Theories of the origin of the solar system 1956–1985

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Attempts to find a plausible naturalistic explanation of the origin of the solar system began about 350 years ago but have not yet been quantitatively successful. The period 1956–1985 includes the first phase of intensive space research; new results from lunar and planetary exploration might be expected to have played a major role in the development of ideas about lunar and planetary formation. While this is indeed the case for theories of the origin of the moon (selenogony), it was not true for the solar system in general, where ground-based observations (including meteorite studies) were frequently more decisive. During this period most theorists accepted a monistic scenario: the collapse of a gas-dust cloud to form the sun with surrounding disk, and condensation of that disk to form planets, were seen as part of a single process. Theorists differed on how to explain the distribution of angular momentum between sun and planets, on whether planets formed directly by condensation of gaseous protoplanets or by accretion of solid planetesimals, on whether the "solar nebula" was ever hot and turbulent enough to vaporize and completely mix its components, and on whether an external cause such as a supernova explosion "triggered" the initial collapse of the cloud. Only in selenogony was a tentative consensus reached on a single working hypothesis with quantitative results.

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I. INTRODUCTION

A. The solar system in the First Space Age

Attempts to find a plausible naturalistic explanation of the origin of the solar system began about 350 years ago (Descartes, 1664) but have not yet been quantitatively successful, making this one of the oldest unsolved problems in modern science. Most historical accounts have concentrated on the period from mid-18th to mid-20th century (Jaki, 1976, 1978, 1981; Merleau-Ponty, 1976, 1977, 1983; Numbers, 1977; Brush, 1978a, 1981, 1987a; Treder, 1987; other works cited in Brush and Landsberg, 1985, pp. 168–183). Thus only those who have actively participated in cosmogonic research or have made a special effort to keep up with the voluminous technical literature are familiar with what has happened in the last 30 years. Popular accounts, even those written by participants, generally fail to do justice to the recent history; review articles are often composed by advocates of a particular theory and do not even mention all of the other theories.

Since the period 1956–1985 coincides almost exactly with the First Space Age—from *Sputnik* to the *Challenger* disaster—one might expect that new results from lunar and planetary exploration played an important role in the development of ideas about lunar and planetary formation. While this is indeed the case for selenogony, it was not true for the solar system in general. Theorists occasionally suggested measurements that planetary probes could make to test predictions or provide constraints on models, but there is little evidence that such measurements influenced planetogonic[1] ideas. Ground-based observations were frequently more decisive. Conversely, particular missions or experiments were sometimes justified on the grounds that they would provide planetogonic information when in fact they had little direct relevance to the origin of the solar system (Witting, 1966, p. ii, 1969, p. 195).

The space program did have an important indirect effect on our subject by creating a planetary science community and encouraging the development of sophisticated experimental and computational techniques. NASA funded the research of several theorists and enabled them to work out the consequences of their assumptions on computers. Conferences held to discuss plans and results of the space program provided convenient forums for discussion of the origin of the solar system—which was, after all, prominently mentioned as a major goal of the program in nearly every official announcement about it.

But the space program had a greater impact on ideas about the formation of the solar system by making possible new kinds of observations of the universe beyond our own system. Infrared, x-ray, and gamma-ray telescopes on artificial satellites placed in orbit outside the Earth's atmosphere, combined with high-tech ground-based observations, provided crucial data on the early stages of star formation and on abundances of certain atoms and isotopes considered significant in planetogonic theories.

Theories were also influenced by research on solar system material that is delivered to Earth free of charge: the meteorites. The Allende meteorite, which fell in 1969, contributed significantly more to our understanding of the solar system than the lunar samples obtained by the *Apollo 11* mission that year.

Has the tremendous quantity of empirical knowledge we now possess about the solar system answered the question of its origin? Can we explain the existence and properties of the planets any better now than we could 50 or 100 years ago? Is it still true, as Boris Levin complained a few years ago (1972), that we have no consistent picture of the formation of the solar system? Have we simply accumulated a lot of facts that we do not understand?

By "do not understand" I mean that the most elementary question cannot be confidently answered: does a solar system naturally form by itself from primordial matter, or does it require assistance from a previously existing star or other entity? On the answer to this question depends the frequency with which planetary systems can be expected to form in our galaxy and hence the probability that other intelligent life may exist.

Other "elementary" questions that few theories even try to answer: why are there nine planets? Why does Bode's law or a similar numerical rule give such a good fit to the sizes of most planetary orbits? It seems that a relatively small amount of effort has been devoted to the final outcome of the planetary formation process compared to its early stages (McCrea, 1974; Levy, 1985, p. 11).

B. Planet formation as a contemporary process

Although we are still profoundly ignorant on these and other aspects of planetogony, we do have a new outlook on the subject, more optimistic than that prevailing in the 1930s and 1940s. The origin of our own system is no longer seen as a mysterious event that took place in the distant past under conditions vastly different from those prevailing in the universe today, and hence almost inaccessible to objective inquiry. Instead it is currently believed that the solar system was formed in an environment similar to that which exists now throughout the galaxy, by processes many of which can be directly observed.

One reason for this view, aside from the above-mentioned observations hinting at star and planetary formation, is that the *age* of the Earth and the rest of the solar system seems now to be established quite accurately and is much less than that of the galaxy or the universe. Up to about 1952 the age of the universe inferred from the Hubble constant for expansion was actually *less* than the best value for the age of the Earth as estimated from radiometric dating (1800–2000 million years compared to 3000–3500 million years). Even if one ascribed large uncertainties to both estimates, it still appeared that the solar system was formed during the infancy of the universe and hence under conditions unlike the present ones. The discrepancy was removed when the astronomers revised their distance scale. The currently accepted value of the age of the Earth, first proposed by Clair Patterson in the mid-1950s, is 4500 million years, compared to values ranging from 10 000 to 20 000 million years for the galaxy and the universe (Patterson, 1953, 1956; Patterson *et al.*, 1955; Tera, 1980; Brush, 1987b, 1989).[2] On the basis of these numbers astronomers conclude that the Earth is relatively young[3] and was formed by processes similar to those we might be able to observe in the galaxy today.

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[1] We use this word for convenience and also to indicate that in discussing the origin of the *solar system* one usually includes the origin of the sun, asteroids, satellites, meteorites, and comets only insofar as they seem relevant to *planet* formation. Like "selenogony" (referring to the origin of Earth's moon) and "cosmogony," the word "planetogony" can denote either the origin of the entity or a theory of that origin.

[2] On the age of the universe see Van den Bergh (1981), Edwards (1982), and Hodge (1984).

[3] Of course 4.5 billion years is *old* relative to most earlier estimates of the age of the Earth; in particular, radiometric dating completely demolishes "young-Earth creationism" (Brush 1982c).

Although radiometric dating thus favors what geologists might call a "uniformitarian" view of the formation of the solar system it allowed a "catastrophic" theory of the origin of Earth's moon to emerge. Before 1969, one could extrapolate the history of the lunar orbit back to an epoch, more recent than the Earth's own formation, when the moon was captured by or ejected from the Earth; this event might even be supposed to have left surviving traces on the Earth's surface. When it was discovered that some lunar rocks are at least as old as the oldest terrestrial rocks, such theories were excluded. Other evidence from lunar samples seemed to refute *all* pre-1969 theories. Instead, selenogony turned for guidance to a theoretical picture of the solar system as it might have been just before the present planets emerged from a battlefield of colliding smaller objects, and postulated an impact far more spectacular than anything that could plausibly be imagined to occur in the past 4000 million years.

The preplanetary stage has recently become observable: satellite and ground-based observations in the 1980s have detected disks formed from solid bodies surrounding nearby stars (Aumann *et al.*, 1984; Black *et al.*, 1984; Harper *et al.*, 1984; Smith and Terrile, 1984; Appenzeller and Jordan, 1987). Claims to have detected planetary companions of other stars were occasionally made before 1956 but remained of doubtful validity; in the last three decades such claims have been increasingly frequent and somewhat more credible (Reuyl and Holmberg, 1943; Strand, 1943; Van de Kamp, 1956, 1986; Gatewood and Eichhorn, 1973; Heintz, 1978). Planetogonists have the opportunity right now to make predictions that will soon be testable on systems other than our own.

II. METHODOLOGY

A. Facts to be explained

Historians of science generally have little use for philosophical analysis; it would just get in the way of telling a good story. Unfortunately in this case our story lacks a dramatic climax. Despite much talk by the actors themselves about "clues" and "detective work," the mystery has not been solved; the perpetrator has not been unambiguously identified, and we are not even sure we have found the murder weapon. Yet it may be precisely because of the lack of a satisfying conclusion that this period in the history of cosmogony offers an excellent opportunity to see how science makes cumulative progress by small steps, and how scientists frustrated by the slow pace of progress, yet certain that they are gradually developing more powerful methods of research, may be forced to articulate their goals and procedures in ways that would be unnecessary if they could point to sensational discoveries. And when scientists speak philosophically it is only fair that we pay attention to what philosphers say about science.[4]

TABLE I. Facts to be explained by theories.

A. The sun has a very small part of the angular momentum of the solar system despite having most of its mass; the giant planets have most of the angular momentum.
B. There are 9 planets.
C. The orbital motion of the planets is quite regular: (a) all move in the same direction around the sun; (b) their orbits are nearly coplanar; (c) their orbits are nearly circular. But the small deviations from regularity may be significant.
D. The set of orbital radii of the planets follows an approximate numerical pattern (Titius-Bode law); the radius of the asteroid belt also fits this pattern.
E. There are two groups of planets: "terrestrial" with masses less than or equal to that of the Earth and high densities; "giant" with much greater masses and lower densities.
F. The giant planets have many satellites, nearly all with direct orbital motion; the terrestrial planets have few satellites or none.
G. There are distinctive differences in the chemical compositions of the terrestrial and giant planets. Earth and other terrestrial planets have much greater solar abundances of Li, Be, B, but much less H and He.
H. Most planets have direct rotation (same sense as orbital motion) but the rotation of Venus is retrograde and Uranus has its rotation axis nearly perpendicular to its orbital axis.
J. There are numerical regularities in the distances of satellites of giant planets from their primaries.
K. There is a belt of small bodies ("asteroids") between Mars and Jupiter.
L. The Earth has a satellite that is unusually large compared to its primary.
M. Numerous comets and meteors move through the solar system with somewhat irregularly distributed orbits.

Let us begin with the formalities. Every popular novel has to have a bedroom scene. Similarly, every survey article on the origin of the solar system includes a list of "facts that any satisfactory theory must explain." I have collected in Table I all the items included in at least two such lists, and in Table II, I have noted the priority assigned to these items by the various authors.

Ostensibly the purpose of presenting such a list is to establish criteria for eliminating inadequate theories and for choosing the best from those that are adequate. But that purpose is almost never accomplished, since *no* theory is generally believed to give a convincing explanation of all the facts.

One might suspect that each author would be tempted to stress those facts that his own theory could best explain. One or two authors may have succumbed to this temptation, but for the most part those with radically

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[4] A comparable period of slow progress occurred in physics and chemistry in the late 19th century; the philosophical writings of Ernst Mach, Pierre Duhem, Wilhelm Ostwald, and other scientists comprise one legacy of that period.

TABLE II. Priorities of facts (Table I) according to various authors.

	A	B	C	D	E	F	G	H	J	K	L	M
McCrea (1963)	1	2	3	10	5	6		4		8	7	9
Witting (1966, 1969)	1		4	4	8		10					
ter Haar (1967)			1	2	3	4			4			
Herczeg (1968)	4		1	2	3	8	5	7				
Williams and Cremin (1968)	2	1	3	6	4		5					
Woolfson (1969)	1	2				4	3	6		7	8	
McCrea (1974)		1		5		6	3	4				
Mestel (1974)	2		1	3	5	4			4			
Dormand (1973)	2	1		6		4	3	7		8	8	
Hartmann (1978)	10		1	4	5	9	5	3	9			8
Williams (1979)	1	2	3	5			4					

different theories have agreed on the list of facts to be explained.

I conclude therefore that the primary function of presenting these lists is merely to inform the reader about the domain of subject matter to be discussed rather than to provide a serious basis for choosing the best theory. But even this purpose is not well served, for the lists fail to reflect changes in the boundaries of the domain during the period under discussion. In particular, hardly any author listed star formation or isotopic anomalies in meteorites, yet much of the discussion after 1960 centered on those topics. Witting (1966, 1969) is a notable exception; he called for specific measurements directly related to facts of theoretical significance, such as isotopic abundances, and for theoretical and observational research on star formation.

The list has about as much relevance to the discussion of planetogonic theories as the bedroom scene has to the plot of the typical novel. And indeed it can also mislead the reader by emphasizing creation at the expense of development. How do we know that all the facts listed in Table I were established at the *beginning* of the solar system? Perhaps the regularities of planetary motion are not original but were slowly established over billions of years by dissipative processes and perturbations. "There is no point in building an elaborate theory of the formation of planets to explain some apparently clear-cut present-day property of the system if in fact that property is not really original at all but has come about otherwise" (Lyttleton, 1968, p. 5).

B. Prediction or explanation?

One step higher on the ladder of methodological analysis is the issue of prediction versus explanation. Should more credit be given to a theory that not only explains known facts about the solar system but also allows the theorist to derive facts to be discovered in the future?

Many scientists have accepted Karl Popper's (1959) doctrine that in order to be truly scientific a theory must lead to predictions that can be refuted by new experiments or observations. This "falsifiability criterion" has recently caused considerable mischief because Popper himself used it to conclude that Darwinian evolutionary theory is not scientific but is merely a "metaphysical research programme;" it is used to explain what has already happened but does not determine what will happen in the future (Popper, 1976, pp. 167–180). If applied in Popper's original sense, the criterion would put not only evolutionary biology but much of geology and astronomy outside the boundaries of science, since those disciplines study phenomena that take place over such broad domains of space and time that they cannot be brought into the laboratory for controlled experimentation, and it would seem that predictions could not be checked for thousands or millions of years. The demand that theories make testable predictions has therefore been seen as just another attempt to force all sciences into the mold of physics and chemistry; defenders of the autonomy of other sciences argue that they should be expected to provide only plausible explanations, not predictions (Mayr, 1965, 1985; Munson, 1971; Halstead, 1980; Lewin, 1982; L. Laudan, 1983, p. 11; but see M. B. Williams, 1981, and references cited therein for testable predictions of evolutionary theory). But a recent comprehensive analysis of historical case studies shows that, even in the physical sciences, prediction plays a fairly minor role in the assessment of theories (Laudan *et al.*, 1988, pp. 18–20, 29; see also Brush, 1990a, 1990b).

At the 1984 conference on the origin of the moon, when one participant argued that we should take seriously only those hypotheses that are testable, another retorted that such behavior would be comparable to that of the drunk who looks for his wallet only under the street lamp (Brush, 1988, p. 52; cf. Hartmann *et al.*, 1986, p. vi). In other words, the set of theories that can be definitely tested by present-day observations is relatively small and may not include the one that is later accepted as correct.[5]

Nevertheless there have been some striking cases of testable predictions deduced from theories about the past. The test in each case was the discovery of previously unknown evidence about the past rather than the occurrence of a new event, but I see no reason why such a discovery should not count as the test of a prediction. (As W.M. Kaula has remarked to me, it is not the phenomena themselves that are predicted but the observations thereof.) The classic example is Charles Darwin's prediction that the earliest hominids would be found in the place where the primates most closely related to humans (according to his theory) now live, namely, Africa; the prediction was confirmed more than 50 years after it was published.

A second famous example is the prediction made from George Gamow's "big bang" cosmology that space should now be filled with "fossil" background radiation whose frequency distribution is given by Planck's law and corresponds to an absolute temperature of a few degrees Kelvin. The discovery (but not the prediction) was rewarded by a Nobel Prize and is generally considered the best evidence for the theory (Alpher and Herman, 1949, p. 1092; Penzias and Wilson, 1965; on the history of this topic see Weinberg, 1977, Chap. VI).

The third example is more relevant to our present topic and also shows that the *refutation* of a prediction may have a significant effect on the progress of science. Harold Urey, from his hypothesis that the moon was captured by the Earth after being formed elsewhere in the solar system, predicted that water and other substances would now be present near its surface. Following the analysis of lunar samples, Urey recognized that his predictions about the chemical properties of the moon had been proven false and he abandoned the theory (Urey, 1976; for a detailed discussion see Brush, 1982a). In fact most scientists decided, on similar evidence, that *all* pre-*Apollo* theories had been refuted; this cleared the way for a completely different theory to be developed—a theory that would previously have been dismissed as too improbable (the giant-impact theory; see Sec. XI.B).

An important point, often neglected in philosophical discussion, is that a theory cannot gain credit through successful predictions unless it is already considered acceptable on other grounds. This is illustrated by the case of planetary rings, discussed in Sec. XII.E.

Although the testing of specific predictions is not a common feature of research on the early history of the solar system, it cannot be completely excluded as a method for evaluating hypotheses. The respect accorded to this method is indicated by the use of the word "prediction" to mean nothing more than the deduction of known facts (McCrea, 1960, pp. 260 and 261; Brush, 1990a).

Explanation is thus a major function of theories. As Lyttleton (1968, p. 5) points out, scientists "cannot really quite relax" until they are assured that "the laws of science are sufficiently comprehensive to allow the solar system to happen"—we demand that the origin of the solar system be explained without invoking any supernatural events.

C. Deduction or naturalism?

Indeed the dominant philosophy of most physical scientists is better characterized as the view that theories should be *deductive* than that they should be predictive, the latter being only a special case of the former. If one could derive the major properties of the solar system from a few simple and plausible postulates, without using adjustable parameters or *ad hoc* hypotheses, one would have a successful theory even if no *new* facts were predicted.[6]

The most extreme version of deductivism was articulated by James Jeans, who asserted that one should start from a simple initial state, deduce its evolution, and then look around in the real world to see if one could find anything corresponding to the result (Jeans, 1917/1919, p. 18). The opposite extreme is the "naturalistic" or "actualistic" method advocated by T. C. Chamberlin and Hannes Alfvén: one should start from facts and processes observed in the present-day solar system and gradually work back to earlier stages, without ever postulating an idealized initial state (Alfvén, 1978, pp. 26 and 27; Arrhenius, 1974). Yet Chamberlin and Alfvén do postulate former states of the solar system from which they try to infer its evolution following physical laws. In fact, Alfvén's theory has probably generated more testable predictions than any other.

Deduction is the preferred method of theorizing, subject to the important qualification that most theorists no longer aspire to derive the entire process starting from diffuse matter and ending with today's solar system. Instead they divide the process into distinct parts in which different phenomena are supposed to be important, and are content to tackle one part at a time. Thus one calculation shows how a dense molecular cloud collapses to a hot protostar; another follows the condensation of a gas-dust nebula to streams of small solid particles in Kepler orbits around a central star; a third shows how planetesimals might accumulate to form terrestrial planets. Induction still coexists with deduction, not too

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[5] "I've got the Kuhn & Popper knee-jerk philosophy of science blues . . . . Any hypothesis can be falsifiable if it's sufficiently dumb." J. Chester Farnsworth, quoted in H. S. Horn (1986, p. 573).

[6] But it seems that such a theory would necessarily entail *some* predictions of presently unknown facts; cf. Lyttleton (1961, p. 54).

peacefully, especially in the field of meteoritics, whose leaders insist that they can confidently infer from the structure and composition of chondrules a former high-temperature stage not recognized in astrophysical models (Boynton, 1985, p. 772; Wasson, 1985, pp. 136, 156, 157, 184, and 185; Wood, 1985, p. 701; Wood and Morfill, 1988, pp. 342–344).

D. Theory change

A more general question of interest to historians and philosophers of science, going beyond issues such as prediction/explanation and deduction/induction, is how does a scientific community decide to accept or reject a theory? The question arises because detailed study of several specific episodes shows that scientists do not generally follow the rules of "scientific method" as decreed by traditional philosophy of science. They do not abandon a theory just because one of its predictions has been proven false, and they do not change from one theory to another because of the result of a "crucial experiment" (Brush, 1974).

A well-known attempt to explain theory change is Thomas S. Kuhn's *Structure of Scientific Revolutions* (1970). Kuhn describes an idealized process in which a "paradigm" comes to dominate a scientific discipline, then is undermined by the discovery of anomalies that it cannot explain, and finally is replaced by a different paradigm that may not be objectively superior in all respects but includes a new way of looking at the world and new criteria for judging theories. Although no actual episode of theory change conforms exactly to Kuhn's description, important features of the idealized Kuhnian revolution can be found in the adoption of Newtonian physics, Darwinian biology, quantum mechanics, and plate tectonics.

Kuhn's theory has limited usefulness in understanding the recent history of planetary cosmogony because there has been no fundamental revolution in planetary science, despite an immense increase in the quantity of empirical data and the emergence of several new theoretical concepts in the last 50 years.[7] There is no single paradigm in the sense that Newtonian or quantum mechanics provided paradigms for physics; instead, researchers in planetary science follow the paradigms they learned in astronomy or geology or chemistry or physics. The phenomenon of "partial incommensurability," which Kuhn invoked to explain the difficulties of communication between followers of the old and the new paradigms within a science, is much more striking in planetary science; it does not disappear through the fading away of an old paradigm, but it may be ameliorated as scientists learn to work within two or three paradigms at the same time.[8] This outcome would be contrary to Kuhn's theory, which denies the possibility of coexistence of more than one paradigm in a scientific community and generally ignores the interaction of different sciences.

The strongest case for the existence of a single shared paradigm in planetogony has been made by George Wetherill (1988), who claims that the community has now arrived at "a more normal type of science, which is not simply pursued by eccentric, elderly gentlemen who fight with one another's theories," but rather one in which "one can undertake finite tasks and receive rewards in the form of an audience and perhaps even employment." This is because the community has agreed on a "standard model of solar system formation, which of course has many bifurcations and branch points and mainly poses questions rather than gives answers." The existence of such an agreed model gives meaning and value to the solution of special problems such as "whether or not planetesimals would be expected to disrupt as they go by a planet." Yet he considers it inappropriate to call this model a "paradigm"—a term that "has meaning only in retrospect, not in the real time advance of science."

A. G. W. Cameron does accept the term "paradigm;" he claims that there has been a transition in solar system cosmogony that has been "marked by a widespread acceptance among the relevant scientific communities of a central paradigm that would allow individual investigators to address parts of the overall problem." According to this paradigm, star formation is the beginning of a monistic process leading to sun, planets, satellites, and comets. Papers that do not assume the central paradigm need not be discussed in a review article (Cameron, 1988, pp. 441 and 442).

Reflecting the acceptance of this terminology, the Astronomical Society of the Pacific scheduled sessions on "Constructing a Paradigm for the Formation of Planetary Systems" and "Testing the Paradigm" at its Centennial Meeting held in Berkeley in June 1989.

I would add that it is only insofar as the community does agree on a standard model or paradigm that one can

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[7] Alfvén (1981, p. 3) refers to his own theory of plasmas as a paradigm, but according to Kuhn's definition this is not correct unless the scientific community accepts it.

[8] "In my opinion our difficulty in understanding the nature of the energetic chondrule- and CAI-forming processes stems not from a shortage of data or observational opportunities, but from the fact that the problem lies squarely on the boundary between two dissimilar disciplines, mineralogy and astrophysics. Mineralogists lack the background to understand the forces and processes in the nebular regime where their meteorites were formed; astrophysicists lack the background to evaluate the conflicting interpretations they hear from mineralogists, or to appreciate the reality of the constraints that meteoritic data place on nebular processes. Solution of the chondrite problem will require a degree of collaboration between the two disciplines that has not yet been attempted." J. A. Wood (1985, pp. 701 and 702).

understand why no credit is given to successful predictions based on *nonstandard* models (see Sec. XII.F).

Yet this limited agreement on a standard model has been achieved only quite recently, if at all (Sec. XII.G). To describe the earlier period, the "methodology of scientific research programs" proposed by Imre Lakatos (1970, 1971) may be more useful. A "research program" is not as all-encompassing as a paradigm; it is a series of theories developed for a specific purpose, in competition with other programs. Lakatos makes the important point that scientists do not treat theories as independent entities, each to be tested against evidence and accepted or rejected on its merits; instead, a scientist will continually modify his or her theories in the light of new evidence. A minor part of the theory (an "auxiliary hypothesis") may be dropped and replaced by one that agrees better with observations, but the new theory obtained in this way will share a set of basic assumptions (the "hard core") with the earlier one. Lakatos asserts that scientists do not directly test theories against observations, and do not abandon a research program just because one or several of its predictions have been proven false; instead they look at the track records of competing programs and may switch their allegiance to a more successful one. Lakatos claims to avoid the irrationality of Kuhnian paradigm changes by providing objective criteria for judging the track record of a research program as "progressive" or "degenerating." He does not claim that scientists actually follow these criteria in the short term, but that the long-term behavior of the community can be explained in this way.

Historians and philosophers of science have published a number of criticisms and modifications of the Lakatos methodology. In an earlier study of the history of planetogony (Brush, 1978a), I pointed out that Lakatos' claim, that scientists do not abandon a refuted theory unless a better one is available, was contradicted by the general rejection of the tidal hypothesis after 1935 despite the absence of any acceptable alternative. Thus it should be clear that I do not consider Lakatos' theory an accurate description of theory change in science. Nevertheless I find it a useful idealized model for discussing the behavior of scientists in choosing and modifying theories, just as Kuhn's theory is a useful idealized model for discussing the behavior of scientists in articulating and switching paradigms. Both theories have stimulated research in the history and philosophy of science by suggesting new hypotheses about the behavior of scientists, even if those hypotheses have not been verified.

E. Scientometrics

Although my interest is primarily in the intellectual rather than the social or institutional history of science, I think there is some value in compiling data on the support and dissemination of research. In particular, I have made considerable use of the *Science Citation Index (SCI)* developed under the direction of Eugene Garfield at the Institute for Scientific Information in Philadelphia. With the help of the *SCI* it is possible to locate most of the articles that referred, during the last three decades, to any specified publication. By actually reading these articles—*not* just by counting the citations—one can gain a fairly good idea of how the major planetogonic theories were regarded by the scientific community during this period. Unfortunately, the *SCI* does not include citations from most of the important conference proceedings and book-length surveys on planetary science; my own research suggests that these publications are absolutely crucial for substantive assessments of theories.[9]

TABLE III. Publications supported by funding agencies.

	1956-1965	1966-1975	1976-1985	30-year total
NASA	26	71	161	258
NSF	7	30	51	88
AEC/ERDA/DOE	16	12	7	35

In the course of preparing the bibliography for this review, which of course included verifying each citation, I decided that with a small amount of additional effort I could record the institutions at which the authors conducted their research, and their sources of financial support. This information was noted only when it was stated in the article itself, and only when the article was related in some way to planetogony and was published in the period 1956–1985. Tables III and IV present the results of this compilation.

The total number of publications cited on planetogony in the 30-year period was 608, of which 116 were for the period 1956–1965; 198 for 1966–1975; and 294 for 1976–1985. Table III shows that NASA, the major supporter of research during each decade, sponsored 55% of the publications in 1976–1985 and 42% for the entire period. According to Table IV, Chicago and UCSD

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[9] *SCI* does not cover the conferences on the origin of the solar system edited by Jastrow and Cameron (1963), Reeves (1974), Wild (1974), and Brahic (1982); it does not cover the cooperative review volumes on "Protostars and Planets" edited by Gehrels (1978) and by Black and Matthews (1985). It does not cover the proceedings of the Soviet-American Conference on Cosmochemistry, edited by Pomeroy and Hubbard (1977). It covers some but not all of the *Proceedings* of the Lunar and Planetary Science Conferences held annually at Houston since 1970; it covers *none* of the volumes of *Lunar and Planetary Science*, containing long summaries of papers presented at those conferences [and in some cases not published in the *Proceedings* or elsewhere, e.g., Cameron and Ward (1976)]. It apparently does not cover most of the proceedings of the Soviet conferences on cosmogony, *Voprosy Kosmogonii.*

TABLE IV. Institutions of authors of publications.

	1956-1965	1966-1975	1976-1985	30-year total
California Institute of Technology and/or Jet Propulsion Laboratory	5	14	36	55
University of Chicago and/or Yerkes Observatory	11	17	22	50
Harvard and/or Smithsonian Astrophysical Observatory	6	9	27	42
University of California at San Diego, La Jolla	11	18	13	42
NASA-Ames Research Center, Moffett Field, CA	0	8	24	32
(Shmidt) Institute of Physics of the Earth, Moscow	10	10	7	27
Massachusetts Institute of Technology	2	11	8	21
Goddard Institute for Space Studies, New York	10	7	1	18
Max-Planck-Institutes, Germany	0	2	17	19
University of Arizona, Tucson	2	6	11	19
Royal Institute of Technology, Stockholm, Sweden	3	11	4	18
Rice University, Houston	0	2	15	17
Carnegie Institution of Washington and/or Mt. Wilson and Palomar Observatories	3	2	11	16
Yale University	3	8	3	14
University of California at Los Angeles	0	4	9	13
Goddard Space Flight Center, Greenbelt, MD	3	2	7	12
Australian National University, Canberra	4	5	2	11
University of Cambridge	4	6	1	11

were the institutions at which the largest numbers of papers were written from 1956 through 1975, but they were overtaken by Caltech/JPL, NASA-Ames, and Harvard/Smithsonian for the decade 1976–1985.

III. OVERVIEW

A. Monistic versus dualistic theories, hot versus cold

From the modern viewpoint the most fundamental feature of a theory of the origin of the solar system is the relation it postulates between the formation of the planets and the birth of the sun. Cosmogonies that assume both arose from a single process may be called "monistic;" those that take for granted the prior existence of the sun and attribute the rest of the solar system to the action of an alien entity such as another star are "dualistic."[10]

According to current ideas about the structure and evolution of the galaxy, the probability of stellar encounters is so small that relatively few planetary systems could have been formed by such a dualistic process (see, however, Woolfson, 1979, pp. 245 and 246). But if our system was formed by a monistic process it is reasonable to suppose that the formation of stars in general (or at least the formation of single stars) is accompanied by the formation of planets. Thus to estimate the abundance of life in the galaxy one needs to know whether the formation of our own system was monistic or dualistic (Drake, 1962; Pearman, 1963; Huang, 1965, 1973).

Monistic versus dualistic is an astronomical classification of cosmogonies; of more interest to Earth scientists is the initial temperature: hot or cold? Planets formed by condensation from a hot gas would presumably have different physicochemical properties and geological histories than those assembled from cold solid particles.

Two major theories proposed in the 18th century employed opposite astronomical assumptions but had the same geological consequences. Buffon (1749) invoked a huge comet to eject planet-forming material from the sun; the comet became a star in later dualistic theories. Laplace (1796) imagined the planets to have formed from rings spun off by the hot extended, rotating contracting

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[10] According to D. ter Haar (1967) this terminology was first used by Belot.

atmosphere of the sun; this, combined with William Herschel's scenario for star formation from nebulae (1811), became the *nebular hypothesis*, the paradigm of monistic cosmogonies. Both Buffon and Laplace assumed that Earth and the other planets started as hot gaseous spheres that gradually cooled and solidified. This assumption became the basis for most 19th-century geological theorizing (Lawrence, 1977).

In the 1840s a major advance in physics showed that one could have a hot primeval Earth without assuming a hot primeval nebula. The law of energy conservation, and in particular the thermodynamic transformation of mechanical to thermal energy, indicated that an extended cold cloud, collapsing by its own gravitational attraction, would become hot enough to power the sun's present output for 20 million years or so (Helmholtz, 1854; Kelvin, 1862), while allowing the planets to start their existence as gaseous or molten balls.

The nebular hypothesis was closely related to the evolutionary world view popular among 19th-century natural and social scientists (Brush, 1987a). It gave a plausible explanation of how the complex present could have developed from a simple past through the gradual operation of identifiable causes. It was only in the 20th century that "evolution" came to mean Charles Darwin's random, harshly competitive, undirected process.

Lord Kelvin, calling himself an evolutionist (1894, p. 77), used the hypothetical hot origin of the Earth and Fourier's heat conduction theory to estimate the Earth's age as less than 100 million years, perhaps only 20 million years. This caused serious difficulties for Darwin, who had thought much longer periods were available for biological evolution by natural selection. The resulting controversy helped to establish the superiority of physics over the other sciences, despite the fact that Kelvin's estimates were refuted after the discovery of radioactivity (Burchfield, 1975; Brush, 1978b, 1987b).

The American geologist T.C. Chamberlin challenged Kelvin's result in 1899, pointing out that there was no convincing evidence for a hot origin; the Earth might have been formed by accretion of cold particles, in which case Kelvins' calculational method could not put any limit on its age. Chamberlin was able to use another argument from physics to undermine the hot-origin hypothesis: according to the kinetic theory of gases, if the Earth had even been hot enough to vaporize iron, molecules of lighter elements would have acquired velocities great enough to escape from the Earth's gravitational field entirely. Thus the Earth would have lost its atmosphere and oceans.

Chamberlin called his primeval particles "planetesimals" and, with F. R. Moulton, worked out a detailed cosmogony (see Brush, 1978a, for a historical account of their work and its reception). He recognized that planets formed by accretion would not necessarily have retrograde rotation (as had earlier been inferred from a simplistic application of Kepler's Third Law), but could have direct rotation (if one takes account of Kepler's First and Second Laws).

Having shown that the nebular hypothesis failed to explain the distribution of angular momentum within the solar system, along with other deficiences, Chamberlin (1905) and Moulton (1905) introduced a dualistic hypothesis. They postulated that the tidal force exerted by a passing star released hot gases from the sun. These gases, which originally formed the arms of a spiral nebula, eventually cooled and condensed to planetesimals. The resulting planets are thus "children" of the sun, deserted by their wandering father whose present location is unknown.

A similar dualistic theory was proposed independently by James Jeans (1917/1919) and Harold Jeffreys (1917, 1918a, 1918b) in England. Jeans also provided another argument against the nebular hypothesis: if the matter in the present solar system were spread uniformly throughout its volume, gravitational forces would not be strong enough to start the condensation process (Jeans' stability criterion). As a result, the "tidal" or "encounter" theory was generally accepted by astronomers until about 1935. But Jeans and Jeffreys did not adopt Chamberlin's cold-origin hypothesis; Jeffreys argued that even if solid planetesimals could be formed, their mutual collisions would quickly vaporize them.

The tidal theory was abandoned because of criticism by the American astronomer Henry Norris Russell (1935) and others in the 1930s. One objection was dynamical: the tidal interaction could not put material into orbit with the required angular momentum. Another was thermal: according to Eddington's theory of stellar structure, based on radiation rather than convection, the gases extracted from the sun would have a temperature of about a million degrees. Atomic speeds would be so great that the gases would escape into interstellar space before they could condense.[11]

B. Revival of the nebular hypothesis

Although it appeared in the early 1940s that both possible theories, monistic and dualistic, had been refuted, new ideas provided a plausible basis for reviving a monistic theory. Russell's own earlier work inspired one of these ideas. Confirming the discovery of Cecilia Payne (1925), he found in 1929 that the sun is mostly hydrogen and helium. If one assumed that the primeval "nebula" (no longer identified with the nebulae seen through the

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[11] The dynamical argument was reconfirmed by Lyttleton (1960). More recent models which allow for some convection lead to lower estimates for the temperature near the sun's surface, but do not invalidate the argument. These models also take account of thermonuclear reactions, which rapidly destroy deuterium, and thus indicate that the Earth and other solar system bodies that contain significant amounts of deuterium could not have come from a mature sun (Burbidge *et al.*, 1957, p. 618; Fowler *et al.*, 1961; Cameron, 1965, p. 557).

astronomer's telescope) had the same composition as the present-day sun, it must have been much more massive than the present-day planets and must have subsequently lost most of its hydrogen and helium. The enhanced density would make it easier for processes such as gravitational instability, viscosity, and turbulence to start the condensation of gases and dust.

Another new idea was "magnetic braking." If the early sun had a strong magnetic field and the early nebula was ionized, lines of magnetic force would be trapped in the plasma and transfer momentum to it. In this way one might explain why most of the angular momentum of the present solar system is in the giant planets, rather than in the sun as one would have been expected from the nebular hypothesis.

These two ideas were exploited by C. F. von Weizsäcker (1944, 1988) in Germany and Hannes Alfvén (1942a, 1942b, 1943a, 1943b, 1946, 1954, 1960a, 1960b) in Sweden, respectively. The net result was a movement away from the dualistic cosmogonies dominant in the first third of the 20th century toward monistic cosmogonies (Brush, 1981). While it was generally agreed that the planets could not have been formed from material pulled out of the sun,[12] it was not universally accepted that the sun and planets came from the same nebula. Thus Alfvén, and Shmidt (1944, 1949, 1958/1959) in the U.S.S.R., postulated that a previously formed sun captured material from interstellar space—either a single "protoplanetary cloud," as Shmidt's followers called it, or several different clouds (Alfvén). Such theories could be called dualistic, although they ascribed a different role to the two actors in the creation drama; the planets are adopted rather than natural children of the sun.

Most theorists also accepted T.C. Chamberlin's hypothesis that the Earth and perhaps all the planets formed at fairly low temperatures, by the accumulation of small solid particles called "planetesimals," rather than by the cooling and contraction of a hot gaseous ball. There was considerable disagreement as to whether there had been a high-temperature stage before the formation of planetesimals.

Among those who developed nebular theories in the United States, Gerard Kuiper (1951a, 1951b, 1951c, 1956, 1974) and Harold Urey were the most influential. Kuiper had initially judged the origin of the solar system a problem not yet soluble by direct attack, so he turned instead to what he considered an easier problem: the origin of double stars. He then developed a picture of the solar system as an "unsuccessful" double star (Kuiper, 1973; on Kuiper's impact on modern astronomy see Waldrop, 1981). Kuiper postulated a massive solar nebula, about $0.1M_\odot$ (exclusive of the mass of the sun itself), i.e., about 100 times the present mass of the planets, and assumed that it would form large protoplanets by gravitational collapse (Jeans instability). After the planets formed, the excess material would be blown away by the sun's radiation pressure.

Urey started from Kuiper's theory but soon rejected the protoplanet hypothesis, assuming instead that numerous smaller objects of asteroidal and lunar size were first formed and later accumulated into planets. He was primarily interested in explaining the chemical properties of solar system constituents and in elaborating the consequences of his assumption that the moon was formed before the Earth and later captured by it. Both Kuiper and Urey employed primarily qualitative or semi-quantitative reasoning.

Urey (1952b, p. 153) argued that the high abundance of hydrogen in the primeval solar nebula should be taken into account in research on the origin of life; the first organic compounds could have been formed under reducing conditions in the Earth's early atmosphere, while later stages in the process took place as the hydrogen escaped and conditions changed from reducing to oxidizing. The famous experiment by Urey's student, S. L. Miller (1953), which initiated a new epoch in research on chemical evolution, was thus indirectly inspired by the revival of the nebular hypothesis with the help of the Payne-Russell discovery of the high cosmic abundance of hydrogen.

Shmidt's theory was developed by V. Safronov and others throughout the 1960s and 1970s; it became primarily a model for the accumulation of solid particles from the protoplanetary cloud into planets.[13] Safronov worked out the quantitative results of the model by analytic approximations. Evgeniya Ruskol applied the theory to the formation of the moon by simultaneous accretion in orbit around the Earth. Safronov's model was adopted, with some modifications, by G. W. Wetherill in the United States; he explored its consequences with the help of computer calculations. The Safronov-Wetherill model is now considered the most plausible one for the formation of the terrestrial planets, though it does not yet account quantitatively for their properties.

The theory of Woolfson (see footnote 12) and Dormand develops Shmidt's capture hypothesis in a different direction, leading to gaseous protoplanets rather than

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[12] The presence of significant amounts of Li, Be, and B in the Earth is considered by many planetogonists to be conclusive evidence that the planets were not formed directly from stellar material, since those elements would have been destroyed by nuclear reactions (Reeves in Dermott, 1978, pp. 4–6). The theory of M. M. Woolfson (1960, 1964, 1978) is the major exception; he postulates that the planets came not from the sun but from a passing light, diffuse star or protostar, presumably one in which nuclear reactions had not yet begun.

[13] According to Shklovskii (1988), Shmidt's theory was promoted so aggressively in the U.S.S.R. that scientists holding other views felt somewhat intimidated, creating for a few years a situation comparable to Lysenko's domination in genetics.

planetesimals as the precursors of planets (Dormand and Woolfson, 1971, 1974, 1977, 1989). But while Safronov's theory discarded the capture hypothesis and became part of the dominant (monistic) paradigm in the 1970s, Woolfson and Dormand remained in the dualistic camp and their work was ignored, except in England, by others who advocated gaseous protoplanets.[14] I.P. Williams is perhaps the only scientist outside of Woolfson's group who has carefully evaluated their theory; he says it "is capable of explaining most of the features of the solar system" and that he "can see no fundamental fault in this theory" although, "perhaps because of personal bias," he prefers another type of theory (Williams, 1975, p. 49).

Of the theorists still active in 1985, Alfvén is undoubtedly the one who has pursued a cosmogonic research program most persistently for the longest period of time, starting in 1942. His original suggestion that magnetic braking of a plasma cloud could transfer angular momentum from the sun to the planets (thus accounting qualitatively for Fact A in Table I) was adopted by Kuiper, Hoyle, and other theorists even though they rejected other aspects of this theory. Alfvén postulated that the basic structure of satellite and ring systems, as well as of the solar system as a whole, was determined by the nature of plasmas and the process by which they condense (the "critical velocity" effect and "partial corotation"). In addition he proposed that inelastic collisions between planetesimals in orbit around the sun would focus them into a "jet stream," thereby promoting accretion of planets. In his work with Arrhenius he dropped the earlier assumption of separate formation of the planet-forming clouds and adopted a monistic scenario.

C. The last three decades

The most striking new feature of the period 1956–1985 was the role played by isotopic anomalies. Although these anomalies have little bearing on most of the traditional problems of planet and satellite formation, they were believed to offer important clues to the initial stages of formation and contraction of the solar nebula as related to nuclear processes in the sun and other stars. The best known example is the "supernova trigger" hypothesis, based in part on the excess ^{26}Mg found in the Allende meteorite; the earlier history and recent demise of this hypothesis are not so well known. Starting with the discovery of excess ^{129}Xe in the Richardton meteorite by J.A. Reynolds in 1960, theorists reasoned that a short-lived isotope (in this case ^{129}I) must have been synthesized in a supernova, ejected into the interstellar medium, and incorporated into a meteorite parent body that cooled down enough to retain xenon gas, all within a period of only about 100 million years. Since a supernova explosion also produces a shock wave that might compress rarefied clouds to densities high enough for them to become unstable against gravitational collapse, the isotopic anomalies might indicate that a supernova *caused* the solar system to form (Cameron, 1962b).

If a supernova is *necessary* to produce a planetary system, then one loses an attractive feature of monistic cosmogonies, viz., the inference that the same process that forms a star generally forms a planetary system as well, hence planets and life are widespread in the universe.

The supernova trigger hypothesis was not taken seriously until the establishment of the ^{26}Mg anomaly by Lee, Papanastassiou, and Wasserburg (1976); this was attributed to the isotope ^{26}Al, which has a half-life of only 700 000 years and thus was synthesized less than a few million years before the formation of the solar system (Cameron and Truran, 1977).

Before 1976, aside from the lack of convincing evidence, there was an alternative explanation for isotopic anomalies: they might have been produced by irradiation of planetesimals in the early solar system. That explanation was primarily associated with the names of W.A. Fowler, J. Greenstein, and F. Hoyle (the "FGH theory").

The FGH theory was linked to Fred Hoyle's (1960) theory of the origin of the solar system. Building on Alfvén's magnetic braking hypothesis, Hoyle postulated that planetesimals would be formed in a disk surrounding the sun and then pushed outward by the gas flowing from the sun as magnetic forces transferred angular momentum to the disk. The dissipation of magnetic energy in this process would accelerate protons to high speeds and they would bombard the planetesimals. Spallation reactions would produce D, Li, Be, and B. The FGH theory was primarily intended to supplement another theory of element synthesis in stars (Burbidge, Burbidge, Fowler, and Hoyle, 1957), which could not account for the abundances of the light elements D, Li, Be, and B. But it was also put forth as an alternative to the supernova trigger for producing ^{26}Al, ^{129}I, and other isotopes (Fowler, Greenstein, and Hoyle, 1961, p. 403).

The FGH theory was later abandoned (Reeves, Fowler, and Hoyle, 1970; Reeves, 1974) as an explanation for the production of light elements. As a theory of pla-

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[14] Woolfson (1988) points out that no one actually criticizes the capture theory anymore, "it is simply unread and scientifically invisible." He blames this on a "cosmogonic semiconductor"—the Atlantic Ocean—"which allows information to flow well from West to East but very poorly the other way." While it is true that most of the citations of the Dormand-Woolfson papers are by British scientists (primarily I. P. Williams and his colleagues at Queen Mary College in London), it also seems that those papers are cited mainly in connection with the gaseous protoplanet hypothesis rather than capture. During the past 20 years only four papers explicitly supported the Dormand-Woolfson *capture* theory, including one from the U.S. (Kobrick and Kaula, 1979), and one from Australia (Gingold and Monaghan, 1980; an earlier paper by those authors was somewhat tentative in its conclusions). The other two (not including self-citations from Woolfson's group) were by J. Geake and D. G. Ashworth in England.

net formation it had already come into conflict with new ideas about the early evolution of the sun proposed by C. Hayashi (1961). Hayashi argued that before a star reaches the main sequence it must go through a convective stage in which it will be highly luminous. This stage was thought by some theorists to be associated with the strong mass outflow (greatly enhanced solar wind) observed for T Tauri stars. The young star's emission of radiation and matter would destroy or sweep away the kinds of planetesimals postulated by Hoyle, although other theorists used the same process to get rid of excess nebular material *after* planets had been formed (Cameron, 1969a, p. 15; cf. Hoyle, 1963b, p. 68; Faulkner, Griffiths, and Hoyle, 1963).

A.G.W. Cameron became the most influential North American theorist after 1960. Previously an expert on nucleosynthesis in stars, he could speak authoritatively on the significance of isotopic anomalies. He could substantiate Hayashi's ideas about the early evolution of the sun with independent calculations (Ezer and Cameron, 1963). Taking full advantage of the fast but cheap computers available in the 1960s and 1970s, he developed a series of numerical models for the condensation of a solar nebula, experimenting with a range of different physical assumptions. He was one of the first to disover that the mathematical collapse of a cloud does not ordinarily lead to a large central body surrounded by smaller bodies unless special processes are postulated (Cameron, 1963d, pp. 88–93). In contrast to Alfvén, Hoyle, Mestel, and others, Cameron concluded (1966b, 1969a) that turbulent viscosity rather than magnetic braking is primarily responsible for the transfer of angular momentum from sun to planets. In 1976 he revised his models to incorporate the theory of accretion disks developed by Lynden-Bell and Pringle (1974); at the same time he concluded, contrary to his early views, that the planets were probably formed from giant gaseous protoplanets. Cameron was also one of the major proponents of the theory that the moon was formed by impact of a Mars-size planet on the Earth (Hartmann and Davis, 1975; Cameron and Ward, 1976; Boss, 1989, p. 783).

Cameron's approach stressed calculations with hydrodynamic models (even in the analysis of the impact selenogony model) and was quite compatible with the strong interest in star formation among astrophysicists. Yet in 1985 calculations of cloud collapse had not reached the point where they could be used as a firm basis for theories of solar system origin (Morfill *et al.*, 1985, p. 495).

Cameron's new model, along with the discovery of isotopic anomalies, did have a strong negative impact on another hypothesis that had been popular in the early 1970s. To explain the specific chemical and physical properties of planets and meteorites, several scientists had suggested that all of the material in the solar system (or at least in the region of the terrestrial planets) had been completely vaporized and thoroughly mixed. This assumption was justified by evidence, found in the 1930s and 1940s, that the isotopic abundance ratios of many elements are the same in terrestrial and meteoritic samples (Manian *et al.*, 1934; Valley and Anderson, 1947, and other work cited therein). It seemed reasonable to infer that the primordial nebula had a fairly uniform composition (Brown, 1950a, 1950b). Thus the solar system was "born again," preserving no evidence of its earlier history aside from its overall chemical and isotopic composition. As the homogeneous gas cooled down, its components would condense in a sequence determined by their thermodynamic properties and the pressure-density-temperature profile of the primordial nebula. With some additional assumptions about the relative rates of cooling and aggregation, and about the extent to which thermodynamic equilibrium prevails in the nebula, one could then calculate the chemical compositions of the solid bodies formed at different distances from the sun.

This "condensation sequence" model was very attractive to meteoriticists. When J.S. Lewis (1972b) used the pressure-density profile from Cameron's nebular model to deduce the densities of the terrestrial planets, it appeared that a method was also available to explain the chemistry of the entire inner solar system on the basis of a simple hypothesis about its initial state.

But the fact that meteorites with similar chemical composition varied in their isotopic ratios undermined the assumption that the primordial nebula was well mixed, and it became increasingly difficult to account for the details of their structure by simple condensation from a high-temperature gas. In the late 1970s and early 1980s meteoriticists began to favor more complex histories, including the possibility that certain components had been formed elsewhere in the galaxy and survived as interstellar grains through the formation epoch of the solar system.

Cameron's new models reinforced this view: whereas his calculations in the 1960s and early 1970s led to temperatures of thousands of degrees in the region of the terrestrial planets, now the temperatures were no more than a few hundred degrees, not high enough to vaporize the more refractory elements and compounds. At the same time the "Hayashi track," with its superluminous early sun and powerful T Tauri stellar wind, almost vanished in the face of more accurate computations (Larson, 1969). Some meteoriticists continued to report evidence for high temperatures in the solar nebula, but they could not count on astrophysics to support them—at least not until 1984, when Cameron again reversed his conclusions and suggested that small bodies might be vaporized out to the Mars region during a later stage of nebular evolution (Cameron, 1984a).

Those scientists who were more interested in learning how planets were constructed than in analyzing star formation still preferred to concentrate on the condensation of planetesimals from a gas/dust cloud and the accumulation of larger bodies from planetesimals. The fundamental problem was: how can dust particles stick together to form bodies large enough to capture more particles and gas by gravitational attraction?

In 1973 Peter Goldreich and William Ward pointed out a plausible solution to this problem. Gravitational instabilities may develop in the thin disk of dust that collects at the midplane of the nebula, even though the nebula as a whole is too rarefied and hot to be unstable. This will cause collapse to planetesimals with sizes up to the kilometer range, whether the dust particles are sticky or not. Although it was soon recognized that a similar phenomenon had been discussed earlier by several theorists (e.g., Gurevich and Lebedinskii, 1950) and had been invoked by Safronov in his theory, it had not played an important part in Anglo-American theories. After 1973, "Goldreich-Ward instability" became an essential concept in most theories of planetary formation, though it is by no means considered a definite solution to the problem of growing kilometer-sized objects from centimeter-sized objects (Boss, 1989).

In the late 1970s it was generally agreed that the terrestrial planets were formed by accretion of solid planetesimals, but this process seemed too slow to account for the outer planets; Cameron's alternative of giant gaseous protoplanets was still a viable hypothesis. The accretion calculations of Safronov and Wetherill were based on the assumption that no gas was present. Hayashi and his colleagues (1977) developed a theory in which planetesimal accretion was accelerated by gas drag. Building on this theory, Mizuno (1980) showed that, after accretion had built up a solid core of 10 or 15 Earth masses surrounded by a gaseous envelope, the envelope would collapse onto the core. Giant planets formed by this process would have the same size core regardless of their distance from the sun, in agreement with planetary models developed by Hubbard, Slattery, and others.

According to Gautier and Owen (1985, pp. 832–837), the infrared measurement of high carbon abundances in the atmospheres of Jupiter and Saturn by the *Voyager* space probe favors the Mizuno nucleation model over the gaseous protoplanet model. Gautier and Owen argue that, during the formation of a core from planetesimals, accretional heating would have vaporized methane ice and enriched the gaseous envelope in carbon, whereas in the gaseous condensation model no significant deviation from solar abundances would be expected. This conclusion has been disputed by Pollack (1985, p. 810), but if accepted it would be the first case in which planetary observations made by spacecraft (other than lunar) have clearly supported one theory of planetary formation over another.

IV. NUCLEAR COSMOCHRONOLOGY AND HOYLE'S RESEARCH PROGRAM

A. Nucleosynthesis in stars

Do not assume that all the research described in this article was originally motivated by the desire to understand the origin of the solar system. Sometimes the purpose was to solve a more fundamental problem, such as the space-time structure of the entire universe or the formation of the elements; sometimes it was to explain a specific observation or work out the applications of a new theoretical or observational technique. Only a handful of scientists would identify planetogony as their primary occupation, and even fewer are actually paid to work full time on the origin of the solar system.[15]

FIG. 1. Sir Fred Hoyle, British astronomer (astrophysics and cosmology). Photo courtesy of AIP Niels Bohr Library.

An example of the pitfalls of ascribing motivation is the work of Fred Hoyle (Fig. 1). For many years Hoyle was best known as an advocate of the steady-state "continuous creation" cosmology, which he, Hermann Bondi, and Thomas Gold had more or less independently proposed in 1948 as an alternative to the "big bang" cosmology. One consequence of his cosmological views was the need to explain how the chemical elements could have been synthesized from hydrogen in stars without invoking the high-temperature high-density environment of the big bang (called the "primeval fireball" after Hiroshima). It has been suggested that this need had something to do with the major effort undertaken by Hoyle in cooperation with William Fowler, Geoffrey Burbidge, and Margaret Burbidge, although he had started his research on nucleosynthesis in stars *before* his work on the continuous creation theory (Gribbin, 1986, p. 172). But that effort yielded an achievement whose scientific value is completely independent of the validity of the

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[15] The institutional support for planetary cosmogony in 1985 was nevertheless substantially greater than it was in 1956; see Tables III and IV.

TABLE V. Processes employed by Burbidge *et al.* (1957) to explain the cosmic abundances and nuclides.

(i) Hydrogen burning (fusion) to form helium.
(ii) Helium burning to form carbon, further alpha-particle addition to produce ^{16}O, ^{20}Ne, and perhaps ^{24}Mg.
(iii) α process: successive addition of α particles to ^{20}Ne to synthesize ^{24}Mg, ^{28}Si, ^{32}S, ^{36}A, ^{40}Ca, probably ^{44}Ca and ^{48}Ti; the source of the alpha particles is different from that in helium burning.
(iv) *e* process: "equilibrium process" at very high temperature and density in which elements comprising the iron peak (V, Cr, Mn, Fe, Co, Ni) are formed.
(v) *s* process: "slow process" of neutron capture with emission of gamma rays, on long time scale (from 100 to 100 000 years for each neutron capture); this is slow compared to the intervening beta decays. The *s* process produces most isotopes in the range $23 \leq A < 46$ except those produced mainly by the alpha process, and in the range $63 \leq A \leq 209$. This process produces the abundance peaks at $A = 90$, 138, and 208.
(vi) *r* process: neutron capture on a very short time scale, °0.01 to 10 sec, rapid compared to beta decays. This process produces many isotopes in the range $70 \leq A \leq 209$ and also U, Th; it may be responsible for ^{36}S, ^{46}Ca, ^{48}Ca, ^{47}Ti, ^{49}Ti, ^{50}Ti. It produces the abundance peaks at $A = 80$, 130, 194.
(vii) *p* process: proton capture with emission of gamma radiation, or emission of neutron following gamma-ray absorption; this is responsible for several proton-rich isotopes having low abundances compared with nearby normal and neutron-rich isotopes.
(viii) *x* process: responsible for synthesis of D, Li, Be, B. All are very unstable inside stars, so they were probably produced in regions of low density and temperature.

steady-state cosmology—just as the value of Fowler's experimental work on nuclear reactions, for which he received the Nobel Prize, is independent of the ultimate success of the theory of nucleosynthesis in stars that suggested which reactions should be investigated.

It was generally agreed by the 1950s that the initial composition of the solar system could be characterized by a single set of abundances of all elements and isotopes, and that this set could be estimated from the present abundances, making appropriate allowances for the changes in hydrogen and helium abundances resulting from processes in or due to the sun. This assumption was part of the general view that the solar system was formed in an environment not unlike that observed at present. The empirical "cosmic abundance curve" was determined from a systematic survey of available data by Hans Suess and Harold Urey in 1956 and revised numerous times by Cameron and others during the 1960s and 1970s (Suess and Urey, 1956, 1958; Cameron, 1959, 1968, 1973c; the most recent compilation is that of Anders and Grevesse, 1989).

Burbidge, Burbidge, Fowler, and Hoyle, in a review-monograph that came to be known as B^2FH, discussed the problem of explaining the cosmic abundance curve by synthesis of all nuclides from hydrogen by nuclear reactions in stars (Burbidge *et al.*, 1957; see also Cameron, 1957). They proposed eight basic processes, listed in Table V.

The theory of nucleosynthesis applied to abundance data leads to inferences about the chronology of events relevant to galactic and solar system history, hence the name "nuclear cosmochronology" given to this field. In particular, B^2FH pointed out that if the uranium isotopes were produced by the *r* process, e.g., in a supernova, then nuclear reaction theory would predict the initial ratio $^{235}U/^{238}U = 1.64$. (This is roughly the ratio of the number of progenitors of these isotopes.) Once formed, ^{235}U decays much more rapidly—its half-life is 713 million years compared to 4510 million years for ^{238}U—and at present the ratio is only 0.0072. If all of the uranium in the solar system were produced in a single supernova, a simple calculation shows that this event would have occurred 6600 million years ago.[16]

A more realistic model, proposed by B^2FH, is based on the assumption that production of uranium started at time $t_0 \times 1000$ million years ago and continued at a constant rate until $t_1 \times 1000$ million years ago. They suggested that a lower limit to t_1 could be inferred from the abundance of ^{129}Xe in meteorites. Wasserburg and Hayden (1955, p. 130) had argued that this isotope would be

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[16] If the event was $t_0 \times 1000$ million years ago, the amount of ^{235}U would have decreased by a factor $2^{-t_0/0.713}$ while the amount of ^{238}U would have decreased by a factor $2^{-t_0/4.51}$ Thus

$$2^{-t_0[1/0.713-1/4.51]}(1.64) = 0.0072 \ ,$$

$$2^{-1.18t_0} = 0.004\,39, \quad t_0 = 6.6 \ .$$

For a survey of earlier estimates see Houtermans (1947).

enriched in meteorites that crystallized before its parent ^{129}I decayed; the absence of any such anomaly in the Beardsley chondrite which they analyzed implied that the last supernova to have produced ^{129}I must have occurred more than 5000 million years ago.[17] If $t_1 \geq 5$, the model gives t_0 between 6.6 and 11.5 (Burbidge *et al.*, 1957, p. 608).

The ulterior motive for this calculation could have been Hoyle's cosmological views, since B^{2}FH note that the steady-state theory, which predicts t_0=infinity, is consistent with the data on uranium isotopes if t_1 is as great as 5.7.

Of more immediate relevance to planetary cosmogony is the mysterious "*x* process" that must exist in order to produce the observed amounts of the light elements D, Li, Be, and B. These elements are eaten up by the nuclear reactions that synthesize heavier elements from hydrogen; to survive, they must have been produced in a low-temperature, low-density, low-hydrogen environment. Possible locations are stellar atmospheres or gaseous nebulae; possible reactions are spallation of elements like C, N, O, F.

B. Hoyle on the early sun

During this time Hoyle was developing his own theory of the origin of the solar system, qualitatively outlined in a popular book on astronomy (Hoyle, 1955, Chap. 6) and presented in more detail in a 1960 paper (see his recollections in Hoyle, 1986). He suggested that the sun and planets were formed in "a whole shower of stars" from a cloud enriched by material from a supernova explosion (1955, pp. 83 and 84). A typical interstellar cloud has a rotational speed of 1 cm/sec, which would be increased to 100 km/sec if it simply contracted to form a star; there must be some process that acts to reduce this speed, presumably Alfvén's magnetic braking mechanism. Hoyle also invoked magnetic forces to explain how heavy elements (from the supernova) were separated from hydrogen (from the interstellar cloud) after first being mixed with it. Magnetic forces pushed gases away from the sun, but did not move the refractory substances that had condensed out as solids or liquids. This means that the disk of material around the sun would already have become fairly cool—not much more than 1000 K—yet his theory also required a sufficient amount of ionized gas to maintain the magnetic coupling between sun and disk needed to transfer angular momentum.

When J. Reynolds announced his discovery of anomalous ^{129}Xe in the Richardton meteorite (1960), he interpreted it to mean that 350 million years had elapsed between the time of nucleosynthesis and the formation of the meteorite. Fowler and Hoyle (1960, p. 286), using a more elaborate model of continuous nucleosynthesis over an earlier time period, concluded that the time interval after the last nucleosynthesis was only 200 million years. Reynolds noted the discrepancies between different estimates but considered them of little significance—the important point was that "the possibility that billions of years intervened between the formation of the elements of the solar system and the time its planetary bodies were formed is now conclusively ruled out" (Reynolds, 1960, p. 182; see also Kuroda, 1961; Cameron, 1962b).

But the problem of explaining the production of light elements (D, Li, Be, B) surfaced again with a new twist. Walter K. Bonsack and Jesse L. Greenstein reported (1960) that four T Tauri stars have about ten times greater lithium abundance (relative to heavy elements) than the sun. They suggested that the lithium is formed in the atmospheres of the stars by spallation; this would require a source of high-energy protons. Thomas Gold, who collaborated with Hoyle on a paper discussing the production of solar flares by the sudden release of energy stored in twisted magnetic lines of force (Gold and Hoyle, 1960), suggested that a similar process could account for the high abundance of Li in the Earth as well as in T Tauri stars (Gold, 1960, p. 275). Gold's picture of the formation of planets was qualitatively similar to Hoyle's—they come from material ejected from the sun's outer envelope and are then pushed outward as they acquire additional angular momentum from the sun by magnetic interactions.

C. The Fowler-Greenstein-Hoyle theory

Hoyle's theory of the origin of the solar system, the B^{2}FH project on the origin of the elements, and the new ideas about the formation of lithium were combined into a new theory by W.A. Fowler, J.L. Greenstein, and Fred Hoyle (1961).[18] In Hoyle's theory (Gold and Hoyle, 1960) angular momentum was transferred from sun to planets in a process that entailed dissipation of magnetic energy through a "sequence of powerful solar flares similar to those which occur on the surface of the present sun." The magnetic energy was converted into kinetic energy of high-speed particles, mostly protons, that traveled out along the lines of force to bombard the condensed preplanetary material. The bombardment produced spallation products and neutrons (Fowler *et al.*, 1961, p. 395).

In a discussion of the recent suggestions that short-

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[17] "Later it turned out that Beardsley was *unusual* in having a very small Xe^{129} anomaly" (Anders, 1989).

[18] The theory was first announced in the Richtmyer Memorial Lecture in New York City on 2 February 1961 by Fowler, who called it "Deuteronomy" since it proposed to account for the formation of deuterons.

lived isotopes such as ^{129}I, ^{107}Pd, and ^{26}Al were produced just before the formation of the solar system and incorporated into parent bodies of meteorites before they decayed (Fish *et al.*, 1960; Murthy, 1960; Reynolds, 1960), FGH doubted that these isotopes could be attributed to "galactic" nucleosynthesis. In what seems to be the first published reference to the idea of a supernova trigger for the origin of the solar system,[19] they wrote

> It can be argued that a supernova event triggered the condensation of the solar system or that the sun originated in an "association" of stars such as those young stars which seem to be moving radially outward from a common point. Discounting these interesting possibilities, it would seem reasonable to ascribe the relatively late production of the I^{129}, Pd^{107}, and Al^{26} to the synthesis in the solar nebula described in this paper. [Fowler *et al.*, 1961, p. 403].

In a longer paper published the next year, FGH pointed out a serious difficulty in their own theory. If Li and B were produced by spallation, theory predicts that roughly equal amounts of ^{6}Li and ^{7}Li would be formed, and likewise roughly equal amounts of ^{10}B and ^{11}B. But the observed abundance ratios are $^6\text{Li}/^7\text{Li}=0.08$ and $^{10}\text{B}/^{11}\text{B}=0.23$. To avoid this difficulty they proposed an auxiliary hypothesis: a large flux of thermal neutrons depleted the ^{6}Li and ^{10}B while augmenting the ^{7}Li through the reactions $^6\text{Li}(n,\alpha)\ ^3\text{H}$ and $^{10}\text{B}\ (n,\alpha)\ ^7\text{Li}$ (Fowler *et al.*, 1962, p. 150). This process would not be effective in the presence of hydrogen, since the thermal neutrons would be captured by hydrogen. Hence they must also postulate that most of the hydrogen present in the original solar nebula has already escaped. But not all of it, since they need a *few* hydrogen atoms to manufacture deuterium from the neutrons by the reaction $^1\text{H}(n,\gamma)^2\text{H}$. So "the D Li Be B abundances demand a stage for primitive terrestrial material intermediate between the original solar and the present terrestrial composition" (Fowler *et al.*, 1962, p. 180.

In describing the physical conditions in the solar nebula, FGH relied on the earlier model of stellar evolution worked out by Henyey *et al.* (1955). This gave a gradual increase in luminosity as the radius shrank in the early contracting phase of the star, following the relation $LR^{0.78}=\text{const}$ (Schwarzschild, 1958). FGH estimated that the temperature at the surface of the solar condensation did not exceed 500 K, and at 1 AU it had dropped to 170 K. Iron and oxidized silicates would already have condensed near the primeval sun; ice formed in the region from Venus to Mars and promoted the growth of planetesimals to metric dimensions. In the region of Jupiter and Saturn, solid ammonia played the role of ice but, in addition, H and He gases remained and could be accreted by gravitational capture.

In contrast to the proposal of Fish, Goles, and Anders (1960) that ^{26}Al was formed by galactic nucleosynthesis and incorporated into asteroid-sized bodies, where it provided a heat source, FGH argued that their theory did not demand such a rapid formation of large bodies (in only a few million years). Instead the ^{26}Al could be produced by "last-minute synthesis in the solar nebula," e.g., by solar proton bombardment through the reactions $^{26}\text{Mg}(p,n)^{26}\text{Al}$, $^{27}\text{Al}(p,pn)^{26}\text{Al}$, and $^{28}\text{Si}(p,2pn)^{26}\text{Al}$.

D. Objections to Hoyle's theory

Hoyle presented his theory at a conference at the Goddard Institute in New York City in January 1962 and encountered two objections. First, E. J. Öpik criticized Hoyle's assumption that hydrogen and helium could be selectively lost from the nebula simply because their molecules attained sufficient velocity to escape from the gravitational field of the sun. According to Öpik, gases might be "blown off" from the nebula, but there is no separation process that would effectively eject H and He without getting rid of other elements as long as they were all in the gas phase (Öpik, 1963, p. 75). I have not found any published reply by Hoyle to this criticism.

The second objection was based on Hayashi's theory that the sun went through a convective highly luminous phase early in its evolution (Hayashi, 1961; Hayashi *et al.*, 1962; see also Ezer and Cameron, 1963, 1965; Moss, 1968; Tayler, 1968). This would prevent water from condensing at distances of the order of 1 AU, as proposed by Hoyle to assist the growth of planetesimals. The existence at this distance of metric-size planetesimals, containing heavy elements that could be split into light elements by proton bombardment, as postulated in the FGH theory, was thus called into question.

To counter this threat to his theory, Hoyle undertook a critical examination of Hayashi's theory to see whether his conclusions "are really inescapable or not." This critique was admittedly motivated by a desire to preserve the role of an icy matrix in forming planetesimals in the FGH theory (Faulkner, Griffiths, and Hoyle, 1963, p. 2). On the basis of his own calculations Hoyle had to admit that a fully convective, highly luminous stage must occur, although the theoretical temperature and luminosity could be somewhat reduced by postulating the presence of additional free electrons produced by high-energy particles, or by inserting a magnetic field. The Hayashi effect could explain so many astrophysical facts as to make it "virtually certain that very deep convection did occur in the Sun as it approached the main sequence. . .if it were not for the necessity for the condensation of water within the elementary planetesimals, we should feel that the observational evidence distinctly favoured the completely convective models of Hayashi. But on the other hand the requirement for the condensation of water ap-

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[19] The idea of a supernova trigger for *star* formation had been discussed earlier by Öpik (1953, 1955), Hoyle and Ireland (1960), and Murthy and Urey (1962, p. 628).

pears so strong that an episode of low luminosity for the primitive Sun seems essential" (Faulkner, Griffiths, and Hoyle, 1963; p. 9).

Hoyle eventually decided to abandon the hypothesis that planetesimals were formed with the help of ice in the region of the terrestrial planets. He reformulated his theory, keeping the assumptions (1) that the contracting sun became rotationally unstable and ejected a disk of material and (2) that angular momentum was transferred to this disk by magnetic coupling, but with the new assumption that (3) these processes occurred during Hayashi's "overluminous" stage. Temperatures throughout the nebula would be much higher than in his previous theory; as a result, only metallic iron, MgO, and SiO_2 would condense in the region of terrestrial planets. Water would condense only at the distance of Uranus or Neptune. To aggregate the planetesimals without ice he had to rely on the stickiness of hot metallic iron—but this problem was dismissed in one sentence. Without primordial water, Hoyle's Earth now had to acquire water (and other volatiles) by capturing gases ejected later from the sun. The major advantage of the theory was claimed to be its explanation of "why the inner planets are largely made up of iron and magnesian silicate" (Hoyle and Wickramasinghe, 1968, p. 415). It also implied that the Earth was formed hot (>1000 K) rather than cold—not necessarily molten at first but with the possibility of large-scale convection and energy release associated with the flow of iron toward the center.

E. Decline of the Fowler-Greenstein-Hoyle theory

The FGH theory was facing other difficulties in persuading the experts to accept its explanation for the origin of light elements. Cameron (1962b) dismissed it as "unlikely" and inconsistent with chemical and isotopic data on meteorites (1963b, 1965, p. 570). Murthy and Urey (1962) preferred to believe that these elements were synthesized before the formation of the solar system. McCrea complained that it had too many adjustable parameters (1963, p. 286). Within 2 or 3 years Fowler and Hoyle were beginning to have serious doubts about their own theory. A paper by Burnett, Fowler, and Hoyle (1965) noted that meteoritic and terrestrial material had the same isotopic composition for Li, Gd, and K; if the meteorites had been formed farther from the sun, e.g., in the asteroid belt, they would have experienced a different particle flux and (according to the FGH theory) should have different isotopic compositions. [This was one of the tests suggested by McCrea (1963), and Cameron used this result as a major objection to the FGH theory (1965, pp. 570 and 571).]

By 1970 the FGH theory was dead. Experimental and theoretical research by R. Bernas, H. Reeves, and their colleagues at Orsay showed that it would be difficult to explain the observed light-element abundance ratios by spallation reactions (Bernas *et al.*, 1967, 1968; Reeves, 1968; for other evidence against the FGH theory see Murthy and Sandoval, 1965; Burnett *et al.*, 1966). Reeves, Fowler, and Hoyle (1970) proposed an alternative model for the production of light elements: bombardment of interstellar matter by cosmic rays. In this model D, He, Li, Be, and B would already be present in the solar nebula and their origin would have nothing to do with the origin of the solar system. This conclusion seemed to be generally accepted by the time of the Nice conference in 1972 (Reeves, 1974, pp. 51 and 52).

Although the FGH theory was abandoned, the idea of accounting for anomalous abundances of certain elements or isotopes by irradiation in the early solar system is occasionally revived on an *ad hoc* basis (D. Clayton *et al.*, 1977, p. 300; Podosek, 1978).

F. Decline and transmutation of Hoyle's theory

The demise of the FGH theory did not force Hoyle to abandon his broader theory of the origin of the solar system, but for several years he published nothing on the subject except for a brief note on volcanism, and his theory gradually went out of favor.[20] He continued to worry about the source of volatile substances in the terrestrial planets, which should consist only of refractory materials according to his theory. In 1972 he proposed that the volatiles were ejected as ices from Uranus and Neptune and eventually found their way to the Earth and other terrestrial planets (Hoyle, 1972, pp. 333 and 334). His 1979 book *The Cosmogony of the Solar System* elaborated this idea and connected it with the hypothesis that life originated on these particles, now identified as comets.

Dallaporta and Secco (1975) worked out Hoyle's theory quantitatively; they concluded that the magnetic field of the protosun was too weak for his basic mechanism to be effective. Nevertheless there seems to be some evidence from meteorite studies for a strong magnetic field in the early solar system (Banerjee and Hargraves, 1971, 1972; Brecher, 1971, 1973; Brecher and Arrhenius, 1974; Lanoix *et al.*, 1978; Sonett, 1978; Nagata, 1979; Sugiura *et al.*, 1979). There also are indications of such a field in the lunar crust (Sonett *et al.*, 1975; Banerjee and Mellema, 1976) and around young (T Tauri-type) stars (Gershberg, 1982; Appenzeller and Dearborn, 1984; Gnedin and Red'kina, 1984; Lago, 1984; Bouvier and

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[20] After the first *Apollo* moon landing Hoyle suggested that igneous lunar and terrestrial rocks could have been formed in the hot phase of the solar nebula rather than by volcanism after the formation of those bodies (Hoyle, 1969, p. 401).

Bertout, 1986; Gnedin *et al.*, 1986; Uchida, 1986).[21]

Although Hoyle's theory was frequently mentioned as a leading contender in the 1960s (Wood, 1962; Cameron, 1963b, p. 23; McMahon, 1965, p. 228; Herczeg, 1968, pp. 187 and 188; Woolfson, 1969, p. 157; Dormand, 1973; Dorschner, 1974), it had dropped out of sight by 1985. Specific criticisms of his theory were occasionally published: a solar nebula whose mass was only $0.01M_\odot$ would not provide enough material in the region of the outer planets for them to grow to their present sizes during the lifetime of the solar system (Safronov in Hoyle, 1971, p. 202); his ideas about how a cloud would collapse to form a solar nebula are not in agreement with modern ideas on this process (Cameron in Hoyle, 1971, p. 200; Whipple, 1979, p. 819); his mechanism for pushing rocky material away from the sun will not work (Whipple, 1964, 1979, p. 819); his mechanism for pulling volatiles back to the terrestrial planets is "hard to believe" (Page, 1980, p. 325); and his account of the action of magnetic fields is faulty (Okamoto, 1969, p. 48; Woolfson, 1971, p. 268; Alfvén and Arrhenius, 1976, p. 259). Urey judged Hoyle's entire theory to be "quite artificial" (1963, p. 154). But these objections are not much stronger than those made against other theories, especially in view of the acknowledged fact that Hoyle's overall picture is in several respects qualitatively similar to generally accepted ideas (Whipple, 1979, p. 819; Friedlander, 1985, p. 293).

I attribute the failure of Hoyle's theory to retain its popularity to the fact that it does not look like a "progressive research program" in the sense defined by Lakatos. Being associated with the ill-fated FGH theory was certainly no help, though that need not have been a fatal defect. But, as noted, Hoyle changed one of his original auxiliary hypotheses in order to conform to the Hayashi theory of the early superluminous sun, after having previously stated that his explanation of the aggregation of planetesimals was in conflict with Hayashi's theory. This move would be called a "degenerating problemshift" in the Lakatos scheme unless Hoyle had succeeded in using his modified theory to make a successful new prediction. But in fact Hoyle never claimed to make any predictions at all, and he made no effort to use new data from the space program or from meteorite analysis to develop his theory. Nor did he take advantage of the later demise of Hayashi's superluminous sun to revive his original theory. In his 1979 book he did not refer to any recent observations except for the study of lunar samples, and even there he was not able to come to any definite conclusion about the origin of the moon. The reader of that book might well conclude that most of it could have been written 30 years earlier. The failure of Hoyle's program seems to find a reasonable explanation in terms of the Lakatos methodology, or indeed under most orthodox accounts of scientific method.

V. CAMERON'S PROGRAM

A. Collapsing clouds

During the 1960s the two most popular theories (at least in the English-speaking world) were those of Hoyle and A.G.W. Cameron. Cameron (Fig. 2), like Hoyle, was much occupied in the late 1950s with the problem of element synthesis in stars. A nuclear astrophysicist who had been working at Atomic Energy of Canada Ltd. (Ontario), Cameron was visiting Caltech in 1960 when Reynolds published his results on xenon isotopic anomalies. This discovery, suggesting that ^{129}I had been present in some meteorites at the time of their formation, "transformed the direction of much of my research,"

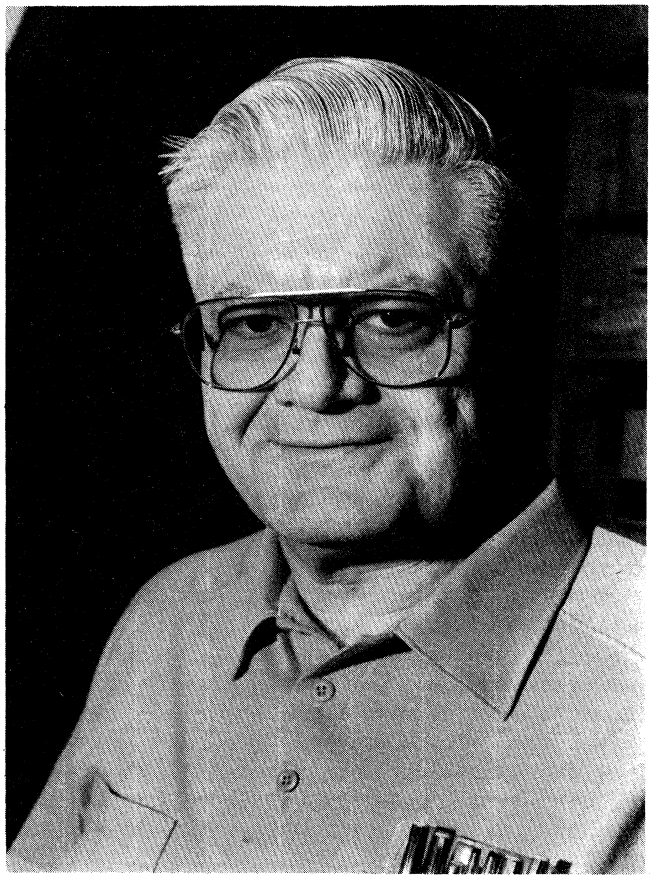

FIG. 2. A. G. W. Cameron, Canadian-American astrophysicist (nucleosynthesis, formation of stars and planetary systems, origin of the moon).

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[21] More recent papers on this subject can be retrieved from *Astronomy and Astrophysics Abstracts*; see index under "Magnetic Fields—T Tauri Stars."

Cameron recalled in 1985. "I was already interested in the meteorites as an interesting problem in connection with the abundance of the elements, and now it was clear that there were clues in the meteorites about the time scale on which they had been formed. Therefore I spent the rest of my time at Caltech thinking about problems of star formation and the origin of the solar system and their related time scales. . . . Eventually all of this thinking was published in one lengthy paper" (Cameron, 1986).

In December 1960 Cameron sent Harold Urey "the first draft of a paper I have written on the formation of the sun and planets. This work represents a kind of vacation from nucleogenesis that I have been taking since last spring. . . . In the course of this cosmogony I do not find any place for your primary, secondary, and tertiary lunar-size objects in the asteroid belt" (Cameron, 1960).

Cameron also rejected Kuiper's gaseous protoplanets on the grounds that they would be subject to thermal disruption by the Jeans criterion unless they were much larger than the effective thickness of the nebula (Cameron, 1962c, p. 41; 1973, p. 385).

These views on the formation of planets were not firmly held and proved to be subject to modification on short notice. The hard core of Cameron's program was his assumption that the early stage of collapse of the solar nebula must be consistent with a plausible model for formation of a solar-mass star from the interstellar medium under presently observable conditions, and with the time scale determined from isotopic anomalies interpreted on the basis of a reasonable theory of nucleogenesis (Cameron, 1962b; 1962c).

Although Cameron started with the same kind of assumptions as Hoyle—they agreed on what phenomena had to be explained and what type of explanation would be acceptable—their theories were quite different. Cameron started with a very massive solar nebula derived from a cloud compressed by external pressure, and with angular momentum corresponding to its rotation around the center of the galaxy, whereas Hoyle postulated only enough mass to restore solar composition to the planets and enough angular momentum to make the early sun rotationally unstable (Cameron, 1963b, pp. 23–25).

Cameron could criticize Hoyle for failing to explain how the extra angular momentum that the solar nebula inherited from its galactic rotation was lost (Cameron, 1963b, p. 23). In an earlier decade Hoyle could have defended his minimum-mass, minimum-angular-momentum nebula as a naturalistic assumption based on the well-known present state of the system as opposed to deduction from a hypothetical initial state. But Cameron also claimed the benefits of naturalism because his initial state was consistent with known conditions in the present galaxy. Cameron had enlarged the domain of the problem from the solar system to the galaxy, though it remained to be seen whether he could cover planetary formation, which had been central to the original domain.

Cameron attributed isotopic anomalies to "a period of nucleosynthesis in the galaxy which enriches the interstellar medium with fresh radioactivities a relatively short time before the formation of the solar system" (Cameron, 1963d), in place of the FGH hypothesis that these radioactivities were produced by spallation in the solar nebula.[22]

Cameron agreed with Hayashi that the early sun went through a convective, highly luminous phase (Cameron, 1962b; Ezer and Cameron, 1962, 1963); as noted above, Hoyle resisted this idea for several years, since it conflicted with the hypothesis that planetesimals could be formed with ice in the inner solar system. Cameron proposed that the planets were formed in the primitive solar nebula *before* the sun (1965, p. 572; 1966a, p. 238; 1974, p. 68); residual gas later streamed inward to form the sun.[23] Nascent planets were then exposed to a high-temperature environment when the sun went through its Hayashi phase.

Another difference between Cameron's and Hoyle's basic assumptions is that Cameron did not accept the Alfvén hypothesis, adopted by Hoyle, that transfer of angular momentum from the sun to the solar nebula took place primarily by magnetic braking in an ionized gas. Instead, Cameron attributed this transfer to turbulent viscosity (1962b;1969a,1969b).

As ter Haar and Cameron pointed out in their historical review (1963, pp. 34 and 35), *none* of the existing theories is satisfactory, because they lack a quantitative basis; their authors suggest processes that might lead to our solar system but fail to demonstrate by rigorous calculation that they would actually do so. Cameron attempted to satisfy this demand by deducing the quantita-

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[22] Cameron (1963d) accepted Kuroda's (1961) suggestion that spontaneous fission of the extinct radionuclide ^{244}Pu accounts for the Xe isotope abundances in the Earth's atmosphere. Pepin concluded that this hypothesis could account for the observed anomalies at least as well as the FGH hypothesis (Pepin, 1964, p. 209).

[23] Cameron suggested that the planets could have acquired their atmospheres in this way but immediately pointed out that this suggestion seemed to be refuted by the xenon isotopic abundances, which indicated that the atmosphere was acquired gradually from the sun throughout terrestrial history. He regarded this as an objection to his theory of the formation of the solar system (Cameron, 1965, p. 580). To avoid the objection he added yet another auxiliary hypothesis: the Earth lost its primordial atmosphere when it became rotationally unstable and spun off the moon. But this process (G.H. Darwin's selenogony recently revived by D.U. Wise) would work only if the Earth were already greatly deformed and rotating close to the limit of stability; that might ~~be so if it had been~~ formed from two bodies of comparable size in the primitive solar nebula (Cameron 1965, p. 582). This idea reappeared with some modifications in the Cameron-Ward (1976) selenogony.

tive consequences of a series of models for the collapse of a gas cloud, using computers to solve the hydrodynamic equations. The problem seemed similar to the collapse of larger gaseous spheres to form galaxies, discussed by Leon Mestel (1962), who found that two kinds of disks can be formed by rotational instability: one with nearly uniform mass distribution, the other "axially condensed." On the scale of individual stars, the uniform disk presumably would eventually form a binary or multiple star system whereas the axially condensed one might form a single star surrounded by planets (Cameron 1962a, p. 341; 1963b, pp. 26 and 27).

But the actual calculations showed that no central stellar body was formed in hydrostatic equilibrium with the disk (Cameron, 1963d; 1965, p. 571). Instead, ringlike condensations were formed; Cameron conjectured that these might break up into separate disks that could be precursors of the giant planets (1969a). The "Sun must be formed as a result of gaseous dissipation processes" (Cameron, 1974, p. 58). Other theorists also found that the collapse of a rotating cloud gave a ring with no central condensation, and concluded that the process would result in a binary or multiple star system (Larson, 1972a, 1974; Black and Bodenheimer, 1976; see Bodenheimer and Black, 1978, p. 288, for a brief history of these calculations).

B. The viscous accretion disk

Another result of the computer calculations was the discovery that dissipative processes such as angular momentum transfer took place much more rapidly—only a few thousand years—than previously assumed (Cameron and Pine, 1973). "Thus, the principal goal of the research shifted to the calculation of sequences of models of the primitive solar nebula in which dissipation occurred during the formation. This goal was achieved during the winter 1975-76. Contributing in a very material way to the achievement of this goal was the development of the theory of the viscous accretion disk" (Cameron, 1978c, p. 55).

The concept of a viscous accretion disk, on which Cameron now based his theory, had been developed by D. Lynden-Bell and J.E. Pringle (1974)[24] to account for the high and rapidly varying radiation from T Tauri stars. They concluded that "whatever the dissipation mechanism, the basic form of evolution is the expansion of the outermost parts to carry all the angular momentum together with the collection of an ever increasing fraction of the mass towards the center. This process is much slower in systems of larger scale and this fact encourages us to see analogies between the present state of the Galaxy and a very much earlier stage of the solar system that was a spinning disc of gas and dust" (Lynden-Bell and Pringle, 1974, p. 604).

In the following decade accretion disks were invoked to explain a number of exotic astronomical phenomena such as quasars, cataclysmic variables, and powerful x-ray sources in binary systems (Lin and Papaloizu, 1985, p. 985). The existence of a well-developed mathematical theory for this model seems to compensate for the failure to show that it evolves from a specific previous state. Thus the domain of solar system cosmogony has shrunk somewhat from the ambitious galactic scale previously envisioned. Accretion disk theory also covers up ignorance of physical processes by lumping the effects of several unspecified sources of turbulence into a single parameter, thus sacrificing or postponing a more fundamental understanding of how the solar nebula works (Cabot *et al.*, 1987, p. 451).

Cameron also inferred from his new model that the solar nebula would be unstable against the formation of rings that might condense to giant gaseous protoplanets in the outer solar system (1978b, p. 5; Cameron, 1978c, p. 64; DeCampli and Cameron, 1979). Cameron continued to defend this mode of formation for the outer planets despite criticism and arguments for the alternative planetesimal model (Lin and Papaloizou, 1980; Mizuno, 1980; Gautier and Owen, 1985; Podolak and Reynolds, 1985). He also claimed that the terrestrial planets could be formed from giant gaseous protoplanets; after a core of refractory material condensed at the center of the protoplanet, the gaseous envelope would be evaporated as the temperature rose in the inner regions of the dissipating solar nebula (Cameron *et al.*, 1982; Cameron, 1985a, p. 1096).

The accretion disk model led Cameron to estimate substantially lower maximum temperatures for the inner part of the solar system than in his earlier models. This estimate helped to undermine the condensation-sequence models, which had relied on the assumption that all material in the region of terrestrial planets had once been completely vaporized (see Sec. IX.C).

But in 1983, calculations on star formation adapted to data for T Tauri stars again led to a high-luminosity phase of the early sun (Mercer-Smith, Cameron, and Epstein, 1984). Temperatures in the nebula during later stages of accretion would "exceed the condensation temperature of iron to surprisingly large radii. . . small bodies will be totally evaporated to a distance beyond that of the formation of Mars. Bodies of planetary size, such as remnant cores of condensed matter left over from the evaporation of giant gaseous protoplanets, may survive

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[24] Historical note by Cameron (1978c, p. 55); "The basic elements of such a theory were published many years ago by Lüst (1952), but this important pioneering paper did not become well-known. The theory was twice again independently worked out, by Lynden-Bell and Pringle, but these authors discovered each other's work and recently published a joint paper outlining the theory." According to Lynden-Bell and Pringle (1974, p. 604), the work originated in the former's 1960 thesis. "We worked out the basic similarity solutions for the evolution of time-dependent Newtonian discs under the action of viscosity but failed to find any solutions that evolved under their own self-gravity. However, in many of the more recent applications the self-gravity is negligible, so these solutions are now of greater interest."

this period" (Cameron, 1984a, p. 119). In particular, the temperature at the distance of Mercury could be in the range 2500° to 3500°K; Cameron therefore proposed that Mercury was first formed as a much more massive planet and then lost most of its rocky mantle by vaporization, leaving behind a high-density core (Cameron, 1984a, 1985c).

Cameron's frequent reversals on such matters as the temperature of the solar nebula, the supernova trigger (Secs. VI.A–VI.C), and the origin of the moon (Brush, 1988) have sometimes puzzled other scientists. In his autobiographical notes he acknowledges that "many of my friends are never sure what my current thoughts on a subject are" since he is "ready at a moment's notice to abandon a favorite hypothesis when presented with a good reason." The "good reason" may be a new calculation as often as a new observation. Moreover, Cameron reserves the right to reinterpret evidence in a way consistent with his own views (Cameron, 1986).

VI. ISOTOPIC ANOMALIES AND THE SUPERNOVA TRIGGER

A. Magnesium, Xenon, Oxygen

In the meantime Cameron had revived the theory briefly alluded to before: the "supernova trigger." Supernova explosions had been invoked as a cause of star formation since Öpik's 1953 suggestion, and the idea that isotopic anomalies in the solar system could be explained by injection of nuclei recently synthesized in such an explosion—an explosion which also caused the collapse of the presolar nebula—had been discussed by Cameron and others in the 1960s. But these ideas remained speculative and had to compete with other speculations such as that by Fowler, Greenstein, and Hoyle (1961, 1962), which postulated the formation of light nuclei in the early solar system by spallation. The supernova might also be replaced, as a source of compression to initiate collapse of the presolar nebula, by the density waves thought to account for the spiral arm structure of galaxies (Lin and Shu, 1964; Lin, 1971, p. 97; Wetherill, 1975b, p. 298).

Three events in 1969 led to a revival of the supernova trigger theory in the 1970s. First was the abandonment of the Fowler-Greenstein-Hoyle theory on the grounds that the most probable mechanism for the production of light elements was the bombardment of interstellar matter by galactic cosmic rays (Reeves, Fowler, and Hoyle, 1970). Second was the fall of the huge Allende meteorite in Mexico, making material for the study of isotopic anomalies suddenly much more easily available (Begemann, 1980; Grossman, 1980, 1981; for a 50-year review see Pillinger, 1984). Third was the *Apollo* lunar landing project, which involved the development of extremely sensitive instruments for analyzing the isotopic composition of samples and stimulated the interest of the scientific community in "hands-on" solar system research.

Of the many isotopic anomalies, the most intriguing was the possible excess of ^{26}Mg, considered as the decay product of ^{26}Al. Simanton and his colleagues had discovered in 1954 that the latter nuclide had a previously unknown ground state (below the known 6-second positron-emitting state), which decayed by positron emission to ^{26}Mg with a half-life less than a million years (later found to be about 720 000 years). Harold Urey (1955) proposed that ^{26}Al in the early solar system could have been a source of heat to melt meteorites but then rejected this mechanism because it would have melted the moon. It was revived and developed by Fish, Gole, and Anders (1960); see also Murthy and Urey (1962).[25] Because of its short life the ^{26}Al much have been produced fairly recently, perhaps by proton irradiation of magnesium in the reaction $^{26}\mathrm{Mg}(p,n)^{26}\mathrm{Al}$ (Fowler *et al.*, 1962, p. 192; Reeves and Audouze, 1968). Everyone assumed, with Cameron (1962b), that the time interval between initial collapse of the presolar nebula and formation of meteorites must have been much more than a million years.

In 1970, W.B. Clarke and his colleagues reported a 4–6 % excess of ^{26}Mg in the meteorites Bruderheim and Khor Temiki. But Schramm, Tera, and Wasserburg (1970) could find no anomalies in several samples, including the ones analyzed by Clarke's group.[26]

Schramm (1971) stated that there is no evidence for the presence of ^{26}Al at the time of final solidification of the meteorites, although it could have been a significant heat source before solidification. He discussed the possible synthesis of ^{26}Al by silicon or carbon burning in supernovae but remarked that "time scales in the early solar system make it more likely that ^{26}Al, if present in planets,

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[25] Anders (1989): "Urey had completely disowned this idea by 1956, and strongly resented our attempts to revive it. Our paper submitted to *Astrophysical Journal* in 1959 was rejected, and an eminent astrophysicist wrote: 'The time scale necessary for the operation of short-lived radioactivities ($\leq 10^8$ years) is just impossible. . . . The current trend in astronomical ages makes your time-scale ridiculous.' Our revised paper met with a friendlier reception when resubmitted in 1960, presumably because Reynolds had meanwhile found evidence for extinct 16-Myr I^{129}."

[26] In order to distinguish between isotopic anomalies due to material coming from distinct nucleosynthetic processes and anomalies due to later fractionation processes in the solar system, one has to look for effects that are nonlinear in the mass-number differences. Thus a fractionation process that tends to separate an isotope of mass number i from one of mass number j will generally produce an effect proportional to $(i-j)$. The abundance ratio of the two isotopes would be

$$R_{ij}=R_{ij}{}^0[1+k(i-j)] ,$$

where $R_{ij}{}^0$ is the normal ratio and k is the fractionation factor.

was synthesized by proton irradiation in the early solar system."

Two Australian scientists, C.M. Gray and W. Compston, reported finding excess ^{26}Mg in the Allende meteorite in 1974. In agreement with Schramm (1971) they concluded that the parent ^{26}Al was made within the solar system by proton bombardment of light elements. But their results were regarded as inconclusive by American scientists (e.g., Lee *et al.*, 1976), as were the preliminary results of Lee and Papanastassiou (1974).[27]

Of the numerous other isotopic anomalies discovered and discussed in the 1970s, the most important were those in the noble gases and oxygen. The xenon anomalies had become more complicated since Reynolds' work in the early 1960s. In 1969, three papers independently proposed that a "strange" xenon component, discovered by Reynolds and Turner (1964), came from fission of a superheavy element with atomic number about 114 (Anders and Heymann, 1969; Dakowski, 1969; Srinivasan *et al.*, 1969). The Anders group at Chicago continued to support this hypothesis for more than a decade against proposed supernova and other explanations, finally abandoning it in 1983 (R.S. Lewis *et al.*, 1983).

In 1972 Manuel, Hennecke, and Sabu reported that carbonaceous chondrites contain two isotopically distinct components of trapped xenon that cannot be explained by nuclear or fractionation processes, and suggested that isotopes 131 through 136 might have been produced by a high flux of thermal neutrons on ^{235}U. This flux could be due to an early deuterium-burning stage in the outer region of the sun or to the irradiation of planetary material before accretion as proposed by Fowler, Greenstein, and Hoyle.[28] At the same time David Black (1972) found in the carbonaceous chondrite Ivuna a component of neon which he called "E" and suggested an extra-solar-system origin for it.[29]

In 1973 Robert Clayton, Lawrence Grossman, and Toshiko Mayeda found that the oxygen in certain minerals in carbonaceous chondrites is depleted[30] in isotopes 17 and 18. They attributed this to admixture of a component of almost pure ^{16}O, which "may predate the solar system and may represent interstellar dust with a separate history of nucleosynthesis." Since ^{16}O is produced by alpha-process reactions in stars, one might expect that ^{16}O-rich samples would also have an excess of isotopes with other integral numbers of α particles such as ^{24}Mg and ^{28}Si (compared to other stable isotopes of those elements).

Donald Clayton (1975b, p. 768) argued that the ^{16}O anomaly found by Robert Clayton *et al.* (1973) proves that it is possible to form grains containing material produced by nucleosynthesis before it is diluted by interstellar matter, and proposed that the xenon anomalies were also due to grains formed near exploding stars.[31] Salpeter (1974) had argued that a supernova would be surrounded by a cold dense gaseous shell where grains could form. During the next few years Donald Clayton elaborated the hypothesis of "presolar grains" as an alternative to the conventional assumption that meteorites condensed only after the formation of the solar system and thus reflected the isotopic composition prevailing at that time (see Sec. IX.C).

The first generally accepted proof of the presence of ^{26}Al in the early solar system came late in 1975, when Wasserburg's group at Caltech announced their discovery of a large anomaly in the isotopic composition of magnesium in a chondrule from the Allende meteorite. The ^{26}Mg excess was nonlinear, so it could not be attributed to fractionation effects.[26] According to the report by Lee, Papanastassiou, and Wasserburg (1976), ^{26}Mg is enriched by about 1.3%, and "there is a strong correlation *in this chondrule* between the ^{26}Mg excess and the Al/Mg ratio so that the most plausible cause of the anomaly is the *in situ* decay of now extinct ^{26}Al" (Lee *et al.*, 1976, p. 109).[32]

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[27] The view that Gray and Compston's work was inconclusive "is uncharitable and reflects the chauvinism of the US scientists" who expressed that opinion, according to Ringwood (1988). Ringwood states that "The quality and reliability of Compston's mass-spectrometry is internationally accepted. There is no doubt that they resolved and measured a real effect and recognized it for what it was. They should therefore receive unqualified credit as discoverers of the ^{26}Mg excess."

[28] They do *not* mention a supernova here, though they later were credited with this idea by Cameron and Truran (1977, p. 447).

[29] Schramm (1978, p. 386) says this is the first anomaly for which the most reasonable explanation seemed to be to postulate primitive material of different isotopic composition. Donald Clayton (1979a, p. 162) said that Black's suggestion about the origin of the "Ne-E" component was "a far-reaching conclusion, perhaps the first of its kind based on good data soundly analyzed rather than on pure speculation. Nevertheless, the argument had little impact on astrophysics or on solar system science at the time. It was really after the ^{16}O anomaly that Black's discovery was well remembered, and is now regarded as being of fundamental importance."

[30] Schramm (1978, p. 387) says this work "initiated much of the present activity" concerning isotopic anomalies.

[31] "It is, I think, one of my most creative ideas" though strongly resisted by other scientists (D. Clayton, 1975a, p. 64). The idea that interstellar grains had survived the formation of the solar system and might be present in meteorites had also been suggested by Anders (1964) and Cameron (1973c). The resistance of the community is indicated not only by Clayton's own testimony but by the delay in publication of his papers, e.g., a paper submitted in April 1979 but not published until December 1981 after arguments with "four epochs of anonymous referees" (D. Clayton, 1981a, pp. 374 and 386).

[32] The result had been announced at the winter meeting of the American Physical Society in Pasadena, 29 December 1975 (Lee *et al.*, 1975); their paper, submitted to *Geophysical Research Letters* in October 1975, appeared in the January 1976 issue of that journal but its first two paragraphs were missing so it had to be republished in the next issue.

Previously, isotopic anomalies had been attributed to production of ^{26}Al by irradiation in the early solar system, but the Caltech group doubted this explanation on the grounds that such irradiation would have produced other anomalies that are *not* observed, e.g., ^{53}Cr from decay of ^{53}Mn (Lee *et al.*, 1976, p. 112). Another paper left open this possibility, however, stating that ^{26}Al could be attributed either to the "injection of freshly synthesized nucleosynthetic material into the solar system immediately before condensation and planet formation, or local production within the solar system by intense activity of the early Sun" (Lee *et al.*, 1977, p. L107; see also Lee, 1978, p. 226).

Clayton, Dwek, and Woosley (1977) criticized the irradiation model in more detail, showing that a proton fluence large enough to produce the inferred quantity of ^{26}Al would have created anomalies in ^{36}Ar, ^{80}Kr, and other isotopes. Failure to observe those anomalies, together with discoveries of other anomalies that could not be produced by irradiation, such as ^{16}O and ^{202}Hg, weakened the credibility of the irradiation model.[33]

B. Revival of the supernova trigger

Several scientists at the Spring 1976 meeting of the American Geophysical Union discussed the possibility that a supernova explosion shortly before the formation of the solar system could be responsible for the recently discovered isotopic anomalies (Cameron and Truran, 1977, p. 447). The first published discussion of this possibility based on what is now considered reliable evidence was that of Lewis, Srinivasan, and Anders (1975), in a paper on Xe isotopes in the Allende meteorite, but these authors concluded that a supernova origin for the anomalies was unlikely. Soon afterward Sabu and Manuel (1976) proposed that the data of Lewis *et al.* (1975) indicated that a supernova did explode in the vicinity of the present solar system. Unlike other theories, the Sabu-Manuel hypothesis assumed that the sun already existed before the explosion and had formed a binary system with the star which was to explode. But their interpretation of the Xe data has generally been considered unacceptable by other scientists (Anders, 1989, private communication).

In July 1976 Cameron and Truran submitted to *Icarus* their paper "The Supernova Trigger for the Formation of the Solar System." The basic idea was the same as had been proposed by Cameron 16 years earlier: "the supernova responsible for injecting short-lived radioactivities into nearby interstellar clouds may also have been responsible for triggering the collapse of those clouds to form stars and accompanying planetary systems" (Cameron and Truran, 1977, p. 448). But now there was much better evidence for those short-lived radioactivities, and the best alternative explanation (that they had been produced by irradiation in the early solar system) had been discredited. The hypothesis that supernova explosions can form new stars was gaining increased support from astronomical observations; shortly after Cameron and Truran began to circulate their paper, William Herbst and George Assousa (1977) reported new observations of supernova-induced star formation in Canis Major.

The supernova trigger theory quickly became enormously popular, receiving wide publicity in both the technical journals[34] and the press (Edmunds, 1977; Spruch, 1977; Sullivan, 1977a, 1977b; Falk and Schramm, 1979; O'Toole, 1979). The Cameron-Truran hypothesis was attractive because it promised to explain many diverse phenomena by a single event. But it promised more than it could deliver, as Cameron himself soon realized. In a report on the 8th Lunar Science Conference in March 1977, Cameron was quoted as saying his work was "trying to make a synthesis of a lot of different ideas into a single picture, and perhaps it was too ambitious an attempt." He thought perhaps he and Truran had gone too far in attempting to explain the heavy-element anomalies, though their description of light elements was satisfactory (Spruch, 1977, p. 19). Three years later Cameron retreated further by admitting that a class of anomalies known as "FUN" (for Fractionated Unknown Nuclear) could be better explained by changes in the proportions of products from different nucleosynthetic processes, as Donald Clayton had long argued, than by postulating a single nearby synthesis site (Consolmagno and Cameron, 1980).

At the same conference in 1977, Wasserburg told a reporter that "the discovery of new isotopic effects, which are related to nuclear, chemical, and kinetic effects, is taking place very nearly on a weekly basis. Therefore, anyone trying to play God is in a crap game with very

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[33] This argument was reinforced by Schramm (1977, 1978) and Reeves (1978). Typhoon Lee (1978) showed that an irradiation model could explain the ^{16}O and ^{16}Al anomalies qualitatively and is consistent with the nearly normal Mg, Ca, and Ba observed in most samples; hence "we may not need a supernova to explode immediately before the formation of the solar system" (Lee, 1978, p. 226). However, he admitted that the model could not explain the large ^{48}Ca excess recently discovered by Lee, Papanastassiou, and Wasserburg (1978) and other anomalies in heavy elements that seemed to require large neutron fluxes, which would be hard to achieve in the early solar system. Thus "at least some isotopic anomalies are to be explained by an extra-solar source" (Lee, 1979, p. 1605).

[34] *Science Citation Index* (augmented by scanning a few of the publications mentioned in footnote 9) lists the following numbers of citations for the years 1977–1986:

1977	1978	1979	1980	1981	1982	1983	1984	1985	1986
11	28	27	24	18	6	12	10	10	7

rapidly changing rules (Spruch, 1977, p. 19). Nevertheless, Wasserburg's group found independent support for the recent injection of nucleosynthetic material when they discovered unexpectedly large amounts of ^{107}Ag, presumably produced by decay of ^{107}Pd, in the Santa Clara meteorite (Kelly and Wasserburg, 1978). At the end of 1978 Wasserburg was quoted by a Washington *Post* reporter as saying "something went off with a helluva bang just before the solar system was born" (O'Toole, 1979).[35]

While the Cameron-Truran trigger theory was clearly the most widely accepted in the late 1970s, several other theories invoked supernovae in somewhat different ways to explain the formation of the solar system:

(1) Wilbur Brown proposed a model for formation of the solar system from massive fragments of the shell ejected by a supernova (1970, 1971a, 1971b, 1974). Herbst and Assousa (1978, p. 369) called this the first suggestion that a supernova was involved in the origin of the solar system, though Brown himself cited earlier papers by Cameron (1962b) and Hoyle (1945). A modified version of this model was presented under the name "snow plow model" by Herbst and Rajan (1980), on the basis of work by Roger Chevalier (1974; Chevalier and Theys 1975) showing that the expanding shell of gas from a supernova may be compressed to a high enough density to initiate star formation. The advantage of this model is "that there is no need for supernova ejecta to penetrate and mix with a preexisting cloud" (Herbst and Rajan 1980, p. 42), a difficulty with the trigger model raised by the calculations of Steven Margolis (1979). Wark (1979) also used this model to discuss the condensation of the presolar nebula.

(2) Donald Clayton's model suggested that presolar grains could be formed as condensations in supernova ejecta ("SUNOCONS"), but since the grains were solidified before the formation of the solar system, their isotopic anomalies would not indicate a single recent nucleosynthetic event but could come from several earlier supernova (D. Clayton 1977a, 1977b, 1978a, 1981a, 1982, 1986).

(3) Hubert Reeves proposed a "bing bang" model in which "the sun was most likely born amidst a fireworks of supernovae" (1978, p. 400). Like Donald Clayton, Reeves proposed that ejecta from many supernovae had been incorporated into our solar system even though no single supernova played the role of "trigger." His emphasis was more on the astronomical side, whereas Clayton's was more on the chemical; Reeves proposed that the sun was born in an OB association such as Orion, where a large molecular cloud was compressed by passage through a galactic spiral arm and stars of various masses begin to form. The heaviest stars evolved gradually to the supernova stage and the remnants of their explosions swept across the region where other stars like the sun were still forming.

(4) In the flypaper model, proposed by Thomas Gold, an already collapsing cloud caught the ejecta from a supernova. Gold has not published the details of this model [there is only a brief reference to it by Donald Clayton (1977a, p. 267)], and it is not clear how much it differs from the others.

(5) Perhaps the most bizarre idea is the proposal of Manuel and Sabu (1975) that the supernova was actually concentric with our sun, which formed on its remnant core, while the planets condensed from the debris of outer layers (see also Manuel, 1981). One consequence of this model is that the sun's interior should contain a significant amount of iron (Manuel and Hwaung, 1983), a conclusion reached for completely different reasons by Carl Rouse (1983), though it contradicts the view generally accepted since 1929 (Russell, 1929) that the sun is mostly hydrogen and helium.

It might appear that acceptance of any but the last of these hypotheses would imply rejection of the monistic principle (Sec. III.A) in favor of a model requiring at least one other star to assist the formation of planets orbiting our sun. In the classical dualistic theories, that would entail a very low probability for planetary systems elsewhere in the galaxy and thus an even lower probability for the existence of intelligent extraterrestrial life. But the new theories are not so pessimistic: a supernova can trigger the collapse of many clouds without having to be very close to any one of them, so the older estimates of the chances of stellar encounters do not apply. More generally, the clouds that collapse to form stars and planets are strongly affected by the presence of nearby clouds and other stars, so the monistic/dualistic dichotomy may simply be obsolete.

C. Rejection of the trigger hypothesis

Up to 1984 it was widely believed that one or more supernova explosions were directly involved in the formation of the solar system, although there were several competing models based on this idea (see end of Sec. VI.B). A crucial assumption of most of these models was

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[35] In an article in *Sky and Telescope* (July 1979), Sydney Falk and David Schramm mentioned the discovery of excess ^{107}Ag and concluded "whether or not a supernova directly caused the collapse of the solar nebula, the injection of supernova grains from a nearby event over 4.5 billion years ago seems to be the only explanation for the existence of the short-lived species aluminum-26 and palladium-107 at the time the solar system was formed" (Falk and Schramm, 1979, p. 22). Kelly and Wasserburg, in a letter to the editor of *Sky and Telescope*, pointed out that the Santa Clara meteorite probably contained material that had accreted, melted, differentiated metal from silicate, and then solidified again, whereas the Allende meteorite had probably never been molten since its formation. Thus the discovery of evidence for the extinct radioactivity ^{107}Pb in Santa Clara was *stronger* evidence for a recent nucleosynthesis "(perhaps in a supernova)" than the discovery of evidence for ^{26}Al in Allende (Kelly and Wasserburg, 1980, p. 15).

that the ^{26}Al that decayed to produce the ^{26}Mg found in Allende must have been synthesized in a supernova. Truran and Cameron (1978) attempted to support this assumption by a new and more comprehensive analysis of the process by which ^{26}Al is synthesized. They suggested that ^{14}N, produced in the usual carbon cycle, is converted to ^{18}O by absorption of an α particle and subsequent positron emission. Two more α particles and ejection of a neutron give ^{25}Mg. The dominant mechanism that produces ^{26}Al is absorption of a proton by ^{25}Mg, but it is destroyed by the reaction $^{26}\mathrm{Al}+n \rightarrow {}^{26}\mathrm{Mg}+p$. Thus the amount of ^{26}Al produced depends on the concentration of free neutrons and protons (as well as on the cross sections of these reactions). Heavy elements may be competing to absorb the neutrons and thus decrease the rate of destruction of ^{26}Al. Taking account of all these factors, Truran and Cameron estimated that the ratio $^{26}\mathrm{Al}/^{27}\mathrm{Al}$ should be between 4×10^{-4} and 2×10^{-3}. They concluded that "the supernova event forming ^{26}Al occurred between 2 and 3.7 million years prior to condensation of solar nebula material"—the implication being that their calculation supports the trigger theory.

Other groups confirmed that ^{26}Al could be produced in supernovae. Arnett and Wefel (1978) calculated the production of ^{26}Al in the carbon shell of a massive $(12M_{\odot})$ star and found a $^{26}\mathrm{Al}/^{27}\mathrm{Al}$ ratio of about 10^{-3}, within the range estimated by Truran and Cameron, but based primarily on quasihydrostatic rather than explosive burning. Woosley and Weaver found a similar result (1980) by considering explosive neon burning rather than carbon burning. In either case the products would eventually be ejected in a supernova explosion. Vangioni-Flam, Audouze, and Chièze (1980) calculated much higher ratios of $^{26}\mathrm{Al}/^{27}\mathrm{Al}$, up to 0.1, but only under conditions of very high temperatures and densities.[36]

But evidence soon emerged that ^{26}Al could be produced more abundantly in other processes. Henry Nørgaard argued (1980) that ^{26}Al could be produced from ^{25}Mg in the outer envelope of red giant stars, giving a $^{26}\mathrm{Al}/^{27}\mathrm{Al}$ ratio ranging from 0.5 to 1.0 in some cases. Since "such stars are known to be losing mass at a considerable rate and. . .there is strong observational indication of the presence of grains in the outer atmosphere of these stars," Nørgaard suggested that they could have contributed to the ^{26}Mg excess in Allende. He noted the discovery by Srinivasan and Anders (1978) in the Murchison meteorite of isotopic anomalies in the noble gases of the kind expected to result from nuclear processes in red giants; this was additional evidence that dust grains from red giants had been injected into the solar system.

Wolfgang Hillebrandt and Friedrich-Karl Thielemann (1982), following up earlier work by Arnold, Nørgaard, Thielemann, and Hillebrandt (1980), proposed that nucleosynthesis in novae could be a significant source of both Ne-E and ^{26}Al. They obtained $^{26}\mathrm{Al}/^{27}\mathrm{Al}$ production ratios of about 1, although they considered the total production rate of ^{26}Al quite uncertain because several of the relevant proton capture rates were not accurately known.

In 1981 Worden, Schneeberger, Kuhn, and Africano questioned the need for a supernova to produce ^{26}Al on the basis of their analysis of flare activity on T Tauri stars. They suggested that "the expected proton flux from these events may explain early solar system abundance anomalies without recourse to nearby supernovae" (p. 520). "While estimates based on T Tauri energetics cannot refute the supernova theory [as an explanation of isotopic anomalies], we find the consistency of the irradiation models with the flux estimates considerably more satisfactory than appealing to the special circumstances of a supernova to explain the abundance anomalies" (p. 526). They conclude: "The total proton flux expected from the flares is consistent with the irradiation model for solar isotopic abundance anomalies, thus precluding the necessity for a nearby supernova" (p. 527; see also Feigelson, 1982.)

Another piece of research in nuclear physics strengthened the hypothesis that ^{26}Al could be produced more abundantly in sites other than supernovae. Champagne, Howard, and Parker (1983a,1983b,1983c) found a low-lying resonance in the reaction $^{25}\mathrm{Mg}+p \rightarrow {}^{26}\mathrm{Al}+\gamma$, indicating that its rate would be ten orders of magnitude greater at low stellar temperatures than previously estimated. They noted that "a supernova explosion is still a most efficient dispersal mechanism, but may not be the primary production route. The actual source of ^{26}Al in the early solar system is therefore still open to question" (Champagne *et al.*, 1983c, p. 689).[37]

These calculations acquired new significance when the gamma-ray telescope on the High Energy Astronomical Observatory satellite (HEAO-3) revealed relatively large amounts of ^{26}Al throughout the galaxy (Mahoney *et al.*, 1982, 1984).[38] Donald Clayton (1984) argued that these amounts could not have been synthesized by supernova explosions if current calculations of the production ratio are correct. "The observed ^{26}Al is more likely due to about 10^8 dispersed novae, or to a single old (10^4-10^6 yr) supernova remnant that today surrounds the solar sys-

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[36] These calculations depend on assumptions about the equilibrium between the ground state and the short-lived isomeric state of ^{26}Al; see Ward and Fowler (1980).

[37] Fowler (1984, p. 169) says his view that ^{26}Al could not be synthesized in supernovae at high temperatures because of the large cross section for $^{26}\mathrm{Al}(n,p)^{26}\mathrm{Mg}$ was confirmed by measurements of Skelton *et al.* (1983) on $^{26}\mathrm{Mg}(p,n)^{26}\mathrm{Al}$.

[38] ^{26}Al was expected to provide one of the sharpest lines in the diffuse galactic background; see Ramaty and Lingenfelter (1977, 1978); Lingenfelter and Ramaty (1978).

tem. If the ^{26}Al is dispersed, the high interstellar ratio today. . .calls into question the requirement that a supernova trigger for formation of the solar system was the cause of a concentration 3-times larger than" (D. Clayton, 1984, p. 144). He stated that novae are better candidates, and that the value of the ^{26}Al concentration inferred from Allende "was simply the average interstellar value at that time, negating the need for a "supernova injection" of ^{26}Al into the forming solar system" (1984, p. 144).

During the previous decade Clayton had been undermining the supernova trigger hypothesis from another direction by showing that heavy-element anomalies could be more plausibly interpreted in terms of presolar grains (see Sec. VI.B). He proposed that the barium and neodymium isotopic anomalies found by McCulloch and Wasserburg (1978a) should be interpreted as extinct radioactivities resulting from radioactive decay within interstellar grains, and that the Ca-Al-rich inclusions in Allende are not condensates from a hot gaseous solar nebula but admixtures of precondensed matter formed by heating with some separation of *r*-process and *s*-process products taking place during accumulation processes (D. Clayton, 1978b; cf. McCulloch and Wasserburg, 1978b). Clayton states that the remeasurement of the Nd cross sections by Mathews and Käppeler (1984) "totally vindicated my approach and my suggestions" (D. Clayton, 1988). Similarly he argued that isotopic anomalies of strontium (Papanastassiou *et al.*, 1978) and samarium (Lugmair *et al.*, 1978) could best be explained by gas/dust fractionation in the protosolar accumulation rather than by supernova injection (D. Clayton, 1978c,1978b).

Early in 1984 Cameron (1984b) announced that the reasons that led him and Truran to propose the supernova trigger no longer seemed compelling. Many of the isotopic anomalies could be explained without postulating injection from a nearby supernova (Consolmagno and Cameron, 1980); processes in red giants and novae could account for more copious production of ^{26}Al than supernovae; and the HEAO-3 observations proved that ^{26}Al was indeed copiously produced. Others might continue to support the trigger hypothesis (McSween, 1984; Couper and Henbest, 1985, p. 126; Goldsmith, 1985, p. 368; Lee, 1986; Hughes, 1988), but for Cameron it was time to put it back on the shelf.

VII. SAFRONOV'S PROGRAM

A. Accretion of particles in the protoplanetary cloud

By 1960 the hypothesis that planets formed by gravitational collapse of massive gaseous protoplanets, advocated primarily by G. P. Kuiper, had been abandoned by most planetogonists (Urey, 1956; Cameron, 1962c, p. 41; Ruskol, 1960). The most popular alternative was accretion of solid particles, with or without the presence of gas during the later stages of planetary formation.

Accretion theories originated in the 19th century, when they were associated with the idea that the Earth and other planets were built up from meteoritic material. Chamberlin (1903, 1905) revived the idea under the name "planetesimal hypothesis," giving it both astronomical and geological respectability (Brush, 1978a). On the astronomical side he removed the objection that planets formed by accretion would have retrograde rotation, by showing that coalescence of planetesimals in intersecting elliptical orbits would somewhat favor the formation of objects with prograde rotation. His conclusion was confirmed long afterward by modern planetary theorists (Artem'ev and Radzievskii, 1965; Giuli, 1968; A. Harris, 1977; see Brush, 1978a, p. 34, footnote 62, for further discussion). On the geological side Chamberlin worked out the properties of an Earth assembled slowly enough to remain cold and solid throughout its early history. His theory avoided the consequences (especially the excessively small age) of the assumption that it had condensed from a hot fluid ball.

Although several scientists such as Urey and Ringwood discussed the chemical aspects of the formation of terrestrial planets (Secs. IX.A and IX.B), there were few attempts before 1970 to develop quantitative physical models of the accretion process itself. This seems odd in view of the fact that powerful theoretical methods for treating very similar processes were widely known in the physical sciences. The kinetic theory of gases, formulated by James Clerk Maxwell and Ludwig Boltzmann, had been actively developed for a hundred years; it provided systematic techniques for computing the properties of systems of colliding particles and could be modified to take account of inelasticity of collisions, combination and fragmentation of particles, their nonspherical shape, spatial inhomogeneities, external fields, etc. (Hirschfelder *et al.*, 1954; Brush, 1972, 1976). Physical chemists had worked out approximate theories to describe coagulation and chemical reactions in fluid media. Astrophysicists were familiar with the application of stochastic models to systems of interacting stars (Chandrasekhar, 1943). And, when analytic techniques could not adequately handle more complicated "realistic" models, computers were available to grind out numerical solutions. It appears to me that most of the theoretical research on planetary accretion done in the 1970s and 1980s—with the possible exception of some projects requiring very fast, large-memory computers—could have been done at least 10 or 15 years earlier if anyone had been interested.

In fact, the only person who seems to have been seriously interested in pursuing this kind of research during the 1960s was V. Safronov (Fig. 3) at the O. Yu. Shmidt Institute of Earth Physics in Moscow. Following the ideas of Shmidt (1944, 1958) and other Soviet cosmogonists (Levin, 1948, 1953, 1956; Gurevich and Lebedinskii, 1950), Safronov worked out in considerable detail the dynamical and thermal aspects of a model of colliding, accreting, and fragmenting solid particles (Shmidt, 1944, 1949, 1958; Levin, 1948, 1956; Gurevitch and Lebedinskii, 1950; for general surveys see Safronov, 1983; Safro-

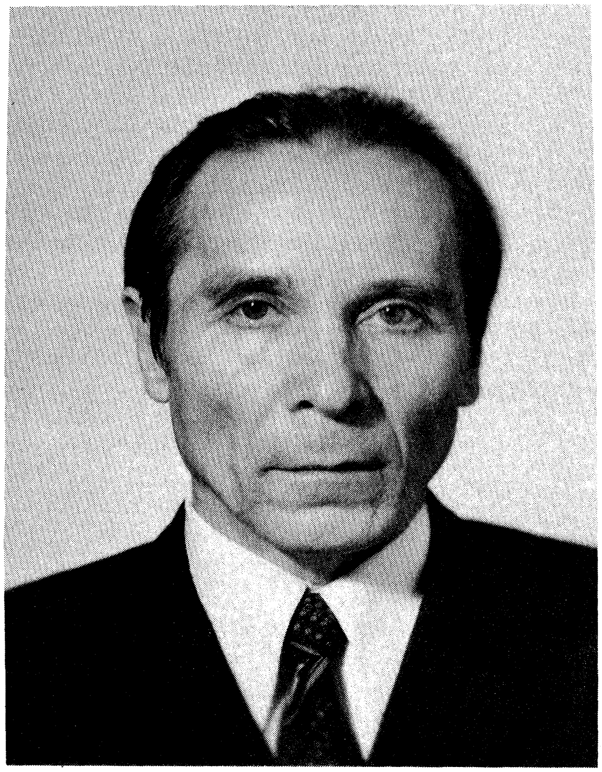

FIG. 3. Viktor Sergeyevich Safronov, Soviet astronomer (planetary cosmogony).

nov and Vityazev, 1983; comments on the Shmidt capture process by Lyttleton, 1968, pp. 28–32).[39]

Although a few of his papers appeared in English translation shortly after publication (Safronov, 1959, 1962a, 1965, 1966), Safronov's achievements were not generally recognized in the West until 1972, when an English-language version of his 1969 book *Evolution of the Protoplanetary Cloud* became available. Since then the Safronov model or one of its variants has been the most popular explanation for the formation of the terrestrial planets. It has also played a major role in the leading theories of the origin of the giant planets and their satellites, asteroids, comets, and meteorites.

Safronov urged a division of labor in cosmogony: the problem of the origin of the protoplanetary cloud (PPC) could be treated separately from the problem of its evolution into planets, and that problem in turn was distinct from the history of planets after their formation. He preferred the hypothesis of common formation of the sun and PPC over Shmidt's assumption that the PPC was formed elsewhere and later captured by the sun, but considered himself a proponent of Shmidt's ideas since his model pertained only to the second stage. Thus Safronov's theory did not compete with those of Hoyle and Cameron in trying to explain the formation of the sun. He did dispute Cameron's assumption that the PPC was very massive (two to four solar masses), preferring a low-mass PPC (about 0.05 solar masses). He also rejected the assumption of Weizsäcker, Cameron, Hoyle, and others that turbulence played an important part in the evolution of the cloud.

Starting with a relatively low-mass gas-dust cloud in which any primeval disordered motions had been damped out, Safronov assumed that dust particles would settle to the central plane and grow to centimeter size. As suggested by Edgeworth (1949) and by Gurevich and Lebedinskii (1950), the dust layer would break up into several condensations by local gravitational instability. These condensations would then combine and contract.

Coagulation theory goes back to the work of Marion von Smoluchowski on Brownian movement at the beginning of the 20th century, as presented in Chandrasekhar's influential review article (1943). In Safronov's first model, fragmentation by collisions was ignored; the coagulation coefficient was assumed to be proportional to the sum of the masses of two colliding bodies. The number of particles with mass m was found to vary approximately as $m^{-3/2}$ for long times, except for large m, where an exponential damping factor became important. Fragmentation did play a role, especially when the relative velocity of two colliding particles was high. But if the relative velocity was very small, the particles would tend to move in similar orbits and collide so rarely that growth could not occur. Safronov argued that as the particles grew, encounters that did not lead to collisions would increase the relative velocities. The relative velocities most favorable for growth are those somewhat less than the escape velocity, which of course depends on the mass of the particles. The average relative velocity would tend to increase as the particles grew, so that it would remain in the range favorable for further growth (Wetherill, 1980a, p. 5; D. Fisher, 1987, pp. 224–226).

Safronov also concluded that when one body in a region happened to become significantly larger than the others, it would start to grow even faster because its effective cross section for accretion of other bodies would be enhanced by gravitation. In this way a single planet could emerge in each "feeding zone" within the PPC and then sweep up the rest of the material in that zone.

Safronov (1959) noted the importance of high-speed impacts of a few large bodies in the formation of the

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[39] Witting (1966, p. 83) wrote that "Shmidt's theory appears to be on solid ground as far as the boundary conditions [facts to be explained] are concerned; none are violated, and the theory is able to explain many of the dynamical boundary conditions well and completely." English translations of many of the major Soviet articles on planetogony and an extensive bibliography are being prepared for publication (A. Levin and Brush, 1991). The earliest comparable work in the West is that of Stephen H. Dole (1970), but his calculation was much less ambitious than those of Safronov.

Earth, a feature he attributed to B. Yu. Levin. He estimated that the formation of the Earth was essentially completed in 10^8 years, and that in spite of the large impacts the initial temperature inside was only a few hundred degrees. Using an equation derived by Lyubimova (1955) he found that heating by contraction would raise the central temperature to about 1000 K at the end of the formation process; radioactive heating would later raise this to several thousand degrees. Thus the 19th-century scenario—cooling from an initial temperature of several thousand degrees—was completely reversed. Here Safronov's model was in agreement with Western studies of the thermal history of the Earth (e.g., Urey, 1951).

Using a theoretical relation between the impacts of small bodies on the accreting planets and the resulting inclination of their axes of rotation, Safronov estimated from the observed inclinations that the largest bodies striking the Earth during its formation had masses about one-thousandth that of the present Earth (Safronov, 1965, 1972a, p. 134). Thus the large tilt of the Uranian axis was ascribed to impact of a body having $\frac{1}{20}$ the mass of that planet.

If the initial temperature of the Earth was only a few hundred degrees, one might think that planets further from the sun started out much colder—perhaps cold enough to freeze hydrogen and helium from the PPC. But Safronov (1962a, p. 278) argued that the gas-dust layer was so thin that the sun's radiation would go not only through it but along its surface, so that it could be scattered into it through a boundary layer. This effect would keep the temperature from falling below 30 K at the distance of Jupiter and 15 K at the distance of Saturn. Thus these planets could not condense hydrogen directly but could only accrete it gravitationally after reaching a sufficiently large mass at a later stage of their growth.

A major drawback of Safronov's theory was that the estimated time for formation of the outer planets, using the equations derived for the terrestrial planets, was about 10^{11} years. In addition to the obvious disadvantage of requiring a time longer than the present age of the solar system (4.5×10^9 yr) to form these planets, it is inconvenient not to have a fairly massive proto-Jupiter present while Mars is being formed, if one wants to attribute the small size of Mars (relative to Earth) to interference from its giant neighbor.

To alleviate this difficulty Safronov assumed that the outer regions of the PPC originally contained a much larger amount of material, much of which was ejected by gravitational encounters with the growing embryos of massive planets. This hypothesis would accelerate the early stages of the accretion process, while gravitational trapping of gas would accelerate the later stages (1972a, Chap. 12, 1972b). But the *ad hoc* or qualitative nature of these hypotheses damaged the credibility of the theory.

The extremely low initial temperature of the Earth also created a problem if one wanted to explain the segregation of iron into the core. Safronov was temporarily attracted by the idea that the Earth's core is not iron but silicate, chemically similar to the mantle but converted to a metallic fluid by high pressure. This was the hypothesis of V.N. Lodochnikov (1939) and W. H. Ramsey (1948, 1949), widely discussed in the 1950s. As pointed out by Levin (1962) it had cosmogonic advantages which Safronov recognized (1972a, p. 152). But the postulated silicate phase transition proved elusive, and it was shown both experimentally and theoretically that silicate compounds did not have high enough density at core pressures. So Safronov was forced to accept either the traditional iron core or a compromise iron-oxide core, with a correspondingly higher internal temperature (Safronov 1972c, pp. 445 and 446).

Safronov's program lacked the glamour of more abitious schemes that promised to explain the formation of the sun as well as the planets from a simple initial state, and it encountered difficulties in explaining the properties of the present solar system. Yet he was successful in building up a body of basic theory that turned out to be useful as a starting point for other cosmogonists.

B. The Americanization of Safronov's program

The *Science Citation Index* gives a rough measure of the visibility of selected publications in the Western scientific community. Of course one cannot get any information about the nature of the reception or influence of those publications from citation counts alone, and citations not listed in the *Index* may turn out to be more important that those that are (see footnote 9). Bearing in mind these caveats, I still think it is significant that the total number of citations (excluding those by Safronov and other Soviet scientists) of all of Safronov's publications from 1961 through 1971 was only 25. For comparison, one paper by Cameron (1962b) was cited 101 times in this period (excluding self-citations). Starting in 1972, the year when Safronov's *Evolution of the Protoplanetary Cloud* was first available in English translation, going through 1982, that book was cited 107 times by non-Soviet scientists, and Safronov's earlier papers were cited 31 times. So his visibility in the West was more than five times as great in the second 11-year period, primarily because of the English translation of his book.

Looking at the papers that cited Safronov in the 1970's one finds that almost all of them contain favorable remarks, even when disagreeing on specific technical points. Here are some examples of the Western response to Safronov's work.

(1) Peter Goldreich and William W. Ward, in a note added in proof at the end of an influential paper on the formation of planetesimals by gravitational instability in a dust disk without the need to invoke "stickiness," admitted that Safronov had given a similar discussion, which they had apparently read only after finishing their own work (Goldreich and Ward, 1973, p. 1061). Safronov subsequently received partial credit for what nevertheless was most often called "Goldreich-Ward instability."

(2) S. J. Weidenschilling (1975) supported Safronov's

suggestion that matter ejected from Jupiter's zone could deplete the zones of Mars and the asteroids.

(3) R. J. Dodd and W. Napier (1974) reported that numerical simulations based on Safronov's model confirmed his conclusion that a dominant nucleus arose which quickly incorporated lesser objects; the simulations gave correct values for the rotation rates of terrestrial planets but not for Jupiter and Saturn.

(4) Joseph Burns (1975) suggested that the angular momentum of Mars could be attributed to the impacts of the last few bodies falling on it, as in Safronov's theory.

(5) S. F. Singer (1977) agreed with Safronov that the observed obliquities of the planets could be explained by late impacts.

(6) In an elaborate calculation of the thermal evolution of Earth and moon based on Safronov's model, Kaula (1977) found that accreting planetesimals would add enough heat to the Earth to bring about core segregation if not vaporization; he also inferred from his results that an impact origin of the moon (Sec. XI.B) was more likely than binary accretion.

(7) P. Farinella and P. Paolicchi (1977) found from their theory results on the mass distribution consistent with those of Safronov.

(8) J. N. Goswami and D. Lal (1979) stated that their observations of particle tracks in chondrites provided evidence for Safronov's accretion model and against Cameron's (1978a, 1978b) gas collapse model.

(9) W. K. Hartmann and D. R. Davis (1975) acknowledged that they had been "influenced by some of the early Soviet accretion theories, published in the 1950s and 60s," in developing their ideas about lunar origin, although they had not studied Safronov's (1972a) book in detail.

Many other scientists simply quoted and used Safronov's results without bothering to discuss their validity. Weidenschilling's remarks, quoted in Sec. XII.G, help to explain the acceptance of this model.

In 1976, George Wetherill (Fig. 4) at the Carnegie Institution of Washington announced the first results of his calculations on a modified version of Safronov's theory. Wetherill's work was motivated in part by photographs of Mercury's surface taken by the *Mariner 10* spacecraft on 29 March and 21 September 1974, analyzed by Bruce Murray's group (Murray *et al.*, 1975, 1977). It appeared that Mercury, like the moon, had suffered a "late heavy bombardment" after its formation (Wetherill, 1975a). Hence it was likely that there was a high flux of asteroid- or moon-sized bodies throughout the inner solar system, 4–4.5 billion years ago.

Wetherill's research, unlike Safronov's, made extensive use of computer simulation. Although he confirmed many of Safronov's results, he found one important difference. When the Earth was half-formed, its feeding zone merged with that of Venus. The resulting perturbations produced higher relative velocities and thus reduced the cross section for capture of planetesimals by massive bodies. This would prevent runaway growth of the largest embryo in each zone. The second-largest body in the Earth's zone could then have a mass as large as $\frac{1}{20}$ of the Earth's, rather than only $\frac{1}{1000}$.

FIG. 4. George W. Wetherill, American astronomer (planetary science).

Such large bodies, though having only a transient existence in the last stage of accretion, would produce substantial heating by their impacts on the terrrestrial planets and the moon (Kaula, 1979). Since Safronov accepted the conclusion that the Earth had been heated by large impacts during its formation (Safronov and Kozlovskaya, 1977; Safronov, 1978, 1981), Wetherill could say (1981a) that every current theory predicted high initial temperatures for the formation of planets.[40]

A group at Tucson announced another numerical simulation project based on a modification of Safronov's theory (Greenberg *et al.*, 1978a, 1978b). They supported the idea that large bodies were prevalent in the early solar system by showing that planetesimals as large as those generated in Wetherill's scheme could have been generated without invoking perturbations by proto-Venus (Greenberg, 1979). Further numerical results (Greenberg, 1980) generally supported Safronov's analytic work but contradicted his conclusion that relative velocities of planetesimals would tend to be comparable with the escape velocity of the dominant body. More of the total mass of the system was found to be in smaller planetesimals, which would collide mostly with each other and therefore tend to have smaller velocities; hence when they did collide with a larger body they would be

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[40] Murray *et al.* (1981, p. 9) stated that this was one of the two major new ideas in planetogony that "have gained increasing acceptance since the space age began," the other being heterogeneous accretion (Sec. IX.D). Ringwood (1988) points out that he had persistently advocated it during the 1960s when the cold origin was a generally accepted dogma.

more likely to accrete and promote its runaway growth (cf. Levin, 1978a, 1978b). One consequence of this result was that Uranus and Neptune could grow "in a reasonably short time, well below the actual age of the system, without the need for *ad hoc* assumptions about excess mass or artificially-low relative velocities among the icy planetesimals" (Greenberg *et al.*, 1984).

To sum up the situation at the end of the period covered by this review: we are still not sure how centimeter-sized particles grew to kilometer-sized bodies (Boss, 1989, p. 784). Given the existence of such bodies, Wetherill (1985) has shown that a modified Safronov model may be able to explain the existence of four terrestrial planets starting from 500 bodies each of mass 2.5×10^{25} kg (one-third lunar mass). But this result is clearly stochastic and depends on the existence of large impacts. Several runs gave three or four planets, but none reproduced precisely the observed distribution of masses and distances. So the best theory of the formation of terrestrial planets was not quite capable of explaining the simplest properties of those planets as known 200 years ago.

VIII. THE GIANT PLANETS

A. Gaseous condensation models

During the 1950s it was generally held that the interiors of the Jovian planets are mostly or entirely solid, consisting mostly of hydrogen with smaller amounts of helium and heavier elements (Ramsey, 1951; DeMarcus, 1958; Wildt, 1958, p. 244, 1961, pp. 197–202; Öpik, 1962, p. 248).[41] These heavier elements ("ices" of H_2O, CH_4, NH_3, and dust) may have separated into a small central core (Öpik, 1962, pp. 222 and 223; Peebles, 1964, pp. 344 and 346). It seemed likely that they had formed by gravitational instability in the gaseous primordial solar nebula, although Öpik (1962, p. 255) argued that accretion from a cloud of solid particles was also a plausible origin.

In 1968 William Hubbard, on the basis of Frank Low's (1966) estimate of the excess thermal radiation of Jupiter, concluded that it must have an internal energy source. Estimating its central temperature as about 10^5 K, he inferred that a rigid atomic or molecular lattice could not exist; the interior must behave like a convecting fluid.[42] He developed thermal models of both Jupiter and Saturn on this basis (Hubbard, 1968, 1969).

In the 1970s two competing models for the origin of the giant planets were developed (Williams, 1979). One model treated them as miniature protostars, contracting gaseous subcondensations from the primordial nebula that reached a maximum temperature that was not high enough to initiate thermonuclear reactions (so they could not become real stars) and then cooled down. A rock/ice core could form later, by precipitation of dust inside the collapsed cloud or by capturing particles from outside (Bodenheimer, 1974, 1976, 1977; Graboske *et al.*, 1975; Pollack *et al.*, 1977, 1979; A. S. Grossman *et al.*, 1980). Safronov (1974, p. 101) criticized this model, arguing that gaseous condensation could not explain the formation of Jupiter, for example, because one would need 60 times its mass to be initially present in its zone in order to produce gravitational instability. It would then be difficult to explain why only $\frac{1}{60}$ of the original gas ended up in the planet.

The other model, proposed by Cameron (1973d) and worked out in detail by Perri and Cameron (1974) and by Podolak and Cameron (1974a, 1974b), might be considered an application of Safronov's program, although it was introduced for other reasons. They postulated that solid material would first accrete up to a critical size, which would then cause the surrounding gas to become unstable and collapse onto it.

But, as noted in Sec. V.B, when Cameron reformulated his nebular models on the basis of accretion disk theory, he concluded that all the planets were formed from giant gaseous protoplanets. According to Cameron (1988, private communication), the motivation for introducing this hypothesis was that Safronov-type theories predicted that several billion years were needed for the formation of Neptune. "I considered that to be entirely unacceptable and adequate grounds for rejecting the theory entirely (although, of course, not the mechanisms which obviously played some role in planetary accumulation). What appeared to be necessary was an alternative theory that could get Nepture together in a reasonable time, and if it could do this, should it not also be a faster way of assembling the other planets as well? It seemed likely that such an alternative theory would probably involve gravity, which is capable of acting quickly. And thus I was led to investigate the mechanism of gravitational instability in the gas of the nebula (very early when there was little mass around anyway)."

DeCampli and Cameron (1979) applied the giant gaseous protoplanet hypothesis (supported by British astronomers since the 1960s—McCrea, 1960, 1963; Dormand and Woolfson, 1971, 1974, 1977, 1989; Donnison and Williams, 1974, 1978, 1985; Schofield and Woolfson, 1982a, 1982b) to the formation of giant planets. They noted that it had earlier been rejected (a) because it was believed that rocky cores exist in the giant planets and that such cores could not have been formed by settling of grains within a gaseous object and (b) because it was assumed that a giant protoplanet would be destroyed by solar tidal forces. They argued against these two objections, claiming that rapid grain growth can take place in-

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[41] Öpik's paper was written in part as a critique of the only serious alternative, Alfvén's hypothesis (1954), which implied that these planets consisted primarily of C, N, and O.

[42] Öpik had also concluded, on the basis of older measurements, that Jupiter radiated more energy than it received from the sun. For additional measurements of the temperatures and estimated energy fluxes of Jupiter and Saturn, see Aumann *et al.* (1969); Ingersoll *et al.* (1980); Hanel *et al.* (1981, 1983).

side gaseous protoplanets and that the protoplanets would not be disrupted by tidal forces. The DeCampli-Cameron model was defended by Cameron in later papers (1979b, 1985, p. 1096; Cameron *et al.*, 1982) and further developed by Bodenheimer *et al.* (1980).

After 1985, Cameron says, he "abandoned this idea and its consequences. . . when some reasonable ways of forming Neptune quickly were suggested. The most important suggestion was due to Lissauer, who postulated a much more massive solar nebula from Jupiter on out. However, in the meantime I had examined the case for Mercury on both planetary accumulation pictures." In work with Fegley, Benz, and Slattery, Cameron found that "Mercury in the GGP [giant gaseous protoplanet] scenario barely squeaked by as possible but improbable, whereas Mercury in the Wetherill scenario turned out to be very plausible. All of these things coming together convinced me Wetherill was right" (Cameron, 1988, private communication).

B. The Kyoto program and the nucleation model

Going back to the situation in the 1970s, we see that there were two distinct theories for the formation of the giant planets; neither seemed to have been developed far enough to make specific predictions about observational data. In order to find a crucial test, planetary scientists turned to a theory developed by Chushiro Hayashi and his colleagues at Kyoto University. In particular Hiroshi Mizuno, a member of the Kyoto group, worked out a quantitative application of the theory to the formation of Jupiter and Saturn. Mizuno's model (1980) can be plausibly interpreted as an extension of the Safronov model, and its success is regarded as a victory of the planetesimal accretion theory over the gaseous condensation theory.[43]

Hayashi's group published a series of papers on protostars and the solar nebula during the 1960s and 1970s. The papers by Hayashi, Adachi, and Nakazawa (1976) and by Hayashi, Nakazawa, and Adachi (1977) took up the growth of protoplanets from 10^{25} g to the mass of the Earth (10^{28} g) or Jupiter's core (10^{29} g). They concluded that the capture of planetesimals could be accelerated by gas drag, so that the Earth would be formed in 10^7 yr and Jupiter's core in 10^8 yr.

Mizuno, Nakazawa, and Hayashi (1978) then investigated the instability of a gaseous envelope surrounding a planetary core. Perri and Cameron (1974) had found that if the core mass of proto-Jupiter were greater than about 70 Earth masses the envelope could no longer sustain hydrostatic equilibrium and would collapse onto the core. They assumed the envelope to be adiabatic, but Mizuno *et al.* argued that its outer layer should be isothermal because of its low opacity. In this case the critical mass would be reduced to only 15 Earth masses for proto-Jupiter and 6 for proto-Saturn. (A similar calculation was reported briefly by Harris, 1978.) These values are roughly consistent with those of Slattery (1977), who found cores of about 15 Earth masses for both Jupiter and Saturn.

In a more elaborate calculation using a three-layer envelope (isothermal, radiative, and convective), Mizuno (1980) found that the critical core mass was nearly independent of the protoplanet's distance from the sun. With a reasonable value for the grain opacity in the envelope, this critical mass came out to about ten Earth masses, an acceptable value for all four giant planets.

Mizuno's remarkable result for the critical core mass and the Mizuno-Nakazawa-Hayashi scenario for formation of giant planets were quickly acclaimed by experts on planetary structure and evolution (Hubbard and MacFarlane, 1980, p. 232; Hubbard, 1981, p. 321; Lunine and Stevenson, 1982; Safronov and Ruskol, 1982, p. 286; Stevenson, 1982a, pp. 277–288; Smoluchowski, 1983, pp. 137 and 138; Weidenschilling, 1983, p. 209; Weidenschilling and Davis, 1985). Stevenson (1982b, p. 762) and Pollack (1984, p. 404) noted that the Kyoto "nucleation" model still left unanswered some important theoretical questions, such as the conditions that actually determined collapse. Stevenson (1982b, p. 763) and Bodenheimer (1982, p. 47) also suggested that the nucleation model had not yet solved the difficulty of excessively long accretion times for giant planets characteristic of Safronov's theory. But Stevenson (1982b, pp. 755 and 763) also concluded that *no* current model of giant planet formation satisfied all the observational constraints (see Boss, 1988a, 1989, for the current status of this problem).

C. A crucial test?

The empirical observation that has been claimed to provide a crucial test between the Kyoto nucleation model and the gaseous condensation model is the infrared spectrometer (IRIS) measurement of the carbon/hydrogen ratios in giant planet atmospheres by the *Voyager* spacecraft. Gautier and Owen argued in several papers that the observed enhancement of carbon in the four giant planets relative to its solar abundance could be explained by the nucleation model, in which methane ice as well as grains of refractory materials and ices of H_2, NH_3, etc., accreted to form cores. The accretion process would have heated the core, releasing methane, which

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[43] Mizuno's work is still known to only a small group of experts. The recent popular book by David Fisher, which is generally quite accurate on many aspects of the recent history of planetary cosmogony, gives a rather misleading account of this episode and fails to mention either Hayashi or Mizuno (Fisher, 1987, pp. 162 and 163). The *Science Citation Index* would be of no help unless one already knew that Mizuno's paper was important and read all the papers that cited it, rather than just counting them, since the citation rate is still lower than that of the abandoned supernova trigger theory of Cameron and Truran (1977).

then enriched the surrounding gaseous envelope. The giant gaseous protoplanet model of Bodenheimer (1974) and Cameron (1978a, 1978b), on the other hand, implied, according to Gautier and Owen, that the composition remained solar and homogeneous during collapse. Thus the observed carbon enhancement favors the nucleation model over the GGP model (Gautier *et al.*, 1982; Gautier and Owen, 1983, 1985; see also Hubbard, 1981, p. 321; Torbett *et al.*, 1982; Weidenschilling, 1983, p. 209; Baines *et al.*, 1984; Courtin *et al.*, 1984).

Pollack (1985, p. 810) disagreed, arguing that the enhanced C/H ratio could be explained in either model. He stated that the nucleation model still had difficulty forming giant planets quickly enough, especially if one needed Jupiter before Mars; on the other hand the giant gaseous protoplanet model had trouble forming cores for Jupiter and Saturn.

Others argued that the crucial test should be the ice-rock ratio in Uranus and Nepture; but the results of this test were not yet conclusive at the end of 1985 (Podolak and Reynolds, 1984; Pollack, 1984; D. Fisher, 1987, p. 163).

Cameron (1985, pp. 1097 and 1098) apparently saw no conflict between Mizuno's model and his own. He suggested that giant protoplanet cores might be formed in the inner parts of the nebula, then moved by tidal interactions to the outer part, where they could capture the surrounding gas to form massive envelopes by Mizuno's process. But he later abandoned the gaseous protoplanet hypothesis as a result of his work with Fegley on Mercury and Stevenson's analysis of the formation of Jupiter and Saturn (Cameron, 1988, p. 461).

IX. CHEMICAL COSMOGONY: THE TERRESTRIAL PLANETS

A. Urey and the formation of terrestrial planets

In the previous section we saw that a possible crucial test between two theories turned out to involve chemical composition, although chemical considerations were not central to either theory. We now turn to a group of theories that depend on chemistry in a much more direct way: those designed to explain and predict the chemical composition of the *terrestrial* planets. Although much of this literature is devoted to technical details, an important general question emerges: did solar system material pass through a stage when the temperature was high enough to vaporize and mix it, so that all evidence pertaining to its possible previous existence in a condensed state was lost? Was the solar system "born again" with no "memory" of a previous incarnation, or can we identify the place where its atoms were synthesized and learn how we are descended from the rest of the universe? Thus we arrive by a different route at the same problem addressed by the supernova trigger hypothesis.

Chemical cosmogony, or more generally cosmochemistry, acquired its importance in the American scientific community after World War II primarily because of the efforts of Harold Urey (1893–1981). As a winner of the 1934 Nobel Prize (for his discovery of deuterium) and an expert on nuclear chemistry, Urey (Fig. 5) had the requisite prestige and energy to lead the younger generation of physicists and chemists into planetary science (Ringwood, 1979, p. v; Taylor, 1980, pp. 2 and 3; Sagan, 1981; Ezell and Ezell, 1984, p 17), a subject scorned earlier in the century as not worthy of the best minds (Sagan, 1966; Brush, 1978b; Whitaker, 1985; Tatarewicz, 1986).

Urey became interested in the formation of the Earth when he agreed to give a course on "Chemistry in Nature" with Harrison Brown at the University of Chicago in 1948 or 1949 (Urey, 1952b, p. ix; on Brown see K. R. Smith *et al.*, 1986). To prepare his first lecture on the heat balance of the Earth he read Louis B. Slichter's 1941 article and was surprised to learn that the temperature of the Earth must actually be rising rather than falling. He wrote in 1952: "This led on to the consideration of the curious fractionation of elements which must have occurred during the formation of the earth. One fascinating subject after another came to my attention, and for two years I have thought about questions related to the origin of the earth for an appreciable portion of my waking hours. . ." (1952b, p. ix).

In a long article (1951) and a comprehensive book (1952b), Urey presented his views on the planets and the moon from a physicochemical perspective. He assumed

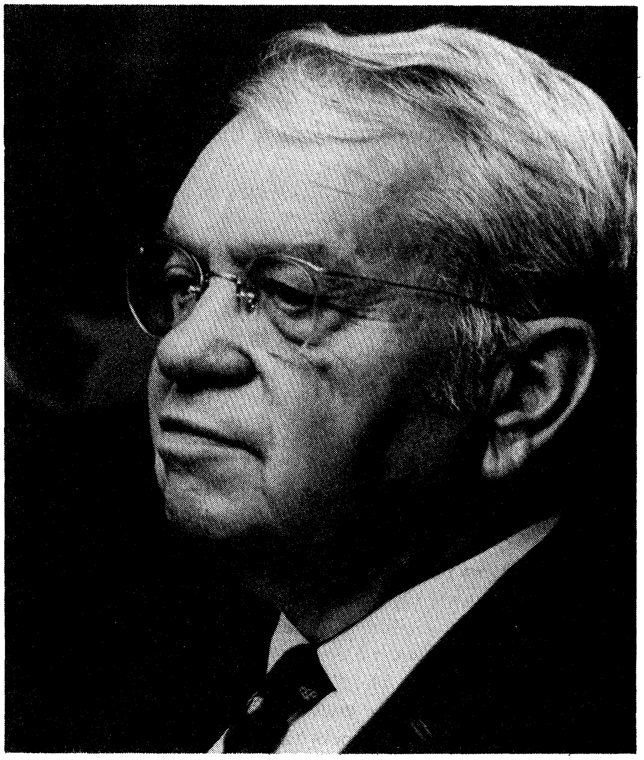

FIG. 5. Harold Clayton Urey (1893–1981), American chemist (isotopes, cosmochemistry).

as a matter of course that the original nebula "was once completely gaseous and at very high temperatures" (1951, p. 237) and undertook to determine the sequence in which different chemical compounds would condense as the nebula cooled (1952a). He had initially supposed that the Earth accumulated at about 900 °C as a "concession to traditional high-temperature assumptions relative to the earth's origin," but quickly revised this estimate downward on the basis of chemical reasoning (Urey, 1953, p. 290). He suggested that the accumulation of the Earth must have started at temperatures below 100 °C. Much higher temperatures, such as those assumed in Eucken's (1944) theory, would be incompatible with the presence of iron sulfide and silicates mixed with the metallic iron phase in meteorites, since iron sulfide is unstable in the presence of cosmic proportions of hydrogen and iron above 600 K. Silicon dioxide and silicates are unstable at higher temperatures, yet both are present in meteorites. Although he expected that gravitational contraction of the growing Earth would have generated higher temperatures, these could not have been greater than about 1200 K without contradicting geological evidence (Urey, 1951, pp. 238, 244, 274, and 275). Consideration of the abundances of volatile elements at the Earth's surface made it "overwhelmingly obvious" that the high-temperature origin hypothesis was invalid (Urey, 1953 p. 286).

Urey thus assumed that the terrestrial planets accumulated at low temperatures from small solid planetesimals; they initially consisted of a grossly homogeneous mixture of silicate and iron phases. The iron would initially have been in an oxidized condition in the presence of cosmic proportions of water vapor; it was therefore necessary to postulate a later high-temperature stage during which the iron was reduced and partially fractionated from the silicates. At that time iron was thought to be much less abundant in the sun (even after removing hydrogen and helium) than in the Earth, so it was necessary to find some precess that could concentrate iron in the terrestrial planets. Urey went to considerable lengths in devising schemes to fractionate iron from silicates in a manner consistent with other processes needed to allow the planets to retain volatile compounds (Ringwood, 1966a, p. 46).

Having initially adopted Kuiper's giant protoplanet theory, Urey soon began to have doubts about that theory. It was difficult to understand how silicates could have evaporated to the extent necessary to explain the composition of the terrestrial planets, while still allowing them to retain some water, nitrogen, and carbon (Urey, 1954).

Two years later Urey abandoned the hypothesis that protoplanets (in the sense of large masses of gas and dust of solar composition) had been involved in the formation of the terrestrial planets. Instead he postulated that two sets of objects of asteroidal and lunar size, called "primary" and "secondary" objects, were accumulated and destroyed during the history of the solar system. The primary objects were suddenly heated to the melting point of silicates and iron, perhaps by explosions involving free radicals triggered by solar-particle radiation. After cooling for a few million years these primary objects "were broken into fragments of less than centimeter and millimeter sizes. The secondary objects accumulated from these about 4.3×10^9, and they were at least of asteroidal size. These objects were broken up . . . and the fragments are the meteorites" (Urey, 1956, p. 623). The reason for constructing this scheme was to explain the presence of diamonds (presumably formed only at very high pressures) in meteorites.

At the first "Symposium on the Exploration of Space" (April 1959), Urey suggested that "the moon may be one of these primary objects, as I realized after devising what seemed to me a reasonable model for the *grandparents* of meteorites (1959, p. 1727). As *The New York Times* headlined one of his speeches two years later, "Urey holds moon predated earth," and he also viewed the moon as likely to be one of the few relics of an early stage of the solar system (Sullivan, 1961). Urey could therefore prescribe a set of chemical and physical observations to be made from the moon's surface to give information not only about meteorites but also about the formation of the planets.

B. Ringwood's program

As Urey turned his attention increasingly to the moon, other scientists took up the challenge of reconstructing the chemical history of the early solar system. One was A. E. Ringwood (Fig. 6), an Australian geochemist and cosmogonist. Ringwood proposed to interpret the densities of Earth, Venus, and Mars as representing different redox states of primordial condensed material of chondritic or solar composition. Previous interpretations of the densities of these planets had assumed that iron/silicate ratios were a free parameter, and Urey had invoked complex processes in the solar nebula which fractionated iron from silicates prior to accretion. Ringwood did not consider such processes necessary because he rejected the supposed fact (subsequently disproved on other grounds) that meteorites are greatly enriched in iron compared to solar composition (see Sec. IX.C)

Like Urey, Ringwood (1960) assumed that the Earth formed by accretion of planetesimals in a cold gas-dust nebula, and that meteorites could provide clues to the nature of the primeval material. Carbonaceous compounds would initially have been mixed with nonvolatile oxides, silicates, and ices. The heat generated by accretion would raise the temperature high enough to allow carbon to reduce iron oxide to metallic iron; the Earth would then have melted enough to allow the denser iron to sink to the center. At the same time H_2O and CO_2 produced by the reduction reactions would have provided a dense atmosphere. This atmosphere would have absorbed solar radiation and further raised the temperature. But then

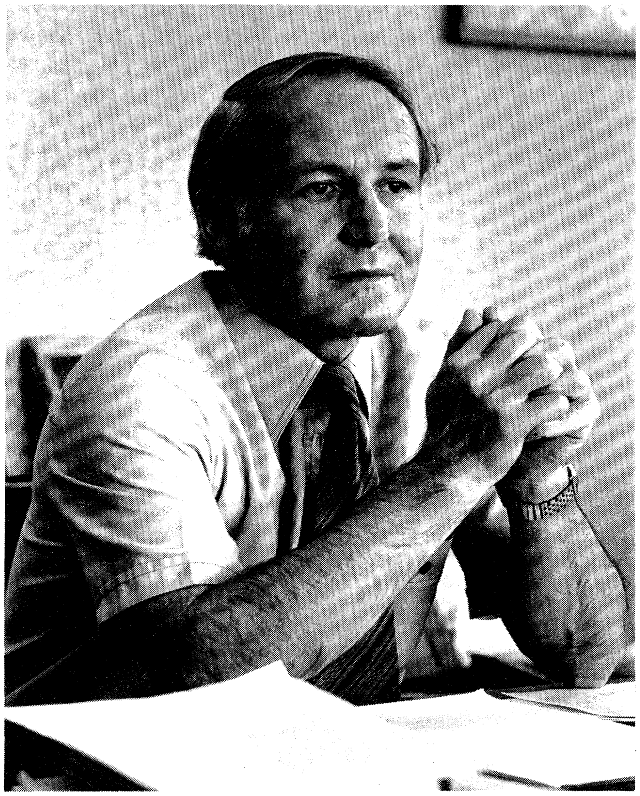

FIG. 6. A. E. Ringwood, Australian chemist (cosmochemistry, geochemistry, planetary and lunar formation).

one has to explain what happened to the atmosphere.

Ringwood suggested that the loss of the primeval atmosphere resulted from the catastrophic formation of the core, which suddenly reduced the total moment of inertia and generated additional heat. If the Earth's rotation were already fairly rapid, this additional rotational speed (to conserve angular momentum) could make it unstable against fission. Part of the mantle could have flown off and gone into orbit around Earth, becoming the moon; the primeval atmosphere would have been stripped off at the same time, and dissipated into space.

In the earlier fission hypothesis for the origin of the moon (G. H. Darwin, 1878, 1879) there was a difficulty in making the proto-Earth unstable because the present Earth-moon system does not have great enough angular momentum; Ringwood's theory allowed the missing angular momentum to be carried off from the system by the escaping atmosphere.

Carbonaceous chondrites, according to Ringwood (1962), are similar in chemical composition to the Earth. Unlike other meteorites, they have the same abundances of nonvolatile elements as the sun (with the possible exception of iron and copper), and those abundances are consistent with those calculated from nucleosynthetic models. They contain some iron which, after reduction and heating, could have constituted the core of the Earth. But they contain large amounts of volatile substances, suggesting that they have not undergone the kind of thermal evolution that other meteorites have experienced. Moreover, the fact that iron and nickel are found to be completely oxidized in these chondrites indicates that they have always been cold.

Ringwood's hypothesis that the primordial Earth-substance resembled carbonaceous chondrites, composed of low-temperature minerals, was threatened by the discovery that high-temperature minerals were replaced by low-temperature minerals in carbonaceous chondrites, hence the high-temperature minerals were earlier (DuFresne and Anders, 1961, 1962; Sztrokay *et al.*, 1961). Ringwood (1963) retreated somewhat from the position that carbonaceous chondrites are primordial, conceding that they must have been radioactively heated for a short time (not more than 10^8 yr), as suggested by Fish, Goles, and Anders (1960), but insisted that they are still "the nearest approach which we possess to primordial material."

In order to construct an Earth model from carbonaceous chondrites, Ringwood found that not only iron and nickel but another metallic component must be transferred from the mantle to the core. Since SiO_2 is the common oxide most easily reduced to metal after the oxides of iron and nickel, he proposed that the core contains some Si (Ringwood, 1966c, p. 296). Since Si is less dense than iron, this hypothesis was qualitatively consistent with the shock wave compression experiments indicating that pure iron is too dense to be the sole constituent of the core (Al'tshuler *et al.*, 1958).

Another geochemical influence was that the core was not in chemical equilibrium with the mantle when it formed, otherwise the iron would have removed essentially all of the Ni, Co, Cu, Au, and Pt from the mantle as it separated. This was another piece of evidence favoring a cold rather than hot origin for the Earth (Ringwood, 1966c, p. 317). But Ringwood also suggested, contrary to Urey, that the Earth was extensively or completely melted at some time in its early history (1966a, pp. 71 and 72).

Ringwood considered his own scheme for the evolution of the Earth to be much simpler than Urey's; the latter postulated a complex multistage process involving high-temperature processing of the material (e.g., in lunar-sized bodies) before it was assembled into the Earth, whereas Ringwood's did the job in a single step (Ringwood, 1966c, p. 329).

In keeping with the desired simplicity of his theory, Ringwood then abandoned his earlier hypothesis that the primeval material had been subjected to radioactive heating in the nebula, and with it the assumption that this material was similar to carbonaceous chondrites. Instead he postulated a higher proportion of hydrogen in the primordial material and gave a more important role to a primeval atmosphere, consisting primarily of H_2, CO, and H_2O, in reducing iron oxides.

C. High-temperature condensation

In the early 1960s several events encouraged cosmogonists to include a high-temperature stage in their scenarios for the formation of terrestrial planets. Astrophysical models proposed by Hoyle (1960a, 1960b), Hayashi (1961), Ezer and Cameron (1963, 1965), and others implied a superluminous phase for the early sun, perhaps the same phenomenon as the copious mass ejection observed in T Tauri stars (Herbig, 1962). Paul W. Gast (1960) found that alkali metals are depleted in the Earth's upper mantle as compared to chondrites, suggesting that some volatilization had occurred during the Earth's formation. John Wood (1958, 1962) proposed that chondrules are direct condensates from the solar nebula; they could have formed near the sun's surface, then have been pushed out by the process described in Hoyle's (1960) theory. The T Tauri stellar wind might provide the brief high-pressure surge needed to allow them to condense (Wood, 1963b, p. 165). Thus chondrules would be surviving planetesimals of the type from which the terrestrial planets condensed (Wood, 1963a).

Edward Anders became a leading advocate of the hot-origin hypothesis. He accepted Wood's proposal for the origin of chondrules (Anders, 1963, p. 102) and considered this an argument in favor of an early high-temperature phase for the solar nebula. He pointed out that, after Urey had proposed his cold-origin theory, new evidence indicated that many volatile elements are depleted in chondrites, implying a high-temperature process. But "no model involving a common, unitary history of chondritic matter can account for this abundance pattern. One is driven to the assumption that chondritic matter is a mixture of at least two kinds of material of widely different chemical histories" (Anders, 1964, pp. 5 and 6). One kind has been significantly more depleted than the other and must therefore have been separated at much higher temperatures.

Hans Suess (1963) recalled that the idea of direct condensation of chondrules from a gas phase had been popular 30 or 40 years earlier, but that Urey had persuaded him to abandon it in the 1950s. But now, with new evidence and the recognition of different kinds of chondrules, the idea could be revived. Contrary to B^2FH, who assumed that solar system material was a mixture of atoms from several sources, Suess (1964, 1965) argued that the solar nebula was quite homogeneous. "Among the very few assumptions which . . . can be considered well justified and firmly established, is the notion that the planetary objects . . . were formed from a well-mixed primordial nebula of chemically and isotopically uniform composition. At some time between the time of the formation of the elements and the beginning of condensation of the less volatile material, this nebula must have been in the state of a homogeneous gas mass of a temperature so high that no solids were present. Otherwise, variations in the isotopic composition of many elements would have to be anticipated" (Suess, 1965, p. 217).[44]

A pioneering calculation of the molecular equilibria and condensation in a solar nebula was carried out by Harry C. Lord (1965), with support and encouragement from Urey (cf. Urey, 1952a). Previous calculations had been limited to only a few major species or assumed conditions more appropriate to stellar envelopes. Lord considered 150 species in a gas with cosmic elemental abundances, at temperatures of 2000 K and 1700 K and total pressures of 1 atm and 5×10^{-4} atm. He found that Al_2O_3, W, ZrO_2, and $MgAl_2O_4$ were condensed at 2000 K and 1 atm pressure; nothing condensed at 2000 K and the lower pressure. At 1700 K many molecules condensed, including oxides of Ti, V, Ca, Mg, and Zr. (Silicates were not included because of the inadequacy of thermodynamic data for them.)

John Larimer, in Anders' group at the University of Chicago, generalized Lord's calculations to determine the temperatures at which a number of elements and compounds would condense, using pressures indicated by Cameron's (1962b, 1963d) models of the solar nebula. He attempted to trace the entire cooling history of a gas of cosmic composition in order to account for the fractionation patterns observed in meteorites (Larimer, 1967; Larimer and Anders 1967, 1970). In particular, Larimer used the same kind of data that Urey had earlier used to infer low-temperature formation, to support high-temperature formation. The elements Pb, Bi, In, and Tl, which are strongly depleted in chondrites, are among the last to condense.

Anders (1968) argued that evidence on the depletion of volatile elements, obtained by the precise techniques of neutron activation analysis, made it necessary to reverse Urey's conclusion that the Earth and meteorites had accreted at temperatures of about 300 K. Elements that are depleted by factors of 10 to 100 in ordinary chondrites, such as Hg, Tl, Pb, and Bi, often occur in nearly their "cosmic" abundances in carbonaceous and enstatite chondrites (Reed *et al.*, 1960). Anders concluded that the Earth and ordinary chondrites accreted at about 600 K.

The Anders group argued that their high-temperature condensation hypothesis was justified by Cameron's mod-

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[44] In his recent book Suess (1987) insists that the isotopic anomalies discovered in the 1970s constitute only a "minute fraction" of solar system material, while the rest "shows no measurable indications of incomplete mixing of genetic components. This can only be explained by assuming that both the *R*- and the *S*-components [as defined by B^2FH; see Sec. IV.A] were gaseous when the mixing occurred. At some time, a practically homogeneous gas mass must have existed with a temperature sufficiently high (higher than ca. 2000° K) and a total gas pressure sufficiently low that no condensed matter was present." But he admits that chondrules cannot be explained by direct condensation from such a gas (Suess, 1987, pp. 91–93).

els, which made it "virtually certain that the nebula passed through a stage of catastrophic collapse when temperatures rose to $\gg 2000$ K, causing complete vaporization of any preexisting solids" (Larimer and Anders, 1970, p. 367). Temperatures would be as high as 500 K, as far out as the asteroid belt (Anders, 1968, p. 296). Moreover, as Suess (1965) stated, and as Anders restates, "it is almost an axiom that the solar nebula was well mixed in an isotropic and elemental sense. Certainly no *isotopic* differences have yet been found that might be attributed to incomplete mixing of material with different nucleosynthetic histories . . . we are probably justified in assuming that the solar nebula once had completely uniform elemental composition" (Anders, 1971, pp. 4–5).

The attractive idea that meteorites are direct condensates from the primordial solar nebula was at first apparently refuted by the fact that the abundance of iron in the solar atmosphere was 5 to 10 times smaller than in meteorites (Goldberg *et al.*, 1960; Aller *et al.*, 1964; Urey, 1966, p. 210; Goles, 1969, pp. 121 and 122).[45] Several more or less plausible mechanisms to separate iron from silicates in the solar nebula were proposed (Taylor, 1965; Banerjee, 1967; Harris and Tozer, 1967; Tozer, 1968; Weidenschilling, 1978). Urey (1967) had concluded that probably no meteorite was accurately representative of the composition of the solar nebula. But in 1969 Garz and Kock found a systematic error in earlier determinations of the oscillator strengths of iron lines. As a result, the solar abundance had to be increased by an order of magnitude; the corrected value was now in good agreement with meteoritic abundances (Garz *et al.*, 1969; Pagel, 1973, p. 5; Ross and Aller, 1976).[46]

At the same time new evidence emerged for the hypothesis that some meteorites are early high-temperature condensates from the solar nebula. Shortly after the fall of the Allende meteorite in February 1969, Ursula Marvin, John Wood, and J.S. Dickey (1970) pointed out that its calcium-aluminum rich phases have the composition to be expected for early condensates according to Lord's (1965) calculations. This interpretation was supported by Lawrence Grossman (1973) and his colleagues (Grossman and Clark, 1973; Grossman and Larimer, 1974; Grossman and Olsen, 1974; Olsen and Grossman, 1974; Grossman, 1975; Ganapathy and Grossman, 1976; Grossman and Steele, 1976).

John Lewis (1972) extended the Larimer-Anders approach by using Cameron's (1969a) temperature-density-pressure profiles for the early solar nebula and the recent upward revision of the solar iron abundance. He showed that the model could explain the density trends in the inner solar system without invoking any special mechanism for iron/silicate fractionation. The high density of Mercury would follow from its condensation at temperatures so high that $MgSiO_3$ was only partially retained but Fe metal was condensed (see also the discussion of this point by Grossman, 1972); the densities of the other terrestrial planets would be accounted for by "different degrees of retention of S, O, and H as FeS, FeO, and hydrous silicates produced in chemical equilibrium between condensates and solar-composition gases." Lewis predicted that Earth's outer core is an Fe-FeS melt, that Venus has essentially no sulfur but a massive core of Fe-Ni alloy, and that Mars has virtually no free iron but may have a core of FeS. Only Earth has heavy alkali metals in its core, giving it a large internal heat source (from decay of ^{40}K) and a resulting magnetic field.

D. Inhomogeneous accumulation

Another version of the initially uniform, high-temperature hypothesis was proposed in 1969 by K.K. Turekian and S.P. Clark. Rather than assuming that the Earth was initially homogeneous and later evolved into its core-mantle-crust structure by a segregation process, they proposed that the present stratification directly reflects the sequence of condensation: iron condensed first and formed the core, then silicates condensed around it to form the mantle, and finally the volatile elements and gases were collected. Outer layers of the Earth would be more oxidized than inner ones because of the changing nature of the nebular gas during cooling, as hydrogen was expelled from the solar system. This scenario would avoid the problem of getting rid of immense quantities of CO and CO_2 resulting from the reduction of iron oxides by carbon, as in Ringwood's model.

The Turekian-Clark model, known as the "inhomogeneous accumulation" or "heterogeneous accretion" hypothesis, was based like the Larimer-Anders model on a condensation sequence starting with a low-pressure gas at 2000° K, but differed from it in one significant feature. Accretion was assumed to be rapid compared to cooling of the gas. As each element or compound condensed, it was assumed to be sequestered inside a solid body so that it would no longer react chemically with the remaining nebular gas. (This is an old idea, going back to Ampère, 1833.) The late-condensing material that formed the

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[45] Stuart Pottasch (1963) found a higher value, but his results were ignored, according to Ringwood (1974, p. 58). Ringwood (1966b, pp. 123–128) argued that uncertainties and discrepancies in abundance determinations were so large that there was no justification for the conclusion that iron is significantly less abundant in the sun than in meteorites; see later comments by Goles (1969, p. 127) and Ringwood and Anderson (1977).

[46] Recent data on the composition of the sun's corona and photosphere indicate that the Fe abundance should be raised another 40% (Breneman and Stone, 1985), suggesting that meteorites are unrepresentative of the solar nebula because they contain *too little* iron. Anderson (1989) has discussed the implications of this result for models of the Earth. But there are still discrepancies in solar iron abundances inferred from different spectral lines (Blackwell *et al.*, 1984) and a need for more accurate atomic data (Grevesse, 1984).

crust and upper mantle would never have been in contact with the core. This would explain, for example, the puzzle pointed out by Ringwood: the nickel content of basalts is much higher than would be expected if they had ever reached chemical equilibrium with an iron-nickel alloy; the absence of chemical equilibrium between core and mantle is hard to explain on Ringwood's hypothesis but easier in the inhomogeneous accumulation theory (Clark *et al.*, 1972, pp. 14 and 15). But the same feature prevents the inhomogeneous theory from explaining the presence in meteorites and the Earth of those minerals that were apparently formed by chemical reactions between gases and previously condensed compounds, such as troilite (FeS) (J. S. Lewis, 1974, pp. 54–56; Wood *et al.*, 1981, p. 645). It is also inconsistent with hypotheses that assume the Earth's core must contain sulfur or silicon in addition to iron (Goettel, 1976, p. 374; Wood *et al.*, 1981, p. 647).

During the 1970s there was considerable discussion of the merits of homogeneous versus inhomogeneous condensation (J.S. Lewis, 1973a, 1973b, 1981; Goettel, 1976; Kerridge, 1977, p. 63; Walker, 1977, p. 184; J. S. Lewis *et al.*, 1979; Ringwood, 1979; Wood, 1979; Murray *et al.*, 1981, pp. 9 and 10; Wood *et al.*, 1981, p. 647; Kuskov and Khitarov, 1982; J. V. Smith, 1982; Henderson-Sellers, 1983). Some authors questioned whether the assumption of thermodynamic equilibrium could legitimately be used to describe the condensation process, in view of the presence of nucleation barriers (Blander and Katz, 1967; Blander and Abdel-Gawad, 1969; Grossman and Larimer, 1974, p. 91; Donn, 1975; Goettel and Barshay, 1978) and the likelihood that solid particles are likely to be much cooler than a surrounding gas (Arrhenius and De, 1973; Tozer, 1978). But new developments in astrophysics threatened to make *all* these theories obsolete, by undermining the basic assumption that the terrestrial planets and meteorites were formed from material that had been completely vaporized when the solar system was formed.

E. Rejection of high-temperature condensation

The first challenge to the hot-origin postulate came from calculations of Richard Larson on the dynamics of a collapsing protostar. He found that "a star of one solar mass first appears on the Hayashi track with a much smaller radius and luminosity than the very large values which have commonly been assumed," and for more massive stars there is no Hayashi phase at all (Larson, 1969, p. 287, 1972b, 1988), Thus the sun may have been formed without reaching very high temperatures until after the planets had been accumulated. During the last decade the role of a hypothetically hyperactive early sun in planetogony has been considerably diminished (Kaula, 1986, p. 26). Cameron, whose earlier models had provided much of the justification for high-temperature condensation models, announced in 1973 that his latest calculations with M. R. Pine indicated that "the temperature will not rise high enough to evaporate completely the interstellar grains, contained within the gas, beyond about one or two astronomical units" (Cameron, 1973c, p. 545). Thus it is possible that some of the meteorites in our museums are interstellar grains that survived the formation of the solar system without being vaporized.

Cameron then abandoned the Cameron-Pine model (Sec. V.B) and adopted a new model in which the sun was formed not at the beginning of the accretion period but throughout that period; the temperature in the region of planet formation would be only "a few hundred degrees" (Cameron, 1975a, p. 37). Subsequently he stated this conclusion more sharply: "At no time, anywhere in the solar nebula, anywhere outwards from the orbit of formation of Mercury, is the temperature in the unperturbed solar nebula ever high enough to evaporate completely the solid materials contained in interstellar grains. For some time a number of people have argued that the entire solar nebula started out at a high temperature and cooled while solids underwent a sequence of condensation processes. In fact, there was no available energy source for any such high temperatures to have been initially present" (Cameron, 1978c). The reason why the temperatures are low is that "During the collapse of the interstellar cloud fragment, the energy released by the compression of the gas is readily radiated away, and most of the collapse of the gas cloud occurs with interior temperatures which are likely to be close to 10 K. When the material falls onto and merges with the primitive solar accretion disk, there is plenty of time for the infall energy of accretion to be radiated away into space .... The temperature in the disk can ... be increased only if the disk contains much more mass or if the viscous dissipation per unit mass is increased," but the highest reasonable values of those parameters have already been assumed, and indeed the estimated temperatures are more likely to be too high than too low (Cameron, 1978a, p. 469, see also 1979b, p. 998).

A conflict thus developed between astrophysics and meteoritics. In the words of meteoriticist John Wasson, "At the present time most numerical models of cloud collapse yield the result that temperatures were never above about 1000 K[at distances] ≥ 1 AU from the axis of the forming solar system. In contrast, most meteorite researchers hold that higher temperatures were necessary to account for a variety of elementary fractionations found between groups of meteorites, between members of a single group, and between components of a single meteorite" (Wasson, 1978, p. 489). Wasson argued that simple aggregation of interstellar grains could not have produced the observed range of properties of chondrites. He concluded that meteoritic evidence required maximum nebular temperatures greater than 1500 K in the region from 1 to 3 AU, and insisted that "satisfactory astrophysical models for the formation of the solar system must be able to generate" such high temperatures (Wasson, 1978, p. 501).

Wasson continued to defend the high-temperature hypothesis throughout the time period covered by this review, suggesting that astrophysicists should be willing to modify their models in order to agree with meteoritic evidence rather than expecting meteoriticists to look for ways to produce high-temperature assemblages in a low-temperature nebula (1985, pp. 156 and 184). Arnold (1980) also maintained that solar system material was completely mixed at high temperatures, despite the view of astrophysicists.

John Wood, who works in the same institution as Cameron, pointed out several times that meteoriticists are basing their theories on models that Cameron himself proposed but has now rejected (Wood, 1979, p. 161; Wood and Motylewski, 1979, pp. 913 and 914). Yet there is still strong evidence from the Ca-Al rich inclusions in Allende and from other meteorites that material was condensed from hot gases in the early solar system (Wood and Motylewski, 1979, p. 914; Wood and Morfill, 1988, p. 342). To resolve the conflict he suggested that chondrites were produced in regions of localized transitory heating of the nebula (Wood, 1979, p. 165). For example, the infalling interstellar material might have been heated on passing through a standing shock wave as it entered the nebula (Wood, 1982).

In the late 1970s and early 1980s, most meteorite researchers concluded that meteorites did *not* provide strong evidence that the solar nebula was hot throughout. Insofar as meteorites appeared to have been formed at high temperatures, other explanations such as local heating events might be found (J. V. Smith, 1979, p. 11; Anders, 1989). Direct condensation did not seem to provide a satisfactory account for refractory inclusions in Allende or for chondrules in general (Boynton, 1975; Kurat *et al.*, 1975; Kerridge, 1977, p. 48, 1979; Herndon, 1978; J. S. Lewis *et al.*, 1979; Gooding *et al.*, 1980; Leitch and Smith, 1981; Wood, 1981; MacPherson and Grossman, 1981; MacPherson, Grossman, *et al.*, 1984; MacPherson, Paque, *et al.*, 1984; McSween, 1987, p. 58). According to R. Clayton, Mayeda, and Molini-Velsko (1985, p. 765), existing data on calcium-aluminum-rich inclusions "*are totally incompatible with a simple history of a single stage of condensation during monotonic cooling from an initially hot gas, the first-order framework on which many cosmochemical models have been built*" (italics in original).

The discovery of isotopic anomalies in meteorites (Sec. VI.A) also encouraged scientists to abandon the hot nebula hypothesis, since that hypothesis as formulated earlier by Suess (1965, p. 217) and Anders (1971, p. 4) implied that the nebula material was well mixed. The easiest way to account for the anomalies was to assume that presolar grains had survived without being vaporized (J. V. Smith, 1979, p. 1; Wood, 1981, p. 35).

Donald Clayton was one of the strongest critics of the hot nebula hypothesis and an advocate of the view that surviving presolar grains carry a "cosmic chemical memory" (D. Clayton, 1981b, p. 1782, 1982, pp. 174 and 192) that may provide the key to the origin of the solar system. He argued in a series of papers that the concept of "high-temperature thermal condensation in the early solar system," which meteoriticists had come to accept as an established fact, should be completely abandoned (D. Clayton, 1978a, p. 110, see also 1979a, p. 156, 1980a, p. 1477, 1980b, p. L37). He complained that his own hypotheses had been treated "rudely" by the cosmochemical community (D. Clayton, 1979, p. 168), but by the early 1980s was able to claim widespread support for his views.

The measurement of rare gas isotopes in the atmosphere of Venus by the American *Pioneer* Venus probe and the Soviet *Venera* lander mission, in December 1978, yielded additional evidence against high-temperature condensation. It was found that the abundances of ^{36}Ar and ^{38}Ar are relatively about 100 times greater on Venus than on Earth (Hoffman *et al.*, 1980; Blamont, 1982, p. 741). This is just the opposite of what would be expected from the equilibrium condensation theory if Venus had been formed at a higher temperature than Earth, unless one invoked additional hypotheses such as a strong pressure gradient in the nebula or some process to incorporate volatiles into the planetesimals accreted by planets late in their formation (Pollack and Black, 1979), or implantation of argon isotopes by the solar wind (Wetherill, 1980b, p. 1239, 1981b, p. 70). Attempts to explain the pattern of rare gas abundances in terrestrial planet atmospheres continued in the 1980s, but with little credence being given to high-temperature condensation theories (Donahue and Pollack, 1983).

Although much of this evidence is equally damaging to all high-temperature condensation theories, it is easier to show that it is inconsistent with the Lewis theory because that theory made more specific statements than others about the properties of the terrestrial planets. Lewis himself placed considerable emphasis on testing theories against observational data. He wrote

> Remarkably, it seems that theoreticians have devoted very little of their time to comparing the results of their modeling to the present observational data on the solar system. Faced with the exciting possibilities inherent in designing one's own solar system, many authors have lacked the self-discipline to see to it that their creative art emulates nature. (There is often the heady implication that theory is its own excuse, and nature can fend for itself.) The theorist's art might very well apply to some undiscovered solar system, but what we are really interested in right now is *ours*. There is an enormous wealth of data on the Earth, the Moon, meteorites, the terrestrial planets, and, in the near future, the outer planets and their satellites, all of which will require assimilation. There will be ever-increasing pressure on the theoreticians to make their models bear some resemblance to the available observational data. (J. S. Lewis, 1973c, p.34)

One could argue that even though the original nebular model on which Lewis based his calculations was withdrawn by its inventor, Cameron, and replaced by another one that excluded the possibility of temperatures high enough to vaporize interstellar dust in the region of the terrestrial planets, the Lewis condensation model still should be tested against observational data. In view of its initial success in explaining the densities of the terrestrial planets in terms of the specific substances expected to condense at the temperatures and pressures corresponding to the positions of those planets in the solar nebula, it deserves to be judged by its success in predicting other planetary properties (Fanale, 1976, p. 180; Pollack *et al.*, 1976, p. 36, 1979, p. 500; Solomon, 1976, p. 511; Lange and Ahrens, 1982, p. 107). (Recall that van der Waals' equation of state gives a reasonable first approximation to the properties of gases and liquids, over a much greater region of densities and temperatures than those in which the molecular assumptions from which it was derived are valid; a theory can be useful even if it is based on erroneous principles.)

In this connection it is interesting to look at one particular problem: the difference between Venus and Earth with respect to sulfur content. Before Lewis developed his equilibrium condensation theory, he had predicted that sulfur compounds should be found in the Venusian atmosphere (J. S. Lewis, 1968, pp. 437 and 454), although he estimated that the dominant cloud-forming species were compounds of mercury (J. S. Lewis, 1969). But his condensation calculations led him to state that Venus had virtually no sulfur (1972, p. 288), and this together with the absence of FeO was what made its density slightly lower than that of the Earth. The validity of this explanation of the Venus-Earth density difference was disputed by Ringwood and Anderson (1977; see also Zharkov, 1983, p. 140) and defended by Goettel, Shields, and Decker (1981; see also Phillips and Malin, 1983, pp. 161 and 167). But the discovery of large quantities of sulfur compounds in the atmosphere of Venus (Sill, 1972; A. T. Young, 1973, crediting L. D. G. Young; Cruickshank, 1983, p. 5), shortly after Lewis had stated that Venus has no sulfur, looked like a direct refutation of his theory (Ringwood and Anderson, 1977, p. 249; McGetchin and Smyth, 1978, p. 514).[47] Lewis could still point to his earlier statements about sulfur compounds in the Venusian atmosphere as having been confirmed, yet, as he pointed out, these statements were based on an assumption of Earthlike composition, which was superseded by his later model (Lewis and Kreimendahl, 1980, pp. 332 and 336).

In their 1984 monograph, Lewis and Prinn continued to use the equilibrium condensation model. They claimed that the peak temperatures and pressure profiles calculated from Cameron's more recent (1978b) model in the region of condensation of preplanetary solids are similar to those in his earlier model; they admitted that "it is by no means sure or even likely that solid solar system materials were fully vaporized in the nebular phase," but suggested that their "condensation temperatures" could be interpreted as "the *highest* temperature at which gases and solids were intimately mixed" (Lewis and Prinn, 1984, pp. 9, 59, and 67). These statements acknowledge the fact that the planetary science community has largely abandoned the basic principles on which the Lewis model was originally based; perhaps the best justification for retaining it is to have a *simple* model that explains *some* features of the planetary system, as a basis for comparison with more realistic models (cf. McSween, 1989, p. 151). And of course one may always hope that astrophysical fancy will once again favor high-temperature models (Cameron, 1984a, 1985c; Boss, 1988b).

X. SELENOGONY BEFORE THE LUNAR LANDINGS

A. Darwin's fission theory

The chemical approach was especially successful in the development of ideas about the origin of the moon—selenogony—after *Apollo*. Chemical analysis of lunar rocks led, after some initial confusion, to the conclusion that the moon's similarities to the Earth outweigh its differences. Although this conclusion did not by itself point uniquely to one theory, it did play a major role in eliminating alternatives and encouraging theorists to look for plausible ways to extract the moon from the Earth.

Modern theories of the origin of the moon go back to 1878 (see Brush, 1986, for further details). In that year George Howard Darwin, son of the evolutionist Charles Darwin, proposed that the moon had once been part of the Earth and was ejected from it by an instability triggered by the action of the sun's tidal force. The additional hypothesis that the scar left by the moon's departure become the Pacific Ocean was proposed by Osmond Fisher (1882). (An earlier theory imagined that the moon was ejected from the Mediterranean basin; see Owen, 1857, p. 66.)

Darwin's theory was not based on any direct evidence that the moon was once part of the Earth, but rather on an interpretation of the observed "secular acceleration" of the moon. In the 18th century astronomers thought that the moon was gradually moving faster in its orbit around the Earth. That would imply (by Kepler's Third Law) that it was approaching the Earth. But in the 19th century quantitative analysis of the gravitational actions of the other planets on the Earth and moon indicated that the moon was actually moving more slowly than in the past, and that the apparent acceleration was due to a slowing-down of the Earth's rotation. The physical cause was identified as dissipation by lunar tides in the Earth's oceans. Darwin pointed out that since the angular momentum of the Earth-moon system is conserved, the angular momentum lost by the Earth must be transferred to the moon. As a result the moon's orbit is gradually receding from the Earth; conversely it must have been

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[47] Lewis (1974) suggested that the sulfur could have been added later by comets and meteorites; Ringwood and Anderson called this "*ad hoc.*"

closer in the past. Making specific assumptions about the mechanical properties of the Earth, he traced the lunar orbit back to a state in which the moon moved around the Earth as if rigidly fixed to it, in a period of 5 hours 36 minutes, with its center about 600 miles from the Earth's surface. Before that state there was no unique solution.

The major objection to the hypothesis that the moon was ejected from the Earth was dynamical: a body with the combined mass and angular momentum of Earth and moon, rotating in about five hours, would not be unstable against spontaneous fission. Darwin was aware of this objection but proposed to circumvent it by invoking a resonance of the sun's tidal action with the free oscillations of the proto-Earth.

Another objection was that the moon could not go into orbit as a single body because it would initially be inside the Roche limit and would therefore be broken up into many smaller bodies by the Earth's tidal force. Darwin argued that this flock of small bodies would still produce tidal dissipation, which would expand its orbit out beyond the Roche limit, so it could eventually recombine into a single satellite.

Roche himself was the major proponent of the hypothesis that the moon was condensed from a ring spun off by the rotating gaseous proto-Earth, just as the Earth was condensed from a ring spun off by the solar nebula in the nebular hypothesis (Roche, 1873). This became known as the coaccretion or "sister" hypothesis.

A third hypothesis, advocated by T.J.J. See (1909) and others, created the moon in some other part of the solar system and later brought it to be captured by the Earth; it was thus Earth's "wife."

Darwin's hypothesis, which described the moon as Earth's "daughter," remained the most popular until 1930, when Harold Jeffreys criticized it, arguing that viscosity in the Earth's mantle would damp out the motions required to build up the postulated resonant vibration.

B. Capture

In the 1950s the capture hypothesis was revived by Harold Urey in the United States and Horst Gerstenkorn in Germany. (This section and the next one summarize detailed accounts published elsewhere: see Brush, 1982a, 1988.) Urey's theory was developed as part of a general chemical theory of the origin of the Earth, the other planets, and meteorites, described in Sec. IX.A. Gerstenkorn (1955, 1969) worked out a quantitative dynamical theory of the capture process, following the approach of G. H. Darwin but with different assumptions about the initial state and the mechanical properties of Earth and moon.

Urey's theory (1960b, 1962a, 1962b, 1967) was largely qualitative; he was less interested in the dynamics of the capture process than in the nature of the moon itself as a key to the early history of the solar system. He argued that the moon was a frozen relic, a surviving example of bodies that used to populate the solar system. For example, the abundance of iron in the moon was much less than that in the Earth (as inferred from the densities of the two bodies) but comparable to that in the sun (according to solar measurement before 1969). It had always been cold since its capture and thus preserved on its face a record of events that left no trace on the surface of the geologically active Earth. This was a powerful argument for manned exploration of the moon: analysis of the lunar surface not only should be able to tell us the conditions prevailing at the time and place of the moon's formation, but might reveal facts about the Earth's history that could not be learned by studying the Earth itself. If, on the other hand, Darwin were right and the Moon were just a piece of the Earth, it would not be worth the trouble to go there.

Gerstenkorn's theory raised different kinds of questions. What is the range of initial conditions for which capture is dynamically possible? Could the moon have been captured from a retrograde orbit? How could the lunar orbit have acquired its present eccentricity and inclination? How much energy had to be dissipated during the capture process, and would this energy have been enough to melt the Earth or at least produce some effects that could be detected today? Extensive calculations by MacDonald, Goldreich, Singer, and others in the 1960s indicated that while capture of the moon was not dynamically impossible, it would be extremely difficult to satisfy all the conditions necessary to produce the present lunar orbit.

It might appear that the only way to test the capture theory would be by mathematics: to see if the known astronomical facts about the moon could be deduced from a plausible initial state. The larger the set of possible initial states that could be shown to lead to the given final state, the more likely that the hypothesis is correct.

From Urey's point of view this kind of test is irrelevant. Even if the probability that any given moon-sized body would be captured by the Earth were very small, there were so many such bodies in the early solar system that there was a reasonable chance of capturing any of them. In any case such calculations cannot be used to compare capture with other hypotheses since different kinds of adjustable parameters are involved in those hypotheses (viscosity of proto-Earth, conditions in the primeval nebula, etc.). Instead, the real test must be chemical: if the moon is unlike the Earth it must have been formed elsewhere; if it is like the Earth it was at least formed in the same part of the nebula (coaccretion), if not actually inside the Earth (fission).

C. Are Earth and moon chemically similar?

To say that the moon is "like the Earth" does not mean it has the same chemical composition throughout. Cosmochemists were of course aware that the average density of the moon is significantly lower than that of the

Earth. This was generally explained by assuming that the Earth had a substantial high-density iron core, whereas the moon had little or no core. The fission theory would predict that the moon should be similar to Earth's mantle, if fission occurred after the Earth's core had already been formed. The coaccretion theory needs some kind of fractionation mechanism to get rid of the iron from the material that would form the moon. Such a mechanism was suggested by Orowan (1969): iron particles would stick together because of plastic deformation when they collided, whereas silicates would be brittle and break up in collisions. Thus the Earth would collect the iron in its region, leaving a shell of silicate particles around it to aggregate into the moon. [The possibility that iron particles would adhere by magnetic forces was considered by Harris and Tozer (1967) but rejected by Banerjee (1967).]

Another way to preserve lunar-terrestrial similarity would be to adopt the hypothesis of Lodochnikov (1939) and Ramsey (1948, 1949) that the Earth's core was *not* iron but a silicate compound that had undergone a phase transition to a high-density fluid metallic state (Sec. VII.A). If this hypothesis were adopted, one might suppose that the greater average density of the Earth was due merely to its greater total bulk and consequent higher initial pressure; the pressure inside the moon would be insufficient to produce the phase transition. The coaccretion theory would then be able to get along without any mechanism for separating iron from silicates (Ruskol, 1966, p. 225). But the Lodochnikov-Ramsey hypothesis was disproved by experiments and theoretical calculations in the 1960s, so this alternative was eliminated. This was not, of course, a serious problem before 1969, since there was no direct evidence for lunar-terrestrial similarity. Nevertheless there were hardly any advocates of coaccretion except in the U.S.S.R., where Evgenia Ruskol took the lead in developing this theory (1962, 1963a, 1963b, 1975).

A few scientists revived the fission hypothesis in the 1960s, in some cases because it was congruent with certain theories about the early development of the Earth. Thus Ringwood (1960, 1966a) and Cameron (1963c) used the ejection of the moon to get rid of Earth's primeval atmosphere. Wise (1963, 1966) argued that the traditional objections to fission had been weakened by recent developments, while Soviet photographs suggesting that the far side of the moon differed from the near side provided new evidence in favor of the hypothesis. John A. O'Keefe (1963, p. 56) argued that if (as he believed) tektites come from the moon, then the moon came from the Earth; he emphasized the idea that the fission process involved high temperatures and resulting loss of a substantial amount of volatile substances from the moon (O'Keefe, 1963, pp. 56 and 57, 1966, 1969a, 1969b).

Both capture and fission hypotheses, being based on the evolution of the lunar orbit through tidal dissipation, ran into difficulties because the time scale for this evolution was estimated to be only one or two billion years. Since the age of the Earth had been determined to be about 4.5 billion years and the oldest rocks were about 3.5 billion years, the question arose: why is there no sign of such a catastrophic event as the ejection or capture of the moon in the geological record? If one accepted Hartmann's (1965) estimate of 3.6 billion years for the age of the lunar maria, this time scale would rule out the fission theory and present the capture theory with the problem of "storing" the moon outside the Earth's zone for more than a billion years after its formation. But the time-scale estimate was quite dubious; it depended on the assumption that the dissipative force coefficient had remained constant through the entire period of evolution. Since dissipation was thought to take place primarily in shallow seas, it would depend on the arrangement of land masses on the Earth's surface; with the acceptance of plate tectonics in the 1960s that became a highly variable quantity. In the 1980s the time-scale problem is no more an objection to capture and fission than it is to any other modern theory.

Thus it was quite difficult to find any conclusive test of selenogonies, before the return of the first lunar samples. On the other hand, few theorists actually published specific predictions about what would be found in those samples. I have found only four: Urey's discussion based on his capture theory, and Ringwood's, O'Keefe's, and Wise's based on fission hypotheses. Urey expected to find evidence of water on or near the surface. O'Keefe argued that the moon would be poorer than Earth in water and other volatile substances and would also be deficient in siderophiule elements such as nickel. (Roughly speaking these predictions reflected the consequences of a cold or hot origin, respectively.) Wise predicted that the near side of the moon should have the same composition as the Earth's mantle, while the far side should have a less dense proto-Earth's crust. Ringwood predicted a thermal history in which a temperature maximum started near the surface and gradually moved toward the center, possibly exceeding the melting point for a brief period about 10^9 years after formation. This implies loss of volatiles from the crust but not from the deep interior, and in fact the density should decrease with depth (Ringwood, 1966a, p. 90).

XI. SELENOGONY: *APOLLO'S* IMPACT

A. All theories are refuted by the data

In July 1969 the *Apollo 11* mission brought back the first lunar samples from Mare Tranquillitatis. Preliminary analysis of these samples indicated a high concentration of refractory elements (Ti, Zr, etc.); low concentration of volatiles (Pb, Bi, Tl); strong depletion of siderophile elements, especially Ni and Co; and an absence of hydrated minerals, showing a scarcity of surface water (Lunar Sample Preliminary Examination Team, 1969).[48]

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[48] For further details and recollection of the team leader, S. R. Taylor, see Brush (1988) where detailed references for this section may be found.

The Anders group at Chicago quickly concluded that these results, together with a strong depletion of Au and Ag, provided good evidence against the fission theory (Ganapathy *et al.*, 1970, p. 1133; Anders *et al.*, 1971, p. 1026). Ringwood and Essene (1970) argued that the scarcity of volatile metals, siderophiles, and water did rule out the original fission hypothesis but not the high-temperature version of Ringwood (1960, 1966a) and O'Keefe (1969a, 1969b).

Urey's capture theory seemed to be refuted by the *Apollo* data, in particular by the scarcity of water and the evidence for an early high-temperature stage. Moreover, new measurements of the solar iron abundance (Garz *et al.*, 1969) showed that it was greater than previously believed, thereby removing one of Urey's arguments that the moon was more like primordial solar system material than the Earth. After extensive discussions with O'Keefe, Urey decided to abandon his capture theory and eventually leaned toward fission, though he was not very enthusiastic about that or any other theory (Brush, 1982a). Other versions of the capture theory that had relied on a time scale of one to two billion years for evolution of the lunar orbit seemed to be refuted by evidence that the moon was more than 4.5 billion years old and had not undergone any significant heating or other catastrophic event more recently than 3.5 billion years ago.

Although it was frequently stated during the 1970s that the fission theory had been refuted, cosmochemists were finding increasing similarities between lunar and terrestrial composition. The early conclusions about excess refractory abundance and depletion of siderophiles were later judged to have been somewhat exaggerated. Moreover, oxygen isotope abundances were found to be the same in lunar and terrestrial material.

The major opposition to lunar-terrestrial similarity came from the Anders group, which favored coaccretion after fractionation in the solar nebula; they argued that the moon was formed in a circumterrestrial orbit from material that had condensed at higher temperatures than the Earth (Ganapathy and Anders, 1974). Another version of the coaccretion theory, developed by Ruskol (1971a, 1971b, 1972) attributed compositional differences to processing of incoming planetesimals by collisions with the circumterrestrial swarm over a long period of time (10^8 yrs); volatiles would be removed from the outer edge by the solar wind, silicates would be broken up and remain in the swarm, while iron would pass through and be accreted by the growing Earth. Elaborations of this model were proposed by Harris and Kaula (1975).

In the late 1970s and early 1980s, the consensus of the lunar science community was that none of the three pre-*Apollo* theories offered a convincing explanation of the origin of the moon:

Fission. In addition to the original angular momentum difficulties of this theory, new calculations on viscous rotating fluids indicated that they could not be spun fast enough to cause fission; instead they simply lost matter from equatorial regions. Rotational instability could be produced by planetesimal accretion only if one planetesimal were about one-tenth of the mass of the proto-Earth, in which case the fission model would go over to the impact model. Fission models were deemed incapable of explaining why the moon is substantially richer in both iron and refractory elements than the Earth's mantle.

Capture. This hypothesis lost its original advantage of being able to explain Earth-moon compositional differences when it was shown that capture was dynamically impossible unless the moon were formed at about the same heliocentric distance as the Earth, and even then it would be rather unlikely. Disintegrative capture was also unlikely. On the other hand, even if the Earth could have captured a moon formed far away from the Earth (in order to account for chemical differences), one would then have difficulty accounting for the *similarity* of oxygen isotope composition.

Coaccretion had difficulty in explaining the composition differences between Earth and moon, even with a postulated "composition filter" to separate iron from silicates; moreover, it could not account for the angular momentum of the Earth-moon system.

Selenogony seemed to have reached an impasse. Other areas of planetary science were also slowing down. Cutbacks in funding for space science, especially in the U.S., made it difficult to acquire new data except from the *Voyager* missions to the giant planets, which began to occupy the attention of planetary scientists in the early 1980s.

B. Giant impact is proposed

What happened next—the emergence of the giant-impact hypothesis—bears a superficial resemblance to a Kuhnian revolution. Selenogony before 1969 had been dominated not by a single theory but by a paradigm: the evolutionary cosmogony exemplified by the 19th-century nebular hypothesis, supplemented by relevant results of physics, chemistry, astronomy, and geology. Within this paradigm, cosmogonic processes had to be deterministic and uniformitarian, even if their net result was the formation of a qualitatively new system. Thus fission, a catastrophic event, could occur only when certain physical conditions were present, and its result was predetermined. Two-body interactions, as in the capture theory, or the earlier tidal theory of the origin of the solar system, should be treated as deterministically as possible; actual collisions or extremely improbable initial states should be avoided. Mainstream cosmogonists were unwilling to postulate random catastrophic events, for reasons that may be called philosophical.

Thus, when the accepted paradigm was afflicted with insuperable difficulties, so that the very existence of the moon became an "anomaly" in the Kuhnian sense, the constraints of the old paradigm were discarded and the first steps were taken toward a new one. The new hypothesis, which is not yet a fully developed theory, suddenly attracted the enthusiasm of many scientists in what even its proponents describe as a "bandwagon" effect

(Stevenson, 1987, p. 271). Since the hypothesis explicitly invokes a random catastrophe, it is difficult to show that it is objectively superior to theories that exclude such catastrophes on philosophical grounds; if the criteria for testing hypotheses change, the paradigms are at least partially incommensurable (Kuhn, 1970). This is not to say that the new criteria are less strict; on the contrary, because of the availability of better computers, proponents of any hypothesis are now expected to demonstrate quantitatively that their mechanism will actually work with reasonable physical assumptions, where previously one could get away with qualitative arguments.

I used the phase "superficial resemblance" to warn the reader that the Kuhnian revolution is only an abstract historiographic model. One cannot expect to find a real historical event that is accurately described by the model, any more than one can expect to find a perfectly rigid sphere in nature. Moreover, most historians and philosophers of science insist that Kuhnian revolutions have (or should have, respectively) nothing to do with how science works. Nevertheless, many earth scientists affirm that the establishment of plate tectonics in the 1960s was a Kuhnian revolution, and the issue will inevitably arise whenever any radical change in accepted theories occurs. It is therefore worthwhile to point out some Kuhnian and non-Kuhnian aspects of the rise of the giant impact hypothesis.

The most obvious non-Kuhnian feature is that all discussions and calculations on the giant-impact hypothesis employ the same established principles of physics that were used to develop the previous theories, and the major dynamical problem that the new hypothesis was designed to solve is precisely the one that was considered of paramount importance in traditional cosmogony. As Howard Baker pointed out more than 30 years ago, "that the Moon was forcibly separated from the Earth by some extraneous force is indicated by its excess angular momentum about the Earth," and this force must have been exerted by a close gravitational encounter, if not an actual collision, with some large heavenly body (Baker, 1954, pp. 12 and 16).

But Howard Baker's hypothesis, published as a pamphlet by the Detroit Academy of Sciences, was completely ignored by the scientific community; as far as I can determine it was unknown to mainstream selenogonists in the 1970s. Aside from the fact that *most* scientific papers, even those published in respectable journals, are never cited by anyone except their authors (Menard, 1971, pp. 96–103), one may attribute the neglect of this work to the general dislike of scientists in the 1950s for catastrophic theories of solar system history, as shown by their reaction to Immanuel Velikovsky's books. (There were many objective reasons for rejecting Velikovsky, but the emotional tone of the criticism indicates that the argument was partly on a metascientific level; see Bauer, 1985.) The dominant paradigm defined such theories as unscientific.

By 1973 the situation had changed enough for another Baker to win serious consideration, though not actual publication, of a giant-impact hypothesis.[49] James Baker, a consultant to the Aerospace Corporation, proposed that Mars and Earth, their orbits perturbed by a massive proto-Jupiter, suffered a grazing collision. Calculations carried out by B. E. Baxter at Aerospace based on Baker's hypothesis indicated that fragments of Mars could have been captured into orbit around the Earth; Baker suggested that this material eventually formed the Moon.

James Baker's hypothesis was praised by D. H. Menzel and mentioned in at least two papers by other scientists, but was rejected for publication in July 1974 and never became part of the recognized literature. It was just on the borderline between "crackpot" and "respectable," and contained several features that would even today be considered unacceptable. Nevertheless, I suspect that if Baker had been a well-known planetary scientist, or if he had marshaled the existing evidence to support his ideas more effectively, he might now be recognized as the inventor of the giant-impact hypothesis. (It is not my function as a historian to give him that title, and experts who have recently looked at his theory deline to do so.)

Before the epoch of planetary exploration, a giant impact on the Earth might have seemed unlikely. But Mercury's cratered surface, revealed by *Mariner 10* in 1974, suggested that the terrestrial planets were bombarded by somewhat smaller bodies for hundreds of millions of years after their formation (Sec. VII.B). This made it much more plausible than before that the Earth could have been struck by an object large enough to eject a substantial amount of material from its mantle.

In August 1974 William K. Hartmann presented to an IAU Colloquium at Cornell the hypothesis that the moon was formed from material ejected into a circumterrestrial disk by a large (>1000-km radius) body that struck the Earth. Cameron, in the audience, remarked that he had been working on a similar hypothesis with an even larger impacting body, comparable in size to Mars. Hartmann worked with D. R. Davis to develop a theory published in 1975, while Cameron collaborated with W. R. Ward to obtain results that they summarized in a three-page abstract published in 1976. Both theories (unlike that of James Baker) were directly related to mainstream planetary cosmogonies and were sponsored by scientists with established reputations.

Hartmann (Fig. 7) had long been interested in lunar craters and the time-variation of their size distribution. During the 1960s he had also been impressed by Safronov's papers on planetary formation by accretion of solid planetesimals, including the hypothesis that the tilt

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[49] Acceptance of the meteoritic in place of the volcanic hypothesis for the origin of lunar and terrestrial craters helped to foster the recognition of impact as a widespread process (Hoyt, 1987). By the 1980s, scientists were willing to give serious consideration to the suggestion that large impacts could be an important component of geological and biological history.

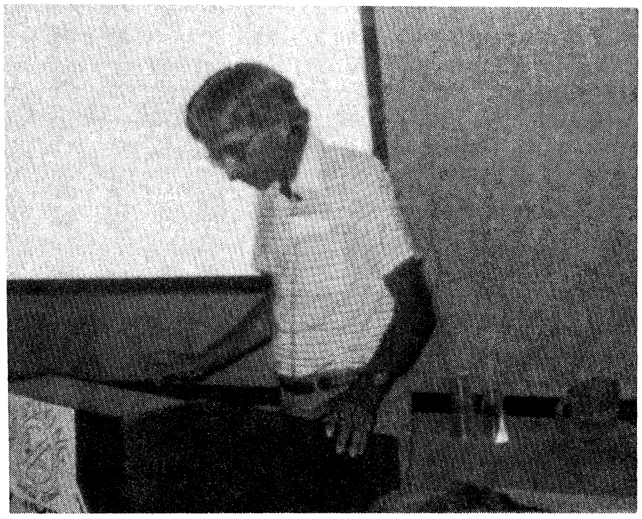

FIG. 7. William K. Hartmann, American astronomer (solar system, planetary and lunar formation).

of the Earth's rotation axis ("obliquity of the ecliptic" in astronomical terminology) was primarily due to the impact of the last large body that was added during its accretion. Moreover, measurement of the flux of impacting bodies as a function of time showed that the pre-mare cratering rate was enormous and included basin-forming bodies much bigger than the crater formers in the last few billion years (Hartmann, 1966). Those were debris left after planetary formation. Extrapolation back to an earlier epoch suggested that bodies as large as the moon itself could plausibly have been moving in the vicinity of the Earth.

Hartmann and Davis attempted a numerical reconstruction of the size distribution of bodies that could have grown during planetary accretion and that were left behind near the end of planet formation, assuming a process starting with accretion of small particles. They found that "the probability of the planet interacting with a large body is much larger than has been considered in some past descriptions of planetary growth" (Hartmann and Davis, 1975, p. 511). For certain assumptions, they found that among Earth-sized planets the second-largest bodies could be of radius 500–3000 km, and there could be tens of bodies larger than 100 km radius. Half of the kinetic energy of a planetesimal about 1200 km in radius, arriving at the Earth's surface at a velocity of 13 km/sec, could eject two lunar masses to near-escape speeds. Assuming that the collision occurred after the Earth's core had started to form, one would expect the ejected material to be depleted in iron, as in the fission theory.

The advantages of impact over fission, according to Hartmann and Davis, are (1) that an energy source to raise the material off the Earth is provided; and (2) that "the theory is not purely evolutionary" (i.e., the outcome for a given planet is randomized, not purely deterministic), "depending on a chance encounter so that it does not require prediction of similar satellites for Mars or other planets." After the material is ejected it forms a cloud of hot dust, enriched in refractory elements and rapidly depleted in volatiles. The subsequent evolution follows the "widely admired" theory of Ringwood (1970).

Although Cameron was present at the original presentation of the Hartmann-Davis hypothesis he had already begun to develop a similar hypothesis independently and was not directly influenced by the Safronov planetesimal accretion model, since he preferred a gaseous protoplanet model instead. According to Cameron (1985b), he and W. R. Ward "were led to the suggestion of a collisional origin of the Moon through the following consideration. The angular momentum of the Earth-Moon system is less than sufficient to spin the Earth to rotational instability; we were nevertheless interested in determining the mass of the body which, striking a tangential blow to the proto-Earth, could impart the angular momentum of the Earth-Moon system to the proto-Earth. ... The required projectile turned out to be about the mass of Mars.... That defined the basic scenario of our lunar formation process" (Cameron, 1985b, p. 319).

Cameron and Ward (1976) postulated that both colliding bodies were differentiated and possibly molten at the time of impact. (This followed from the assumption that they were formed in gaseous protoplanets.) The mantle material of both bodies would be largely vaporized, and later the more refractory silicates would recondense into particles. These would form a thin disk at a distance of 2 to 4 Earth radii; beyond the Roche limit (about 3 Earth radii) gravitational instability would produce clumps, which would have substantial tidal interactions with the Earth. Further suggestions about the later stage of this process, based on the Lynden-Bell and Pringle theory of accretion disks (1974), were offered by Ward and Cameron in 1978.

C. Giant impact is accepted

William Kaula was one of the earliest supporters of the impact theory. In 1977 he mentioned it as a promising explanation for the early differentiation needed to account for the moon's bulk composition, although the probability of such an impact still seemed quite low (Kaula, 1977c). Subsequent calculations of thermal evolution based on Safronov's model seemed to tip the balance in favor of impact (Kaula, 1979).

Additional support for the giant-impact theory was provided by George Wetherill's calculations on the accretion of planetesimals (Sec. VII.B). Wetherill found from his version of Safronov's model that a substantial fraction of the total mass in each region would reside in bodies only one order of magnitude smaller than the dominant planetary embryo at a fairly late stage of the process. It was therefore an essential feature of this process that a terrestrial planet would be hit by an object as large as Mars during the final stage of its growth, although Wetherill emphasized (1986) that impact was only one of several processes that could be expected to provide material for the formation of the moon in his model.

A consensus in favor of the giant-impact theory emerged at a conference on the origin of the moon held in Hawaii in October 1984. According to Stevenson (1987) this was "not because of any dramatic new development or infusion of data, but because the hypothesis was given serious and sustained attention for the first time. The resulting bandwagon has picked up speed (and some have hastened to jump aboard)." Hartmann, one of the organizers of the conference, says his idea "had languished" since its publication; "when I went to a planning session for the conference to look over the abstracts for the proposed papers, I found, to my amazement and joy, that eight or ten of the abstracts—independently of each other—were about the impact idea" (quoted by Cooper, 1987, p. 80).[50]

The impact theory has been endorsed by several scientists who had previously favored a terrestrial origin for the moon, and mentioned favorably by others. A reporter for *Science* magazine wrote "The idea that the impact of a Mars-size body on the young earth could have formed the moon has breathed new life into a long-stagnant field" (Kerr, 1984). Similar language was used in an article in the Smithsonian's *Air and Space* magazine: the 1984 Hawaii conference "brought a fresh breeze of scientific thought to the problem of the origin of the moon—along with a sudden burst of research" (Frazier, 1987, p. 89).

But when theorists started to work out the details of the impact hypothesis they found that it might not perform one of the functions that made it seem attractive: getting the moon out of the Earth. Contrary to what had been generally assumed (except by Baker and Menzel, see above), Cameron stated at the Hawaii conference in October 1984 that, in the collisions he had simulated, most of the material in the disk came from the impactor rather than the Earth. Cameron (1985) found that at most one-third of the lunar mass would come from the Earth. This might not make much difference if the impactor was chemically similar to the Earth, but that seemed unlikely unless it was formed at the same distance from the sun and had a mass comparable to the Earth's. Otherwise it would be vulnerable to the same objection as the capture theory: the chemical composition of the moon must simply be postulated rather than predicted or derived from specified processes acting on known terrestrial material. If one thinks of the impactor as a planet chemically like Mars, then it would have a composition significantly different from Earth and moon. This point is sometimes overlooked by those who support the giant-impact theory because it makes the moon "primarily of terrestrial mantle material" (Abell *et al.*, 1987, p. 288).

Taylor (1987) sees this as an *advantage* of the large-impact model. He argues that lunar samples do not show "an identifiable signature of the terrestrial mantle, despite heroic attempts by proponents of fission." If most of the material making the moon comes from the impactor, then one can explain the *differences* between Earth and moon by attributing them to the composition of the impactor.

Detailed calculations of the physical processes postulated by the impact theory were started only recently (see Hartmann *et al.*, 1986; Stevenson, 1987; Benz *et al.*, 1989, and references cited therein). The results so far indicate that the impact mechanism is capable of placing material into orbit around the Earth, at the cost of melting the early Earth and thus coming into conflict with geochemical evidence (Kato *et al.*, 1988; Hartmann, 1989; Kerr, 1989). But other scientists say this objection can be overcome and that the geochemical evidence on balance favors impact (Garwin, 1989; Newsom and Taylor, 1989). Much more work will be needed to develop a well-defined model that is clearly distinguishable from other models, and even then this model may not appeal to scientists who think the moon was made from terrestrial material.

XII. ALFVÉN'S ELECTROMAGNETIC PROGRAM

A. Methodology

Although the cosmogony of Hannes Alfvén (Fig. 8) was first introduced several years before the theories of Hoyle, Cameron, Ringwood, and Safronov, I discuss it last for two reasons: first, it has not been as widely accepted as an explanation for the formation of the planets; second, it pays more attention to the smaller members of the solar system—satellites, rings, and asteroids.

The development of Alfvén's theory is fairly well described by the Lakatos "methodology of scientific research programs" mentioned in Sec. II.D. There is a well-defined "hard core"—the postulate that electromagnetic phenomena in plasmas are of primary importance in cosmogony (and in space science in general). The hard core is surrounded by auxiliary hypotheses: the strong magnetic field of the early sun, clouds of different composition falling toward the sun, the critical velocity effect, accretion of planetesimals, jet streams, and partial corotation. Several of these auxiliary hypotheses are separately testable in the laboratory and/or in space. The program cannot be tested as a whole, but individual scientists may judge for themselves whether its track record is progressive or degenerating.

Alfvén's cosmogony is also of philosophical interest because he explicitly advocates a methodology of "actual-

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[50] Kaula (private communication) disagrees with Hartmann's statement that the giant-impact theory was not popular before 1984, pointing out that he had called Wood's review of selenogony "obsolete" for failing to mention it (Kaula, 1977b, p. 1148).

FIG. 8. Hannes Olof Gosta Alfvén, Swedish physicist (plasma phenomena, cosmic electrodynamics). Photo courtesy of AIP Niels Bohr Library, Weber Collection.

ism" (Sec. II.C). This methodology seems to have two distinct components: first, one should start from the present state of the system and try to infer what might have happened at successively earlier times, rather than postulate a particular initial state and see whether the present state can be deduced from it; second, one should try to explain the development of a system in terms of physicochemical processes that can be observed in operation at present.[51] Alfvén has not always observed the first rule, but he has done quite well in following the second, so that other scientists have credited him with discovering specific new phenomena even if they do not accept his general cosmogony.

B. Magnetic braking

The first and perhaps the most important example of Alfvén's actualistic style is magnetic braking. He showed that an ionized gas surrounding a rotating magnetized sphere will trap magnetic field lines, acquire rotation, and thereby slow down the rotation of the sphere (Alfvén, 1942a). Ferraro (1937) had obtained this result earlier but did not suggest its possible use in cosmogony. Alfvén (1942b, 1946) proposed that the early sun had a strong magnetic field and that its radiation ionized a cloud of dust and gas, which then trapped the magnetic field lines and acquired most of the sun's original angular momentum. Magnetic braking would resolve one of the major difficulties in the original nebular hypothesis, which implied that the sun should be spinning very rapidly after the planet-forming rings had been spun off from the cooling, contracting nebula.

As noted in Sec. IV.B, Hoyle's theory involved a form of magnetic braking, and other scientists developed different versions of this idea (Lüst and Schlüter, 1955; Schatzman, 1962; Mestel, 1968; Mogro-Campero, 1975; on the difference between Hoyle's and Alfvén's approaches see Hoyle, 1988, pp. 35 and 36). But the postulate that the early sun's magnetic field was strong enough to make this process an important factor in redistributing angular momentum was rejected in the 1970s because there seemed to be no independent evidence for it (Cameron and Pollack, 1976, p. 63; Freeman, 1978; Prentice, 1978, p. 364; Dai and Hu, 1980). Such evidence may yet come from studies of remanent magnetism in meteorites or from observations of T Tauri stars (see references cited in Sec. IV.F). There is also considerable doubt that the nebula was sufficiently ionized to make magnetic braking effective as compared to other mechanisms (Tscharnuter, 1984).

While there is no direct evidence that the sun lost its angular momentum by magnetic braking, measurements of stellar rotation from the Doppler shifts of spectral lines show that stars in later stages of evolution generally rotate more slowly than those in earlier stages. There seems to be some fairly universal process by which a star loses most of its angular momentum at a particular stage of its evolution (Struve, 1950; Kraft, 1967). Thus one can no longer use the slow rotation of the sun as a conclusive argument against the nebular hypothesis.

Alfvén did not advocate a nebular hypothesis in the sense that both the sun and planets evolved from a single cloud. Instead, he proposed that a previously formed sun encountered several clouds of neutral gas (A,B,C,D), which were ionized and stopped at different distances from the sun and eventually condensed into planets. In later publications (e.g., Alfvén, 1960b, p. 1189) these clouds were identified as fragments left at the periphery of a cloud from which the sun formed.

C. Critical ionization velocity

To explain why clouds of specified chemical composition stopped at particular distances from the sun, Alfvén introduced his "critical velocity hypothesis": a cloud of neutral gas will begin to ionize when it encounters plas-

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[51] "We should not look for *the* cosmogonic theory ... which solves the whole problem at once, but for a number of theories clarifying the great multitude of detailed questions of which the total cosmogonic problem consists"—Alfvén (1967b, p. 223).

ma with relative velocity v such that its kinetic energy becomes equal to the ionization energy,

$$(1/2)mv^2 = eV_{\mathrm{ion}} ,$$

where V_{ion} is the ionization potential of the atoms. The velocity cannot exceed this critical value until the ionization is almost complete (Alfvén, 1954, 1960a). The ionized cloud is then stopped by the braking action of the sun's magnetic field.

Cloud *A*, consisting mostly of H, He, C, and some metals, has the highest ionization potential, so it falls furthest into the gravitational field of the sun, to a distance determined by equating the potential energy GM_0m/R to its kinetic energy $(1/2)mv^2$. The cloud then cools and refractory elements condense, later forming planetesimals. Clouds *B*, *C*, and *D* are stopped at greater distances from the sun; because of the lower temperatures at those distances, hydrogen and other gases can condense from clouds *C* and *D* in the region of the outer planets.

Although Alfvén was not able to give a satisfactory explanation of the atomic mechanism by which ionization occurred at the critical velocity, he was able to find extensive experimental evidence for the validity of his formula (Brush, 1990b). Even if one rejects the idea that it has anything to do with the formation of planets, one has to accept the critical velocity phenomenon as a substantial contribution to plasma physics, inspired by a cosmogonic problem.

The critical velocity effect could be applied to the formation of planetary rings. According to Alfvén (1960a, p. 617; 1960b, p. 1191), Saturn acquired rings because its cloud was braked at a distance 7.9 times its radius, inside the Roche limit, whereas Jupiter's was braked outside the Roche limit at 22 radii and therefore formed satellites instead. Uranus supposedly would have no rings because the critical velocity would not have been reached before the cloud hit the planet; the Uranian satellites were formed from the next (*D*) cloud, which had a different chemical composition.

Alfvén's theory implied that the giant planets would consist mostly of elements in the carbon-nitrogen-oxygen group. This contradicted the views accepted in the 1950s that those planets were composed mainly of hydrogen and helium (Sec. VIII.A). Some scientists considered the failure to account for the chemical composition of the planets a fatal objection to Alfvén's theory (Levin, 1962, p. 324; Urey, 1963, p. 154; Tai and Chen, 1976, p. 175; Goettel and Barshay, 1978, p. 612; I. P. Williams, 1979, p. 7).

D. The 2/3 effect

But Alfvén was more interested in explaining the physical rather than the chemical properties of the solar system. In 1967 he revived another cosmogonic plasma hypothesis from his early work, giving it the name "partial corotation." Whereas other discussions of magnetic braking assumed a tendency for the entire plasma to rotate at the same angular velocity as the magnetized central body, implying infinite electrical conductivity, recent space physics observations (Persson, 1963, 1966) indicated the presence of electric fields parallel to the magnetic field. In this case the transfer of angular momentum to the plasma would be restricted and only partial corotation achieved. If the plasma later condensed to grains that moved in Kepler orbits, with critical velocities equal to those of the plasma element, then their orbital eccentricity would be 1/3. They would cross the equatorial plane at the circle $r_n = 2r_o/3$, where r_o = original distance of the plasma element from the sun. If there were a small body ("embryo") moving in a circular orbit in the equatorial plane, it would absorb the grains so that they would eventually move in circles at a distance 2/3 of that at which they condensed (Alfvén, 1942b, pp. 24 and 25). Similarly a solid body moving in an orbit within the original plasma would cast a "cosmogonic shadow"—a gap in the distribution of particles formed by condensation at 2/3 of the radius of its orbit (Alfvén, 1967a).[52]

Alfvén proposed that the inner boundary of the asteroid belt was the shadow of Jupiter and that the Cassini division in Saturn's ring system was the cosmogonic shadow of the satellite Mimas, whose orbit is 3/2 times as large (Alfvén, 1942b, p. 25, 1967a, 1968b; Alfvén and Arrhenius, 1973, pp. 164–167). The *A* ring extends from this division out to the Roche limit; it has only medium intensity because grains had to fall through Mimas' orbit in order to form it, and some were captured by Mimas. The *B* ring was formed by grains originally between the Roche limit and Mimas. The *C* ring is weak because it comes from a region partially swept by grains of the *A* ring.

According to most theorists, the structure of Saturn's rings was primarily determined by resonances with satellite orbits. Alfvén argued that this explanation was not quantitatively sufficient. He proposed to use the new satellite "Janus," whose discovery was reported by Dollfus (1967, 1968), as a crucial test of the resonance and corotation hypotheses. According to Dollfus, the Janus orbit was at a distance of 1.6×10^{10} cm from the center of Saturn, so its cosmogonic shadow should be at 2/3 of this or 1.07×10^{10} cm. The resonance gap would be at $(1/2)^{2/3}=0.63$ of its orbit, or 1.01×10^{10} cm. Alfvén argued that his own hypothesis was supported in this case because there was a minimum in the luminosity curve at 1.06×10^{10} cm but none at 1.01×10^{10} cm (Alfvén, 1968b; Alfvén and Arrhenius, 1973, p. 167).

Unfortunately for the impact of this test, later observations from the *Voyager 1* and *2* space probes failed to confirm a Saturnian satellite at 1.6×10^{10} cm (Stone and Miner, 1981, p. 161, 1982, p. 503); the name Janus was

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[52] Vasyliunas (1987) disputes the theoretical reasoning leading to the 2/3 factor. (I thank David Stern for this reference.)

subsequently assigned to one of the two coorbiting satellites at 1.51×10^{10} cm (Lissauer and Cuzzi, 1985, p. 923; Alfvén, 1983, p. 87).

Alfvén noted a luminosity minimum at 1.11×10^{10} cm and argued that it was also of cosmogonic origin; he predicted the existence of a previously undiscovered Saturnian satellite at 2.80 Saturn radii, one magnitude fainter than Janus (Alfvén, 1968b). But no satellite has yet been discovered at this distance (Stone and Miner, 1981, 1982; Lissauer and Cuzzi, 1985, p. 923), and Alfvén did not mention this prediction in his more recent papers on Saturn's rings, although he claimed new confirmations of his theory from the *Voyager* results (Alfvén, 1981, 1983; Alfvén *et al.*, 1986).

Alfvén's interpretation of the structure of Saturn's rings has received little support from scientists outside of his own group. Pollack (1975, p. 13), one of the few who has even mentioned Alfvén's theory in print, rejected it in favor of the resonance theory; Franklin and Colombo (1970, p. 338), in their paper on resonance theory, dismissed Alfvén's hypothesis as "speculative." Recent review articles ignore Alfvén entirely (Cuzzi, 1978; Pollack, 1978; Pollack and Cuzzi, 1981; Cuzzi *et al.*, 1984; Lissauer and Cuzzi, 1985); and an eyewitness report on the discussions of scientists working on the *Voyager* project fails to mention Alfvén's name (Cooper, 1983).

E. The rings of Uranus

In December 1972 Bibhas R. De, a student of Alfvén, submitted a paper "On the Possibility of the Existence of a Ring of Uranus" for publication in *Icarus.* He inferred from the critical velocity hypothesis that Earth, Jupiter, and Uranus should originally have had ring systems. The Earth's original rings were probably swept up or dissipated by the moon after its capture; in the case of Jupiter, the satellite Amalthea, near the Roche limit, probably formed from matter that would otherwise have become a ring, although "it is still possible that some particles remain in orbit around Jupiter within its Roche limit." Uranus, whose satellites seem to have a regular pattern and were therefore probably not captured, has its innermost satellite well beyond the Roche limit, so a ring system should have survived within that limit (De, 1978, p. 341).

The paper was rejected by *Icarus* when first submitted, although one referee later stated that he had recommended its publication (Öpik, 1977, p. 48). After the rings of Uranus were discovered in March 1977 (Elliot *et al.*, 1977; Millis *et al.*, 1977), De tried again to get it published. In a letter to Carl Sagan, then editor of *Icarus*, De noted that the paper had originally been rejected because it was based on "Alfvén-Arrhenius numerology." But, De argued

> My prediction was based on ... an astrophysical model—the Alfvén-Arrhenius model. All astrophysical predictions are necessarily based on a model, and a successful prediction in part vindicates the model. And surely a paper that scientifically predicted the existence of a ring around a specific planet has to be significant vis-a-vis your journal. Your rejection of the paper reflected to me not the spirit of Icarus, but rather that of cautious Daedalus—and also a lot of scientific parochialism on the part of your referees. Having been a graduate student at the time, I did not have the courage to contradict the rueful remarks of your referees. [De, 1977a]

While his paper was being reconsidered by *Icarus*, De attempted to secure some public credit for his prediction by contacting Brian Marsden, Director of the Bureau for Astronomical Telegrams at the Smithsonian Astrophysical Observatory in Cambridge. But Marsden discounted the scientific value of De's 1972 paper:

> ... I do not think that a general remark of this type, backed up with some theoretical ideas though it may have been, can really be classed as a *prediction.* If you could have specified the distance from Uranus more precisely, or if you could have said that the rings would consist of something like five extremely narrow structures, it would have been a different matter; but I am sure that it must have occurred to other astronomers that there was no real reason to believe that Saturn was unique in having rings, and that Uranus was an excellent second candidate. Such thoughts would have been completely independent of the Alfvén-Arrhenius ideas. After all, it is a straightforward observation that Saturn has a well-developed regular satellite system in rings. The only other planets known to have well-developed regular satellite systems are Jupiter and Uranus. If the rings are made of ice (say), Uranus obviously becomes a better candidate than Jupiter, and in any case, direct detection of a Jovian ring would probably have been much easier than a Uranian ring. As a matter of fact, I believe that A. G. W. Cameron made a "prediction" on much these grounds, but he could not predict the detailed structure of the Uranian rings either. [Marsden, 1977][53]

It may be true that the Uranian rings *could* have been qualitatively anticipated from other cosmogonic theories, and it is certainly true that astronomers from William Herschel onward thought they had glimpsed them, but I am not aware of any specific published prediction based on an accepted theory. A leading expert on planetary science remarked, just before the discovery of the rings, that "the reason why Saturn alone has rings may be explainable by a 'condensation' theory according to which only the early Saturnian environment had the right nebular density and temperature for the nucleation and growth of ring particles from the gas phase" (Stevenson,

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[53] Marsden (private communication, 1988) points out that Cameron (1975b, p. 283), while not actually stating that Uranus *now* has rings, suggested that it may have had them previously, since ice, the major component of Saturn's rings, is stable at the distance of Uranus. He said the Uranian rings would have been lost because of perturbations by the satellites. A more definite (but still qualitative) prediction that *all* planets have rings, because of plasma corotation effects, was made by Gold (1964, p. 193).

1978, p. 404). Sagan, on the other hand, recalled that he was "really puzzled" at that time as to why Saturn was the only planet to have rings (Sagan, 1988, p. 13, 1975, p. 29). Elliot, one of the 1977 discoverers, was quoted in the press as saying that the discovery "caught everyone by surprise" (Sharma, 1977).

De (1977b) protested that his prediction "was based on a fundamental phenomenon of plasma physics known as the critical velocity effect," which was predicted by Alfvén in the 1950s and subsequently confirmed by laboratory experiments. In view of other successful predictions from Alfvén's theory, this one should not be dismissed as the lucky result of a " 'shotgun approach' of making a large number of predictions a few of which may come true. ... If indeed Cameron and anybody else had made a similar documented prediction on the basis of specific physical arguments ... they should go on record as predictors of the rings as well."

Three new *Icarus* referees remained unsympathetic to De's work (anonymous reports quoted in Sagan, 1977a). All complained that De had not said anything specific about the structure of the Uranus rings and had not based his prediction on a quantitative deduction from accepted physical ideas. One compared it to Velikovsky's claims "to have predicted all sorts of things, but among most astronomers his predictions do not command respect, since they are not based upon logical claims of reasoning." But another warned

> There is, however, an obvious political problem with rejecting it again since the title is so pertinent and the paper's conclusion so correct. To make matters worse, the Alfvén-Arrhenius ideas have received nearly the scorn reserved for Velikovsky and so the public might wonder whether we are open to new ideas. [Anonymous report quoted in Sagan, 1977a]

In response, De (1977c) insisted that his prediction had been based on a model that was as well established as any other cosmogonic model (in view of the experimental evidence for the critical velocity effect) and that to demand a more specific description of the structure of the rings as part of the prediction that would be expecting more than any other theory of the formation of the solar system had achieved.

Sagan (1977b) justified his rejection of the resubmitted paper on the grounds that, having sent it to a large number of referees, he could not find one who advocated its publication:

> The essential problem is, as you know, the feeling of all the referees that we are engaged in a fallacy sometimes called the enumeration of favorable circumstances—that is, that erroneous theories, if there are enough of them and if they make a sufficiently large number of predictions, must on occasion make a subsequently validated prediction.

Here Sagan ignored De's claim that Alfvén's theory had not made any *incorrect* predictions and had made several correct ones:"

> In addition, however, the essential argument of your paper has already appeared in the Alfvén-Arrhenius volume published as a NASA special report. Either of these reasons alone and certainly the two of them together constitute in my opinion grounds for rejection of the resubmitted paper. [Sagan, 1977b]

[The NASA report was published after 1972 (Alfvén and Arrhenius, 1976), so this could not have been a legitimate reason for the original rejection of De's paper.]

De published the paper a few months later in an issue of *Moon and Planets* commemorating Alfvén's 70th birthday. One scientist has said that the successful prediction enhances Alfvén's credibility as a ring analyst and makes his general theory more plausible (McLaughlin, 1980), but others share the view of the *Icarus* referees that the prediction has no scientific value because it is based on an incorrect theory. "To be right for the wrong reason does not hold much weight in scientific circles" (Elliot and Kerr, 1984, p. 74). This view prevails even though in this case the "wrong reason" includes the critical velocity phenomenon, which has been experimentally confirmed (Newell, 1985, p. 99; Brush, 1990b). Most publications on the rings of Uranus (e.g., Elliot and Nicholson, 1984) do not mention De or Alfvén at all.[54]

F. Jet streams

Planetary scientists respected Alfvén's contributions to plasma physics and cosmic electrodynamics (for which he received the 1970 Nobel Prize in Physics); few of them felt qualified to criticize his ideas on magnetic braking, critical velocity, and partial corotation in plasmas; but his next hypothesis involved only classical mechanics and generated a large literature, both pro and con. He proposed that inelastic collisions of solid particles moving in Kepler orbits will tend to focus them into "jet streams."

The jet stream idea was first applied to the Hirayama asteroids; Alfvén (1968) proposed that the "Flora" family "contains three groups of bodies traveling in almost identical orbits, thus constituting three jet streams." This would provide an alternative to the earlier view that these families resulted from exploded planets and would make the asteroids an intermediate stage in the formation of planets (Alfvén, 1970). Alfvén and Arrhenius (1970) therefore urged NASA to undertake a mission to an asteroid to determine its chemical composition.

In his 1968 paper Alfvén mentioned only briefly the

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[54] An exception is Petelski *et al.* (1980), who do give credit. Similarly no credit is given to the prediction of Jovian rings, based on an "eruption" theory, even though it was published in the principal Soviet astronomical journals and available in English translation in major Western libraries (Vsekhsvyatskii, 1962). In this case Americans seem to have dismissed the priority claim as just one more Soviet attempt to claim credit for discoveries made elsewhere (Shabad, 1979). Sagan (private communication, quoted in Brush, 1990b) argues that this is another example of reaching the right conclusion for the wrong reason.

more general significance of the jet stream phenomenon in cosmogony. He cited Kiang's (1966) suggestion that the phenomenon may be due to a viscosity effect and remarked "It can be shown that viscosity interaction may produce focusing [*sic*] of small bodies into jet streams. However, it is beyond the scope of the present paper to develop a theoretical explanation" (Alfvén, 1968, p. 102).

Detailed calculations of the dynamics of systems of particles indicated that Alfvén jet stream effect—a sort of "negative diffusion"—could indeed occur, but only if the collisions were sufficiently inelastic (Trulsen, 1971, 1972a, 1972b, 1972c; see also Baxter and Thompson, 1971, 1973; White, 1972, p. 304). Alfvén's interpretation of the Hirayama family was supported by several scientists (Danielsson, 1969; Chapman *et al.*, 1973; Ip and Mendis, 1974, p. 240; Ip, 1975, 1978a, 1978b; Hämeen-Antilla, 1977, p. 437; Shukhman, 1984) but others argued that asteroid data cannot be explained by the jet stream hypothesis (Napier and Dodd, 1974; Gradie and Zellner, 1977; Degewij *et al.*, 1978, p. 648) or opposed the hypothesis for other reasons (Arnold, 1969; Whipple, 1974, pp. 86 and 87; Brahic, 1975; Tai and Chen, 1976; Kaula, 1977a, p. 182; Pratap, 1977, p. 448; Henon, 1978; Gradie *et al.*, 1979, p. 365; Safronov, 1979, p. 978; Stewart *et al.*, 1984, pp. 484 and 485).[55] Alfvén and others have argued that the *narrowness* of planetary rings can be explained as a jet stream effect (Alfvén, 1983; Ferrin, 1978; Ip, 1978; Houpis and Mendis, 1983, p. 40).

G. The evaluation of theories

Although scientists often reject specific hypotheses, they rarely publish comprehensive critiques of entire research programs. The historian must search in letters and referees' reports (see Sec. XII.E) or try to extract from oral interviews the reasons why some general theories were ignored or rejected.

In Alfvén's case, the glaring discrepancy between his high reputation in space plasma physics and the slight attention given to his cosmogonic theories since the 1970s seems to call for some explanation.[56] The most extensive critique of Alfvén's planetogony that I have been able to find in the public record is a review by William Kaula (1977a), less than two pages long, of the monograph by Alfvén and Arrhenius (1975). My informal discussions with other scientists suggest that Kaula's critique is not significantly different from their private opinions.

Kaula questioned the validity of Alfvén's extrapolation from selected data on laboratory and space plasmas to phenomena differing in scale by orders of magnitude. He went on to state that Alfvén's model for the origin of the solar system fails to deal quantitatively with ten problems, ranging from the very high density that the interstellar medium must have had for the sun to acquire planetary material, as assumed in Alfvén's model, to a mechanism for the Earth to capture a moon coming from a different part of the solar system. But he stipulated that the model should not be judged by the number of problems it solved or failed to solve, nor did he mention any other model that gave a more satisfactory treatment of these ten problems. Instead, he suggested that the theorist has an obligation to recognize defects pointed out by others and revise his model in a way that is responsive to the concerns of the rest of the community. "All scenarios of solar system origin are imperfect, but most scenario writers are readier to admit their imperfections and to try to remedy them" (Kaula, 1977a, p. 182). Moreover, Alfvén lacks influence because of his "scornful but vague criticism of others, ignoring others' work on similar problems" and failure to revise his work by responding to "subsequent findings and speculations." Presumably it would be useless for Alfvén to reply that this is just the way his own work had been treated by the scientific community, and that other models also fail to deal quantitatively with many aspects of planetogony.

It seems clear that Alfvén's personal style of interaction with other scientists is partly responsible for the community's resistance to his work. The same is probably true for E. J. Öpik (1893–1985), who is privately credited by scientists with having made important contributions to the theories of the evolution of the solar system, but whose work is not adequately cited in the published literature (Schwarzschild, 1984; DeGroot *et al.*, 1986).

Weidenschilling (1988) points out that scientists rarely say explicitly in print that they "accept" a theory, whereas they may state that they reject it (or at least one aspect of it) in order to justify a different course of investigation. But they do "vote with their feet" by addressing questions relevant to a particular model. Thus one "accepts" a theory if (a) it poses interesting questions for further work; (b) the questions are relevant to one's own expertise; (c) "some combination of data, analytical techniques, and/or computational ability ... allow progress toward answering those questions" (Weidenschilling, 1988); and (d) funding is available. From this point of view it would be risky to accept one of Cameron's

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[55] According to Weidenschilling (1988), "the mainstream view is that [asteroids] are fragments of larger asteroids that were disrupted by collisions with other asteroids."

[56] According to Witting (1966, p. 83), "Alfvén's theory fits a large number of dynamical boundary conditions, even Bode's law, and leads naturally to the Jovian-terrestrial classification. It requires a very hot nebula during at least the start of planetesimal formation, and it is difficult to reconcile these high temperatures with the observed absence of differentiation of substances volatile at these temperatures .... Furthermore, the theory requires a large number of *ad hoc* assumptions, which has led later theorists to reject most of Alfvén's theory, keeping only the hydromagnetic aspects which led to a reasonable solution of the angular momentum problem." I do not discuss here Alfvén's views on the evolution of the universe, which are even further removed from the mainstream of cosmological opinion; see, for example, Alfven (1966); Lerner (1988); Horgan (1987).

theories because it will probably have been revised by the time it appears in print; only those working directly with him have access to the latest version. Alfvén's theory, by contrast, is too rigid. Moreover it fails on points (b) and (c) because most workers in cosmogony lack the expertise in plasma physics to develop it.

Safronov's theory, according to Weidenschilling (1988), "provides a 'golden mean' with a content sufficiently stable for meaningful work, but with many areas for progress. The dynamical questions are accessible to the rapidly growing power of computers. This in itself has kept the Safronov model dynamic; without computers it would have reached a dead end at the limits of analytic modeling in the early 1970's. ... I regard the existence of computers to be the greatest single factor in 'acceptance' of Safronov's general model in the sense of inspiring further work. The same can be said of the Kyoto model," which is less popular perhaps because there is less contact between Japanese and Western scientists than between Soviet and Western scientists.

If we look at what some experts now consider to be the "established" theory (see Cameron's "central paradigm," Sec. II.D), it is hard to find much agreement on *content.* Levy, introducing an authoritative compendium of review articles (Black and Matthews, 1985), asserts that "today, to a first approximation, there exist no competing theories for the origin of the solar system" and thus, unlike previous conference proceedings that began by reviewing different theories, only one theory need be presented (Levy, 1985, p. 3). As far as I can determine, none of the theories presented in the compendium even claims to give satisfactory quantitative explanations of more than one or two of the facts listed in Table I. The authors of different chapters of the book disagree on such fundamental points as whether the mass of the nebula is about that of the present sun or only $\frac{1}{50}$ as great (Hayashi *et al.*, 1985, p. 1107); whether angular momentum was transferred from the sun to the planets primarily by magnetic forces or by turbulent viscosity (Cameron, 1985; Safronov and Ruzmaikina, 1985); and whether giant planets are formed by gravitational instability of a gaseous nebula or nucleation (Pollack, 1985). The apparent consensus on other points seems rather precarious in view of the rapid swings of opinion we have seen during the last three decades on hypotheses such as the supernova trigger and high-temperature condensation of the terrestrial planets.

Closer examination of Levy's (1985) statement shows that by "origin of the solar system" he does not mean the actual formation of planets as we now observe them, but only the beginning of the process of forming a star that will "inevitably" be accompanied by some kind of planetary system. This is a much more modest definition than was used in the past. In particular, it gives little weight to "naturalistic" attempts to explain the present features of the system as the outcome of somewhat earlier stages (Sec. II.C). It gives primary importance to astrophysics and puts considerable pressure on other disciplines, such as meteoritics, to make their models consistent with astrophysics. Meteoriticists may say that their data provide "constraints" or "evidence" for astrophysical models of the solar nebula (Wood, 1985; Boynton, 1985), but what the astrophysicists really want from meteoritics is isotopic abundances that will indicate where the atoms came from *before* they belonged to the solar nebula (Wasserburg, 1985; Kerridge and Chang, 1985; R. N. Clayton *et al.*, 1985).

It is thus the physics of the prenebular epoch (molecular clouds, galactic density waves, supernovae, red giants, etc.) that wags the body of planetogony. The word "tail" does not seem quite appropriate here, but the metaphor may help to explain why there have been such radical changes in the theories used to explain a largely unchanging set of planetary parameters. Planetogonic theories have been evaluated not by their success in accounting for the properties of planets (with a few exceptions), but rather by their consistency with accepted theories of star formation and models of the early sun. The decision as to whether the solar system could have formed monistically or required an external stimulus was made not by observing the solar system itself but by measuring the distribution of ^{26}Al in the galaxy and determining the rates of certain nuclear reactions in a laboratory at Yale (Sec. VI.C).

It seems clear that physical arguments and data (including isotopic anomalies) will continue to provide the most important tests for planetogonic theories. Even in selenogony, where immense quantities of chemical and geological data were collected at considerable expense, calculations of angular momentum turned out to be crucial (Secs. XI.A–XI.C). The only thing that is likely to change this situation is the discovery of other planetary systems. "Astronomical" evidence might then become more important in evaluating theories—provided that those theories had been developed sufficiently to yield specific deductions about the observable orbits and sizes of planets. Alfvén himself once tried to play that game (1943b); it is time for another round with more players.

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FIG. 1. Sir Fred Hoyle, British astronomer (astrophysics and cosmology). Photo courtesy of AIP Niels Bohr Library.

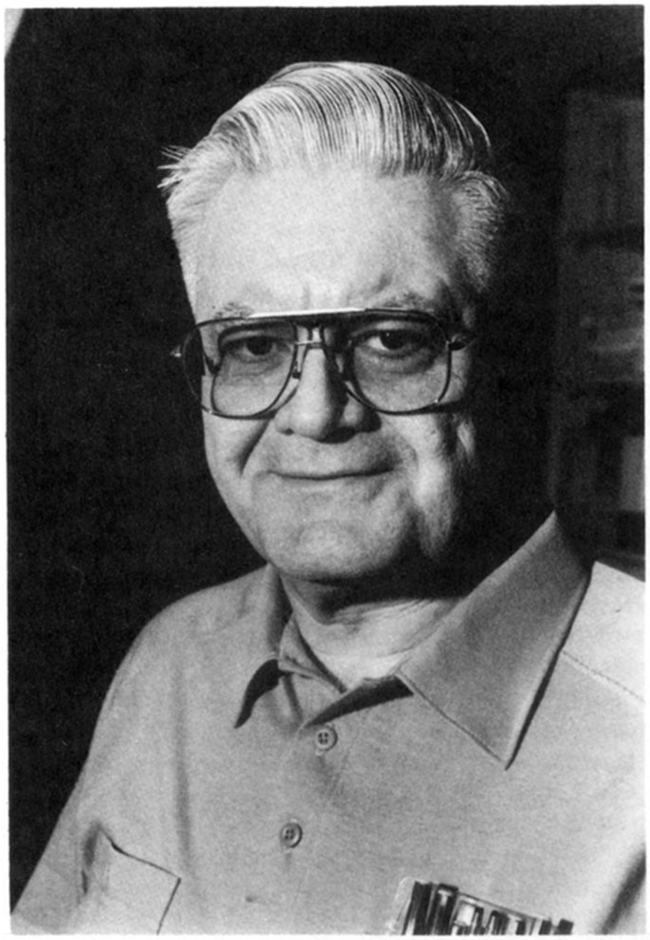

FIG. 2. A. G. W. Cameron, Canadian-American astrophysicist (nucleosynthesis, formation of stars and planetary systems, origin of the moon).

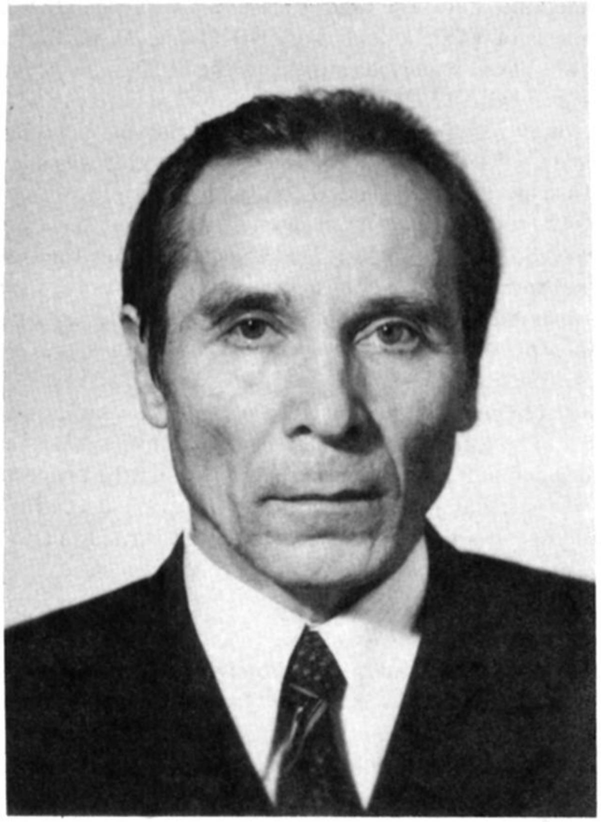

FIG. 3. Viktor Sergeyevich Safronov, Soviet astronomer (planetary cosmogony).

FIG. 4. George W. Wetherill, American astronomer (planetary science).

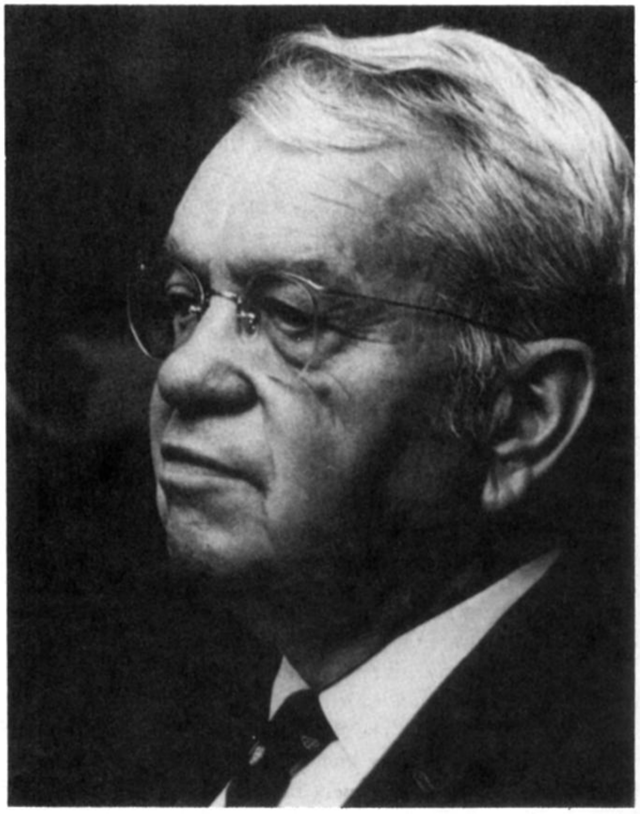

FIG. 5. Harold Clayton Urey (1893–1981), American chemist (isotopes, cosmochemistry).

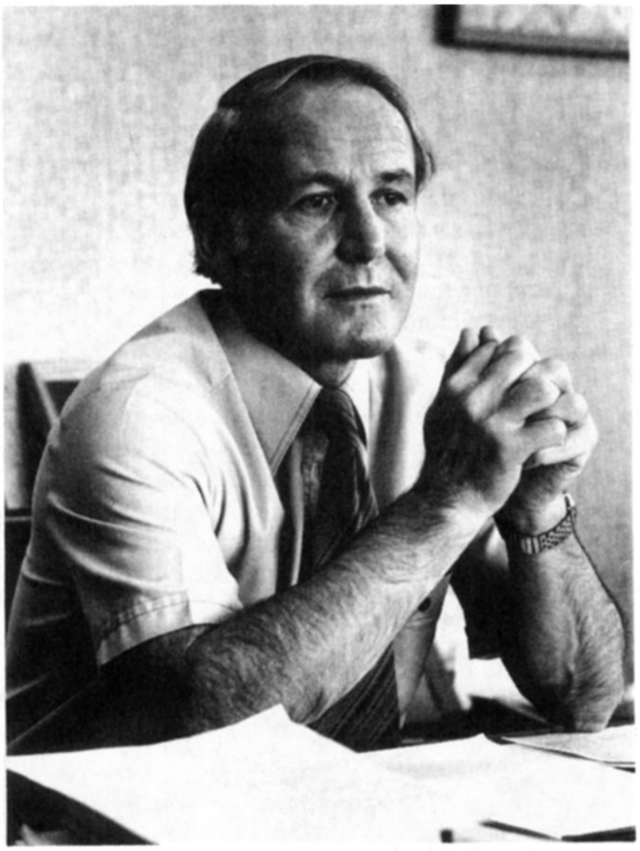

FIG. 6. A. E. Ringwood, Australian chemist (cosmochemistry, geochemistry, planetary and lunar formation).

FIG. 7. William K. Hartmann, American astronomer (solar system, planetary and lunar formation).

FIG. 8. Hannes Olof Gosta Alfvén, Swedish physicist (plasma phenomena, cosmic electrodynamics). Photo courtesy of AIP Niels Bohr Library, Weber Collection.