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Schöpf, Maclure, Werner, and the Earliest Work on American Geology

Abstract. *Schöpf produced the first substantial report on the geology of North America, a work that has long suffered unjustified oblivion. In at least one feature this treatise is superior to that of Maclure (the "Father of American Geology")—Schöpf attempted interpretation whereas Maclure did not. The suggestion is here offered that Maclure, who adopted Werner's classification of the stratigraphic succession, was simply shying away from Wernerian interpretation because he did not like the looks of it.*

We honor William Maclure (1763–1840) as the "Father of American Geology," and in a strictly practical sense this is a just recognition, for it was indeed Maclure's work that started things moving in the study of American geology; and yet, Maclure was not the actual pioneer. He was preceded some 22 years earlier by another man who produced a much larger work (*1*) which, however, in utterly mysterious circumstance went virtually unknown in America for more than a century (*2*). This, admittedly, is hard to believe, but extensive investigation leaves it just such—one of those freakish, incomprehensible oddities that do turn up now and then in the long course of human history.

This other man was a young German army surgeon, Dr. Johann David Schöpf (1752–1800), who came over with the Hessian troops hired by King George III of England to help combat the rebellion in the American colonies, and at the end of the war, instead of going straight home with the soldiers, he stayed here for some 8 months and traveled as far west as Pittsburgh and as far south as Charleston and St. Augustine, gathering data on just about every aspect of American life and environment that anyone could well be expected to observe. The geological part of all this he published in the work just mentioned (*1*) and the other parts of general interest in a two-volume work (*3*) that was translated into English by Professor Alfred Morrison of Hampden-Sydney College and published in 1911 at Philadelphia in an edition (*4*) that itself soon fell into the category of rare books, but fortunately a facsimile reproduction (*5*) has recently been issued. I say "fortunately" because the "Reise" is well worth having by anyone who is at all interested in a picture of the newborn United States that evolved through the eye and mind of a highly intelligent and cultured European naturalist who was not only trained in all of his education and experience to observe sharply and critically, but more than that, was definitely a member of the avant-garde in his time (the time, we might recall, of Goethe, Voltaire, and Rousseau). Further, the "Reise" should be of special interest to geologists, geographers, and economists for its many mentions and descriptions of commercially important mineral deposits over and above those dealt with in the "Beyträge," and other natural resources as well.

The main purpose of this note is to bring the whole of Schöpf's American presence to attention, to compare one aspect of his treatise on geology with that of Maclure, and to offer a suggestion that has apparently never been ventured in discussions of the history of geology.

Schöpf's work was done mainly between late July 1783 and January 1784. This was the time when the science of geology was just beginning to take form. In other words, there was no geology; Schöpf had to create his own. (Note that the title of his book is "Contributions to the *mineralogical* knowledge. . . .") The most important feature in which his work is superior to that of Maclure is his constant attempt at interpretation, whereas Maclure modestly abstained from any such enterprise. As Schöpf made his way across our Coastal Plain, Piedmont, the great limestone valleys, and the Allegheny-Appalachian province, for nearly everything he saw he tried to understand its origin, to explain it—to interpret. Some of his ideas are remarkably sound and prescient; some are wrong or otherwise deficient, but in all such cases he was cautious and in many he realized that he must be in error even though he could not think of anything better. (For example, he thought the whole area, in essentially its present kind of configuration, must have been flooded by universal ocean, but he most positively was uncomfortable with the idea, recognized specific objections to it, and said frankly that he must be missing something, which was indeed the case.)

Now Maclure would have none of such business. He said (*6*), "In adopting the nomenclature of Werner, I do not mean to enter into the origin or first creation of the different substances, or into the nature and properties of the agents which may have subsequently modified or changed the appearance and form of those substances; I am equally ignorant of the relative periods of time in which those modifications or changes may have taken place; such speculations are beyond my range, and pass the limits of my inquiries."

I fail to see how anybody can read that statement and still call Maclure a Wernerian. If this is not a disclaimer I do not think I have ever seen one. Further, it has occurred to me that Maclure, the canny Scot, was not merely avoiding elaborate speculation as to possible origins, but was just not going to associate himself with ideas (Wernerian) that did not look any too good to him. We remember Maclure's work, and rightly so, first of all for his map, the first of its kind in America. It is hard to believe, however, that he studied and recognized the rock units he mapped, which he was at pains to define and distinguish as formations, without any thought as to their origin and subsequent history. And he was smart. There is nothing inherently wrong with Werner's major division of the rocks that make up the visible crust. On the other hand it is one of the axioms learned sooner or later by every serious beginner in geology that almost all of Werner's ideas about the origin and structure of these rocks were gorgeously wrong. I cherish more than a hunch that before 1809 Maclure realized the probable validity of this now proverbial dictum, and I have been pleased recently to see that White (*7*) has found basis in later evidence for a similar conclusion.

Why, then, did Maclure not either say so, or offer a few ideas of his own? Here we may only speculate, but let us remember that (i) geology was yet in its infancy and (ii) the power of Werner's word, propagated by men who had been virtually hypnotized by this unquestionably marvelous genius of a teacher, was something we today may find hard to understand or appreciate. At all events put me down in Maclure's corner.

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Magnesium-Iron Replacement in Clay Minerals in Anoxic Marine Sediments

Abstract. *In anoxic marine sediments iron is extracted from clay minerals to form a sulfide. Magnesium from the surrounding solution then replaces the extracted iron in the clay mineral structure. This process removes magnesium from seawater and serves as a control on the composition of interstitial waters in sediments.*

The residence time of dissolved magnesium in seawater is short as compared with the age of the oceans (*1*). Hence, over geological time, magnesium must be removed from seawater at approximately the same rate as it is supplied to the ocean by rivers. Apart from atmospheric recycling, the removal process must consist of the incorporation of magnesium into the solid phases of sediments. I propose here that a reaction of the type

$$2\,\mathrm{Fe}_{(\mathrm{clay})} + 3\,\mathrm{Mg}_{(\mathrm{soln})} + 4\,[\mathrm{S}] = 3\,\mathrm{Mg}_{(\mathrm{clay})} + 2\,\mathrm{FeS}_{2(\mathrm{pyrite})} \tag{1}$$

occurs in anoxic marine sediments. In Eq. 1 [S] represents some form of reduced sulfur or sulfide ion derived from the metabolic reduction of seawater sulfate, and $\mathrm{Fe}_{(\mathrm{clay})}$ represents ferric iron at a site at which no exchange of cations may occur in a clay mineral. Ferric iron is used in this discussion rather than ferrous iron, as the ferric form is more abundant in marine sediments; a similar equation could be written for ferrous iron. Both ferrous and ferric oxides and silicates are unstable with respect to pyrite in anaerobic marine sediments (*2*). An example of this type of reaction would be the conversion of the nontronite component in a montmorillonite to a saponite component:

$$\begin{aligned}&(\mathrm{X})_{0.66}\mathrm{Fe}_4{}^{3+}\mathrm{Al}_{0.66}\mathrm{Si}_{7.34}\mathrm{O}_{20}(\mathrm{OH})_4 + \\ &\quad 6\,\mathrm{Mg}^{2+} + 8\,\mathrm{SO}_4{}^{2-} + 15\,[\mathrm{C}] + 13\,\mathrm{H}_2\mathrm{O} = \\ &\qquad (\mathrm{X})_{0.66}\mathrm{Mg}_6\mathrm{Al}_{0.66}\mathrm{Si}_{7.34}\mathrm{O}_{20}(\mathrm{OH})_4 \\ &\qquad + 4\,\mathrm{FeS}_2 + 15\,\mathrm{HCO}_3{}^- + 11\,\mathrm{H}^+\end{aligned} \tag{2}$$

In Eq. 2 (X) represents exchangeable cations and [C] represents reduced carbon. Similar reactions could be written for iron-containing chlorites and illites. The net effect on the clay is to change its chemical composition without markedly changing its mineralogy, as determined by x-ray diffraction.

The processes of sulfate reduction and pyrite formation have been well described in the past (*3, 4*). No detailed study has been made, however, of the source of iron for pyrite formation. Berner (*5*) stated that the reduction process could be responsible for the extraction of iron from a range of silicate minerals, but he did not investigate resulting changes in the silicates.

In Recent marine sediments from Banderas Bay, Mexico (*6*), the amount of nonexchangeable magnesium (*7*) is higher in clays from sulfide-rich environments than in clays from less strongly reducing environments. The clay fraction ($< 2\ \mu\mathrm{m}$ in equivalent spherical diameter) of these sediments consists of montmorillonite, kaolinite, and illite. There are no systematic differences in mineralogy between the clay fractions of the oxidizing and reducing sediments. The differences in the magnesium content cannot be explained on the basis of differences in the percentage of montmorillonite, the principal magnesium-containing phase (Fig. 1). The behavior of calcium indicates that the magnesium distribution cannot be explained on the basis of the formation of a carbonate phase. In interstitial water squeezed from the reducing sediments, magnesium was depleted relative to the overlying seawater, but the bulk of the magnesium taken up by the sediment must be provided by the diffusion of magnesium into the sediment from overlying seawater. Pyrite is present in the silt fraction ($> 2\ \mu\mathrm{m}$) of the reducing sediments. It appears that when iron is removed from the clay fraction of the sediment to form a sulfide in the silt fraction, a corresponding amount of magnesium enters the clay fraction from the surrounding solution (Fig. 2). Part of the scatter in Fig. 2 is caused by differences in the mineralogy of the sediments. The reaction shown in Eq. 2 should cause a lowering in the *p*H or a dissolution of calcium carbonate, or both. The *p*H is lowered (to 7.0 to 7.5) in both oxidizing and anoxic sediments, presumably by carbon dioxide from the oxidation of organic matter. Calcium carbonate is precipitated in the anoxic sediments, however, which suggests that Eq. 2 is an oversimplification of the process of metabolic sulfate reduction. Berner (*8*) has shown that the decomposition of organic matter in sediments can