BEING A METHODOLOGICAL OMNIVORE

Interview with Adrian Currie, Senior Lecturer in the Department of Sociology, Philosophy and Anthropology at the University of Exeter

(Episode 5, May 7, 2021)

Welcome to 'Sciences of the Origin' where we delve into the philosophical and methodological foundations of the scientific quest for the origins of the universe, life, and mind. The main aim of this project is to discuss common methodological challenges of cosmology, biology, and archaeology. The ‘Sciences of the Origin’ interviews are supported by the University of Oxford project ‘New Horizons for Science and Religion in Central and Eastern Europe' funded by John Templeton Foundation.

We bring you an interview with philosopher of science Adrian Currie, Senior Lecturer in the Department of Sociology, Philosophy and Anthropology at the University of Exeter. His research interests include philosophy of biology and historical sciences, as well as social epistemology and the quest for creativity in knowledge production. He is one of the founders of ‘The Extinct', a blog covering the intersection between philosophy and palaeontology. Adrian Currie is the author of ‘Rock, Bone, and Ruin: An Optimist's Guide to the Historical Sciences' (2018), ‘Scientific Knowledge and the Deep Past: History Matters' (2019), and ‘Comparative Thinking in Biology' (2021). The interview is hosted by Monika Milosavljević, Assistant Professor of Archaeology at the University of Belgrade.

Monika Milosavljević: Hello, Adrian. Thank you for being with us today.

Adrian Currie: Hi, Monika. Thank you so much for having me.

MM: It's great having you here. I would like to start by asking maybe very general questions. Why are some important questions only answerable from the perspective of deep science?

AC: That's a great question. So maybe the way to start answering that is to get to grips with how incredibly unrepresentative this little time slice is—this minutia of time that we currently live in. If you think about climate science, we've had good records of global temperatures for, what, maybe a century roughly? And if you think about the enormous variety in global temperatures over millions of years, a century's information is just not going to be good enough to get to grips with how varied things are. If you were to go around and look at global biota today, at large animals, for instance, you would think—oh, that's kind of interesting that all of the big animals seem to live in Africa and Western Asia. Where are all the big animals in America or Australia or Europe? And the answer, of course, is that many of them went extinct towards the end of the Pleistocene.

If you just take the way the world is now as a guide to the way the world has always been, you are going to massively misrepresent and misunderstand the way that the world is. The way that things are now is a result of the way that things were in the past. So if you want to understand the way things are now, you need to understand the past as well. Another more social, moral, or political aspect is that the past is often used as a way of shaping the way we see the world now. The past is a kind of furniture that we live in. I'm a colonial product of Europeans moving to New Zealand in the 19th century and that's the thing that sort of shapes my self-conception but it's also something that can be used to control me or to manipulate me in various kinds of ways. And caring about what actually happened in the past, caring about the truth of the past gives us, in a sense, a kind of bulwark against the use of history to manipulate people.

MM: Well said. We do have some problems with the past and sciences of the past, so let's discuss that a little bit. If scientists are hostages to their own theoretical ideas, and if trace-based reasoning is vulnerable, is there room for optimism when we are studying historical sciences?

AC: Well, I certainly think there is, but I want to go through each three parts of that question and try and give my view on them. First, of course, any sort of scientific investigation is going to be deeply theory-laden in various ways. No one thinks in any area of investigation that, somehow, the world just comes to me for free and goes 'here is my data'. Lots of folks, particularly those in archaeology, are deeply worried that because the remnants you get from the past are so degraded, there aren't going to be many constraints on our interpretation. That my pre-existing ideas, my sort of theories, are going to be in the driving seat. And I think that the ways that archaeologists, in particular, respond to this should give us a reason for some kind of optimism.

So, what archaeologists do, in my view, is three things. The first is a kind of reflective practice. There are lots of archaeologists, whether they're kind of archaeologists who fall more on the science-y side of the spectrum or more on the humanities or art side of the spectrum, who are all continually reflecting on the ways in which their pre-existing ideas might be leading their interpretation. There's also a social aspect to it, a sort of inter-subjectivity aspect; archaeologists are very aware that it's not their interpretation standing on its own that does the work, but rather a community of people with a community of interpretations, and them being engaged with each other. And where I think the rubber really hits the road, and this is a lesson I take from Alison Wylie's work, is that the materiality makes an enormous difference. I think that Wylie is right when she says that archaeologists aren't really led by theory but by materials; what you're doing when you're an archaeologist is you're engaging with this material record, which just won't bend any which way. The material record can be extremely stubborn; there are many examples of people trying to get the material record to fit their pre-existing ideas and the material record flatly refusing. And so, I do think that there is a lot of room for optimism when that kind of three-fold strategy is in play. I think it's a mistake to think that the way that we understand historical science, the way we understand investigations of the deep past, is in terms of there being these overriding theories. Things are actually a patchwork. Things are messy. And that's part of their power.

In terms of optimism—the title of my book has the word optimism in it and there's something very odd about that. What am I talking about? Well, on the one hand, it's doing two bits of work for me. One is I'm trying to change the conversation. When people think about the philosophy of science generally, they ask this question, 'Should I believe what scientists say?', 'Do scientists give us true stuff?'. And that's a very good question; that's a question we should pay attention to. But that's not the only question we should care about. In the cases with things like palaeontology and archaeology, one question you might ask is, 'Should I believe that', say, 'the dinosaurs were knocked out by a giant rock?', or 'Should I believe that the reason that the megafauna in the Pleistocene was wiped out was because modern humans turned up and ate them?' Are those hypotheses I should believe or not? Optimism is supposed to lead us away from asking about whether we are getting the actual knowledge of the past and towards asking after a much broader array of epistemic goods. I think there are lots of examples where historical sciences are chasing a hypothesis, which is very unlikely to be true; nonetheless, chasing that hypothesis generates a whole bunch of surprising interesting knowledge about a whole variety of different things. I think that there's been this very narrow focus, particularly in philosophy of science, but also more generally when folks think about the 'the epistemic credentials', whether there are good science questions about things like palaeontology and archaeology. They focus on 'Does it tell me what actually happens in the past?' And sometimes, yes, it does that. But focusing narrowly on that question means that we miss this beautiful array of other kinds of knowledge. Often this knowledge takes the form of, for instance, capacities: we've learned that this is a type of social arrangement that could exist; we've learned that under certain conditions, this is how the environment and, say, the atmosphere might interact. It gives us modal knowledge, knowledge of how things could be. Maybe I'm weird, but I tend to find that knowledge more exciting than what happens to happen on this one little timeline that we're following.

MM: Yeah, I like the plurality of knowledge that archaeology can give us and also that material culture and context can surprise us. But I want to ask, is there any difference in

how we should treat different traces? For example, material culture, written record, animal remains, fossils? Is there any difference in epistemological terms between those?

AC: What a great question. I guess it depends a little bit on how abstract we're asking the question. I can easily give you a framework that can make them all look the same. That framework might be what Anton Killin and I have called 'trace-based reasoning', which I think you mentioned earlier, which is just—I've got an object, I've got a bit of knowledge, a regularity that tells me about how that object would change through the past, and then I infer backwards. So I have a bit of historical text, I have my historical knowledge about how it is that that text may be produced in that context, what would lead to it to be developed, and I use that bit of theory to infer from the past. Insert whatever trace you want, that's going to kind of fit. But, of course, that's pretty abstract. I suppose my view on this is a bit like: there are lots and lots of differences, but I don't think there are any special differences.

Let me explain that. There's this beautiful discussion that I find myself keep going back to in Collingwood, of all people, where he says, 'Look, there's just a fundamental difference between what a palaeontologist is doing and what an archaeologist is doing'. When palaeontologists are digging up remains, they're doing what they think of as a kind of science. They are thinking of these things as the remains of biological creatures. They're thinking about them in causal terms. An archaeologist is digging up materials that they're thinking of as intentional. They're digging up materials that humans did stuff with, humans with purposes. And for someone like Collingwood, that's a profound difference. Now, I agree, but of course, there's a difference between conceiving of what I'm digging up as being the remains of an extinct organism and conceiving of what I'm digging up as an artefact that was put to some use by some previous human. And that makes some epistemic difference. But I'm not sure whether that epistemic difference is supposed to be more important or more interesting than, for instance, a palaeontologist who is digging up an invertebrate fossil versus a palaeontologist who's digging up a vertebrate fossil because invertebrate fossils, particularly aquatic ones, we should think

to fossilize more often than vertebrate ones. That's going to make a big difference to the way that I treat that data, the kinds of inferences I'm interested in making, the kind of targets I might have. There's this kind of idea that runs through quite a lot of the philosophy of history that says something like 'something special happens once intentionality is around, once you've got these humans around'. And I agree that humans make things in some ways more complicated. It's just not obvious to me that they make them specially more complicated.

MM: I agree, but I'm also thinking about the intentionality of animals, and that's an interesting question for me, how do we recognize that in human-animal relationships. So that's something to think about, but maybe another time. So, you came up with the great phrase 'methodological omnivore strategy'. How can the sciences of the deep past progress by using that strategy?

AC: It's important to have catchy labels and 'methodological omnivore' is a mouthful, but it has a certain evocativeness to it. Here's the kind of way I was thinking when I was developing this model—there are lots of sciences, like particle physics or certain forms of experimental lab biology, particularly that working on model organisms, where it seems that they've struck a very, very deep vein of knowledge. They've got a really productive kind of strategy—if I sit here with this E. coli, I've got a whole bunch of different, really effective techniques that I can use to generate a lot of knowledge from this little experimental system. You might think of that as a kind of methodological obligate. An obligate carnivore is something like a polar bear. Polar bears eat seals and that's pretty much it. They're very, very good at it. If you want something to kill a seal, the polar bear is your guy. So, the thing that's going to make you a sort of obligate is this kind of focus—a small set of very effective tools and techniques.

Now, if you're trying to understand the deep past in many contexts, not necessarily all, it doesn't look like that strategy is going to work, because you've got such a degraded, heterogeneous, messy set of data, and, for that matter, a messy system that you're trying to understand. The past is very rarely simple and easy. And so, what you want to do is throw everything at the wall you can and see what sticks. That's the kind of strategy. So

you behave more like a brown bear. Brown bears are omnivorous. What they do is eat whatever on earth they can. If you're a brown bear, you're like, 'Alright, winter's coming soon, I'm gonna have to hibernate, I just need to maximize my calorie intake'. There are some real differences between the sorts of standards you should do if you're that type of scientist. So the obligate, like the polar bear, is going for the best possible, highest quality calories. Whereas, the brown bear is like 'Just give me whatever calories you can'. And that is a good strategy, that is justified because brown bears and polar bears live in such different contexts. Similarly, if you are trying to understand the lifeways of long-extinct animals, or if you're trying to understand the way that, say, human cultures developed thousands of years ago, you are much more in a sort of brown bear scenario. You need to take in and integrate as much evidence as you can. The thing that I think makes this notion of a methodological omnivore distinctive—because you could just say, 'Sure, you're just talking about the convergence of evidence. As in, we need a lot of consilience in order to do this'—the thing that I am really struck by when I look at palaeontologists and archaeologists, is the extent to which they're not merely in the business of finding evidence and integrating it. They're in the business of creating these highly attuned, really flexible tools that they're able to adapt to particular situations.

Across lots of sciences, it's very rare to find someone, for instance, doing a master's thesis and that master's thesis being a technological or methodological innovation. Usually, when you're doing your little master's thesis in a lab, the head of the lab is just like, 'Hey, generate this data, do some stuff'. Whereas it's quite often in my experience that you find palaeontologists and archaeologists, even at the level of master's thesis, being like, 'I'm developing a new computer model to do this', 'I'm trying to see what happens if I do this with the CT scanner', 'I'm trying to build this weird thing'. So, it's a much more creative and open-ended way of knowledge creation. And I think why they do it like that is intimately connected to the kind of epistemic situation they're in. When you're in this apparently impoverished situation, then you need to be very creative in order to get anywhere with that knowledge. And that's what makes the science so exciting to me.

MM: That's great. So as a part of that story, you said that underdetermination, which is the reason why most of us are pessimistic, improperly frames the challenges of historical sciences. So can you explain your view on underdetermination?

AC: I can try. This is something that is developing in an ongoing way and I'm pretty sure I contradict myself in various papers about this. So I'll do my best to summarize one version of what I think. The underdetermination we're interested in here is what Derek Turner has called local underdetermination. Often when philosophers of science talk about underdetermination, they're interested in a question which I consider pretty boring, which is something like, 'Hey, for any collection of evidence, in principle, there can be a hypothesis other than the one that's going that could equally accommodate that evidence'. Sure, maybe in principle that's true but it doesn't tell me very much about the practice of scientific knowledge. The type of underdetermination that we're interested in this local time is one that says, 'Well, given the available evidence that we have, and given the plausible hypotheses'—whatever plausible means—'we have on the table, this evidence is not enough to distinguish between those hypotheses, it speaks equally to both of them'. That's a very common situation that paleontologists find themselves in. They may disagree on whether or not it speaks equally but often things are not closed in that sense—they're open. And, indeed, that's not surprising because if it wasn't underdetermined, we would know the answer.

The thing that really matters here is—should we expect underdetermination to be solved downstream? Should we think that it's a kind of robust underdetermination where no matter what we try, no matter how hard we try, we're not going to get anywhere with it, or should we think that it's something that will be resolved. Now, one thing to immediately say is that it's generally a slightly weird question to pose because surely, there are going to be cases of underdetermination where I should be very optimistic that we'll find some kind of answer to it. For instance, 15 years ago, had you asked me will we ever know whether Neanderthal or Homo sapiens interbred, I would have said 'I don't know, I'm not sure', or 'Maybe not. I can't imagine'. Go 5-10 years in the future, and if someone's like, 'Hey, there's this thing called ancient DNA and we're able to sequence it.

Do you think we ever find out whether the Neanderthals and Homo sapiens interbred?', I'd be like, 'Yes, I can see that now'. So, context is going to matter enormously.

Picking up on the sort of question you asked, which was why do I think that underdetermination is a wrong way of thinking about things, it is because I just think that underdetermination leads us to a certain type of impasse. Because the more you think about scientific practice and the actual processes of how scientists generate knowledge, the more complex and intractable become the bits about whether something is going to remain undetermined or not. It isn't simply that you have to consider whether or not the information was preserved across paleontological time, or whether the archaeologists have the right background knowledge to be able to make those inferences—it's also how they control the data, what access do they have to the data, how are the data travelled, what sort of publishing contexts do they have, what kind of resources do they have at their commands to help them generate this knowledge. When you think about science as this kind of social-economic, political entity, then the question that says 'Should we expect underdetermination to be overcome or not?' starts to be incredibly intractable.

And so, what I think we should do instead is think about the kinds of strategies that scientists adopt given the resources at their commands. Different scientists have different 'affordances', some of them have access to different kinds of things and they're interested in different types of systems, and these things challenge them in different ways. And so when I'm asking after the success of a science, if I'm saying 'Should I be optimistic about the science?', the question should not be 'Should we expect this particular bit of knowledge that's currently underdetermined to stop being undetermined?', but rather 'Given the sort of scenarios and the challenges they're facing, is this the right strategy? Is this a productive way of doing science?' Now, if so, that does let us be optimistic in a certain kind of way. Perhaps I shouldn't be optimistic about whether a particular bit of knowledge will be generated, but I should be optimistic that lots of knowledge will be generated. And I take that part of what's supposed to be exciting about this is precisely that I don't know what that could end up being. I don't

think you could go back 20, 25 years ago and look at, for instance, the state of paleoanthropology—the science into the evolution of how Homo sapiens arose—and possibly predict the way things look now; they're just so different. And I think that's what's so exciting about this stuff.

MM: Yeah, sciences are evolving and that's great. But I also need to ask about non-replicability, because I often get the comment: 'Your science, archaeology, is not real science because of non-replicability'. And I don't know how to answer that, so please help me.

AC: Here's an extremely philosophical beginning to a response. What do we mean by replicability here? Because palaeontologists do have a really interesting notion of replicability. In a recent letter, the Society of Vertebrate Paleontology argued that it was really important to have publicly-owned, well-controlled fossils because it's important for people to be able to go and look at the original specimen. Why? Because that's a form of replicability. They are thinking of replicability in terms of 'one paleontologist has gone and done an analysis of the specimen and then they've written it up'. Now, to replicate that, another paleontologist needs to go and look at the specimen and then write that up, which is a very different notion of replicability than what we see in, basically, statistics. So replicability is more or less a notion that comes out of the combination of experiments combined with statistics. And there's a lot of really interesting work at the moment about the nature of replicability, even in those sciences.

So that's one response, that there are notions of replicability which are suitable to the sorts of sciences that are based on specimens. Another thing that might be lying behind this notion of replicability could be the idea that maybe the things that we're interested in understanding in the past are 'unique'. Maybe, in some sense, the data isn't replicable because the objects aren't replicable. They are the contingent outputs of this highly heterogeneous, contingent set of forces in the world. That makes things hard. But to say that something somehow doesn't count as scientific because it's not meeting a particular kind of standard, I think is, to put it bluntly, extremely naïve about the nature of science. There are not really any philosophers of science that I know of that take

seriously the idea that there's such a thing as the scientific method. And if you go on the internet and google the scientific method, you'll find lots of cute flow diagrams, and so forth. And those are very abstract and very nice, but calling on what I was talking about earlier, when we're thinking about what science is and how science works, you can't just grab some one-size-fits-all abstract idea about what good knowledge production looks like, because knowledge production is incredibly sensitive to context. What's a great thing to do for someone who is an experimental scientist working in a lab would be a completely fruitless thing for a paleontologist to do and vice versa. Here's one way of putting it: given that often replicability is not a thing that historical scientists are able to do, I think it speaks to how impressive their knowledge-producing capacities are when we get such rich and often highly plausible pictures of how the past was.

MM: Thank you for saying that. That's extremely important for my responses to that question in the future. However, when researching origins of the mind, for example, or cultural phenomena in the Palaeolithic, or art in prehistory, is there a danger of the selection effect or any archaeological biases there?

AC: Well, of course, they're always there, but you might think that certain questions are especially prone to these sorts of problems. And I take it that the things that are going to be the real sort of trouble spot are cases that, first, don't get many constraints from the evidence that we have, meaning things that are very open; and second, things that matter to us in particular ways, that are politically or culturally or emotionally charged, because those are the places where you're gonna get real conflicts between interpretations. And, of course, it's impossible to tell the history of the study of human origins without telling the story of colonialism and racism and imperialism. These things are directly tied into one another and you can't explain why things weaned as they did without paying attention to that. And I suppose one thing is to sort of hail back to what I said earlier on in our conversation about archaeology and the way that archaeologists respond to these problems of interpretation, the sort of reflexivity of really being conscious of the fact that you're inevitably going to be biased. You can't get rid of those biases because, of course, that's what you're often bringing to your science. Why are you

doing the science that you're doing? Why am I interested in these questions? Why is it that if I discover like some tiny little hominid that existed 100,000 years ago, that gets me the front page of 'Nature', whereas me discovering something that seems equally important gets a much smaller publication? Because we care about humans. We care about ourselves. We think we're special. And so, anytime we're doing a sort of science of humanity, we need to be careful.

This is maybe easy to say from the privilege of both being someone who's not very vulnerable but also someone who is a philosopher and can sit from the sidelines while the scientists do their thing, but there's a sense in which it can be very difficult to recognize the ways in which these prejudices are affecting and shaping our science. So, in the 1950s, when people were assuming that adopting a kind of hunting thing and eating meat was critically important for the evolution of human culture, I don't think if you'd asked them, 'You don't think that the fact that you're living in a highly masculinized culture that sort of has these things isn't affecting the way you're thinking now?', they'd say, 'I don't think so'. I imagine that lots of these things are going on now and I can't wait for it to be 40 years from now, and to have, hopefully, found out some more of that. I've found that, to speak personally, my life has been this slightly annoying unfolding of all of the various ways in which I've been unable to realize things that are very obvious in retrospect. And you might think that with things like the understanding of human evolution at a social level, it will probably look really similar.

MM: That will be interesting to see. Another thing that worries me is public archaeology. How should scientists and philosophers treat the ambiguity of evidence and how do we interpret, for example, origins to the public? Should it be something simple? Or should we say, 'Oh, it's complex', 'That is the paradoxical nature of archaeological evidence', 'I'm not sure', and so on? What do you think?

AC: I think that scientists are, generally speaking, in this fascinating bind when they're communicating with the public. Part of that has to do with having conflicting aims, and part of that has to do with having conflicting audiences. On the one hand, you want to defend the authority of science, and certain types of communities or audiences. The way

that you defend the authority of science is by making science sound like it's some God-like being that comes down claiming: 'Science says that this thing!' and you go, 'Okay, science says that that thing'. In a sense, it means that the public is supposed to shut up and take a non-critical attitude and just accept what science says. If I want everyone to wear masks, it would be super useful if I could just say the big person in the lab coat in the sky has told us that we all have to wear masks. The problem with this strategy, of course, is that that's not what science is like. And as soon as the public sees that, suddenly it looks like this kind of 'Wizard of Oz' scenario where there's just been this weird little old man behind the screen the whole time. And that's really bad! On the other hand, if you try and communicate the complexity of science, that can lead to kinds of 'postmodernist narratives', but postmodern in the derogatory rather than the literal sense, which go like, 'You know, knowledge is relative', 'You can believe what you want, or rather, what I believe is as epistemically credentialed as what the person in the lab coat is saying', and so on. And so I think acts of science communication are generally incredibly fraught because of this relationship between multiple evidence, multiple audiences, and power. And in terms of archaeology and the science of origins in particular, there are a couple of things to say and I don't think I have any real answers.

For what it's worth, my general answer is—educate the public, so they can think critically. Science education is not simply an education in the content of science; it's an education in the philosophy of science. And sociology of science, anthropology of science, and history of science. These are things that are required for you to have the right sort of critical attitude, one that isn't merely sceptical. There's denying science and then there's taking science at its word, and then there's somewhere in the middle where you're able to take a critical attitude and think about science more carefully. And I think that many people can't do that.

MM: Can we do that by storytelling?

AC: Yes! If we're talking about it in a pedagogical way, I think there are lots of really interesting ways of teaching science to students. Like material engagement, getting them to really think about how you would try and figure something out, as well as telling

stories about science. So I think storytelling can be a really good way of developing these lessons.

I'll give you an example. Derek Turner and I have had papers back and forth where we've talked a little bit about this display of a Tyrannosaurus Rex fighting a Triceratops, I've forgotten the museum. And this is a sort of classic example of something which I don't think any paleontologist really thinks would happen, that Tyrannosaurus Rex would lock itself in combat with the Triceratops. There are much easier things to eat. Those things [Triceratops] are nasty; it's just a weird thing for a Tyrannosaurus Rex to do, let's put it that way. Moreover, it feeds into a dual, a lightsaber fight; it's got this real cultural resonance to it. And so it seems like this is a striking image, not because Triceratops and T-Rex went into combat all the time, but because it speaks to certain types of cultural tropes that we have. I could imagine a way that a museum exhibit could fix that by engaging the audience about it. You could imagine having the T-Rex and the Triceratops there, and then having a picture of Luke Skywalker fighting Darth Vader, and then a picture of a shootout, and going, 'Notice how these kind of looks similar?'

Another example that is maybe more direct is this theory about how it is that Tyrannosaurus Rexes would have killed Triceratopses. This, so far as I know, still hasn't been published as a peer review, but it's been used to do some museum displays. And the worry is, 'Hang on! This is way too speculative. What role could speculation play in the public? The public is gonna come away with a false idea about how confident we are about what happened in the past'. And again, I think that there's a balance to be struck here by going, 'Hey, here is the evidence that underwrites this reconstruction. What do you think? Do you think this is relevant? Here's what someone else thinks'. It's all about shifting the public from seeing themselves as passive containers that are getting content and instead becoming engaged with science and questioning it. Yeah, that's what I'll say about that.

MM: And when we are there, can you tell us about the role of speculation in scientific interpretations? Why is it important?

AC: Speculation is this really weird word because when I try and publish on speculation, maybe 25-35 percent of the audience or referees will sort of go, 'No, no! Speculation is a derogatory term. Speculation just means bad science or a certain type of bad science. If you're speculating, you're going beyond your evidence in a problematic way'. And I think this is an interesting kind of linguistic quirk where some have associated speculation with just this negative thing. I happen to think that that plays a pretty nasty role in science because it's often a form of gatekeeping. I say 'Oh, this science is speculation', which is a way of me saying, 'I'm gonna dismiss it. I'm gonna not pay attention to it'. Maybe we need words that play that role, but I think that particular sciences, like paleontology and archaeology, need to be speculative in a certain type of way. They need to be speculative for at least two reasons. One is because when you don't have material constraints, when you don't have huge amounts of empirical data to play with, you really need to make bold bets. You need to stretch beyond what you've got. And that doesn't mean that this stretching beyond is directly giving me the true hypothesis. What it's doing is giving me the sort of framework or the backdrop that's required to start developing a more plausible story. You're sort of making an initial stab, and that initial stab is critically important for you to be able to start building evidence around it. And it shifts and changes as you go through.

Another is going back to what I said previously about when we're doing science, for instance, historical science, that we don't just care about what actually happened. It's often a science of capacities—a science of what could happen. And, if you want to understand these 'modal spaces', if you want to understand what's possible in addition to what's actual, then you do need to speculate. You need to say things that sort of go beyond the way things actually are. And I think it's a real shame that there isn't an explicit discussion of the methodology of speculation within science. If I'm right and scientific speculation is really important, then why is it that we have these very particular standards for assertability when we're in the justification rooms? If I want to say that, 'I've got this level of certainty that this thing is this old', or when I got to have a certain level of statistical significance or things have to be replicable, these are set standards

that help guide when something is going to work or not. But with speculation, so far as I can tell, there's nothing like that. There's no attempt to even think about what the heuristics might be for doing that kind of science, at least within science. I tend to think that if we go along with Adrian in thinking that speculation is really important, we need to think about how we can make a place for it within scientific institutions and scientific structures. In addition to your research paper and your review paper, could you have your, 'Hey, I've got this weird idea' paper? Could there be journals for science that is explicitly speculative? Could there be guidelines and standards or maybe just heuristics for under what circumstances this kind of speculation can be done?

And maybe the last thing to sort of point out is, just as in the 'justification room', when I'm trying to work out whether I can assert that I'm confident that this thing is this old or whatever, there's an ethical and social aspect to it; there's a kind of research ethics. There also, I think, need to be a sort of research ethics of speculation, because, as it has been underriding some of your questions, it's very easy for certain things that scientists say to be taken out of context, to be used in really problematic and dangerous kinds of ways. And you might think that upping your speculation ups the possibility of that happening. And how it is that you control that, I don't really know. One general worry I have is that folks who are going to misuse science are going to misuse science no matter how careful you are. It's not obvious to me what scientists or even scientific institutions could do to stop people… They're just going to do it, they're going to misread whatever they need to. But, at the very least, I do think that it would be nice if people who were thinking about science in a speculative frame of mind would think about the ethical considerations just as when you're designing your experiment.

MM: Yeah, I would say that speculation is an integrative part of every archaeological thinking, as far as I know. And coming from Serbia, I know how speculation can be dangerous when used for, for example, nationalistic purposes. That's very close to me, so…

AC: Sorry, I just realized there was part of your question I really wanted to hit that I didn't, which was this reference to ambiguity, is that okay?

MM: Yeah, yeah.

AC: So, the person who I really like on ambiguity is the archaeologist Joan Gero. She points out that the way that archeological papers and scientific papers in general are often written is geared towards a kind of certainty; you only say the things that you have the evidence to say. And so, the way that you write the paper is both quite conservative, but also really geared towards saying—here is the positive evidence. Of course, that's not always true. There are exceptions to this rule. But generally speaking, that is how scientific papers are often constructed. What Gero wants to say is that we should do things differently and, what she calls, embrace ambiguity. Namely, instead of trying to maximize our certainty by saying, 'I'm really confident that this thing is 6,000 years old, and we can attach it to this culture', we should be embracing the ambiguity of 'Oh my, it's either, 6,000 or 6,500 years old', but in these interesting ways, where we start generating different hypotheses. To start exploring the different spaces that are open, given the evidence that we have. There is a beautiful example of how to embrace ambiguity in this wonderful book—I can't remember the authors' names, but I believe it's called 'All Yesterdays'. They are paleoartists who give us pictures of extinct animals and draw a bunch of dinosaurs that fit with the current evidence we have as much as how dinosaurs are currently presented, but look extremely different. And so you have these kinds of fat cow-like theropods and those sorts of things. It's a beautiful way of embracing ambiguity and also challenging the presuppositions that we have. So I suppose, another answer to how should we be communicating with the public about science is helping them to, on the one hand, embrace that ambiguity, understand that what's exciting about science isn't just what we know but the spaces that we don't know, and how fascinating that is, while allowing science to retain the authority that it often needs, which is kind of under threat. And I don't think those things are necessarily in conflict with one another.

MM: I agree. I think that Steve Fuller said that today biomedical science is viewed as a standard, like physics was in the 1960s. So what do you think about that? Because in contemporary archaeology we can see a predominance of DNA analysis, isotope

analysis, and it seems that we are losing the aspect of humanities. I may be wrong, but what is your opinion?

AC: I'll do the first bit quickly. I've said this a few times, but I'll just say it again. The idea that there should be one standard for scientific knowledge is to do a great injustice to the beautiful heterogeneous complexity of human minds and the nature of the world. I'm not against there being scientific standards, but I'm dead against there being general scientific standards. There is no one science you can point to and say that's the science.

The more interesting question you ask is more particular about the status of archaeology at the moment. It's increasingly the case that archaeologists rightly have been bringing on board the new scientific techniques to do with molecular data sequencing, ancient DNA, these kinds of features. And they should do that because they are methodological omnivores, they think 'This is a really powerful new stream of evidence, let's grab it and use it'. But often there are two problems. One is the danger of people thinking that that is the golden evidence, that is the evidence we need. And increasingly, you find archaeologists saying, 'Oh crap, I need to do some sequencing of something in here 'cause otherwise, I can't get my paper published'. As a side, philosophers occasionally will think like, 'Oh, I should put an equation in here, or some logic that'll make it look like it's proper philosophy'. This is a similar kind of signaling that's involved in trying to get your paper published. I think a deeper problem is that that process can change the question that you're asking. Alexandra Ion's got some nice work on this, where she shows that demographic questions are not the same as archeological questions. Archaeologists are traditionally anthropologists of the past; you're interested in understanding human culture in the past. Of course, demography, the movements of people is important for that, but that's not all it is. And so one of the dangers of having these powerful, sexy new technologies is that scientists, particularly when we're talking about publishing practices, start acting as if they're obligates rather than omnivores. They start acting as if they really want to be focused on this one kind of data. And not only does that massively undermine the strategy that you should take, given you're doing

this kind of science, it also manipulates the kinds of questions that you ask. And I think that's bad.

In the long-term, my hope is there's a kind of a rhythm to the introduction of new technologies. Bob Chapman and Alison Wylie in their book 'Evidential Reasoning in Archaeology', I think it is, have this really nice chapter on the introduction of carbon dating. And it's a familiar story, where there's this period where everyone's really excited about a thing. Then a period where everyone realizes it doesn't work as well as you thought. And then a period where it just gets integrated; it just becomes part of the furniture of doing that kind of research. A thing I would add is that it also becomes something that those scientists do. It's not something that has simply been exported from, say, physics. In the case of carbon dating, it rather becomes integrated and part of paleontological and archeological practice. Similarly, my sort of expectation, and this may speak to my optimism about things like molecular DNA data, is that there is going to be a flourishing period where folks are all excited about it, the technology is new. We can understand lots of cool new stuff with it, and that's going to push other questions to one side—but not for very long. Two things will happen. People will become more and more aware of the limitations of that data taken on its own. And people will become more and more annoyed that they have to talk about demography all the time, when they'd rather ask anthropological questions of the past, for instance. And so I suppose my prediction would be that sometime downstream, just like carbon dating, this is going to become a very run-of-the-mill, humdrum kind of thing that some archaeologists do, an irrelevant kind of thing. Now, that doesn't help archaeologists right now who want to be able to publish without making friends with someone who can do DNA sequencing or more importantly, archaeologists who aren't in the kinds of financial or institutional situations where they have access to that kind of thing. I don't really have a suggestion for them, but I have a suggestion for the rest of archaeology, which is to let people publish without that data because it's bad. (laughs)

MM: Thank you, let's hope your prediction comes true. I want to ask something from social epistemology. For example, how can we organize scientific communities,

particularly regarding scientific creativity, so that we include the messy field of archaeology, for example, or any other field?

AC: It's a really tricky question. And, I don't think there's going to be one answer. I suspect that one way that we can make that question slightly more manageable is to think about various kinds of ways in which scientific work gets funneled. That kind of decreases its creativity. Kyle Sanford has a beautiful paper where he goes through these things and lists them quite well. So, there're things about pedagogy—the way that scientists are taught leads them to all think in particular ways. There are things about the way that publishing works, with peer review, where there's me and I've got the other scientist who is trained the same way as me, who is going to decide whether my paper's good or not. So how should I write my paper? Well, I should write it such that my peer will like it, and that means that there's not going to be all that much room for creativity because I have to fit within that structure that has been bequeathed to us by our scientific pedagogy. Then there are things like publishing practices and publishing standards that are going to funnel scientific research down particular avenues, and it seems like intervening on those different things can be ways of opening up scientific research. One that is kind of obvious is grant structures. Something which I think makes it really hard to be a paleontologist or an archaeologist, depending on what sort of grant you're going for, is that the structure of scientific grants tends to be 'What's your hypothesis? How are you gonna test it? Why should we care?' If you want to go and dig up some stuff in a field somewhere because you think it might be cool, it's not clear what your hypothesis is, it's not clear what you're gonna find. And, also, the chances of you finding nothing are really high. (laughs) So, the criteria for getting grants, in addition to often being peer-reviewed, which means that you have to run it by people who think like you, tends to go towards sure bits, towards conservative things. And even when you have grants that are supposed to be kind of blue-sky thinking, that are supposed to be pushing the envelope, from what I've heard at least, and this is an empirical claim that I don't have any evidence for, it's rare that they manage to achieve these sorts of things.

Earlier, I was talking about how we haven't really thought about how to do good speculation in a scientific context. We thought a lot about how to tell whether a vaccine works; I think we should trust very well the sort of evidence we have because we've got a really well-developed way of tracking that stuff. We don't have a really well-developed way of tracking what's going to be a productive, speculative kind of science. So given that we don't have that, I'm open to that not being a thing one could have; it might be that it is so context-sensitive and weird that there isn't even a set of heuristics. But even if that's the case, I think the answer is going to be diversity of publishing practices, diversity of grant structures and grant applications. We do now begin to have, I think, diversity in publishing. Particularly in places like biology, you have various journals that have quite different criteria for when something will be published; and I think that's funky. I think for grants, as well, there should be diversification in the kinds of grants.

Shahar Avin has lots of great work on how maybe we should decide grants with lotteries. So the idea is that there are some minimal standards, so the application's like two pages long and you get a board to just make sure that none of the applicants are straight out trolling, and then for the rest of them, you just decide by lottery. And you imagine, if you were writing a grant under those conditions, you would do something very different than what you would write if you had to think about getting it past all of the gatekeepers. And so that could be a way of generating a more diverse science. But again, generally, I don't think that there's one answer to this. I think that what I would like to see in terms of the grant landscape, as well as the publication landscape, is diversity in standards and practices. I guess we could think of it as a kind of experiment. It could be that some just lead to absolute trash; it could be that there are some ways of organizing these things which just don't work, at which point, cool, we've learned not to do that thing. But more importantly, it means that there are more ways in which folks are able to get their ideas out there and developed.

MM: Okay. To diversity!

AC: I mean, it's a very boring answer. (laughs)

MM: We are near the end of this interview and maybe the last question should be about ethical challenges of sciences of the deep past. So, what do you think are the main ethical challenges that we are facing now?

AC: Certainly, in archaeology, it's very obvious, in a sense, what the ethical challenges are. They are often to do with the relationship between archaeological practice and indigenous owners and traditional knowledge and these kinds of things. There's some incredibly complex—and I am far too unsettled a thinker to be able to grapple with them—issues of how to navigate the space between the folks who are often very underprivileged, who are occupying and have some cultural lineage with the folks that you are nominally kind of 'studying' as an archaeologist or for that matter, as an anthropologist. And especially, as in some areas such as North America and Australia, a lot of these disputes are tied into very direct political and social concerns about the sort of well-being of these folks and them having the various rights of autonomy, and so forth. It's just an incredibly complex minefield. In terms of paleontology, there are some interesting ethical issues to do with basically the fossil version of blood diamonds, where lots of amber are being taken out of what's effectively war zones in Africa, and there are reasons to have real ethical worries about that.

In a more general high-minded sense, I think that there is a really important lesson that is partly ethical that we get from thinking about the deep past, which is relevant for the world today and has to do with the notion of normality. In 2016, when we had particular votes in particular Anglophone Western countries that made various Anglophone Westerners a bit worried, there was this talk about that we must repeat that this is not normal, this is not the way that we are. With the pandemic, there's been this talk of a new normal or going back to normal. Normality has become this very important notion, for some reason. And I think that what we learned from looking at the past is that there is no such thing as normal. Humans are very parochial. What we tend to do is we look around us and we see the way things immediately are and we just assume that that's the way things have always been or maybe the way that they need to be. And when you go and look at history, 120 million years ago, there were no flowers. A world with no

flowers is fundamentally different than a world with flowers. You had 120 million years of sauropods eating cycads, roughly, and then you get flowers emerging, and all of a sudden you get pollinating insects, you get these huge radiations, and mammals, and non-dinosaur reptiles, and birds. And you shift from a world that is dominated by really large herbivores eating non-flowering plants to a world, where the terrestrial biota for the last 100 million years is a story of pollinating insects and flowering plants. And those are profoundly different normal. So this idea that there's some stable way things are supposed to be, that we should kind of return to, is to really mistake the ways in which we make decisions about the way our world could be. There may be rules, there may be constraints, perhaps the world can't just be any way. But to think that there is a normal to return to, or to say we want to return to that normal, is rather to say that I want to reconstruct the world as it was. And I think it's worth being aware that there are many other options.

MM: Okay, thank you. And I will just repeat 'to wild, messy, creative sciences of the past and visions of the past' for the end of this interview. Thank you very much for sitting with us today.

AC: Thank you, that was so much fun.

To find out more about the 'Sciences of the Origin' project, make sure to follow us on Twitter, subscribe to our YouTube channel, or visit our webpage.