Kon-Tiki Experiments

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We identify a species of experiment—Kon-Tiki experiments—used to demonstrate the competence of a cause to produce a certain effect, and we examine their role in the historical sciences. We argue that Kon-Tiki experiments are used to test middle-range theory, to test assumptions within historical narratives, and to open new avenues of inquiry. We show how the results of Kon-Tiki experiments are involved in projective (rather than consequentialist) inferences, and we argue (against Kyle Stanford) that reliance on projective inferences does not provide historical scientists with any special protection against the problem of unconceived alternatives.

1. The Voyage of Kon-Tiki. In 1947, Thor Heyerdahl and a small crew set sail from Peru on a balsa raft, hoping to reach Rapa Nui (Easter Island). Heyerdahl was convinced that the island was initially settled via such a voyage. Although it never gained a foothold in academic circles, Heyerdahl's hypothesis possessed some explanatory power: it accounted for linguistic parallels between ancient Polynesian and Peruvian societies and for continuity in material culture, and it was consistent with the legends of both societies (Heyerdahl 1990, 138–39). However, Heyerdahl faced one seemingly insurmountable challenge: ancient Peruvians only had balsa rafts, thought

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insufficient for the long sea voyage. Heyerdahl embarked from Peru to show such rafts could survive the trip and thereby to "destroy one of the weightiest arguments against the theory" (26).

Heyerdahl's voyage is a paradigmatic example of a Kon-Tiki experiment, a species of scientific experiment philosophers have hitherto underappreciated. Kon-Tiki experiments attempt to demonstrate the competence of a cause to produce some effect, usually with the aim of projecting the operation of that cause to some otherwise experimentally intractable system (e.g., targets in the distant past).

We argue that Kon-Tiki experiments play a central role in scientific reconstructions of the deep past.[1] They do so in virtue of providing evidence that can be invoked in inductive projections. We distinguish two species of projection (general projection and specific projection) and argue that these differ in the sorts of considerations that support them.

A caveat: our analysis does not commit us to any particular notion of "experiment" or account of the epistemic powers of experimentation per se (for discussion, see Cleland 2002; Parker 2009; Parke 2014; Currie and Levy 2019). Rather, we identify an epistemic function—testing the capacity of some cause to generate some effect—without providing an exhaustive account of the kinds of investigations able to fulfill that function.[2] Although the term 'experiment' picks out a varied bunch of scientific practices, we think the epistemic function of Kon-Tiki experiments is sufficiently distinct, unified, and important to warrant independent examination. Relatedly, although we discuss Kon-Tiki experiments in the context of historical science, we acknowledge that they are not unique to sciences concerned with the deep past. However, the importance of Kon-Tiki Experiments in historical science is revelatory of their method.

We begin by developing an abstract characterization of Kon-Tiki experiments via an examination of Polynesian settlement (sec. 2), then expand our analysis in light of a second, paleobiological case study (sec. 3). We then

1. Other prominent examples of Kon-Tiki experiments in the historical sciences are the reconstruction of stone tools to test the kinds of marks they might leave on butchered kills (Jeffares 2008), Rudwick's method of exploring functional morphology via replicating fossilized structures (Rudwick 1964; Turner 2000), and early life experiments attempting to establish whether the earth's chemical soup was capable of generating living systems. A possible example of a natural Kon-Tiki experiment is the Pinatubo eruption, which is frequently cited in investigations of geoengineering as showing that aerosol injection into the atmosphere can reduce global temperatures.

2. For instance, we leave open whether simulations could perform Kon-Tiki experiments. That they may be able to is suggested in the simulations used to undermine drift models of Pacific migration (Finney 1977), as well as in Turner's (2009a), Bokulich's (2018), and Currie's (2018, chaps. 9 and 10) discussions of simulations in historical science.

demonstrate the importance of Kon-Tiki experiments within the broader context of scientific inquiry. First, we highlight several epistemic roles that Kon-Tiki experiments play. Second, we argue that such experiments challenge common philosophical accounts of historical reconstruction (sec. 4). Third, we challenge Stanford's (2010, 2011) claim that projective inferences are often insulated from the problem of unconceived alternatives (sec. 5).

2. Kon-Tiki Experiments. In this section, we expand on the fate of Heyerdahl's experiment and use this as the basis for an abstract characterization of Kon-Tiki experiments.

2.1. Polynesian Migrations. The Polynesian Pacific is a vast tract of ocean, broken by only the occasional island or archipelago. Twentieth-century debate concerning the initial settlement of Polynesia centered on two questions. First, was the migration eastward, from Asia, or westward, from the Americas? Second, were the migrations intentional or accidental? In professional circles, the former question was decided in favor of eastward migration, but the latter was up for debate. Heyerdahl believed that migration was both westward and accidental. He doubted ancient techniques were up to navigating the Pacific and thought settlement occurred inadvertently via 'drift voyaging': caught in the occasional squall, unlucky sailors founded new settlements via chancy distribution. Indeed, the Kon-Tiki lacked rudders or other means of being directed. Heyerdahl also considered eastward settlement unlikely because of the prevailing westerly Pacific winds. With his experiment, he aimed to resuscitate the hypothesis of westward migration via drift voyaging.

Although Heyerdahl arrived safely at Rapa Nui, he failed to convince anthropologists of Peruvian settlement. The evidence for eastward settlement—including linguistic and material evidence, written records by explorers like James Cook, and cultural continuity—remained convincing. For decades, even the more minimal hypothesis of ancient Polynesian–South American contact was widely rejected (Lawler 2010), although recent genetic evidence supports it (Thorsby 2012; Moreno-Mayar et al. 2014). Such contact may have occurred via balsa raft voyages, but the evidence is equivocal (Moreno-Mayar et al. 2014).

Although he failed to convince anthropologists of westward migration, aspects of Heyerdahl's perspective were taken seriously by Sharp (1966). Sharp accepted eastward colonization but, like Heyerdahl, questioned whether Stone Age technology and navigational techniques could be used to explore the Pacific deliberately. Instead, he argued that the Pacific was explored via drift voyaging.

Sharp's challenge led to a new set of Kon-Tiki experiments, these examining whether traditional Polynesian navigational techniques and materials could be used for intentionally exploring the Pacific. As Finney (1996, 80) recalled decades later: “Those of us who questioned Sharp’s assertions about the limitations of Polynesian canoes and navigation . . . as well as Heyerdahl’s pronouncements about the impossibility of sailing eastward across the Pacific, soon found that we could not conclusively refute them. . . . To understand the role of indigenous technology and skills in voyaging and colonization in Polynesia, we concluded that we had to reconstruct the ancient voyaging canoes, relearn the old ways of navigating, and then test these on the long sea roads of Polynesia.”

In 1965, a modern catamaran traveled the Pacific using traditional navigational techniques (Lewis 1994). A decade later, Finney and his team constructed a 40-foot-long double-canoe, *Hokule’a* (Finney 1977), and over the next 2 decades undertook a series of voyages throughout the Pacific. The early journeys were under the navigation of Mau Piailug, the first in 1976 between Hawaii and Tahiti (about 2,500 miles). “All in all *Hokule’a* has sailed over 60,000 nautical miles of open ocean, and on all but a few of her long crossings she has been navigated without instruments, charts, or other aids” (Finney 1996, 81). The voyagers relied on traditional navigational techniques and on local knowledge about traversing the Pacific. For instance, Maori traditions specify a particular part of the year to travel from Rarotonga to Aotearoa: precisely when the prevailing westerly winds rest in favor of an easterly direction, thus avoiding the headwinds that Heyerdahl thought so damning of eastward migration. These, in combination with computer simulations that undermined the chancy models of Sharp’s account, gradually reinstated the view of Polynesian settlers as masterful, intentional navigators.

2.2. Abstract Characterization. We define Kon-Tiki experiments in terms of two key features. First, Kon-Tiki experiments attempt to establish the competence of a cause to produce a particular effect. This is accomplished by creating a context in which the cause produces that effect. Heyerdahl, for example, constructed a craft similar to those available in prehistoric Peru to test the possibility of westward migration, while Finney and company used traditional navigational techniques to retrace an eastward migration. Second, with the cause’s competence established, the results may be projected to explain particular events beyond the experimental context. Heyerdahl took the Kon-Tiki voyage to reflect actual voyages made by ancient Peruvians, and Finney saw *Hokule’a*’s journeys as necessary to ‘conclusively refute’ both westward and drift models of Polynesian migration. Both aspects of this definition require further elaboration. The first feature captures an intrinsic property of Kon-Tiki experiments; the second relates them to a broader context of inquiry.

Our analysis draws on the *vera causa* tradition in philosophy of science (Novick and Scholl 2018; Scholl 2019). This tradition distinguishes between three epistemic tasks: showing that a cause *exists*, showing that that cause is *competent* to produce some effect type, and showing that that cause is *responsible* for producing a particular token effect of that type (Herschel 1831, sec. 141; Hodge 1977, 1992).

Distinct evidence is required for accomplishing each of these epistemic tasks. Evidence establishing a cause's existence leaves open what effects that cause is competent to produce. Ancient Peruvians possessing balsawood rafts does not demonstrate that the rafts were competent to reach Rapa Nui. Similarly, evidence of a cause's competencies leaves open which particular effects that cause has in fact produced. Showing that Peruvian vessels can reach Rapa Nui does not establish that they did. Evidence for existence and competence need not be wholly distinct. Causes are known by their effects, so establishing the existence of a cause requires establishing at least one of its competencies. However, the full range of effects that a cause is competent to produce may be discovered well after establishing the cause's existence. The existence of Peruvian rafts was known to Western science well before their competence to reach Rapa Nui was established. Likewise, while establishing a cause's competence to produce some effect type often requires showing that the cause produced some particular effect token of that type, this still leaves open whether the cause was responsible for producing other tokens of that effect type.

Consider the following epistemic situation:

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.

This is the paradigmatic situation that calls for a Kon-Tiki experiment. Heyerdahl wished to determine what cause was responsible for the colonization of Rapa Nui. He knew of the existence of a potential cause (the Peruvians' balsa rafts), but there was serious reason to doubt whether these were competent to make the voyage. Similarly, for Finney and company, serious challenges were raised against the competence of traditional navigational techniques to support purposeful exploration of the Pacific, and their experiments were designed to address these challenges.

This helps to separate Kon-Tiki experiments from other experiments that seek to probe causal competencies. Many experiments demonstrate causal competencies in contrived scenarios that never occur outside laboratory settings (Cartwright 1983, 1999; Hacking 1983). Because Kon-Tiki experiments are concerned with competency in particular nonlaboratory contexts, such as whether balsa-wood rafts can traverse the Pacific, they often involve re-creating more of the world's complexity than other experiments.[3] In this regard, Kon-Tiki experiments involve an element of reenactment, although what is reenacted depends on the competencies at hand, the contexts we are interested in, and the availability of the materials involved. Lewis's voyages reenacted some Polynesian navigational techniques, while those of the *Hokule'a* also re-created the crafts. Sometimes the element of reenactment is prominent, as in archaeological cases in which tools can be re-created largely as they existed historically (Jeffares 2008). In the case of experimental taphonomy (considered below), the fossilized organisms under study are often extinct, requiring the use of relevantly similar proxy organisms. Generally speaking, all experiments involve some element of departure from natural states, but it is characteristic of Kon-Tiki experiments to minimize this departure.

A successful Kon-Tiki experiment (1) tests the competency of a cause (often with a particular context in mind) in order to (2) inform a debate in which the cause's competency is assumed by some live hypotheses. When the experiment demonstrates causal competence, it does so by creating a context in which the cause produces the effect. There can be an asymmetry between demonstrating competence and incompetence. While the success of Heyerdahl's voyage shows unequivocally that balsa rafts can reach Rapa Nui from Peru, failure to reach Rapa Nui would not have shown that such voyages are impossible—any number of chance factors could have accounted for the failure. In other contexts, in which the role of chance factors is more limited (e.g., the experimental taphonomic contexts considered below), this asymmetry is lessened.

As the aftermath of the Kon-Tiki voyage shows, a demonstration of causal competence does not license inferring that the cause was in fact responsible for producing the effect in the instance of interest. Heyerdahl arguably committed this fallacy, at least in his popular work. By contrast, Lewis (1966, 94), discussing the implications of his catamaran voyage, carefully avoided it: "We have of course done nothing to prove whether the old Maoris [*sic*] made long deliberate voyages or indeed made any at all. What I believe we have demonstrated is that methods such as they used are accurate enough to render the major traditional voyages navigationally quite feasible."

Although the results of a Kon-Tiki experiment are by themselves insufficient to license inferences of responsibility, they may serve as the basis for a projection beyond the experimental context. Such projections are the second aspect of our characterization of Kon-Tiki experiments. It is here that Heyerdahl failed, whereas Finney et al. succeeded.

3. For this reason, Kon-Tiki experiments need not be limited to what are traditionally considered "historical" sciences (ecology, e.g., makes heavy use of them).

Consider the distinction between projective and consequentialist reasoning (Stanford 2010, 2011). In consequentialist reasoning, hypotheses are confirmed by checking their empirical consequences. Often, this involves determining which of several hypotheses provides the best explanation of a given set of empirical phenomena (Lipton 2004). Inference to the best explanation is often taken to be our best window into events that are "remote": extremely large, extremely small, from the distant past, very far away, and so on (Cleland 2002; Stanford 2006).

Stanford (2010, 2011) argues, however, that confirmation sometimes involves projecting causes from known to unknown instances, rather than drawing consequences from hypotheses. For example, in experimental taphonomy (Briggs 1995), scientists attempt to re-create fossils in the laboratory and then project their results into nature.

Projecting from an experimentally demonstrated competence to an actual case requires evidence connecting the experimental and historical contexts. Although Heyerdahl established competence in his specific experimental instance, simulation evidence undermined the projection. Modeling suggested it is unlikely that chancy, drift-based colonization could generate the widespread migration represented in the historical record (Finney 1977). Further, establishing responsibility—that Peruvian seacraft in fact were the medium of migration, that early Polynesians in fact purposefully navigated—requires combining evidence that those seacraft are competent to support such migration with evidence that the requisite voyages actually occurred.[4] Here, the myriad evidence supporting the eastward migration scuppers the westward hypothesis.

Finally, note the iterative productivity of Finney's experiments. The journeys led to new discoveries. For instance, scientists only realized the relevance and accuracy of traditional Maori knowledge about navigating between Aotearoa and Rarotonga once they attempted the voyage. Good Kon-Tiki experiments do not simply establish competence but provide platforms for further discovery (see sec. 4 below).

To summarize, Kon-Tiki experiments aim to establish the competence of a cause to generate some effect. They do so by setting up the relevant cause in the relevant context and seeing whether it produces the effect. Importantly, they do so for causes known to exist; they do not aim to demonstrate the existence of novel causal factors (although surprising results may lead to their

4. This illustrates an important point about the distinction between existence, competence, and responsibility claims: they are relative to particular causes. A responsibility claim about one cause may entail an existence claim about a different cause. In this case, the claim that the Peruvians' rafts were not merely competent to support voyages to Rapa Nui but in fact were responsible for supporting such voyages entails that such voyages occurred (i.e., existed). Relative to one cause (the rafts), it is a responsibility claim; relative to the other (the voyages), it is an existence claim.

discovery). In combination with further evidence, the results of a Kon-Tiki experiment can serve as the basis for projecting the cause from the experimental context to some target context. We take this second feature, which concerns the role of Kon-Tiki experiments within the broader structure of inquiry, to be as essential as the first. We discuss some of the complications of projection in the next section.

3. Kon-Tiki Experiments in Paleobiology. In this section, we develop a second case study. This serves two functions. It shows that Kon-Tiki experiments are not restricted to anthropology, furthering our claim that such experiments are a widespread and important feature of successful historical reconstruction. Second, the case allows us to clarify the distinction between two types of projection (general and specific) that can be made on the basis of Kon-Tiki experiments.

3.1. Experimental Taphonomy. Experimental taphonomy is a science of Kon-Tiki experiments. By attempting to (partially or fully) re-create fossils of uncertain origin in the laboratory, experimental taphonomists demonstrate the (in)competence of known fossilization processes to produce fossils from particular types of remains. The hope is that the laboratory demonstration will allow for projection from the known case (the laboratory re-creations, which establish competence) to the unknown case (the putative fossils, for which responsibility is in doubt).

Consider a 2006 study by Elizabeth Raff and colleagues. Raff et al. (2006) performed experiments testing whether marine embryos can maintain structural integrity after death for long enough to fossilize. Their study nicely illustrates the requirements of justified inferences on the basis of Kon-Tiki experiments.

Their experiments were motivated by the discovery of putative embryos in Ediacaran Age rocks (>580 million years old) from the Doushantuo formation. Although the fossils were initially interpreted as algal remains, Xiao and Knoll (1999, 2000) argued that these fossils are most likely metazoan embryos. If that is correct, they are of immense scientific importance, as they would provide a window into the nature of animal development before the Cambrian "explosion," the geologically brief period in which the majority of animal phyla first appear in the fossil record (Erwin and Valentine 2013). As the Cambrian explosion involved major modifications of animal development (Davidson, Peterson, and Cameron 1995), the Doushantuo embryos could provide insight into the precise nature of these modifications—if, that is, they are embryos at all.

There were serious reasons to doubt the embryo interpretation: the "simple geometric forms also resemble other organic and inorganic structures," the putative embryos "are large in comparison with many modern embryos," and the fossils of cleaving embryos are difficult to distinguish from the fossils of other multicellular forms (Raff et al. 2006, 5846). But the most pressing challenge was as follows: "Anyone who works with marine embryos would consider preservation for sufficient time for mineralization via phosphatization unlikely, given the seeming fragility of such embryos. Freshly killed marine embryos in normal seawater decompose within a few hours" (5486).

This situation resembles that in which Heyerdahl found himself before the Kon-Tiki voyage. An attractive hypothesis suffered from a potentially fatal flaw: citing an incompetent cause. Despite Xiao and Knoll's arguments that the Doushantuo fossils are early metazoan embryos, there was reason to doubt that marine embryos' morphology could be preserved for sufficient time to fossilize. This situation calls for a Kon-Tiki experiment, which Raff and her colleagues duly performed.

Raff et al. (2006) exposed sea urchin embryos to an anoxic reducing environment, in which proteins that would otherwise rapidly degrade the embryo are suppressed. Under these conditions, cleavage-stage embryos with an intact fertilization envelope preserved their morphology for 3 weeks. Prehatching blastulae (a later developmental stage) were preserved but did not retain their normal morphology quite as well as cleavage-stage embryos. Posthatching stages were not preserved at all. Note that Raff et al. did not actually fossilize the embryos. They merely showed that they could preserve their morphology for a sufficient length of time for mineralization to occur. This is appropriate, as the issue at hand concerned the ability for such tissues to avoid degradation. In this way, Raff et al.'s experiments demonstrated the competence of an anoxic reducing environment to preserve cleavage-stage marine embryos for sufficient time to allow for mineralization, as well as its competence to partially preserve prehatching blastulae stages.

3.2. General and Specific Projection. While establishing the various competencies of anoxic reducing environments to preserve marine embryos was their most direct result, Raff et al. (2006) drew further conclusions. The inferences they made, as well as those they declined to make, illustrate the distinction between general and specific projection.

When making a *general projection* on the basis of a Kon-Tiki experiment, we infer from the demonstration of competence in the particular instance to the operation of the cause in nature, but we do so without necessarily committing to its operation in any particular instance. For example, from their Kon-Tiki experiment, Raff et al. infer that sometimes cleavage-stage embryos retain their structure long enough to fossilize. This contrasts with *specific projection*, in which one attributes to a cause responsibility for producing a particular effect; that is, one projects it to a specific case or set of cases.

The conclusions that Raff and her colleagues drew were not primarily about the Doushantuo embryos—they were not, in other words, specific projections. Instead, their conclusions mostly concerned taphonomic biases and other general features of the marine embryo fossil record: What sorts of embryos will be discovered? What sorts of information can we glean from them? What artifacts must we avoid misinterpreting? Answering such questions is critical for drawing meaningful inferences from traces. However, this does not require claiming that any particular fossil is of a marine embryo. In other words, they are questions addressed by general projection.

Answering such questions was Raff and colleagues' (2006) primary aim. Thus, they argued that cleavage-stage embryos from a wide phylogenetic spectrum should be found, while posthatching stages should not. Later prehatching stages may be found, but with badly preserved morphology, so apparent structural features may be artifacts. Finally, they suggested that embryo size should not affect preservation, a surprising claim given that the known record of putative fossil embryos is size biased.

With these conclusions, the authors engaged in general projection. Their claims concerned the general operation of the cause, not its operation in any particular instance. They asked what general features the marine embryo fossil record should possess, not on the basis of particular fossil embryos but on the basis of the causal competencies their experiment demonstrated. Insofar as they did discuss the known record of fossil embryos (as when they contrasted features of the actual record with those expected on the basis of their results), they drew on grounds independent from their experimental results (Donoghue et al. 2006). In this way, they projected the biases seen in their experiments into nature in a nonspecific way.

This projection requires that the cause in the laboratory experiment operates in a sufficiently similar manner in nature. Raff et al. (2006) provided three strands of justification for this assumption. First, they cited chemical evidence that the chemical used to create the anoxic reducing environment (β-mercaptoethanol) is a reliable proxy for the chemical responsible for such conditions in ancient oceans (H_2S). Second, they provided evidence that anoxic reducing conditions obtained during the Ediacaran and Cambrian, examining both similar conditions in modern environments and identifying the presence of pyrite (indicating high H_2S levels at the time of fossilization) in the putative fossil embryos. Finally, they used phylogenetic reasoning to argue that early metazoans likely possessed fertilization envelopes.

Raff et al. did draw two conclusions that appear to involve specific projection. First, Raff et al. (2006, 5850) argued that "hatched embryos and soft-bodied larvae are unlikely to be preserved even under reducing conditions" (a general projection) and that, therefore, "claims of fossilized larvae among the Doushantuo fauna . . . appear even more unlikely" (a specific projection). In this case, they draw on a negative result: an anoxic reducing environment is incompetent to preserve hatched embryos and soft-bodied larvae. Therefore, the cause cannot be invoked to explain putative fossil larvae in the Doushantuo formation. Note the asymmetry: establishing a cause's incompetence directly undermines hypotheses of responsibility that rely on that cause's competence, while establishing competence does not alone establish responsibility. Thus, the move from incompetence to lack of responsibility requires less additional evidence than the move from competence to responsibility. The former only requires that the conditions under which incompetence was demonstrated are realistic (in short, it requires successful general projection), while the latter requires additional evidence to establish responsibility. This is why the apparent incompetence of balsa rafts to make it from Peru to Rapa Nui, or for traditional navigation techniques to guide pacific exploration, were such powerful objections.

Second, Raff et al. (2006, 5850) argued that, "although concern has been expressed over the patterns of cleavage exhibited by Doushantuo embryos, the equal size of the blastomeres within these embryos suggest a rapid death and a faithful representation of the living embryo." At first glance, it may seem that Raff and colleagues are using their results to argue that the putative Doushantuo embryos are truly embryos. In fact, however, their claims rests on hypothetical reasoning. Raff and colleagues take for granted the interpretation of the Doushantuo fossils as embryos. If this interpretation were correct, skeptics had argued, then the pattern of cleavage the fossils showed was odd. Raff et al. counter by showing how their results can explain the pattern of cleavage seen. That is, they show that an existing challenge to the marine embryo interpretation fails, but they do not directly project from competence to responsibility. It might turn out that the fossils are not embryos at all, but their hypothetical reasoning would still hold. Thus, this is not really a specific projection at all but a general projection in disguise. By projecting experimentally determined causal competencies into nature, it removes a challenge to the claim that the Doushantuo fossils are of embryos.

Thus, Raff et al.'s conclusions primarily involve general projections. We have begun to see why this should be so: general projection requires less additional evidence than specific projection. The one instance of genuine specific projection in the paper involved incompetence rather than competence: if a cause is incompetent to produce an effect in general, then it is incompetent to produce particular instances of that effect. In such cases, once the general projection is made, the specific projection comes for free.

In summary, a general projection takes us from the results of a Kon-Tiki experiment to nature generally but nonspecifically and requires demonstrating that conditions in the experiment are relevantly similar to some natural conditions. A specific projection claims responsibility for the cause in a particular instance. In cases of competence rather than incompetence, this requires further evidence still.

4. Prototypical Historical Science. In recent decades, philosophers of science have seriously investigated methods of historical reconstruction. Many claim that historical science proceeds via inference to the best explanation on the basis of appeals to common causes or to consilience (e.g., Sober 1988; Wylie 1999; Tucker 2004, 2011; Kleinhans, Buskes, and de Regt 2005, 2010; Forber and Griffith 2011; Vezér 2017). Although they differ in detail, these views agree that historical science primarily involves drawing consequentialist inferences from traces.[5] The most well known of these accounts is Cleland's (2001, 2002, 2011, 2013), so we focus on hers, but similar arguments apply, mutatis mutandis, to the others.

Cleland claims that historical science progresses via discovering 'smoking guns': traces (found through fieldwork) that discriminate between existing hypotheses. She characterizes this process via an idealized sequence. First, a surprising correlation between traces is identified. Second, a set of hypotheses about the past that explain these correlations is postulated. Third, further traces are sought that are expected on the basis of some hypotheses in the set but not others. These traces are 'smoking guns'. They empirically discriminate between hypotheses in a consequentialist manner. For Cleland, this is the primary means of testing in historical science.

Cleland emphasizes that her account is of *prototypical* historical science: she does not claim that all and any historical reconstruction must follow her pattern. Nonetheless, Cleland's model of historical testing is firmly focused on the discovery of new traces—smoking guns—which provide consequentionalist tests of historical hypotheses.[6] This obscures the critical role that projective inference from Kon-Tiki experiments plays in many debates between archaeologists, paleontologists, and other scientists concerned with the deep past.

Against this focus on consequentionalist, trace-based reasoning, we contend that Kon-Tiki experiments play three distinct epistemic roles: testing middle-range theory, testing central assumptions in historical narratives, and opening fruitful new avenues of research. If Kon-Tiki experiments are as important as we think, then approaches like Cleland's miss a critical part of prototypical historical science.

Kon-Tiki experiments play a role in testing middle-range theory: hypotheses that explain the formation of traces and thereby allow inferences from traces to their causes (Kosso 2001; Jeffares 2008). Raff et al. (2006), for

5. Important exceptions to these accounts of historical reconstruction are Jeffares (2008), Currie (2015, 2017, 2018), Bromham (2016), Chapman and Wylie (2016), O'Malley (2016), and Currie and Sterelny (2017).

6. Cleland provides both an account of method in historical reconstruction (testing via smoking guns) and its justification (common cause reasoning backed up by general but a posteriori facts about the world, i.e., the overdetermination of the past by the present). Note that our complaint is compatible with her account of justification but not her account of method.

instance, infer that soft-bodied larvae and hatched embryos are unlikely to be preserved. This claim counters interpretations of the Doushantuo traces in terms of those morphologies. In other words, Raff et al. use their Kon-Tiki experiment to reject the inference from the known traces to a particular hypothetical cause. Kon-Tiki experiments are not limited to challenging inferences, however. Jeffares (2008) provides a positive example: he describes a classic Kon-Tiki experiment involving paleoanthropologists butchering carcasses using replica stone hand tools and comparing the marks to those left by dogs, in order to determine whether older bones were butchered by *Homo sapiens* or carnivores. As Jeffares points out, such experiments put pressure on Cleland's distinction between prototypical experimental and historical sciences (cf. O'Malley 2016).

Beyond testing whether a particular kind of historical process could produce the target traces, Kon-Tiki experiments can test more general narratives about historical patterns. Consider the relationship between the original Kon-Tiki voyage and that of *Hokule'a*. Heyerdahl asked whether it is possible for balsa-wood craft to make it from Peru to Rapa Nui, a critical plank in the westward-migration theory. Finney and his team asked whether traditional navigation techniques are capable of underwriting intentional migration through the Pacific, a key element of their model of eastward Pacific migration. In this case, there was no particular link between a trace and a historical process being probed: they were not testing a particular middle-range theory but were instead testing an important assumption in a historical narrative.

Finally, Kon-Tiki experiments are often productive beyond providing tests of middle-range theories and historical narratives. *Hokule'a*'s voyages revealed the significance of Maori traditional knowledge about travel to Aotearoa. Raff et al.'s experiments revealed an established pattern (the size bias in the marine embryo fossil record) to be an anomaly in need of further research and explanation. Experimentally probing causal capacities opens new, fruitful avenues of research.[7]

Kon-Tiki experiments are a central feature of historical science. Therefore, establishing causal competencies via Kon-Tiki experiments should be included in any idealized or prototypical account of method in those sciences. Kon-Tiki experiments establish the very links between traces and the past on which smoking guns rely, test the validity of historical narratives, and expand our toolkit for understanding the past. Historical scientists engaging in a Kon-Tiki experiment are not acting out of character. They are simply doing what historical scientists do.

7. To be clear, we are not claiming that this feature distinguishes Kon-Tiki experiments from other sorts of experiments, merely that it is an important role that Kon-Tiki experiments play in historical inquiry.

Cleland has responded to this sort of objection by arguing that the regularities captured by middle-range theory, while important to historical science, are mere auxiliaries to the real business of historical reconstruction: trace-based inference. As she says in response to a similar point from Jeffares (2008): "Generalizations of this sort play a secondary role in historical research. They are not the targets of historical research but rather useful tools borrowed from other disciplines for special purposes" (Cleland 2011, 566).

This mischaracterizes historical science. First, even granting that historical research is centrally concerned with explaining traces, the 'inference tools' historical scientists develop are not simply 'borrowed' from other disciplines. They are developed within the historical sciences and geared toward the local, idiosyncratic needs of historical inference (Chapman and Wylie 2016, chap. 4; Currie 2018, 262–65). Kon-Tiki experiments are among the central "in-house" methods that historical scientists have at their disposal for developing these inference tools. They are not properly treated as being of merely secondary importance.

Second, we have shown that Kon-Tiki experiments do not play second fiddle to smoking guns in debates between historical scientists. The debate concerning whether the Pacific was settled via drift or purposeful navigation centered on clashing Kon-Tiki experiments. The proper interpretation of the Doushantuo fossils (if they are fossils at all) turned on Raff and colleagues' examination of the conditions of fossilization. In such debates, Kon-Tiki experiments are not 'secondary'. For Cleland, prototypical debates turn on finding new traces, new 'smoking guns'. This does not capture the cases we have examined, and these cases are not unusual (see n. 1). Further, the productive nature of Kon-Tiki experiments plays an important role in driving and shaping historical research. Sometimes it is not the discovery of new traces but the discovery of new causal competencies that opens up historical research.

Third, although Cleland characterizes prototypical historical science as narrowly concerned with making particular inferences about particular events and processes in the deep past, this can be questioned. Although Kon-Tiki experiments are often made with particular instances in mind, we have highlighted how they are used to make general projections. The role of general projections supports a picture of historical scientists being interested in establishing and examining the general capacities of historical causes and regularities (Jeffares 2008; Turner 2009b; Currie 2018, chap. 7) and linking these to narrative explanations of the past (Currie 2017; Currie and Sterelny 2017). This is a richer enterprise than merely explaining contemporary patterns, one in which establishing the competence of causes to produce effects via Kon-Tiki experiments plays a central role.

This final point raises an interesting speculative idea. Perhaps construing historical science as narrowly focused on reconstructing the past misses out on a broader aim: understanding the possibilities of existence in their requisite domains. That is, as opposed to caring about the paleontological past alone, paleobiologists care about what life on the macroevolutionary scale could be like: exploring possible rather than actual histories. Although our discussion here is insufficient to establish this (both case studies focused on particular matters of history), our account of Kon-Tiki experiments is amenable to it. Regardless, Kon-Tiki experiments are more than a sideshow in historical science: they are part of the prototypical business of historical reconstruction.

5. Projections and Alternatives. In the previous section, we considered the strengths of Kon-Tiki experiments. We now consider their limitations. In particular, we will challenge Stanford's (2010, 2011) recent arguments that projective inference on the basis of Kon-Tiki experiments furnishes historical scientists with special resources for mitigating the problem of unconceived alternatives. We will show that, although Kon-Tiki experiments play a central role in historical reconstruction, they are vulnerable to unconceived alternatives in their own way.

In an important book and paper, Stanford has argued that, in at least some contexts, the nature of both scientific theorists (Stanford 2006) and scientific communities (Stanford 2019) leaves even our best theories undermined by the likelihood that there exist unconceived alternatives that, if conceived, would be similarly well confirmed by the available evidence. This motivates his selective antirealism about science.

However, Stanford believes that scientists, when able to rely on projective rather than consequentialist reasoning, are able to insulate their reasoning from the problem of unconceived alternatives, by way of generating what he calls an "affirmative challenge" that the antirealist must meet. Stanford takes experimental taphonomy as his central example. Since experimental taphonomy makes heavy use of Kon-Tiki experiments, Stanford can be understood as claiming that Kon-Tiki experiments have a special role to play in generating an affirmative challenge and, thus, in helping scientists to mitigate the problem of unconceived alternatives.

In this section, we investigate the relationship between Kon-Tiki experiments, the problem of unconceived alternatives, and the affirmative challenge. While we agree with Stanford about the importance of projective reasoning, we argue that it is still vulnerable to forms of the problem of unconceived alternatives. Moreover, the problem affects general projections and specific projections differently. Further, we argue that Kon-Tiki experiments cannot generate an affirmative challenge by themselves but only in combination with further evidence. Thus, Kon-Tiki experiments do not furnish historical scientists with any special defense against the problem of unconceived alternatives.

Stanford (2010) presents the affirmative challenge while discussing the hypothesis that fossils are the mineralized remains of once-living creatures. He argues that the evidence supporting this hypothesis against neo-Aristotelian and Neoplatonic alternatives was consequentialist and thus vulnerable to unconceived alternatives.[8] However, thanks to the evidence of experimental taphonomy, it is now supported by the projection of experimental results into the distant past. As a result of this shift in reasoning, Stanford argues that the hypothesis is insulated against the problem of unconceived alternatives.

Why think that the switch to projective reasoning has this advantage? According to Stanford (2010, 237), a demonstration of causal competence produces, in some cases, "an affirmative *challenge* for the suggestion that some alternative process" is responsible for a particular effect. For the hypothesis of organic fossil origins, the challenge takes this form: if we refuse to project the results of experimental taphonomy to fossils produced in the distant past, then (*a*) "we will then have to explain why the taphonomic processes we have investigated in such detail in the field and lab have *failed* to produce fossils over geological time" or (*b*) we will have to explain "where *those* fossils have gone" (237–38). Thus, rejecting the hypothesis of organic fossil origins comes with a heavy explanatory burden. One cannot merely provide an alternative mechanism for producing fossils. One must also explain why organic remains have failed to fossilize, despite the existence of processes competent to produce them or why, despite the fact that fossils of organic remains have been produced, we have failed to find any of them. The antirealist is challenged to provide such an explanation.[9]

It is important to be clear about which hypotheses are supported by an affirmative challenge. When we ask, "Are fossils the remains of organisms?" there are multiple questions that we might have in mind, and "the hypothesis of organic fossil origins" is ambiguous as to the question it is intended to answer. Today, 'fossils' are organismic remains by definition, so the question should be understood as concerning organism-shaped rocks of disputed origin. With this in mind, we can distinguish three questions, each of which bears on the question of fossil origins:

8. The Neoplatonic view took fossils to be a manifestation of a "hidden network of correspondences, analogies, and affinities that linked all the diverse parts of nature into a coherent and intelligible whole" (Stanford 2010, 222). The neo-Aristotelian view took fossils to be produced by the "forms" of organisms acting on inorganic rather than organic materials (223).

9. Stanford's main interlocutor here is Turner (2007), who argues that historical hypotheses fare worse, vis-à-vis realism, than experimental hypotheses.

1. Do processes capable of converting organic remains into organism-shaped rocks occur in nature?
2. What proportion of the organism-shaped rocks hitherto discovered are the products of these processes?
3. Is this particular organism-shaped rock the product of these processes?

Each of these questions may be answered on the basis of projective reasoning from the results of Kon-Tiki experiments. However, different kinds of projection are involved. Answering the first question involves general projection: projecting from the processes observed in the lab to processes in nature, without attributing any particular effect to the action of those processes. Answering the third question involves specific projection: projecting from the processes observed in the lab to a specific instance of their operation (in producing the putative embryos of the Doushantuo formation, say). The second question is intermediate: one takes processes observed in the lab to account for some proportion of a specific set of effects but without committing to any particular effect within that set being the product of those causes. Henceforth, we call this "intermediate projection."

General, intermediate, and specific projection are all vulnerable to the problem of unconceived alternatives. However, the different types of projection are vulnerable to different kinds of alternatives. In what follows, we argue that the affirmative challenge has two parts, one of which supports general projection, the other of which supports intermediate projection. We further argue that the affirmative challenge arises, not in virtue of the evidence provided by Kon-Tiki experiments alone but in virtue of additional evidence that supports projection on the basis of such experiments.

Let us start with general projections. Here, the hypothesis of organic fossil origins is not in competition with Aristotelian and Platonic alternatives, for the simple reason that multiple kinds of processes, all capable of producing similar effects, can coexist. It does not matter how many real processes are competent to produce mineralized structures resembling organisms. Accumulating alternative processes cannot undermine a general projection because it cannot show that there is not a class of cases produced by the process in question.

This does not mean that general projection is invulnerable to the problem of unconceived alternatives. It means that Aristotelian and Platonic theories are not the relevant alternatives. What undermines a general projection is evidence that there are pertinent differences between the experimental conditions in which the competence was demonstrated and the natural conditions into which its operation is projected. The reason it would be astonishing if fossils were never produced by the processes studied by experimental taphonomists is that there is good evidence that, in many cases, the conditions in which they make fossils can be considered reasonable simulacra of those in nature.

This corresponds to the first part of the affirmative challenge for the antirealist: the challenge of explaining why a process, revealed in the lab, has failed to occur in nature. By itself, this is not much of a challenge: lab environments and nature differ in myriad ways, any number of which might be relevant to such projection. The affirmative challenge arises only when there is evidence that lab conditions are relevantly similar to natural conditions and is only as strong as that evidence. Because Kon-Tiki experiments seek to reenact natural contexts, a good Kon-Tiki experiment requires that such evidence is available. The Kon-Tiki experiment does not itself provide it. Thus, for instance, Raff et al. (2006) needed to provide evidence that sufficiently anoxic environments existed in the deep past and that the chemical they used to create such an environment is sufficiently similar to the chemical that would have occurred in the natural context.

The antirealist, however, need not be especially impressed by this part of the affirmative challenge. We are never able to consider every distinction between lab conditions and natural conditions—there are too many, and many are unknown (especially when reasoning about the deep past). Thus, while the alternative hypothesis (that the process does not occur, or occur in relevantly similar ways, outside of the lab context) is conceived, many of the potential sources of concrete support for this alternative are not. In this way, the problem of unconceived alternatives rears its head here as well, in the form of the problem of unconceived differences (or unconceived relevant factors). Scientists do what they can to mitigate these, using existing knowledge of what causes are relevant, just as they attempt to protect against unconceived alternatives when reasoning in a consequentialist fashion. In both cases, they rule out as many alternatives as possible, but, in both cases, numerous potentially relevant alternatives remain unconceived. Citing the affirmative challenge amounts to the claim that the unconceived differences between lab and natural conditions very likely are insufficient to threaten the general projection. One is not in a better position regarding sweeping claims about the unconceived simply because one is engaged in projective, rather than consequentialist, reasoning. The switch to projective reasoning, enabled by Kon-Tiki experiments, changes the shape of the problem, but it does not make it less pressing.

Now consider intermediate projections. In the case of organic fossil origins, the intermediate projection, which claims that some proportion of known organism-shaped rocks are the products of taphonomic processes, is, like general projection, not in direct conflict with alternative accounts of how such rocks originate. The claim that many known organism-shaped rocks are organic remains is consistent with the claim that some of them are not, and these may well have been produced by Aristotelian or Platonic processes.[10] We no longer accept the existence of such processes, but that is beside the point. The worry, discussed by Raff et al. (2006), that embryo-like forms could be produced by inorganic processes is no challenge to the second claim. However, while these alternatives are irrelevant to general projection, they are relevant to intermediate projection: alternative theories of fossil origin can become part of a relative significance dispute over which accounts for more of the known cases (Beatty 1995, 1997).

The intermediate projection is supported by the second half of the affirmative challenge, and understanding this will also clarify how it is vulnerable to the problem of unconceived alternatives. The second part of the affirmative challenge assumes that the general projection goes through (that the processes occur in nature) and asks why, if they occur, we have failed to find their traces. If we accept that taphonomic processes occur in nature, it would be quite surprising if none of the organism-shaped rocks we have discovered were produced by them, although it might still be the case that most of them are produced by some alternative process. In this case, the relevant alternatives concern the existence of trace-destroying processes in nature. If there exist processes that preferentially destroy taphonomic (but not Aristotelian) traces, the affirmative challenge can be met.

Many processes are known to destroy fossils (Turner 2005), and these are routinely invoked to explain biases in the fossil record. It is true that known processes are insufficient to explain the destruction of all fossils in all circumstances. Nonetheless, it is possible that unconceived processes exist that could destroy any fossils that are produced, thus requiring an alternative explanation of the origin of organism-shaped rocks. Both features seen in the case of general projection are relevant to the case of intermediate projection. First, evidence beyond that provided by Kon-Tiki experiments is required to generate the challenge. Second, the challenge is only as strong as our grounds for doubting that such unconceived processes exist.

To be clear, we are not claiming that the problem of unconceived alternatives is particularly pressing for general and intermediate projection—we are not arguing for antirealism. The point is that Kon-Tiki experiments do not themselves protect one against the unconceived. Projective reasoning, as much as consequentialist reasoning, is vulnerable to unconceived alternatives. Mitigating these problems requires additional evidence that goes beyond that yielded by Kon-Tiki experiments. This undermines Stanford's

10. It is true that if the hypotheses are construed as each claiming that most (>50%) of the organism-shaped rocks are the product of a particular process, then they are incompatible. However, we will see that the second part of the affirmative challenge does not support such claims of proportion.

appeal to projective reasoning as a principle of selection required by his selective realism. If selective realism can be defended in this area, it must be on different grounds.

Finally, consider the case of specific projection. Only here are the Aristotelian and Platonic alternatives in direct competition with the hypothesis supported by projection. A particular organism-shaped rock might have been produced by taphonomic processes or by Aristotelian processes but not by both at once. Or, to give a contemporary example, embryo-like forms might be the remains of embryos, or they might be the mere by-products of purely inorganic processes (Raff et al. 2006), but not both. Conflicts between alternative hypotheses of organic origin are also possible. The Doushantuo "embryos" have been interpreted as giant sulphur bacteria (Bailey et al. 2007), as encysting protists (Huldtgren et al. 2011; cf. Huldtgren et al. 2012; Xiao et al. 2012), and as algae (Zhang and Pratt 2014).[11] Specific projections are vulnerable to the problem of unconceived alternatives in its classic form, and no affirmative challenge arises. The affirmative challenge applies only in the case of general and intermediate projections.

Where does this leave us? Kon-Tiki experiments are performed in order to contribute to both general and specific projections; they are less useful for supporting intermediate projections.[12] Consider the case of specific projections first, corresponding to the third question distinguished above. Was Rapa Nui colonized from the east or from the west? Could the Doushantuo fossils be the remains of embryos? Kon-Tiki experiments can help to answer these questions, but only when supplementary evidence is available. Since the affirmative challenge does not support specific projections, it fails to capture much of what is philosophically interesting about the role of Kon-Tiki experiments in experimental taphonomy and other historical sciences.

However, Kon-Tiki experiments may also underwrite general projections and may do so even when the specific projection at hand falls through (as occurred in the case of the Kon-Tiki voyage). Even though Raff et al.'s (2006) embryo-preservation experiments were performed as part of a broader project of showing that the putative embryo fossils of the Doushantuo formation really are embryos, the inferences that Raff et al. drew from their study were primarily general projections. These projections, however, remain vulnerable to the problem of unconceived alternatives. This is most obvious in the case of their projection that the embryo fossil record should not be size biased, since this conflicts with the known record. This makes it likely that

11. Although now conceived, these alternatives were not considered in the original defense of the embryo interpretation (Knoll, Xiao, and Zhang 1998; Xiao and Knoll 1999, 2000).

12. One of our issues with Stanford's discussion is that he treats Kon-Tiki experiments as furnishing a strong response to the antirealist in the context of intermediate projections.

the affirmative challenge can be met in this case: either there is some relevant difference between the lab context and the natural context (meeting the first part of the affirmative challenge), or there exists some unknown process that preferentially destroys smaller embryos (meeting the second part of the affirmative challenge). Alternatively, it may simply be that the known record is not a representative sample of the actual record.

The upshot is that the affirmative challenge should not be understood as a special argument for local realism. Rather, the challenge is raised and addressed in the course of ordinary science. If the challenge is met, formerly accepted projections are rejected or modified. If not, these projections continue to be accepted. Thus, we understand the proper role of the affirmative challenge in historical science differently than Stanford does. Rather than showing how Kon-Tiki experiments are of special interest to the scientific realism debate, it reveals the different ways that unconceived alternatives affect the ordinary reasoning of historical scientists.

6. Conclusion. When philosophers have considered the confirmatory power of scientific investigation, they have typically focused on either establishing theories about the regular operation of nature (a feature often attributed to 'experimental science') or establishing particular matters of fact (often attributed to 'historical science'). Here, we have focused on a kind of experiment that plays a special role in bridging these aims. Kon-Tiki experiments establish the competence of causes to produce certain kinds of effects. We have shown that Kon-Tiki experiments test the middle-range theories that are essential to historical reconstruction, as well as in testing features of the sorts of narrative explanations that historical science produces. They straddle (and therefore blur) the line between "experimental" and "historical" science. We have also shown how they relate to the problem of unconceived alternatives. While they do not provide any special solace to the realist who hopes to solve the problem, they reveal the diversity of forms that this problem takes and the ways that these diverse forms impinge on everyday reasoning in historical science. Consideration of Kon-Tiki experiments furnishes another reminder of the diverse and often subtle aims, lines of reasoning, and approaches scientists employ in their quest to understand the natural world.

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