Measuring Time with Fossils: A Start-Up Problem in Scientific Practice

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This article is about a start-up problem in scientific practice. Specifically, it is about the problem of justifying paleontological correlation—the practice of using fossils to establish time relations among fossiliferous rocks. Paleontological correlation was the key to assembling a geological timescale during the nineteenth century and remains an important practice in stratigraphic geology to this day. Yet contrary to philosophical expectations, this practice lacked a robust theoretical justification during the first half of the nineteenth century. This article examines what this lack of justification amounted to, as well as how the method of paleontological correlation was justified in practice.

1. Assembling a Geological Timescale. The geological timescale is "a layer cake of odd names," many of them established in a burst of amazingly fruitful research during the first half of the nineteenth century (Gould 1987, 76). Historian Mott Greene describes it as "a triumph of intellectual attention to singularity unequaled in the history of human thought" (2009, 171). Others have called it "the tool 'par excellence' of the geological trade" (Gradstein 2012, 1) and "an invaluable tool for geoscientists investigating virtually any aspect of Earth's development, anywhere on the planet" (Walker et al. 2013, 259). Less sentimental types have called the scale "a residue of nineteenth century geology" (Erwin and Valentine 2013, 13) or else "a rickety old contraption, held together by nineteenth-century rules and current European

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formality" (Ward and Kirschvink 2015, 12). Yet while some may enjoy a potshot at this icon of their field, geologists of all stripes share a profound admiration for the scale and what it represents: nothing less than a synthesis of the geological history of our planet—a *geohistory*.

By the end of the eighteenth century, it was widely appreciated that the earth was ancient—far more ancient than the few thousand years accorded to it by modern creationists (Rudwick 2005). But scientists remained without a strategy for ordering its far-flung "pages" and "chapters" (rock bodies) into a coherent story (a geohistory). In this, they faced a situation "not unlike the hypothetical . . . dilemma that historians would face if they knew that modern cultures had antecedents . . . but did not know whether Cheops preceded Chartres or, indeed, whether any culture, however old and different, might not still survive in some uncharted region" (Gould 1987, 76–77). While it was simple to infer that rocks near the bottom of the pile were older than those near the top (at least in local areas undisturbed by tectonic activity), geologists lacked a reliable way of comparing the ages of widely separated rocks and of ordering these into a coherent sequence. This meant there was no way of saying whether a stack of rocks in Pembrokeshire was as old as a stack of rocks in the Appalachians, notwithstanding that they might resemble one another in superficial appearance.

All this had changed by the middle of the nineteenth century. In an explosion of "conceptual innovation [and] empirical expansion," the newly christened science of geology had burst from the gates and set to work disclosing the long and eventful history of the planet (Rudwick 1985, 3). In less than 50 years, a multinational community of researchers had ordered the pile of formations into a concatenation of systems, "defined by the ever-changing history of life, and recorded by a set of names accepted and used in the same way from New York to Moscow" (Gould 1987, 77). Remarkably, the major features of this history are still recognizable today, at least for the largest divisions of the geological column. Yet no less remarkably, the practice most responsible for this success—the measurement of time using fossils—lacked an adequate theoretical foundation during the early decades of the nineteenth century. It is this observation that supplies the focus for the current article. In particular, I will ask how the absence of a theoretical justification caused no real disruption in stratigraphic geology during the first half of the nineteenth century. My answer will be that geologists managed to solve the "problem of nomic measurement," so named by Hasok Chang (2004)—or if they did not solve it, at least they found a way of lessening its sting.[1] The solution was

1. The problem of nomic measurement arises when researchers want to measure an unobservable quantity X on the basis of an observable quantity Y, but the relationship between X and Y is insufficiently characterized. I discuss the problem further in sec. 4.

nowhere explicitly formulated, yet it was widely understood that ongoing research had rendered the foundations of paleontological correlation increasingly secure. This article aims to explore the logical basis of this (largely implicit) judgment.

The remainder of the article is organized as follows. In section 2, I introduce Richard Boyd's notion of a start-up problem and suggest that the subject of this article can be characterized as a "start-up problem in scientific practice." In section 3, I provide a crash course in nineteenth-century geology, which is followed, in section 4, by a discussion of the problem of justifying fossil-based measurement. In section 5, I consider how this problem was overcome in practice: by a piecemeal strategy, as opposed to a theoretical fix-all. I conclude in section 6 with a brief synopsis.

2. A Start-Up Problem in Scientific Practice. This article addresses what might be called a "start-up problem in scientific practice." I owe the term "start-up problem" to Richard Boyd (1992), who speaks of "*the* start-up problem [in philosophy of science]" as the problem of explaining "the first emergence of approximately true theories within a research tradition, and thus the emergence of the reliable methods they determine [i.e., justify]" (139, emphasis added). The start-up problem is a problem, Boyd thinks, because scientific methods are deeply theory dependent, and as a consequence, it is not an option to explain the emergence of successful scientific theories by appealing to the methods they make possible. In addition, it is not an option to explain their emergence by reference to a more basic theory-independent methodology because no such methodology exists. The upshot, Boyd thinks, is that "the emergence of epistemically successful scientific methods must have depended on the logically, epistemically, and historically contingent emergence of a relevantly true theoretical tradition rather than vice versa" (139). Or, to render this as a motto: no epistemically successful scientific method without a preexisting theoretical justification.

The start-up problem I deal with in this article is not the same as Boyd's start-up problem, for the important reason that it is not concerned with "the emergence of an approximately true scientific theory." Instead, it is concerned with the emergence of a methodological practice in the absence of a justifying theory and, indeed, in the absence of much interest in providing such a theory. The practice is paleontological correlation, and it consists in the "fitting together" of rock layers in different parts of the world on the basis of their fossil contents. It is important because, before the second half of the twentieth century, it was the best way for geologists to compile information from individual outcrops into regional frameworks and, ultimately, to synthesize these into a global timescale. As Goldring puts it: "Until outcrops . . . are correlated by time lines, there is no way of gaining any real appreciation of the temporal distribution of past environments across an area or within adjacent basins and ranges; let alone of clarifying what was going on at distant points on the globe" (1991, 156). This means that absent a reliable means of correlating rocks over long distances, the project of reconstructing geohistory is scarcely possible at all.

But why were fossils so important for stratigraphic correlation? To answer this question, we must familiarize ourselves with some features of stratigraphic geology in the nineteenth century. The next section provides a crash course in nineteenth-century stratigraphy, which will position us, in section 4, to explore our start-up problem in scientific practice.

3. A Crash Course in Nineteenth-Century Stratigraphy. Stratigraphy is the study of layered rocks ("strata"), but on a more elementary level, it is all about time (Torrens 2002). Stratigraphers are interested in determining the ages of rocks and in using this information to delineate a sequence of geological units that can be recognized throughout a region and, even, throughout the world. This involves, first, delineating packages of strata that represent discrete units of time and, second, fitting these packages together through a process called correlation. Correlation refers to the practice of matching geological units found in different localities, or to be more precise, of establishing a correspondence between geographically separated parts of a single geological unit. Sometimes called "temporal correlation," it is the way geologists seek to establish the time equivalence of rock layers and, by this means, to build a framework applicable over a maximally wide geographical extent.[2] The trick is to show that rocks observed in different exposures are actually the same age. Rocks do not come time-stamped, after all, and since geological evidence is notoriously jumbled and fragmentary, considerable difficulties confront the project of assembling a timescale from the scattered windows afforded by natural and artificial exposures.

These difficulties were acutely felt by those nineteenth-century geologists who set about unraveling local sequences and matching them with sequences in other parts of the world. The basic problem was the absence of a criterion for matching time-equivalent rocks in widely separated areas. Rock type, or *mineralogy*, had once appeared a promising criterion. According to the famous theory of German scientist Abraham Werner, all rocks on the earth's surface had precipitated from a universal ocean in order of their densities. So granites, having the highest density, precipitated at the earliest period, and less dense rocks like sandstones and limestones precipitated later. Had this posit been correct, temporal correlation would have been a straightforward affair, since all that would have been required to locate a rock in the pile of formations would have been information about its mineralogical characteristics. Yet Werner's system was untenable, as observations of intrusive granite sufficed

2. There are also nontemporal forms of correlation; these are not my concern here.

to show.[3] This did not discredit mineralogy as a guide to delineating rocks representing discrete units of time, but it did suggest that rock type alone could not supply a "measuring rod of history"—a means of placing rocks in their right temporal sequence (Gould 1987, 81).

Enter fossils. Around the turn of the nineteenth century, the surveyor William Smith had shown that fossils can be used to distinguish a number of discrete formations in England and Wales. The most famous result of this survey was a map that depicted the succession of British Secondary strata at an unprecedented level of detail.[4] Smith produced his map "by collecting fossils from particular localities and strata, precisely noting their geographical and stratigraphical placement, and identifying analogous strata in other locations by finding similar fossils" (Sepkoski 2017, 62). He called the fossils peculiar to a stratum "characteristic fossils." Together they functioned as a kind of stratigraphic reference system, since finding a characteristic fossil told you that the surrounding rock belonged to *this* part of the pile as opposed to *that* part. Although Smith was not terribly concerned with reconstructing geohistory (his concerns were structural rather than geohistorical), his method was quickly adopted by those with more geohistorical interests (Rudwick 2005). A famous example is Sir Roderick Impey Murchison, who declared that "the *zoological* contents of rocks, when coupled with their *order of superposition*, are the only criteria of their *age*" (1839, 9).

Smith's work came close to supplying a paradigm for stratigraphic geology in the sense of a model of exemplary practice. In the years following his publication, no geologist could eschew the task of collecting fossils from stratigraphic sections or at the very least describing them in his notebook. Yet Smith's accomplishment did not quite rise to the status of an exemplar in the Kuhnian sense (Rudwick 1985). This is evident from the fact that, in the early decades of the nineteenth century, doubts persisted about the priority of fossil evidence in stratigraphic correlation. At issue was precisely the matter that Smith regarded as settled: the reliability of fossils as markers of stratigraphic position. The matter was unsettled because—contrary to Smith's claim to have uncovered a Law of Strata—Smith had in fact discovered no law that could underwrite the extension of his method to other parts of the world or indeed to other parts of the pile. What Smith had discovered was that fossils could be used with great reliability to distinguish a large number of Secondary formations and that these identifications could be used to correlate

3. Intrusive rocks are formed when liquid magma penetrates existing rock; so the existence of intrusive granite indicates that less dense rocks can be deposited before granite, contrary to Werner's account.

4. In the eighteenth and nineteenth centuries, the term "Secondaries" (or "Secondary rocks") referred to a collection of well-stratified and fossil-rich limestone and shales (e.g.) that rested atop the more structurally complex "Primaries."

rocks across England and Wales (Rudwick 2005). But it remained open to question whether the existence of certain fossils in a rock reflected the period of time in which that rock was formed (as Smith's method of characteristic fossils required) or whether it sometimes reflected something else, like the presence of certain conditions at the era of "fossil potting." The problem was a serious one, and it was clear to many that it would need to be sorted out before long-distance paleontological correlations could be regarded as anything more than provisional.

4. The Problem with Paleontological Correlation, Characterized. Here is the basic issue. By the 1830s, no one denied that fossils had a role to play in stratigraphic correlation. Yet there remained a question as to what exactly this role should be, particularly when geologists ventured beyond the relatively well-behaved Secondary formations of Great Britain and continental Europe. The question was important since the use of fossils in correlation had both empirical and theoretical vulnerabilities. On the empirical side, what was missing was a demonstration that fossil assemblages had indeed succeeded one another in an orderly way in time, not only at a single location but everywhere in the world these fossils assemblages happened to occur. Absent this demonstration, it would not be possible to infer the age of a rock from the identity of its enclosed fossils, since fossils that occur throughout the column carry no temporal signature. However, in the early decades of the nineteenth century, knowledge of the temporal ranges of fossils remained highly fragmentary and almost necessarily parochial.[5] This meant that the use of fossils in correlation rested on substantial empirical assumptions, which many in the early century regarded as unwarranted, if not downright implausible (see Rudwick 1985).

On the theoretical side, what was missing was an explanation of why the stratigraphic record is amenable to paleontological correlation. Perhaps it could not be shown on empirical grounds that the history of life consists in a linear succession of mostly discrete floras and faunas. Still, if it could be shown that this succession is expected on theoretical grounds, then the absence of an empirical demonstration could be blunted. And by the 1830s, several proposals to this effect had been made. On the continent, Georges Cuvier had articulated a theory of revolutions, which held that massive calamities in earth's past had served to establish divisions between successive periods in the history of life (Rudwick 2005). Later, Léonce Élie de Beaumont

5. This is not to say that geologists lacked evidence that the history of life was broadly directional. It was suspected, for example, that fossils like ammonites were confined to Secondary strata and that mammals were confined to Tertiary strata. What they lacked was detailed information about the spatial and temporal ranges of (most) fossil taxa, and this raised the possibility that apparent trends in the fossil record were just that.

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proposed a similar theory, which held that major periods in geological history were terminated by "epochs of elevation" associated with marine and terrestrial extinctions. These theories enjoyed considerable popularity for a time—at least before the 1840s, when Élie de Beaumont effectively recanted. Still they were far from universally accepted, especially in Great Britain, where the most famous revolution was a bloodless one, and political history after Cromwell was rather less tumultuous than it was in France.

A related theoretical idea was that the earth was slowly cooling from an incandescent state.[6] Because it was believed that organic life must have a constant relationship to the state of the earth's surface, it seemed to follow that the community of living things must have changed in order to keep pace with the state of the earth. Advocates of this view did not interpret these changes in evolutionary terms; rather, they tended to imagine a trickle of extinctions followed occasionally by new creations or else migrations from different climate zones. Yet even apart from this, the view was based on a false premise. The earth is not slowly cooling from an incandescent state, and the drama of life's relationship with climate is significantly more complicated than the directionalist theories of the nineteenth century could comprehend.

Without an empirical demonstration that the fossil record is suitable for correlation, or a theoretical argument that the record can be trusted in the absence of such a demonstration, geologists faced the following dilemma. In order to use the fossil record to correlate strata over large distances, it must be the case that fossil assemblages succeeded one another in an orderly way in time throughout the sampling area. However, to determine whether fossil assemblages succeeded one another in this way, some method is needed to determine whether a succession in one part of the world (e.g., a sequence showing the transition from fauna *A* to fauna *B*) is contemporaneous with a succession in another part of the world (which also shows the transition from *A* to *B*). But this is what fossils are called on to do—in particular, fossils belonging to faunas *A* and *B*. The result is a circularity. Since the practice of correlation presupposes that the transition from *A* to *B* happened at the same time over the relevant area, it cannot establish that this was the case—something Huxley pointed out in an address to the Geological Society of London in 1862: "For anything that geology or paleontology are able to show to the contrary, a Devonian fauna and flora in the British Islands may have been contemporaneous with Silurian life in North America, and with a Carboniferous fauna and flora in Africa. Geographical provinces and zones may have been as distinctly marked in the Palaeozoic epoch as at present, and those seemingly sudden appearances of new genera and species, which we ascribe to new creation,

6. This idea was shared among advocates of geological catastrophes and (some of) their opponents. For the former, it supplied a plausible mechanism for transient disruptions of the earth's surface (Rudwick 2005).

may be simple results of migration" (1880, 213). To mark the absence of "any method by which the absolute synchronism of two strata can be demonstrated," Huxley coined the term *homotaxis*, meaning similarity of arrangement (of fossil successions at distinct locations; 212). His point was that paleontological correlation could not establish that fossils succeeded one another in a regular way in time. All it could establish is that fossils occur in a regular vertical order in strata. This, in a word, was the problem with paleontological correlation during the nineteenth century.

The problem can be further characterized as an instance of what Chang calls the "problem of nomic measurement," which has the following structure:

(i) We want to measure quantity X;
(ii) [But] quantity X is not directly observable, [so] we infer it from another quantity Y, which is directly observable.
(iii) For this inference we need a law that expresses X as a function of Y.
(iv) But the form of this function f cannot be discovered or tested empirically, because that would involve knowing the values of both Y and X, but X is the unknown variable that we are trying to measure (2004, 59).

In the present case, X is time (i.e., the age of a stratum), Y is faunal composition, and f is the form of the relationship between time and faunal composition over a specified area. Early nineteenth-century geologists tended to assume that observed fossil successions reflect temporal successions, not just at a single location, but at many locations separated by hundreds or even thousands of kilometers. But this was just an assumption, and as Huxley said: "It may be so; it may be otherwise" (1880, 213). The reason is that fossils measure time only with the assistance of an empirical assumption: that the fossil record preserves a worldwide directional signal and that certain events recorded at widely separated exposures were effectively synchronous. And this assumption cannot be decisively validated on the strength of fossil evidence alone.

Nonetheless, it was verified, at least to the satisfaction of most geologists. The next section considers how this was done. In particular, it examines the kinds of evidence relevant to assessing the temporal significance of homotaxial patterns, as well as the judgments involved in establishing the time equivalence of stratigraphic events.

5. Validating Paleontological Correlation. It is a remarkable fact about nineteenth-century geology that geologists were aware of the problem with paleontological correlation and yet seemed to be little bothered by it. Yes, there were doubts—not only about particular paleontological correlations but also about the tendency to assign fossil evidence priority in correlational

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practice (Rudwick 1985). But the dominant note in the period was one of optimism and confidence regarding the promise of fossil-based correlation. Indeed, by the time Huxley coined the term "homotaxis" in the 1860s, the tendency to award fossil evidence the right of way in stratigraphic practice had been widely accepted for more than a decade.

Were these geologists behaving rashly? Did they overreach in thinking that a geological timescale could be articulated and refined using fossil data alone? In this section, I suggest that the answer to these questions is no. Nineteenth-century geologists had good reason to think that the succession of fossil assemblages in strata reflected a real historical succession, at least when the appropriate crosschecks had been performed. Moreover, they had reason to think that certain events in the rock record, at least, were approximately synchronous over broad geographical areas.

Consider a sequence of three fossil assemblages (*A*, *B*, and *C*) with suspected nonoverlapping ranges in time.[7] How can the geologist know whether the observed succession of faunas ($A > B > C$) reflects a true temporal sequence as opposed to a sequence of laterally arranged depositional environments, say? To begin, if it is true that the assemblages succeeded one another in the hypothesized temporal order, then it should never be the case that *C* appears beneath *B* at an exposure or that *C* or *B* appears beneath *A* (Harper 1980). Likewise, it should never be the case that these supposedly sequential assemblages appear together in a single stratum (*A* with *B*, *B* with *C*, etc.). Observing any of these forbidden sequences or associations at any exposure is sufficient to disprove the hypothesis that *A*, *B*, and *C* form a nonoverlapping temporal sequence. (Sufficient, that is, if no plausible explanation of the anomaly exists, such as the inversion of a whole succession of strata or the reworking of sediments following deposition.) And while the situation is more complicated if we hypothesize that *A*, *B*, and *C* succeeded one another in time with overlapping temporal distributions (e.g., $A > A(B) > B > B(C) > C$), it remains forbidden that—for example—*C* should appear before *A* at any exposure (although it can be expected that *B* will sometimes appear before *A*, and *C* before *B*—just not that often).

To what extent can crosschecks of this sort vindicate the claim that assemblages that succeed one another in strata also succeeded one another in time? Certainly they cannot prove this. Even if every observed succession is compatible with the hypothesis that *A*, *B*, and *C* succeeded one another in time, this does not establish that they in fact did so. Perhaps in every case the apparent temporal succession was due to accidents of preservation, and *A*, *B*, and *C* in fact existed for exactly the same interval. Or perhaps *A*, *B*, and *C* did succeed one another in time, but only at the examined sections. In other,

7. The line of reasoning pursued in this paragraph is unchanged if *A*, *B*, and *C* name taxa (e.g., individual species or genera) as opposed to assemblages (see, e.g., Harper 1980).

unexamined sections, *B* existed well before *A* and endured long after *C*. There is nothing conceptually incoherent about these proposals, but the crucial point is that they become less plausible as more stratigraphic sections are examined. Once Thomas Jefferson hoped that Mastodons might survive in the vast American interior, but as more of the country was explored, this hope became difficult to sustain. In a like fashion, some geologists in the 1830s were happy to postulate that land plants might have existed in the Cambrian Period, but by the 1850s, these notions had been mostly confined to the fringes of the geological community (Rudwick 1985).

The reason they had become untenable was the absence of certain kinds of evidence—in particular, evidence of land plants interbedded with Cambrian marine fossils. Consider that to postulate that *A* and *B* coexisted for a significant period of time is to suggest the likelihood that at some exposures, at least, members of *A* should be found in association with members of *B*—in particular, if either *A* or *B* contains a taxon that is (1) widespread in distribution and (2) abundantly preserved in a variety of depositional environments (Harper 1980). If members of *A* and *B* are not observed in association at any exposure, the claim that *A* and *B* coexisted for a significant period of time becomes harder to swallow and may come to seem indefensible as more exposures are examined. The claim can never be disproved using fossil evidence alone (perhaps land plants did exist in the Cambrian, despite never being observed in conjunction with any characteristic Cambrian animals). Yet at some point, the failure to observe *A* and *B* in association will tip the balance of evidence in favor of the claim that *A* and *B* did not coexist for a significant period of time. Notice that when *A* and *B* are taken to be successive assemblages, this pattern of reasoning can lend support to the claim that *B* succeeded *A* at approximately the same time throughout its range: that for the purposes of stratigraphic correlation, the transition from *A* to *B* can be taken to mark a time horizon wherever it is preserved.[8]

Did geologists then solve the problem of nomic measurement? In a sense they did. To solve the problem, geologists needed to show, first, that the fossil record preserves a directional signal and, second, that events in the record taken to mark time horizons were roughly synchronous over the relevant areas. And by the middle of the nineteenth century, both of these claims had been rendered increasingly plausible. In both cases, the reasons for supporting the claim flowed not from an overarching theory but instead from judgments of plausibility anchored in knowledge of local stratigraphic sections. Yet they were none the weaker for this—and in fact, the absence of a widely recognized theory of paleontological correlation probably saved the practice

8. Again, the reason is that, were the transition from *A* to *B* not roughly synchronous throughout their respective ranges, we would expect to find members of *A* and *B* preserved in association at some exposure(s).

from disruption, since the most celebrated theories of early nineteenth-century geology were neither universally accepted nor particularly long lived.

6. Conclusion. This article has been about a start-up problem in scientific practice: How were geologists in the nineteenth century able to solve the problem of nomic measurement? Roughly speaking they had two options. The first was to articulate a theory that showed that the fossil record preserves a directional signal and that faunal transitions preserved in the record were roughly synchronous over large geographical areas. The other was to warrant these claims in the absence of an overarching theory. Contrary to Boyd's expectation (that epistemically successful methods presuppose the existence of an approximately true scientific theory), geologists in fact took the second route and were successful in doing so. Their success did not place paleontological correlation beyond the reach of all doubt, as Huxley's criticisms suffice to show. Yet by the middle of the nineteenth century, most reasonable doubts about the practice had effectively been assuaged.

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