What Is the Use of the History of Geology to a Practicing Geologist? The Propaedeutical Case of Stratigraphy

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ABSTRACT

A practicing geologist can benefit from the history of geology professionally in two main ways: by learning about past mistakes so as not to repeat them and by finding out about different ways to discovery. In this article, I discuss some aspects of the history of stratigraphy and point out that the concept of a stratum has shoehorned geologists into thinking time and rock equivalent, which has led to some serious misinterpretations of geological phenomena, such as the timing of orogenic events and the charting of sea level changes. I call this the "tyranny of strata." The very name of stratigraphy comes from strata, but what it does is simply deduce temporal relations from spatial relations of rock bodies, including fossils, by making certain assumptions about processes, that is, invoking inevitably a hypothetical step. What we have learned from looking at the history of geology is that empirical stratal correlation, even when well controlled by index fossils, can never yield perfect temporal correlation, and any assumption that it does is doomed to failure. Geology progresses in a direction that it may soon be possible to date every package of rock in a way to know what process is being dated and where exactly. We can correlate only processes in time hypothetically, not rock bodies empirically. This is the most important lesson a stratigrapher ought to have learned from the history of his or her subject. For such lessons to be usable by professional geologists, they must be narrated by those who are familiar with geological practice as scientists.

Online enhancements: appendix.

To the memory of my dear friend, the great historian of geology,
David Roger Oldroyd
(1936–2014)

Introduction

One can have an interest in the knowledge of the past for its own sake, as part of one's general knowledge: learning about history is entertaining and makes one appreciate one's own environment. But this is not only why a geologist needs to know about the past of his science. He needs it to appreciate what he is doing now and to learn how to do it better. The great English philosopher and archaeologist Robin Collingwood wrote in his most famous philosophical work, *The Idea of History*:

> But a mere inventory of our intellectual possessions at the present time can never show by what right we enjoy them. To do this there is only one way: by analysing them instead of merely describing them, and showing how they have been built up in the historical development of thought. (Collingwood 1946, p. 230)

I am in agreement with this statement. Collingwood saw the job of the historian both as a philosopher and as an antiquary. This is a viewpoint that is unfortunately no longer in fashion with most historians of our days. They seem to be followers of the great romantic German historian Leopold von Ranke, who stated that the job of the historian was simply to state "how it really was" (Ranke 1824, p. VI; *wie es eigentlich gewesen*). Ranke wrote this against those who considered judging the past and forecasting the future as among the tasks of history (see his p. V and VI). Michael Grant pointed out that Ranke's statement was

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probably inspired by the parallel statement "to say exactly how things happened" by the "Syrian" satirist and novelist Lucian of Samosata (now Samsat, southeastern Turkey; ca. AD 120–180) in his *How to Write History* (Grant 1995, p. 94). That this is an unrealizable ideal had been appreciated already during the Enlightenment, however; Wilhelm von Humboldt had famously written that

> The job of the historiographer is the description of what happened. . . . But what happened is available to our senses only in part. The rest must be felt, deduced, guessed. What does appear of it is dispersed, torn up, isolated. What joins up these pieces, what illuminates the individual piece in the right light, gives the whole a shape, is hidden from direct observation. . . . That is why nothing is so rare as an absolutely true narrative. (von Humboldt [1821] 1959, p. 153; see also Şengör 2009*b*)

Humboldt's thoughts are very familiar in spirit to every geologist, who in any introductory course hears that the geological record is woefully incomplete. To approximate what the past was like, we thus need to build models. As the Nobel laureate physicist Richard Feynman once said, we do not recreate the past in our models, but we try to reenact it as part of our attempt to re-create the cosmos in our minds.

The geologist tries to obtain a knowledge of the past of the geological processes he is interested in, and in doing so he needs to create models in his mind. The word "model" is commonly misunderstood by geologists who come from backgrounds such as physics and chemistry. They think that a model must be a quantitative description of the process one is interested in. They do not seem to appreciate the fact that Charles Darwin's and Alfred Russel Wallace's hypothesis of the cause of biological evolution, natural selection, for example, was a model that had no quantitative aspect whatever (Darwin and Wallace 1857; Darwin 1859). It was inspired by Thomas Robert Malthus's very crudely quantitative model of the relation between a growing population and the augmentation of its food source (Malthus 1798, chaps. 1, 2), but Darwin's and Wallace's own model was entirely qualitative. Time has shown what a useful model it is. Eduard Suess's model of orogeny was based on a crude analogy with a wound on a hand that tore the skin, folded it on one side, and let blood well up on the other (Suess 1875, p. 28; 1878, p. 5), yet it proved to be a more accurate description of how orogeny happens than any quantitative model that was offered until the rise of plate tectonics (which largely corroborated Suess's model plus the subduction; see Şengör 2009*a*). Thus, a model as an attempt to capture the past is nothing more than the completion in mind of the incomplete data we can gather on past objects and past processes, regardless of how it is done, as long as it is internally consistent and remains testable by further data. This is what the great German geologist Leopold von Buch, Baron of Gelmersdorf, said in his inaugural speech in the Royal Prussian Academy of Sciences on April 17, 1806, concerning the nature of geology:

> When geology manages to cast this great progress from a shapeless drop to the sovereignty of man into certain laws, then it would be worthy of being admitted into the community of the sciences, which, by supporting each other, attempt to finish the work that nature had begun. (von Buch [1808] 1870, p. 16)

The job of geological models is thus the "completion" of the work of nature in the minds of the geologists, the construction of an intelligible picture from the incomplete store of information available to them.

But the geologist never begins from scratch. There is and there has always been preexisting models of whatever object or whatever process he or she is trying to understand. The earliest mythologies of humankind are nothing but cosmological (Jensen 1890; Gaerte 1914; Kirfel 1920; Henning 1947–1948; von Engelhardt 1979), geographical (Vater 1845*a*, 1845*b*; Buchholz [1871] 1970; Lang 1905; Delage 1930; Rousseau-Liessens 1961, 1962, 1963, 1964; Wolf and Wolf 1983; Richer 1994), meteorological (Gilbert 1907; Engelhardt 1919), geological (Vitaliano 1973; Greene 1992; Şengör 2002; Piccardi and Masse 2007; this book has two articles on mythology-paleontology relationships; the entire book was critically reviewed by Oldroyd 2008), and biological (Abel 1937, 1939; Edwards 1967; Rudwick 1976; Riedl-Dorn 1989; Thenius and Vávra 1996; Mayor 2000, 2005; Gregorová 2006) models (for a general and very scholarly overview of this entire subject from the viewpoint of a natural scientist—namely, both a chemist and a physicist—who had a religiosity not dissimilar to that of Albert Einstein, see Schweigger 1836; also see von Humboldt 1847). For the mythological views of the Jews on scientific questions such as anatomy, physiology, pathology, zoology, chemistry, geology, physics, and astronomy, see Bergel (1880). Because of that ever-present preexistent "knowledge," every increment of knowledge added to geology builds on, and is conditioned by, previous observations and hypotheses. However, no hypothesis can be properly understood if torn from the problem situation and observations that have given rise to it. Moreover, no hypothesis arises naturally out of the problem situation and observations. As Kant ([1781] 1838) had seen a long time ago and Popper (1933, 1935) famously corroborated, every hypothesis is a free invention of the scientist who formulated it. Therefore, to understand a hypothesis every geologist must know the problem situation that necessitated thinking about it, the observations made necessary by the previous hypotheses, and the new hypothesis a former geologist (or a group of geologists) created, plus the milieu in which the geologist(s) who generated the new hypothesis has(have) been educated and functioned or still function(s). Only when armed with such knowledge can a geologist use the hypothesis in question competently and perhaps replace it by a still newer and better one. It is here that a knowledge of the history of geology helps the practicing geologist. No practicing geologist working on interesting problems can do without a general knowledge of the past of his science and a specific knowledge of the history of the particular problem he or she happens to be working on.

In the following I review the history of stratigraphy, perhaps the most basic branch of geology, and show how the concept of a "stratum" (i.e., layer, bed), a volume of rock confined between two parallel or subparallel surfaces, has led geologists to confuse time with rock, leading to serious misconceptions about the behavior of our planet. I hope to show that stratigraphy is more widely "known" than "understood," largely because practicing geologists commonly do not know its conceptual roots and the problems in which those roots have been embedded. The misunderstanding shines through the very name geologists have given to this branch of geology. It has been called stratigraphy ever since the English geologist William Smith called the job of stratal correlation by that name (Smith 1817). But stratigraphy is not just stratal correlation. For a better understanding of what follows, let me define stratigraphy as it is understood now and has been understood since it was first invented by Steno in 1669: stratigraphy is that branch of geology that is concerned with converting spatial relations of rock bodies to temporal relations. Smith's stratigraphy was only a part of this larger concept. For a succinct account of its early history, going over the same ground as the corresponding sections below and giving less detail in some and more in other aspects, see also Morello (2003); for succinct histories of stratigraphic concepts, see Grabau (1921, p. 2–20), Arkell (1933, p. 1–37), Berry (1987), Gohau (2003), Miall (2004), Walsh et al. (2004), and Vai (2007), although their emphases are entirely different from mine, with Miall's and Walsh et al.'s being the closest. Conkin and Conkin (1984) provide extracts from what they consider significant publications in the history of stratigraphy. However, some of their extracts are from derivative articles; others are totally inadequate, the most critical passages and/or figures and tables not having been reproduced; and some major classics (such as Suess's *Antlitz*) are not even cited. So that compendium must be used with some caution. Rudwick's (2005, 2008) two massive volumes deal with the rise of paleontological stratigraphy, but, despite their many useful features, I would not recommend them to the uninitiated, first because they are heavily skewed toward the Anglophone and Francophone literature and second because they are tendentious, having a veiled agenda of showing that religion has not been harmful to the progress of geology and was even helpful. This results in many omissions and some distortions. Anyone intending to read them should first read the four reviews analyzing them in some detail and showing their shortcomings (Şengör 2008, 2009*b*; Wilson 2009; Oldroyd 2010).

The Invention of the Concept of Stratum

It might at first sight seem absurd to consider stratum (i.e., layer, bed) as a concept, that is, an abstract idea. Is a bed not a concrete object seen with our bare eyes? Do we need an abstract idea of it to perceive it? Surprising as it may sound, strata (i.e., beds, layers) had not been recognized (except by such remarkable individuals as Leonardo da Vinci, who depicted turbidite layers in his *Sant'Anna, la Vergine e il Bambino con l'Agnellino*—i.e., *The Virgin and Child with St. Anne*, painted from 1506 to 1510, now in the Louvre (inventory number 776 [319])—with such accuracy that the C division in a Bouma sequence under the right foot of the Virgin can be clearly recognized [fig. A1 herein; figs. A1–A3 are available online]; see the detailed discussions in Vai 1986; 1995, p. 14–16, his fig. 2; 2003*a*; 2003*b*, p. 38–40; 2004) until Descartes (1644) inferred the presence of layers in the earth theoretically in his *Principia Philosophiæ* on the basis of his vortex model of the origin of the solar system (fig. 1 herein). With the exception of Leonardo da Vinci's paintings and drawings, none of the landscape paintings or close-up depictions of rock outcrops before Descartes give us even the faintest idea that their artists may have thought about layers. Another ex-

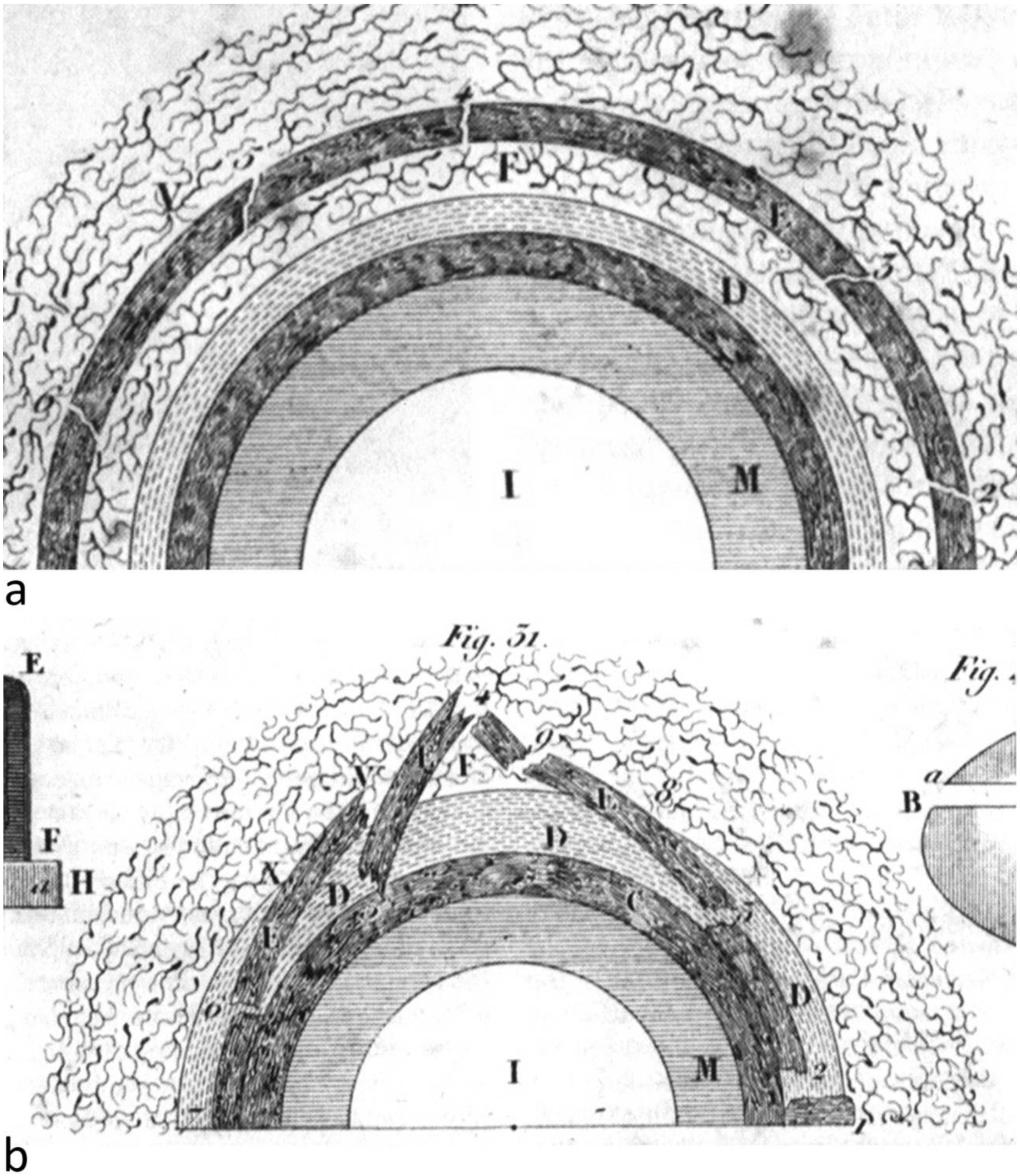


Figure 1. *a*, Descartes's layered earth. *b*, Deformation of the terrestrial layers and origin of mountains and basins according to Descartes. Both figures are from *Principia Philosophiæ* (1644).

ception besides Leonardo has been suggested by Needham and Wang (1959), who claimed that the Chinese artist Li Kung-lin (fl. ca. 1100 CE) depicted layers on a rock wall at Lung-Mien Shan near Thung-chhêng in the province of Anhui, north of the Yangtze River between Hangzhou and Nanjing, west of Lake Tai (太湖; fig. 2 herein). However, Li's picture is so stylized that it is to me unclear whether what he depicted was what he thought to be figures on a rock or actual structural elements of an outcrop. Figure A2

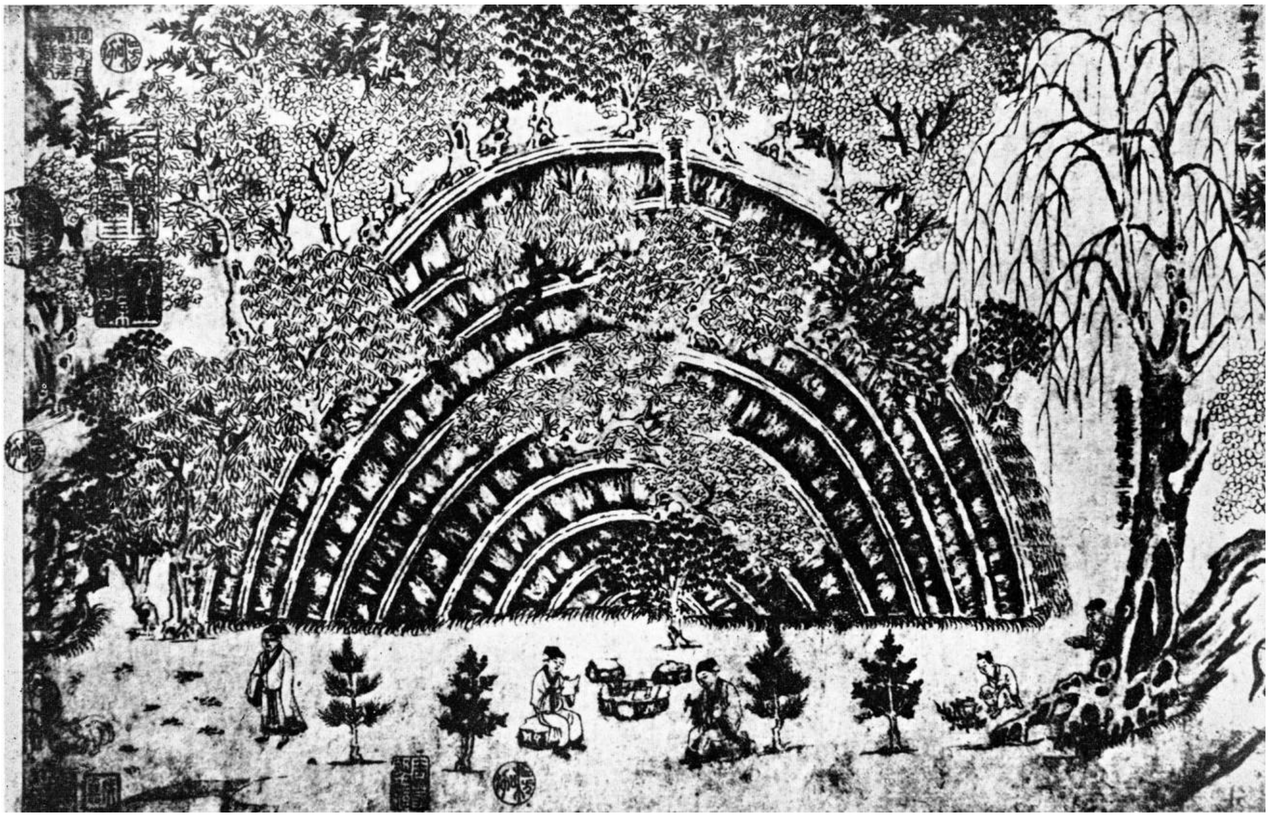


Figure 2. Chinese artist Li Kung-lin's depiction of the layers on a rock wall at Lung-Mien Shan near Thung-chhêng in the province of Anhui, north of the Yangtze River between Hangzhou and Nanjing, west of Lake Taihu (from Needham and Wang 1959, their fig. 263).

herein shows four paintings showing landscapes: one from Roman antiquity and three from the Renaissance. None shows bedding in any form. But they do show something else: they reveal to us how their painters looked at nature. In figure A2*a* herein, the unknown Roman artist from Pompeii considered rocks as heaps on earth with no internal structure whatsoever. This inference from the painting is corroborated by a passage in Vitruvius's *De Architectura*, book 8, chapter 1, titled "On Finding Water":

> Valleys between mountains are subject to much rain, and because of the dense forests, snow stands there longer under the shadow of the trees and the hills. Then it melts and percolates through the interstices of the earth and so reaches to the lowest spurs of the mountains, from which the product of the springs flows and bursts forth. (Granger 1985, p. 143)

The great architect considered mountains as porous heaps of rock sitting on an impermeable foundation. Notice that this is a very different hypothesis of earth structure from the sponge-earth hypothesis of Empedocles (DK31B52; Bollack 1969*a*, p. 88–89; DK31B51; Bollack 1969*b*, p. 62–63; see also Seneca's *Quaestiones Naturales*, III, 24: DK31A68; see also Bollack 1969*a*, p. 88–89; 1969*b*, p. 227–228, 248–249), Socrates (in Plato's *Phaedo*: 111E; see also Serbin 1893; Frank 1923), and Aristotle (*Meteorologica*, 351a, 360a6–7, 365b21–366a), which also did not have any layers (for details on the sponge-earth hypothesis, see Şengör 2003).

In the paintings by Fra Filippo Lippi (fig. A2*b* herein) and Andrea Mantegna (fig. A2*c* herein), the landscape is only an excuse to depict religious scenes; hence, the landscapes have aspects of constructed buildings with hollows and carved steps. However, Vai (2009*b*, p. 194–195, especially his figs. 9 and 10) thought to have found evidence in another painting by Mantegna, the *Madonna delle Cave* (*Madonna of the Quarry*, 1488–1490, in the Uffizi Gallery, Florence) for the depiction of beds and even joints. If accepted, this would predate Leonardo's recognition. However, Mantegna's painting shows only rectangular blocks piled on top of each other like so many books that do not continue laterally. In hindsight, one can interpret them as beds. But did he really see them as beds? The overall aspect of Virgin's throne in the painting, of which the "beds" are a part, leaves me unconvinced, especially when compared with Leonardo's crystal clear depiction, leaving little doubt that he knew what he was looking at. Albrect Altdorfer's painting *Die Schlacht bei Arbela* (*The Battle at Arbela*, fig. A2*d* herein, in the Alte Pinakotek, Munich) also shows mountains as heaps of conical shapes. It is easy to forgive the artists for not seeing the internal structure of rock bodies, but that even an experienced mining expert such as Georgius Agricola (Georg Pawer = Bauer) should not have seen them is extraordinary. Figure 3 herein shows three figures from his great classic *De Re Metallica* (Agricola [1556] 1953). In all three, the various ore-containing veins are depicted in great detail, and even the structures emanating from these veins into the surrounding rock are shown in figure 3*c* herein. Yet the surrounding rocks are left "opaque," with no structure whatever. Agricola saw neither bedding nor any foliation! As von Goethe wrote to his friend the Chancellor Friedrich von Müller on April 24, 1819: "One sees only what one knows: actually, one can catch a glimpse of something that one already knows and understands." Agricola and almost all his predecessors did not know about bedding so they could not see beds, however incredible this may sound to us today.

In the introduction to the English translation of Bernard Palissy's *Discourses Admirables*, Aurèle La Rocque (1957) claimed that Palissy not only understood beds but also derived from this understanding the principle of superposition before Steno (p. 18–19). It is difficult to agree with this interpretation, because nowhere in his book does Palissy (1580) speak of beds, although he clearly observed horizontal seams between what we now know to be beds. But Palissy interpreted these seams as joints that facilitate splitting the rock. He thought that such horizontal seams form because rocks are formed by vertical filling in subterranean cavities. He might have had some vague idea that the age of such cavity-filling rocks might become younger upward in a filling, but such an interpretation is difficult to squeeze out of his words. When he talks about the movement of subterranean waters, for example, he always uses the word "vein" and never "bed" (e.g., Palissy 1580, p. 8). So there were no strata in Palissy's world as far as I can make out.

Lesson 1. So far we can formulate our lesson no. 1 from the history of geology: even such an "obvious" feature as a bed may not be as obvious as we now think. What we consider obvious today may not have been so to our predecessors, implying that we too may be wrong in our identification of the obvious.

René Descartes du Perron, known simply as Descartes, the father of the modern era in human civilization, changed all this with his *Principia Philosophiæ* of 1644. He depicted strata in his book (Johann Scheuchzer, whom Studer [1863, p. 204] called the first geologist of Switzerland, may have been the earliest to credit Descartes for recognizing the importance of strata in geology in 1708; see Ellenberger 1995, p. 42). His strata, however, were entirely imaginary. Descartes's strata were parts of a model, not the results of observation. His model universe consisted of agile yet perfectly fitting particles creating a continuum. Like Aristotle, Descartes allowed no vacuum, and thus the agility of his particles inevitably resulted in mutual rubbing, causing erosion and creating round elements. These round elements Descartes oddly called "secondary" and the scraps resulting from their erosion "primary." A third type of element formed from accumulations of the primary elements. The primary elements were, according to Descartes, "luminous," the secondary "transparent," and the tertiary "opaque." The future solar system originally consisted of 14 vortices, the nuclei of the future sun, planets, and their satellites. Not all original vortices were of the same size owing to piracy from neighboring vortices and consequent losses in their favor. The sun's vortex happened to be larger and more powerful than the rest and thus eventually gobbled up all the others.

Descartes imagined that in the beginning the earth was as much a star as the sun, as the sun was as much a star as all other stars. He abandoned the Aristotelian onion universe, and in his new universe there were no privileged locations. Smaller "stars" formed shells around themselves consisting of the third, opaque element, labeled layer M by Descartes, enclosing a fiery nucleus consisting of the first element (fig. 1*a* herein). The interstellar space, called space B by Descartes (fig. 1*a* herein), also consisted of the tertiary element but in a much looser arrangement (how one obtains a loose arrangement without a vacuum Descartes does not tell us). Its biggest agglomerations soon settled on layer M, forming another opaque layer C on the nuclear sphere, whose formation was accompanied by squeezing out of it a fluid part forming yet another concentric shell outside C, which Descartes called D (fig. 1*a* herein). Above D, Descartes felt it necessary to postulate yet another shell consisting of layered material, which he called E (fig. 1*a*

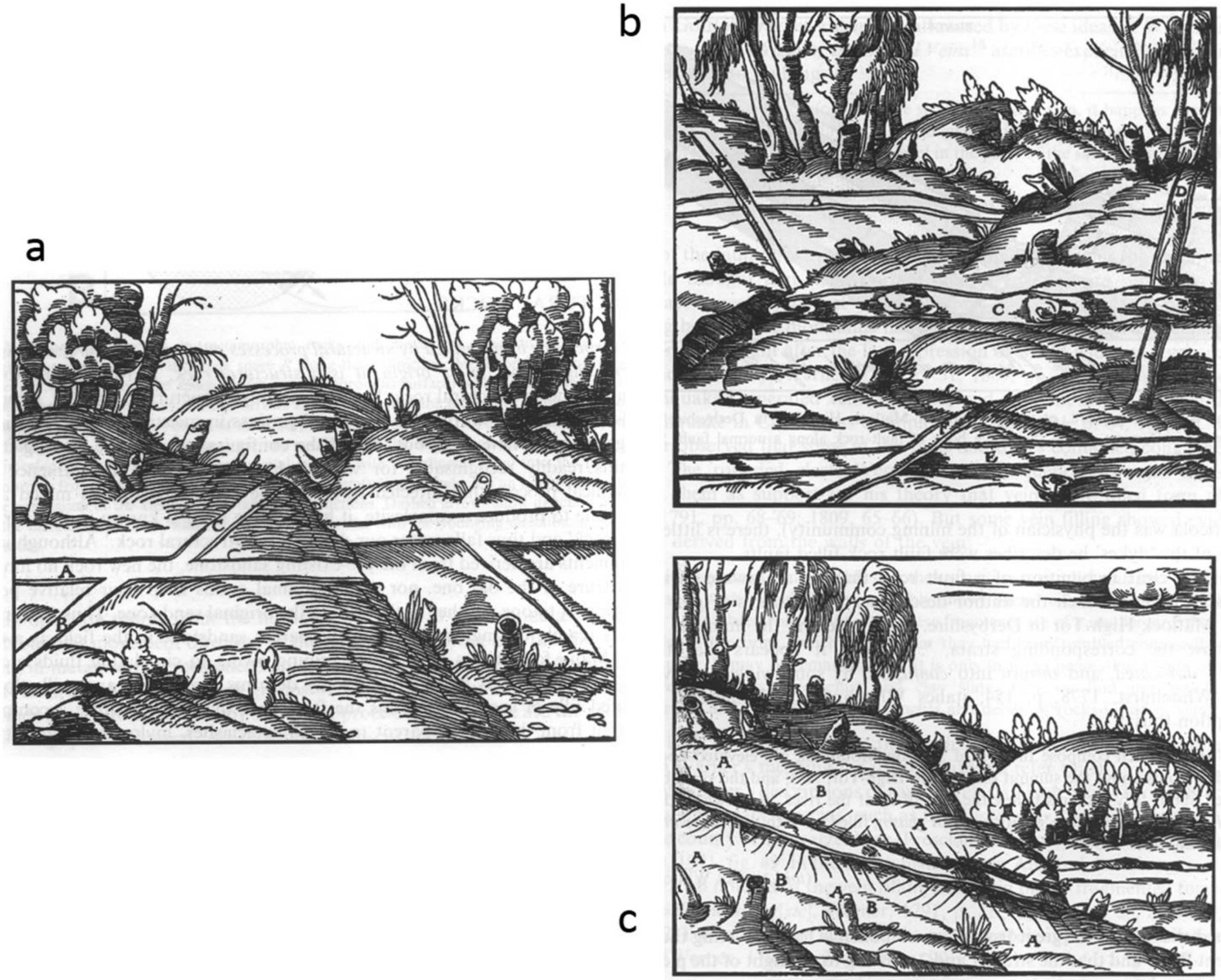


Figure 3. *a*, Vein or dike A displacing veins or dikes BC and BD. The country rock in which these veins or dikes intruded seem opaque with no perceptible structure. *b*, Various veins or dikes with different kinds of fillings. A and B are said to have "massive fillings." C and D have "druses," that is, geodes. E and F are said to be barren, that is, carrying no ores. Notice that except in *c* no structure whatever is depicted for the country rock into which the veins or dikes have intruded. Only in *c* do we see some joints, but again no beds. All these figures were copied from book 3 of Agricola's *De Re Metallica* ([1556] 1953).

herein). This E shell was what we might consider equivalent to our earth's crust. Descartes must have considered its formation a by-product of the "evaporation" of layer D, because the D elements, agitated by the heat of the sun, rose across the pores of the E shell and returned by night owing to the diminished temperature and the resultant loss of agitation. Not all of them were able to make it back to D, though, but got stuck in the pores of element E. Descartes probably thought that this is how an original E shell may have formed by the D elements that "evaporated" during the day and "precipitated" at night. That is possibly why he emphasized that the E shell was "layered." (Descartes must have been familiar with sedimentation, as he was also interested in medicine, having written *La Description du Corps Humain*; Descartes [1648] 1909; also see below.)

In any case, these excess D elements caused with time a space to form between D and E, which Descartes thought was filled by yet another layer, called F (fig. 2*a* herein), consisting of smaller particles than those making up the D layer. The E layer had more weight than the F layer. By continuous D element depletion, the F elements attempted to fill in the D pores left behind in the E shell, but their size was insufficient. Vigorous D influx then took place into the E pores to close them (Descartes repeats the old Aristotelian saying that was so popular during the Middle Ages and the Renaissance: nature abhors the void), but this only ended up enlarging them even more by the intensity of the impacts. Finally, the pores in the E layer became so large that it became incapable of supporting itself above the F and D layers. The E layer disintegrated and fell into the underlying fluids (fig. 1*b* herein). But since the radius of the E layer was much larger than that of the C layer onto which it eventually fell, parts of it had to form broken arches to fit into the lesser area below. Thus, Descartes thought, mountains formed (fig. 1*b* herein).

Steno and His Followers: The Invention of Structural Geohistory

Nicolaus Steno (Niels Stensen) had read Descartes's book. Although he criticized Descartes's scientific method (see Vai 2009*a*), he was much influenced by his geology. So when he was presented by the Grand Duke of Tuscany, Ferdinando II de' Medici, with a carcass of a great white shark (*Carcharodon carcharias*) in 1667 to study it, he was much struck by the great similarity of the animal's teeth with what was being sold in the pharmacies of Florence as *Glossopetrae*, that is, tongue stones. When he found out from the pharmacists that these objects had been collected from the rocks surrounding the city, he was puzzled as to how these solid objects had become embedded in rocks, themselves solid objects. So his problem became "how to get a solid object embedded in another solid object." Having read Descartes and having seen the rocks containing the *Glossopetrae*, which he thought were nothing but petrified shark's teeth, his problem became "how to get a solid object embedded in another solid object occurring in the form of beds." Here a previous bit of knowledge from his medical education helped him solve the problem. The physician Steno must have been familiar with the concept of sedimentation, which had been known in urine already in the Middle Ages (Anonymous 2005, p. 10). He, in fact, cites sedimentation in urine in his *Myologiæ Specimen* (Stenonis 1667, p. 102; for English translations, see Steno 1958, p. 20, 31; Stenonis [1667] 1969, p. 107). In 1659, the English neoplatonic philosopher Henry More also wrote about sedimentation in the ventricles of the brain:

> The Spirits in the Ventricles of the Brain, playing about and hitting against the sides of the caverns they are in, will in process of time abate of their agitation, the grosser parts especially; and so necessarily come to a more course consistency, and settle into some such like moist Sediment as is found at the bottoms of the Ventricles. (More [1659] 1987, p. 132)

Now, it is very likely that Steno was also familiar with this passage, not only because of his special interest in the anatomy of the brain (see Steno 1950; Scherz 1965; Faller 1981) but also because More was a fellow defender of Christianity against atheists.

Once Steno thought about sedimentation as a way of making layered rocks, his problem was solved. His discovery led him to propose three important rules of sedimentation for layered material. These are (1) the principle of original horizontality, (2) the principle of lateral continuity, and (3) the principle of superposition, which remain the foundation stones of lithostratigraphy to this day (Stenonis 1669, p. 29–30; for English translations, see Steno 1916, p. 229–230; Stenonis [1669] 1969, p. 165).

Lesson 2. Here we have our second lesson from history: to make observations a model is commonly necessary because it directs our attention to particular features. Steno learned from Descartes to see layers. His previous knowledge of sedimentation led him to formulate the basic rules of lithostratigraphy. His rules themselves were models.

That Descartes's model had no basis in reality was irrelevant because it was testable. Steno tested it with the model of sedimentation from medicine, which was based on observation. Thus, never discard a model because it seems nonsensical in the light of your previous knowledge, including your "common sense," as long as there is a way to test it.

But then Steno noticed that some outcrops violated his first and second principles, that is, his model of sedimentation. His genius was to recognize that angular unconformities meant secondary change of position of some layers, and he correctly ascribed that change to deformation subsequent to deposition.

Lesson 3. Our third lesson is testing our models. Do not give them up immediately whenever they fail. Try to see if some other, so far unconsidered process interferes. In Steno's case, the interference was deformation subsequent to sedimentation.

Steno's reasons for the cause of deformation were conventional: he thought it happened by subsidence into subterranean cavities, as required by the then conventional wisdom of the day (e.g., Kircher 1657, 1665). Subsidence took care of the deformation but not the source of renewed sedimentation. Here Steno took another conventional step: he ascribed the origin of sedimentation to the hypothesis of the earth's origin and evolution as propounded in the Old Testament. The earliest layers were laid down during the creation. The second set following the first episode of deformation were the products of Noah's flood, and the third set of layers, deposited after the flood layers, had been deformed, to the present-day alluvial processes (fig. 4 herein). This is the birth of what I call "structural geohistory" (Şengör 2009*c*; inspired by Powell and McGee [1888, p. 232], where a "formation" is called a "structural geological unit," and I think rightly). It taught geologists to infer temporal sequences from spatial relations seen in rock layers. It was originally independent of any causal hypothesis except the principles of sedimentation established by Steno. Steno then tied it to the Biblical earth history (Morello 2006; Vai 2007).[1]

1. The literature on Steno the geologist is vast. In addition to his own writings that I here cite, I add only Nicoletta Morello's and Gian Battista Vai's articles as the most recent and the most relevant to the subject of the present article. For those who want a more comprehensive introduction to this pivotal figure in the history of geology, I recommend Scherz's two-volume biography (Scherz 1986*a*, 1986*b*) and its thorough geological critique by Blei (1991).

The introduction of the idea of layering seen on outcrop had momentous consequences: almost immediately afterward people started seeing layers everywhere and depicting them with a view to elucidating the architecture of the mountains and indeed of the earth itself. It made geological observations concerning the structure of the ground communicable. A geologist looking at the drawings of strata by another geologist immediately saw what the structure the first geologist believed he was seeing was. Detailed field descriptions became citable and, thus, also testable.

In 1705, the Italian general and scientist Count Luigi Ferdinando Marsili depicted the geometry of the layers on both sides of Lake Uri (*Urnersee*) in the Swiss Alps (see Gortani 1930; Marabini and Vai 2003). His sections inspired his amenuensis Johannes Scheuchzer to create a structure map of Lake Uri, apparently on the basis of the observations and unpublished figures of Marsili, which first appeared in Scheuchzer's elder brother's *Helvetiæ Stoicheiographia* (1716, pt. 1, pl. 1; fig. 5 herein; see especially Vaccari 2003; Marabini and Vai 2003; Vai 2007; Şengör 2009*a*). Johann Scheuchzer himself composed a paper to be read before the Academy of Sciences in Paris on February 5, 1708, that remained unpublished until it was fortuitously rediscovered in the fifties of the twentieth century in which he had made no reference to his former employer's unpublished work on the same subject and the same geographic area for reasons that remain unknown to me (Studer 1863, p. 203–204; Hoeherl 1901; Koch 1952; Ellenberger 1995). As a consequence, Count Marsili's great contribution to stratigraphy and structural geology remained unrecognized until Michele Gortani reminded the learned community of its significance in 1930. The connection between Marsili and Johann Scheuchzer and the latter's influence on the former were mentioned by Koch (1952, p. 201), Fischer (1973, p. 83, 155 n. 1), and Vacari (2003, especially p. 180–181; 2010, especially p. 60–61) but still has not been appreciated by many historians of geology (e.g., Gohau 1982–1983, 1983, 1990, 2003; Broc 1991; Ellenberger 1994, 1995, p. 127; Felfe 2003, p. 11 n. 48; Kempe 2003).

In 1725, Marsili published a remarkable book in French, one of the earliest works on marine geology (Marsili 1725; see also Sartori 2003), in which he distinguished two kinds of seafloor: "le fond veritable de mer" (the real floor of the sea) represented the seafloor as it existed after the creation. It was thought to be the original surface of the globe. Above that "real floor" there formed another one called "le fond accidental de mer" (the

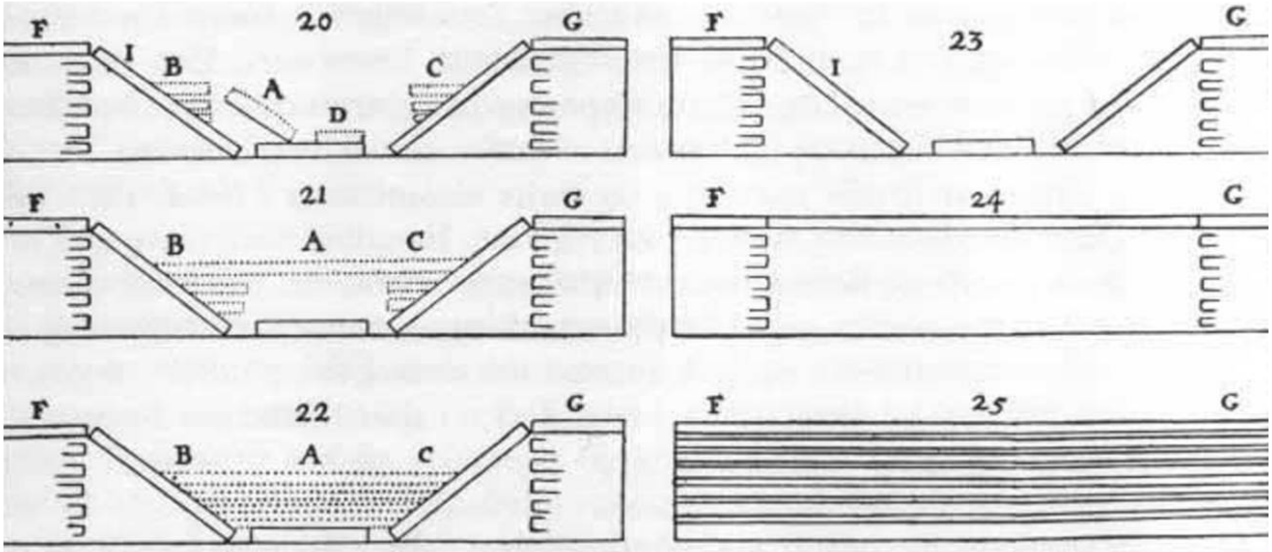


Figure 4. Steno's famous diagrams illustrating his hypothesis for the geological history of Toscana. Steno called each stage a "facies." The sequence begins in the lower right figure (labeled 25), illustrating the strata laid down during the creation of the earth. Figure 24 shows a situation in which the lower layers have been destroyed. Steno thought this could happen by either subterranean water or fire. Figure 23 represents a time when the topmost stratum, having been left without support, has collapsed. Figure 22 represents the time of Noah's flood. New sedimentary strata are thereby laid down. Now they abut at their edges against the previously deformed top layer, forming unconformities. Thus, the unconformities are evidence of deformation. In figure 21, we see that the flood layers are undermined, and in figure 20 the top flood layer has also collapsed (A) and new sediment (D) is laid down by present-day rivers and lakes (alluvium). Gould (1987) claimed that Steno's arrangement of his figures indicated that he had a cyclical evolution in mind. It is not true. Steno arranged his figures in the way he did because the space available to him in his foldout table forced him to do so. A cyclical evolution in time was what Steno was against. He claimed that earth's evolution was a unidirectional development as narrated in the Bible.

accidental floor of the sea). The accidental floor was younger and dependent on the history of sedimentation in any given area (Marsili 1725, p. 15). Let me emphasize that these two floors were defined on the basis of hypothetical processes: the first on the creation myth, the second on the hypothesis of sedimentation on the seafloor conceived on the basis of the dredge work south of the Rhône delta in the Mediterranean. Here again we have two models in Marsili's formulation of his two main seafloors: the Biblical model of the creation of the earth and the model of sedimentation as developed by Steno in a geological context.

Johann Jakob Scheuchzer used his friend's and his brother's figures sketched at the Lake Uri shores in discussing the geology of the Alps (Scheuchzer 1716, 1731), but he did something else: he depicted in two figures on a copper plate the deposits of the flood on the entire earth and the consequences of the deformation caused by the flood (fig. 6 herein). If Steno's ideas did not immediately give rise to the conviction of the worldwide correlatability of the sedimentary rocks, these figures did, and they had been inspired by the ideas of Steno and Count Marsili. Marsili's idea of the two seafloors gave rise, within less than two decades, to the idea of primary and secondary rocks in the book of his countryman, the Abbot Antonio Lazzaro Moro. In his book, Moro describes in great detail the growth of volcanic mountains by accumulation of material ejected from their craters. But he also points out that the previously formed layers, constituted by material ejected from volcanoes and lying between the "essential floor" and the "accidental floor," may be thrown up again by subterranean fire fed from material within the layers and form mountains: "And from this, mountains form, which we call *secondary*, and which consist entirely of strata" (Moro 1740, p. 274). While explaining his plate 8 (fig. 7 herein), Moro depicts wholesale uplifts (e.g., R, M, and P in fig. 7 herein) and depressions (H, N, and S) that deform the essential floor as well as the accidental floor. Moro's distinction between primary and secondary "mountains" rapidly led to a distinction between primary and secondary rocks, although his volcanic enthu-

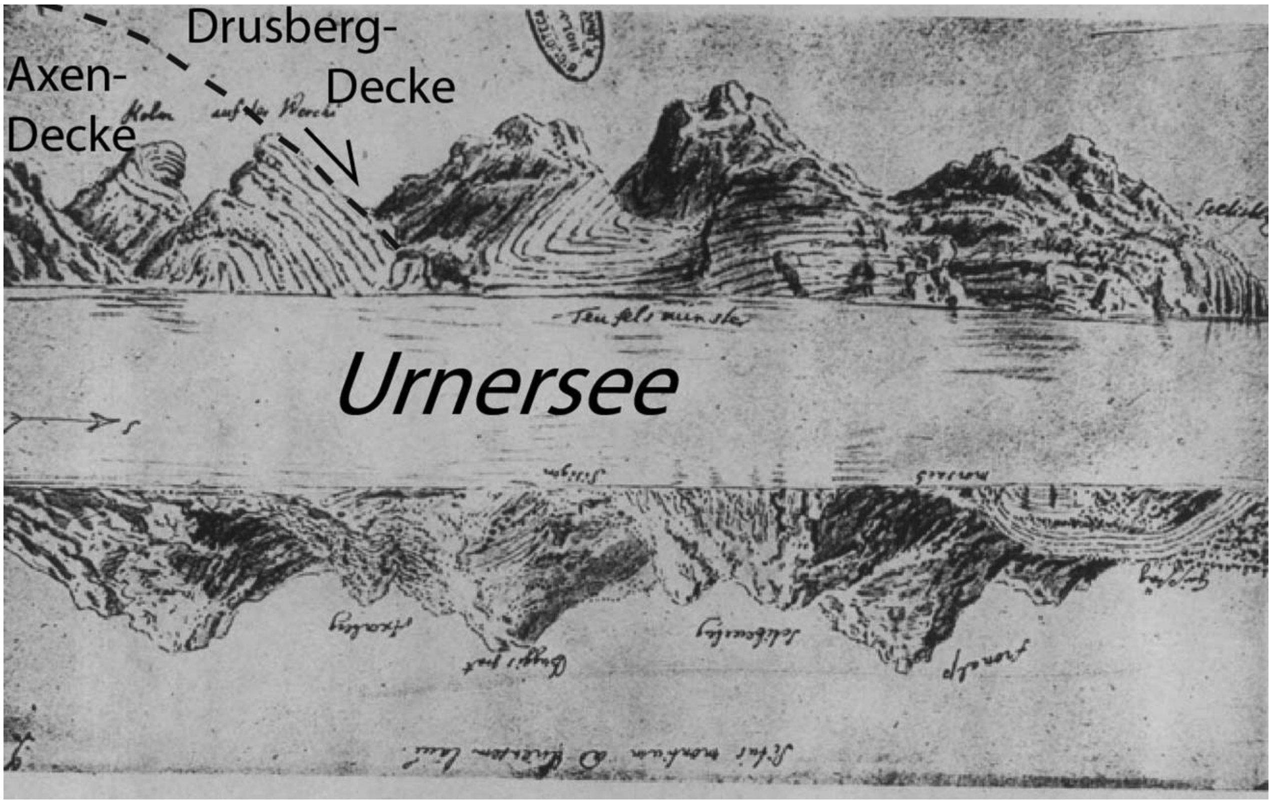


Figure 5. Count Luigi Ferdinando Marsili's unpublished figures of the shores of Lake Uri (the southernmost lobe of the Vierwaldstättersee), which he sent to his friend Johann Jacob Scheuchzer in Zurich. These figures depict the internal structures of parts of the Drusberg and Axen nappes of the Helvetic Nappe pile in central Switzerland (from Gortani 1930). Present-day depictions of the same place look no different. A color version of this figure is available online.

siasm, ascribing all rocks ultimately to volcanism, did not find much sympathy.

Moro's work led the German miner Johann Gottlob Lehmann to distinguish a *Ganggebürge* (literally, "dike mountains," by which he meant the steeply dipping Hercynian basement rocks of the Harz Mountains containing numerous igneous intrusions and ore veins) from a *Flötzgebürge* (literally, "rocks with flat-lying layers," commonly used for sedimentary rocks bearing economic minerals and coal). Lehmann followed Steno in ascribing the Ganggebürge to the creation of the earth and the Flötzgebirge to Noah's flood (Lehmann 1756).

Here we notice a great difference between Marsili's model and Lehmann's model: Marsili ascribed the real floor of the sea to the creation, but the accidental floor was ascribed to sedimentation and was emphasized to be of different ages at different places. The reason for this, I think, was Marsili's familiarity with the marine processes of our own days. He studied not only sedimentation in the Mediterranean but also the currents of the Bosphorus in Istanbul (see Soffientino and Pilson 2009), as opposed to Lehmann's experience with the products of "finished" processes.

Lesson 4. A fourth lesson we deduce from the history of geology is that those geologists studying the products of processes no longer active are more prone to generating models emphasizing discontinuities and erecting spatiotemporal correlations of rock packages inferred to be equally spatially and temporally extensive processes (such as Noah's flood, as in Lehmann's case). By contrast, those geologists studying active processes generally see continuity and shy away from extensive spatiotemporal correlations.

Şengör (1991) distinguished these two types as "miners" and "physiographers." We shall see the persistence of this difference into our own days, as exemplified, for example, by the differences in the interpretation of the record of sea level changes between the Exxon (representing the miner tradition) and the Lamont (representing the physiographer tradition) groups.

As a miner, however, Lehmann could hardly avoid recognizing intrusions—they had been known and

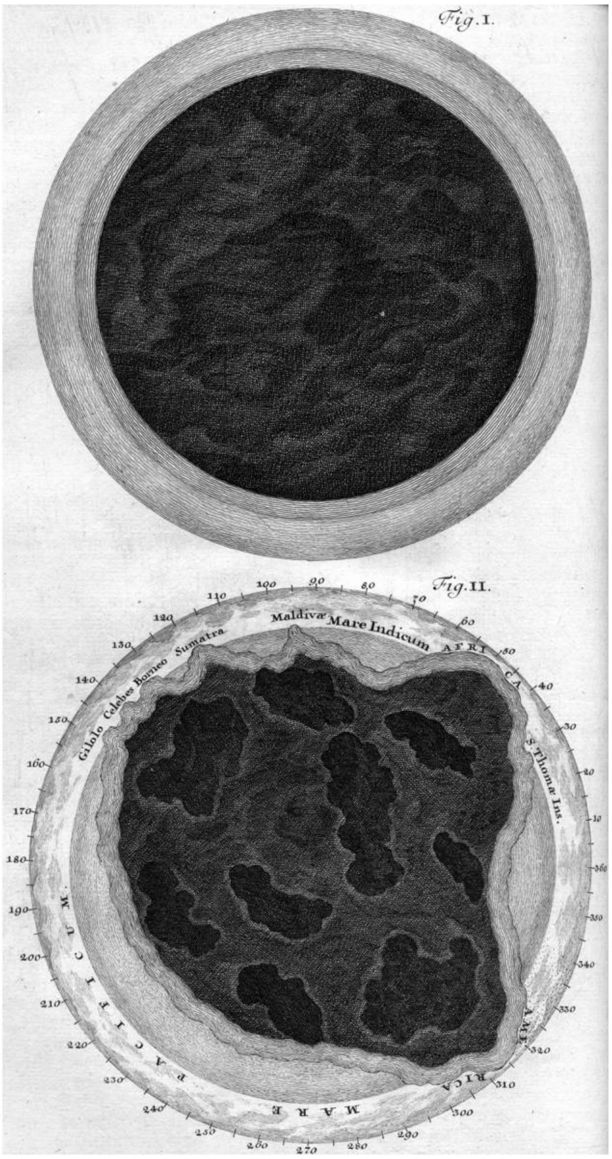


Figure 6. *Top*, Scheuchzer's picture (fig. 1) of the earth during the flood. Note the uniform cover of the waters and the sediment they deposited. *Bottom*, Scheuchzer's figure 2: the earth, already deformed by the flood during its retreat. Not only had void spaces originated within the earth (to which the flood waters had retreated), but the uniform flood sediments were also deformed. Into the depressions thus formed the present-day oceans (e.g., Mare Pacificum, Mare Indicum), and the seas settled. By contrast, the high-standing regions formed the continents (e.g., Africa, Asia). From Scheuchzer (1731), plate 45. A color version of this figure is available online.

illustrated since the days of Diodorus Siculus, Pliny the Elder, and Agricola. In fact, Lehmann had written an earlier book titled *Abhandlung von den Metalmüttern und der Erzeugung der Metalle* (*Essay on The Metal Sources and the Production of Metals*), in which he had written that

> The dikes that we find in mines appear to be only the branches and shoots of an immense trunk [stock = *Stokk* in Lehmann's original], which is placed at a prodigious depth in the bowels of the earth; but in consequence of its great depth, we have not yet been able to reach the trunk [Lehmann says that it is so deep that it cannot be reached at all]. The large dikes are its principal branches, and the slender ones its inferior twigs. (Lehmann 1753, p. 178; see also Werner 1791, p. 30)

Lehmann's metal book does not deal with global stratigraphy but later played a role in informing plutonist ideas. It was his 1756 book that does so, and it led not only to the physician and geologist Georg Christian Füchsel's continuation of his work but also to Werner's ideas, which finally completely derailed stratigraphy. It was in the 1756 book that Lehmann vehemently objected to Moro's ideas that the processes now generating rocks and geological structures have done so during the geological past and that every rock type could be generated at any one time during the history of the earth. Lehmann wrote,

> But his [Moro's] excuse is much worse when he says: nature operates so consistently in one way, with uniform and artless simplicity, that each individual natural event occurs in a certain way and through the cause destined for it and nature has behaved in other similar cases not differently from its present workings. This is easily said but not so easily proved. I shall prove to Mr. Moro the opposite. (Lehmann 1756, p. 47–48; see also Blei 1981, p. 254)

The idea that all the non- to little-deformed sedimentary rocks were laid down during the Noachian flood and that the earth behaved differently in the past from the present led to a regularist-catastrophist interpretation of earth history culminating in Werner's work with which a new kind of geohistory began, which I elsewhere have called "petrological geohistory" (Şengör 2009*a*).

Werner's Stratigraphy: Sequential Deposition on an Inert Earth and the Rise of Petrological Geohistory

Since Werner himself did not publish his theory of the earth except for a posthumously published short piece (Werner 1818), we are compelled to quote his students who state that they reproduce their teacher's material. For summaries of these, see

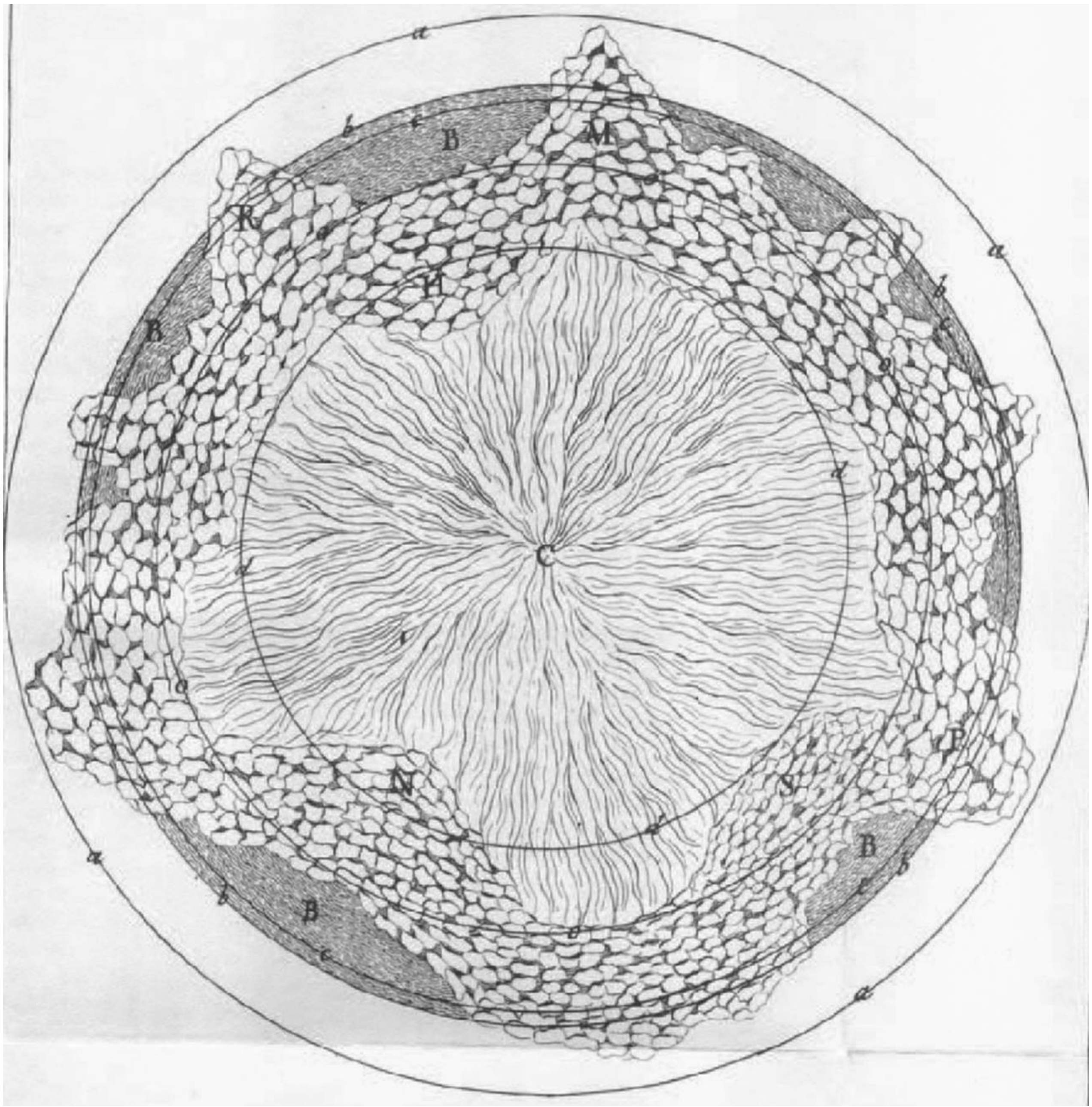


Figure 7. Lazzaro Moro's earth. From Moro 1740, plate 8. A color version of this figure is available online.

Bingel (1934) and, in part derived from Bingel, Ospovat (1960, 1971) and Wagenbreth (1967). I must concede, however, that we are still in need of a comprehensive account of Werner's geology. The summary below is a distillation of my own reading of a large section of the Wernerian literature.

Werner's earliest geohistory was hardly different from that of Lehmann (in fact, it was a step back from Füchsel, who had already posited more than one flooding episode in the history of the earth; Füchsel 1761). He originally postulated two main periods in the history of the earth: during the first main period, the world ocean deposited granites, gneisses, schists, marbles (called *Urkalk*, i.e., ancient limestone), and greywackes (fig. 8*a* herein). These rocks had steep dips in Upper Saxony, where Werner worked, and exhibited no regular bedding. We know now that these rocks correspond to the Hercynian basement deformed, metamorphosed, and intruded before the Permian. During the second main period, the *Flötz* rocks were deposited, consisting of various sandstones, limestones, and

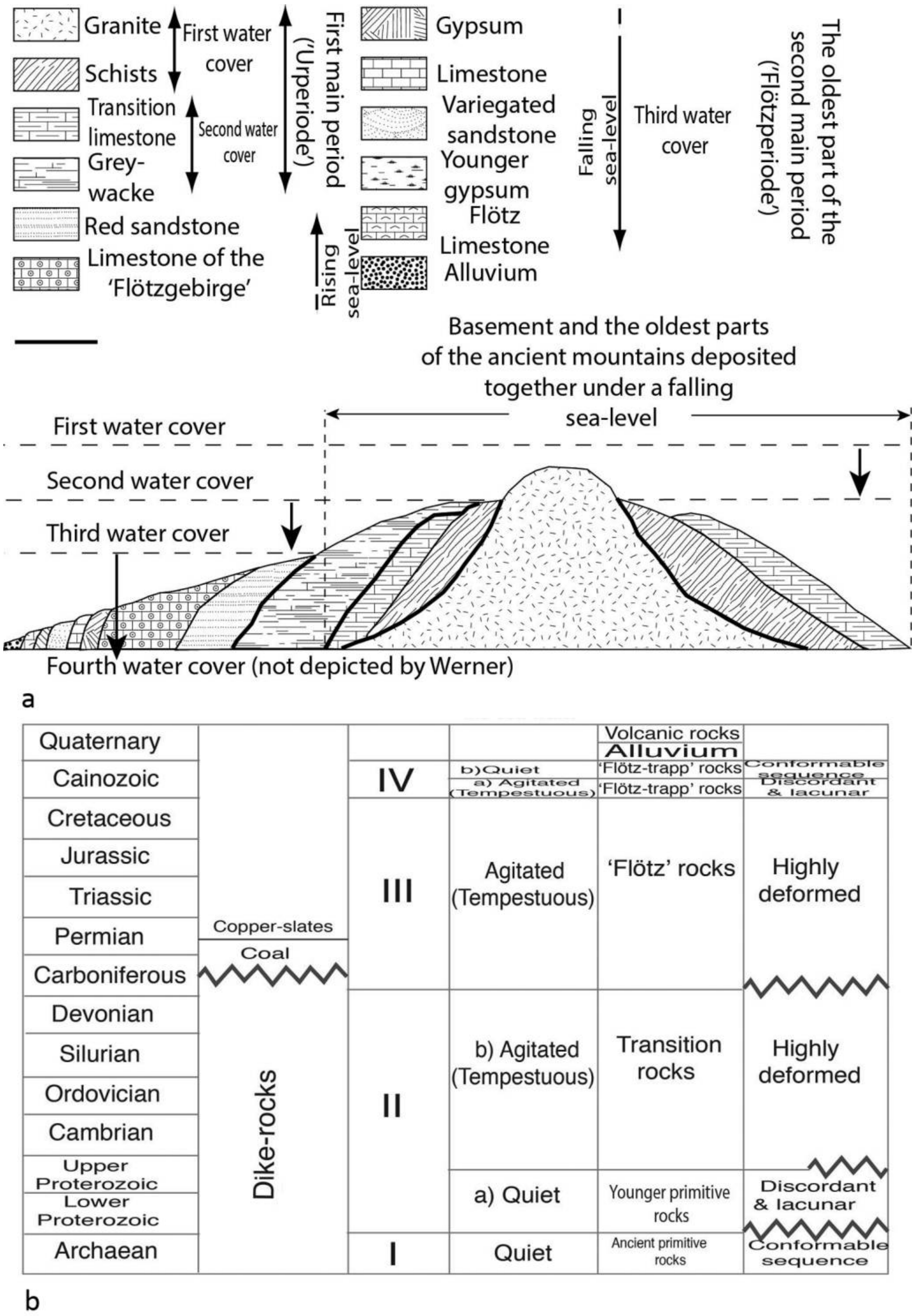


Figure 8. *a*, Werner's theory of earth evolution according to Bingel (1934). *b*, Werner's sequences according to Bingel (1934). The left column is the global stratigraphy according to Lehmann (1756); the column next to that shows the four main sequences of Werner. Next to that is the state of the sea that deposited the four sequences. Next to that are the rocks laid down, and finally the rightmost column shows the state of deformation caused by the state of the depositing sea (tempestuous vs. quiet).

evaporites with horizontal layers or those with very gentle dips. These rocks constitute what is now considered the cover sedimentary rocks in Saxony, beginning with the Permian and going up into the Cainozoic (fig. 8*a* herein).

Soon, however, Werner had to concede that this simple classification was no longer adequate, first because one found strongly deformed crystalline rocks that still appeared regularly bedded, and second because the same type of rocks appeared at different stratigraphic niveaux among the Flötz rocks. To deal with these difficulties, Werner postulated another period in earth history, the so-called Transition Period, during which greywackes and the transition limestones were laid down. Then he distinguished different categories within the Flötz sequence that belonged to the same composition groups, such as limestones or sandstones, but which occurred at different levels in the supposedly worldwide-applicable sequence of beds (e.g., limestone of the Flötz rocks and Flötz sandstone). All of these were tied to the oscillations of the sea level that had found itself in an overall regression (fig. 8*b* herein). Such repeated adjustments destroyed the original simplicity of Werner's classification of rocks corresponding to distinct episodes in the history of the earth, but his terminology nevertheless became widely adopted because it brought an artificial order to the chaos of rock occurrences and made teaching (and learning) easy. The most important legacy of Werner, which had come down to him from Steno and Scheuchzer via Lehmann and Füchsel, is the idea that individual layers of rock are supposedly correlatable worldwide. Not only were the rock types supposed to be correlatable but their structure was as well, since no serious deformation was supposed to have occurred since their deposition. One sees the influence of these ideas in the only purely geological work by one of Werner's greatest students, Alexander von Humboldt's *Essai Géognostique sur le Gisement des Roches dans les Deux Hémisphères* (1823*a*; *A Geognostical Essay on the Superposition of Rocks in Both Hemispheres*; 1823*b*), where the great geographer tried desperately to show that the deposition of the basement rocks created a universal strike direction that allegedly followed loxodromes!

By the time von Humboldt wrote his book it had already become clear to many that Werner's petrological geohistory simply did not work. But oddly this did nothing to shake the conviction that rock layers ought to be correlatable worldwide, very much as Steno and Scheuchzer already supposed. They had two events with two marine sequences. Werner (like Füchsel before him) simply increased the number of events and sequences.

At least once Werner did notice that certain fossils of former organisms were confined to certain beds, but he made nothing of it, despite the fact that in 1799 he gave a course of lectures on fossils (*Versteinerungslehre*) in the Freiberg Academy (Anonymous 1850, p. 10). It was reported by his friend Johann Friedrich Widemann with the following words:

> A very valuable observation, which I owe to one of my worthiest and dearest friends, the Academy inspector Werner in Freiberg, of his friendship I may be proud, very much increases the possibility of this idea. This great mineralogist observed in his birthplace Wehrau [now Osiecznica in Poland] in the Upper Lausitz in a clayey ironstone layer occurring in the lower Ziegelberg that the bed of this layer consists of a somewhat soft clay and a dense clayey ironstone, which alternate with one another many times, and is filled with many fossils, if I may name them, of marine animals. It is curious that according to this observation every layer has its own fossils. So, for example, in one bed only turbinites, in another only chamites, musculites, etc. This seems to me to prove that the sediments, of which this layer consists, were laid down at different times. (Widemann 1792, p. 117–118; for a geological map of the area mentioned, see Solecki and Śliwiński 1999)

Lesson 5. A fifth lesson we deduce from Werner's activity is the adverse influence of specialization. Werner was a miner and had little interest in the history of the organic world. This led him to miss an extremely important discovery, although he did make the relevant observation. This is a repetition of Martin Lister's (1671) and Giuseppe Baldassari's (1751, quoted after Edwards 1967, p. 36) earlier similar observations and similar failures to recognize the temporal significance of fossils.

The Birth of Biostratigraphy and the Tyranny of Strata: The Invention of Biological Geohistory

Hutton's Influence on Stratigraphy. Two important things happened during the last two decades of the eighteenth century that showed that Werner's theory of earth behavior (which he simply inherited from his catastrophist and regularist predecessors, such as Steno, Scheuchzer, Lehmann, and Füchsel) was entirely untenable. The first was James Hutton's revival of Moro's ideas (Hutton 1785, 1788, 1795*a*, 1795*b*). However, when comparing Hutton's work with that of Werner it is important to keep in mind that Hutton's problem was entirely different from Werner's. Hutton, when he was engaged in farming in Slighhouses in Berwickshire in the Scottish Borders (Southern Uplands) in the fifties and sixties of the eighteenth century, was interested in knowing why erosion did not remove the entire soil cover and reduce land to sea level. He needed a mechanism to replenish the available land to be eroded and naturally sought in the elevation of land the necessary cause. He first found angular unconformities showing that a previously deformed and uplifted package of rocks had then again been submerged and covered with sediment, which was then again uplifted. The uplifting agent he found in the internal heat of the planet, which occasioned uplift by means of intrusions (see, in addition to the above, Playfair 1802, 1805). If such local groups of processes repeatedly eroded, then uplifted, then submerged again and covered with sediment, and then uplifted again various pieces of the earth's surface, there could then be no hope of correlating sedimentary layers globally. This was fine when it came to accounting for the history of any given area, but not so if one wanted to create a global geological history because, in Hutton's world, no region could be tied chronologically to any other by any means. This was disturbing for miners because they had hoped to establish a global stratigraphy to enable them to identify areas where they could most hopefully prospect for minerals. Neither was Hutton's theory congenial to those who had hoped to save whatever could be saved from the Biblical earth history (e.g., de Luc 1798, 1809*a*, 1809*b*). There were also those who opposed Werner or Hutton on the details of local geology, as exemplified by the controversy between Werner and his former student Johann Karl Wilhelm Voigt (for a very detailed account of this famous controversy and all the references to the original literature, see Wagenbreth 1955); the anti-Hutton writings of Robert Jameson, a Werner student in Scotland (e.g., Jameson [1808] 1811; here he disputes certain local points against Hutton's popularizer Playfair; see also his general statement in 1808); the Edinburgh surgeon James Murray (1802); or indeed Richard Kirwan (e.g., Kirwan 1799; for a fine account of Hutton's work and its impact, see Dean 1992, 1997). No one seems to have deemed it necessary to point out that what Werner was doing was looking at the finished products of geological processes and making inferences from them about the behavior of the entire planet with a view to predicting the sequence of beds anywhere, while Hutton was studying the record comparatively with the active processes to make inferences as to what sort of a geological record they might produce locally with a view to finding clues about the behavior of our planet. This difference in ultimate purpose might account for Lyell's dislike of global processes. This is an important point to bear in mind while judging the geological hypotheses of their successors. Here again we see the fundamental difference in the way earth history is interpreted by the members of the miner and physiographer traditions.

Cuvier and the Rise of Biostratigraphy. While these two groups were busy arguing whether Neptunism or Plutonism was a correct description of the behavior of the globe without paying much attention to the contexts in which these two theories had been conceived, Georges Cuvier and Geoffroy St. Hilaire in Paris showed in a short abstract published in 1795 that the mammoth, the Asian elephant, and the African elephant were not of the same species of elephant (Cuvier and Geoffroy 1791–1799). Their study had arisen from a desire to test Buffon's and Pallas's theories of earth history. Buffon (1778) had assumed that the earth had an incandescent beginning and had begun to cool from the poles toward the equator. He had postulated that life had been created at the poles and that as the poles cooled it migrated southward. One of the pieces of evidence he had cited was the mammoth cadavers in Siberia, which he had considered those of Asian elephants. Pallas ([1778] 1986), by contrast, thought that the mountain ranges and island arcs of southern and southeastern Asia had been uplifted with such speed and violence that a giant tsunami had swept northward, carrying the carcasses of the unfortunate victims and dispersing them on the plains of Siberia. Cuvier and Geoffroy showed that neither Buffon's nor Pallas's theories could possibly be right by proving that the mammoth and the Asian elephant were not the same animals. But with that discovery had come up a new question: what had happened to the mammoth? The answer was obvious: it had become extinct. This was the first time a convincing proof of extinction of animals was presented to the world scientific community.[2] At the time, no hu-

2. Extinction of organisms has long been on the human agenda. Many myths contain stories of giants and strange creatures that once populated the earth and that have since vanished. Bernard Palissy has hinted at it in a geological context, but Robert Hooke, in his well-known *Lectures and Discourses of Earthquakes and Subterranean Eruptions Explicating the Causes of the Rugged and Uneven Face of the Earth; and What Reasons May Be Given for the Frequent Finding of Shells and Other Sea and Land Petrified Substances, Scattered over the Whole Terrestrial Superficies* was the first to consider extinction in a scientific context (Hooke 1705, p. 291), but it is commonly

man fossils had been found together with the mammoth fossils and with other Quaternary fauna, such as the wooly rhinoceros (*Coelodonta antiquitatis*; originally *Rhinoceros lenenesis* Pallas 1769) found along the River Vilyuy in northeastern Siberia by a Jakutian hunter, Roman Boltunov, and reported to the official Ivan Argunov, who sent it to the governor Lieutenant General Ritter von Brill in Irkutsk, who then gave it to Pallas (Pallas 1776, p. 97–98; see the first report [Pallas 1769] for scientific details); and the giant elk (*Megaloceros giganteus*), first described by Thomas Molyneux in 1697 but first identified by Cuvier (1812*a*, p. 8–25 of the first article) as an extinct animal. Cuvier postulated that some grand cataclysm must have wiped out the mammoths and their contemporaries and that human beings had been created afterward. As the mammoth cadavers were extremely widespread, from Siberia to Europe and North America, Cuvier thought he could draw a time horizon between the mammoth and human fossils that would be valid globally (fig. 9*a* herein).

Cuvier later stated in his partial autobiography that an article he published in 1796 was the first place in which he mentioned his views on extinct animals. The earlier 1795 announcement was only a short abstract, and it is indeed true that the great anatomist first made his views public about the existence of a world anterior to ours that had vanished through some great catastrophe in his famous 1796 lecture. It was with the following clear words that Cuvier had announced his theory:

> But one science that at first seems to have not much of a close relation with anatomy, that which deals with the structure of the earth, which collects the monuments of the physical history of the globe and seeks to trace in a daring way the table of the revolutions which it has proven; in one word, geology, could establish in a sure manner the various facts serving as its basis only by the aid of anatomy.
>
> Everybody knows that in Siberia, in Germany, in France, in Canada, and even in Peru, bones of enormous animals are found in the earth that cannot belong to any of the species that now live in these climates.
>
> For example, one has found them in all the northern parts of Europe, Asia, and America, which resemble the bones of elephants and the texture of their ivory that of their defenses to such a degree that all scholars have taken them to be as such to this day. Others seem to be bones of rhinoceros and indeed approach them much. Now there are neither elephants nor rhinoceros in the torrid zone of the Old World. How come are their cadavers found in such great numbers in the northern parts of the two continents?
>
> On this issue the conjectures are a dime a dozen: some suppose that vast inundations had transported them there; others that the peoples of the south had taken them there during large military expeditions. The inhabitants of Siberia believe in good faith that these bones belong to a subterranean animal similar to our mole, which never allows itself to be taken alive; they call it *mammoth*, and the horns of the mammoth, which resemble ivory, form an extremely profitable branch of commerce.
>
> None of these could satisfy an enlightened mind. The hypothesis of Buffon would have been the most plausible, had it not been combatted by reasons of a different type. To him, the earth, detached as an incandescent mass from the sun, had begun to cool by the poles. It was there the organic nature began. The first-formed species, those that most needed the heat, were chased toward the equator as the refrigeration progressed. Having thus crossed all the latitudes, there be nothing surprising in the fact that one finds their remains all over the place.
>
> A close examination made by anatomy of these bones has taught us that they are never so similar to those of the elephants as to be regarded as belonging to the same species, and we can thus dispense with all these explanations. The teeth and the jaws of the mammoth do not at all resemble those of the elephant. A glance at the same parts of the animal of Ohio will suffice to show that it is still farther removed.
>
> These animals would have differed in the same way and more from the elephant as the dog does from the jackal and the hyena; considering that the dog tolerates the cold of the north, while the other two live only in the south, it could have been the same with these animals, of which we know only the remains.

claimed that Baron Georges Cuvier was the first to prove the extinction of former species. This is not true: the first person to do so was a medical student in Germany, Johann Christian Rosenmüller (1771–1820), in his dissertation titled *Quaedam de Ossibus Fossilibus Animalis Cuiusdam, Historiam Eius et Cognitionem Accuratiorem Illustrantia*, presented to the University of Leipzig in 1794. In this thesis, Rosenmüller identified the fossil cave bear as a separate species and named it, according to the rules of the Linnean binomial classification, *Ursus spelaeus*. This was the first time a new species was identified on the basis of comparative anatomy performed on fossil material and named according to the nomenclatorial rules for living animals. A year later, Rosenmüller published a German version of his thesis (Rosenmüller 1795). For a remarkable history of this discovery and Rosenmüller's work, see Rosendahl et al. (2005). It is, however, true that it was Cuvier's work that made extinction an accepted fact of earth history. The first person to use a binomial nomenclature for both living and fossil animals seems to have been the Italian polymath Ulisse Aldrovandi in his *Musaeum Metallicum* of 1648 (Vai 2003*a*, p. 93).

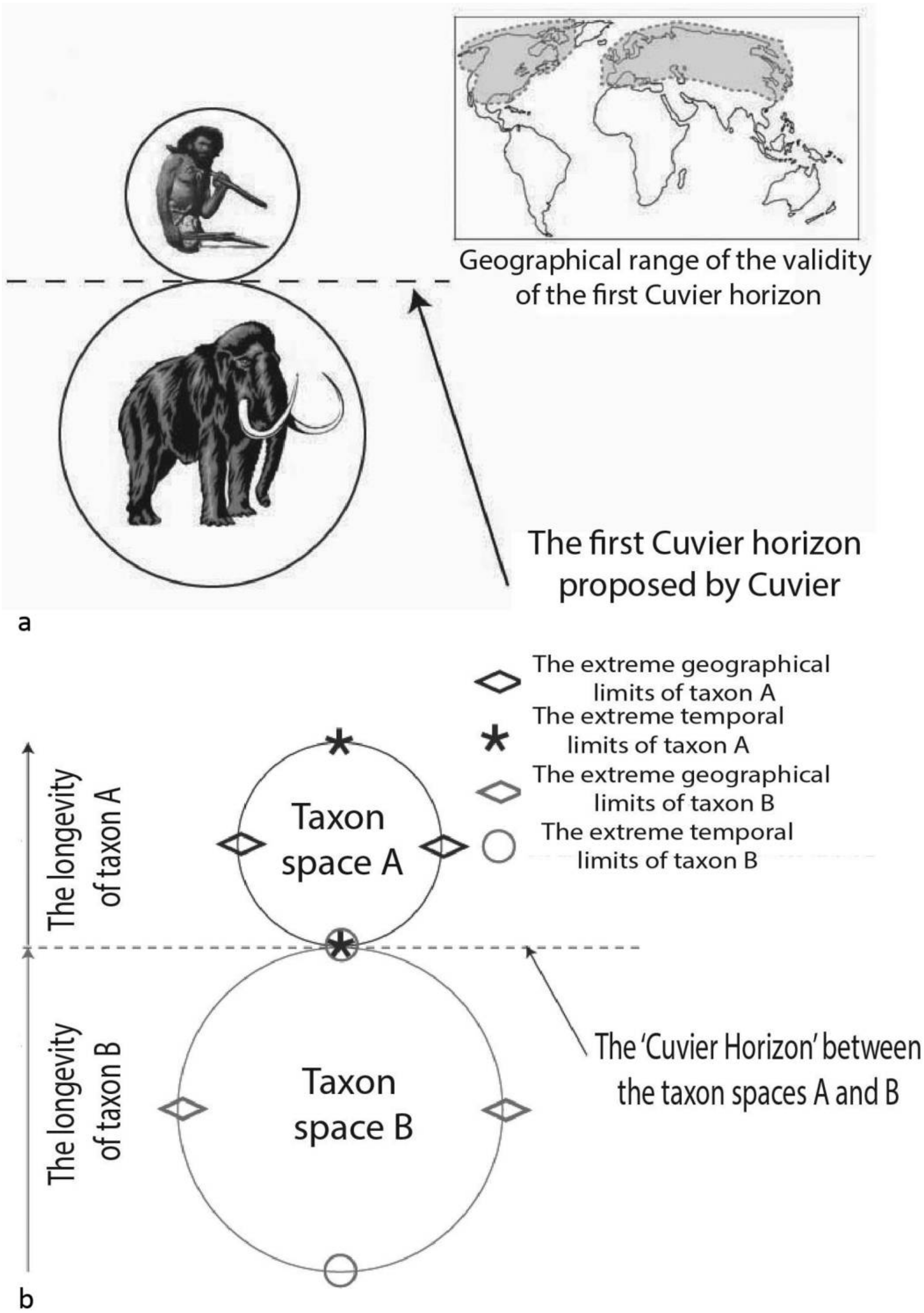


Figure 9. *a*, The first Cuvier horizon ever drawn. Cuvier never illustrated it as it is done in this portrayal, but his descriptions in his many publications make it clear that this is what he had in mind: a horizon between two range zones. *b*, Cuvier horizon separating the taxon spaces of two taxa. The four-dimensional taxon spaces are usually mapped into rocks as three-dimensional range biozones. Regrettably, biostratigraphers show biozones as two-dimensional sticks, forgetting that they also have a third dimension that is crucial for stratigraphy. Şengör and Sakınç (2001) pointed out that biostratigraphic zones are best shown by Venn diagrams as sets.

> But in dispensing with the necessity of admitting a gradual refrigeration of the earth, in distancing the pathetic ideas presented to our imagination by the ice and the frost of the north invading the presently so cheerful countries, into which novel difficulties would we be thrown now by these new discoveries?
>
> What has become of these two enormous animals of which one finds only the vestiges, and of others of which the earth offers us all over the place their remains, and of which none probably exists today? The rhinoceros of Siberia is entirely different from all the known rhinoceri; the same applies to the alleged *bears* of Ansbach,[3] to the fossil *crocodile* of Maastricht,[4] to the species of *deer*[5] of the same place, to the twelve-foot-long animal without incisors and with digits armed with claws,[6] of which a skeleton was discovered in Paraguay: none with a living analogue. Why, finally, is there no petrified human bone?
>
> All these facts, analogous to each other, and against which no established fact can be brought, seem to me to prove the existence of a world anterior to ours, destroyed by some catastrophe.
>
> But what was that primitive earth? What was this nature not subordinated to the empire of man? And what revolution could have annihilated it to the point of leaving only the semidecomposed bones as traces.
>
> It is not for us to enter into the vast field of conjectures these questions present. The more audacious philosophers have entertained them. The modest anatomist, called to detailed investigations, to scrupulous comparison of the objects submitted to his eyes and to his scalpel, is content with the honor of having opened this new path to the genius who will dare to tread it. (Cuvier 1796, p. 442–445; for an independent translation, see Rudwick 1997*a*, p. 21–24)

The idea that fossils delineate time horizons is thus expressed in a language the clarity of which leaves nothing to be desired. This is what I elsewhere called "biological geohistory" (Şengör 2009*a*). The great anatomist not only pointed out that the extinct animals provide an upper time limit for the world in which they lived, but by mentioning (as it later turned out, incorrectly) the absence of fossil human bones[7] he stressed that the present fauna

3. In cave deposits in Bavaria. See Rudwick (1997*a*, p. 22 n. 17).

4. Cuvier soon identified this animal as being a sea lizard, related to monitors. It was in fact the first *Mosasaurus* fossil ever found (Rudwick [1997*a*, p. 24 n. 18] wrongly attributes the name *Mosasaurus* to Cuvier; it was in fact coined by William Conybeare, in Parkinson's *Outlines of Oryctology* [1822]; see de Graaf and Rompen 1995; Bardet and Jagt 1996).

5. Rudwick (1997*a*, p. 24 n. 18) points out that in reality the "deer antlers" were remains of Cretaceous marine turtles, which Cuvier correctly identified once he saw the fossils himself.

6. This is the animal Cuvier was soon to call *Megatherium*, the giant sloth (see Rudwick 1997*a*, p. 25–32).

7. The absence of human bones in strata containing remains of extinct animals was hotly defended by Cuvier, and after his death the debate lasted until the great discoveries in Milhill cave, Brixham, in 1858! Here William Pengelly, a former champion of John MacEnery, who had dug Kent's Hole to find human implements mixed with fossils of extinct mammals, had supervised the digs. Pengelly had led Sir Charles Lyell through the excavations by the time the latter read his presidential address to the Geological Section of the British Association at Aberdeen. In this lecture, Lyell reversed his opinion on the absence of human fossils mixed together with those of extinct mammals expressed earlier in his presidential address to the Geological Society in London in 1851 (Pengelly 1897, p. 76–77; also see Bailey 1962, p. 181–183). Lyell's reluctance to accept the existence of human bones mixed with those of extinct animals stemmed from his religious desire to keep humans a special creation, despite the fact that the Bible expressly mentions antediluvial humans, Methuselah and his grandson Noah being the best known. Cuvier has often been similarly accused. I contend here that Cuvier's motives to keep humans an entirely postdiluvial animal stemmed not from his religious convictions that God had specially created humans after the last great catastrophe had wiped out the antediluvial world but from his desire to make biostratigraphy a neat tool to subdivide earth history. We shall see below that, in a similar vein, his pupil Alcide d'Orbigny "revised" certain fossils to make sure that they occurred in only one of his stratigraphical stages and none other to keep his classification neat. Fossil man's appearance in the rock record provided for Cuvier a lower limit to the postdiluvial deposits, while the disappearance of the antediluvial fauna provided an upper limit for the diluvial deposits. The heat of the debate in scientific circles was fueled by stratigraphical (and thus historical geological, and thus geodynamical) considerations, and the debate raged significantly between those whom I have elsewhere identified as the predecessors of the catastrophist/regularist school (Şengör 1982*a*, 1982*b*, 1991, 1996, 2000), who believed that no humans could have coexisted with antediluvial animals, and those who were the predecessors of the actualist/irregularists, who believed in the possibility of an antediluvial human fauna. This is clearly shown by the conversion of Lyell, at the time of the debate the leader of the latter school, despite his religious convictions. Jules Verne provided possibly the best outline of the character of the fundamental nature of the debate as conceived by the enlightened public in the mid-nineteenth century, from the mouth of the hero of his *Journey to the Centre of the Earth*, Professor Otto Lidenbrock, although regrettably introducing it with what later turned out to be forgeries of stone axes and, in one case, a human jaw bone, introduced by workmen into Boucher's de Perthes pits in the quarries of Moulin-Quignon, near Abbeville, in the department of the Somme (see Bailey [1962, p. 183] about the forgery). Professor Lidenbrock appears in Verne's novel not as a paleontologist but as a mineralogist/geologist interested in the evolution of the earth, an enthusiastic opponent of the views of Élie de Beaumont, and it is from that angle that he rejoices on stumbling on a human skeleton, and later a live specimen, contemporaneous with mastodons! (See Verne 1867, ch. 38.) Verne's grouping of Élie de Beaumont with Cuvier underlines the issue at stake. Verne (himself a religious man, indeed an "intelligent, but orthodox Roman Catholic," especially before 1870; later his religious views broadened to embrace a sort of pan-Christian deism; Verne 1976, p. 9; also see p. 36, 206) puts not one word about the religious implications of antediluvial

provides a lower time limit for the present world. Both of these limits he thought coincided and provided a time horizon that demarcated, in the rock record, the present world from a former one. Such a horizon, separating precisely two what are now called range zones, Şengör and Sakınç (2001) termed a "Cuvier horizon" (fig. 9 herein). Cuvier's particular examples turned out to be unfortunate, and he has been much abused later by historians of geology for having got his facts wrong, but those historians have thus only betrayed their misunderstanding of the nature of scientific progress and of the momentous importance of Cuvier's idea of fossils providing timelines for our investigations of the past (see Stephen Jay Gould's elegant words in defense of Cuvier in his foreword in Smith 1993).

In 1798 and then 1799, Cuvier again published on the fossil elephants and their geological implications more extensively, stressing the confinement of these fossils to barely indurated strata—clearly the deposits of the last convulsion of the earth, as quoted extensively above. Therefore, Cuvier knew, at the latest by the date of his initial joint abstract in 1795, that some animals had vanished from the face of the earth and, by 1796, that this was related to some worldwide event (*process!*) that also affected the rocks in which the fossils represented embedded timelines.

Cuvier thus made known that some fossil animal species had temporal ranges[8] confined by definite limits reflected in their record entombed in rocks and that this had had physical causes that had been nearly universal in effect. When he joined Brongniart in producing the mineralogical geography of Paris (Cuvier and Brongniart 1808, 1811), Cuvier did not think he was doing anything novel in principle because he was simply using a method he had already enunciated in 1796. He was conscious of the fact that he had introduced into geology an entirely new method of dating and correlating rocks. This is obvious from the text of the report he presented to the Academy of Sciences at the Louvre on August 11, 1806, on Father André Chrysologue de Gy's *Théorie de la Surface Actuelle de la Terre* (1806):

Some of the principal objects which appear to us necessary to be profoundly studied, in order to make geology a science of facts, and before attempting, with any hope of success, to answer the grand problem of the causes which have reduced our globe to its actual state. To this end we ought,

1st, To search if the division of great chains in one middle crest, and two orders of lateral crests, observed by Pallas, and developed by de Luc, is invariable, and examine, as Mr. Ramond has done in the Pyrenees, the causes which sometimes mask them.

2ndly, To examine if there is also any thing certain or uniform in the succession of secondary strata,[9] if such a kind of stone is always below such another, and vice versa.

3rdly, To operate in a similar manner on the fossils, determine the species which appear first, and those which come only later; discover if these two sorts never accompany each other, if there are any alternations in their appearance; that is, if the first found appear a second time, and if the second have then disappeared.

4th, To compare the fossil with the living species more rigorously than has hither been done, and determine if there is any relation between the antiquity of the beds, and the similarity or dissimilarity of fossils with the living beings.

5th, To determine if there is any uniform relation of climate between fossils and those living beings which most resemble them; as, for example, if they migrated from the north to the south, the east to the west, or if there have been mixtures and irradiations.

6th, To determine what fossils have lived where they are now found, what others have been transported there, and if there are, in this respect, uniform rules with regard to the strata, species, or climates.

7th, To follow in detail their different strata throughout their whole extent, whatever may be their doublings, inclinations, ruptures, and slopings; and also to determine what counties belong to one and the same formation, and what others have been formed separately.

8th, To follow the horizontal beds and those which are inclined in one or different ways, to determine if there is any relation between the greater or less constancy in their horizontal position, antiquity, or nature.

9th, To determine the valleys in which the reentering and salient angles correspond, and those in which they do not; also those in which the strata are the same on both sides, and those in which they differ, in order to discover if there

humans into the mouth of his hero during his enthusiastic subterranean lecture about the significance and history of the discovery of antediluvial humans but heavily underlines his significance for the geological history of the globe.

8. At the time, Cuvier mentioned only the upper limits. If anyone is inclined to think that he might have assumed all animals to have the same lower limit, namely, the creation (e.g., Bourdier 1969, especially p. 41), we may perhaps remind the reader that his insistence that no fossil human bones had been found shows that he was indeed aware of different lower limits exhibited by different animal groups.

9. Not to be confused with the Mesozoic. Cuvier's secondary comprises roughly Carboniferous to Cretaceous strata.

> is any relation between these two circumstances, and if each of them taken apart has any analogy with the nature and antiquity of the strata composing the heights which limit the valleys. (Cuvier et al. 1806[10] in Chrysologue de Gy 1806, p. 326ff.)

In 1808 and then again in 1811, Cuvier studied the geology of the Paris Basin on the urging of his friend Alexandre Brongniart, who at the time was the director of the Sèvres Porcelain Factory and in that capacity had been studying the geology of the Paris Basin. The two friends justified their joint study with the following words:

> The region in which this capital is situated is perhaps one of the most remarkable that has been observed yet, by the succession of diverse terranes that make it up and by the extraordinary remains of ancient organizations it harbors. Thousands of marine shells with which shells of fresh water alternate regularly make up the principal mass; bones of terrestrial animals entirely unknown, even by genus, fill certain parts; other bones of species noticeable by their large size of which we find some relatives only in very distant countries are distributed in the most superficial beds; a very marked character of a past irruption of the sea from the southeast is marked in the forms of the capes and in the directions of the principal hillocks; in brief, there is no other place that can instruct us better on the last revolutions that terminated the formation of our continents.
>
> However, this terrain has been little studied from this viewpoint, and although for a long time it has been inhabited by considerably educated men, what has been written on it is confined to a few fragmentary essays and always of a purely mineralogical character, without any regard to the organized fossils, or of purely zoological, without any attention to the position of these fossils. (Cuvier and Brongniart 1811, p. 1–2; for an independent translation, see Rudwick 1997*b*, p. 133)

Having discovered such an important method of dating and correlating rocks, one would expect Cuvier and Brongniart to use it in their work as the sole basis of correlating the rocks filling the Paris Basin. Figure 10 herein is a part of their cross section, on which I highlighted their main units and their correlations. What Cuvier and Brongniart in fact did was to correlate beds, not fossils! Why? Because what we today call biostratigraphic zones cannot be mapped in the field with any certainty, as already emphasized by Powell and McGee (1888). Only bed boundaries are visible to the geologist. Using Cuvier's method, Cuvier and Brongniart dated the beds and then drew the beds on their map and correlated them while thinking they were correlating time units expressed by the fossils. This is what I call the tyranny of strata. Cuvier and Brongniart, like countless other geologists following them to our own day, had become a victim of the tyranny of strata because they are the easiest things to be seen and correlated, not fossils. A paleontological stratigrapher hopes to establish the spatial relationships of biostratigraphic zones. In reality, all he can do is to establish the spatial relationships of beds containing those zones. The question of how much a "taxon space" (Şengör and Sakınç 2001; Şengör 2009*d*) is represented in a biostratigraphic zone can never be answered because preservation is never complete. That is why the recommendation to formalize the names of the geological periods as recommended in the successive editions of the *International Stratigraphic Guide* (Hedberg 1976; Salvador 1994) is based on a fallacy (see especially Harland 1977, p. 232; Holland et al. 1978, p. 7). Unfortunately, the suggestion by Zalasiewicz et al. (2004) to simplify the stratigraphic terminology pertaining to time by ending the distinction between the chronostratigraphic and geochronologic units, while justifiably underlining the confusion created mainly by the American stratigraphers, offers no remedy because it does not point out that the root of the problem is the tyranny of strata. It should be made clear that we can know where a geological system begins and where it ends because we define it on the basis of rocks and their content of fossils or dateable minerals and even rock types (which is unfortunate). But we can never know when a geological period began and when it ended because it is an elusive concept, subject to endless revision as more localities are dated by ever-improving methods of dating. It will not do to try to get out of this difficulty by saying that "a geological period is the time in which its corresponding system

10. This text was signed jointly by Lelièvre, Haüy, and Cuvier, but Cuvier had written it alone and read it (Coleman 1964, p. 113; also see Smith 1993, items 209 and 272, and p. 234, where Smith pointed out that "Cuvier was presumably the author of those reports for which he served as reporter," as was the case here). The text of my quotation is from an anonymous English translation published in 1809 in the *Annals of Philosophy* (vol. 33, p. 315–316), which I altered in a few places to make it correspond to the original better. See Rudwick (1997*a*, p. 106–108) for an independent translation; Rudwick does not refer to the earlier anonymous translation that I had been using in improved form as reading material for my classes in Istanbul since the early nineties. The report was also published in 1807 in *Journal des Mines* (vol. 21, p. 413–430) and in the *Procès verbaux* of the *Académie des Sciences* (1913, 3 [1804–1807], p. 408–413).

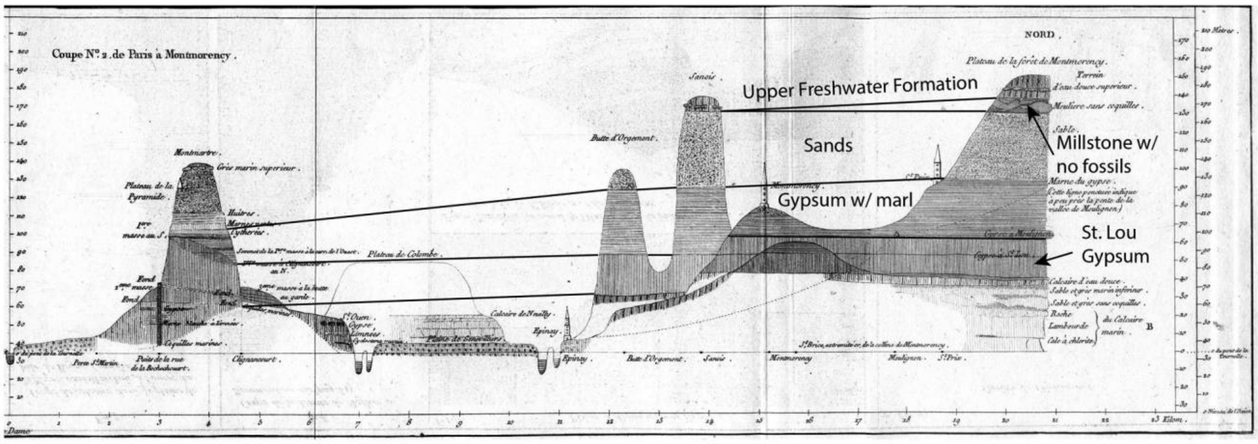


Figure 10. Correlations made by Cuvier and Brongniart in their 1811 book (pl. 1, fig. 2, sec. 2). Notice that what is mapped and what is correlated are rock types and beds, not age groups. Cuvier and Brongniart believed that these bed groups corresponded to distinct time intervals. A color version of this figure is available online.

of rocks were deposited" simply because the time correspondence of the rocks are subject to constant change as geological exploration of the globe advances and our dating methods are improved. In other words, rock is not time.

Now let us see how the lack of appreciation of the simple fact that rocks and/or fossils do not represent time led to very grave errors that haunt geology to our own day. In the famous *Discours Préliminaire* he placed at the beginning of his great work on fossil bones, Cuvier reviewed the ideas put forward to explain what we see in the geological record and concluded that what had happened in the past was not what we see going on today and that the thread of the operations of nature had been somehow cut (Cuvier 1812*b*, p. 16–17; also see Cuvier 1825, p. 27–28). Great catastrophes had allegedly destroyed landscapes and created new ones during which the fauna and flora living on the former landscapes had been supposedly wiped out and new ones had appeared to inhabit the new landscapes created by the convulsions. Cuvier never said in any scientific publication what the nature of such catastrophes were or where the new fauna and flora came from, although he did express the opinion in a letter to his friend Father Henry de la Fite that the catastrophes may have been caused by

> several ruptures in the crust of the globe that changed the level and position of the seas as they had already been changed at other periods and by other catastrophes. But I must confine myself to these general terms, and I present them only as the expression of a simple conjecture. (Cuvier's sketch of a letter to de la Fite, 1824, in the archive of the Museum National d'Histoire Naturelle; see Coleman 1964, p. 135).

While on a visit to London, Cuvier visited the apocalyptic painter John Martin's studio and while in front of his flood painting expressed the opinion that a comet passing near the earth could have caused such a catastrophe. Cuvier here was repeating what had been said before him by a number of people (e.g., Whiston 1696; Leibniz 1749; Halley 1724–1725) but most recently by his friend Marquis Pierre Simon de Laplace (Laplace, An IV [1796]).

Cuver's neglecting the present-day world to interpret the past called forth a very important criticism of his method—that is, biostratigraphy—by his friend Alexander von Humboldt:

> Another objection [against using "organized fossils" uncritically in identifying formations], drawn from the influence that climates exert even on pelagic animals, appears to me still more important. Although the seas, from well-known physical causes, have, at immense depths, the same temperature at the equator and within the temperate zone, yet we see, in the present state of our planet, the shells of the tropics (among which the univalves predominate, as they do among the testaceous fossils) differ much from the shells of northern climates. The greatest number of those animals adhere to reefs and shallows; whence it follows that the specific differences are often very sensible in the same parallel on opposite coasts. Now if the same formations are repeated and extended to immense distances, from east to

> west, from north to south, and from one hemisphere to the other, is it not probable, whatever may have been the complicated causes of the ancient temperature of our globe, that variations of climate must have modified, heretofore as now, the types of organization; and that the same formation (that is, the same rock placed in the two hemispheres between two homonymous formations) would have enveloped different species? It no doubt often happens that superposed beds present a striking difference in their fossil organic remains. But can we thence conclude that after a deposit was formed the beings that then inhabited the surface of the globe were all destroyed? It is incontestable that generations of different types have succeeded to one another. The ammonites, which are scarcely to be found among transition rocks, attain their maximum in the beds that represent, on different points of the globe, the Muschelkalk and Jura limestone; they disappear in the upper beds of the chalk, and above that formation. The echinites, extremely rare in Alpine limestone, and even in Muschelkalk, become on the contrary very common in the Jura limestone, chalk, and Tertiary formations. But nothing proves that this succession of different organic types, this gradual destruction of genera and species, coincides necessarily with the periods at which each formation took place. (de [von] Humboldt 1823*b*, p. 52–53; for the French original, see de [von] Humboldt 1823*a*, p. 41–42)

This is the earliest plea I am aware of to keep lithostratigraphy and biostratigraphy distinct and the first firm statement that the two do not necessarily coincide and that neither represents a real chronostratigraphy. Von Humboldt's points about the influence of geography are not terribly different from those that Berry pointed out when discussing limitations of zones in 1987 (see his p. 157ff.). Von Humboldt sounds so modern because he was criticizing a very modern view of the role of fossils in stratigraphy.

Although primarily a zoologist, Cuvier had learnt his geology from the miners' tradition. He found their emphasis on discontinuities in the history of the earth congenial to his hypothesis about extinctions. Other zoologists, under the influence of von Humboldt, Lyell, and Darwin, later began to think differently in the second half of the nineteenth century. Darwin's great champion Thomas Henry Huxley, for example, echoed von Humboldt in his first anniversary address to the Geological Society of London as its president (Huxley 1862). He recommended that fossil equivalence should not be spoken of as indicating synchroneity or contemporaneity but indicating homotaxis, that is, the same order. But by the time he read his 1870 anniversary address to the same body during his second term as its president, he had to concede that his plea had not been taken very seriously by geologists (Huxley 1870).

A Digression on William Smith and His Contribution to the Invention of Biostratigraphy

William Smith belongs to those geologists in the history of geology whose name is extremely widely known but what he actually did is not. I originally omitted him entirely from this article, but the admonition of one reviewer made me realize that a short account of his stratigraphic work was necessary to understand the nature of stratigraphy, especially biostratigraphy.

I think Martin Rudwick was the first to see the real significance of Smith's work in that he classified him as a geognost and not as a biostratigrapher (Rudwick 1997*b*, 2005, p. 431ff.). I agree with him for the following reason: Smith's earliest list of fossils that became public also has inorganic inclusions such as "pyrites and ochre" under "fossils, petrifactions, &c. &c." (Smith 1815, his table 1; fig. 11 herein; also see his manuscript table of 1799 in Kummel [1970], where these words are legible). In his *Strata Identified by Organised Fossils, Containing Prints on Colored Paper of the Most Characteristic Specimens in Each Stratum*, Smith wrote, while describing the fossils of the Clunch Clay and Shale [=Oxford Clay; Arkell 1933, his table 1; Oxfordian] that

> The upper part of this thick Stratum contains large incurved oysters or Gryphea, so much resembling others I have collected from remote parts, of a clay which now appears to be Oak-tree clay [=Kimeridge Clay; Arkell 1933, his table 1; Kimmeridgian] as to be distinguished with difficulty; *but this is only one of the many instances of the general resemblance of organized Fossils, where the Strata are Similar.* (Smith 1816, p. 22; my italics)

One of the many instances referred to was that

> Cockscomb Oysters are also common both to that rock, and to the Septaria above; in fact those large Clay-balls found plentifully in the deep cutting of the north Wilts Canal seemed to partake both of the inhabitants of the rock above, and of that below the Clay; the Trigonia of the Clay-balls being the same species as those large ones which compose the chief part of some beds of stone, about four feet thick, near the bottom of the Swindon rock.

> It may at first appear that the identification of Strata, by the organized fossils they contain, would in such cases be somewhat doubtful; but in the course of the work I shall make further remarks on such apparent repetitions, which will rather show the great utility of them. (Smith 1816, p. 18)

Smith thus had stumbled upon what we now know to be an uncharacteristic oyster for the rocks he was working in, with a range from the Jurassic to the Eocene, but he had no idea what to do with it except to fall back, almost certainly unknowingly, onto the old assumption of Lister, Baldassari, and Werner that similar fossils occurred in similar rocks (which he thought would be useful and welcome news for well-sinkers and others) "in these thick Strata of Clay, abounding with alternations of stony matter and organized Fossils" (Smith 1816, p. 18), an assumption that negates the entire essence of biostratigraphy! Smith did not practice biostratigraphy. What he did was a kind of stratigraphy of inclusions. He noted that specific beds, or groups of beds, had specific inclusions. He later learned from his geologist friends that the fossils of organisms had time connotations, a message that had come France, from Cuvier.

Lesson 6. Here is our sixth lesson from history: the nature of the work of a geologist depends on the questions he poses to nature. One does not usually receive acceptance of a marriage proposal as an answer to a question about the name of a person. If acceptance of a marriage proposal is an answer to a question, we infer that the question was a marriage proposal. Smith simply did not ask the appropriate question to invent biostratigraphy. This lines him into the row of geognosts, as Rudwick rightly maintains, and not into that of biostratigraphers. In stratigraphy also, our answers will depend on the question we ask.

Stratigraphy and Mountain Building: The Tyranny of Strata and the Dating of Deformation during Mountain Building

Shortly after Cuvier's *Discours* was published, Leopold von Buch began publishing articles on mountain building. His interest was kindled not only by his work on dolomite in the Alps but also by his conversion away from his teacher Werner's neptunism after his visit to the volcanoes of Auvergne in central France (von Buch [1809] 1867, p. 483). There he convinced himself that there were two kinds of volcanoes. One class possessed what he called eruption craters. These were the normal volcanoes that erupted lava and other materials from their craters. Another class consisted of large basaltic volcanoes with what von Buch called elevation craters (von Buch [1809] 1867, p. 513ff.; [1820] 1877). In such volcanoes he thought first basaltic lava of low viscosity poured out, creating large lava lakes (this seems a last remnant of his neptunism, forcing him to think in terms of beds). After solidification, a central uplift formed the lava lake into a shield volcano and the tensional stresses caused by the uplift opened not only a central orifice at the top, the elevation crater, but also numerous barrancos that von Buch interpreted as expressions of radial extensional fissures. Once he convinced himself that magmatism had the power to make volcanic mountains, he extended this idea to mountain chains and postulated large dikes or dike systems of augite porphyry running down the crest of mountain ranges and uplifting them into a bilaterally symmetric chains with respect to the generative dike (von Buch [1824] 1877). Mountain building was a "violent" (*gewaltsam*) event (von Buch [1822] 1824, p. 84). The speed of uplift was such that a mountain formed overnight, pushing its superincumbent marine waters away from it in giant tsunamis. This was, in his opinion, the origin of the erratic blocks on elevations in the Jura Mountains (von Buch [1827] 1977). He expressed this opinion to Sir Roderick Murchison during a joint excursion. This was an echo of Pallas's similar views published earlier, and even the catastrophist Murchison found them extreme (for Murchison's testimony, see Geikie 1875, p. 75).

Cuvier's and von Buch's publications formed an image of mountain building in the first quarter of the nineteenth century that was violent and worldwide. In 1829, Jean-Baptiste Armand Louis Léonce Élie de Beaumont, a student and collaborator of the Werner pupil Brochant de Villiers in Paris, developed a theory of mountain building based on the thermal contraction of the globe (Élie de Beaumont 1829) that posited simultaneous worldwide orogeny in phases of short duration (Élie de Beaumont 1829–1830, 1830, 1831, 1833). He claimed that such worldwide orogenies caused widespread extinction in the organic world in the manner suggested by Cuvier. He also resurrected Steno's ideas and used angular unconformities as means for dating the mountain-building "revolutions" (see Élie de Beaumont 1832, especially p. 350–355). He wrote,

> When examined with some care, it is seen that along almost all mountain chains beds extend in a horizontal position as far as the foot of the mountain, indicating that they were laid down in the sea or in lakes whose shores were partly formed by these mountains. By contrast, other beds, which are upturned and which turn around

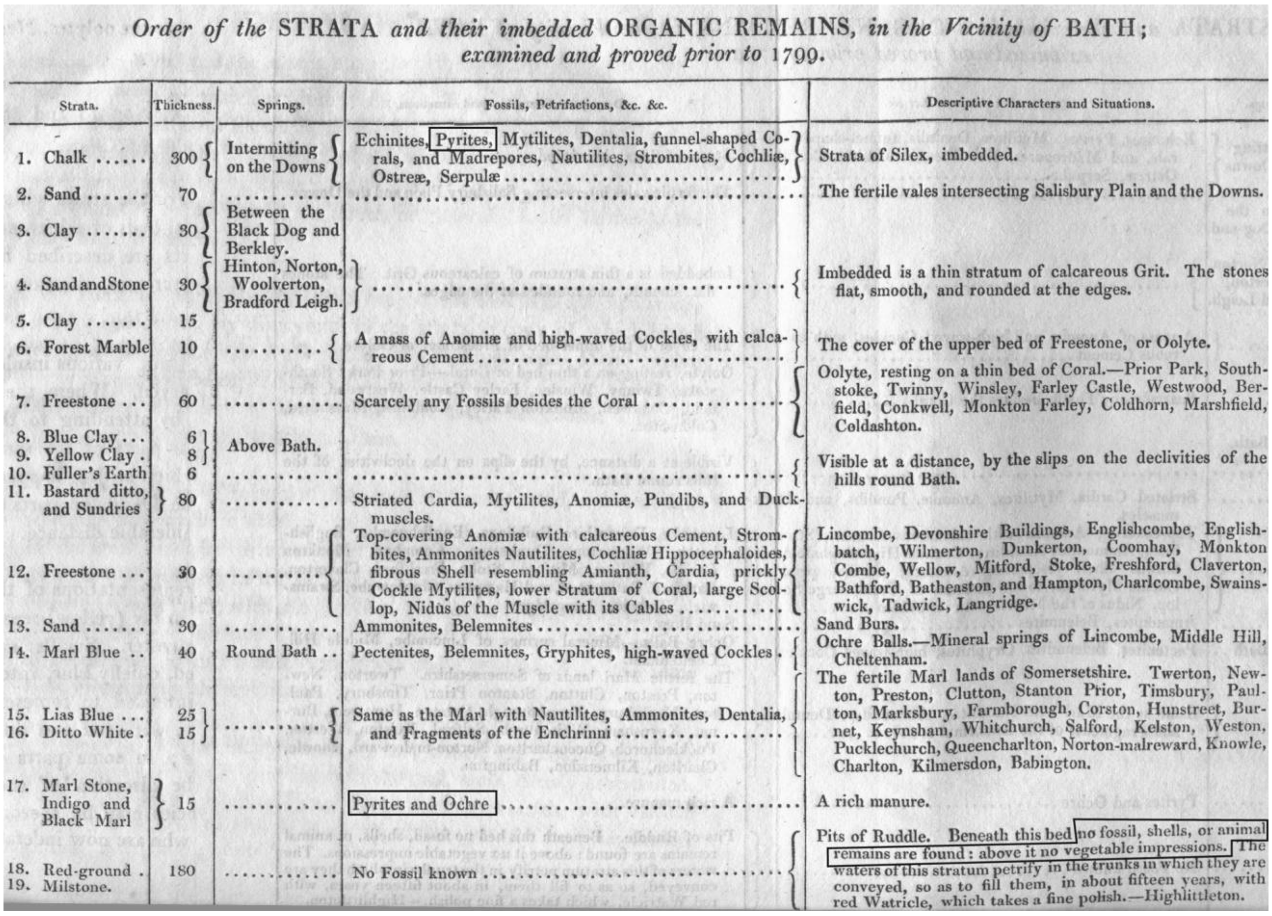
the flanks of the mountains, reach in some regions as far high as the summit. (Élie de Beaumont 1829–1830, p. 5–9)

But it is necessary to make a remark also, before all others, about this natural division of the beds into two classes in every mountain chain, namely, those that have been upturned and those that have not: it is the constant sharpness of the separation of the two classes. (Élie de Beaumont 1830, p. 7–10)

Now a distinction, which is always sharp, and which allows no intermediaries, thus results from this observation between the upturned and the horizontal beds [fig. A3 herein; this is also what von Humboldt (1823*a*, p. 54) referred to as Werner's idea of the independence of formations with the implication that this independence was worldwide; Élie de Beaumont refers to von Humboldt as his source on p. 231]. One concludes that the phenomenon of upturning was not continuous and progressive; it operated in a time interval between the periods of deposition of the two consecutive terrains and during which no deposition of regular beds took place. In one word, it was brusque and of short duration.

Such a convulsion that upturns the beds in an entire mountain range necessarily interrupts the slow and progressive development of sedimentary terrains, and it is clear that some anomaly must be observed nearly universally at a point in such series that corresponds to the moment at which the upturning of the beds took place. (Élie de Beaumont 1831, p. 242–243)

Élie de Beaumont here takes the biostratigraphic zones defined by Cuvier and other geologists up to his time as equivalent to the two classes of beds that he describes and, equating fossils with time, he thus also equates rock strata with time. The strata in this view become coextensive with biostratigraphic zones and



Order of the STRATA *and their imbedded* ORGANIC REMAINS, *in the Vicinity of* BATH; *examined and proved prior to* 1799.

Strata.	Thickness.	Springs.	Fossils, Petrifactions, &c. &c.	Descriptive Characters and Situations.
1. Chalk	300	Intermitting on the Downs	Echinites, Pyrites, Mytilites, Dentalia, funnel-shaped Corals, and Madrepores, Nautilites, Strombites, Cochliæ, Ostreæ, Serpulæ	Strata of Silex, imbedded.
2. Sand	70			The fertile vales intersecting Salisbury Plain and the Downs.
3. Clay	30	Between the Black Dog and Berkley.		
4. Sand and Stone	30	Hinton, Norton, Woolverton, Bradford Leigh.		Imbedded is a thin stratum of calcareous Grit. The stones flat, smooth, and rounded at the edges.
5. Clay	15			
6. Forest Marble	10		A mass of Anomiæ and high-waved Cockles, with calcareous Cement	The cover of the upper bed of Freestone, or Oolyte.
7. Freestone	60		Scarcely any Fossils besides the Coral	Oolyte, resting on a thin bed of Coral.—Prior Park, Southstoke, Twinny, Winsley, Farley Castle, Westwood, Berfield, Conkwell, Monkton Farley, Coldhorn, Marshfield, Coldashton.
8. Blue Clay	6	Above Bath.		
9. Yellow Clay	8			
10. Fuller's Earth	6			Visible at a distance, by the slips on the declivities of the hills round Bath.
11. Bastard ditto, and Sundries	80		Striated Cardia, Mytilites, Anomiæ, Pundibs, and Duck-muscles.	
12. Freestone	30		Top-covering Anomiæ with calcareous Cement, Strombites, Ammonites Nautilites, Cochliæ Hippocephaloides, fibrous Shell resembling Amianth, Cardia, prickly Cockle Mytilites, lower Stratum of Coral, large Scollop, Nidus of the Muscle with its Cables	Lincombe, Devonshire Buildings, Englishcombe, Englishbatch, Wilmerton, Dunkerton, Coomhay, Monkton Combe, Wellow, Mitford, Stoke, Freshford, Claverton, Bathford, Batheaston, and Hampton, Charlcombe, Swainswick, Tadwick, Langridge.
13. Sand	30		Ammonites, Belemnites	Sand Burs.
14. Marl Blue	40	Round Bath	Pectenites, Belemnites, Gryphites, high-waved Cockles	Ochre Balls.—Mineral springs of Lincombe, Middle Hill, Cheltenham.
15. Lias Blue	25		Same as the Marl with Nautilites, Ammonites, Dentalia, and Fragments of the Enchrinni	The fertile Marl lands of Somersetshire. Twerton, Newton, Preston, Clutton, Stanton Prior, Timsbury, Paulton, Marksbury, Farmborough, Corston, Hunstreet, Burnet, Keynsham, Whitchurch, Salford, Kelston, Weston, Pucklechurch, Queencharlton, Norton-malreward, Knowle, Charlton, Kilmersdon, Babington.
16. Ditto White	15			
17. Marl Stone, Indigo and Black Marl	15		Pyrites and Ochre	A rich manure.
18. Red-ground	180		No Fossil known	Pits of Ruddle. Beneath this bed no fossil, shells, or animal remains are found: above it no vegetable impressions. The waters of this stratum petrify in the trunks in which they are conveyed, so as to fill them, in about fifteen years, with red Watricle, which takes a fine polish.—Highlittleton.
19. Milstone.				

Figure 11. Upper part of Smith's first published table (from Smith 1815). I have boxed the items that are not organic fossils yet were included in his list of "Fossils, Petrifactions, &c. &c." The very title of the table indicates that here he uses the term "fossil" for all inclusions obtained from strata. In the right-hand column, in the boxed area he distinguishes between fossil, shell, and marine animal, although there the usage of the singular for fossil and the comma after it render the meaning doubtful.

those with worldwide time horizons, and he follows these time horizons to define his mountain-building revolutions in time. That these were unacceptable assumptions was clear at the time to people such as Sir Charles Lyell, who responded in the third volume of the first edition of his *Principles of Geology* in 1833. But before that he expressed his disapproval in a letter he wrote to his friend George Poulett-Scrope:

> I took up Élie de Beaumont's new work, published in the 'Ann. des Sciences'. Now I am glad to find out how much my views differ from his. In his memoir entitled 'Recherches sur quelques-unes des Révolutions de la surface du Globe', &c., he begins by saying, that whatever be the causes which have raised mountain chains and put the beds composing them into their present position, it must have been 'an event of a different order of magnitude from those we witness daily'. In my comments on the Huttonian theory [Lyell 1830, p. 473ff.], I throw out that there have been, in regard to separate countries, *alternate* periods of disturbance and repose, yet earthquakes may have been always uniform, and I show or hint how the interval of time done, may make the passage appear abrupt, violent, conclusive, revolutionary. Now É. de Beaumont's reigning notion evidently is, that because 'the upheaval of beds stops abruptly at such and such horizon of the sequence of beds of sediment (in different chains), and affect with equal intensity all older beds, the phenomenon of upheaval was not continuous and progressive, but brusque and of short duration' &c. Now it is undoubtedly brusque as far as the solution of continents [this is a Huttonian way of saying the destruction of continents by external processes, such as erosion, etc.], but why therefore the 'convulsion' of short duration? I do not find anywhere in him the notion of restoring in imagination the geographical features of Europe of N. Hemisphere by causing the land to sink, which has since been upraised—as to climate he has nothing to do with it. I told you before, that I have as far as I, and others I have consulted know, to answer for all the sins committed in that new theory. (Lyell to Scrope, June 25, 1830, in Lyell 1881)

Sir Charles's published answer to Élie de Beaumont spelled out explicitly that rock cannot be equated with time without further ado. Referring to Élie de Beaumont's theory, he wrote,

> Now all this reasoning is perfectly correct, so long as the particular groups of strata b and c [upturned and horizontal beds of Élie de Beaumont, respectively; fig. 11 herein] are not confounded with the geological periods to which they may belong, and provided due latitude is given to the term contemporaneous; for it should be understood to allude not to a moment of time, but to the interval, whether brief or protracted, which has elapsed between two events, namely between the accumulation of the inclined and of horizontal strata.
>
> But, unfortunately, the distinct import of the terms 'formation' and 'period' has been overlooked, or not attended to by M. de Beaumont, and hence the greater part of his proofs are equivocal, and his inferences uncertain; and even if no errors had arisen from this source, the length of some of his intervals is so immense, that to affirm that all the chains raised in such intervals were *contemporaneous*, is an abuse of language. (Lyell 1833, p. 341)

Lyell clearly saw that the strata deposited during a previously defined time interval by means of fossils do not encompass that entire period even if they contain the very same fossils that helped to establish the time interval in question elsewhere. He had written the following earlier in the same volume:

> Some authors apply the term *contemporaneous* to all the formations which have originated during the human epoch; but as the word is so frequently in use to express the synchronous origin of distinct formations, it would be a surce of great inconvenience and ambiguity, if we were to attach to it a technical sense. (Lyell 1833, p. 52)

Lyell thus objected to consider synchronous even the rocks formed while the humans inhabited the earth (in his day, that time interval was thought very much shorter than we think today), and rightly so. Although Lyell's uniformitarianism triumphed in the nineteenth century, his point about the fallacy of time = rock equivalence was not taken seriously by geologists, neither in his own country nor elsewhere. I think this was so because while destroying the Beaumontian edifice, Lyell offered nothing to replace it, essentially making the establishment of a global geological history impossible. After the triumph of uniformitarianism, geologists the world over began leading lives with split personalities: in dealing with geological phenomena, they have been uniformitarians, that is, Huttonians; in dating geological events, they have been catastrophists, that is, Wernerians. This double personality resulted from their not questioning where their methods and theories came from, what they had been based on. They are not even aware that they have a split personality.

The Wernerian/Cuvierian stratigraphy found its continuation during Lyell's lifetime in the massive work by Cuvier's pupil Alcide Dessalines d'Orbigny. D'Orbigny undertook to describe the entire French fossil record in a massive multivolume work, the famous *Paléontologie Française*, and began with the Cretaceous. (D'Orbigny did not live to see the completion of his mammoth project. He nevertheless published nine volumes, and the series was eventually completed by others in 25 volumes.) He anticipated a question as to why he began not at the beginning or the end of the geological column and pointed out that the periods were so independent from one another that it would have made no difference where he chose to begin (d'Orbigny 1840, p. 18). This is an extraordinary statement: d'Orbigny did not view the geological record as a single book to be read but one consisting of independent stories. It thus made no difference with which story he began to read it. The great paleontologist Melchior Neumayr later said that in d'Orbigny we see the catastrophe theory developed to its most rigid and dogmatic extreme (Neumayr 1887, p. 268). D'Orbigny considered every period to be separated from its predecessor and its successor by global catastrophes that destroyed the entire organic nature, and every time the omnipotent creator ("toute-puissance créatrice"; see d'Orbigny 1842, p. 274) repopulated the earth. In his textbook of 1852, he claimed that the Silurian period (which he took in its Murchisonian sense, i.e., including the Cambrian) closed by the disappearance of 418 species and 21 genera and that during the Devonian 78 genera and 1198 species came to being that were totally unknown from the Silurian. After the Devonian fauna had disappeared, 72 new genera and 1047 new species were supposedly created during the Carboniferous (d'Orbigny 1852, vol. 2, p. 323, §1733, and p. 360, §1761). He was careful to point out that even individual stages in a system did not share fossils of the same species (e.g., d'Orbigny 1840, p. 422–423). D'Orbigny defined his stages ("étages") allegedly according to fossils, but he drew their limits at lithologic boundaries, along stratal divisions! So his stages, although supposedly defined by fossils, were really delineated by strata. This is the most extreme form of the tyranny of strata. At each stage and period boundary, really defined by some bed boundary, there supposed to have occurred a global catastrophe wiping out the entire living world. He also now and then revised fossils occurring in more than one stage to make them fit into a single one (e.g., d'Orbigny 1840, p. 422 n. 1). What d'Orbigny did was not essentially different from Scheuchzer's global deluge-bed correlation. The only difference lay in the multitude of d'Orbigny's cataclysms. But d'Orbigny's scheme had the great authority of his justifiably much-lauded careful paleontology behind it, and it gave the geologists the comfort of being able to correlate their strata worldwide.

With d'Orbigny, the onion-shell biostratigraphy of the earth became established in the heads of most geologists. We see the first effect of this in Oppel's (1858) formulation of the zone concept. In his preface ("Vorrede"), Oppel complains that so far in the Jurassic stratigraphy of Europe bed groups had been correlated, but it had not been demonstrated that each horizon in a given place is marked by species peculiar to it and that it can be found in the farthest places with the same certainty (Oppel 1858, p. 3). Let us note here that what is to be correlated, according to Oppel, is still beds and that his demand that each bed be securely identified with its fossils is no different from d'Orbigny's. He continues:

> This task is difficult, but only by accomplishing it entire systems may be precisely compared with one another. It is therefore necessary to study the vertical distribution of each individual species in various places and according to this establish zones, which, through constant and general occurrence of certain species, distinguish themselves from the surrounding ones as definite horizons. One thus obtains an ideal section the coeval members of which in various places are always characterized by the same species. (Oppel 1858, p. 3)

Here we see that fossil equivalence is, for Oppel, evidence for contemporaneity. But this is not all. Let us consider figure 12 herein. This is a copy of Oppel's subdivision of the Lias into its zones. Notice that every zone corresponds to a certain bed. Only in the Bucklandi bed are there two zones, but these are depicted as if they were perfectly parallel with one another and laterally boundless. When one reads Oppel's classic one sees that here, too, it is the beds that are being correlated and considered synchronous. Helmut Hölder, a fine stratigrapher and an accomplished historian of geology, noted the following concerning Oppel's work:

> Oppel understands under a zone a "stratum," a "layer," a "bed," or a "horizon," "which, in a locality, is characterized by a number of constant species and in the farthest places is to be found with the same reliability." (Hölder 1964, p. 4)

Although Oppel was no follower of Cuvier's catastrophe theory, it is precisely Cuvier's assumptions, most likely via Oppel's teacher Friedrich August Quenstedt (1843, 1851), that underpin his stratigraphy. Moreover, his first major work, *Der Mittlere Lias Schwabens* (Oppel 1853), was occasioned by a prize assignment from Quenstedt formulated by the following description:

Eintheilung des unteren Lias nach seinen paläontologischen Characteren.

Nro. 1.

Raricostatusbett.	Zone des *Amm. raricostatus.*	Amm. densinodus. Amm. muticus. Amm. Carusensis. Pentacrinus scalaris.
Oxynotusbett.	Zone des *Amm. oxynotus.* Amm. bifer. Amm. lacunatus.	Acteonina Dewalquei. Mytilus minimus. Leda Romani. Plicatula ventricosa. Rhynch. oxynoti. Lingula Davidsoni.
Obtususbett.	Zone des *Amm. obtusus.*	Panopaea crassa. Pholadomya Fraasi. Cardinia hybrida. Terebratula Causoniana. Amm. Brooki, stellaris. Amm. planicosta, ziphus, Dudressieri.
Tuberculatusbett.	*Saurierbett.* Ichthyos. platyodon. „ intermedius. „ communis. „ tenuirostris. Plesiosaurus.	Amm. Birchi, Bonnardi, Turneri. Gervillia lanceolata, Inoceramus Faberi. Gryphaea obliqua beginnt hier. Acrosalenia minuta.
	Bank des *Pentacrinus tuberculatus.*	
Bucklandibett.	Zone des *Amm. geometricus.*	Bel. acutus erscheint hier zum ersten Male. Amm. Sauzeanus „ Scipionianus „ laevigatus.
	Zone des *Amm. Bucklandi.*	Amm. Bucklandi, „ bisulcatus, „ Sinemuriensis, „ Kridion, Amm. Conybeari. „ rotiformis. „ liasicus. „ spiratissimus.
Angulatusbett.	Zone des *Amm. angulatus,* Schl. (Moreanus d'Orb.)	Chemnitzia Zenkeni. „ solidula. Acteonina fragilis. Littorina clathrata. Natica subangulata. Nerita liasina. Cerithium subturitella. Panopaea Galathea. Tancredia securiformis. Cardinia elongata. „ concinna. Mytilus nitidulus. „ Hillanus. Perna Gueuxi. Asterias lumbricalis. Cidaris arietis.
Bett des **Amm. planorbis.**	Zone des *Amm. planorbis* u. Amm. Johnstoni.	Avicula Kurri. Pecten Trigeri.
Bonebed. Knochenbett	Microlestes, Nothosaurus, Termatosaurus, Gyrolepis, Saurichthys, Sphaerodus, Ceratodus, Acrodus, Thectodus, Hybodus.	Eine Anzahl unbestimmter Muscheln: Avicula, Gervillia, Pecten u. s. w.

Keuper = New Red = Marnes irisées.

Figure 12. Albert Oppel's table showing the zones of the Lower Lias. Notice the bed dependence of the paleontologically defined zones.

An exact listing of the beds of the middle Lias (the Numismalis Marl and Amalthean Clay) with special attention to their contained fossils. Of the latter, especially those easily mistaken for others should be draughted well or, even better, should be provided as samples for the evaluation of the assignment. (Oppel 1853, p. 1)

Lesson 7. Here we learn a seventh lesson from history: it is critical to be aware of the fact that one commonly makes assumptions in geology even when one thinks one is working purely empirically as well as of the foundations of one's assumptions. Oppel learned not only the time equivalence of fossils from Cuvier but also to equate bed boundaries with time horizons, although he was not aware of this. He had given up Cuvier's catastrophes but not their stratigraphic implications. He considered the time equivalence of fossils a simple given. Clearly, he had not paid much attention to von Humbodt's and Lyell's criticisms.

However, Lyell's critique of Élie de Beaumont's theory of mountain building eventually did hit the target, at least in part. In fact, even before he came out publicly against Élie de Beaumont's views, Gérard Paul Deshayes—from whom (Lyell 1833, p. 49–61; see Rudwick 1978; and from Gian Battista Brocci; see Vai 2009*a*, p. 192ff.) Lyell had learned the principle of how to subdivide the Tertiary stratigraphy—had written that the sudden deformations Élie de Beaumont was talking about seemed to have taken place without disturbing the organisms, that is, without giving rise to Cuvierian catastrophes (Deshayes 1832). In 1857, Vicomte d'Archiac expressed the same view (d'Archiac 1857, p. 599–600), although in America the Cuvierian view continued to dominate via James Dwight Dana.

In the minds of the stratigraphers the problem then became this: if worldwide revolutions of mountain building did not punctuate the geological record by causing extinctions, what did? This question glided over and avoided answering Lyell's criticism about equating rock with time simply because at the time there could be no answer except to give up the attempt at precise global stratigraphic correlations. No geologist at the time could be bold enough to accept such a radical proposal.

In 1857, the Sorbonne geologist Edmund Hébert developed the idea that it was not the sudden vertical motions of the mountain ranges that divided geological systems but the vertical oscillations of the continents (Hébert 1857, 1859), at the time warmly defended by Lyell. Lyell had concluded that continents did move up and down slowly without causing violent earthquakes after having vehemently denied it in criticism of Leopold von Buch's suggestion that Scandinavia was slowly rising (Lyell 1835*a*). After having called the retreat of the beaches in the Baltic "a most peculiar, odd, striking phenomenon" (von Buch [1810] 1870, p. 503), von Buch, the most influential geologist of the first half of the nineteenth century, stated categorically that

> It is certain that the sea level cannot sink; the balance of the seas will not allow it. But as the phenomenon of reduction cannot be doubted, as far as we can now see, there remains only one way out, and that is the conviction that the whole of Sweden is slowly rising. (von Buch [1810] 1870, p. 504)

He pointed out that, according to the information he was able to gather during his 1807 trip, this rising was not confined to the Baltic but was also felt along the North Sea coasts.

Lyell decided to go to Scandinavia to disprove von Buch's conjecture. However, in the entry dated "Oregrund: July 1," Lyell wrote the following in his travelogue in Sweden:

> It seems true, as Galileo said in a different sense, 'that the earth moves'. (Lyell 1881, p. 433)

By October he wrote a letter to Gideon Mantell stating that

> In Sweden I satisfied myself that both on the Baltic and Ocean side, part of that country is really undergoing a gradual and insensibly slow rise. (Lyell 1881, p. 442)

On November 27 and December 18 of the same year (1834), Lyell presented the Bakerian Lecture to the Royal Society of London in two installments (Wilson 1972, p. 410). In this he was

> willing to confess, after reviewing all the statements published previously to my late tour for and against the reality of the change of level in Sweden, that my scepticism appears to have been unwarrantable. (Lyell 1835*a*, p. 2–3)

The published version of the lecture is a long article, which is a remarkably careful, detailed, and conscientious account of the evidence he saw or heard. There is in it neither an attempt to test von Buch's statement that the north was rising faster than the south in Sweden nor the slightest suggestion as to what the cause of the observed rise might be.

It was only in the fourth edition of the *Principles* that he hazarded a guess as to some possible causes:

> The foundations of the country, thus gradually uplifted in Sweden, must be undergoing important modifications. Whether we ascribe these to the expansion of solid matter by continually increasing heat, or to the liquefaction of rock, or to the crystallization of a dense fluid, or the accumulation of pent-up gases, in whatever conjecture we may indulge, we can never doubt for a moment, that at some unknown depth the structure of the globe is in our own times becoming changed from day to day, throughout a space probably more than a thousand miles in length, and several hundred in breadth. (Lyell 1835*b*, p. 349)

Lyell's preferred mechanism was heat, generated by chemical reactions (see Lawrence 1978), expanding and heaving up a crust about 200 miles in thickness. He believed that such a process might explain his observations regarding the uplift of land in Scandinavia (Lyell 1835*b*, p. 384). Although Lyell's least successful attempts at geological theorizing comprised those pertaining to tectonics, his eventual conversion to the slow, continuous, and aseismic upheaval hypothesis helped the ideas on continental uplift, as distinct from mountain uplift, to gain wide currency in the middle of the nineteenth century.

Lyell's attempts at theorizing encouraged some of his friends to formulate some extremely interesting and fruitful speculations. Sir Henry T. de la Beche (1796–1855), in the first edition of his *Researches in Theoretical Geology*, adopted the contraction hypothesis of Élie de Beaumont (with whom he had been in contact earlier and helped to ventilate his ideas in Britain; see Şengör 1991). He pointed out that contraction

> would not only appear to raise large areas, composing continents, bodily out of the water, by producing great depressions, but would squeeze the principal surface fractures into mountain ranges. (de la Beche 1834, p. 162)

The views of both Charles Babbage (1792–1871; Gridgeman 1981; Hyman 1987; Babbage 1994, his fig. 47) and Sir John Herschel (1792–1871; Evans 1981) are found in appendixes to the former's *Ninth Bridgewater Treatise* (Babbage 1837, notes F–I, p. 182–217; 1838, notes F–I, p. 204–247) and are concerned primarily with the means of generating uplifts and depressions through the internal heat of the earth. Both contend that the lines of equal temperature must mimic the topography grossly, subaerially, or subaqueously. While erosion depresses (with respect to the center of the earth) the geotherm below a given point near the original surface, deposition raises it. This may cause metamorphism or even melting under thick sedimentary piles and might liberate water vapor and other gases, causing volcanic eruptions. Herschel, in his letter to Lyell (in Babbage 1838, p. 225–236), pointed out that since a fluid substratum must exist beneath the crust, sedimentation would load any basin floor and depress the crust underneath into the substratum. By contrast, erosion would occasion uplift.

This is an early form of the theory of isostasy (Longwell 1928) and is identical with that of Élie de Beaumont's earlier idea (although Herschel seems to have conceived it independently; neither was aware of Count Marsili's [see Vai 2006] and Benjamin Franklin's [in a letter he wrote from Passy in Paris, France, on September 22, 1782, to Jean-Louis Giraud Soulavie; see Morgan 2006, p. 133–134] even earlier ideas on isostasy). Both Babbage and Herschel were mainly concerned about explaining the cause of the uplifts. Their theories satisfied them as far as the causes of volcanoes—and of broad uplifts and subsidences—were concerned. Their arguments must have also pleased Lyell, especially Herschel's assurance that central heat (in the sense of Cordier [1827], with which Lyell disagreed; see Wilson 1972, p. 386–387; Lawrence 1978; Rudwick 1990) was not a necessary condition for their theories to be true (Babbage 1838, p. 246), although Herschel did emphasize the "*frightfully* rapid progression" of temperature downward into the earth (Babbage 1838, p. 246). Neither Lyell nor anyone else made use of these geophysical speculations until much later, despite the fact that, within a decade, gravity observations began to make it just possible to constrain the thickness of the crust (see Petit 1849; Airy 1855; Pratt 1855; see Daly 1940, p. 36–64, for a well-informed and concise history of gravity observations; also Oreskes 1999).

Hébert's idea of making the continental oscillations à la Lyell responsible for determining the period limits was nothing more than replacing Élie de Beaumont's mountain-building revolutions with continental oscillations. As I said above, it did nothing to answer Lyell's criticism of equating rock with time. Surprisingly, the man who created modern geology as we know it, Eduard Suess, implicitly followed the basic spirit of Cuvier's and d'Orbigny's stratigraphy in considering the subdivisions of the geological record as natural units and overlooked the fundamental criticism of Lyell, although he was both a Lyellian and a Darwinian in his interpretation of the geological past. His stand was defensible because he associated it with a *vera causa*, as we shall see below. But this was not understood and caused immense confusion in the stratigraphic discussions of the twentieth century. In fact, Suess's work is possibly the most neglected episode in the history of stratigraphy because most geologists considered him a tectonician and did not realize that his work in tectonics was mainly an outgrowth of his earlier stratigraphic studies.

Global Stratigraphy and the Rise of the Theory of Eustasy out of the Tyranny of Strata

Shortly after he was appointed a professor of paleontology in the University of Vienna on August 10, 1857, Eduard Suess began taking his few students to excursions in the Vienna Basin, in which the imperial capital is situated. He saw that the basin had a Miocene marine fill to begin with, which then switched to a brackish sequence and ended up in lacustrine and fluvial beds reaching into the Pliocene (now known to reach into the Quaternary). He thought this was evidence for the successive phases of uplift of the basin as the entire continent of Europe became elevated during the course of the later Tertiary (Suess 1862). Suess was later surprised to see that the same sequence at the same elevations also occupied the interior of the Hungarian plains and became curious as to how far this stratigraphy extended. He began a correspondence with experts as far afield as southern Russia in the Crimea, the Caucasus, and the North Caspian depression. He obtained the most surprising news from the Russian geologist Nikolai Pavlovich Barbot de Marny, who assured Suess that the same undeformed stratigraphy at similar elevations showed up from the western margins of the Black Sea all the way to the Caspian Sea. Suess was extremely excited and presented Barbot de Marny's communication to a meeting of the Imperial Academy of Sciences in Vienna on April 26, 1866 (Barbot de Marny 1866).

He was troubled. How could continental vertical oscillation à la Lyell bring about such uniformity of stratigraphy at such immense distances with barely any sign of deformation? He remembered that Edmund Hébert in his thesis had defended the view that it was not Élie de Beaumont's sudden and worldwide mountain uplifts that punctuated the geological record but the gentler up-and-down movements of entire continents (Hébert 1857, 1859). Hébert had found in sea level changes the main cause for the introduction of significant interruptions in the global stratigraphic record and thought that he had no other option than to ascribe these to Lyell's continental oscillations, although he did not say how a global synchrony could thus be achieved. As a paleontologist, Suess knew that the history of life was not as random as Darwin's and Wallace's theory of natural selection might first lead one to believe but that during the course of geological epochs large plant and animal groups, which Suess later called "economic units," had suddenly appeared and also suddenly disappeared. But how could continental oscillations create such uniformity over such large distances? Light dawned on him during one of his excursions near Eggenburg, a small, charming town north of Vienna in the extra-Alpine part of the Vienna Basin:

> During such wanderings in the plains exhibiting long-stretched zones of Mediterranean deposits maintaining a constant height on the slopes of the old rocks before me and filled with the idea that a similar thing occurs in the wide Hungarian plain, I was first possessed by the idea that such extensive evenness could not be brought about by raising the land but only by depressing the sea level.
>
> This idea bit deep into the foundation of the prevailing geological views, but many factors invited a closer inspection, especially the fact that many larger islands rising up high from the waves of the ocean carry an animal population and a plant cover identical or very closely related to those of the nearest continent, so that one would like to see them as parts of these continents. These insular terrestrial faunas and floras could not possibly have been raised up from the depth of the sea, but a change in the level of the sea surface could cut them off and leave them as relicts.
>
> First the facts, such as relations of elevation, the fossil shells, etc., had to be followed as closely as possible. For that Eggenburg offered a convenient opportunity. But only 15 years later, after I had learned more about the distribution, did I dare to pronounce this opinion publicly. (Suess 1916, p. 138–139)

The reason why he dared only 15 years later to talk about his idea of sea level change was that he also had found in the meantime a theoretical framework for the global sea level oscillations. Suess knew that without a theory as to the why of the sea level oscillations, empirical evidence alone was insufficient. That theory came from his considerations on the nature of mountain building.

When he published his book on the geology of the city of Vienna in 1862, Suess still adhered to the then-conventional view that mountain chains were structures that were bilaterally symmetric about an axis running down their middle and along which igneous rocks had risen to uplift them. Although in 1829 Élie de Beaumont had added a component of thermal contraction to shorten the mountains across their trend, this had not done anything to change their overall symmetric structure or the presence of igneous rocks along their axes that helped their uplift in the minds of the geologists. In the early seventies Suess began taking his students to southern Italy for excursions, and it was there, he tells us in his memoirs, that he had become aware of the fact that the Apennines were an asymmetric chain with easterly vergence. He published an article in 1872 announcing his finding, but in that article he was still conventional. He thought that the Apennines as a whole was the eastern wing of an originally symmetric orogen, the central crystalline axis of which was only partly preserved in Calabria and the Peloritani Mountains of Sicily, but the rest had been submerged beneath the surface of the Tyrrhenian Sea (fig. 13 herein). The southwest-vergent structures of the sedimentary successions in Sicily southwest of the Peloritani crystallines Suess considered the remnants of a once-extant western wing (fig. 13 herein). Armed with these ideas, he decided to look at the mountains of central and western continental Europe, possibly hoping to corroborate his ideas developed in Italy. But he was in for the shock of his life! He was so surprised by what he found in Europe that he rushed to present it to the Academy on July 24, 1873. Of that remarkable communication only an abstract is published. Incredibly, despite its momentous importance, it has been completely ignored in the geological literature and in the literature on the history of geology (see Şengör 2014*a*, 2015). This justifies my quoting it here in full:

> The full member Professor Suess presented a paper with the title "On the Structure of the Middle European High Mountains." It was first shown that the opinion prevailing until now about the symmetrical structure of the high mountains and their uplifting through a central axis is no longer defensible because of many reasons, especially because a detailed study shows that with the exception of a small part of the Alps and perhaps the southernmost part of the Italian peninsula, southern marginal zones do not exist in the Middle European mountain chains. The newer explanations, based on the one-sidedness of mountains, such as those of Dana and Mallet, correspond better to the circumstances but are still not sufficient. The Alps do not fork in the inlet of Graz, as commonly said; instead, the Middle European Mountains constitute, in their entirety from the Apennines to the Carpathians, a group of mountains that follow each other in the form of a fan. They exhibit regular folds toward the north or toward the northeast, but on the opposite side they show fields of extension and subsidence, volcanic constructions, and earthquakes.
>
> The first of these chains that follow each other in the form of a fan is the Italian Peninsula [i.e., the Apennines]; Dalmatia with the Karst and the Bosnian Mountains form the second group; the more or less east-west-striking Croatian and then the Styrian chains constitute the third group; the next is already the southwest-striking Bakony Forest; and finally the last is the great chain of the Carpathians.
>
> The Jura and the Swabian Alb are also such chains.
>
> The trends of all of these mountains depend on the position of the older massifs, and the way they are dammed against the old massifs can be recognized not only in the French Jura, in the Swiss Jura along the southern margin of the Black Forest, or in the course of the anticlines of the Austrian limestone zone south of the Bohemian Massif but also the whole arc-shaped surrounding of the individual chains of the Western Alps, the unity of which was recognized by Desor as a consequence of damming.
>
> If one regards the old massifs of Sardinia with Corsica and the Hyères, that of central France, central Germany, and Bohemia as islands, and imagines that a sea fills the space around them in which a flood wave originates in the southwest, so the trend of this wave would be entirely similar to those of the great mountain chains.
>
> The old mountains themselves seem to rend locally and to follow a similar direction, such as Riesen- and Erzgebirge. Far in the east, the mountain chains seem to obey similar laws, such as the Balkan, the trachytes of which had already been compared with the basalts of the Riesengebirge, with the trachytes of the Carpathians, and with the volcanoes of Italy by Hochstetter. Also the Caucasus with the block at the southern point of the Crimea.
>
> The author came to the conclusion that the entire surface of the earth is in a state of general but very slow and heterogeneous motion, which, in Europe, between the 40th and the 50th latitudes, is directed to the northeast or to the north-northeast. The so-called old massifs move more slowly than the regions lying between them, which form chains that are dammed up. In Middle Europe, on the polar side regular folds are built, and on the equatorial side tears are produced.
>
> This peculiar movement of the surface of the earth behaves, with respect to the rest of the planet, like the so-called peculiar movement of the sunspots with respect to the rotation of the entire body of the sun. Their direction in various parts of the earth are also various. (Suess 1873, p. 130–131)

This is truly an astonishing text. On reading it, one expects Suess in his next publication to pronounce the evidence and rules of continental drift. Yet it was not to be, because the horizontal motions he described above did nothing to help him change the sea level independently of the continents. He needed something else, and he found it in Constant Prévost's version of the theory of thermal contraction of the globe (see Prévost 1839, 1840). That theory not only allowed the making of asymmetric mountains, as opposed to Élie de Beaumont's version of the contraction theory, which produced symmetric mountains, but it also allowed oceanic subsidences to take the form of large, fault-bounded, elliptic

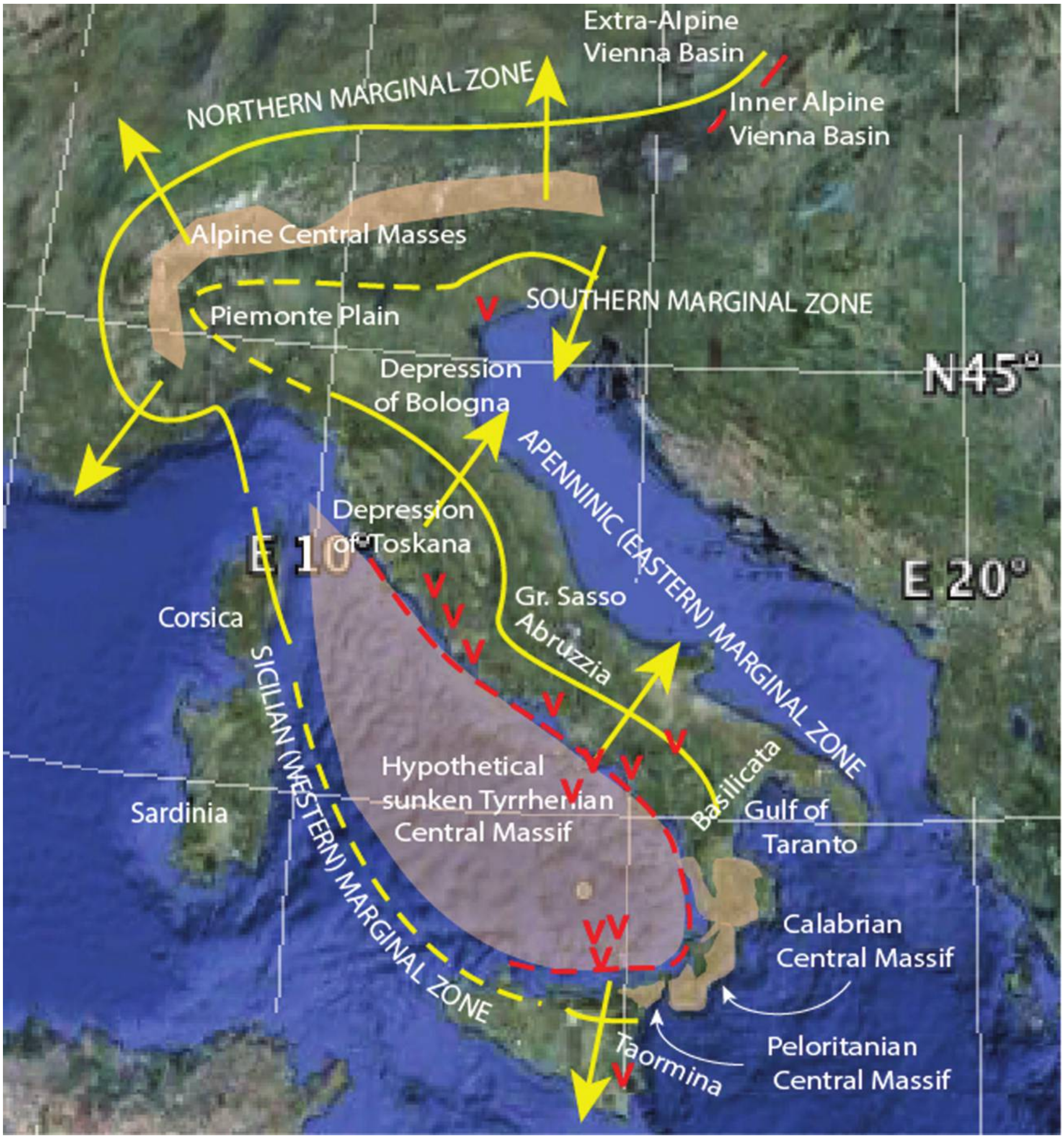


Figure 13. Eduard Suess's interpretation of the structure of the Apennines and the Alps in his 1872 article.

depressions, as opposed to Élie de Beaumont's negative *bosselements*, essentially giant folds in the earth's crust, which Dana later first called geoclinals (1863, p. 722), then geosynclinals (1873, p. 430), and finally geosynclines (1894, p. 106; the positive bosselements allegedly forming the continents). In his epoch-making *Die Entstehung der Alpen* (1875; *The Origin of the Alps*), Suess elaborated on mountain building within the framework of the contraction theory à la Prévost, denying any role to primary vertical uplift in making mountains, but he also found occasion to describe the large late Cretaceous transgression. As yet, there was no word on what he was to call eustatic movements.

His earliest publication on the independent movement of the sea level came out in 1880 in the form of the text of a lecture he gave to the Imperial and Royal Austro-Hungarian Geological Survey (Suess 1880). In that lecture, Suess pointed out that numerous observations on the Quaternary terraces worldwide indicated that it was the sea level that was moving and not land itself. It was seen that the sea was retreating both in the north and in the south, whereas in the equatorial zone it was rising. He pointed out that during the ice ages it had been the other way around. He did not know why that was so, but he decidedly opposed any suggestion that land in the north was now rising and that around the equator it was sinking.

Suess then pointed out that he had long been bothered by cyclic stratigraphic sequences (see what he wrote later about them in greater detail in Suess 1883, p. 16). He thought in some way or another they were the deep-sea responses to the movements of the sea level. He also suspected that sea level changes must have had a serious influence on the geological timetable and expressed his surprise that this table, established in a tiny place in England and continental Europe, should remain valid around the entire globe. This is a crucial point in his thinking, but he did not elaborate on it in the 1880 lecture. He did so in the first volume of his magnum opus, *Das Antlitz der Erde*, in 1883. In 1880, he simply emphasized that there were no primary uplifts of the lithosphere, neither in mountains (all uplift there was due to shortening) nor elsewhere in the continents. He introduced the problem in 1883 with the following words addressed to a fictional student:

> Supposing our listener to have now reached the point, so that he stands on the threshold of stratigraphical geology, and at the same time of the history of life: he will find himself surrounded by an overwhelming mass of details concerning the distribution, stratification, lithological character, technical utility, and organic remains of each subdivision of the stratified series. He stops to ask the question: what is a geological formation?[11] What conditions determine its beginning and its end? How is it to be explained that the very earliest of them all, the Silurian formation,[12] recurs in parts of the earth so widely removed from one another—from Lake Ladoga to the Argentine Andes, and from Arctic America to Australia—always attended by such characteristic features, and how does it happen that particular horizons of various ages may be compared with or distinguished from other horizons over such large areas that in fact these stratigraphical subdivisions extend over the whole globe? (Suess 1883, p. 10)

Notice here that Suess takes the systems to be natural subdivisions of the stratigraphic record and views them as packages of rock laid down uniformly over the entire globe. This is the old, Wernerian idea of "universal formations," as recognized by the great genius Marcel Bertrand, his friend and admirer, in the preface he wrote to the French translation of the *Antlitz*:

> The same alternations of movements and similar deposits of ancient seas are found from the plains of the United States to Russia; all of this had been ignored or hardly supposed; all of this is today classic and incontested. (Bertrand 1897, p. XIV)

Neither Bertrand nor Suess seem to have realized that the systems were artificial entities born in the minds of the stratigraphers based on arbitrary decisions made on diverse criteria. That is why there had been such fierce debates as to their boundaries. Suess knew that:

> This question is certainly obvious and justifiable, but if we could assemble in one brilliant tribunal the most famous masters of our science and could place this question of the student before them, I doubt whether the reply could be unanimous, nay, I do not even know if it would be definite. Certain it is that in the course of the last few decades the answer would not always have been the same. (Suess 1883, p. 10)

While writing these lines, he must have been thinking not only of the Cambrian-Silurian dispute between Murchison and Sedgwick (Secord 1986), the Devonian dispute between those two and Sir Henry de la Beche (Rudwick 1985), or the continuing slicing of the Tertiary into ever narrower subdivisions (Suess 1883, p. 12) but also of his own experience in which the Rhaetian was considered by the French geologists (and by himself for a long time) a part of the Lias, whereas it was thought a part of the Triassic by the German geologists (see Şengör 2014*b*). Only 2 years later, during the Third International Geological Congress in Berlin, the officers of the United States Geological Survey pointed out clearly that the formations, however one is inclined

11. While reading this entire quotation from Suess, it is of utmost importance to bear in mind that when he writes "formation," he means a "system" in our present stratigraphical usage.

12. Here Suess uses the term Silurian in its Murchisonian sense, i.e., including the Cambrian, Ordovician, and Silurian proper.

to take them (as systems or just as mappable units), could not be time units but were only structural, petrographic entities (Powell and McGee 1888). Suess never cited them.

Suess was working around Eggenburg in 1860 and 1861 (Suess 1916, p. 137–139) and published his local results in 1866 (Suess 1866). Darwin's theory had just been published, and he was still a Cuvierian in his paleontology and in his stratigraphy. After *On the Origin of Species* was published in 1859 and after Suess had read it, nothing was the same again for him, and this remarkable book brought its own serious problems into stratigraphy. In the *Antlitz*, Suess continues his discussion of the problems of stratigraphers with a confession by Darwin:

> At this point, Darwin's book on the *Origin of Species* made its appearance:
>
> "But just in proportion as this process of extermination had acted on an enormous scale," says the author, "so must the number of intermediate varieties, which have formerly existed on the earth, be truly enormous. Why then is not every geological formation and every stratum full of such intermediate links? Geology assuredly does not reveal any such finely graduated organic chain, and this perhaps is the most obvious and gravest objection which can be urged against my theory. The explanation lies, as I believe, in the extreme imperfection of the geological record."
>
> In a later passage Darwin says:
>
> "I believe that the world has recently felt one of these great cycles of change; and that on this view, combined with modification through natural selection, a multitude of facts in the present distribution both of the same and of allied forms of life can be explained."
>
> These words, although applying only to the geographical distribution of forms of life at the present day, contain nevertheless the important admission that the development of life has been, according to Darwin, uninterrupted, but by no means uniform; nay, it almost appears as though the reader were to be introduced to a further problem, that of a great and as yet unknown rhythm in the evolution of living beings—a rhythm dependent on episodic changes in the external conditions of existence. (Suess 1883, p. 12–13)

Suess at this point cites Aristotle from his *Meteorologica* (*Μετεωρολογικά*) and gives away what his suspicion is as to the causes of the abrupt changes in the biota:

> Aristotle seems to be alluding to the same problem in the remarkable passage: "The distribution of land and sea in certain regions is not always the same, but that becomes sea which once was land, and that land which once was sea, and there is reason to believe that this change take place according to a definite system and at definite intervals of time." (Suess 1883, p. 13)

After this small—but extremely significant—digression into antiquity, which will reveal its full significance in the rest of *Das Antlitz der Erde*, Suess returns to the paleontological and stratigraphic records:

> More than 20 years have passed since the publication of Darwin's book. Since then observations have multiplied; we are now able with much greater certainty to trace the descendence lines of organisms among the relicts of the past. . . .
>
> The continuity of life is thus more and more clearly illustrated by the results of paleontology; yet the fact remains that we do not find species varying gradually within the limits of single families or genera, and at different times, but that whole groups, whole populations and floras, or if I may so express myself, complete economic unities of nature appear together and together disappear. This is the more remarkable, as the transformations effected by in the populations of the sea and in those of the land by no means invariably coincide: a fact that has been proved in the most convincing manner by a study of the various subdivisions of the Tertiary formation in the Vienna Basin. From this we may conclude with certainty that the determining factors in this case have been changes in the external conditions of life.
>
> It is true that the record is extremely incomplete. A certain proof of this lies in the local recurrence of some groups. The recurrence of certain species of ammonites in the Jurassic system of Central Europe has already been made use of by Neumayr to determine, in their main outlines, the boundaries between the zoological provinces that existed during the several subdivisions of the Jurassic period. Communications have from time to time been established between these provinces and again suppressed; yet not only may the synchronism of the subdivisions in one province and another be determined in many cases with certainty, in spite of subsidiary differences, but throughout the whole earth we see the well-known general type of the Jurassic formation succeeded by the equally well-known type of the Cretaceous; and from this we may conclude that changes must have occurred that have exerted an influence over an area still more extensive than that of these great provinces.
>
> On this fact depends the unity of stratigraphical terminology. The excellent work of English

geologists in East Australia, the reports of the Geological Survey in India, the accounts of our explorers in China and in the arctic regions, the voluminous publications presented to us by North America, as well as the works of German investigators on the Andes of South America, the descriptions of the Cape, and the scantier but most valuable accounts that we have received from the less easily accessible parts of Africa; all these works, when they wish to designate the more important parts of a stratified series, make unhesitating use of terms that were originally chosen to describe the classification of the deposits in a limited portion of Europe. When it is a question of marine deposits, the geologist in New Zealand or Victoria knows as well as his colleague in north Russia or Spitzbergen whether he has Paleozoic, Mesozoic, or still younger deposits before him, and expressions such as "Carboniferous Limestone," "Jurassic," "Cretaceous" have now become naturalized in all parts of the world visited by geologists. (Suess 1883, p. 14–15)

Reading the above passages, one would be inclined to believe that Suess was a naive layer-cake stratigrapher. But he immediately counters any such suspicion in the next paragraph:

> The greater part of this nomenclature originated in England and has obtained general recognition in spite of the fact that certain vast marine deposits occur in Central Europe, the chronological equivalents of which in England bear an entirely different character and are not immediately recognizable. Such are the Triassic formations of the Eastern Alps and the Tithonian series. At the same time Abich, in his works on Armenia, and Waagen and Griesbach in India, are making known to us marine faunas by which the mighty gap occurring in Europe toward the close of the Paleozoic period is being steadily filled up. But more careful consideration easily convinces us that it is not the completeness of the series of marine formations in southeast and central England but rather, so to speak, that mean frequency of gaps among them, which has facilitated the conception of natural groups, in a manner that would never have been suggested by other places where one marine deposit regularly follows another. In those districts, however, where the incompleteness of the series is particularly great, and where, for example, the encroachment of the Cenomanian is apparent, there is a most striking correspondence over large areas and in both hemispheres. It was this correspondence that led me long ago to suppose that the so-called secular elevations and depressions of continents are not sufficient to explain the more limited distribution of some and the wider distribution of other formations, a phenomenon of which the cause, though unknown, must be general. (Suess 1883, p. 15)

Here we immediately perceive two problems concerning the appreciation of the history of geology by Suess. He seems not aware that he too had become a victim of the tyranny of the beds, just like Cuvier and almost all stratigraphers in the nineteenth century. He was too impressed by the similarity not only of fossils but also of facies of different parts of the geological sequences in Europa and America and along the Tethyan chains. He was also too impressed by the abruptness of the changes of rock type and fossils at certain bed boundaries. He was judging similarities of not only fossils but also of beds—which we now know to have resulted from continental drift—on the present-day distribution of continents and oceans. That is why he was not content to correlate only fossils but also wanted to find a rational, phenomenological basis for stratigraphic correlation. He was trying to re-create Werner's world, in which global correlations would be made on rock types. But Suess realized that to do this a process basis had to be found. Simple correlation of beds would not stand up to close scrutiny, as the officers of the United States Geological Survey also saw at the same time.

Although he was led by the tyranny of strata into thinking the divisions of geological time natural units, he nevertheless realized that those subdivisions still needed physical explanations. The development after Suess forgot this critical desideratum and turned into two blind alleys.

Tyranny of Strata Leading to Two Blind Alleys in the Twentieth Century

Episodic Orogeny as an Artifact of Time = Rock Equality. One blind alley was represented by a return to the idea of Élie de Beaumont and to hold worldwide synchronous orogeny responsible for the sea level changes and thus for the universal breaks in the stratigraphical record by such geologists as Dana (he never abandoned this idea during his entire long career, as reflected by the various editions of his immensely influential *Manual of Geology* [Dana 1863, n.d. (1863?), 1864*a*, 1875, 1880, 1894] and *Text-Book of Geology* [Dana 1864*b*; as far as I know, this is the only textbook by Dana that was translated and adapted into another international language: Dana n.d., 1874, 1883], Le Conte (1895), Haug (1900, 1907), Chamberlin and Salisbury (1904, p. 517–562; 1909, p. 542–589), Chamberlin

(1909), Ulrich (1911), Kober (1921, 1928), Stille (1924, 1940), Bucher (1933),[13] Grabau (1936*a*, 1936*b*, 1936*c*, 1937, 1938, 1940[14]), and Umbgrove (1942,[15] 1947). In none of these books is there any major idea that had not been expressed before; they represent them with more or less supporting data than before. Almost all of their authors were ignorant of the details and the foundations of the ideas of their predecessors, even when they cited them! All postulated some kind of global episodicity of orogeny and eustatic events, and some even proposed a kind of cyclicity. Most tried to justify their scheme within the framework of the contraction theory but without any detailed mechanism. Others took refuge in earth expansion or alternating episodes of contraction and expansion. Every single one of them was a step backward from Suess's great work, which was often cited for its rich database but not for its theoretical arguments. His theories were so forgotten that a great geologist such as Pierre Termier, who knew Suess personally, was able to write a shockingly misleading statement: after having written that Suess never claimed anything but simply showed (Termier 1915, p. 717), he continued "but Eduard Suess was never a theorist. This man once accustomed to teaching and conquering, ardent also in political disputes, had for a long time ceased to argue on scientific matters" (Termier 1915, p. 718). See Oldroyd (2006) for a succinct history of all the ideas of cyclicity in the history of the earth, in which he concludes that in our own day, cyclicity in earth history seems to have more adherents than in Hutton's time!

Of all the publications cited above concerning some sort of episodic, global behavior of the earth affecting global stratigraphy, the influence of the work of the German geologist Hans Stille on the geological world internationally (Europe, United States, Russia, Japan, and even China; he was the only non-Soviet geologist whose selected works were published by the Soviet Academy of Sciences in its *Izbrannie Trudi* series even before Stille died; Bogdanov and Khain 1964), including the stratigraphers, was immense and much more decisive in pre-plate-tectonics days, although still today we feel his breeze in many fields in geology. Even such a careful stratigrapher as William Joscelyn Arkell wrote in the midfifties of the twentieth century that the starting point of all modern considerations on the timing of orogenic events was Stille's "monumental book 'Grundfragen der Vergleichenden Tektonik' (1924)" (Arkell 1956, p. 638).

Arkell did not think that the worldwide orogeny idea held water, but he agreed that orogeny was an episodic event. Yet he still felt that he had to point out the following:

> But some spasms in the mobile belts were great enough to affect very large areas. Perhaps a strong spasm in one part of a mobile belt would touch off others at points of weakness or mounting unbalance in distant parts of the globe. (Arkell 1956, p. 641)

But this was essentially what Stille claimed.

Stille was a doctoral student of the conservative and fairly dogmatic German geologist Adolf von Koenen in Göttingen. Von Koenen had obtained his degree in Berlin, where Wernerian tendencies were still perceptible when he was a student (see von Zittel 1899). Stille's dissertation was on the structure of the Teutoburg Forest between Altenbeken and Detmold, where strongly faulted and gently folded Triassic-Liassic beds are covered by much less deformed Neocomian strata. Stille thought here was evidence of pre-Neocomian folding, but von Koenen would have none of it. To him, all post-Hercynian deformation in Central Europe was Cainozoic. So Stille was forced to state in his thesis that the contact between the Necomian layers and earlier beds was tectonic and that all the deformation in the area was Cainozoic in age (Stille 1900). After graduation, Stille entered the service of the Prussian Geological Survey, and the southern part of his former PhD area was assigned to him to map. Stille found there faults along which the Lias had been downthrown into older layers and that there was an unconformity at the base of the Neocomian (as he had already noticed during his dissertation mapping but was not allowed to publish). But he was still hesitant and decided to finish mapping the Teutoburg Forest before making a final statement (Stille 1901, p. 11–12).

Further mapping confirmed that there had indeed been deformation involving faulting (and folding, Stille famously thought, because he interpreted every tilting as an expression of folding! Stille 1910, [1910] 1912, 1913, 1924) older than the Neocomian

13. This book had four reprints: one by Princeton University Press itself in 1941 and three by Hafner in New York in 1957, 1964, and 1968. Only in the last three Bucher found it necessary to point out that he had given up the idea of successive expansion and contractions of the terrestrial globe. The text of the entire book remained unchanged as the thought that it had stood the test of time!

14. Reprinted in 1978 with the addition of a portrait of Grabau and his biography by Albert V. Carozzi. All plates in the reprint are black and white.

15. Reprinted in 1971 by the same publisher.

(Stille 1903, p. 311–312). The following is what Stille concluded from his mapping:

> With respect to the age of these dislocations, one can only say that they are younger than the Middle Lias and older than the Neocomian sandstone. (Stille 1903, p. 322)

This was so far a justified statement, and I do not think any geologist would quarrel with it. But Stille did not stop there. He continued:

> But for all that it is likely that they may have formed just at a time when widespread displacements of the strand and transgressions, i.e., phenomena that are related to the movements of the earth's crust, were going on in our Mesozoic hill country of northwest Germany, namely, in the latest Jurassic or earliest Cretaceous. (Stille 1903, p. 322)

This still would have been all right had it remained a working hypothesis. But let us see what happened afterward. Stille extended his studies to the Osning Zone, immediately to the north and northwest of the Egge Mountains. Here, in contrast to the Egge Mountains, the Jurassic and the Cretaceous are conformable with some local disconformities. In the Portlandian (top Jurassic, i.e., ±Tithonian) of the Osning Zone (in the so-called Serpulit, a limestone characterized by the holes of the Serpulidae, a family of annelid worms; in its lower parts it is a true conglomerate consisting of shell and rock fragments and balls of *Serpula coecervata*; von Koenen 1906) Stille found clasts of Dogger, Malm, and Paleozoic rocks that had been clearly derived from the south, that is, from the Egge Mountains. These Serpulit conglomerates were also deformed, but in the early Cainozoic (Stille 1909). Stille interpreted the geological history of the entire area as follows: First, a phase of deformation had occurred in the Egge Mountains between the late Jurassic and the early Cretaceous. Then the material from the mountains produced by this deformation were shed northward and deposited in the Serpulit, which then became folded in the next phase in the early Cainozoic.

Nothing in his data set forced Stille to this spasmodic model of evolution; the data were only just permissive. To conclude such a stop-and-go model, he postulated that everywhere in the Egge Mountains the Jurassic-Cretaceous boundary was an angular unconformity, and everywhere in the Osning Zone the same boundary was conformable. Otherwise, he could not have supposed that the Egge Mountains as a whole were deformed with one shock during the Jurassic-Cretaceous boundary and then the Osning Zone in toto would have followed with a later shock of deformation. Stille knew that this was not so, but he still made the postulate because only with such an assumption could the stratigraphic sequences be considered independent from one another with no transition between the two: this gave a neater, more regular model and enabled him to set rock equal to time exactly as Élie de Beaumont had done almost a century earlier.

Stille then interpreted what he thought he saw in the Teutoburger Wald in terms of the structure of the molasse basins in front or behind major orogenic belts, such as the Hercynides. He compared the migration of the "orogenic phases" he allegedly established with the migration of deformation toward the molasse basins in major orogens. He thought this migration took place spasmodically, in distinct phases, although by that time it had been established that orogeny caused the formation of the molasse basins by loading and flexing the foreland and that the deformation spread into these basins gradually (e.g., Suess 1909, p. 718). Stille rejected that interpretation and argued that, exactly as in the Teutoburg Forest, in the Hercynides too everywhere in the internal zones the Upper Carboniferous was unconformable on a deformed but internally conformable Devonian sequence and everywhere in the external zones the Mesozoic was unconformable on internally conformable Devonian through Upper Carboniferous sequence. Stille often wrote emphatically that molasse basins generated conformable sequences while they were subsiding and that this was observation (e.g., Stille 1919, p. 347 ff.). However, this was only a postulate he made by generalizing a few observations in areas that were not molasse basins. Stille further made this postulate immune to any future criticism by taking the type example of molasse basins—namely, the Alpine molasse basin in Switzerland, Germany, and Austria—as his target:

> And if a real orogenic phase is one day proven within the molasse time, this would alter nothing in the fundamental interpretation about the episodicity of movement toward the foredeeps and indeed about the Alpine folding itself. Only in such a case the Miocene foredeep time would be divided into two subtimes, during which, however, the absence of a forward movement of the Alpine chain would still apply. (Stille 1919, p. 349n)

Why was he so insistent on his episodic and compartmentalized tectonic interpretation? Because this made it possible to treat beds as perfect time markers, identifying instances of deformation between them. Thus, Stille returned entirely to a Beaumontian worldview of tectonics. His entire tectonic theory, including the geosynclines and the geoanticlines, the orogenic phases, the preferred directions of orogeny (minus the *reseau pentagonal* of Élie de Beaumont), were all Beaumontian concepts, but Stille thought he was taking them from James Dwight Dana (see Dana 1873, 1875, 1894; Stille 1924, 1940)! He seems to have been totally unaware of Suess's criticism of Élie de Beaumont and thus Dana. (Had he not read Ferdinand Löwl's [1906, p. 173] unjustified praise of Élie de Beaumont's model and equally unjustified criticism of Suess in his popular textbook? Had he done so, he would have recognized in both Löwl and Élie de Beaumont congenial soulmates.) He also seems to have been unaware of the distinction between Élie de Beaumont's theory of contraction and Constant Prévost's theory of contraction. He knew that Suess had abandoned the theory of geosynclines but did not bother to see why. His soulmate in Austria, Leopold Kober, also abandoned entirely his teacher Suess's ideas because he did not bother to learn what they were based on and, very especially, in criticism of what earlier theories they had been developed and what the essence of Suess's criticism was. In the second edition of his famous textbook, *Der Bau der Erde*, which, like Stille's two books, had a great influence on tectonics until the rise of plate tectonics and, in certain topics, even beyond, he wrote,

> The evolution of the epeirogen [Kober here means continents] is determined by the fundamental distinction between orogen and cratogen and takes place in great natural periods (cycles). The end effect is rigefaction. This is the total picture of the epeirogen. . . . But it must be emphasized that ideas of this kind, pertaining to the entire earth, are old. They root into the contraction theory of Élie de Beaumont, in the theory of catastrophes of Cuvier, in the geosynclinal theory of Hall and Dana [neither Stille nor Kober knew that the idea of geosynclines went straight back to Élie de Beaumont and that both Hall and Dana had known it but chose not to give the due credit to their predecessor; see Şengör 2003; this shows that here Kober had a superficial idea of the history of his field, but not in sufficient detail to allow him to make a critical judgment], in the theory of "sedimentary cycles" of Bertrand [Bertrand's sedimentary cycles that simply described changes in sedimentary environments and regimes during one orogeny had nothing to do with the global cycles Kober was thinking of, but Kober ignored that critical distinction] and found a rapid rise in America. Dana had already separated the "stable parts" and the "geosynclines," the periods of mountain building. Le Conte pointed out the importance of the "critical times" in the history of the earth [but all of this had been already put forward by Élie de Beaumont and later rejected because it was found inadequate by geologists from Lyell to Suess!]. E. Suess did not enter into this important circle of ideas because he had rejected Dana's geosynclines in principle. In more modern times Barrell, Chamberlin, Schuchert and others pointed out the importance of the theory of cycles. In Germany it was especially Stille who distinguished geological "evolution" from "revolution." Dacqué, Tornquist, Salomon, v. Seidlitz, Kober, Quiring, Sonder, Kossmat, Kraus spoke in favor of the theory of cycles, in France Bertrand, Haug in Italy F. Sacco and others. [I cannot corroborate that many of these authors spoke in favor of cycles that Kober had in mind.] (Kober 1928, p. 19)

Stille's and Kober's ideas did not go unopposed. A whole school of thinking represented by Alfred Wegener and Émile Argand continued Suess's way of doing geology in that they assumed no a priori regularities in the behavior of the planet, rejected perfect global temporal correlatability of stratigraphic sequences, and saw no need to abandon uniformitarianism (see especially Şengör 1982*a*, 1982*b*, 1991). Fritz Kerner von Marilaun already pointed out in 1922 that the inability to precisely temporally correlate geological systems would have critical implications for paleotectonic and paleogeographic interpretations, making the entire theoretical edifice of Stille and other like-minded geologists indefensible. But even among the less theoretically inclined geologists there was objection. Already in 1923 the postgraduate student Francis Parker Shepard in Chicago published a criticism of worldwide synchronous orogeny against the father and son Chamberlins, two of Chicago's mighty pillars in geology (in this connection it helps to remember that Shepard was an undergraduate under Reginald Aldworth Daly at Harvard, whose thinking was along the lines of those of Wegener and Argand). Shepard had organized his attack on three separate lines. First, he showed that the present knowledge of the distribution of orogenic deformation was in no way suitable to induce the law of orogenic synchroneity. Stille could have objected to him by pointing out that Shepard had included cases in his compilation that he, Stille, had earlier disputed (such as his argument against Brouwer's claim that orogeny was going on in the Dutch East Indies; Stille 1920). But Shepard's second line of attack closed that door: he pointed out that the law of orogenic synchroneity was based on circular reasoning and could not be used to document its own premise. Therefore, objections to his compilation on its strength were logically inadmissible. Finally, he showed that the favorite theory of Kober and Stille, the Beaumontian version of the contraction theory (Shepard was unaware that the theory he was discussing was the Beaumontian version), made worldwide synchronous orogeny highly improbable. Twenty-six years later James Gilluly (1949, 1950) made a renewed attack, which unfortunately was far less sophisticated than Shepard's critique, and Stille (1950*a*, 1950*b*) was able to brush it off by pointing out that Gilluly had not bothered to learn properly the theory he had set out to criticize.

After the rise of plate tectonics, it was John Rodgers of Yale University who took up the debate and pronounced in his presidential address to the Geological Society of America on November 11, 1970, the victory of the idea of Suess that orogeny was a semicontinuous random-walk process now interpreted as resulting from the capricious movements of the plates and the continents they carry (Rodgers 1971, p. 1171). One would have thought that this should have settled the centuries-old controversy, especially in view of the nature of the theory of plate tectonics. Alas, it did not! In 1973, the great Alpine geologist Rudolf Trümpy declared himself a moderate Stillean. He wrote,

> Apparently, compressional deformations took place during distinct, relatively short periods; uplift and erosion followed only after a significant delay. . . . In the Alps, at least, the movements involving crustal shortening seem to be spasmodic rather than continuous. (Trümpy 1973, p. 247; see also Trümpy 1972, 1987, p. 66)

But when one reads his article, one sees no trace of worldwide synchronous orogenic phases. Şengör and Bernoulli (2011) pointed out that Trümpy could not possibly have been a Stillean simply because by his own confession he did not fulfill the necessary criteria. Trümpy was simply pointing out that the stratigraphic evidence at his disposal, in his own interpretation, indicated orogenic phases of short duration separated by long periods of repose in the Swiss Alps only. At most, he could have been an Arkellian. Yet even an intellectual giant such as Rudolf Trümpy had not bothered to read Stille carefully or Arkell, although he himself was a Jurassic ammonite specialist just like Arkell!

His colleague from Zurich, Kenneth Jinghwa Hsü, opposed even his spasmodic Alpine model in his Fermor lecture in 1989:

> The interpretation of the episodic nature of orogeny is a relic of the defunct catastrophic theory of orogeny of Élie de Beaumont (1831). Divorced from that discredited assumption, the orogenic data in the Alps are consistent with a postulate of continuous orogenic deformation since the early Cretaceous. (Hsü 1989, p. 421)

The issue was still so hot that Hsü asked me to talk about it during the special symposium on controversies in modern geology on September 4 and 5, 1989, at ETH Zurich on the occasion of his 60th birthday. I complied and in my talk tried to show the basic historical and philosophical roots of the problem (Şengör 1991). In the limited space available, however, I could not go into the basic stratigraphic problem that bedevilled the entire issue from the beginning.

However, this controversy seems finally to have cooled in consequence of the detailed methods of dating now available.

Lesson 8. From all this we learn an eighth lesson from the history of geology. Many hypotheses die and are resurrected again because those who reinvent them no longer remember that they had died and that all they do is resurrect dead ideas. In the case of spasmodic orogeny, none of the authors who repeatedly resurrected it remembered that it is based on Cuvierian stratigraphy and Beaumontian tectonics, that is, two defunct models. It is hard to believe that pre-plate-tectonics geology in the twentieth century was run by ideas stemming from the late-eighteenth and early-nineteenth centuries. They were finally killed by an entirely new data set, that of high-precision isotopic dating and high-precision micropaleontology.

Synchrony of eustatic movements claimed by seismic stratigraphy as a consequence of the tyranny of strata (actually seismic reflections only!). The other blind alley into which geology turned after Suess is of a much more recent date and is thus much more surprising. Its beginning is usually dated to Laurence Louis Sloss's 1963 article on the cratonic sequences of North America, and it is thought to have initiated a "revolution" (Dott 2014), although Sloss himself wrote in 1963 that "the sequence concept is not new and was already old when it was enunciated by the writer and his colleagues in 1948" (Sloss 1963, p. 111).

I hope that the present article has shown the veracity of what Sloss wrote in 1963. However, Sloss himself thought that his predecessors had all been in North America and dated them back to Ulrich's long 1911 article (Ulrich 1911; the longest article the *Geological Society of America Bulletin* ever published), which equated rock with time

(Rodgers 1959) and did untold damage to stratigraphy in the United States because of the dogmatism of its author. Ulrich's article has nothing new in it and many backward steps, such as the synchronism of his sequences. The sequence idea itself goes back to Werner, but in Ulrich's world it was Suess who had reintroduced it in a modern form. Ulrich cites Suess for his tectonic ideas (which he did not wholly comprehend) but not for his stratigraphic ideas. He seems to have been unfamiliar with them. Sloss never cited Suess but did cite Stille (1940), of which at the time there were only four copies in North America (Kay 1974). Larry Sloss was not only a fine geologist but also a good scholar.

Dott (2014) points out that it was in the late thirties and forties, when Sloss was working in Montana, that he became aware of regional unconformities separating distinct sequences. On November 11, 1948, Larry Sloss, William C. Krumbein, and Edward C. Dapples read a magnificent article titled "Integrated Facies Analysis" at a Geological Society of America–sponsored symposium on sedimentary facies in geologic history (Longwell 1949). In that article they defined four unconformity-bound sequences and named them Sauk, Tippecanoe, Kaskaskia, and Absaroka. These sequences were separated by unconformities that were not of the same age everywhere on the North American craton. They were not dissimilar to the regressive breaks Suess had mapped on the earth's continents in the second volume of the *Antlitz* (Suess 1888; see table 1 herein).

Sloss et al. (1949) were very careful in pointing out that time and rock were not equivalent. They wrote,

> A number of elements contribute to the lack of uniformity of treatment of time-stratigraphic units. One of these is the continued acceptance by some workers of the classical belief ["classical" is here probably a polite version of "ancient and wrong"] in the concordance of time and rock units. According to this philosophy, the established major time units, the periods, are natural and universal divisions of geologic time to which the rocks conform. Adherents to this philosophy seek discontinuities in the stratal record of any area and choose certain of these "breaks" as marking the horizons of separations of systems. (Sloss et al. 1949, p. 107)

What Sloss et al. (1949) criticize here is the hypothesis of rock = time equivalence propounded in the United States by Ulrich at the time that had come down from Steno, Scheuchzer, Lehmann, Werner, Cuvier, d'Orbigny, and Dana. It seems that Suess also committed the same error but with large permitted error margins and with the proviso he emphasized that for eustasy to be a useful tool, a general process to create the unconformities observed must be identified. He suggested that it was the contraction-driven subsidence-related enlargement of oceanic basins that caused the regressions and their refilling with sediment that led to transgressions. This also explained the rapidity of the regressions and the slowness of the transgressions. But he was dissatisfied with his own hypotheses. In a letter to his regular correspondent Charles Schuchert at Yale University, Suess wrote,

> When I wrote of eustatic movements in 1883 [he had done so without yet naming them], I confessed that I did not understand the transgressions. I thought that variations in rotation might somehow have influence. I also believed and still think that the accumulation of sediment was a vera causa, but hardly sufficient. Now, after 27 years, I cannot offer you more than a heap of

Table 1. Comparison of the Times of Regression and Transgression of Suess (1888) and the Sequences and Horizons of Sloss and His Coworkers in 1949

Periods and epochs	Suess (1888)	Sloss et al. (1949)	Periods and epochs
Permian	Very limited transgression	Absaroka	Mississippian to ?Jurassic
Late Carboniferous	Oscillations accompanying a regression	Fourth horizon	Mississippian
Later early Carboniferous	Transgression	Kaskaskia	Devonian to Lower Carboniferous
Early Carboniferous	Regression	Third horizon	Middle Devonian to Middle Lower Carboniferous
Medial Devonian	Transgression	Tippecanoe	Middle Ordovician to Middle Devonian
End of Silurian (i.e., Cambrian to Silurian, because used in its Murchisonian sense)	Regression	Second horizon	Middle Ordovician (i.e., stratigraphic break: Chazian to Mohawkian)
		Sauk sequence	Uppermost Precambrian to early Ordovician

> doubts regarding the explanation. I have learnt more and know less about it. (Suess 1911, p. 107)

To the end of his life, Sloss believed, as Suess did, that the sequences, which are *not* time units, were somehow tectonically controlled (Sloss 1963, 1966, 1972; Dott 2014) and that the synchroneities he proposed embraced broad time intervals of emersion of the cratons. I do not think a serious quarrel can be picked with his conclusions, especially because he made a point of emphasizing that his scheme could not be used in geosynclinal areas. His results were very much parallel with those of Suess, although I do not know to what extent Sloss himself was aware of this.

In light of this, imagine my shock when I read the following from the pen of two of Sloss's own students:

> Seismic stratigraphy is basically a geologic approach to the stratigraphic interpretation of seismic data. The unique properties of seismic reflections allow the direct application of geologic concepts based on physical stratigraphy. Primary seismic reflections are generated by physical surfaces in the rocks consisting mainly of stratal (bedding) surfaces and unconformities with velocity-density contrasts. Therefore, primary seismic reflections parallel stratal surfaces and unconformities. Whereas all rocks above a stratal of unconformity surface are younger than those below it, the resulting seismic section is a record of the chronostratigraphic (time-stratigraphic) depositional and structural patters and not a record of the time-transgressive lithostratigraphy (rock stratigraphy). (Vail and Mitchum 1977, p. 51)

One does not know where to begin to take this paragraph apart! The properties of reflections in seismic sections do not automatically allow application of the principles of physical stratigraphy. Not all reflections can be interpreted as beds or unconformities without further ado. A sill in a section of high-velocity sedimentary beds, for example, would look like a sedimentary bed; changes in degree and/or type of induration within one bed may create one or more diachronous reflectors independent of bed boundaries. Second, and critically, the statement that "all rocks above a stratal of unconformity surface are younger than those below it" is simply wrong (see Christie-Blick et al. 1990, p. 124–125), and their teacher Sloss could have told them that, which, in a humorous way, he implied later:

> This group was pre-adapted to recognize unconformity-bounded units on reflection seismic records and they are deeply impressed by the apparent global synchrony of stratigraphic patterns clearly related to the freeboard of continental margins. . . . They find that they sleep well when they place their faith in eustatic sea levels and dream pleasant dreams when glacial controls on eustatics can be invoked. (Quoted from Dott 2014, p. 26)

Sloss never followed individual unconformities but pointed out that he was following regional unconformities that formed from conflation of numerous smaller ones that could be grouped into broad times of emersion of cratons. The entire concept of "seismic stratigraphy," by contrast, was born in an environment of blissful ignorance of the history of geology, and its creators were unaware that they had gone straight back to Wernerian stratigraphy by ignoring tectonics. They made precisely the same mistakes as Cuvier, Élie de Beaumont, Suess, Ulrich, Stille, and Kober, who also appeared unaware of the details of the development of the ideas they were propounding either by taking them from the literature or by reinventing them. Pitman and Golovchenko (1991) showed that tectonics of Atlantic-type continental margins have a controlling influence on the architecture and the chronology of the sequences and that these could be diachronous along the same margin, with a difference reaching 3 m.yr. I myself showed the diachroneity of the sequences along the Pacific-type continental margins (Şengör 1991). The Vail curve failed because its creators forgot that the earth had a dynamism exactly in the way that Werner had ignored it. The packaging of sedimentary sequences in fact look random, showing the suspicious property of representing shorter and shorter intervals of time as they become younger (see Dickinson 1993). This is a property also of Stille's orogenic phases that crowd ever more closely as their ages become younger (Stille 1924, 1935, 1940), an aspect Gilluly (1949) rightly criticized by pointing out that their closer crowding was simply a function of better preservation of younger sequences. In a private note, the late William R. Dickinson appended the following to the offprint he sent me of his 1993 comment herein cited:

> I kept this piece deliberately understated (for effect), but to my mind it simply destroys the Vail/Exxon interpretation—they are hoist by their own petard! Amazingly enough, none of the Exxon crew (so far as I know) has ever noticed or commented upon this inherent property of their own chart! (W. D. Dickinson, written communication, 1993)

Had they known of the same property displayed by Stille's chart of orogenic phases, they might have noticed the same thing in their own chart. Miall (1986) pointed out that the chronostratigraphic correlations of unconformities allegedly indicating global sea level change are in part based on circular reasoning (very much like Stille's orogenic phases; see Gilluly 1949, 1950). Only a process-driven approach—not a purely empiric one, as imagined by Vail and Mitchum (and their many followers, especially in the petroleum industry)—can identify what are called "islands of order" in the sequence (Schlager 2010). Even when the Vail group has invoked processes (e.g., Posamentier and Vail 1988; Posamentier et al. 1988), their grasp of them has been incomplete (Miall 1991) or remained in the shadow of their own enthusiasm of the global synchroneity of their sequences. I have not gone into the continuing debates on seismic and sequence stratigraphy for lack of space, but the interested reader will find an excellent critical introduction in Miall (2010).

Lesson 9. The ninth and final lesson I wish to emphasize in this article are the dangers of misused empiricism. As Lord Bertrand Russell once said, "Whatever presents itself as empiricism is sure of wide-spread acceptance, not on its merits, but because empiricism is the fashion" (Russell 1946, p. 697). The rise and spectacular failure of the so-called Vail methodology is an excellent example of the failure of empiricism when used without a careful consideration of theory. Both of the broad groups of ideas considered by Miall (2004), the allegedly empirical biostratigraphy and the model-dependent stratigraphies, are in reality all model dependent. But their truth value can be checked by intersubjective testing, and therefore the truth itself in stratigraphy is not model dependent.

Conclusions

I have tried to document in this long article that to the end of the twentieth century the same errors and faulty reasoning have recurred many times in the history of geology. I have deliberately concentrated on stratigraphy because it happens to be the most fundamental of all the geological sciences. Von Zittel called it the most purely geological branch of all the branches of geology. The cause of this unfortunate recurrence is that geologists seem not to know much about the origin and evolution of the concepts they use every day. They learn them in a very superficial and commonly misleading form from their teachers or colleagues and take them on board without digging into their history to find out how and why particular concepts and methods were created. Geology is not physics. Physics deals with abstract concepts that stand or fall according to simple observations any physicist can make or to expensive experiments made by large groups of physicists that are reported in the literature (such as the Large Hadron Collider experiments in Geneva). Geology is by contrast done by single geologists in extensive regions that are not practical to resurvey every time one needs to check the statements of the geologist who originally surveyed the area. One has to go back to their original reports and read them critically to make one's own judgement. Reading Stille's 1924 textbook does little good if one has not read his intellectual development from his doctoral thesis to his arrival in Göttingen in 1913, when he started making theoretical claims. To do that, one must carefully read his earlier publications in which he developed his ideas step by step. I think it would have been difficult to follow Stille's conceptual development through his earlier articles and then take his 1924 book very seriously.

This kind of archaeology of geological ideas must be done on original material to avoid errors of translation that can be serious in even the most authoritative translations. For example, Eduard Suess observed in the third volume of the *Antlitz* that an arc once existed north of Spain, in Asturias and Cantabria, defined by folds. Here occurs, in all the foreign editions, one of the graver errors in translation. Suess wrote (I supply my own translation in square brackets after his German):

> Es ist aber schwer zu verstehen, wie sich eine solche neue Curve bilden soll, wenn nicht irgend eine Art frei anspülender Erdwellen vorausgesetzt werden will. [But it is difficult to understand how such a new curve can be formed when a kind of free washing up of earth waves is not assumed.] (Suess 1901, p. 8)

In the authorized English translation, Hertha Sollas translated it as follows:

> But it is hard to conceive how such a curve could be formed a second time, unless we assume the existence of some kind of wave propagating itself freely through the crust of the earth. (Suess 1908, p. 4)

The critical German word here is *anspülen*, which literally means "washing up *onto* something." Suess clearly implies a décollement under the advancing arcs; there is no implication of waves propagating through the entire crust. To the contrary, only an upper flake moves and gets folded atop an undeformed substratum. This is consistent with his earlier statements, as far back as the *Entstehung der Alpen*. Hertha's incorrect translation leads to the loss of the décollement idea in the translation, which happens to be critical here (see Şengör 2013). The very same error in translation recurs in the French and Spanish versions (Suess 1902, p. 8; 1928, p. 9), the latter possibly influenced by the earlier renderings. There are not a few similar mistranslations in all of the various renderings of the *Antlitz* into different languages. They occur almost always in the theoretical parts, not in the straightforward descriptions. That is why the geologist must go to the writings or maps or samples or even to the field(s) of the original authors, and their writings must be read in their original languages. This is a tall order, but there is no getting away from it if one wishes to understand the origins and evolutions of geological concepts and theories. We have seen in one subfield of geology what sorts of problems of amnesia that absence of knowledge and understanding can lead to. Remember that the English translation of the *Antlitz* was edited by Hertha's father, William Johnson Sollas, the holder of the geology chair at Oxford. The French translation was done by Emmanuel de Margerie, an extremely scholarly geographer who was well versed in tectonics and was a close friend of Suess. The Spanish translation was undertaken by a respected geologist, Pedro de Novo y F. Chicarro. Despite all the competence of the translators and editors, the translation of the passage in question was wrong because the translators clearly did not know in any detail the fundamentals of Suess's thinking in tectonics.

In addition, the history of geology, if it is to be useful to the geologist in understanding the backgrounds of the concepts and theories he or she has to deal with professionally, must be written by geologists or by historians with a thorough grounding in geology. Geology is a complex science, and those not trained in it or lacking experience would have great difficulty understanding the problem situations of the past. Histories of geology, written by professional historians without a geological background, are useful to give the geologist a general framework and, in many cases, the better-known (not necessarily the more important) sources. Some historians have gone to great lengths to familiarize themselves with geology and consequently produced fine pieces of work useful to geologists. The writings on specific problems of the historian to whose memory I dedicate this article, for example, are superb examples of writing history of geology with a competent grasp of the problems considered. But I cannot say the same of his general history of geology (Oldroyd 1996) because, as he himself once wrote (Oldroyd 2003), he was unable to read some of the critical publications in their own languages and had to resort to second-hand information published by his fellow historians, which proved misleading.

A thorough understanding of the history of the origin and evolution of concepts in geology are critical for those dealing with those concepts. That history must be written by someone with a competent grasp of geology, and it must be written not as a part of social history but as a part of science. As von Goethe once said, the history of science is the science itself. Those not competent in science cannot expect to understand any stage of its history, however primitive the early stages of a science may appear.

As dating techniques advance, we should be able to date more precisely ever-smaller packages of rock, and a time may come when the geologist's competence in stratigraphy would not be so dependent on his mastery of the history of the development of his science. As of now, that time seems fairly far in the future.

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Abbildungen, die am gehörgen Orte eingefügt sind, unter Angabe der lateinischen und deutschen Bezeichnungen aufs klarste vor Augen gestellt werden Sowie Sein Buch von den Lebewesen Unter Tage: In neuer deutscher Übersetzung bearbeitet von Carl Schiffner unter Mitwirkung von Ernst Darmstaedter, Paul Knauth, Wilhelm Pieper, Friedrich Schumacher, Victor Tafel, Emil Treptow, Erich Wandhoff: Agricola-Gesellschaft beim Deutschen Museum, zweite Auflage, in Kommission. Düsseldorf, Deutscher Ingenieur, XXXII + 564 p.

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