Brush, Stephen G. (1996) *A History of Modern Planetary Physics*. Cambridge University Press.

Brush, Stephen G. (1996a) *Nebulous Earth: The Origin of the Solar System and the Core of the Earth from Laplace to Jeffreys*. Vol. 1. Cambridge: Cambridge University Press.

> Like many controversies in science, this one stimulated the participants to seek more evidence for their theories. Kelvin suggested that experiments should be done to measure the tides of the solid Earth—that is, the vertical motion of the Earth's surface associated with lunar and solar positions—in order to determine the rigidity of the Earth. The results seemed to confirm his claim that the Earth is "as rigid as steel." (Brush 1996a: 143)
>
> By the beginning of the 20th century, geologists had been forced to accept the doctrine that the Earth is *completely* solid. Not only did they abandon the contraction theory as an effective explanation of mountain building and earthquakes; they were also reluctant to consider any horizontal movement of parts of the Earth's crust, since a solid moving through a solid would encounter enormous resistance. Thus when Alfred Wegener proposed his theory of continental drift in 1912, it was rejected on grounds of physical impossibility. (Brush 1996a: 143)
>
> The rigidity of the Earth was so strongly emphasized by the solidists that it was difficult for several new ideas to get a fair hearing. The acceptance of a liquid core, suggested by seismological evidence as early as 1906, was delayed for two decades; my guess is that this was in part because of reluctance to contradict Kelvin's doctrine of complete solidity. When the Oldham—Gutenberg discontinuity was eventually interpreted as a transition between solid and liquid regions, this conclusion was seen as a new discovery, not a reversion to 19th-century ideas. (Brush 1996a: 174)
>
> The solidist victory was probably a major reason for the hostile response to Wegener's theory of continental drift when it was first proposed. Here we have some fairly clear evidence of the influence of the 19th-century debate in a remark of R. D. Oldham at a discussion of Wegener's theory in 1923:
>
>> I can remember that when I started as a geologist the idea was not unknown that a good deal of geological evidence was, at any rate, not inconsistent with the notion that the continents have not always occupied the positions on the surface of the globe which they do now. But I also can remember very well that in those days it was unsafe for anyone to advocate an idea of that sort.
>>
>> The physicists, who before that had forced on us the notion of a fiery globe with a molten interior and thin crust on it, had gone round and insisted on a solid globe, and any notion of the shifting of continents was incompatible with that theory. Those ideas held the ground so strongly that it was more then any man who valued his reputation for scientific sanity ought to venture on to advocate anything like this theory that Wegener has nowadays been able to put forward. (Oldham 1923: 180)
>
> -- (Brush 1996a: 174)

Brush, Stephen G. (1996b) *Transmuted Past: The Age of the Earth and the Evolution of the Elements from Lyell to Patterson*. Vol. 2. Cambridge: Cambridge University Press.

> When Alfred Wegener proposed his hypothesis of continental drift in 1912, he could make little headway against the prevalent belief in a rigid Earth (see Oldham's recollections, quoted in *Nebulous Earth*, 2.2.10). Wegener himself, in a 1927 statement that he later repudiated, mirrored the contemporary feeling that physical evidence is superior to geological evidence:
>
>> I believe that the final resolution of the problem can only come from *geophysics*, since only that branch of science provides sufficiently precise methods. Were geophysics to

Brush, Stephen G. (1996) *A History of Modern Planetary Physics*. Cambridge University Press.

> come to the conclusion that the drift theory is wrong, the theory would have to be abandoned by the systematic earth science as well, in spite of all the corroboration, and another explanation for the facts would have to be sought. (Wegener 1929/1966: vii)

--(Brush 1996b: 54)

Brush, Stephen G. (1996c) *Fruitful Encounters: The Origin of the Solar System and of the Moon from Chamberlin to Apollo*. Vol. 3. Cambridge: Cambridge University Press.

> With Chamberlin's view of geological processes involving rearrangement of the solid materials of the Earth's interior, lubricated by temporary melting and driven by the heat of gravitational contraction, one might have expected a more favorable climate for hypotheses such as continental drift. But in fact Chamberlin did not establish a new worldview and did not support Wegener's continental drift hypothesis in spite of his remarks in the 1896 lecture about differential motion of the crust. (Brush 1996c: 37)
>
> As Frankel (1976) noted in connection with Wegener's theory of continental drift, a theory that ranges over many diverse fields is unlikely to perform as well as a special theory in each individual field, and since most scientists are specialists rather than generalists they judge a theory primarily by how well it accounts for the evidence in their own field. He sees this as the main reason why Wegener's theory was rejected for several decades even though it had greater overall explanatory power than the established theories in geology, geodetics, paleoclimatology, and biology. (Brush 1996c: 87)

Brush, Stephen G. (1996a) *Nebulous Earth: The Origin of the Solar System and the Core of the Earth from Laplace to Jeffreys*. Vol. 1. Cambridge: Cambridge University Press.

In the introduction to volume one, Nebulous Earth, he writes:

> The origin of the Solar System is one of the oldest unsolved problems in science. It was first perceived as a scientific question distinct from the origin of the universe as a whole, in the 17th century. The introduction by Copernicus of the heliocentric theory made it meaningful to use the modern phrase "Solar System." Astronomers began to think of the Sun as one of many stars; it became conceivable that our Solar System was one of many such systems, and that it had been affected or even created by celestial bodies from other systems. René Descartes, in the 1630s, developed a qualitative hypothesis for the development of the Solar System within a larger system, using his theory of vortexes.[1] Thus the most fundamental question one could ask about the origin of the Solar System is: Did it develop autonomously along with the Sun itself, or did it come into existence because of the action of outside entities? (Brush 1996a: 3)
>
> Twentieth-century astronomers have argued that these two alternatives, known as the "monistic" and "dualistic" kinds of theories,[2] lead to radically different conclusions about the probability of finding life elsewhere in the universe. If the development of our Solar System was monistic, then we may infer that planet formation is a natural consequence of star formation, and hence there are many habitable planets. But if a dualistic process like the close encounter of two stars is needed to explain the origin of the Solar System, then because of the great distance between stars, planet formation will be a rare event and the chance of life extremely small.[3]
>
> [Why must the assumption be that the two alternative theories are mutually exclusive; in a universe of billions of galaxies, with perhaps trillions worlds, it seems probable that their origins have transpired by a multitude of means, some of which we have yet to discover.]
>
> Sometimes people want to know the presently accepted "right answer" to a question before studying its history. Is the monistic or dualistic theory really correct? The last time I consulted

the experts, they were quite convinced that the origin of the Solar System was monistic, although they disagreed about some important aspects of planetary development. But the history of planetogony during the last two centuries doesn't give much reason for confidence that this conclusion is final. Throughout the 19th century scientists accepted the monistic Nebular Hypothesis; then they switched to a dualistic theory (close encounter of another star with the Sun). But this theory was rejected after 1935, and a monistic theory (collapse of a gas—dust cloud) was revived in the 1940s. Between 1976 and 1984 the dualistic "super—nova trigger" theory was accepted, then rejected. It was revived in 1995. The time scale for reversing the answer gets shorter and shorter as one approaches the present, giving us very little reason to think that today's answer will still be considered correct tomorrow.[4] That's why I said that the problem is unsolved.

For the historian of science, this uncertainty about the correct answer does have one important advantage. It undermines the tendency to judge past theories as being right or wrong by modern standards. This tendency is the so-called "Whig interpretation of the history of science" that one usually finds in science textbooks and popular articles.[5] The Whig approach is to start from the present theory, assuming it to be correct, and ask how we got there. For many scientists this is the only reason for studying history at all; Laplace remarked, "When we have at length ascertained the true cause of any phenomenon, it is an object of curiosity to look back, and see how near the hypothesis that have been framed to explain it approach towards the truth" (1966: vol. 4, 1015).

But Whiggish history is not very satisfactory if it has to be rewritten every time the "correct answer" changes. Instead, we need to look at the cosmogonies or planetogonies[6] of earlier centuries in terms of the theories and evidence available at the time.[7]

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[1] He mentioned in his 1637 "Discourse on Method" a treatise that he had prepared but decided to withhold from publication after hearing of the Church's condemnation of Galileo's heliocentric system. The work was first published posthumously in 1664 and was reprinted with his collected works (Descartes 1664/1824b).

[2] According to Haar & Cameron (1963), this terminology was first used by Belot.

[3] The effect of making the monistic or dualistic assumption in estimating the abundance of life in the galaxy has been discussed by Drake (1962), Pearman (1961/1963), and Huang (1965, 1973). One might suspect that wishful thinking on this point leads to bias in favor of monistic theories. But James Jeans, an advocate of a dualistic theory, seemed to relish the idea that we are alone in the universe, the chance product of an event that will probably never happen again (Jeans 1932: 2-5).

[4] For 20th-century theories see the sequel to this book, *Fruitful Encounters*.

[5] The term comes from historian Herbert Butterfield (1931), who used it to characterize the presentation of British political history as progress toward Whig (liberal) democracy and the tendency to judge each person or event on the basis of helping or hindering that progress. Butterfield (1944) was able to find some value in "whig history" in a book he wrote during World War II. One of the first to use the term in connection with the history of science was George W. Stocking, Jr. (1965); a detailed critique of a similar approach, "inductivist history of science," was published earlier by Joseph Agassi (1963).

[6] From now on I will use the term *planetogony* to refer to the origin of the Solar System, as distinct from the more general term *cosmogony* … "

[7] In their enthusiasm for denouncing Whiggism some historians have gone to the opposite extreme, priggism (E. Harrison 1987): intentional ignorance of modern science, and refusal to recognize that the significance of events at one time may depend on events at a later time. Even Butterfield recognized that "history is not the study of origins; rather it is the analysis of all mediations by which the past was turned into our present" (1931: 47). The anti-Whig doctrine of "contextualism," though overall much superior to the Whig interpretation, implies the existence of an objective historical reality independent of the historian (Novick 1988)—a thesis for which there seems to no direct evidence, and which is contradicted by modern quantum physics. For recent critiques and defenses of contextualism see Brush (1995).