Chapter 7
The Twentieth Century History of the Extraterrestrial Life Debate: Major Themes and Lessons Learned

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Abstract In this chapter we provide an overview of the extraterrestrial life debate since 1900, drawing largely on the major histories of the subject during this period, *The Biological Universe* (Dick 1996), *Life on Other Worlds* (Dick 1998), and *The Living Universe* (Dick and Strick 2004), as well as other published work. We outline the major components of the debate, including (1) the role of planetary science, (2) the search for planets beyond the solar system, (3) research on the origins of life, and (4) the Search for Extraterrestrial Intelligence (SETI). We emphasize the discovery of cosmic evolution as the proper context for the debate, reserving the cultural implications of astrobiology for part III of this volume. We conclude with possible lessons learned from this history, especially in the domains of the problematic nature of evidence, inference, and metaphysical preconceptions; the checkered role of theory; and an analysis of how representative general current arguments have fared in the past.

7.1 Major Themes of the Debate

When the twentieth century began, the idea of a universe filled with life was widely accepted, completely unproven, and heavily burdened with a long and checkered history that finally held the promise of more successful scientific scrutiny. The challenge was to bring new data to bear on an age-old controversy. The infamous episode of Percival Lowell and the canals of Mars, resolved to the satisfaction of most astronomers by 1912 (Crowe and Dowd 2013), demonstrated just how difficult that challenge could be. Difficulties notwithstanding, the search for life would continue

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not only in our solar system with tools ranging from ground-based telescopes to *in situ* observations on Mars, but also in the realm of the stars with the search for extrasolar planets, in laboratories and environments on Earth performing research bearing on the origins of life, and with the radio search for signals from extraterrestrial intelligence. We now examine the major themes of each of these areas in turn.

7.1.1 Planetary Science

In the wake of the demise of the canals of Mars, the red planet remained a focus for the search for life in the solar system. After Lowell's death in 1916, with the close approach of Mars in 1924 attention focused on the possibility of Martian vegetation rather than intelligence. In one particularly important case this was still tied to the old visual method and canals; using the 36-inch Lick Observatory refractor astronomer Robert J. Trumpler concluded that the canals were the result of natural topography but that vegetation caused the dark Martian areas and made the canals visible (Trumpler 1927). But the mid-1920s mark a new era in Martian studies: physical methods of spectroscopy and infrared astronomy now came into widespread use in the attempt to determine temperature and atmospheric conditions. Respected scientists like W. W. Coblentz of the National Bureau of Standards, C. O. Lampland of Lowell Observatory, and Edison Petit and Seth Nicholson at Mt. Wilson Observatory, pioneering in the field of infrared astronomy, determined that the temperature conditions on Mars were adequate for some form of Martian vegetation (Coblentz and Lampland 1924; Petit and Nicholson 1924).

The belief in a harsher, but Earth-like Mars with vegetation was still very much alive at mid-century. At that time astronomers believed Mars had an atmospheric pressure of about 85 millibars at its surface, ten times thinner than Earth's. In 1949 the Dutch-American astronomer Gerard Kuiper had used early near-infrared techniques to discover carbon dioxide, one of the principle gases in the process of photosynthesis (Kuiper 1949). Seasonal vegetation across parts of Mars was commonly accepted, based on visual and photographic observations showing unmistakable seasonal changes on the surface as the polar caps melted, spreading a wave of darkening (Slipher 1927; Barabashev 1952; Kuiper 1955). The second edition of the standard astronomy textbook of the time was pessimistic about the existence of even primitive animal life, but asserted that the existence of vegetation was "more likely than not" (Russell, Dugan, and Stewart 1945, 344). Meanwhile, in the Soviet Union the astronomer Gavriil Adrianovich Tikhov assumed the mantle of the Russian Lowell, with a passion for Martian vegetation rather than Martian canals. In a career spanning many decades Tikhov used reflection spectra to study the optical properties of terrestrial vegetation in harsh climates and applied the results to Martian observations, claiming a new science of "astrobotany" (Tikhov 1955; Tikhov 1960). Tikhov's work, like Lowell's, provoked great criticism in his own country as well as abroad.

Using spectroscopic techniques, others found evidence of oxygen and water vapor in the Martian atmosphere, but in increasingly minute amounts, now known to be largely spurious (Spinrad et al. 1963). Despite the desert conditions revealed by the new physical methods, by 1957 and the dawn of the Space Age the existence of hardy, perhaps lichen-like Martian vegetation was widely accepted, especially in the wake of William Sinton's claims in that year to have discovered infrared bands in the Martian spectrum that were unique to vegetation (Sinton 1957; Sinton 1959).

These hopes were partially dashed in the early 1960s when the Sinton bands were found to be caused by deuterated water in the Earth's own atmosphere, and the water content of the Martian atmosphere was lowered almost to the vanishing point. But hopes were completely dashed two decades into the Space Age when the Viking orbiters and landers in 1976 seemed to demonstrate not only the lack of vegetation on Mars, but also the complete absence of any organic molecules at the two landing sites (Dick 1996, 153). And they showed an average atmospheric surface pressure of only 6 millibars. As we shall see in the next section, the Viking results on organic molecules—the *sine qua non* for life—have been questioned, and in the decades since that time other spacecraft have shown evidence of abundant water flow on Mars in the past. The Mars Global Surveyor and Mars Odyssey missions have both indicated that water ice still exists in plentiful amounts just below the surface, and the Mars Exploration Rovers have found strong evidence for plentiful liquid water below and on the surface in the past.

Nonetheless, evidence for life itself has not been found on Mars. The tantalizing seasonal changes were shown not to be due to vegetation, but to seasonal wind-blown sand. With the discovery at mid-century that Venus was a victim of the greenhouse effect, with temperatures consequently at the 800 °F level, it appeared that the solar system was bereft of life beyond Earth. Hope of microbial life in the solar system has not totally disappeared, due especially to the possibility that organics exist on some of the moons of the outer gas giants, notably Europa, Ganymede, Callisto and Titan. But because Mars had been viewed as a test case for life in the universe, the apparent absence of life there was a correspondingly great blow to the concept of a universe filled with life.

7.1.2 Planetary Systems

Long before the Viking results were in hand, attention had turned beyond the solar system to the possibility of the existence of other planetary systems—a prerequisite for life in the realm of the stars. Since they could not be directly observed, belief in such systems was greatly affected for most of the century by theories of their origin (Dick 1996). The nebular hypothesis of Laplace, whereby planetary systems were theorized to originate from the same rotating gas clouds that formed the stars themselves, indicated that planets were a natural by-product of star formation and, therefore, very abundant (Brush 1996). At the turn of the century, however, this theory was under heavy attack. In its place the geologist T. C. Chamberlin and the astronomer F. R. Moulton, both at the University of Chicago, proposed that solar systems originated by the close encounters of stars, which resulted in the tidal ejection of matter, which then cooled to form small planetesimals, which in turn accreted to form planets (Chamberlin and Moulton 1900). This "planetesimal hypothesis," elaborated and modified by the British astronomer James Jeans from 1916 almost until his death three decades later (Jeans 1917), implied that solar systems were extremely rare, since stellar collisions in the vastness of space were extremely rare. For this reason, during the 1920s and 1930s belief in extraterrestrial life was at a low point; it was difficult to conceive of life without planets.

But the 15 years from 1943 to 1958 saw once again a complete turnabout in opinion (Table 7.1). In 1943 two astronomers independently claimed they had observed the gravitational effects of planets orbiting the stars 61 Cygni and 70 Ophiuchi (Reuyl and Holmberg 1943). Although these observations were proven spurious decades later, they filled a need at the time. Doubts expressed in 1935 about Jeans's stellar encounter hypothesis by the dean of American astronomers, Henry Norris Russell, had grown to a crisis point by the early 1940s. Carl Friedrich von Weizsäcker began the revival of a modified nebular hypothesis in 1944, and the theoretical basis was once again laid for abundant planetary systems. The turnabout involved not only possible planetary companions and the revived nebular hypothesis, but also arguments from binary star statistics and stellar rotation rates. Helping matters along was Russell, whose *Scientific American* article "Anthropocentrism's demise" enthusiastically embraced numerous planetary systems (Russell 1943). Definitive evidence, however, would be much more elusive, for it turned out that Russell's declaration was 50 years premature.

Even as the nebular hypothesis has been elaborated in ever more subtle form, attempts to pin down the abundance of planetary systems proved very difficult. Through the 1960s and 1970s the search was dominated by the astrometric method,

Table 7.1 Estimates of frequency of planetary systems, 1920–1961

Author	Argument	Number of planetary systems in galaxy	Number of habitable planets in galaxy
Jeans (Jeans 1919, 1923)	Tidal theory	Unique	1
Shapley (1923)	Tidal theory	"Unlikely"	"Uncommon"
Russell (1926)	Tidal theory	"Infrequent"	"Speculation"
Jeans (1941)	Number of stars	10^2	–
Jeans (1942a, b)	Improved tidal	One in six stars	Abundant
Russell (1943)	Companions	Very large	$>10^3$
Page (1948)	Weizsäcker	$>10^9$	$>10^6$
Hoyle (1950)	Supernovae	10^7	10^6
Kuiper (1951)	Binary star statistics	10^9	–
Hoyle (1955)	Stellar rotation	10^{11}	–
Shapley (1958)	Nebular hypothesis	10^6–10^9	–
Huang (1950)	Stellar rotation	10^9	10^9
Hoyle (1960)	Stellar rotation	10^{11}	10^9
Struve (1961)	Stellar rotation	$>10^9$	–

Adapted from Dick (1996, 199)

whereby the proper motions of stars are studied for the gravitational effects of planetary systems. In the 1960s Peter van de Kamp and others made claims for planetary systems around other stars (Van de Kamp 1963). In the 1980s another method for determining planetary effects on stars—this time utilizing their line-of-sight "radial velocities"—came into use. At the same time the Infrared Astronomical Satellite spacecraft discovered circumstellar disks, initially interpreted as proto-planetary disks (now believed to be debris disks left over after planet formation). But it was only in 1995 that the radial velocity method proved unambiguously successful, when the Swiss astronomers Michel Mayor and Didier Queloz discovered a planet around the star 51 Pegasi (Mayor and Queloz 1995). The American astronomers Geoff Marcy and Paul Butler confirmed the discovery almost immediately, and after that the floodgates were opened for more discoveries (Marcy and Butler 1998). They came not only from the radial velocity method, but also from the "photometric method," whereby milli-magnitude dips in stellar brightness were measured as a planet passed in front of its parent star. It was this method that the Kepler spacecraft used beginning in 2009, discovering more than 2,000 planetary candidates by 2012. Of these almost 900 are Earth- or Super-Earth-sized, 1,200 are Neptune sized, and about 250 are Jupiter sized or larger. 48 planet candidates were found in the habitable zones of their stars, and it is estimated that at least 5% of all Sun-like stars host Earth-sized planet candidates.

7.1.3 Origins of Life

Even as the idea of abundant planetary systems was being revived in the 1950s, work was also progressing on the biological question of the origins of life, a crucial factor in the question of extraterrestrial life (Fry 2000). In the 1920s the Russian biochemist Aleksandr Ivanovich Oparin (Oparin 1924, 1936). And the British biologist J. B. S Haldane had independently suggested that life originated on Earth by chemical evolution in a hot dilute soup under conditions of a primitive Earth atmosphere. The experiments of Harold Urey and Stanley Miller in 1953 showed how amino acids could be produced under just such conditions, believed at the time to be highly "reducing" atmosphere, rich in hydrogen compounds such as methane and ammonia (Miller and Urey 1953). Their success set off numerous experiments around the world in chemical evolution as related to the origins of life. The major thrust of NASA's exobiology program, begun in the early 1960s, was to undertake such experiments on the origin of life, as well as to research life detection methods for spacecraft headed to Mars (Dick and Strick 2004).

Since the original Miller-Urey experiments, a better appreciation of the difficulties of the many steps in the origin of life—as well as uncertainty about the nature of the primitive Earth atmosphere—has somewhat tempered optimism among biologists. Whereas astronomers focus on the enormous size of the universe and the likelihood of planets emerging from an abundance of stars, biologists point to the extremely complex steps in the origin and evolution of life. Thus a dichotomy of opinion has developed between astronomers and biologists, further widened by the biologists' recognition that the evolution of life beyond Earth might lead to forms of life and intelligence very different from the humanoid form and alien to the human concept of intelligence.

Over the past quarter century theories of the origin of life have proliferated, with various implications for exobiology. Furthermore, the discovery of life in extreme environments—around deep sea hydrothermal vents, in deep underground rock, and in conditions of great salinity and acidity—has fostered a new appreciation for the tenacity of life, and broadened our idea of the conditions under which life might originate on another planet, or on Earth. As the possibilities of panspermia have become more widely accepted, spurred on by the Mars rock controversy (discussed in the next section) and by the realization that material does transfer between planets, some researchers believe that so-called "exogenous delivery" of organic compounds may be the key to the origin of life on Earth.

The question of the origin of life on Earth and in space shared many philosophical issues. Old problems such as chance, necessity, and the nature of life—already recognized in the terrestrial realm—were magnified in the extraterrestrial realm. The crucial question for exobiology was whether life would arise wherever it could, or whether the Earth was a fluke. The contingency or necessity of life would be one of the greatest scientific and philosophical questions of the extraterrestrial life debate. The two points of view are classically represented by the French biologist and Nobelist Jacques Monod on the one hand, and the Belgian-American biochemist and Nobelist Christian de Duve on the other. In his classic work *Chance and Necessity* (Monod 1971, 144–146) argued "the universe was not pregnant with life, nor the biosphere with man. Our number came up in the Monte Carlo game." Nor was Monod the only one to favor chance; the astronomer Fred Hoyle agreed that the chance of a random shuffling of amino acids producing a workable set of enzymes was miniscule, and went one step further in asserting that life must have been assembled by a "cosmic intelligence," though not necessarily the supernatural intelligence of Christianity (Hoyle 1983). de Duve, on the other hand, argued just the opposite, declaring Monod wrong and viewing life as a "cosmic imperative," while evolutionary biologist Richard Dawkins argued that "climbing Mt. Improbable" was not impossible (de Duve 1995; Dawkins 1997).

7.1.4 Search for Extraterrestrial Intelligence

All these questions in the origin of life arena are multiplied when it comes to the nature of consciousness, mind and intelligence. In many ways defining "intelligence" remains more problematic than defining "life," with many different possible approaches undertaken in a very large literature (Sternberg 2000; Sternberg 2002). To frame it another way, there is no "general theory of intelligence" or even of human brain function, much less a general theory of intelligence in a cosmic context. Carl Sagan argued in his *Dragons of Eden* that "once life has started in a relatively benign environment and billions of years of evolutionary time are available, the expectation of many of us is that intelligent beings would develop. The evolutionary path would, of course, be different from that taken on Earth… But there should be many functionally equivalent pathways to a similar end result. The entire evolutionary record on our planet, particularly the record contained in fossil endocasts, illustrates a progressive tendency toward intelligence" (Sagan 1977, 230).

That conclusion embodies many assumptions that others have questioned. Evolutionists such as George Gaylord Simpson and Theodosius Dobzhansky, for example, had already argued just the opposite (Simpson 1964; Dobzhansky 1972), and Harvard evolutionist Ernst Mayr also differed strongly with Sagan, arguing that intelligence (by his definition) had emerged only once on Earth (Mayr 1985; Mayr 1988). Outspoken Harvard evolutionist Stephen Jay Gould (1989, 301) agreed with the non-prevalence of *humanoid* intelligence, arguing in an entire book on the Burgess Shale fossils of the Cambrian explosion that if we "Wind back the tape of life to the early days of the Burgess Shale; let it play again from an identical starting point, and the chance becomes vanishingly small that anything like human intelligence would grace the replay." By contrast evolutionary paleobiologist Simon Conway Morris (Conway Morris 1998, Conway Morris 2003) has argued from the same evidence, and others, that evolutionary convergence applies not only to morphology, but also to intelligence, if only the conditions are present. He is, however, skeptical that the proper conditions often obtain, summarizing his position in the subtitle of his 2003 book *Life's Solution*: *Inevitable Humans in a Lonely Universe*. In this he reached the same conclusion as had Peter Ward and Donald Brownlee (Ward and Brownlee 2000), who famously argued that complex life and thus intelligence in the universe will be rare, not from a lack of convergence but because so many factors must come together in order for it to exist.

These problems are leapfrogged to some extent by the radio search for extraterrestrial intelligence, or, to put it more accurately, the search for extraterrestrial technology. In 1959 the physicists Giuseppe Cocconi and Philip Morrison, both at Cornell, proposed a search in the radio region of the spectrum using the 21-cm hydrogen line (Cocconi and Morrison 1959). The radio astronomer Frank Drake independently undertook the first search of such signals at the National Radio Astronomy Observatory in 1960. It was in the context of a meeting in 1961 in the wake of this search that the so-called Drake equation was formulated. A general equation embodying the various factors of star and planet formation, the likelihood of the origin and evolution of life and intelligence, and the lifetimes of technical civilizations, it came to serve in the last third of the century as a paradigm for discussion of the issues (Dick 1996, 431–454). Although almost everyone acknowledges that the parameters of the equation are not well known, resulting in values ranging from one planet in our galaxy with intelligence (our own) to 100 million or more, this uncertainty has not prevented its use as a basis for discussion of the abundance of technological civilizations in the galaxy. Many radio searches have been undertaken worldwide since 1960, all unsuccessful.

7.1.5 Birth of a New Discipline

In the 1950s and 1960s these four scientific fields—planetary science, the search for planetary systems, origin of life studies, and SETI—converged to give birth to the field of exobiology (Dick 1996). At first quite separate in terms of researchers, techniques, and goals, these fields over four decades gradually became integrated, in large measure because of the scientific and public desire to search for life beyond Earth. NASA served as the most important patron for the new field. By 1963 NASA's life sciences expenditures (including exobiology) had reached $17 million. The $100 million spent on the Viking biology experiments was closely related to origin of life issues, since an informed search for life required a definition of life and a knowledge of its origins. Even though exobiology saw a slump in the 1980s in terms of space missions in the aftermath of the Viking results, NASA kept the program more than alive with a grant program of about $5–$10 million per year, funding research on such broad topics as deep ocean hydrothermal vents and their associated archaea, the primitive Earth atmosphere, the Gaia hypothesis, mass extinctions, exogenous delivery of organic compounds, and the RNA world (Dick and Strick 2004). At the same time NASA also operated the largest exobiology laboratory in the world at its Ames Research Center in California.

In 1995 a deep organizational restructuring at NASA precipitated a rebirth of the field under a new name, "astrobiology." NASA's strategic plan for 1996 used he term astrobiology for the first time anywhere in a NASA document (though it had been sporadically used elsewhere as much as 50 years earlier). Astrobiology under NASA would focus on three key questions. It was "the study of the living Universe" to be sure, but in particular it was seen as providing the scientific foundation for studying the origin and distribution of life in the universe, the role of gravity in living systems, and the study of the Earth's atmosphere and ecosystems. In 1998 an astrobiology 'roadmap' laid out three specific questions: How does life begin and evolve? Does life exist elsewhere in the universe? And what is life's future on Earth and beyond? Specific goals were set to answer these questions (Des Marais et al. 2008).

The contrast between the exobiology and astrobiology programs was quite striking. They both shared the core concerns of origin of life research and the search for life beyond Earth. But astrobiology placed life in the context of its planetary history, encompassing the search for planetary systems, the study of biosignatures, and the past, present and future of life. Astrobiology added new techniques and concepts to exobiology's repertoire, raised multidisciplinary work to a new level, and included the study of the history of Earth's life and present organisms. Today astrobiology is a robust field, a worldwide effort supported especially by NASA, but also by other international research-funding agencies.

All of this did not occur without skepticism, extending even to the period 50 years ago when exobiology was born. In 1964 George Gaylord Simpson, pointing to the long history of the debate, wrote that "There is even increasing recognition of a new science of extraterrestrial life, sometimes called exobiology—a curious development in view of the fact that this 'science' has yet to demonstrate that its subject matter exists!" Simpson noted that this supposed new science was very expensive, and called exobiology "a gamble at the most adverse odds in history," resembling "more a wild spree more than a sober scientific program" (Simpson 1964, 775). Simpson concluded with a plea "that we invest just a bit more of our money and manpower, say one-tenth of that now being gambled on the expanding space program," on studying the systematic and evolution of earthly organisms—that is to say, his own field! An interesting case of the rhetoric of science, clearly Simpson had an ulterior motive in declaring that exobiology was not a science. But with Isaac Asimov's article in the *New York Times Magazine* the following year entitled "A Science in Search of a Subject" (Asimov 1965), the phrase was too good to ignore as a kind of mindless meme deployed innumerable times in the course of the following decades, despite the article's positive assessment of exobiology (Strick 2004).

Even a minimal consideration of this idea suffices to show it is a misrepresentation of science, even if admittedly a catchy phrase. One could say the search for gravitational waves, or the Higgs boson, or planetary systems, are, or were, "sciences without a subject." But this hardly seems a productive way of approaching the problem. Every science is looking for a subject until it finds it (planetary systems), thinks it may have found it (the Higgs boson), or does not find it (gravitational waves, at least so far). From an epistemological point of view, the methods of astrobiology are as empirical as in any historical science such as astronomy or geology (Cleland 2001; Cleland 2002), though it is true that astrobiological observations and experiments are often especially difficult, and the inferences more tenuous. With the broad array of research now being undertaken in astrobiology, the "science without a subject" meme has outlived its usefulness.

Although Simpson criticized the pioneer in the field, Joshua Lederberg, by claiming that exobiology was not strictly biology because its techniques differed (Wolfe 2002), certainly astrobiologists today would be surprised to learn they are not doing science; from their point of view their endeavors constitute not only science, but cutting-edge science. While more than one practitioner early on heralded astrobiology or its equivalent as a new scientific discipline (Shklovskii 1965; Billingham 1981), these claims may have been premature (Dick 1996, 475–478). Moreover, being labeled a discipline may be good or bad in terms of "Balkanization" and isolation from broader parent fields, such as was contemplated, but did not happen, in the case of radio astronomy in relation to astronomy as a whole (Sullivan 2009, 435–438). An historical comparison of discipline formation in other fields such as biochemistry (Kohler 1982), molecular biology (Abir-Am 1992), and geophysics (Good 2000) would help illuminate the problem for astrobiology.

7.1.6 Cosmic Evolution as the Context for Astrobiology

The concerns of astrobiology—the origins and evolution of life, intelligence and culture—are embedded in the larger process of cosmic evolution, the 13.7 billion

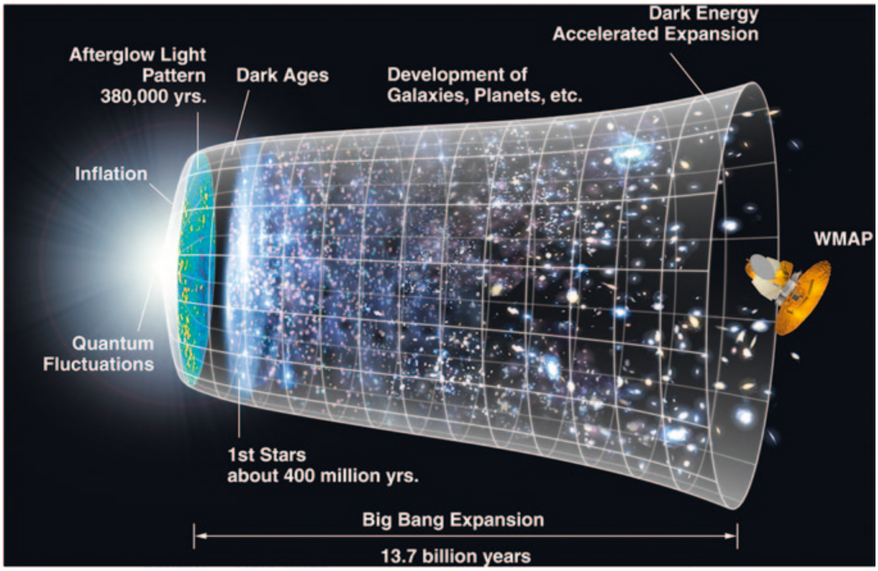

Fig. 7.1 The Master Narrative of the Universe, 13.7 billion years of cosmic evolution, as depicted by the Wilkinson microwave anisotropy probe (*WMAP*) program, which narrowed the estimated age of the universe to within 100 million years. The current model has the universe beginning with the Big Bang, stars forming within the first few hundred million years, followed by the development of galaxies, planets and life. The concerns of astrobiology must be seen within this framework, which encompasses physical, biological and cultural evolution. Courtesy NASA/WMAP Science team

year Master Narrative of the Universe (Fig. 7.1). The concept has its roots in the 18th and 19th centuries, but only became widely accepted and a major driver for research programs in the last half of the 20th century (Dick 2009; Zakariya 2010). I have argued elsewhere that the outcome of cosmic evolution may result in a physical, biological or postbiological universe, in other words, a physical universe composed of planets, stars and galaxies in which life is a fluke; a biological universe full of carbon-based life; or a postbiological universe in which cultural evolution has resulted in a universe full of artificial intelligence (Dick 2003). These outcomes determine the long-term destiny of humanity, and because the scope of astrobiology as set down in the Astrobiology Roadmap applies not only to the past and present, but also the future, the destiny of humanity falls within the purview of the philosophy of astrobiology

7.1.7 The Biological Universe as Worldview

The 20th century view of a universe full of life may perhaps best be seen as a cosmology in its own right, a “biophysical cosmology” that asserts the importance of both the physical and biological components of the universe. Like all cosmologies, it makes a claim about the large-scale nature of the universe, and its claim is that life is not only a possible implication, but also a basic property of the universe. Over the last four decades some scientists have come to question why the laws of nature and the physical constants appear to be "biofriendly," giving rise to what has been termed the "anthropic principle." The principle has many variants, all having to do with the apparent fine-tuning of the physical constants for life (Carter 1974; Barrow and Tipler 1986; Carr 2007). The phrase is a spectacular misnomer, and the term "biocentric principle" is much preferred, since in the context of astrobiology the universe appears to be friendly to life, and the very question to be answered is whether humans are the only intelligent life (Davies 2007).

The prospect of a fine-tuned universe has given rise to the idea of an ensemble of universes, termed a "multiverse," as an explanation for why we happen to be in a universe particularly suited for life (Carr 2007). Whether or not we invoke the multiverse, the physicist Freeman Dyson has suggested that the prospects are bright for a future-oriented science, joining together in a disciplined fashion the resources of biology and cosmology (Dyson 1988). In such a "cosmic ecology," life and intelligence would play a central role in the evolution of the universe, no less than its physical laws.

Like other cosmologies the biophysical cosmology redefines our place in the universe. And most importantly, like other cosmologies in the 20th and 21st centuries the biophysical cosmology has become increasingly testable; this is the role and the importance of modern astrobiology and SETI programs. Viewed in this light, the transition from the physical world to the biological universe is one of the great revolutions in Western thought, no less profound that the move from the closed world to the infinite universe described by the French historian of science Alexandre Koyré almost a half century ago (Koyré 1957). That transition has already occurred to some extent in the minds of most people. Whether the biological universe exists in reality, and what its effect will be on culture when and if it extraterrestrial life is actually discovered, remains to be seen. Its potential cultural impact is discussed in Section III of this volume.

7.2 Lessons Learned from the Twentieth Century Extraterrestrial Life Debate

Now that historians have completed surveys of the extraterrestrial life debate (Dick 1982; Crowe 1986; Guthke 1990; Dick 1996; Dick and Strick 2004), we can begin to study the possible lessons learned from that history. In this section we make that attempt in three overlapping areas: (1) the problematic nature of evidence and inference, and its relation to scientific preconceptions; (2) the role of theory in raising expectations, interpreting observations, and generating conclusions; and (3) an evaluation of the success or failure of some of the debate's most general arguments, including the Principles of Plenitude and Mediocrity and

"Goldilocks-type" arguments that life occurs under such tight constraints that it is rare in the universe. Another widespread general argument, the argument from analogy, we reserve for section III of this volume because of its overriding use and importance.

Whether or not there are lessons to be learned from history is a subject of some contention among historians. It is, of course, a dangerous game, with some (politicians in particular) reading into history whatever lessons they want to learn based on their own ideology. My attitude is more optimistic: lessons may be ambiguous, but they are there and can be debated and deployed. After all, not without reason does there exist a National Archives in the United States with the words "What is Past is Prologue" scrolled along the top of its impressive facade, a building whose function is duplicated in all civilized countries of the world. Not without reason did the Columbia Accident Investigation Board devote an entire chapter to history in its official report on the Space Shuttle's demise, and conclude that "history is not just a backdrop or a scene-setter, history is cause" (Columbia Accident Investigation Board 2003, 195). And not without reason does every high school, college and university teach history, ever hopeful that at the very least it will provide context, if not lessons, for students as they enter a complex world.

My optimism in this regard holds despite the fact that many thinkers—from Samuel Taylor Coleridge to Georg Wilhelm Friedrich Hegel, from Aldous Huxley to scholars today—have concluded that the main lesson of history is that the lessons of history are either misused or never learned. Thus Coleridge: "If men could learn from history what lessons it might teach us! But passion and party blind our eyes, and the light which experience gives is a lantern on the stern, which shines only on the waves behind us!" (Coleridge 1831). Hegel: "What experience and history teach is this—that people and governments never have learned anything from history, or acted on principles deduced from it" (Hegel 1832). Aldous Huxley: "That men do not learn very much from the lessons of history is the most important of all the lessons of history" (Huxley 1959, 222). Or, as a recent author put while contemplating Herodotus's ancient message about intercultural understanding: "it goes unheeded, as it always has and it always will, because history teaches us that we do not learn from history, that we fight the same wars against the same enemies for the same reasons in different eras, as though time really stood still and history itself as moving narrative was nothing but artful illusion" (Marozzi 2008, 95). With such cautions in mind, we nevertheless proceed to examine possible lessons to be learned from the history of the extraterrestrial life debate, in the (perhaps misguided) hope that scientists are more receptive to lessons learned than politicians.

7.2.1 Evidence, Inference and Preconceptions

Evidence, inference and interpretation are problems in all areas of science, not to mention broader areas such as law, where 5–4 votes are not uncommon on the

Supreme Court, based on interpretation of the best available evidence. Ask any three people to describe in detail *any* event they have just witnessed, much less something unexpected and emotional like a UFO event, and the likely outcome is three different answers. Scientists are trained in gathering evidence and making conclusions from that evidence, so one would hope their record would be better. Sometimes it is, but often not, especially when pushed to the limits of science, as is certainly the case in astrobiology. The episode surrounding the canals of Mars centered around the beginning of the 20th century is the most infamous example (Crowe 1986; Dick 1996). But we need not set our gaze back that far. Here we examine four episodes in the second half of the 20th century that we have already mentioned: William Sinton's claim of spectroscopic evidence for supposed Martian vegetation; Peter van de Kamp's claim of planets around Barnard's star; the ambiguities of the Viking experimental results; and the controversy surrounding the Mars rock ALH 84001. While many more exemplars could be used, these will suffice to illustrate the problems of evidence, inference and preconceptions.

7.2.1.1 Vegetation on Mars?

As the favorable close approaches of Mars neared in 1954 and 1956, interest in the red planet, driven by interest in the Martian vegetation hypothesis based on seasonal changes, was increasing. As the 1956 opposition approached, Harvard astronomer William Sinton planned a direct search for vegetation by spectroscopic methods. Keenly aware that previous tests for infrared reflectivity characteristic of plants had been negative, Sinton's own search had a new element: it depended on the fact that organic molecules have absorption bands at about 3.4 microns in the infrared part of the spectrum—beyond Gerard P. Kuiper's work that had been done in the 1–2.5 micron region leading to the discovery of carbon dioxide on Mars (Kuiper 1949). Sinton still used a lead sulfide photoconductive cell, as had Kuiper, but now cooled to 96 °K with liquid nitrogen to increase its sensitivity to 3.6 microns. The difficulties of the observations can be appreciated from the fact that the sensitive area of this cell was only 0.16 mm^2, and the diameter of Mars was less than 1 mm. Nevertheless, after four nights of observations Sinton believed he had enough evidence for his conclusion that the probability was "very high that an organic spectrum is required to account for the data" (Sinton 1957, 237).

Sinton was very much aware of previous visual evidence for vegetation in the form of seasonal changes in the size and shape of the Martian dark areas. In fact he saw the dip at 3.4 microns as "additional evidence for vegetation," and concluded that "this evidence, together with the strong evidence given by the seasonal changes, makes it seem extremely likely that plant life exists on Mars" (Sinton 1957, 237). Thus, his claim of infrared absorption did not constitute direct visual confirmation, but depended on the interpretation of spectrograms, an interpretation undoubtedly affected by preconceived ideas. The result caused considerable excitement, especially when Sinton confirmed it with equipment ten times more sensitive on the 200-inch Palomar telescope during the 1958 opposition. Although the image of Mars was only 2 mm, this time Sinton separated the dark areas from the bright areas on Mars and confirmed his previous conclusion of absorption bands near 3.5 microns (Fig. 7.2). Again he concluded that "the observed spectrum fits very closely … that of organic compounds and particularly that of plants" (Sinton 1959, 1237). In addition a 3.67 micron absorption band was confirmed, which Sinton attributed to carbohydrate molecules in plants, analogous to tests on plants on Earth.

Although Sinton's results were widely hailed and cited in the literature, they were open to interpretation: not only were other biological interpretations possible, by 1963 researchers had done extensive work on infrared reflection spectra of terrestrial compounds and were critical of Sinton's interpretation (Rea, Belsky, and Calvin 1963). And by 1965 Sinton himself suggested that two of the Sinton bands were due to heavy "deuterated" water (HDO) in the Earth's atmosphere, with the remaining band still possibly organic (Rea, O'Leary, and Sinton 1965). In the end, Sinton's refined methods had been mitigated by refined problems. Whereas V. M. Slipher a half-century before believed he had found oxygen and water vapor on Mars only to find that his results were contaminated by the Earth's own

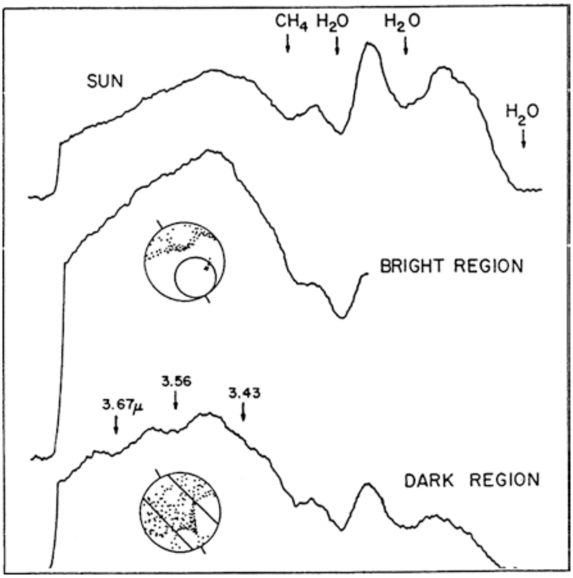

Fig. 7.2 Sinton's infrared spectroscopic evidence for vegetation on Mars, obtained on the Palomar 200-inch telescope. The top curve shows a solar spectrum, with superimposed absorptions by methane and water in the Earth's atmosphere. The middle curve shows a spectrum of a bright desert area of Mars, where no vegetation was expected. The bottom spectrum, obtained when the spectrograph slit was placed over one of the dark areas of Mars, shows three apparent absorption features (indicated by *arrows*) that were interpreted as due to vegetation. The evidence turned out to be spurious; the absorptions were actually due to deuterated water (*HDO*) in the Earth's atmosphere, as Sinton himself published six years later. With permission, from Sinton (1959), 1234. Copyright 1959 AAAS

atmosphere, Sinton's results too were contaminated, this time by heavy water, and despite his attempt to separate analysis of Martian dark areas from its bright areas.

As with the canals of Mars, the search for Martian vegetation demonstrates again differences in approach and world view among scientists, with one extreme much more likely to go out on a limb and to extrapolate than the other. Some astronomers probing the physical conditions on Mars presented their data and left it at that. Others used their data—indeed, were probably first inspired to gather their data—in the service of the question of extraterrestrial life. Still others rendered no opinion at all. In his book *Physics of the Planet Mars*, the astronomer Gerard de Vaucouleurs only rarely mentioned the problem of life because "It is our belief that such a problem is still, to a large extent, beyond the limits of our positive knowledge and can only be the subject—either way—of vague speculations in which general 'principles' of a metaphysical nature have always to be taken as a guide" (de Vaucouleurs 1954, 19).

To many, such a cautious attitude was not satisfying. They undoubtedly realized that the stakes in the debate extended far beyond Mars: as Kuiper wrote "If life truly exists on the only two planets of the solar system that are at all suitable to sustain it, it is tempting to conclude that, after enough time has elapsed, it will develop spontaneously wherever conditions permit. Since planetary systems are presumed to be very numerous, life would then be no exception in the universe" (Kuiper 1952, 404).

At the dawn of the Space Age, then, the canal controversy had receded, and much was known about the physical conditions of the planet Mars. Vegetation of some sort was still a very real possibility, dependent to some extent on what one saw as the limits to the adaptability of life. Vegetation did not have the popular appeal of intelligence, but to the scientist it was still a holy grail that held the promise of revealing the secrets of life. That promise was to play no small role in making Mars and important target for interplanetary probes of the space age.

7.2.1.2 Organics on Mars? The Viking Experiments

The culmination of the twentieth century search for life in the solar system was the landing of two Viking spacecraft on the surface of Mars in 1976, surely one of the great adventures in the history of science and technology (Ezell and Ezell 1984). The Viking project, initiated in 1968 after the demise of the Mars Voyager project and now managed by NASA's Langley Research Center, was an example of "big science" at its best in terms of budget, staff, goals and results. The cost of the Viking spacecraft, including the orbiters, landers and support (but not launch vehicles) was $930 million. Although the usual funding hurdles had to be overcome and many critics answered, in the end two Viking "orbiters" arrived at the planet on June 19 and August 7, 1976. After suitable reconnaissance, as the United States celebrated its Bicentennial back on planet Earth, two Viking landers set down on Mars in July and September. Under the guidance of project scientist Gerald A. Soffen, thirteen teams with a total of 78 scientists undertook thirteen separate investigations, including three mapping experiments from the orbiter, one atmospheric experiment, one radio and radar experiment, and eight surface experiments. The total costs for development and execution of these experiments was another $227 million. The results increased knowledge of Mars far beyond all previous investigations combined, finally providing definitive answers to age-old questions, including the issues of temperature, atmospheric composition and pressure so crucial to life.

From beginning to end, though the various science teams grappled with the myriad problems of meteorology, seismology, chemistry, imaging and physical properties of the planet Mars, the Viking biology experiments were the driving force behind the project, as evidenced by both budget and public, Congressional and even scientific interest. $59 million was spent on the Viking biology package and another $41 million on the molecular analysis experiment that was relevant to the question of life because of its ability to detect organic molecules. Harold P. Klein of NASA's Ames Research Center headed the Viking biology science team; Klaus Biemann of MIT headed the separate molecular analysis team. While the results of several of the teams were relevant to the question of Martian biology, these two out of the thirteen teams were most directly relevant.

The Viking biology package (Fig. 7.3) embodied in one piece of technology the most sophisticated thinking of the 20th century on the subject of extraterrestrial life in the solar system. The assumptions behind its experiments, the results obtained, and the ensuing controversies over the interpretation of these results are therefore of considerable importance. The diverse ideas about the nature of Martian life led to three different biology experiments aboard Viking, each representing a different approach to the problem of life. Indeed, biology team leader Klein later stated that had it not been for the constraints of 15 kg weight and about 1 cubic foot volume for the biology package, even more of the approaches conceived during the previous two decades would have been included on the spacecraft. The idea was that the three experiments, singled out and recommended by the Space Science Board of the National Academy of Sciences in 1968, would test for life using different philosophies, environmental conditions, and detectors.

One approach, which came to be known as the "labeled release" experiment, was developed by Gilbert Levin, who had spent much of the 1950s trying to improve methods for the detection of bacterial contaminants in city water supplies, and believed his method could be applied to the search for life on Mars. He was awarded a NASA contract for his "Gulliver" concept in 1961, and was reporting on his experimental apparatus already in the early 1960s. Levin's approach assumed that any Martian microorganisms, like those on Earth, would assimilate (eat) simple organic compounds, decompose them, and produce gases such as carbon dioxide, methane or hydrogen as end products. For this reason a dilute aqueous solution of seven such organic compounds, radioactively labeled for detection purposes, was added to the incubation chamber containing the Mars soil sample. The experiment tested for the expected "labeled release" of the gas produced as any organisms ate the organics and breathed out the decomposition products. The output was in the form of radioactive disintegrations, measured by a carbon-14 detector, in counts per minute.

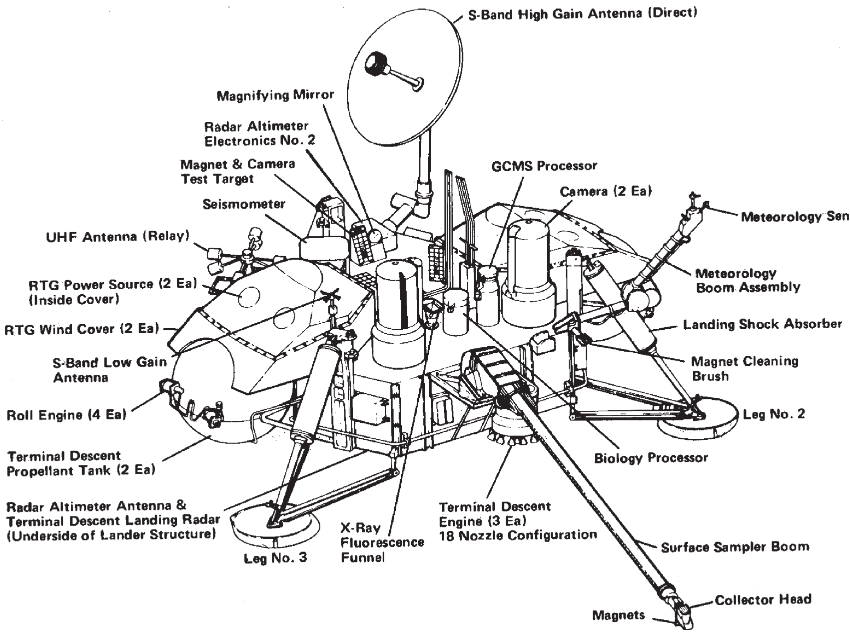

Fig. 7.3 The Viking lander, a complex machine incorporating eight experiments, landed on the surface of Mars in July, 1976, followed by another lander in September. The biology processor (labeled to the *lower right*) was within a small canister of volume 0.03 m^3. Nearby is the gas chromatograph mass spectrometer (*GCMS*), which detected no organic molecules at the two landing sites, down to parts per billion. Its conclusions have recently been called into question, and the biology experiment results are being reevaluated in the light of new evidence of the nature of the martian surface. (For scale: diameter of the lander body is ~3 m.) Courtesy NASA

The second biology test, the "gas exchange" experiment, was developed by Vance Oyama of NASA Ames Research Center, a veteran of life detection experiments on Apollo lunar samples. The gas exchange experiment tested for life under two different conditions. In the first mode, it was assumed that any organism in the dry Martian environment would be stimulated to metabolic activity by the addition of slight water moisture, and would give off a gas that could be detected by chromatography in the area immediately above the sample. In the second "wet nutrient" (or chicken soup) mode a rich nutrient of 19 organic compounds was added as an additional stimulus to metabolic activity, the products to be detected in the same manner. In both cases, the liquid added did not come into contact with the soil, but was added underneath the cell in which the soil "incubated." Water vapor gradually seeped up through the porous bottom of the incubation chamber, creating gradations of moisture through the soil. Experiments were also undertaken without the addition of any moisture.

The "pyrolytic release" experiment (also called the carbon assimilation experiment) was headed by Norman Horowitz of Caltech. Horowitz, a member of the WESTEX group in 1959, had cooperated with Levin's project in the early 1960s, but after Mariner IV showed that liquid water could not exist on the planet, he split with Levin and became convinced that it was best to test for Martian organisms under conditions known to exist on Mars when the experiment was designed. Thus to the small sample of Martian soil Horowitz proposed in his experiment to add only carbon dioxide and carbon monoxide, gases known to exist in the Martian atmosphere and now radioactively "tagged" for detection purposes. It was assumed that any organism on Mars would have developed the ability to assimilate these gases and convert them to organic matter. After 120 hours of incubation, the soil chamber was to be heated to 635 °C to pyrolyze the organic matter and release the volatile organic products, thus the name "pyrolytic release." A radiation counter yielded disintegrations per minute.

All three experiments sought to detect metabolic activities. Of the experiments Oyama's "wet nutrient" mode was the most Earth-like approach, in that it added rich terrestrial organics to stimulate any Martian organisms. Horowitz's was the most Mars-like, making few assumptions about Martian life except that it would be carbon based. Levin's, with his weak organic nutrient, fell in between. Levin and Oyama's experiments attempted to detect life by the decomposition of organics into gas during metabolism (a universal property of terrestrial organisms), while Horowitz sought to synthesize organic matter, which he would then pyrolyze in order to be able to detect. For detection purposes both Levin and Horowitz made use of standard techniques of radioactive carbon-14 as a "tracer," a method that did not change the chemistry, but provided a means of distinguishing atmospheric carbon from metabolized carbon. Oyama made use of the well-known method of gas chromatography for detection, as did Biemann (in conjunction with a mass spectrometer) for the organics experiment, which had nothing to do with metabolism. Ignorant of the nature of Martian life, the fondest hope of all the experimenters was that at least one of the experiments—hopefully their own—would turn up something.

Summer 1976 finally brought the day that Lowell, Kuiper and a host of scientific ghosts would have savored: the landing of two spacecraft on the surface of Mars to test for life in situ. They would not have been disappointed: Viking 1 landed successfully on the Chryse plain on July 20, and the first results of the biology experiments returned from Viking were exciting, to say the least. Although no visible life forms walked across the field of view of the camera, once the soil samples were collected on July 28, the biology experiments quickly began to return major surprises. Levin's experiment evolved gas into the chamber after the nutrient was added, then the reaction tapered off. Horowitz's pyrolytic release test was also positive, and Oyama's gas exchange experiment evolved not only CO_2 but also oxygen, the latter a reaction never before seen in tests on terrestrial or lunar soils. Because of the speed and course of the latter reaction, Oyama's experiment was not believed to be biological in nature. In short, two of the three biology experiments gave "presumptive positive results" for biology, and the third gave evidence of an oxidizing material in the surface at the Viking site. There was only one problem: in another unexpected finding, Biemann's organic analysis showed no organic molecules present to the level of a few parts per billion, a result Klein later called the most surprising single discovery of the mission. As Klein has subsequently recounted, these first results caused the carefully laid out experimental strategy to be abandoned, as the scientists attempted to discover whether chemical or biochemical reactions were taking place (Klein 1977; Dick and Strick 2004).

By eight and a half months after the first Viking had landed, 26 biological experiments had been carried out, and the first relatively complete results were reported, along with other Viking experiments, in *the Journal of Geophysical Research*. By then, shortly before the biological experiments were terminated in May 1977, Klein's considered judgment was that the positive result from Horowitz's pyrolytic release experiment was probably non-biological in origin, while Levin's labeled release experiment remained ambiguous (Klein 1977). Ironically, the gas exchange experiment of Oyama—the Viking scientist most optimistic about Martian life—showed no evidence at all for biological activity. Oyama and most of his colleagues concluded that the spontaneous evolution of oxygen was due to a chemical reaction involving "superoxides" such as hydrogen peroxide, perhaps by the effect of solar radiation on the small amount of water vapor in the upper atmosphere of Mars. "It's like the three bears," Klein later said. "Not too much water, not too little water, just the right amount of water in its atmosphere to produce something like this. This is one of the big mysteries, and any future missions to Mars have to find out what this stuff is" (Klein 1977, 4677–4680; Dick 1996, 155).

In the end, there was not complete consensus among the experimenters themselves. Writing for *Scientific American*, Horowitz concluded that although "it is not easy to point to a nonbiological explanation for the positive results" of his pyrolytic release experiment, "it appears that the findings of the pyrolytic-release experiment must also be interpreted nonbiologically," mainly because the reaction was less sensitive to heat that one expected from a biological process (Horowitz 1977, 61). Levin, however, did not agree; for decades he continued to argue forcefully that a biological interpretation of his data was still possible (Levin and Straat 1976; DiGregorio, Levin, and Straat 1997).

Clearly sensitive to their own assumptions, the Viking biologists continued to ponder the strategy of their experiments. What if their assumptions about Martian life, on which the biology experiments were based, were not correct? With this in mind Klein concluded his summary of Viking biology results with the astonishing remark that "we must not over look the fact, in assessing the probabilities of life on Mars, that all of our experiments were conducted under conditions that deviated to varying extents from ambient Martian conditions, and while we have accumulated data, these and their underlying mechanisms may all be coincidental and not directly relevant to the issue of life on that planet" (Klein 1977, 4679; Dick 1996, 157).

Ten years later, contemplating the experiments conducted for some ten months on the surface of Mars, Horowitz remained convinced that they not only proved the absence of life on Mars, but by extension "Since Mars offered by far the most promising habitat for extraterrestrial life in the solar system, it is now virtually certain that the earth is the only life-bearing planet in our region of the galaxy" (Horowitz 1986, 146). Although most scientists were not ready to make that quantum leap, it is also fair to say that they were much less optimistic about life on Mars in the aftermath of Viking. The Viking results were impressive enough that most scientists shifted the focus of their biological Martian interests to either past Martian history, or to different Martian environments such as rocks, polar caps, subsurface soil, volcanic regions, and the ancient river valleys.

That, however, is not quite the end of the Viking story. Though a consensus seemed to have been reached for several decades that life (indeed even organics) had not been found on the Martian surface at the two Viking landing sites, the issue was reopened especially after NASA's Phoenix lander discovered perchlorates on Mars in 2008. Some well-known and indisputably reputable scientists argued that such perchlorates would have destroyed any organics present in the Martian soil when it was heated during the Viking experiments (Navarro-Gonzalez et al. 2010). The issue remains open among prominent researchers today, with Levin more than ever convinced he discovered life on Mars (Levin 2011; Bianciardi et al. 2012). Surely, the extended discussion of the Viking results provides a cautionary note on the need for sensitivity to the preconceptions that enter into the design of experiments, and the difficulties of interpretation of the resulting evidence.

7.2.1.3 The Mars Rock

As the twentieth century approached its end, it appeared that the Viking landers had written the last chapter in the search for life on Mars. But almost exactly 20 years after the Viking landings, the world was startled with the announcement that organic molecules, possibly biogenic minerals, and even microfossils may have been found in a meteorite that originated on Mars. The result was controversial, though one might have thought that the inconclusive evidence would be balanced to some extent by the fact that the Martian meteorite could now be examined, not with the limited resources of a spacecraft on the surface of Mars, but with the full power of analytical techniques in many laboratories on Earth. A new era in Martian life studies had begun.

Meteorites had long been associated with the question of extraterrestrial life, but those meteorites were a special variety known as carbonaceous chondrites, and their parent body had not been identified. Only in the post-Viking era was a new category of extremely rare meteorites identified, and a case slowly built that they had originated on Mars. Known as SNC meteorites after the locations of their three types (shergottites, nakhlites and chassignites), they were also stony meteorites, but "achondrites," because they exhibited none of the millimeter-size embedded mineral spheres characteristic of chondrites. They were known to have come from Mars not only because of their chemical composition, but also because the gases trapped in them were precisely the same composition and proportions as those of the Martian atmosphere, as determined by the Viking landers. Thus, although Viking did not unambiguously find life itself, ironically it enabled the identification the SNC meteorites as Martian in origin.

The surprising announcement in the summer of 1996 centered on the Martian meteorite known as Allan Hills 84001, believed to have fallen on the ice fields of the Antarctic 13,000 years ago. The first meteorite found in the Antarctic during an NSF-sponsored search season in 1984 (thus the name ALH 84001), it was not identified as Martian in origin until 1994. One of only 12 such meteorites identified at the time, the 4.5 pound (1.9 kg) softball-sized rock was by far the oldest of the 12, estimated to have formed about 4.5 billion years ago, from a period when Mars was warmer and had water and an atmosphere. It was hypothesized that a meteorite impact on Mars fractured the rock about 3.6 billion years ago, and that another impact about 16 million years ago launched the rock into space, where it eventually intercepted the Earth.

The evidence, announced by a NASA team led by David McKay of NASA Johnson Space Center in Houston, consisted of four parts (McKay et al. 1996). None of these parts, the participants pointed out, were conclusive in themselves, but taken together they could be interpreted as biogenic. First, the multidisciplinary science team reported, the fractured surfaces of the rock contained large complex organic compounds in the form of polycyclic aromatic hydrocarbons (PAHs). This was already a step beyond what the Viking landers had found, but even though the NASA team undertook analysis that showed to their satisfaction that the PAHs were not contamination from Earth, this was not proof of life, since organic molecules could have originated by non-biogenic processes on Mars. But then the plot thickened: in the fractures the team also discovered carbonates and magnetite, minerals that are produced (among other ways) by certain "magnetotactic" bacteria on Earth. Finally, using a high resolution scanning electron microscope the team suggested the existence of microfossils in the carbonates and other mineral grains (Fig. 7.4); at only 20–100 nm they were 100 times smaller than the smallest known bacteria on Earth.

Less than two months later, a British team of scientists led by Colin Pillinger of the Open University announced independent evidence of possible traces of life, both in ALH 84001 and in a much younger Martian meteorite known as Elephant Moraine 79001 (EETA 79001, again named after the location of its discovery in the Antarctic). The latter meteorite was only 175 million years old, and was blasted from Mars only 600,000 years ago. This was so recent, geologically speaking, that it held open the possibility that life might still exist on Mars.

As in past controversies over Martian life, the stakes were high and the skeptics numerous. One of the chief objections came from Ralph Harvey of Case Western and Harry Y. McSween of the University of Tennessee, who had reported in *Nature* shortly before the NASA announcement that their analysis of the same meteorite showed that the carbonates formed not as a result of microbial life, but during the asteroid impact when carbon dioxide combined with the rock at temperatures of 1,200 °F. Such temperatures are inimical to life; if this method of carbonate formation was confirmed it would cast severe doubt on the claims of past Martian life. Others, however, argued for low-temperature formation of the carbonates, one that did not rule out life.

More general questions of inference from evidence were also asked. For example, do four independent but (critics said) weak arguments—from the morphology of the nanostructures, carbonate globules, the presence of magnetite and polycyclic aromatic hydrocarbons—add up to a strong argument for biogenesis? The

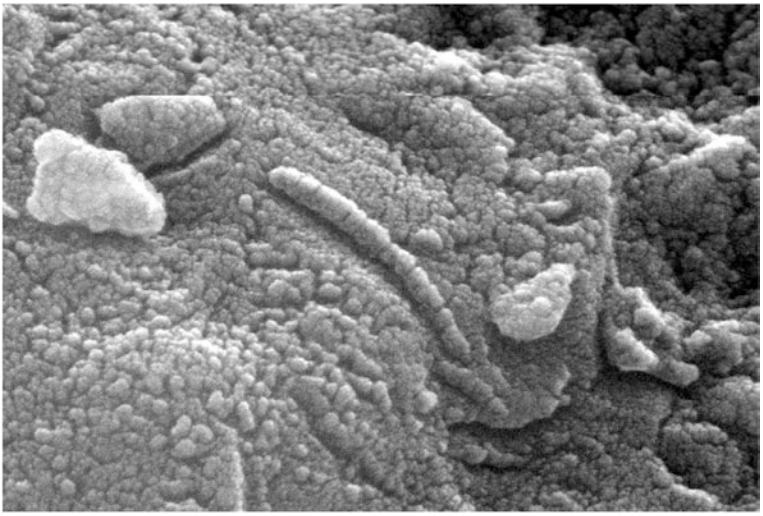

Fig. 7.4 High-resolution scanning electron microscopic image showing an unusual tube-like structure less than 1/100th the width of a human hair, found in Martian meteorite ALH 84001 and interpreted by some to be evidence of fossil life on Mars. Such morphological evidence was challenged by paleobiologist J. William Schopf, among other critics. Courtesy NASA

authors of the discovery paper thought so, ending their paper with the argument "Although there are alternative explanations for each of these phenomena taken individually, when they are considered collectively, particularly in view of their spatial association, we conclude that they are evidence for primitive life on early Mars" (McKay et al. 1996, 930). Critics, including paleobiologist J. William Schopf, thought not, arguing that "spatial association" held no persuasive value at all, and citing Carl Sagan's dictum "extraordinary claims require extraordinary evidence" (Dick and Strick 2004, 191).

Definitive proof of past life on Mars would come only by sectioning thin sections of the microfossils to search for cell walls, DNA or other structures unambiguously linked to life. In the years following the announcement many teams did precisely that, but with still ambiguous results. Though there is now consensus that Martian nanofossils have likely not been found, some (including most scientists who made the original claims) have not given up. Thus, the pattern is similar to that of the claims of Gilbert Levin about extant life on Mars based on the Viking experiments.

7.2.1.4 Planetary Systems?

If the evidence from the relatively nearby solar system proved problematic, the evidence for other much more distant solar systems would be even more difficult, if of an entirely different type. It is some measure of the difficulty of the search for planetary systems that the Space Age did not bring immediate advances in the problem. Unlike solar system studies, where planetary spacecraft brought immediate and revolutionary progress in our knowledge of the planets, no such prospect was in store for planetary systems. It is true that increased knowledge of our own planetary system provided voluminous data for the refinement of theories of the origin of the solar system, which by the usual gross analogies could be applied to other solar systems. But, although substantial, these refinements changed little the fortunes of planetary systems. Perhaps the largest impact of the Space Age on planetary systems science was the infusion of funds from space agencies such as NASA, which displayed an interest in both observational and theoretical aspects of the subject almost from the beginning, but with delayed results.

We should therefore not be surprised that, while most astronomers in the second half of the 20th century were optimistic about other planetary systems, observational proof of their existence through the 1970s remained entirely dependent on the old astrometric technique. That technique, the results of which remained elusive in many cases, created a public and scientific sensation with the announcement in the 1960s of the detection of several planetary systems. The promise and limitations of this technique, and the difficulties of tackling a problem at the limits of science, may best be seen in the famous case of Barnard's star. The central figure in the case is astronomer Peter van de Kamp, who had begun his search for low mass companions at the Sproul Observatory of Swarthmore College in Pennsylvania in 1937. Such is the long-term nature of the problem of determining perturbations in stellar motions that only 25 years later was Van de Kamp beginning to announce results with planetary companions.

Barnard's star was a star of 9.5 magnitude, so-called after Barnard's discovery in 1916 of its enormous proper motion of about 10.3 arcseconds per year. This meant that it was a close star (the closest known after the Alpha Centauri system), and it was immediately placed on observational programs, including the parallax program at Sproul in 1916–1919. In 1938 van de Kamp had placed it back on the Sproul parallax program with his arrival as Director in 1937, and by 1944 he announced a low-mass companion stellar in nature. Over the next 20 years, as Kuiper and others were predicting an abundance of planetary systems based on their own work, and as theory once again made plausible abundant planetary systems, van de Kamp patiently collected data on Barnard's star and other nearby stars.

There is no doubt that van de Kamp was sensitive to the question of whether low-mass companions were "stars or planets," at least since his 1944 article on the subject, an article undoubtedly stimulated by the observational claims of 1943. In a progress report on "Planetary Companions of Stars" in 1956, van de Kamp pointed out that while numerous unseen objects had been detected over the last two decades with masses 0.05 of the Sun's or greater, it was "extremely likely" that these objects were stars. "There are tentative indications of unseen companion objects with about 0.01 solar masses or more, and these may be planetary companions. However, definitive interpretation can hardly be reached at present, partly due to limitations of accuracy," he wrote in that year (Van de Kamp 1956, 1040). In particular, van de Kamp pointed out that the 1943 claims for a planetary companion of 70 Ophiuchi had not been confirmed as of 1952; and he held out hope only for the claim in 1943 for a companion of 61 Cygni of 0.016 solar masses, well into the planetary range, as confirmed by two other observers. As for his own program, now almost two decades old, van de Kamp claimed only that the companion to Lalande 21185 was probably a star of low luminosity, and that no satisfactory explanation existed for some perturbations seen in the motion of Barnard's star. Almost two decades after the start of his observational program at Sproul, no one could accuse van de Kamp of rushing to judgment on planetary companions.

All of this was to change in the 1960s. First, in 1960 Sarah Lippincott, van de Kamp's colleague at Sproul, announced that the companion of Lalande 21185 had a mass of only 0.01 that of the Sun. Although this was at within planetary range (recall that the 1943 claims for planetary companions to 61 Cygni and 70 Ophiuchi gave them about this same mass), Lippincott's technical article in the *Astronomical Journal* made no mention of the word "planet," and perhaps for this reason her announcement did not raise much of a stir. But van de Kamp's 1963 article with the mundane title "Astrometric Study of Barnard's Star from Plates Taken with the 24-inch Sproul Refractor" created a sensation (Van de Kamp 1963). In it he announced the discovery of a companion to Barnard's star with a mass of only 0.0015 the mass of the Sun, only 1.6 times the size of Jupiter, which he specifically characterized as a planet. He further found the distance of the planet from Barnard's star to be similar to that of Jupiter from the Sun, and its surface temperature about 60 K compared to 120 K for Jupiter.

Van de Kamp's claim was based on 25 years of photographic observations, using three types of photographic emulsions, and 50 observers, yielding 2413 plates. To the extent that the public was aware of such details, they might have been persuaded by this alone that such an intensive scientific effort must have yielded a definitive result. But they would have not been aware of the subtleties of the technique, which included taking into account a variety of insidious errors that could affect the results. Having taken into account all known sources of error as best as he could, van de Kamp found a perturbation in the motion of Barnard's star with a period of about 24 years (Fig. 7.5). In order to come up with an actual mass for the companion, he further had to carry out a "dynamical interpretation" calculation, using the mass of Barnard's star. By Kepler's law, once this mass was known, and the period of the orbiting body, the mass of the latter could be calculated. It was here that van de Kamp finally came to the figure of 0.0015 times the mass of the sun for his new planet: "The orbital analysis leads, therefore, to a perturbing mass of only 1.6 time the mass of Jupiter. We shall interpret this result as a companion of Barnard's star, which therefore appears to be a planet, i.e., an object of such a low mass that it would not create energy by the conventional nuclear conversion of hydrogen into helium" (Van de Kamp 1963, 521).

Like the announcements 20 years before, the reaction to van de Kamp's result was swift. From *Time* magazine to popular science magazines and more sober scientific journals, countless reports of van de Kamp's results hailed the discovery of another planetary system. Independent verification of the result, on the other hand, was more difficult, since the observations were very specialized and required

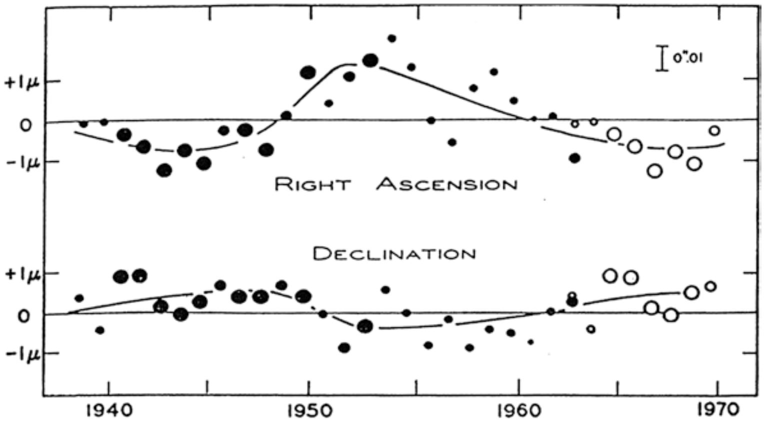

Fig. 7.5 Peter van de Kamp's evidence for a planet around Barnard's star made use of the classical astrometric method for planet detection. Van de Kamp reported that the star underwent minute, periodic gravitational perturbations of a few hundredths of an arcsecond over three decades. The two plots show the star's measured east–west and north–south relative positions (in microns) on photographic plates taken with a 24-inch refractor. The data was proven spurious, but only after several decades. From van de Kamp (1963), reproduced by permission of the American Astronomical Society

decades to reach a result—van de Kamp had been at it for a quarter century. It is not surprising, therefore, that van de Kamp himself was the first to reinforce his own result. In 1969, with five more years of photographic measures of Barnard's star, he reiterated his claim that a planetary companion existed around that star, with a slightly revised mass of 1.7 times that of Jupiter. In the same year, Van de Kamp proposed an alternate analysis of his data that held out the possibility that two planets orbited Barnard's star, with periods of 26 and 12 years, and masses of 1.1 and 0.8 times Jupiter.

But trouble was around the corner, and the 1970s saw serious questions raised about van de Kamp's momentous result. In 1973, John Hershey, one of van de Kamp's own students, found that changes made to the Sproul telescope, in particular a change of lens cell in 1949, caused jumps in the data at that point and may have affected the results for Barnard's star. In the same year George Gatewood of the Allegheny Observatory in Pittsburgh and Heinrich Eichhorn of the University of South Florida concluded "with disappointment," based on an independent analysis of 241 photographic plates, that no perturbations existed in the motion of Barnard's star. Attempting to explain their result, they pointed to the disadvantages of van de Kamp's analysis technique, to changes in the optical system of van de Kamp's telescope over the extended period of time of his study; and to the fact that his claimed perturbation was just "on the verge of significance," a status similar to claimed measurements of parallax before Bessel (Gatewood and Eichhorn 1973, 776). A similar analysis by Gatewood published the following year gave the same null result for Lippincott's 1960 claim of a planetary companion around

Lalande 21185. Two other studies of van de Kamp's data in 1973 were more favorable to his claim of one or more planetary companions, but a decade after the first announcement by the van de Kamp group, planetary systems were once again in trouble.

Van de Kamp understandably did not take lightly this negation of the main result of his work of 25 years. In order to take the objections into account, especially the finding that changes in the instrument might have affected positional measurements, van de Kamp re-measured his plates on a new machine, and included only material from 1950 onwards. He confirmed the existence of the shorter period planet, with a mass now 0.4 Jupiter, while the second planet was "less well determined." In 1977, 60 years after the discovery of Barnard's star, van de Kamp took the occasion to reassert his belief in the reality of its planetary companions. In addition to the now familiar scientific defense, the article concluded with a Rembrandt etching on the appearance of Christ to Thomas, with a caption "blessed are they that have not seen, and yet have believed," suggesting a religious invocation of faith undoubtedly seen by some as not readily transferable to the scientific realm (van de Kamp 1977, 521). Van de Kamp's last paper based on new data, published in 1982, again supported the conclusion of two planets around Barnard's star, a conclusion he never relinquished.

The Barnard's star episode was only the most notorious of several claims made for planetary companions by the mid-1970s, all subject to the same limitations of technique and inference. Although the assault on Barnard's star continued to receive the greatest attention, in the field of astrometric perturbations it was not unique, and thus could not be written off as a fluke. In an extensive 1975 review of the subject of unseen astrometric companions van de Kamp could list 17 "well-established perturbations" of stars by unseen companions, including Barnard's star and three others with possible planetary companions. Another 14 stars, including the famous 61 Cygni, were listed with "perturbations of provisional, suspected, or uncertain nature" (van de Kamp 1975, 312–313).

Not everyone was convinced that even those stars showing well-established perturbations necessarily harbored planets, for this depended on theoretical ideas about the cutoff point for stable hydrogen burning in stars. While CalTech geochemist Harrison Brown supported the idea of numerous planets by an extension of the "luminosity function" (the distribution of the stars with their visual magnitudes) to low masses, S. S. Kumar, for example, argued that all of the objects claimed as planets were probably very low mass "degenerate" objects that he termed "black dwarfs" (Kumar 1967). The dividing line between stars and planets remained the subject of vigorous discussion, and the accompanying search for what came to be known as "brown dwarfs" (objects not massive enough to sustain nuclear fusion) remained almost as elusive as the search for planets themselves.

Numerous other instances could be examined, including the controversy surrounding the claims of extrasolar planets in the 1980s and 1990s (subsequently demonstrated to be true), the claims of protoplanetary systems found by the Infrared Astronomical Satellite (IRAS) and the Hubble Space Telescope, and the recent reaction to the claims of arsenic life in the Halomonadaceae bacterium. In fact, it is safe to say, controversies about the interpretation of evidence are the central core of science, the rule rather than the exception.

All these examples also bear on the role of preconceptions, manifesting themselves especially in the form of prior assumptions feeding into the design of experiments, the interpretation of observations and the definitions of life and intelligence. For example, the Viking experiments were designed with metabolic conceptions of life, as opposed to other possibilities such as Darwinian processes, metabolism, energy and thermodynamics, complexity theory, cybernetics, or some new insight. This was in part due to the constraints of spacecraft investigation; nevertheless with the perspective of four decades we can look back on these experiments and ask how they might have been done differently. Certainly had scientists known about perchlorates on the surface of Mars, the experiments would have had a different design.

But what are the lessons learned from these representative case studies? Scientists need hardly be reminded to be careful with the interpretation of data. But the general public can never be reminded too often that this is the nature of science. The lesson is not that science should be mistrusted or abandoned because of its imperfections, but that despite the difficulties history shows that something approaching “the truth” eventually emerges as Nature is continuously interrogated at increasingly more subtle levels, even if the final outcome may take decades. The lesson is both pedestrian and profound: with all its personal and cultural biases, science is the best way we have of interrogating nature, and even in extremely difficult areas such as astrobiology progress can be made, even if we have to resort to analogies, which much of microbial astrobiology does. As scientists know—but as much of the public doesn’t seem to understand—science, including astrobiology, is a series of trial and error observations, subject to constant interpretation and re-interpretation, and therefore to constantly changing ideas, asymptotically approaching the truth—until the truth may suddenly change with a new way of looking at things. In this it in no way resembles religion, as is sometimes charged. The difficult and expensive nature of science, especially space exploration, makes experimental and observational iterations very extended. But in the end, once again, most scientists would say the objective truth is out there (though this is a deep philosophical problem), and remarkably the human mind can eventually ferret it out.

7.2.2 The Role of Theory

The role of theory in science is a huge and complex subject. We confine ourselves here to one example that demonstrates that theory can serve both as catalyst and as hindrance: the case of the origin of solar systems. As we have seen, during the first half of the 20th century, the primary and most widely accepted theory of the origin of solar systems was the Chamberlin-Moulton and the Jeans-Jeffreys tidal theory, whereby solar systems arise by material tidally pulled out during the close encounters of stars. This theory lowered expectations for observing other solar systems, since encounters between stars were believed to be extremely rare. Only in the 1940s, when the tidal theory was replaced by a revived nebular hypothesis, which postulated solar systems as a common byproduct of stellar evolution, did the expectations radically change, providing the backdrop first to Van de Kamp's work, and to the real discoveries of exoplanets 30 years later. So theory can affect both the undertaking and the interpretation of observations (Dick 1996).

In the absence of decisive observational evidence for planetary systems, one might expect that theories of solar system formation would play an especially important role, at least in determining the plausibility of such systems. This had indeed been the case for the nebular hypothesis, which favored abundant planetary systems because the formation of planets from a rotating gaseous disk was assumed to be a universal process. But as we have briefly seen in the first part of this chapter, that hypothesis was under serious attack in 1900, and for the first two decades of the twentieth century the new theory, to the limited extent that it addressed the issue at all, gave conflicting indications about the possibility of other planetary systems. Developed by T. C. Chamberlin, chairman of the Geology Department at the University of Chicago, and F. R. Moulton, an astronomy graduate student at the same university, the Chamberlin-Moulton hypothesis sought to surmount the technical weaknesses of the nebular hypothesis by proposing instead that solar systems were formed by the close encounter or actual collision of stars in space. According to this hypothesis, the close encounter caused material to be ejected from the Sun. The passing intruder then caused the ejected material to form spiral arms. These arms contained knots of denser material that condensed into nuclei, which in turn grew into planets and satellites by the capture of planetesimals, cold particles in the nebula. The spiral nebulae recently observed in the heavens, they believed, might be evidence of such collisions and of solar systems in formation. Curiously, rarity or abundance of planetary systems does not seem to have been an issue for Chamberlin or Moulton. To the extent that their rarity or abundance was an issue at all, it oscillated between the twin pillars of the "planetesimal hypothesis:" the spiral nebulae, which implied abundance, and stellar encounters, which implied rarity. With the gradual realization that spiral nebulae were too large to represent planetary systems in formation, the stellar encounter aspect of the theory was free to gain the upper hand—and with it the implication of the rarity of planetary systems.

This, in fact, is precisely what occurred, not in America but in Britain, where in the tradition of William Whewell and A. R. Wallace, the scientific community seemed more skeptically inclined toward planets and life. It was at the hand of the British mathematical physicist and astronomer James Jeans that the question of other solar systems would become closely linked with the rarity of planets and life in the universe. Jeans, a 1903 graduate of Trinity College, Cambridge, had done important work on atomic theory and statistical mechanics prior to 1914. After 1914 he turned from the microscopic to the macroscopic, from atoms to astronomy, and specifically to cosmogony. Jeans' attention was at first devoted to the stability of rotating bodies, on which subject he published two lengthy papers in 1915 and 1916. This work he applied to cosmogony in 1916 with reference to tidally distorted masses, in other words, determining how a rotating astronomical body would be affected by tidal forces raised by another passing astronomical object, as would happen in the case of a close stellar encounter. In a paper read before the Royal Astronomical Society in 1916 and published in the Society's Memoirs in the following year, Jeans dealt not only with the origin of solar systems, but also with binary star formation and spiral nebulae. In contrast to the binaries and spirals, Jeans concluded that the solar system might well have been formed from a tidally distorted mass, in particular by another star approaching our sun. Unlike the Chamberlin-Moulton hypothesis, however, Jeans' analysis showed that neither spiral nebulae nor planetesimals played a role in planet formation, and he thus emphasized that for solar systems "the origin which seems most probable is not that of the planetesimal hypothesis" (Jeans 1917, 48). Instead, his analysis showed that rather than the streams of gas torn from the Sun condensing into numerous small cold planetesimals that in turn accreted to form the planets, a single cigar-shaped filament of hot gas would be ejected and condense directly into the planets. As the theory was later elaborated, he pointed out that the largest planets would form near the center where the filament was thickest, and the smaller ones at each end, giving the distribution of planets observed in our solar system.

The central question in determining whether this mathematical conclusion could really occur in nature was the frequency of close stellar encounters. It is clear at the outset of the paper that Jeans was already thinking in these more general terms, not only with regard to the origin of our solar system, but in connection with the frequency of planetary systems. In his earliest statement on what would become a lifelong contentious issue, he wrote: "We have absolutely no knowledge as to whether systems similar to our solar system are common in space or not. It is quite possible, for aught we know to the contrary, that our system may have been produced by events of such an exceptional nature that there are only a very few systems similar to ours in existence. It may even be that our system is something quite unique in the whole of space" (Jeans 1917, 46).

Jeans' analysis showed that the issue of abundance was very sensitive to the assumptions made about a variety of parameters, including the density of stars in the universe, the velocity of the stars in space, the age of the stars and of the universe, and the size and mass of the stars at time of encounter. All of these parameters were subject to change in the discussion that ensued over the next three decades. For now, using the best estimates known in 1916 and assuming stellar masses and velocities similar to the Sun, Jeans found that at most 1 star in 4,000 might have experienced a "non-transitory" encounter at the distance of Jupiter in a lifetime of 10 billion years, the upper limit that he placed on the age of the universe. If the encounter distance were a hundred times greater and the other parameters adjusted accordingly, one star in three might have experienced such an encounter, and "we may, without postulating anything very improbable, suppose our system to have experienced an encounter as close as this…" (Jeans 1917, 46–47). However, Jeans clearly did not think all these conditions would ensue at one time, and in the end he labeled these occurrences as "somewhat improbable" and systems similar to our own "somewhat rare," but in general the entire process not "impossible or very improbable." Given the number of parameters and their uncertainty, Jeans' waffling is not surprising. But he emphasized that no reasonable choice of parameters was likely to alter the result that only very few stars can have experienced non-transitory encounters. And most importantly, Jeans stressed, the theory violated no quantitative criterion.

In his classic work *Problems of Cosmogony and Stellar Dynamics* (1919), Jeans discussed the problem in more detail, and ended with results even more pessimistic: only one encounter in 30 billion years, a situation so improbable in the present universe as to cast doubt on the validity of the close encounter hypothesis. Pointing out that the parameters were not well known, Jeans concluded that while tidal breakup by a passing star was hardly a likely event, its improbability was not grounds for rejecting the tidal theory. In whatever case one adopted, the solar system seemed to be very exceptional, "and for aught we know may be unique" (Jeans 1919, 290). In his 1923 lecture "The Nebular Hypothesis and Modern Cosmogony," Jeans carried his train of thought one step further, arguing that it was just possible, though not probable, that only the earth could support life in the universe. "Astronomy does not know whether or not life is important in the scheme of nature, but she begins to whisper that life must necessarily be somewhat rare" (Jeans 1923, 30).

In the hands of Jeans, this whisper soon grew to a crescendo. In both his technical and popular publications by the late 1920s, Jeans spread his view far and wide. The numbers varied somewhat, but always present was the basic scenario that the stars are sparsely scattered in space, close encounters exceedingly rare, and the conditions for life very exacting. "All this suggests," Jeans inevitably concluded, "that only an infinitesimally small corner of the universe can be in the least suited to form an abode of life" (Jeans 1930, 335). In his popular works this view of the disruptive approach of stars was vividly drawn, and the rarity of such approaches and their ensuing solar systems was an integral part of this picture—clear even to the public.

For two decades the Jeans tidal theory—with contributions by Sir Harold Jeffreys was widely accepted, and when the beginning of the end came in 1935 it was once again because of problems with physical principles. This time it was the Americans' turn again, in the form of Henry Norris Russell, who criticized the tidal hypothesis because it could not account for the present orbits of the planets. Russell could not see how a close stellar encounter would remove the planets so far from the Sun and give them most of the angular momentum of the system rather than the Sun, which was a thousand times more massive (Russell 1935). He also could not see how the planets could condense out of the high-temperature matter ejected from the Sun, an objection given definitive form by Russell's student Lyman Spitzer four years later. In their discussion the possibility of other planetary systems played no role, but their fatal objections left science without a workable theory of the origin of the solar system, and by association placed in limbo the idea that such systems were rare.

The 19th century view of abundant planetary systems was thwarted for decades by the tidal theory of Jeans and Jeffreys. Far from the teleological view of R. A.

Proctor or J. E. Gore, Jeans' colleague Sir Arthur Eddington asked "How many acorns are scattered for one that grows to an oak? And need she be more careful of her stars than of her acorns? If indeed she has no grander aim than to provide a home for her greatest experiment, Man, it would be just like her to scatter a million stars whereof one might haply [sic] achieve her purpose" (Eddington 1929, 179). To have provided a theoretical underpinning for this startlingly different worldview was no small part of the legacy of James Jeans.

But alas, this world view had no more claim to objective truth than the 19th century belief in abundant planetary life, for if the early observational claims for planetary systems at the turn of the century had yielded no definitive result, by 1940 neither had theory solved the problem—nor could it—especially with the departure of spiral nebulae as confirming evidence. The discredited nebular hypothesis had been superseded by the planetesimal hypothesis of Chamberlin and Moulton and then the tidal theory of Jeans and Jeffreys, only to have Russell and Spitzer overturn the latter, leaving only the void. Reviewing the collisional and nebular hypotheses in 1938, Lick Observatory Director Emeritus Robert G. Aitken still saw the development of planetary systems as an "exceptional event." "Exceptional" did not mean unique to Aitken, who pointed out that even if only one star in a million had planets, there would still be 30,000 solar systems in the Milky Way Galaxy—and two million galaxies were within the range of current telescopes (Aitken 1938).

As we saw at the beginning of this chapter, the 15 years between 1943 and 1958 saw a remarkable turning point in the fortunes of planetary systems. It had begun with Russell's criticism of the Jeans-Jeffreys tidal theory, but it was fueled by the revival of a modified nebular hypothesis, developments in fields as diverse as double star astronomy and the measurement of stellar rotation periods, and—most surprising of all—by insistent claims that planetary systems, or their effects, had been actually observed (Table 7.1). Moreover, broader events in the field of cosmology conspired toward change also, events that Jeans himself could not ignore.

The implications of the revolution in cosmology of the 1920s and 1930s—a greatly enlarged Galaxy, the existence of innumerable "island universes" full of stars, a universe expanding in space and expanded in time—are evident in Jeans' review of the subject of life on other worlds published in 1942. Having given a dim view of the chances of life on Mars and Venus, Jeans turned to the realm of the stars, and the origin of planetary systems. He pointed out that under present conditions in the universe the frequency of stellar encounters would be only 1 in 10^{18} years, so that for stars two billion years old, one star in 500 million might have planets. So far this was his old argument. But it was a sign of the times that he went on to say that though this seemed like a small fraction, in a universe with 10 billion galaxies, each with 100 billion stars, this minute fraction still represented 2 million stars that might have planetary systems! Statistics—and the new cosmology—had caught up with Jeans, even if only 2,000 of these systems might be located in our own galaxy. Straining the definition of "rare," Jeans was forced to conclude that "although planetary systems may be rare in space, their total number is far from insignificant" (Jeans 1942a, 83).

Later that year, however, Jeans' view had undergone a much more radical change. In a letter to *Nature* of June 20, 1942, reacting to recent claims of serious dynamical problems arising for the tidal theory assuming the Sun was about its present size at time of encounter, Jeans asserted that the Sun was most likely comparable in size to the present orbit of Uranus or Neptune when an encounter took place. In a last-ditch effort to save the tidal theory from dynamical objections, Jeans was forced to increase greatly the size of the Sun at the time of supposed planetary formation, a concession that greatly increased its cross-section and by analogy the cross section of other suns. Not only did this address the dynamical objections in Jeans' opinion, it also led to another conclusion: that the total chance of planet formation was now 1 in 6 with such a size for the Sun. Thus, "there is no longer any need to strain the probabilities to account for the existence of the planets" (Jeans 1942b, 695). And the final conclusion is one hardly expected from Jeans: "A far larger proportion of the stars than we have hitherto imagined must be accompanied by planets; life may be incomparably more abundant in the universe than we have thought" (Jeans 1942b, 695) The whole exercise demonstrated the fragility of the argument, and the dangers of using equations whose parameters were not well-determined. For 25 years Jeans had epitomized the concept of the rarity of life in the universe. Now in the last years of his life he recanted, and his death in 1946 left no substantial heirs to his theory.

Jeans' turnabout was just the beginning, and the cracks opening in the tidal theory in 1941–1942 were to become a breach through which the floodwaters of change would rush in the following year, when strong and independent observational claims were made for the existence of two planetary systems around nearby stars. Many astronomers were quick to draw general conclusions, especially in light of the new observations; as Henry Norris Russell wrote in 1943, "On the basis of this new [observational] evidence, it therefore appears probable that among the stars at large there may be a very large number which are attended by bodies as small as the planets of our own system. This is a radical change—indeed practically a reversal—of the view which was generally held a decade or two ago" (Russell 1943, 19). Table 7.1 indicates how completely the change was, due in no small part to a change in theory.

Examples of the role of theory in the extraterrestrial life debate could be multiplied, but the lesson in this case seems to be that theories can play both positive and negative roles in getting at the truth, especially when empirical evidence is lacking. One thinks of Harold Urey's theory of a reducing atmosphere on the primitive Earth, based on his speculation that the solar nebula was largely hydrogen. This led to Stanley Miller's famous experiment in which amino acids were produced under such a simulated primitive Earth atmosphere, a result that in turn gave much hope to those who believed extraterrestrial life might be common. The nature of the primitive Earth atmosphere has since been called into question, leaving scientists oscillating between optimism and pessimism—one might almost say between hope and despair—undoubtedly also influenced by predispositions to one side or the other.

7.2.3 Testing History: Plenitude, Mediocrity, Anthropocentrism and Rare Earth

History demonstrates a mixed record of success for general arguments in the extraterrestrial life debate, including the uniformity of nature, the Principle of Plenitude, the Principle of Mediocrity, the Goldilocks argument for rare Earth, large number arguments, and the Fermi paradox. In closing we focus here briefly on three related arguments: plenitude, rare Earth, and the Principle of Mediocrity, with the idea that our current understanding of the number of exoplanets, the conditions for the origins of life, the conditions for life on planetary and satellite surfaces, and "The Great Silence" can be used to evaluate the validity of arguments in astrobiology over the last 50 years and more. The arguments of Ward and Brownlee (2000) in their book *Rare Earth*, makes such an evaluation in the light of history an important endeavor.

That general arguments can sometimes be illuminating is illustrated in the case of the principle of plentitude, which posits the fecundity of God or Nature and states that whatever God or Nature can do, they will do. In his classic volume *The Great Chain of Being* the historian Arthur O. Lovejoy put it this way: that "no genuine potentiality of being can remain unfulfilled, that the extent and abundance of the creation must be as great as the possibility of existence and commensurate with the productive capacity of a 'perfect' and inexhaustible 'Source,' and that the world is better, the more things it contains" (Lovejoy 1936 1960, 52). He posited this argument as the chief argument of the entire plurality of worlds debate through the 19th century, a claim now seen to be too simplistic.

That does not mean, however, that ideas of plenitude have not played a significant role throughout history, as Chap. 1 of this volume demonstrates (Crowe and Dowd 2013). Nor does it mean that the same principle is not invoked even today, consciously or unconsciously, implicitly or explicitly. Despite skepticism prior to 1995, the last two decades have shown that a Principle of Plenitude does indeed apply in the case of exoplanets, which we now know exist in abundance around normal Sun-like stars. That the idea of plenitude has its limits is indicated by the fact that, although some planets are also found around exotic stars such as pulsars and in binary systems, they do not exist around all classes of stars. General metaphysical ideas such as plenitude must be mediated by sober physical reality and empirical findings. Whether or not this kind of mediated general argument carries over to life and intelligence remains to be seen—indeed that is the very question to be answered by more scientific means. But the Principle of Plenitude remains a kind of guiding argument for the optimists, and so far a useful one.

Other arguments, such as the "rare Earth" and related "Goldilocks" genre, do not fare so well when evaluated in terms of history. For example the natural philosopher William Whewell, who coined the term "scientist" in the mid-19th century, used numerous Goldilocks arguments to "prove" that other worlds could not exist—that the Earth was indeed rare. His treatise *Of a Plurality of Worlds: an Essay*, which appeared anonymously in 1853, was the most learned, radical, and influential anti-pluralist treatise of the century (Crowe 1986). As with all participants in the debate, Whewell had his predispositions in the matter of extraterrestrial life. When it came to the compatibility of other worlds with Christianity, unlike Thomas Paine and others Whewell argued that it was other worlds, not Christianity, that should be rejected. To the argument that all the vast space must have some purpose, he countered that geology reveals human existence on Earth to be but a short "atom of time" compared to the age of the Earth; therefore why could not intelligence be confined to the "atom of space" that was the Earth?

Moreover, although the universe was indeed vast (about 3,000 light years by his estimate), Whewell argued that the possible locales for inhabitants had been vastly overrated. With arguments that were plausible at the time he held that (1) all nebulae are gaseous rather than stellar systems, meaning that not nearly as many stars existed as some believed; (2) the analogy between the Sun and the stars may be less strong than many thought—all stars may not be similar to our Sun; (3) many stars are double stars, unsuitable for planets; and (4) there is no evidence of planets around other stars. In short, Whewell saw the analogies as greatly exaggerated in the case of other worlds. No longer was the Copernican implication that the planets were Earths a sufficiently precise argument; greater attention had to be given to the details of their physical conditions. But he was wrong that all nebulae are gaseous, and while there was no evidence at the time of other planets, today they are known to exist in abundance, even around double stars. Whewell was indeed correct that not all stars are similar to the Sun, but this argument was obliterated by the fact that we now know we live in a vastly larger universe that he conceived, harboring billions of stars even in a single galaxy.

All of Whewell's conclusions were based on uncertain evidence, but his arguments narrowing the number of habitats and claiming that everything had to be just right for life on those habitats were misguided. Today, with our knowledge of the extremities of life in deep sea hydrothermal vents, deep underground environments, and in conditions of extreme salinity, acidity, pressure, radiation, and almost every variable one can imagine, Goldilocks arguments seem even less relevant as a guiding principle. Such "rare Earth" type of arguments are now known to be fallacious in light of new knowledge, and it seems likely that the modern rare Earth hypothesis (Ward and Brownlee 2000) will suffer a similar fate, as the Kepler spacecraft is already beginning to indicate. Of course, the rare Earth declaration depends on the definition of "rare," and on the definition of "Earths," especially with the discovery of the category of planets called "Super Earths." But with cases in our solar system such as Europa, Callisto, and Ganymede, the very concept of a "habitable zone" has been revolutionized.

Related to the rare Earth argument is the insidious role of anthropocentrism, deployed consciously or unconsciously. Fifty years after Whewell's treatise, at the beginning of the 20th century, A. R. Wallace—the co-founder with Darwin of the theory of natural selection—used arguments similar to Whewell's to demonstrate the Earth had a favored place in the universe, that the sole purpose of the universe was to produce humans, and that humans were the only life in the universe. His influential work *Man's Place in the Universe: A Study of the Results* *of Scientific Research in Relation to the Unity or Plurality of Worlds* (Wallace 1903a) incorporated many of the biological problems that would be elaborated in ever more subtle form throughout the century. Although Wallace's book in some ways marks a signal advance in the debate about other worlds, its failure is marked by the dominance of the anthropocentric worldview over all other arguments. Convinced of the nearly central position of the Sun in the universe, Wallace first sought—and found—the significance of this fact in the uniqueness of life, and then adduced arguments in favor of the view that life was found beyond the Earth neither in our solar system nor in others. Fifty years after Whewell's treatise, Wallace confidently concluded that "Our position in the material universe is special and probably unique, and … it is such as to lend support to the view, held by many great thinkers and writers today, that the supreme end and purpose of this vast universe was the production and development of the living soul in the perishable body of man" (Wallace 1903b, 474). Although professing a scientific approach, Wallace's book serves as a lesson on the limits of science when worldviews dominate empirical evidence (Dick 1996). It is a lesson the twenty-first century should take to heart.

The same lesson, however, needs to be applied to the other side of the argument. During the 20th century the tug-of-war between anthropocentrism and other worlds was profoundly affected by radical changes in astronomical worldview. While it was still possible as the century began for scientists to argue for an anthropocentric universe based on the Earth's privileged physical position in the cosmos, by 1930 advances in astronomy had destroyed this argument. The resultant world view—an expanding universe of enormous dimensions in which the solar system was at the periphery of one galaxy among millions—tipped the scales strongly toward the presumption of other worlds for the rest of the century. "The assumption of mediocrity" became an underlying current of thought favoring other inhabited worlds, superseding the assumption of uniqueness that had opposed it. The hopes for anthropocentrism at the beginning of the century, and its rapid demise thereafter, constitute one of the profound shifts in twentieth century thought. In this sense the proponents of extraterrestrial life therefore champion not only a scientific theory, but an entire philosophy. But a Principle of Mediocrity cannot be taken for granted any more than can an anthropocentric worldview. Only observation will provide the answer.

Whatever the scientific merits of the extraterrestrial life debate, the emotional issue of human status is inextricably linked to all discussions of inhabited worlds. Pluralism and anthropocentrism have long been locked in a deadly battle that has not been completely decided by the dawn of the twenty-first century. Committed anthropocentrists, whether for religious or other reasons, are likely to be the staunchest foes of pluralism, no matter what the evidence, and pluralists—whether they liked it or not—contributed significantly to the demise of anthropocentrism. For all the appeal to scientific argument, the continuing battle between anthropocentrism and other worlds pervades modern discussions of extraterrestrial life, and carries the Darwinian debate on the status of humanity into the universe at large.

7.3 Overview of Part II

The chapters that follow in this section provide only the briefest glimpse at the richness of the history of the 20th century extraterrestrial life debate and the many approaches to it. Danielle Briot (2013) focuses on a single individual, Gavriil Adrianovich Tikhov, a Soviet astronomer whose work is not well known, but that is of pioneering interest for current work in the field. As we mentioned above, by mid-century Tikhov claimed to have created the science of "astrobotany" by his comparison of the reflection spectra of terrestrial plants with observations of Martian surface spectral features. Tikhov was correct in his negative detection of chlorophyll characteristic of plants, foreshadowing the work of Gerard Kuiper and William Sinton (if not the latter's conclusion), but, like them, he was mistaken in his belief that Martian vegetation existed at all. While this undermined the claims of a science of astrobotany for Mars, Briot points out that Tikhov's method was valid in two ways: today observations of Earthshine (which Tikhov also pioneered) have provided the biosignature of Earth's atmosphere, and our own planet's biosignature in turn provides the basis for eventually determining the biosignatures of the many planets now being discovered beyond our solar system.

Astronomer Chris Impey (2013) details the development of the field of extrasolar planet detection, including biosignatures. His chapter is of interest not only for the modern history of the field over the last two decades, but also because of his wider philosophical claims, namely, that the Copernican Principle, also known as the Principle of Mediocrity has been robust enough to lead us in a direction that is now confirmed empirically. "Our situation on a rocky planet that orbits a middle-weight star on the outskirts of an unexceptional spiral galaxy appears not be unusual or unique," he writes. He concludes that a billion habitable locales in our Milky Way galaxy is a conservative estimate, and this must be multiplied by a hundred billion to arrive at the number of potentially habitable locales in the observable universe. Twenty years ago we knew of no other planets outside our solar system; today we know of thousands, with the number increasing exponentially. Only the future will tell whether these locales are actually inhabited.

Douglas Vakoch (2013) represents another approach to the history of the modern debate touched on in this chapter: an investigation of how individual conceptual frameworks affect views on extraterrestrial life. In particular he shows how the modern evolutionary synthesis affected the opinions of four leading evolutionary biologists: Theodosius Dobzhansky, George Gaylord Simpson, and less directly, H. J. Muller and Ernst Mayr. In contrast to astronomers, he shows how increasing acceptance of the evolutionary synthesis led to a consensus by 1980 among biologists, anthropologists and paleontologists (at least the relatively few who addressed the subject) that complex life was rare in the universe. Such a conclusion, though based on a relatively small sample and still open to discussion and interpretation, points the way toward a research program in which personal, cultural, and conceptual factors may be examined with an eye toward their role in belief in extraterrestrial life.

Finally, Aaron Gronstal's (2013) chapter provides a transition to a subject that will be explored in more detail in Section III of this volume: the societal impact of the discovery of extraterrestrial life. His approach is unusual in focusing on the discovery of microbial rather than intelligent life. He makes the crucial point that the discovery of microbial life could have more immediate impact on Earth than the discovery of extraterrestrial intelligence, which is likely to be extremely distant and limited in its ability to communicate with us. The impact of microbial life, he argues, would likely not affect our theological and philosophical worldviews. Rather, it could have an immediate economic and technological impact in the areas of biotechnology and medicine. Drawing an analogy with the economic benefits of extremophile research over the last few decades, Gronstal concludes that microbial ecosystems on other worlds could provide similar economic value. On the other side of the coin, the traditional concerns about back contamination of Earth by extraterrestrial microbes must be heavily weighed against any economic benefits. Wiping out Earth life in the process of trying to create new medicines would not be an optimal outcome. The cost-benefit debates over the discovery and uses of microbial life beyond the Earth are thus likely to be heated and protracted.

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