not the product of the emergence of species-specific capacity. Rather it is the result of a unique set of evolutionary circumstances—the adaptive benefits of extractive foraging—which favoured capacities aligned towards cooperation and tool production. These were the adaptive conditions that produced a mutual ratcheting-up of two pre-existing and phylogenetically diverse traits to un-before seen levels of sophistication.

This account is more in-line with the available empirical evidence regarding technical reasoning and cumulative technological culture in other species, yet also maintains a key role for technical reasoning. Our view attempts to strike a “Goldilocks zone” between the two traditional camps of social cognition and technical reasoning. By our lights, high-fidelity cultural transmission via social learning is a crucial aspect of any account of cumulative technological culture. However, our ability to reason about the properties and causal powers of objects also plays a key role. Understanding the evolution of material culture thus involves producing a more co-evolutionary account of social learning, technical cognition and cumulative technological culture.

5. Conclusion

Osiurak & Reynaud propose a compelling account of cumulative technological culture; one that challenges much contemporary theorising. However, the account contains two problematic aspects: species specificity and the cognition-first clause. The inference that technical reasoning is unique to humans is based on claims about substantive differences in tool use and manufacture across human and non-human animals. We argued that, on the evidence that Osiurak & Reynaud offer, we can equally infer that technical reasoning is present in a range of species at different levels of sophistication. We then surveyed a range of further evidence, which undermined those aspects of tool-use that Osiurak & Reynaud claimed were unique to humans.

Next we looked at the cognition-first clause. Here Osiurak & Reynaud face a theoretical choice: either posit the *de novo* appearance of a domain-specific cognitive capacity or adopt a gradualist story. We argued that the former was evolutionarily implausible. We then surveyed a range of evidence that suggests that cumulative technological culture is not unique to humans. Accommodating this evidence requires either relaxing species-specificity or accepting the falsity of the cognition-first hypothesis. Finally, we argued that the most plausible account jettisons both of these conditions, and sketched a brief outline of that account.

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Statement of Contribution

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I declare that the research presented in this Thesis represents original work that I carried out during my candidature at the Australian National University, except for contributions to multi-author papers incorporated in the Thesis where my contributions are specified in this Statement of Contribution.

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CHAPTER 5

Stone Tools, Predictive Processing and the Evolution of Language

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Abstract: Recent work by Stout and colleagues' indicates that the neural correlates of language and Early Stone Age toolmaking overlap significantly. The aim of this paper is to add computational detail to their findings. I use an error minimisation model to outline where the information processing overlap between toolmaking and language lies. I argue that the Early Stone Age signals the emergence of complex structured representations. I then highlight a feature of my account: it allows us to understand the early evolution of syntax in terms of an increase in the number and complexity of models in a cognitive system, rather than the development of new types of processing.

1. Introduction

There is renewed interest in developing an old hypothesis regarding the evolution of language.[62] This hypothesis begins with the observation that there are important similarities between the production of speech and the production of stone tools. In particular, both are hierarchically organised, goal-oriented tasks that require fine-grained motor-control regulated by top-down processing. Consequently—so the idea goes—selective pressure for one behavioural phenotype may have aided the development of the neural substrates required to produce the other, and vice versa. This observation has produced a range of co-evolutionary hypotheses regarding the emergence of language and toolmaking, and of communication and skilled behaviour more broadly (see for instance Reynolds 1976; 1993; Montagu 1976; Isaac 1976; Greenfield 1991; Kimura 1993; Planer 2017; Planer and Sterelny 2021).

[62] As Stout and Chaminade note (2012, p. 75), historical proponents include both Darwin (1871) and Engels (1963/1876).

Recent neuroimaging work by Stout and colleagues lends weight to tool-language co-evolutionary hypotheses (my focus here will be Stout et al. 2008; but see also Stout and Chaminade 2012; Stout et al. 2021). This work indicates that the areas of the brain used to produce language and those used to produce the tool industries of the Early Stone Age overlap significantly. My goal in this article is to add computational detail to Stout and colleagues' account by integrating their results with the predictive processing framework. According to proponents of predictive processing, the brain uses hierarchical generative models to produce predictions of its future sensory states, and updates those models based on any difference between predictions and actual sensory states. Over the long-term, this results in more accurate predictions. Cognitive systems are thus characterised in terms of their ability to *minimise prediction error* (see, e.g., Clark 2016; Hohwy 2013). Adding this computational detail to tool-language co-evolutionary accounts is important. As Stout and Chaminade note, any behaviour can be analysed using a hierarchically structured sequence of units; consequently, "a *special* evolutionary relationship between toolmaking and language predicts more particular overlap in information processing demands and/or neuroanatomical substrates between these two behaviours." (Stout and Chaminade 2012, p. 76). In other words, if co-evolutionary hypotheses are to be convincing, the similarities between toolmaking and language must extend beyond the mere fact that they can be modelled using hierarchies. Ideally, there would be similarities between: (i) the areas of the brain the two behaviours co-opt; and (ii) the information processing required to produce them. Stout and colleagues' work has focussed on (i); in this article I focus on (ii).

Predictive processing has features that make it well-suited to this task.[63] At a minimum, proponents of the theory claim to account for perception and action using prediction error minimisation. Predictive processing thus offers an account of the sensorimotor skills that unite toolmaking and language production using mechanisms that operate according to a common computational principle. In addition, the notion that cognition is hierarchical is at the core of the theory. This suggests the possibility of linking the structure of cognition with the hierarchical structure we find in action sequences and language.[64] However, as we shall see, this requires clarifying precisely what it means to describe a system as "hierarchical" across these three domains.

[63] Indeed, Stout and Chaminade themselves note that predictive processing might be used to account for evidence that the inferior parietal cortex is a supramodal processing region (Stout and Chaminade 2012, p. 78).
[64] Put together, these two features make predictive processing a tantalising fit with Stout's (2021) *perceptual motor hypothesis* (thanks to an anonymous reviewer for this point).

I have two main goals in this article. The first is to use a prediction error minimisation model to identify where the information processing overlap between toolmaking and language production might lie. The second is to situate the resulting account within the literature on language evolution, and highlight some of its features.

Achieving the first of these goals requires getting clear about what it means to say that there is an information processing overlap between toolmaking and language production. I previously noted that both are hierarchically organised, goal-oriented tasks that require fine-grained motor control regulated by top-down processing. As such, any account of the cognitive capacities underwriting the two behaviours must explain these features. This challenge can be made more precise in the following way. It is typically thought that both complex intentional action and language display *nested part-whole structure*. Consider the action of brushing your teeth. When you reach out and apply pressure to the tap, these actions are nested within—or are a *constituent of*—the broader goal of wetting the end of your toothbrush. Likewise, both applying pressure to the tap and wetting the end of your toothbrush are nested within the broader goal of brushing your teeth.[65] This observation suggests that we can *represent* nested part-whole structure. More specifically, the thought is that the representations controlling such action must themselves be structured such that lower-level motor commands are nested within—or are a constituent of—higher-level goal representations. It follows that complex intentional action requires representations with nested part-whole structure; and, as we shall see, producing the tools of the Early Stone Age required complex intentional action.

Sentences also display nested part-whole structure. For instance, consider the sentence "The woman brushed her teeth vigorously". "The" is a constituent of "The woman", insofar as the article is nested within the broader noun phrase. Likewise, the noun phrase "The woman" is nested within the broader sentence. And again, explaining this feature of language plausibly requires positing representations with nested part-whole structure.[66] The upshot is this: if we treat the production of language as a complex intentional action, then we can understand the information processing overlap between toolmaking and language as an attempt to explain the nested part-whole structure of the representations required for complex intentional action.[67] Of

[65] See Pulvermüller (2014) for a hierarchical action schema based on the tooth-brushing example; see Moro (2014) for further discussion on the apparent similarities between syntax and action (thanks to an anonymous reviewer for pointing out these references).
[66] For instance, those outlined in the generative grammars produced by linguists in the Chomskyan tradition.
[67] Importantly, there are some who dispute the claim that language is composed of nested part-whole relations—for instance Frank, Bod, and Christiansen (2012) or Christiansen and Chater (2016). I will look in more detail at these issues in section 4.

course, there are significant differences between action and syntax. The claim is *not* that the toolmakers of the Early Stone Age had full-blown syntactical capacities. Rather, the claim is that one feature of the capacities underwriting the nested part-whole structure found in toolmaking—namely, structured representations—might plausibly have been co-opted and evolved to produce the nested part-whole structure found in syntax.

The above considerations highlight an important terminological point. In the predictive processing literature, the layered sequence of models thought to be embodied in the brain are described as 'hierarchical'. In the linguistics literature, the ability to combine meaningful units into strings of meaningful units (or, the ability to combine words into sentences) is described as 'hierarchical'. These are very different. In many cases, context will be sufficient to determine which meaning is in use. However, in some cases the meaning will need to be specified. I will hence distinguish *stratified hierarchies* from *tree-like hierarchies*. 'Stratified hierarchies' corresponds to the way 'hierarchies' is used in the predictive processing context; that is, to refer to a series of levels within a cognitive system. 'Tree-like hierarchies' refers to the branching structures used by generative linguists to model syntactic properties and cognitive scientists to model action (e.g. Stout 2011; Fitch 2014). Finally, I will use 'structured representations' to refer to representations with nested part-whole structure, which plausibly underwrite our toolmaking and syntactical abilities. I am particularly concerned with the way lower-level sensorimotor representations are influenced, or modulated, by higher-level goal representations. The former are nested within, and hence constituents of, the later. For instance, the representation that controls the action of applying pressure to the tap and wetting my toothbrush is a constituent of the goal representation of brushing my teeth, because the latter modulates the former. As a result, these are 'structured' representations. Motivating this claim, using the resources of predictive processing, will be my task in section 3.

In addressing my second goal, I outline some features of the error minimisation account that distinguish it from others in the literature. A key dispute concerns whether language evolved suddenly (*saltationism*) or whether it evolved slowly via incremental stages (*gradualism*). An important issue in this dispute concerns the evolution of structured representations. On some accounts, structured representations are treated as a necessary condition for language production. Consequently, explaining the evolution of language requires positing an evolutionary transition from cognitive systems without structured representations to cognitive systems with structured representations. This transition might be sudden (e.g. Berwick and Chomsky 2016) or gradual (e.g. Planer and Sterelny 2021).

Typically, this is achieved by positing an associated transition from sequential information processing to tree-like hierarchical information processing. Others emphasise evolutionary continuity, and want to avoid a commitment to transitions; either between non-structured and structured representations or between sequential and tree-like hierarchical processing. They thus deny that structured representations are required for language production (e.g. Frank, Bod, and Christiansen 2012). The error minimisation account occupies a unique place within this dialectic, because the transition between non-structured representations and structured representations can occur *without* positing a transition between sequential and tree-like hierarchical processing. The reason for this is that the former transition is made possible solely by increasing the number and sophistication of layered models in a cognitive system, which nonetheless remains governed by the principle of error minimisation. I argue that this feature makes the account an attractive choice for those attempting to produce gradualist explanations of language evolution.

In sum, I aim to motivate two claims. First: the error minimisation approach demonstrates that producing the tools of the Early Stone Age required sophisticated structured representations. Second: the error minimisation approach can account for the evolution of structured representations without needing to posit a corresponding transition in types of processing.

I proceed as follows. Section 2 outlines the results of Stout and colleagues' neuroimaging work on Early Stone Age toolmaking. Section 3 provides an overview of predictive processing and outlines a minimal model for understanding the information processing overlap between toolmaking and language. Section 4 situates this account in the literature and highlights its advantages. Section 5 concludes.

2. Stout and colleagues on the neuroanatomy of toolmaking

Despite the long history of tool-language co-evolutionary hypotheses, attempts to confirm neural overlap between toolmaking and language production are a relatively recent addition to the literature (e.g. Higuchi et al. 2009; Putt et al. 2017; Putt 2019; Stout and Chaminade 2007; Stout et al. 2008; Stout 2011; Stout and Chaminade 2012; Stout et al. 2021). In their 2008 study, Stout et al. took three right-handed, expert Early Stone Age toolmakers and used fluorodeoxyglucose positron emission tomography to identify the areas of the brain recruited by three separate tasks: (i) a control task, which involved striking cobbles together without attempting to produce tools; (ii) Oldowan toolmaking; and (iii) Late Acheulean toolmaking (Stout et al. 2008).[68] In this section I provide a summary of the results and implications of this work, as well as a more general overview of Early Stone Age toolmaking and its cognitive demands.

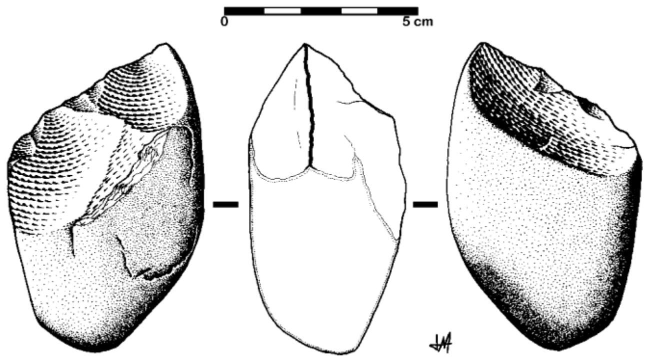
2.1 Oldowan and Late Acheulean toolmaking

The Early Stone Age runs from roughly 3.3 million years ago (mya) to around 300 thousand years ago (kya). It is typified by three stone tool industries: the *Lomekwian*, the *Oldowan* and the *Acheulean*. The Lomekwian is the earliest known industry, is associated with *Australopithecus*, and predates the first *Homo* fossils by around 500,000 years. The Oldowan spans 2.6 mya to 1.7 mya, and is associated with late *Australopithecus* and early *Homo* variants (particularly *Homo habilis*). The Acheulean spans 1.7 mya to 300 kya, and is associated with *Homo erectus* and *Homo heidelbergensis*. Toolmaking processes across the three industries become more complex and refined over time. In particular, a distinction between Early and Late Acheulean production is often made. Late Acheulean tools are thought to emerge between 600-500 kya, and are smaller, thinner and more symmetrical than tools of the Early Acheulean (Stout 2018, 263; Stout et al. 2014, 577). The Early Stone Age also sees a significant increase in hominin brain size, from $450cm^3$ to $1200cm^3$ (Klein 2009). There is thus a general trajectory of increasingly sophisticated toolmaking and expanding brain size across this period. Stout and colleagues' work focuses on the Oldowan and Late Acheulean industries, and I will do likewise.

Oldowan toolmaking involves using a *hammer-stone* to strike a *core*. This process produces *flakes*, which typically have a very sharp cutting-edge. It also shapes the core into a *chopper* (see Figure 1). Typically, reduction of the core via flaking is not comprehensive, and so large parts of the original surface of the core are preserved. At a minimum, Oldowan toolmaking requires refined visuomotor coordination and the ability to evaluate the physical properties of stone. The latter might be thought of in terms of *causal*, or perhaps *technical* reasoning (see Spellman and Mandel 2006; Khemlani, Barbey, and Johnson-Laird 2014 for accounts of causal reasoning; see De Oliveira, Reynaud, and Osiurak 2019; Osiurak and

[68] There are complicated questions that arise when the neuroscience of modern humans is used to support inferences about the cognitive capacities of ancient hominins. Addressing such questions is beyond the scope of this paper; but, at the very least, Stout and colleagues' results offer some general proof of principle for co-evolutionary theories. If, for instance, no neural overlap was found, this would not be the case.

Reynaud 2020 for accounts of technical reasoning; see Moore 2011 for an account of the specific causal relations Early Stone Age knappers may have understood). More refined Oldowan toolmaking plausibly utilises strategic planning. For instance, repeated striking leaves "scars" on the core, which produce better conditions (a *striking platform*) for future strikes. Flake removal can thus be performed in a manner that aids future flake removal (see Pargeter et al. 2020; Stout et al. 2019 for more on the technical demands Oldowan knapping).

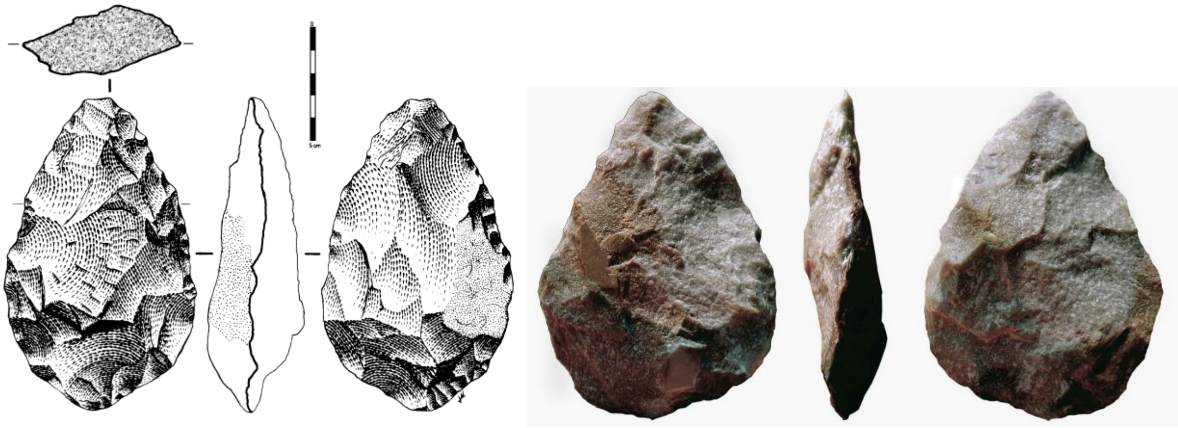


(a)

(b)

Figure 1: Oldowan tools. Oldowan toolmaking involves striking a *core* with a *hammer-stone* in order remove *flakes*. Both the resulting flakes and modified core (a *chopper*—(a) and (b) above) can then be used. Core reduction is partial, and so large parts of the original surface are preserved. Source: José-Manuel Benito Álvarez/Wikimedia Commons. Image (a) is reproducible under the terms of the Creative Commons Attribution-Share Alike 2.5 Generic license; image (b) has been released into the public domain.
URL (a) https://en.wikipedia.org/wiki/Stone_tool#/media/File:Chopping_tool.gif
URL (b) https://en.wikipedia.org/wiki/Oldowan#/media/File:Canto_tallado_2-Guelmim-Es_Semara.jpg

Late Acheulean toolmaking requires a significant increase in strategic planning capacities. The archetypal artefact of this industry is the *hand-axe*; a pear-shaped tool characterised by its bifacial, symmetrical structure (see Figure 2). Creating this tool requires using a series overlapping and increasingly refined procedures to impose a pre-determined form onto a core. Reduction via flaking is more comprehensive as compared with the Oldowan, and typically none of the original surfaces of the core will be preserved. Stout et al. (2006) divide Late Acheulean toolmaking into three phases: *roughing-out*; *primary thinning and shaping*; and *secondary thinning and shaping* (see also Figure 4). During the roughing-out phase, a hammer-stone is used to impose the general form of the hand-axe onto the core. This involves creating a sharp edge around the perimeter of the core, centred between the two faces, by removing large, long flakes. The primary thinning and shaping phase is characterised by the removal of long, thin flakes which thins the biface and starts to impose the desired symmetry on the tool. This requires the creation of striking platforms through intensive light flaking along the edge, and also the use of a *soft-hammer*; that is, a hammer made of antler, bone or wood. In the secondary thinning and shaping stage, a soft-hammer, along with abrasion and grinding, is used more intensively to create a sharp, regular edge around the perimeter of the tool. In addition to those required for Oldowan toolmaking, the cognitive demands of producing Late Acheulean tools include: (i) more fine-grained motor control; (ii) better understanding of the physical properties of stone, particularly with respect to the effects of different striking techniques; (iii) greater strategic planning, particularly with respect to understanding the link between individual actions (striking, grinding, etc.), short-term goals (creating a striking platform) and long-term goals (imposing symmetrical form on to the core).



(a) (b)

Figure 2: Acheulean tools. Acheulean hand-axes are characterised by their bifacial structure and symmetry. Producing them requires performing a sequence of overlapping procedures in order to impose a predetermined form onto a core. Core reduction is usually comprehensive, and typically none of the original surfaces of the core will be preserved. Source: José-Manuel Benito Álvarez/Wikimedia Commons. Image (a) has been released into the public domain; image (b) is reproducible under the terms of the Creative Commons Attribution-Share Alike 2.5 Generic license.

URL (a) https://en.wikipedia.org/wiki/Stone_tool#/media/File:Hand_axe_spanish.gif

URL (b) https://en.wikipedia.org/wiki/Acheulean#/media/File:Bifaz_cordiforme.jpg

2.2 The neural correlates of Early Stone Age toolmaking and language

We would predict Late Acheulean production to have different neural correlates as compared to Oldowan production (and the control task). Stout and colleagues found significant overlap in early visual processing in the posterior occipital cortices across Oldowan and Late Acheulean tasks (Stout et al. 2008; see also Stout et al. 2021 for further corroboration of these results). This is what would be expected given the base-level complexity of core and hammerstone manipulation required for each task. In addition, both toolmaking procedures placed increased demands on an inferior parietal-ventral premotor circuit that is anatomically and computationally similar to circuits involved in phonological processing (Stout and Chaminade 2012, p. 81).

However, Late Acheulean toolmaking produced a marked increase in right hemisphere activity, particularly in supramarginal, ventral precentral and inferior prefrontal gyri. This is likely a product of the increasing length and complexity of the motor actions involved, and of the more technical role played by the left hand in manipulating the core. In addition, hand-axe production saw an increase in activity in the right inferior and ventrolateral prefrontal cortex. The former is thought to be at the apex of sensory and motor processing, and hence play a central role in producing goal-directed and flexible behaviour. The latter is thought to play a role in matching perceptual cues with action sequences (Stout et al. 2008, p. 1946). More broadly, the activation of inferior frontal gyrus in Late Acheulean toolmaking is of interest. The region forms part of Broca's area, and so is associated with language production and, increasingly, the general computation of supramodal and tree-like hierarchical information (e.g. Koechlin and Jubault 2006; Fadiga, Craighero, and D'Ausilio 2009; Poldrack 2006; Fedorenko, Duncan, and Kanwisher 2012).[69] Also of interest is the activation of the right and left dorsal portions of inferior frontal gyrus *pars triangularis*. The former is thought to play a role in syntactic/semantic integration, and in processing high-level motor representation and complex action-plans. The latter is associated with working memory and sentence processing (Makuuchi et al. 2009; Elmer 2016; Matchin 2018).

2.3 Summary

[69] An important issue here is lateralisation, as language processing appears to be more left lateralised than manual praxis tasks. Stout (2018, p. 264) provides some discussion of why this might be the case, including that it represents increasing developmental accommodation to linguistic and technological environments; a theme I will return to (thanks to an anonymous reviewer for raising this issue).

Stout and colleagues' produce two findings important for my goals. First, the increasing complexity of toolmaking from Oldowan to Late Acheulean triggers activity in areas commonly associated with language production. Second, activity in inferior frontal gyrus in both Late Acheulean toolmaking and language production suggests that it plays a central role in processing information associated with behaviour displaying nested part-whole structure (I will go into further detail on this in sections 3 and 4). This corroborates claims that Broca's area more broadly is important in the production of complex intentional action (e.g. Koechlin and Jubault 2006; Fedorenko, Duncan, and Kanwisher 2012). These results lend weight to tool-language co-evolutionary hypotheses, insofar as the emergence of a sophisticated new goal-oriented behavioural phenotype—language—could be supported by a pre-existing domain-general mechanism that had evolved to support a pre-existing complex, goal-oriented behavioural phenotype—toolmaking.

3. An error minimisation model for Late Acheulean toolmaking

With the details of Stout and colleagues' work in mind, we can now move to the task of integrating their results with the framework of predictive processing. I do this in two moves. First, I outline the basic computational details of predictive processing. Second, I propose a minimal model for understanding the cognitive demands of Late Acheulean toolmaking based on those details.

My general strategy here was laid out in the introduction: both complex intentional action and language production are thought to require goal representations with nested part-whole structure. As a result, any information processing overlap between toolmaking and language production may be thought of in terms of the computational mechanisms required to produce structured goal representations. The idea, then, is that we can explain the early evolution of syntax by appeal to more domain general capacities—namely, those required for the broader class of complex intentional action.[70]

Predictive processing offers an account of perception and action using a single computational principle. Moreover, as we shall see, the stratified hierarchical nature of the theory provides a way of understanding how lower-level sensorimotor representations are

[70] In adopting this general line I borrow from Planer and Sterelny (2021). I will say more about the differences between our respective approaches in section 4.

modulated by higher-level goal representations. For these reasons, it provides an attractive framework for understanding the cognition underlying complex intentional action.

3.2 The predictive processing framework

There are plenty of excellent overviews of predictive processing (e.g. Clark 2013; 2016; Hohwy 2013; Wiese and Metzinger 2017). Here I focus on two aspects of the theory that are important for the explanation of complex, goal-directed action trajectories: (1) hierarchical prediction and prediction error; and (2) prediction error minimisation. But before I begin, an important caveat. Predictive processing, in some of its formulations, has been claimed to offer the cognitive sciences a unifying principle governing brain function (see Hohwy 2013 and Friston 2010 for strong versions of this claim; Clark 2013, §5.2 is more cautious). None of my conclusions here are predicated on such claims. All I borrow from the framework is the notion that perception and action are governed by prediction error minimisation in generative hierarchical models.

3.2.1 Stratified hierarchical prediction and prediction error

Predictive processing understands top-down processing as the transfer of *predictions*, and bottom-up processing as the transfer of *prediction errors*.

Predictions are a cognitive systems' attempts to predict future sensory input. They are generated according to encoded models of the world, which in turn are the product of evolution, experience and learning. These models allow the brain to generate a hypothesis about the source of sensory input, which can then be used to produce predictions about future sensory input. The models are hierarchically organised according to the spatiotemporal scale of the causal regularities they address. At lower levels of the hierarchy there are models generating predictions at finer-grained spatiotemporal scales; for instance, regarding the sensory input associated with adjusting the force of a hammer-stone strike in response to scars on a core. At higher levels of the hierarchy there are models generating predictions at broader spatiotemporal scales; for instance, regarding the sensory input associated with assessing the properties of a potential core. In this way, cognitive systems work to produce accurate predictions of future sensory input over a hierarchy of spatiotemporal grain.

Bottom-up processing also plays an important role in the predictive processing framework. But rather than being the construction of perceptual experience from the raw data of sensory input, bottom-up processing is instead the transfer of any difference between top-down predictions of sensory input and actual sensory input. In other words, bottom-up processing involves the transfer of *prediction error*. At any given layer in the hierarchy, a model will receive prediction error signals from the model below it, attempt to explain away this error, and forward any error that it cannot explain to the model above it. Linked probabilistic models of this sort are called *generative models* due to their ability to recreate incoming sensory states via top-down processing (Hinton 2007).

In sum, predictive processing envisages global interaction between top-down and bottom-up processing. Top-down processing involves the flow of increasingly spatiotemporally constrained predictions from more abstract to less abstract representational layers; bottom-up processing involves the transfer of prediction error signals—mismatch between the predicted and actual signal—from less abstract to more abstract representational layers.

3.2.2 Perception, action, and prediction error minimisation

According to predictive processing, cognitive systems attempt to minimise the difference between predicted sensory input and actual sensory input. In other words, they attempt to *minimise prediction error*. The brain has two strategies for eliminating any prediction error rising through the hierarchy: it can either change the parameters of its models to account for the error, or else it can change the sensory input causing the error such that it aligns with the current parameters of its models. In simpler terms, the system either adjusts the model to fit the data or adjusts the data to fit the model. Perception is the product of adopting the model-to-data direction of fit; action is the product of adopting the data-to-model direction of fit. This yields a unified computational account of perception and action: although the direction of fit changes, both phenomena are strategies for minimising prediction error.

In the case of perception, models generate a new hypothesis regarding the source of sensory input. This produces new predictions, which, if successful, will have the effect of dampening any prediction error rising through the hierarchy. For example, suppose I catch a glimpse of a figure at the end of my room from the corner of my eye. On closer inspection, however, said figure is revealed to be a pile of clothes on a chair. The best explanation of the initial (quite patchy) sensory input is that there is a person in my room. Yet this explanation does not account for the data produced when I focus my attention on the figure. As a result, prediction error rises through the system. By producing a new explanation of the source of the sensory input—the untidy chair hypothesis—the brain can eliminate this prediction error.

In the case of action, the brain holds its hypotheses about the world fixed. Any prediction error rising through the system is then addressed by moving around in the world such that the incoming sensory data is changed to fit the hypothesis. To continue the previous example, the hypothesis that there is a person in my room generates the prediction that by turning my head I will be able to get a better look at them. That prediction will be inconsistent with the current, patchy sensory input emanating from the corner of my eye. This mismatch between predicted sensory input and actual sensory input generates prediction error, but by moving my head in order to look to the corner of the room, I can minimise this error.

Here the brain actively tests a hypothesis against the world. As Clark notes, this account of action is, in a certain sense, *subjunctive* (Clark 2016, p. 121). Motor control involves predicting future proprioceptive and exteroceptive input were a certain action to be carried out, and reducing the gap between those predictions and sensory input by performing the modelled action. The mechanism of prediction error minimisation works because prediction errors carry information about the difference between the way the body is currently situated and the way the body would be situated were an action to be carried out. Implementing that action sequence thus eliminates prediction error. Importantly, the predicted proprioceptive and exteroceptive input of an action is generated across all levels of the hierarchy. At lower levels, models will be predicting sensory input over the short-term; for instance, the proprioceptive change associated with the action of turning my head. At higher levels in the hierarchy, models will be predicting sensory input over the long-term; for instance, the exteroceptive input associated with identity of the person.[71]

Moreover, each level of the hierarchy recapitulates this general procedure in the following way. Just as the system as a whole uses its active states to influence the world in ways that alter its sensory states, each model in the hierarchy uses prediction—here equivalent to active states—to influence the layer below it in ways that will alter incoming prediction error, and thus influence the model's sensory states (e.g. Kirchhoff et al. 2018, p. 3-4). This top-down effect on lower levels is also described in terms of "modulation" or "guidance". In Clark's words, higher level predictions "[…] guide and nuance lower level response. This guidance involves expectations concerning the most likely patterns of unfolding activity at the level below it" (Clark 2016, p. 146). As such, predictive processing architectures display a kind

[71] This general picture of multi-level processing according to spatio-temporal grain integrates well with the notion of hierarchical process memory (Hasson, Chen, and Honey 2015; Hasson 2017). Thanks to an anonymous reviewer for this pointer.

of transitivity. If level A predicts and guides the activity of level B, and level B predicts and guides the activity of level C, then level A has, to some extent, predicted and guided the activity of level C (see Drayson 2017 for more nuanced discussion). Consequently, higher-level goal representations are able to predict, and hence guide, the predictive outputs of the layers below them. And reducing error against this cascade of predictions will bring about the original goal. As I will emphasise in the following sub-section, such architectures produce representations with nested part-whole structure.

3.3 An error minimisation model for Late Acheulean toolmaking

Stout and colleagues' data suggests that the neural overlap between toolmaking and language (specifically in inferior prefrontal gyrus and Broca's area more broadly) may indicate that the region underwrites the production of complex intentional action. My task now is to outline how these behaviours might be implemented using a prediction error minimisation model.

We have seen that Early Stone Age toolmaking—and, in particular, Late Acheulean toolmaking—is a complex goal-oriented task requiring significant strategic planning and technical skill. The nested part-whole structure of these tasks have been outlined in some detail by Stout, using tree-like hierarchies (see Figure 3). This work is important for my purposes, as it outlines the complexity of the action plans required to produce a tool, rather than the complexity of the tool itself. Indeed, Stout (2011) argues that focusing on the latter has meant we have underestimated the level of cultural variation across the Early Stone Age (see also Stout et al. 2021). Transposing Stout's action plans into the error minimisation framework gives us a potential reading of the cognition of those toolmakers. The results indicate that the Early Stone Age signals the presence of representations with sophisticated nested part-whole structure. I take my proposal to be broadly consistent with a range of more detailed work applying generative hierarchical models of cognition to complex intentional action in general, and to language in particular (e.g. Farmer, Brown, and Tanenhaus 2013; Koechlin and Jubault 2006; Lupyan and Clark 2015; M. J. Pickering and Garrod 2013; M. Pickering and Gambi 2018; M. J. Pickering and Clark 2014); though none of these accounts deal specifically with the demands of stone toolmaking.

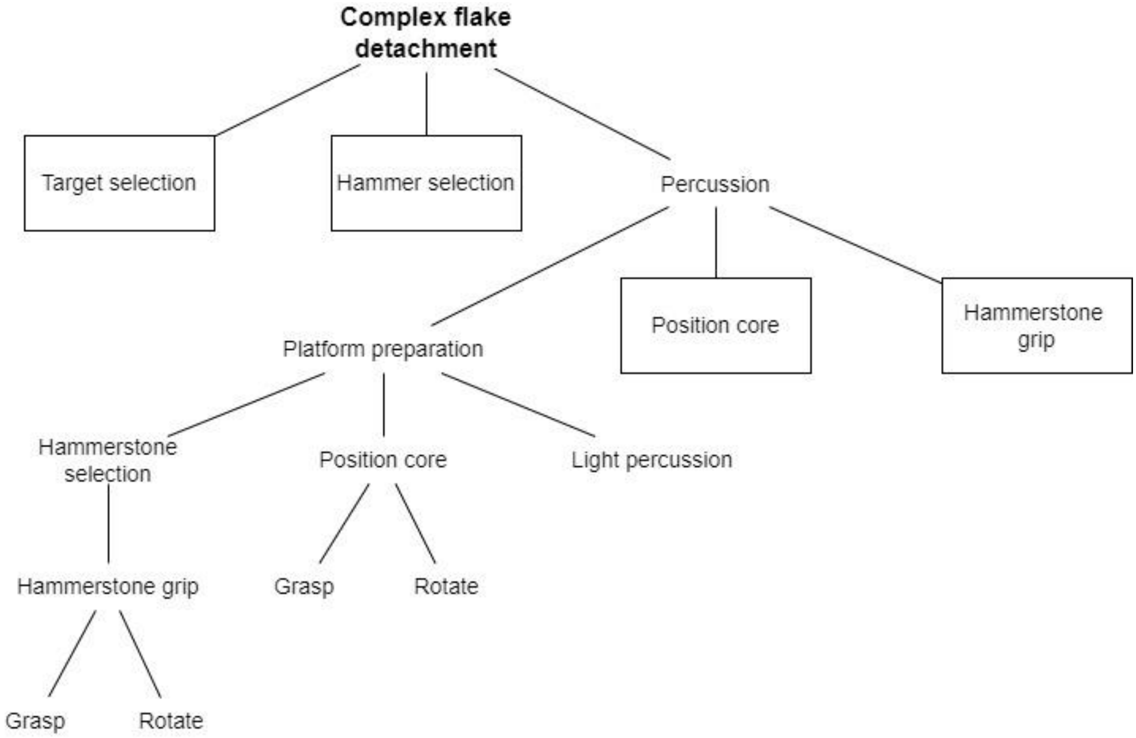


Figure 3: An action plan depicting complex flake detachment in Late Acheulean toolmaking. Lines connect subordinate goals/actions with broader goals/actions. Boxes enclose action chunks that have been ‘collapsed’, and whose subordinate elements have been omitted to avoid crowding (adapted from Stout 2011, p. 1052).

The basic mechanics of the proposal are as follows. Consider the action trajectory required for complex flake detachment, outlined in Figure 3. We can transpose this into the error minimisation framework as follows. First, a goal representation, <complex flake detachment>, becomes salient in the upper levels of the model hierarchy. This produces a prediction of the sensory input expected were this goal to be brought about, which modulates the structure of goal representations in the models below it; to <target selection>, <hammer selection> and <percussion>. These predictions select further specific goal representations further down the hierarchy. For example, from <percussion> to <platform preparation>, and so on. Lower-level goal representations, such as <hammerstone selection>, <position core>, and <light percussion>, are nested within—or constituents of—<platform preparation>. And all of these are constituents of <percussion>, which itself is a constituent of <complex flake detachment>.

Each new goal representation within a model produces predictions, which in turn modulate the goal representations of models below it, right down to predictions of the proprioceptive input expected from fine-grained motor outputs such as <grasp> and <rotate>.

The initial goal thus produces a set of structured representations running down the generative hierarchy. If these goal representations are held fixed, the system will encounter a corresponding cascade of prediction errors running back up the hierarchy. However, by moving in such a way as to reduce these errors, the motor system will produce behaviours that achieve the initial goal of <complex flake detachment>.

During the performance of an action plan, new goal representations will be selected at various levels in the hierarchy. For instance, the system might select <position core> and deselect <hammerstone grip>. When this new goal representation becomes salient, a new set of predictions is produced. These then remodulate the goal representations in models beneath it, such that <grasp> and <rotate> become goal representations regarding the core, which in turn modulate further fine-grained proprioceptive predictions further down the hierarchy. This new set of structured representations will produce a new set of corresponding prediction errors. By reducing this error through action, and by repeating this process, the system can reiterate a behaviour.[72]

So the combination of top-down and bottom-up transfer of predictions and prediction errors between hierarchical layers of models, and the presence of transitively linked goal representations, produces representations with nested part-whole structure. And this is a product of the fact that creating a Late Acheulean hand-axe is a particularly complex, goal-oriented activity requiring significant strategic planning abilities.

At this stage it is important to make clear what I am—and what I am not—claiming via this account. First, I am not claiming that structured representations emerged in the Late Acheulean. Goal representations with some level of nested part-whole structure were required to produce both the Early Acheulean and the Oldowan (though with much less sophistication in the case of the latter). Indeed, Planer and Sterelny (2021, p. 121-126) argue that structured representations are found in the cognitive profiles of the great apes and some primates, and I agree with them. What I am claiming is that the production of stone tools—and in particular, given the significant increase in strategic planning capacities required, the Late Acheulean—drove a marked increase in the complexity and sophistication of pre-existing representational

[72] An interesting question following from this is how a new goal representation becomes salient. There are at least two ways an account like mine may go on this issue, depending on how unifying one takes the error minimisation framework to be. On one hand, some theorists think the error minimisation can do the explanatory work—Hohwy and colleagues (2013; Corcoran, Pezzulo, and Hohwy 2020) appear to think this. On the other hand, we might posit some other computational mechanism to account for the raising of a new goal representation. I suspect the latter move is the better bet, but elaborating those details is beyond the scope of this paper.

capacities with nested part-whole structure. This echoes Stout's work emphasising the increasing complexity of action plans across the Early Stone Age (e.g. Stout 2011; Stout et al. 2021). Second, I am not claiming that the above framework provides a computational account of the cognitive capacities required to produce the language abilities we find in modern day humans. Proponents of tool-language co-evolutionary theories are often accused of failing to provide such an account,[73] despite the fact that doing so would actually negate any need for a co-evolutionary approach. If we were to find that all the cognitive capacities required for modern day language abilities were present in the Early Stone Age, then there would be no need to make the claim that a new behaviour was able to co-opt cognitive resources that had evolved for a pre-existing behaviour. The claim is not that the cognitive underpinnings of toolmaking and language are the same. Rather, what is required from a co-evolutionary theory is a stipulated set of capacities, that are importantly *not* equivalent to those required to produce language, but which are nonetheless capable of being acted on by selection to bring about full-blown language abilities. My point is that the above model offers an account of this kind. In particular, the level of planning and motor skill required to produce Late Acheulean tools signals the emergence of representations displaying sophisticated nested part-whole structure, and these are an important ingredient in explaining the evolution of language.

An important related issue concerns a dis-analogy between the way language and toolmaking are learnt. Becoming an expert Early Stone Age toolmaker takes a long period of time and significant focussed mental effort.[74] Sequential steps are reinforced over many repetitions, and the fast, fluid, tree-like hierarchical action performed by experts is the end product of a prolonged, iterated learning process. By comparison, it is often claimed that children appear to learn language almost effortlessly.[75] This might suggest that, in modern humans, sequential processing is required to learn how to produce a hand-axe, whereas tree-like hierarchical processing is employed from the outset by children learning to speak.

The above observation can be easily accommodated by the error minimisation account. To reiterate, the claim is not that the cognitive capacities required to produce language and

[73] For instance, Berwick and Chomsky (2017, p. 171) and Botha (2020) have criticised Stout and colleagues on such grounds. Gabrić (2021) makes a similar point to mine regarding the inadequacy of such criticisms.
[74] For instance, Stout reports that it took him 300 hours of practice to produce a Late Acheulean hand-axe that equalled the skills of those produced by ancient toolmakers at the famous Boxgrove site in England (Stout 2016, p. 33).
[75] My own view is that there is considerable evidence that language learning in childhood is far from "effortless" (e.g. Chater and Christiansen 2018; Yang and Piantadosi 2022). However, I want to show here that the error minimisation view can accommodate both sides of this debate.

stone tools are the same; rather it is that language evolution involved co-opting and refining some pre-existing cognitive resources that had evolved for toolmaking. Furthermore, there is plenty of scope between the Early Stone Age and today for syntax specific, tree-like hierarchical capacities to have evolved in response to increasingly language rich cultural environments. The differences in the way language and toolmaking are learnt may well be a product of the fact that domain specific mechanisms have evolved with respect to the former, but not the latter. It may even be the case that, in modern-day humans, sequentially learnt action plans exploit linguistic cognitive resources. All this is perfectly consistent with the claim that those syntax specific capacities had an evolutionary precursor in the structured representations required to produce stone tools; which is to say that it is perfectly consistent with a co-evolutionary error minimisation account.[76]

Finally, it is worth saying something about how the predictive processing account relates to traditional, Chomskyan computational models. There are at least two ways this could go. On one hand, some theorists believe the two frameworks can be fruitfully combined. For instance, Fitch's account of tree-like hierarchical cognition builds in aspects of both predictive processing and Chomskyan computation (in particular Fitch 2014; see also Fitch and Martins 2014). On his view, integrating the two frameworks provides a route to a broad theory of neuro-computation; one that does justice to the fact that we are animals, and also to the fact that we are animals with a peculiar set of capacities. Key here is the claim that humans have an advanced (but not species-specific) propensity to infer tree-like hierarchies from sequential data. The programme outlined in this article fits well with Fitch's account. In particular, we can see how a phylogenetically diverse trait—the control of action via error minimisation—might have provided the right kind of machinery that could be evolved, via toolmaking, into the more advanced tree-like hierarchies that are typically thought to underly language production.

On the other hand, some theorists are sceptical that Chomskyan computational models can be neurobiologically realised. Petersson, Folia and Hagoort, for instance, argue that the infinite memory resources required by Chomskyan computation is in contradiction with the fact that the brain is a finite-state system (2012). As a result, they think it "seems natural to try to understand language acquisition and language processing in terms of adaptive stochastic dynamical systems" and "syntax processing in terms of noisy spiking network processors."

[76] Thanks to an anonymous reviewer for pushing me on this issue.

(2012, 93). The error minimisation account is an attractive package for theorists of this stripe for two reasons. First, predictive processing is comparatively well-grounded in empirical neuroscience (e.g. Fitch 2014, p. 343). Second, the probabilistic neural networks of predictive processing lend themselves well to interpretation in terms of "stochastic dynamical systems" and "noisy spiking network processors". As such, the framework might be fruitfully developed by those who reject Chomskyan computation.[77]

3.4 Summary

In this section I proposed an error minimisation model for understanding the action sequences required to produce Late Acheulean tools. Stout and colleagues have demonstrated the neuro-anatomical overlap between toolmaking and language. The model I propose outlines a plausible computational overlap between the two behaviours: namely, the requirement for complex structured representations. As language production is one instance of the broader category of skilled intentional action, the practice of toolmaking may well have produced the right kind of cognitive platform required to support the production of language.

4. The evolution of language

In this section I situate my proposal within broader debates concerning the evolution of language. I begin by showing that the error minimisation account supports gradualism—the view that language evolved via incremental steps over a long period of evolutionary time—and rejects saltationism—the view that language appeared via sudden random mutation. I then emphasise an important feature of the account: it can explain the evolution of structured representations without positing a corresponding transition from sequential information processing to tree-like hierarchical information processing. This, I argue, makes it better suited to gradualism than some other gradualist proposals; in particular that offered by Planer and Sterelny (2021).

Saltationism is typified by Berwick and Chomsky (2016; 2017; 2019). In their view, the ability to produce syntax is the result of a random genetic mutation that produced a neural 're-wiring'. This event happened comparatively recently—some 80 to 120 kya. Consequently, among the hominin genus, the cognitive capacities required to produce syntax are restricted to

[77] Including Petersson, Folia, and Hagoort (2012), but also others, e.g. Christiansen and Chater (2016); Rodriguez (2001). Thanks to an anonymous reviewer for helpful comments here.

Homo sapiens. In contrast, the error minimisation account suggests that the cognitive capacities driving the early evolution of our syntactical abilities were not specific to either language or *H. sapiens*. Rather, those capacities are to be found at low levels in any organism capable of intentional action with nested part-whole structure. They have, however, reached particularly high levels of sophistication in the hominin lineage—notably from *Homo erectus* through *Homo heidelbergensis* to *H. sapiens*—in part through the co-evolutionary effects of toolmaking and language production. This is significant, as the capacity to produce syntax is often thought to be a—if not *the*—distinctive, domain specific feature of the language faculty (e.g. Hauser, Chomsky, and Fitch 2002). Language production utilises many capacities that play a broader role within cognitive systems; for instance, memory, executive control and theory of mind. While these are integral to the production of language, they are not language specific. The capacities underwriting the production of syntax, however, are plausibly language specific. The account developed here suggests that evolution of syntax was able to co-opt pre-existing capacities—structured representations—that had been increasingly refined through toolmaking. As such, the error minimisation approach supports gradualism.

Indeed, the gradualist credentials of the error minimisation model run deeper than this. Consider the following three competing views. As we have seen, Berwick and Chomsky (2016) are saltationists: they hold that explaining the complex nested part-whole relations we find in language commit us to structured representations, and that there are no plausible intermediary stages between representations without structure and representations with structure. So language cannot have evolved incrementally, and instead must have appeared suddenly. Frank and colleagues (2012) believe that parsimony and evolutionary continuity considerations make saltationism unattractive, and hence want an account that avoids sudden transitions. Their solution is to reject the claim that language production requires structured representations—instead, they develop an account of syntax based on sequential processing. Planer and Sterelny (2020) take a different line. On their reading, some basic nested part-whole structure is present in the behavioural and learning profiles of the great apes and some other primates. Thus there is no need to posit any transition from non-structured to structured representations between the last common ancestor with the chimpanzee and modern day humans; all that is required is the incremental sophistication of pre-existing capacities across that time-frame (some 7 million years).

These various accounts are attempting to balance two desiderata. The first is evolutionary continuity, which motivates a reluctance to posit any sudden transition to structured representations. The second is the notion that language production requires structured representations. Both Berwick and Chomsky and Frank and colleagues see these two desiderata as mutually incompatible. The former are committed to both the psychological reality of generative grammars and structured representations, and to the view that a gradualist account of such mechanisms is impossible. A sudden transition is thus an unavoidable aspect of their theory. For the latter, evolutionary continuity concerns outweigh the commitment to language production requiring structured representations. They thus attempt to generate a sequential account of the production of syntax. In developing their gradualist theory, Planer and Sterelny neatly sidestep this dilemma by decoupling the evolution of structured representations from the evolution of language, insofar as the former long predates the latter. However, this does not rid them of the need to posit a transition—all be it gradual—from representations without structure to representations with structure; rather, they have pushed that transition further back in phylogeny. And accounting for *that* transition, on their account, requires positing a corresponding transition from sequential information processing to tree-like hierarchical information processing.

The error minimisation account provides a different story; one that offers a more straightforward fit with the demands of gradualist explanation. This is because predictive processing architectures can account for the transition between non-structured representations and structured representations without needing to posit a corresponding transition in types of information processing. On the error minimisation account, structured representations evolve via a gradual increase in the quantity and complexity of models in a cognitive system; not via the emergence of a new type of processing. In what remains of the article, I will develop this point a bit further.

Planer and Sterelny tie the evolution of behaviour displaying nested part-whole structure to the evolution of Broca's area (2021, p. 141). In their view, the development of this area in the great apes, our own species, and some other primates—particularly macaques (e.g. Rizzolatti et al. 1996; Ferrari et al. 2003)—represents the emergence of a new type of information processing: tree-like hierarchical processing. Such capacities are capable of producing structured representations, which in turn are required to get complex goal-oriented behaviours like toolmaking and language up and running. However, complex goal-oriented behaviours are not confined to primates. For instance, consider dolphin hunting techniques (Gazda et al. 2005; Krützen et al. 2014), tool use among New Caledonian Crows (Miller et al. 2020; Taylor et al. 2009; Bugnyar 2019) or the ability of Elephant matriarchs to locate water during a drought (Foley, Pettorelli, and Foley 2008). On Planer and Sterelny's account, these behaviours will need to be explained without recourse to structured representations, or they must appeal to convergent evolution. Now, there is no in-principle reason why these explanations are not possible; but some story must be told.

The error minimisation approach avoids this explanatory burden, and offers a more evolutionarily continuous account. On this view, the stratified cognitive hierarchies of organisms displaying very simple, reactive, inflexible behaviour will be composed of a small number of models characterised by sensorimotor representations that issue simple predictions to the motor plant. Systems of this kind will not produce structured representations, as they lack the higher-level, multi-modal goal representations required to modulate representations further downstream in the hierarchy. On the other hand, organisms displaying more complicated behaviour will have extended generative hierarchies, with models in the upper levels employing sophisticated, multi-modal representations capable of producing predictions over broad spatiotemporal scales, and capable of structuring the representations in models below it according to specific goals (see Corcoran, Pezzulo, and Hohwy 2020 for a detailed account of how generative hierarchies might be elaborated in this way).

So the predictive processing approach allows us to move from inflexible, reflexive behaviour to more complex intentional action via the gradual increase in number and sophistication of models in a stratified hierarchy. As such, there is no need to posit a transition from sequential information processing to tree-like hierarchical information processing; it is error minimisation all the way down. The key transition to be explained on this approach is the transition from basic, low-level sensorimotor representations to more abstract, multi-modal goal representations. The role that Broca's area plays in the processing of complex goal-oriented actions suggests that its function may lie in processing representations of this latter kind. Explaining this transition in representational sophistication may be a formidable task; but it is a task that Planer and Sterelny must also address.

To sum up: the work of Frank and colleagues' indicates that, when it comes to cognitive processing, there are those who take evolutionary continuity concerns very seriously; to the extent that they are willing to reject the dominant view that language processing requires structured representations. Planer and Sterelny's account offers a gradualist route to the evolution of structured representations, but one that is committed to a transition between types of information processing. The predictive processing account also provides a gradualist route to structured representations, but one that doesn't require any transition between types of processing. It is an attractive view, then, for those who emphasise evolutionary continuity.

5. Conclusion

My goals in this paper have been twofold. I aimed first to provide an error minimisation account of the cognitive processes underlying Late Acheulean toolmaking. I argued that doing so reveals that the task requires complex structured representations, and that these represent a plausible pre-cursor to the fully formed syntactic capacities found in modern humans. I then situated my account in the literature, and gave some reasons why it might be a better fit with gradualist theories of language evolution than Planer and Sterelny's account.

More broadly speaking, the error minimisation approach offers a distinctly continuous story of the evolution of complex action; it is thus an attractive option for those who prioritise pragmatics and gradualism in the evolution of language.

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Statement of Contribution

This thesis is submitted as a Thesis by Compilation in accordance with https://policies.anu.edu.au/ppl/document/ANUP_003405

I declare that the research presented in this Thesis represents original work that I carried out during my candidature at the Australian National University, except for contributions to multi-author papers incorporated in the Thesis where my contributions are specified in this Statement of Contribution.

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CHAPTER 6

Cognitive Archaeology Meets Cultural Evolutionary Psychology

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Abstract: Cecilia Heyes (2018; 2019) has recently developed a novel framework for understanding human cognitive evolution. Contrary to many traditional views, *cultural evolutionary psychology* argues that distinctively human cognitive traits are transmitted culturally, not biologically. In labelling these mechanisms of thought “cognitive gadgets”, Heyes draws a direct analogy with the cultural artefacts studied by archaeologists. In this chapter I explore how cultural evolutionary psychology can inform research in cognitive archaeology, and vice versa. On the former line of thought, I argue that adopting Heyes’ framework goes some way to addressing the Wynn’s (2002) methodological challenge by bringing the categories of the psychological and archaeological sciences closer together. Nonetheless, deep inferential challenges remain. I look at how we can interpret the record through the lens of cognitive gadgets, using behavioural modernity as a case study. I then examine the way cognitive archaeology can inform research in cultural evolutionary psychology. Using new work on the evolution of language, I argue that evidence from cognitive archaeology strengthens Heyes’ case that language is a gadget.

1. Introduction: cognitive archaeology meets philosophy

What can philosophy offer the discipline of cognitive archaeology? One answer to this question is: analysis. Philosophers do not have to coordinate excavations, collate findings, or build datasets. Most of our time is spent reading, writing and thinking. But what should philosophers of cognitive archaeology think about? Luckily, there is no shortage of topics apt for analysis.

One popular topic is methodology. Recently philosophers have become particularly interested in analysing the way archaeologists generate evidence, and how they use evidence to produce inferences (Wylie 2002; Chapman and Wylie 2016; Currie 2018; Currie and Killin 2019; Pain 2019; Killin and Pain 2021). Such questions are particularly pressing in the case of cognitive archaeology. As Hawkes (1954) taught us, it is one thing to try and reconstruct the behaviours involved in producing an artefact; it is another thing entirely to try and reconstruct the cognitive and social processes driving those behaviours.

Another popular topic is the nature of cognition. Debates over the representational theory of mind have been a mainstay in the philosophy of cognitive science, particularly in the last thirty years. In a nutshell, this debate concerns the claim that the brain is analogous to a computer. Some people think the analogy is a good one; others less so.[78] This split is also found in cognitive archaeology. Some researchers work in a representational paradigm (e.g. T. Wynn 1989; 1993; 2000; Toth and Schick 1994; Hiscock 2014; Shipton, Clarkson, and Cobden 2019; Cole 2016), while others are influenced by embodied, enactive, extended & embedded (4E) frameworks (Noble and Davidson 1996; Ingold 2013; Malafouris 2013; 2016; Tomlinson 2015). So plenty of scope for philosophical analysis here too.

However, in this chapter I want to do something a bit different. The focus will be more on synthesis, less on analysis. Specifically, I am interested in theoretical and methodological integration. This task represents an acute challenge for cognitive archaeology. As Wynn notes, "[u]nfortunately, the disciplines of archaeology and psychology have never shared much in the way of theory and methodology"; as such, "it is very unlikely that variables taken directly from the psychological literature could be applied to archaeological remains" (Wynn 2002, 390). Moreover, "the traditional categories of archaeology are inappropriate" in so far as "[n]one […] has ever been defined with cognition in mind, and it would be misleading to use them as such" (Ibid. 390). So the success of cognitive archaeology depends on integrating two disciplines with very little crossover in terms of either methodology or foundational categories, and this is no small task.

As a brief illustration of the depth of this challenge, consider the divergence between cognitive psychology and archaeology on the class of capacities considered 'distinctively human'. A cognitive psychologist's list might include metacognition, mental mapping and

[78] I am using very broad brush strokes here. There are in fact important differences between representational theories of mind and computational theories of mind.

episodic memory. Meanwhile, an archaeologist's list would typically list capacities such as symbolic cognition, abstract reasoning, and imagination. There are of course some overlaps—language and theory of mind for instance—but we should certainly hope for more consensus than that. And the reasons for this divergence are precisely those that Wynn identifies: archaeologists are trying to explain transitions in the archaeological record (in the case of 'distinctively human', traditionally the European Upper Palaeolithic); whereas cognitive psychologists are often trying to identify those capacities that distinguish human from non-human animals, and develop mechanistic hypotheses to explain those capacities.

Addressing such problems requires synthesis; and this is an area where philosophers can help. How exactly do we apply conceptual frameworks from the psychological sciences to the archaeological record? How do we integrate the methods and categories from psychology and archaeology to produce a unified discipline? And how do we do all this in a way that produces reliable inferences to the past? Here I aim to make some headway on these questions.

The particular psychological framework I am interested in is Cecilia Heyes' (2018; 2019) cultural evolutionary psychology. According to Heyes, many distinctively human cognitive capacities are transmitted via cultural evolution rather than via biological evolution. Her claim is that cultural processes facilitate not only the intergenerational transmission of information processed *by* cognitive mechanisms, but the development *of* cognitive mechanisms. Prima facie support for this view comes from recent work on adaptive neuroplasticity and neural reuse (e.g. Anderson 2010; 2014; Anderson and Finlay 2014). Heyes builds on this by outlining evidence suggesting that the cognitive capacities involved in cultural learning are themselves culturally inherited.[79] She argues that selective social learning, imitation, theory of mind and language are built during development via processes of cultural inheritance. In labelling these cognitive capacities "cognitive gadgets", Heyes draws a direct analogy with the culturally transmitted material artefacts studied by archaeologists. When considered in the context of cognitive archaeology, cultural evolutionary psychology yields a striking conclusion—stone tools and cognitive mechanisms are more similar than we have traditionally thought, insofar as both are built via cultural forces. As she puts the point "[d]istinctively human ways of thinking are products of the same process—cultural

[79] I will not attempt to defend the cognitive gadgets hypothesis in any detail here—I take the position to be prima facie plausible, and proceed from there. But this is not to say that the view does not face very serious challenges. Some of the most pressing of these, to my mind, are outlined in Roige and Carruthers (2019), Turner and Walmsley (2020), and Brown (2021).

evolution—as machines in the outside world; they are pieces of technology embodied in the brain." (Heyes 2018, 2).

Heyes' theory is bold, novel, and, if true, has far reaching implications for the way we think about the evolution of human cognition. Yet so far there has been relatively few interactions between cognitive archaeology and cultural evolutionary psychology. As Heyes notes, bringing the two research programs together is important work: "connecting the cognitive gadgets theory to key events in human evolution, using the archaeological record, is a priority for future research" (Heyes 2018, 210). Here I set myself two general tasks. First, I address the question of how cognitive archaeology might be conducted within a cultural evolutionary psychology framework. I take up Wynn's methodological challenge, and look briefly debates about behavioural modernity through the cognitive gadgets lens. Second, I assess how cognitive archaeology might inform research in cultural evolutionary psychology. Here I use recent work in the evolution of language as a case study.

Before I begin, a brief caveat: my aims here are modest. I merely want to take some preliminary steps toward the larger project of bringing together the cognitive gadgets framework and cognitive archaeology. My goal is to think through some ways in which the two frameworks might be usefully combined, identify some key problems, and point towards directions for future research. The interaction between cultural evolutionary psychology and cognitive archaeology will hopefully be a long and fruitful one; in what follows I simply aim to help get the conversation started.

I proceed as follows. Section 2 provides a brief overview of cultural evolutionary psychology. Section 3 explores some of the implications of practicing cognitive archaeology in a cultural evolutionary psychology framework. Section 4 outlines how research in cognitive archaeology can inform cultural evolutionary psychology. Section 5 concludes.

2. What is cultural evolutionary psychology?

Cultural evolutionary psychology offers a new answer to an old question. The old question is: what explains the capacities that set humans apart from non-human animals? Heyes' answer to it is: cognitive gadgets.

Cognitive gadgets are unique in the logical geography of human evolutionary theory. This is because they are a novel combination of two otherwise familiar ideas. First, cognitive gadgets are *mechanisms* of thought. They are hence not the outputs of mental processes (such as behaviours or cultural artefacts), but mental processes themselves. Second, cognitive gadgets are produced during ontogeny via cultural learning, and not via genetic inheritance.

These commitments set cultural evolutionary psychology apart from two main theoretical alternatives: *evolutionary psychology* and *cultural evolutionary theory*. Like Heyes', proponents of evolutionary psychology argue that understanding distinctively human mechanisms of thought is central to explaining the evolution of human behaviour and culture. However, contra Heyes, evolutionary psychologists maintain that these mechanisms of thought are genetically inherited (e.g. Barkow, Cosmides, and Tooby 1995; Pinker 1995). Cultural evolutionary theorists, on the other hand, agree with Heyes that cultural evolution is the most important inheritance mechanism producing distinctively human behaviour and culture; hence both reject the emphasis evolutionary psychology places on genetic inheritance. Yet contra Heyes (and evolutionary psychology) cultural evolutionary theorists typically downplay, or at least bracket off, the role of the mind in accounting for human distinctiveness (e.g. Richerson and Boyd 2008; Boyd 2017).

So Heyes unites the cognition-focussed accounts favoured by evolutionary psychology with the emphasis cultural evolutionary theory places on processes of cultural inheritance. A cognitive gadget, then, is a mechanism of thought that is built during ontogeny through cultural forces. So—what exactly does a cognitive gadget look like?

Heyes' proof of principle for a cognitive gadget is literacy (Heyes 2018, 19-22). On one hand, we know that learning to read changes the neural configuration of our brains. In turn, this allows people to access large stores of intergenerationally transmitted information. Literacy is thus a cognitive mechanism that is also involved in processes of cultural learning. On the other hand, writing has only been a feature of human lives for some 5 to 6 thousand years; too short for literacy to have been assimilated genetically. Literacy is thus a culturally inherited cognitive mechanism. Heyes' central argument is that other cognitive mechanisms involved in cultural learning—selective social learning, imitation, theory of mind and language—should be understood in the same way we understand literacy.[80]

In sum, cultural evolutionary psychology prioritises both cognition and culture in explaining human evolution. Although Heyes is careful to stipulate that there are important

[80] More recently, Heyes has expanded this list to include metacognition (Heyes et al. 2020) and moral thinking (Heyes 2021).

genetically acquired pro-social and attentional capacities that allow for the development of cognitive gadgets—the genetic "starter kit" (Heyes 2018, Ch.3)—these are "small ordinary" attributes, and contrast with the "big special" cognitive mechanisms (such as theory of mind, language, causal understanding, etc.) that have reached such a high level of sophistication in humans. So, for Heyes, while the development of human behavioural and cognitive traits is always the product of complicated interactions between biological, environmental and cultural processes, the latter is key.

3. Gadgets and artefacts: doing cognitive archaeology in a cultural evolutionary psychology framework

Let's suppose that Heyes is right. What does cognitive archaeology look like when practiced in a cultural evolutionary framework? In this section, I explore some of the implications and challenges of interpreting the archaeological record using cognitive gadgets.

3.1 On categories and methodology

It is important to note that cultural evolutionary psychology is not first and foremost a theory *of* cognitive capacities; rather, it is theory of how some familiar capacities *get built*.[81] Often, cognitive archaeologists operate using the former type of theory. The general strategy is to take a psychological model of some mechanism, and then trace the evolution of that mechanism by analysing the record using the model. For instance, Wynn & Coolidge (2004; 2018) use Ericsson's (1995; 1999) model of long-term working memory and expert performance; Noble & Davidson (1996) use Gibson's (1979) theory of affordances; and Cole (2016) uses Premack & Woodruff's (1978) model for theory of mind. Heyes' theory proposes nothing new about the structure and operations of, say, our language processing capacities. Rather, it proposes something new about the ontogenetic and phylogenetic development of those capacities. Applying the framework to the archaeological record thus raises a different set of questions.

Recall Wynn's point that none of the traditional categories in archaeology have "… ever been defined with cognition in mind, and it would be misleading to use them as such" (Wynn 2002, 390). Rather, archaeological categories tend to be based on presumed properties like artefact function or social complexity, or else according to more practical concerns, such

[81] Imitation is something of an exception here, as the gadgets account of imitation leans quite heavily on Heyes' associationist framework.

as usefulness of temporal ordering. Wynn's point is that, in approaching the record with, say, Ericsson's model of long-term working memory, categories like "Middle Stone Age" or "Classic Period" are not necessarily useful. Rather, we must reimagine the record in cognitive terms.

Interestingly, the nature of cultural evolutionary psychology goes some way to resolving this categorical gap, in the following sense. We do not have to reimagine the categories of archaeology in psychological terms, because Heyes has, to some extent, *reimagined psychology in archaeological terms*. This is because cognitive gadgets defines psychological categories with technology in mind; as we have seen, for Heyes', capacities like language and theory of mind are "pieces of technology embodied in the brain" (Heyes 2018, 2). Accordingly, the phenomena studied by archaeology and psychology turn out to be more similar than we have traditionally thought.

Now, it is important to reiterate that, for the most part, we are still dealing with the same old neurally realised capacities of more traditional theories. What has changed is how they are built. So there is a broad sense in which the categories of the two disciplines have been brought together: the classic methodological challenge of cognitive archaeology has been to theoretically align a biological category—cognitive mechanisms—with a cultural category—the archaeological record; on Heyes' account however, we are attempting to align two types of culturally produced phenomena.

Coming to grips with the implications of this move is a complicated task. Here is one line of thought that might seem initially attractive. The central inferential problem that faces cognitive archaeology is as follows: how do we produce reliable inferences from the record to the cognitive capacities of past populations? One might think that adopting Heyes' framework goes some way to bridging the epistemological gap between artefacts and minds, for the following reason. On more traditional views of the evolution of human cognition, the challenge is to understand causal relationships between a genetically transmitted capacity and a culturally transmitted artefact. On Heyes' view, both capacity and artefact have their origins in cultural learning. So one might think that, as they are products of the same inheritance mechanism, the epistemic gap between artefacts and cognition is reduced. In other words, the inferential route from artefact to gadget is more straightforward than the route from artefact to instinct.

This move—shifting the traditional categories we use to understand cognition and material culture—has been employed in cognitive archaeology in a different context. Lambrous

Malafouris (2013; 2016) has argued that expanding the ontology of mind beyond the brain can likewise help address the inferential challenge. According to *material engagement theory*, artefacts are more "minded" than we have traditionally taken them to be. Consequently, there is less of an epistemological gap between material culture and the mind than traditional, representational theories of mind would have us believe.

In the case of cultural evolutionary psychology, I do not think this strategy works. Take Heyes' proof of principle for cognitive gadgets: literacy. Suppose we are tasked with trying to identify the cognitive mechanisms governing the production of early writing systems, and are working on the assumption that those mechanisms were built by biology. This task would not get any easier were we to suddenly discovered that they were instead built by culture. Likewise, finding out that theory of mind is culturally inherited would not make it any easier to attribute 3rd order intentionality capacities to the makers of Middle Stone Age handaxes (Cole 2016; 2019). In each case we still need to go through the process of applying a cognitive model to the record and running an inference to the cognitive capacities of past individuals; and knowing that both capacity and artefact were built by the same inheritance mechanism doesn't make that process any less daunting.

This point can be made sharper in the following way. Cultural evolution is typically 'messier' than biological evolution. This is because cultural evolution relies on fleeting and unstable mechanisms like convention, social norms and habits. Cultural evolution also involves vertical, oblique, and horizontal lines of inheritance: I can learn from my parents, my aunts and uncles, or from my own peer group. In contrast, biological evolution relies on genetic mechanisms—and so is more stable—and typically involves only vertical lines of inheritance. This latter fact is thought to mean biological evolution operates on slower time scales than cultural evolution (e.g. Ram, Liberman, and Feldman 2018). So on the traditional view, cognitive archaeologists were trying to infer capacities generated by slow, stable processes from artefacts generated by fast, unstable processes. On the cognitive gadgets view, we are trying to infer capacities generated by fast, unstable processes from artefacts generated by fast, unstable processes. And the later project looks just as inferentially challenging as the former. Indeed, one might even think it looks more challenging.

This provides some illumination regarding the terms of Wynn's categorical challenge. Successful cognitive archaeology does not require an equivalence relation between archaeological categories and psychological categories; rather it requires an explication relation. And the former does not guarantee the latter. We appeal to theories in the cognitive sciences in order to *explain* patterns in the record; however, the record gives us a bound on what people can do, and what people can do is only partly determined by what they can think. So doing cognitive archaeology in a cultural evolutionary framework goes some way to addressing the methodological challenge identified by Wynn—there is a sense in which Heyes has defined the variables of cognitive science with artefacts in mind, and this brings the two disciplines closer together. However, this does not in itself make cognitive archaeology more inferentially robust.

This conclusion might sound a bit pessimistic. There are, after all, some important methodological upshots to the idea that cognitive mechanisms and material artefacts share a common inheritance mechanism. The most important of these, by my lights, is that archaeological record gives us an independent window into the extent and fidelity of cultural transmission in a lineage over a given period. In the next section, I will expand on this idea.

3.2 Force theories & narrative theories

So—what does practicing cognitive archaeology in a cultural evolutionary framework actually look like? Answering this question involves tackling the following question: how do we interpret the archaeological record using the cognitive gadgets hypothesis?

Heyes (2018, 12-13/210) stresses the importance of this task via a distinction between two strategies employed by human evolutionary theorists. *Force theories* emphasise the processes involved in producing some phenomena, such as the role cultural forces play in shaping the human mind. *Narrative theories,* on the other hand, are more interested in chronology. They attempt to outline the series of events leading up to, for instance, the development of language. Theories in cognitive archaeology are typically more narrative in flavour. The great promise of the discipline is that the archaeological record might be used to provide a lineage of the evolution of human cognition. However, as Heyes notes, an ideal theory would combine both of these virtues: it would "…use chronology as evidence of forces, and forces to explain chronology" (2018, 210). In remainder of this section, I engage with the latter project. In section 4, I engage with the former.

The force/narrative distinction resembles a well-known distinction in the philosophy of science between *robust process explanations* and *actual sequence explanations* (Sterelny 1996; see Jackson and Pettit 1992; 1990 and Sober 1983 for precursors to the distinction). Robust process explanations posit high-level causes for an outcome. For instance, we might say that the First World War was caused by an arms-race between European powers. On the other hand, actual sequence explanations focus on the particular series of events leading up to some outcome. For instance, we might say that the First World War was caused by the assassination of Archduke Franz Ferdinand. Robust process explanations are to some extent encapsulated from the contingencies of actual sequence explanations. If a European arms race was the over-riding cause of the war, then the conflict would have begun regardless of Ferdinand's assassination.

In this example, we can see that the "arms race" hypothesis can be used to explain the series of events coming after Ferdinand's assassination leading to war. How would we go about using the cognitive gadgets theory to explain sequences we find in the archaeological record? Heyes (2018: 210-213) makes a start on this task. Here I build on her observations, outline some criticisms, and suggest some future lines of inquiry.

3.2.1 Cognition, demography and transitions

Heyes' focus is on the European Upper Palaeolithic and the African Middle Stone Age, and their association with behavioural modernity. She notes that influential work by Mcbrearty and Brooks (2000) suggests a much more graded and geographically dispersed account of the development of behavioural modernity, which challenges models that appeal to random, one-off genetic events (e.g. Coolidge and Wynn 2018; Mellars 2005; Mellars and Stringer 1989). In place of genetic accounts, cultural evolutionary theorists have proposed the *collective intelligence* hypothesis (Henrich 2015; Muthukrishna and Henrich 2016; Richerson and Boyd 2008; 2013; Boyd 2017). The idea here is that the changes we see in the European Upper Palaeolithic and the African Middle Stone Age are the product of changes in demography, not genetics. Humans thus had all the biological traits required for behavioural modernity perhaps as far back as *H. heidelbergensis*, but such behaviours could only be generated once human groups reached a sufficient size, and/or were sufficiently interconnected.

Powell et. al. (2009) make a similar point. According to the *social-scale hypothesis*, the complexity of material culture that a population can maintain is a function of its size. Powell et al.'s modelling begins with the assumption that skill levels act as a constraint on technological complexity. In turn, skill levels are constrained by cultural learning. Effective cultural learning depends on two factors: (i) how noisy the information flow from expert to novice is; and (ii) the spread of aptitude among experts and novices. As the spread of aptitude among experts and novices emerges from normal variation within a population, aptitude is sensitive to social-scale. Consequently, so is technological complexity. So for Powell et al., the European Upper Palaeolithic is a signal of changing demographic patterns, not upgrades in cognition.

Heyes' take on this debate is to maintain the emphasis on cognition stressed by genetic accounts, but swap in cultural rather than biological inheritance mechanisms. In other words, the collective intelligence and social-scale hypotheses hold that all the necessary cognitive mechanisms required for behavioural modernity were in place well before the European Upper Palaeolithic or African Middle Stone Age. What was missing were the necessary social structures required to generate more complex outputs from those structures. Heyes, in contrast, argues that the necessary cognitive mechanisms were *not* in place before these demographic changes. Rather, all that was in place were the "small ordinary" components of the genetic starter kit (see section 2). The demographic changes leading up to the European Upper Palaeolithic and African Middle Stone Age allowed distinctively human cognitive mechanisms to be built by creating the conditions required for the more high-fidelity transmission of cultural capital. So for cultural evolutionary theorists, the onset of behavioural modernity was a transition in *what* people thought about, whereas for Heyes it is also a transition in *how* people thought.

I believe there are significant concerns with this line of thought. Here Heyes asks us to buy in to a "major transition" account of both the record and of human cognitive evolution. She tells us that stone tool technologies prior to the European Upper Palaeolithic and the African Middle Stone Age were produced in the absence of cognitive gadgets:

> "The Small Ordinary components of the genetic starter kit […] were already in place and had been supporting cooperation and simple stone technologies for millions of years. Demographic changes allowed the Small Ordinary components to be elaborated by cultural group selection into the mechanisms that we now identify as, for example, causal understanding, episodic memory, imitation, theory of mind and full-blown language." (Heyes 2018, 213)

But can the components of the genetic starter kit—increased prosocial tendencies, information processing capacities and attentional biases—really support, say, late Acheulean or prepared-core technologies? There is evidence from cognitive archaeology that suggests otherwise. I will argue that the record supports graded account of capacities like theory of mind and language.

In turn, this indicates that cognitive gadgets have a much older and more gradual evolutionary history.

Let's begin with theory of mind. This refers to our ability to infer, or understand, the mental states of another individual. For instance, I might infer from yawns in the audience that participants in my lecture are bored. Our theory of mind capacities are embedded in a broader framework of orders of intentionality. These orders begin with the awareness of my own mental states, and progress from there. For instance: I intend (1); that my audience understand (2); that Heyes believes (3); that Boyd disagrees (4); with Mellars' commitment to genetic models of behavioural modernity (5); and so on. Theory of mind is thus located in the second order of intentionality. Recently, Cole (e.g. 2016; 2019) has produced a range studies attempting to correlate theory of mind capacities and orders of intentionality with the lithic record. His results suggest that our ability to interpret other people's mental states long predate the African Middle Stone Age. On his reading, early *Homo* variants and even late *Australopithecine* possessed second-order intentionality, and hence theory of mind capacities. *H. erectus* and the appearance of the Acheulean signal third-order intentionality, and fourth-order intentionality arrives with *H. heidelbergensis* and prepared core technologies. This reading contrasts starkly with Heyes' proposal, in which theory of mind capacities only emerge as a result of demographic transitions during the African Middle Stone Age.

Recent work on the evolution of language likewise suggests a deeper evolutionary history than Heyes proposes. Studies from the emerging field of neuroarchaeology indicate that many of the mechanisms required for language production may well have been in place by the Late Acheulean (Putt et al. 2017; Putt 2019; Stout et al. 2008; Stout and Chaminade 2012). Theoretical work also bolsters this conclusion. For instance, Planer (2017b) makes a case that hominins of the early Pleistocene were equipped with a protolanguage. Indeed, Planer & Sterelny (2020) see evidence for the type of hierarchical cognition needed to support language in the behaviour of the great apes. This work will be the focus of section 4, so I will not dwell on it here; suffice to say that plenty of thinking in cognitive archaeology supports the conclusion that the origins of language long predate the demographic events singled out by proponents of the collective intelligence hypothesis. This work adopts a more graded account of the development of complexity in technology, which in turn supports a more graded account of the capacities driving that development.

What might a cognitive gadgets theorist say in response to this evidence? There are various alternatives available. For instance, note that there is no requirement that the entire suite of capacities required for cultural learning should all have the same evolutionary trajectory. So maybe proposing a staggered account is the right move. For instance, we might accept that language and theory of mind have much older origins, but hold that imitation and selective social learning are newer innovations. And the latter were necessary for the transitions we see in the African Middle Stone Age and European Upper Palaeolithic. However, recent work suggests that cumulative technological culture has its origins in the Acheulean and perhaps even the Oldowan (de la Torre et al. 2003; Stout et al. 2010; Stout 2011; Stout et al. 2019). Typically, imitation and selective social learning are thought to play a role in supporting cumulative technological culture. If so, this would count against a staggered account.

A different—and to my mind more promising—solution is to develop a graded account of the evolution of cognitive gadgets. This approach would understand the cultural evolutionary process in terms of small, incremental changes over a long period of time, and reject any appeal to large, sudden changes. The notion that there were major transitions in hominin cognitive/behavioural evolution is one that is tied to the notion that there are major technological transitions signalled by the archaeological record. Yet there are is active debate concerning both the extent and significance of such transitions (e.g. Straus 2009; Clark 2009; Clark and Riel-Salvatore 2006; Shea 2011). And a more graded account of the record suggests a more graded account of the cognitive capacities involved in its production. Consequently, we need a graded account of the cultural evolution of selective social learning, imitation, theory of mind and language. However, while there is no in principle reason why this would not be possible, such an account must address important questions. The piecemeal evolution of cognitive mechanisms over millions of years requires explanation in the cultural case: what factors impede the normally fast processes involved? How do these factors impact on the construction of mechanisms? This is less so in the biological case—slow and steady is the standard *modus operandi* in biology. Working out the details of a gradualist account of cognitive gadgets is an important line of future inquiry for cultural evolutionary psychology.

Heyes notes that her proposal is “a start” for which she uses a “broad brush” (Heyes 2018, 210), and the comments I offer here are an attempt to develop and refine that proposal.[82]

[82] Moreover, there are aspects of the cognitive gadgets framework that make it uniquely interesting from a cognitive archaeological perspective. One of these is that Heyes’ offers a specific account of how new mechanisms (gadgets) are built from older, and more phylogenetically diverse, mechanisms (the genetic starter kit). Rival co-evolutionary accounts are often less specific regarding the latter (though see Planer and Sterelny,

I will engage with these themes in more detail in the next section, but the general point is this: theories of human cognitive evolution are well advised to adopt gradualism, and cultural evolutionary psychology is no different.

4. Is language a cognitive gadget?

In the previous section, we saw how we might use cultural evolutionary psychology to interpret the archaeological record. In other words, we used forces to explain chronology. In this section, we will use chronology as evidence of forces. To return to our World War One analogy, we can see that the series of events coming after Ferdinand's assassination and leading to war provide evidence for the "arms race" hypothesis. So; how can the archaeological record be used to provide evidence for Heyes' theory?

One of Heyes' most contentious claims is that language is plausibly a cognitive gadget. Under the influence of Chomsky, the thought that humans possess innate, language specific cognitive mechanisms has dominated linguistics.[83] Contrary to this, Heyes argues that there is increasing evidence for the view that language is produced by domain general mechanisms that are built by cultural processes (Heyes 2018, 183-189). The cognitive gadgets hypothesis is thus a viable alternative to nativist accounts of language (see also Christiansen and Chater 2016).

The evidence Heyes offers—from research on the neural distribution of language processing, on the role of domain general sequence learning in speech production and comprehension, and on how social shaping affects the way children use language—addresses ontogeny. Cognitive archaeology offers cultural evolutionary psychologists something further—a line of evidence to the phylogenetic evolution of language. Here I provide an overview of recent work on tool-language co-evolutionary hypotheses, and argue that such work lends support to the claim that language is a cognitive gadget.

2021). As such, Heyes framework offers cognitive archaeology important resources for understanding the transition from predominantly biological cognition to more culturally scaffolded cognition. This too is an important avenue for further research.

[83] For reasons of space, I am using broad brush strokes here. However, it is worth noting that the term "language" means different things to different people in this debate. For Chomsky "language" means the ability to combine meaningful units into hierarchically ordered strings of meaningful units (i.e. words into sentences), and the primary function of this ability is the organisation of thought; not communication. Others use (including Heyes) use "language" in the more standard sense.

4.1 The evidence from neuroarchaeology

Over the last 40 years there has been growing interest in the idea that language and tools co-evolved (Reynolds 1976; 1993; Montagu 1976; Isaac 1976; Greenfield 1991; Kimura 1993; Sterelny 2012; Planer 2017b; Planer and Sterelny 2021). Traditionally, cognitive archaeologists have contributed to this research by applying models from the cognitive and language sciences to various tool industries (Noble and Davidson 1996; Mithen 1996; K. R. Gibson and Ingold 1993; Toth and Schick 1994). Adopting this inferential strategy requires addressing a number of methodological problems; from Wynn's (2002) concerns regarding the integration of archaeological and psychological categories, to more recent concerns regarding heuristics for theory choice and issues with cross-cultural sample diversity (Killin and Pain 2021; Killin and Pain in press). More recently, cognitive archaeologists have adopted a different inferential strategy. Experimental neuroarchaeology takes modern human subjects and uses neuroimaging techniques to investigate their brain activity during knapping tasks. In the area of language evolution, attention has focussed on assessing neural overlap between toolmaking and language (e.g. Stout and Chaminade 2007; Stout et al. 2008; Stout and Chaminade 2012; Putt et al. 2017; Putt 2019). If such overlap is found—so the thought goes—then we have evidence for the claim that emerging language capacities were able to co-opt pre-existing capacities that had evolved for toolmaking. Neuroarchaeology inherits a different inferential problem—modern humans are not, for instance, *H. Erectus*, and so the argument produced is one by homology. The strength of the inference thus relies on the level of neural similarity between the two species, and that ratio is notoriously difficult gauge.

Here I put such concerns aside. I will take it that demonstrating neural overlap between toolmaking and language production in modern humans lends some evidential weight to tool-language co-evolutionary hypotheses. The case I want to make is that this is good news for cultural evolutionary accounts of language. However, our first question is: does such an overlap exist?

In their 2008 study Dietrich Stout & colleagues took three expert Early Stone Age knappers and used fluorodeoxyglucose positron emission tomography to assess the areas of the brain co-opted by both Oldowan and Late Acheulean tasks (Stout et al. 2008). They found that late Acheulean toolmaking produced increased activity in areas associated with language production as compared with Oldowan toolmaking. Of particular interest is the activation of inferior frontal gyrus. This area is already associated with language production, and is increasingly thought to play the computational role of a more general-purpose supramodal processor of hierarchically sequenced information (e.g. Koechlin and Jubault 2006; Fadiga, Craighero, and D'Ausilio 2009; Poldrack 2006). Also of interest is the activation of the right and left dorsal portions of inferior frontal gyrus *pars triangularis*. The former is thought to play a role in syntactic/semantic integration, and in processing high-level motor representation and hierarchical action-sequences more generally. The latter is associated with working memory and sentence processing (Makuuchi et al. 2009; Elmer 2016; Matchin 2018).

More recently, Shelby Putt & colleagues have expanded the scope of this work using functional near-infrared spectroscopy (Putt et al. 2017; Putt 2019). Putt & colleagues were particularly interested in investigating whether the mode of learning—either verbal or non-verbal—had any effect on the brain regions co-opted by Oldowan and Late Acheulean tasks. Participants in the experiment were taught to knap using either spoken language or via visual aids alone. Their results showed increased activity in ventral precentral gyrus—associated with the guidance of visual working memory—and the temporal cortex—associated with the integration of visual, auditory and sensorimotor information. Importantly, they found that only participants who had learnt to knap verbally showed any activation in *pars triangularis* in the right hemisphere. This suggests that the activity Stout & colleagues observed in that region may be a product of the way their subjects were taught to knap, rather than an indication of a co-evolutionary relationship between toolmaking and language. The increased reliance on sensorimotor control via working memory and reduced activity in inferior frontal gyrus found by Putt & colleagues suggests a more motor-based account of Acheulean toolmaking than is suggested by Stout & colleagues' results. However, activation in inferior frontal gyrus still lends weight to tool-language co-evolutionary hypotheses (Putt 2019, 313).

To sum up: this work supports the idea that an emerging behavioural phenotype—language—was able to co-opt existing neural substrates that had evolved to support an older behavioural phenotype—toolmaking. But why is this important?

4.2 On the evolution of language

To see this, it is necessary to situate the neuroarchaeological evidence within the broader logical geography of theoretical work on language evolution. I will characterise this work according to three central (and importantly related) debates: (1) saltationism vs selectionism; (2) domain specific vs domain general mechanisms; and (3) cultural evolution vs gene-culture coevolution. This demonstrates that neural overlap in toolmaking and language lends support to the claim that language is a gadget.

4.2.1 Saltationism vs selectionism

A central debate in the field is between those who argue that language evolved in incremental stages and those who hold that it appeared suddenly. The latter position—*saltationism*—is typified by Berwick & Chomsky (2016; see also Hauser, Chomsky, and Fitch 2002). In their view, the evolution of language required a random genetic mutation that resulted in a neural 're-wiring'. This morphological change produced the computational mechanism ('merge') required for syntax, which in turn is a necessary condition for language. This purported event happened comparatively recently—some 80,000 to 120, 000 years ago. On the other hand, *selectionists* (also called 'gradualists' or 'neo-Darwninians') hold that language emerged piecemeal via incremental steps (Culicover and Jackendoff 2005; Pinker 1995; Pinker and Bloom 1990). This thought is captured by the notion of a "lineage explanation" (Calcott 2009). Lineage explanations must satisfy two constraints. First, the path through phenotypic space from one trait to another must proceed via steps that vary only in minor ways. Second, none of those steps can be blocked by selection. The challenge for selectionists is to show how this is possible in the case of language. More recently, the scope of selectionism has been expanded—by differing degrees—to encompass the role of culture (e.g. Christiansen and Chater 2016; Planer and Sterelny 2020; Tomasello 2005; 2009; 2010; 2014).

Saltationism is particular to genetic accounts of language evolution; all cultural evolutionary accounts of language are selectionist accounts. It follows that evidence counting against saltationism counts against one of the two alternatives to cognitive gadgets theory. And the neuroarchaeology we have just reviewed offers evidence of this kind. The reason for this should be clear. If language was able to co-opt pre-existing capacities then we do not have to posit the sudden appearance of language specific mechanisms. Saltationism about the evolution of language thus looks unnecessary.

And, more broadly speaking, this is good news. In evolutionary biology there are reasons to doubt the plausibility of an account that requires the undirected, *de novo* generation of a domain specific cognitive mechanism (Fisher and Bennett 1999). Moreover, this general line of critique has been applied specifically in the case of Berwick & Chomsky's work (Planer 2017a). The neuroarchaeological evidence points us towards a selectionist account of language evolution that avoids these problems.

4.2.2 Domain specific vs domain general mechanisms

Moreover, the neuroarchaeological evidence offers reasons to doubt not only the saltationist position, but also the genetic selectionist account. This is because it points the way to an understanding of language evolution that requires only domain general mechanisms, and hence jettisons the need to posit any language specific capacities.

Theorists of all stripes can agree that language production utilises many capacities that play a broader role within cognitive systems—for instance, memory, executive control and theory of mind. However, these capacities are not thought to be language specific; that is, they are not dedicated solely to language production. Strictly genetic accounts of language evolution typically commit themselves to an additional claim: that the ontogeny of language is guided by innate psychological processes that *are* dedicated solely to language (Hauser, Chomsky, and Fitch 2002). These domain specific mechanisms explain how children begin speaking despite the apparent lack of information in the social environment required to learn the specific languages they learn—the 'poverty of the stimulus' argument. Mechanisms dedicated to syntax production are a key—for some *the* key—innate language specific capacity that is required to overcome poverty of the stimulus concerns (Hauser, Chomsky, and Fitch 2002). Consequently, evidence that the evolution of syntax did not require domain specific mechanisms undermines genetic accounts of both the saltationist and selectionist variety.

And neuroarchaeology provides such evidence. As we have seen, late Acheulean toolmaking triggers activation in inferior frontal gyrus; an area already associated with language production and increasingly thought to play the role of a more general-purpose supramodal processor of hierarchically sequenced information (e.g. Koechlin and Jubault 2006; Fadiga, Craighero, and D'Ausilio 2009; Poldrack 2006). Taken together, this evidence suggests that Broca's area—traditionally thought to be the 'language' part of the brain—is in fact co-opted by a range of different goal-oriented tasks.

Furthermore, attempts have been made to draw a more concrete link between the hierarchical action processes involved in Acheulean toolmaking and those found in syntax (e.g. Planer and Sterelny 2020; Stout and Chaminade 2012). This adds extra detail to the tool-language co-evolutionary picture. The idea here is that the sophisticated action hierarchies evidenced by the late Acheulean indicate that its makers had the cognitive mechanisms required to produce sophisticated action hierarchies. If we then think of speech production in terms of sophisticated action hierarchies—that is, if we think in terms of linguistic pragmatics—then we have the building blocks of an account of syntax. This is an account that relies only on domain general mechanisms; namely, those co-opted by any complex hierarchical action sequence.

This removes an important cornerstone of genetic accounts of language evolution, insofar as we have a route to explaining syntax without appealing to language specific processes. And this is good news for the cognitive gadgets account.

4.2.3 Cultural evolution vs gene-culture coevolution

I have so far talked in terms of language behaviours co-opting toolmaking behaviours. However, a properly *co*-evolutionary account of toolmaking and language acknowledges that the causal arrows go both ways. That is, making tools may have produced the cognitive mechanisms required for the emergence of language, but the emergence and spread of language also enhanced the cognitive mechanisms required to make tools. Moreover, as populations of hominins adapted to an increasingly language rich cultural environment, increasingly language specific and genetically transmitted cognitive mechanisms may have evolved.

There is a family of views that emphasise this scenario. Proponents of these views might agree with all that has been said so far, yet will nonetheless reject the cognitive gadgets account of language. I want to finish by distinguishing these two positions.

Recently, Planer & Sterelny (2020) have developed a selectionist view of the kind just described. Importantly, they maintain that the hierarchical cognitive capacities required to make use of rudimentary syntax long pre-date the appearance of modern humans. They argue that those capacities were likely present, in rudimentary forms, in the last common ancestor with the chimpanzee. Consequently, they reject the claim that explaining the early evolutionary stages of language requires positing the emergence of syntax specific capacities. But this does not rule out, of course, the scenario in which genetically endowed language specific capacities evolve in response to the new cultural niche; namely language. So Planer & Sterelny and Heyes can agree on the early stages of the story, but will disagree on the later stages—they can agree on the phylogeny but will disagree on the ontogeny. Each will emphasise the importance of culture and the need for only domain general mechanisms in the early evolution of language, yet will diverge on whether we find genetically inherited, language specific mechanisms in the development of language in modern day human children.

How do we decide between these two accounts? The question here is a broader one that faces the cognitive gadgets theory. If a cognitive gadget is successful, and if it aids the reproductive fitness of its bearer, then why wouldn't cognitive gadgets be genetically assimilated (Henrich 2015)? Cognitive gadgets are dependent on experience for development, but if selection were to favour mutations that reduce that dependence, then presumably those gadgets would become instincts. Heyes' finds little evidence for genetic assimilation, and suggests this may be because the regularities in cultural environments that cognitive gadgets track move too fast for gadgets to be assimilated (Heyes 2018, 207-210; see also Heyes, Chater, and Dwyer 2020). The idea here is that gadgets have to be "nimble", such that they can adjust to shifting social conventions and technological complexes. Given this, the environment that gadgets need to track is never stable enough for them be assimilated.

Picking between cultural evolutionary and gene-culture coevolutionary accounts of language will require answering complicated questions regarding how "nimble" the language instinct is, and assessing whether conventions in linguistic environments are in fact as variable as Heyes suggests (see Brown (2021) for a general critique of the claim that gadgets are nimble). This will be an important focus of future research.

In sum, work in experimental neuroarchaeology provides a plausible line of evidence to the phylogenetic development of language. For the most part, the implications of this evidence align well with the commitments of the cognitive gadgets account of language. Specifically, neural overlap between tool-making and language supports both a commitment to the importance of the cultural environment in producing the cognitive prerequisites for language, and a commitment to the idea that the early evolution of language was the product of domain general processes. Choosing between cultural evolution accounts and gene-culture coevolution accounts is trickier, and hinges on Heyes' claim that cultural environments are too unstable for cognitive gadgets to be genetically assimilated. More work is needed to assess whether this claim holds up.

5. Closing remarks

I have surveyed some general considerations regarding how to interpret the record using the cognitive gadgets theory, and conversely how the record might be used provide evidential support for cognitive gadgets. This follows Heyes' prescription that good human evolutionary theory should use forces to explain chronology, and chronology as evidence of forces. I want to finish by proposing a line of thought in the latter vein, but on a much broader scale.

The record potentially provides testable evidence for the cognitive gadgets hypothesis in the following way. On the traditional view, we have mechanisms produced by slow, stable processes (biological evolution) generating artefacts via fast, unstable processes (cultural evolution). On the cognitive gadgets view, we have mechanisms produced by fast unstable processes (cultural evolution) generating artefacts via fast, unstable processes (cultural evolution). Should we expect the record to look different according to these two hypotheses? If so, then broad patterns in the archaeological record itself might help decide between the two alternatives.

Here is one line of thought. The slow, stable processes of biological evolution would produce slow, stable patterns in the record. On the other hand, the faster, more unstable processes that underpin the cognitive gadgets view would produce faster, more erratic patterns in the record. So we potentially have testable predictions between the two hypotheses. Now, as we have seen, there is active debate concerning what patterns there are in the record, regardless of these predictions. Assessing cultural evolutionary psychology in this way is thus no simple task. However, as a preliminary step it would be interesting to see what modelling the two alternatives predicts of the record.

My overall goal in this chapter has been to progress the conversation between cognitive archaeology and cultural evolutionary psychology. The cognitive gadgets hypothesis is poised to become a key player in debates about human evolution; it is thus important that cognitive archaeologists engage with the framework. I hope to have shown that, although there are methodological hurdles to be negotiated, there is a bright future for collaborations between the two disciplines.

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Statement of Contribution

This thesis is submitted as a Thesis by Compilation in accordance with https://policies.anu.edu.au/ppl/document/ANUP_003405

I declare that the research presented in this Thesis represents original work that I carried out during my candidature at the Australian National University, except for contributions to multi-author papers incorporated in the Thesis where my contributions are specified in this Statement of Contribution.

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CONCLUSION

Closing Remarks and Future Directions

1. Philosophy of biology and cognitive archaeology

A traditional bugbear of philosophers of biology was that principles developed in the philosophy of "science" were really principles concerning the philosophy of *physics*. This situation has changed considerably as the field has become a more prominent part of the philosophical landscape. And it is good to see that no such parochialism has extended to the domain of philosophy of "biology", which has enthusiastically encompassed disciplines ranging from anthropology, psychology, economics and, of course, archaeology. The field is expanding in fascinating ways, and there is a lot of important research on the horizon (see below!).

Cognitive archaeology is a niche field in its infancy. The great promise of the discipline—a reliable chronology of human cognitive evolution, and explanations of the dynamics involved—is unfortunately matched by its great challenges. How do we integrate theories and concepts across such disparate disciplines as archaeology, cognitive science and evolutionary biology? And how do we overcome the methodological challenge of running inferences to the deep past? Over the course of this thesis, I hope to have shown that philosophers of biology can play an important role in overcoming these challenges.

2. Further research

I have a range of further research projects that have developed from this thesis, many of them collaborative.

With Anton Killin, I am developing a paper that synthesises our work so far on heuristics for theory choice in cognitive archaeology (Chapters 2 & 3). This paper will further develop our defence of leveraging multiple lines of evidence, theoretical pluralism, and cross-cultural sampling in order to bolster the strength of cognitive archaeological inference. We will also build in concerns raised by the current replication crisis in the psychological and behavioural sciences.

With Sam Lin and Alex Mackay (two archaeologists from the University of Wollongong), I am developing a paper examining the use of parsimony reasoning in archaeological inference. Often, archaeologists will argue that one explanation should be preferred over another in virtue of its simplicity; however, we aim to show that there is no consistent metric for "simplicity" at play in these debates. Furthermore, given that the archaeological record is the product of multiple causal inputs, it is unclear what parsimony reasoning offers in the first place. Rather, we should really be aiming for good inferences to the best explanation.

With Ron Planer, I am developing a paper stemming from the work of Chapter 1. Our idea is to apply some of the distinctions being developed in the philosophy of causation literature to debates about archaeological explanation. For instance, there is debate about the cause of the Upper Palaeolithic: some opt for cognitive explanations, others for demographic explanations, and still others for environmental explanations. We plan to show that these various claims can be made clearer by applying causal notions like *background conditions*, *difference making cause*, and *actual difference making cause*.

With Rachael Brown I am developing a follow up paper to Chapter 4. We finished that Chapter with a very cursory overview of our positive proposal, and the aim of this new paper is to further develop our view. We want to draw out a robust middle position between those who argue that the main driver of cumulative technological culture is cognitive, and those who appeal to high-level cultural forces.

My next solo research project will look at the use of recapitulation in theorising about human cognitive evolution. Michael Tomasello, for instance, frequently uses observations regarding stages in the ontogenetic development of children to make claims about the phylogenetic development of the species. However, recapitulation is a controversial concept in evolutionary biology, and the challenges to its viability are only compounded once it is moved to the domain of cognitive evolution. So is recapitulationist reasoning a reliable mode of inference to the past? Ultimately I would like to show that it is; principally because it offers us a unique line of evidence to the human past, and one that does not suffer the same problems faced by either traditional cognitive archaeology or experimental cognitive archaeology. But whether this will work remains to be seen.

Another project I am keen to develop begins with a simple observation. Cumulative technological culture, and its effects on human cognitive evolution, ratchets up over time. This means that inferences we run to the earlier stages of our cognitive evolution are more likely to pick out genetically determined traits, whereas those that run to later stages of human evolution are more likely to pick out culturally built traits. Potentially then, by producing an overview of research on the capacities of the Australopithecines and early Homo variants, and comparing this to Erectine and Heidelbergensian hominins, we can tease apart the products of gene-based evolution from the products of gene-culture co-evolution. This project may also shed some light on various evolutionary frameworks discussed in this thesis, such as Heyes' gadgets theory or Sterelny's take on niche construction.

Finally, with Adrian Currie, Anton Killin, and Andra Meneganzin I am developing a paper on the philosophy of cognitive archaeology for the journal *Philosophy Compass*. This journal publishes overview articles, which are aimed at giving a broader philosophical audience insight into the current state of some sub-field. I take this as a good sign that philosophy of cognitive archaeology is gaining traction!