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Examining life's origins : history and epistemic principles in the search for the origins of life

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Examining Life's Origins:
History and Epistemic Principles in the Search for the Origins of Life

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy

in

Philosophy (Science Studies)

by

Eric Collin Martin

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The Dissertation of Eric Collin Martin is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

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2010

DEDICATION

For my parents, Sandy Martin and Robert Martin

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ABSTRACT OF THE DISSERTATION

Examining Life's Origins:
History and Epistemic Principles in the Search for the Origins of Life

by

Eric Collin Martin

Doctor of Philosophy

Philosophy (Science Studies)

University of California, San Diego, 2010

Professor William Bechtel, Chair

My dissertation provides a novel philosophical and historical analysis of origins of life research, a scientific field that poses significant conceptual challenges to evolutionary theory, standard experimental methodology, and theory integration across disciplines. The origin of life is sometimes considered "the most fundamental problem in biology." Despite its theoretical significance, the field of study is fraught with fundamental disagreements over theories, methods and approaches.

The origin of life presents a boundary problem for central biological processes, including natural selection, which presupposes the existence of an enormously complex physical system of replication that must have been absent in the emergence of life.

Without natural selection, other forces must have driven the origin of life, and I illustrate which alternative explanations are may be involved in explaining life's origin. Such explanations have sometimes been associated with critical receptions of standard neo-Darwinian theory, so far from being independent of evolutionary principles, they have been connected with evolutionary theory in complex ways.

I further investigate the heuristics used to evaluate the new discoveries in the origin of life. One of the most important such heuristics is continuity: an insistence that transformations in the path to life display no saltatory transitions. I use case studies to show how the principle of continuity is not used consistently across research groups in origin of life research, and how its invocation depends on background assumptions about the likelihood of success of alternative research programs. I conclude that the principle of continuity has been, in practice, either unhelpful or even positively harmful to research into life's origins.

The topic of the origin of life underwent a remarkable transformation in the 20th century, from a question disparaged as speculative metaphysics to a legitimate field of scientific inquiry. Central to this transition was the British polymath J.B.S. Haldane. Haldane brought the question of life's origin into mainstream scientific investigation with his 1929 hypothesis of life's emergence from the "hot dilute soup" of Earth's early environment. This theory arose in the context of a protracted debate on holism and mechanism. I show how that philosophical debate figured in Haldane's philosophy and science, and what intellectual and social forces were acting on Haldane's novel chemical-evolutionary proposal for life's genesis.

INTRODUCTION

"[W]e are powerfully drawn to the subject of beginnings. We yearn to know about origins, and we readily construct myths when we do not have data."

--Stephen Jay Gould, 1989. "Creation myths of Cooperstown"

I.1.

Gould's statement is relevant to my dissertation because this essay, too is concerned with the stories that are told when data is lacking. While Gould's proximate concern in the above citation is baseball, he is at pains to formulate a generalization about the ways we generally tell stories to ourselves, and about the ease with which many such stories are generated. (This caution is only appropriate from the foremost critic of "just-so" stories in evolutionary biology – the adaptationist narratives that have been unreflectively applied or assumed by many biologists.)

The statement applies with some legitimacy to the question of life's origin, where scientific data is, arguably, still lacking. That has not stopped the creation of stories accounting for the phenomenon. Only a century ago there were few if any serious scientific theories of life's origin; today there are many theories. And they are only tenuously supported by the data.

To some extent, that is a ubiquitous problem inherent to the logic of science: a finite data set will always underdetermine any general theory. Science never provides logically secure, airtight verifications. But the problem is especially pronounced in a field like origins of life, where even the most ardent proponents of research and enthusiastic optimists admit that data is lacking. One commentator and insider to the science himself recently described the field as "woefully incomplete" (Hazen 2005). This lack of data, I have found, exposes methodological fault lines from which much can be learned about the state of the science. When stories are not straightforwardly entailed by the data, the principles used to construct those stories come into clearer focus.

To make matters worse, it is not immediately evident what sort of data are relevant to forging the answers sought in this field. The relevance of data is discerned though a complex, highly reticulated network of claims establishing links between environmental conditions, inorganic chemistry, and biochemistry. The assessment of relevance often ends up diverging along disciplinary boundaries, such as atmospheric scientists' favoring of neutral atmospheres on the early earth, and biochemists' favoring the reducing atmospheric conditions that allow for particular chemical reactions to proceed.

I think it is not overly provocative (comparing science to "myths") to describe the state of current research in the field in terms of Gould's words. But in any case, Gould's intent was almost certainly for the "myths" in question to refer to actual origin myths – the stuff of religious belief or folk tales or just bad history. If Gould's statement has this broader relevance, the epigraph requires both qualification and contextualization. While actual origin myths are indeed widespread and culturally of profound significance, the "we" in Gould's statement is not the pan-historical "we" of all humans. Rather the "we" is better understood within a particular trajectory of Western thought strongly influenced by a Judeo-Christian tradition prioritizing beginnings and endings of a sequential, linear history.

Furthermore, if Gould's formulation was meant to imply a dynamic *replacement* of myths by truths, the epigraph would imply an overly-simplistic narrative of the history of science. The narrative is that pre-moderns concoct myths that get replaced by truths when the data comes in. This narrative comports well with the secularization thesis and the central tenets of modernism. And it is just this narrative which is found at the beginning of most every book about the origins of life. The familiar incantation is that first there was religion, but now science is proposing answers to those very same questions that were once the purview of the transcendent or religious spheres. Finally, after science had developed to a sufficient extent, truths are found that replace the original myths. It is a widespread narrative indeed, and it is one that scientists especially are fond of.

That said, the secularization of the topic of life's genesis hasn't necessarily removed the topic's quasi-religious ethos. The rhetoric of life's origin sometimes exhibits the reverberations of a sacred pursuit, registering just short of mysticism. For some scientists in the field, the scientific pursuit of these questions hasn't quite emptied them of their transcendent worth or more ultimate meaning. Such devout sentiments might seem to be expressions of those overly-impressed with their own work. But that characteristic optimism and esteem for science is itself part of what science studies scholars take as data. The institutional optimism of natural science is nowhere more on display, or alternatively, more curious – given its spotty record – than in origins of life research, where all parties agree that no "answer" is within sight.

Of course the actual history of science cannot be regarded in such bi-modal categories fluctuating between myth and truth. One of Thomas Kuhn's central points in ([1962] 1996) *The Structure of Scientific Revolutions* is that it's not the same questions perennially being asked by scientists that eventually get resolved once and for all. The questions themselves, along with the standards of evidence, change over time. Familiarity with the historical record on origins shows just that: by virtue of the topic's changing affiliations with religious belief, with particular scientific figures of the day, and with particular institutional forces, the generation of life out of non-life *meant* something very different 150 years ago than it does for today's scientists.

The "modern" version of the science of life's origin is typically dated to the late 1920s, when Alexander Oparin and J.B.S. Haldane independently proposed materialistic, chemical-evolutionary scenarios for life's origin. The research community that arose surrounding the experimental work of Stanley Miller, Harold Urey, and later, Sidney Fox, Juan Oro, and Leslie Orgel, would then trace their roots to the Oparin-Haldane hypothesis, though their work extended far beyond the proposals from the 1920s. Only a few decades before this period, there was no real research program to speak of on the topic of life's origin. And yet by 1963, Freeman Quimby, invoking the cold war imagery of his day, declared, "the problem is ready to be attacked with mass intellectual artillery" (Fox 1965, 1).

My essay is concerned especially with what *makes* a question "ready" for scientific analysis. I am interested in how this project of deciphering the initial emergence of life became understood as a viable scientific project. My efforts are both historical and conceptual in nature, analyzing the principles and reasoning strategies that have been enlisted in origins of life science.

In what follows I will briefly review a few of the relevant historical developments in origins of life science before introducing my chapters in more detail.

I.2.

Spontaneous generation is the doctrine that life has, in the past, arisen from non-biotic initial conditions, and that it most likely continues to do so under certain conditions. Belief in spontaneous generation was the property of no one school of thought – it was held in various forms by Democritus, Aristotle, Augustine, Francis Bacon, Descartes, Buffon, and Lamarck.

As Farley (1977) and Strick (2000) have shown, the Victorian debates over spontaneous generation are hardly reducible to a series of experimental triumphs, demonstrating the undisputable truth of one cast of characters over the other. Rather, question-begging retorts and intractable experimenter's regress were documented among even those on the "right" side of the debate. What put an end to those debates was not just experiments. A richer historical picture reveals the shifting affiliations between scientific doctrines and social significance. Anti-clericalism, atheism, the developmentalist doctrine of preformationism, and Darwinism itself were all at stake in those disputes.

As a result of this complex interaction of experiments, arguments, and politics, spontaneous generation had lost its credibility by about 1880. Today, it shares disrepute with a few other well-known biological theories like vitalism or Lamarckism that one can safely invoke in criticism or derision. Contemporary textbooks can easily poke fun at

Redi's "recipes" for life from an admixture hay and wet clothes. But if, as a result of Pasteur's triumph, the generation of life from non-life had become a *bête noir*, then it remains, curiously, one with a research program devoted to it. If Pasteur and his supporters had generated a "law" that life always comes from previous life, then it was a law with at least one glaring exception! The contemporary science of life's origins typically takes for granted that life is not originating now on Earth or in practically available environments.[1]

The late 19th and early 20th centuries witnessed the establishment of the field of biochemistry, which was discovering the enormous complexity of living material. This time period produced relatively little scientific work on the question of life's origin. Following in the wake of the spontaneous generation debates was what Fry (2000) called "a dead end." Recalling this period, geneticist Herman Muller reported a sentiment that "the subject of life's origin is so taboo" and that it should be left untouched (Muller 1966, 494).

In the post-Darwinian era, the question of life's emergence from a pre-biotic Earth remained mostly theoretical in nature even for decades subsequent to Oparin and Haldane's 1920s chemical-evolutionary hypotheses. The field lacked much empirical evidence until Stanley Miller's iconic 1953 experiment demonstrating that several amino acids can easily be synthesized from what were thought to be primitive earth conditions.

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[1] Philosopher Carol Cleland has suggested that, in absence of classifications or theories of "alternate" forms of life – that is, life forms not based on our typically assumed carbon-based DNA – and- protein biochemistry – and in absence of instruments to look for them, then there could be other forms of life here on Earth without our knowledge (Cleland and Copley, 2005).

That work generated not only enthusiasm for possible route to life's origin, but established a model for future experimentation, investigating the possible origin of the organic ingredients for life.

Since the 1960s the field has exploded, fueled by popular interest, NASA funding, and ever more scientific findings. Today the field features diverse theories: protein-first theories, metabolism-first theories, replicator-first theories, ribosome-first theories, and iron-sulphur worlds. Diverse lines of empirical investigation, too, flood the pages of journals: inferences from fossil data, investigations of deep-sea vents, organic, inorganic, physical and biochemistry experiments, and exploration on other planets.

The field enjoys funding, theories, experimental data, and unfortunately, a reputation for having few broadly accepted conclusions and certainly not a proposed answer to the historical origin that is widely endorsed. One might be excused for asking what the problem is. Doubts about the field have been expressed not only by the usual Doubting Thomases in such matters, such as some religious factions which would preserve a space for God's special creation in the origin of life, but also by a number of philosophers and theoretical biologists themselves. No less an authority than Simon Conway Morris, the eminent paleontologist, reported at a recent conference that he had stopped reading the origins of life literature because field had made "no progress" in answering its central question.

Some have also thought that the field has a troublesome epistemic situation as a pursuit of a single, perhaps unique, historical event – a problem it shares in common with other origin problems, including those in cosmology. In terms of experimental methodology, the gold standard in the field is creating life in lab. Even if that were achieved, it's not obvious that it would reveal the historical pathway that life actually took. Others have simply been skeptical of the ability to actually achieve in vitro biogenesis: "It would be foolish to try to force nature in twenty four hours, with the aid of a bit of stinky water, that which it took her many thousands of years to accomplish" (1940, 189). (This is from someone who had an abiding interest in the nature of life, and who was quite influential to both Haldane and Oparin: Freidrich Engels.) But conversely, asking questions about a historical process does not demand recreating that process. Scientists are able to ask questions about cosmological history without creating any new universes.

The logical space of the field resides within what Garland Allen has called the pendulum poles of biological thinking. One pole is the mechanistic thought that construes biological entities as physical systems that happen to be arranged in particular, complex ways as a result of external forces. This perspective, underwritten by a logic of classical physics, often treats the origin of life as an extremely low-probability event that only became possible given the vast tracts of time elapsed before life's emergence. The other pole attributes to matter some sort of active or organizational powers that enable its development into ordered biological systems. This is the logic of elective affinity, of some versions of holism, and of dialectical materialism.

There remain many conceptual hurdles to scientific accounts of life's origin, of which I will here relate only two. One stumbling block is that most biopolymers (including amino acids and nucleic acids) are formed by dehydration reactions, where molecules of water are removed, in the process linking separate molecules. However, in fully aqueous environments where life is often thought to have emerged, such chemistry is exceedingly unlikely, because hydrolysis, rather than polymerization, is the reaction that is overwhelmingly favored (Pace 1991). There are possible routes around this problem, though, and in any case it is a problem for *any* account of biogenesis in a watery environment.

An even more threatening conceptual challenge is the science's confrontation with the ultimate chicken-and-egg problem. All of the life-like replicating systems we know of, namely, stretches of nucleic acids, require enzymes to facilitate their replication. But any enzyme capable of directing a reasonably high-fidelity template replication is a complex molecule that must be coded for by lengthy nucleic acid! The size of one restricts the size of the other, and there is a lower limit to their respective sizes (called the "error catastrophe" – designating the point at which replication fidelity is too low to continue reproducing). How these two functional capacities, both so basic for life, emerged in tandem with one another has been a longstanding problem in origins research. And that is why there has been so much welcome for the news from Thomas Cech (1986) that RNAs can catalyze their own replication: no chicken, no egg. The so-called RNA World (Gilbert 1986) hypotheses now constitute the most popular models in origins research. This research has correlated with improved understanding of the many different roles played by RNA in other realms of biology.

Some of the most promising contemporary work on life's origins involves the creation of self-replicating, self-sustaining populations of enzymatic RNAs. It has been suggested that these RNAs could serve as "model systems" for the further investigation of the RNA world (Lincoln and Joyce 2009). How this RNA world could itself have emerged, though, remains a very difficult problem, and even leading researchers concede the implausibility of stepwise construction of those systems in early earth conditions. It appears very hard to achieve nucleotide synthesis without the *deus ex machina* role of the scientist manipulating the experimental system and adding particular enzyme components (Orgel 1994, 60, Joyce and Orgel 1993).

I.3.

This essay investigates several important episodes in the history of origins of life science, as well as the its use of particular epistemic principles in its search for the solution to the problem of life's genesis. My first chapter address the conceptual history of the field by looking at the theory of one of the founders of the "modern" incarnation of the field – J.B.S. Haldane. His 1929 theory, developed in parallel with – but independent of – that of Alexander Oparin, proposed a chemical origin scenario that was later thought to be validated through the Miller-Urey experiment. Most scientists credit this Oparin-Haldane theory with providing the groundwork for 20th century work on the origin of life (Fry 2000). I provide a close reading and analysis of Haldane's 1929 article, "The Origin of Life", suggesting that the theory is best understood in a mechanistic, materialistic framework. It has not always been sufficiently appreciated that considerations about life's origin played a prominent role in theoretical debates in early 20th century biology. Many partisans in discussions of materialism, vitalism, and holism had some view on what their theory entailed for the topic of life's origin. It is impossible to extricate J.B.S. Haldane's theory from the context of those debates.

I suggest how J.B.S. became sympathetic to mechanistic thinking, tracing his intellectual development through two of his primary influences: first his father, John Scott Haldane, and then his superior at Cambridge, Sir Frederick Gowland Hopkins.

These two men represented opposing philosophical camps when it came to understanding life: J.S. Haldane had staked out an anti-mechanistic version of teleological holism that he understood as the best alternative to vitalism, and Hopkins was an influential advocate of mechanistic research in biochemistry.

While J.B.S. Haldane's theory is best understood as one grounded in a mechanistic and materialistic philosophical outlook, it is important to note the tentative grasp Haldane held on that philosophy at the time. I provide evidence that Haldane adopted such principles for pragmatic reasons in order to advance a scientific project, and that he was not at all convinced that mechanism was a viable philosophy. Indeed, he suggests that the mechanistic explanations provided by biochemists do not provide a "complete" account of the living world. This passage and others complicate a popular historical narrative which pits the younger Haldane against his father's philosophy. The relationship between their views is much more complex, and less antagonistic, than what many have supposed.

For example, one of J.B.S. Haldane's best-known students, John Maynard Smith, suggested that J.B.S. "spent his life trying to prove [his father] wrong." This narrative cannot account for, among other things, J.B.S.'s 1960 review of his father's scientific career, in which the author lauds his father's philosophy and says it has been misunderstood. J.B.S. wrote that his father's approach to biology was not "fundamentally false"; that it had "great heuristic value"; and that it was "not being applied as fully as it should be" (1960, 104). Furthermore, in a passage extolling the virtues of Marxism from an earlier (1938b) text, J.B.S. went out of his way to note that his father's approach to materialism "was not in conflict" with that of Marxism.

In tracing the genesis of (the younger) Haldane's theory, I also stake out some of Haldane's fluctuating philosophical commitments, distinguishing my own reading from others in the literature. For example, Sahotra Sarkar proposes a periodization of Haldane's work – distinct philosophical phases that Haldane traveled through. My suspicion is that any attempt to rigidly periodize Haldane's thought is bound to be problematic. Haldane was unabashedly and outspokenly "unsystematic" in his thinking. (One exception here is probably a period around 1940 when he strove to articulate and defend a coherent Marxist philosophy that would apply to his evolutionary thinking.) His various invocations of idealism, teleology, mechanism, materialism, and dialectical materialism probably go some way in accounting for why Haldane receives less historical treatment than other period figures who held more consistent ideologies, but this lack of stability has made Haldane an interesting (if complicated) character to study. Haldane's philosophy of science was, very simply, unsteady. He often proclaimed that he was "unphilosophical" and he held a relatively consistent philosophical skepticism; this does not imply, though, that he really did manage to somehow eschew philosophy in order to simply pursue science. All scientific pursuits are underwritten – however consciously – by implicit philosophical presuppositions. Haldane's proposal for life's origin was at root mechanistic, even though, at the time of its composition, Haldane harbored doubts about the sufficiency of mechanism as an all-encompassing philosophy of nature.

Haldane's political evolution has been a topic of some interest for historians concerned with the influence of Marxism in science, and the question of whether Marxist philosophy contributed to his theory of life's origin is still considered "an open question" (Fry 2000, 78), and I briefly enter into that debate as well. Loren Graham and Helena

Sheehan have both found evidence that Marxist principles of dialectical materialism had an important influence on the development of Haldane's and Oparin's "modern" origin scenarios. It is true that both men later became committed Communists, but I challenge the evidence Graham and Sheehan provide about dates for Haldane's "conversion", and find other evidence that suggests, to the contrary, that Marxist principles did not have a significant impact on Haldane's thought until at least the 1930s. While I sympathize with Graham's project of correcting what he sees as a historiographical bias against Marxist thought, I believe the interpretations of Haldane's origin theory go beyond the evidence.

My second chapter again focuses on J.B.S. Haldane, this time with an emphasis on the content and aims of his popular writing. That bears on the topic of this thesis because Haldane's origin of life theory first appeared in *The Rationalist Annual*, a publication of the Rationalist Press Association. His use of that venue as an outlet for a serious scientific proposal says something important about Haldane's views on the relationship between science and society. Its appearance in popular writing also testifies to the enormous cultural appeal of the topic of life's origin, which continues to make news headlines today based on popular interest. The fact that Haldane's theory of life's origin was introduced in the popular press shows that he took this forum seriously as a way to express and disseminate both scientific theories and a scientific world-view. It is worth inquiring further into exactly what that world-view consisted in, and what his aims were when he wrote popular articles about science.

Haldane used the popular press to convey a scientistic outlook, albeit one that is distinct from other views arising from his circle, for example, that of J.D. Bernal. Haldane urged that science could provide the resolution to social problems of all kinds.

He looked to the Soviet Union as a model of state-supported applications of science, and yet was wary of the abuses of power that could be manifest by any particular ideology that became state-sponsored.

> It may possibly be that as a result of that association [of science with the state] science in Russia will undergo somewhat the same fat as overtook Christianity after its association with the State in the time of Constantine. It is possible that it may lead to dogmatism in science and to the suppression of opinions which run counter to official theories, but it has not yet done so (1929, 161).

This was a prescient concern some fifteen years before the disastrous Lysenko affair. Haldane always wanted the scientific management of society to be for the good of people, and to be supported by the people. I describe Haldane's political view as "social technocracy."

I further explore the tension between science as essentially a pursuit of elites (the very ones who best know what science to pursue), and science as something valued, supported, and guided by democratic principles. I think this tension in Haldane's view of science parallels his self-understanding of his own life, as an aristocrat who would make a point to ride the coach class on the train. The thesis I develop is that in order to marshal scientific power in a way that was consistent with his desire to respect democratic principles, Haldane's scientistic outlook was imbued with the values he saw modeled in his own father's scientific work. His father's work was characterized by practical scientific service to miners, soldiers, and other members of the working class.

I pay special attention here to notions of equality and to the relationship between scientists and society. Haldane never was a believer in human equality, which he enjoyed explaining even on the grounds of Communism. "The dogma of human equality is no part of Communism… the formula of Communism: 'from each according to his ability, to each according to his needs', would be nonsense, if abilities were equal" (1949). Haldane consistently held that there were innate inequalities between both classes and races. While such views had clear ramifications for his thoughts on eugenics (see Paul 1984), I use such sentiments to illustrate some relevant facets of the relationship between scientists and society, especially the distinctive moral stature of the man of science.

My point in these two chapters about Haldane is not to imply that Haldane's theory was immediately taken up by the scientific community. In fact, neither his nor Oparin's work was widely cited immediately after their respective publications. Yet Haldane's stature as one of the century's most recognized geneticists does help explain, at least retrospectively, how the origin of life has become an increasingly legitimate scientific project. In the wake of Miller and Urey's famous electric spark experiment, the community of scientists surrounding the project of life's origins would look back to Haldane and Oparin as the founders of their field (Fox 1965).

My third chapter reviews the significance of the experiment widely considered to be the empirical inauguration of origins of life studies (Fry 2000). In 1953, Stanley Miller performed an electric discharge experiment under the direction of Harold Urey, who was then researching planetary formation. The experimental apparatus employed heated water (simulating the early ocean) and an "atmosphere" of reduced gases including methane, ammonia, hydrogen, and carbon monoxide, then subjected to an electric spark, simulating the energy input of lightning. After a week, the liquid contents of the apparatus showed that about 10% of the carbon from these inorganic precursors had been converted into organic products, including the detection of 5 amino acids.

Stanley Miller was reportedly in contention for the Nobel Prize multiple times for this landmark experiment, and I argue that it set a kind of standard for a burgeoning field of research: experiments under hypothesized early earth conditions is a common experimental form which structures much research in the field.

Miller's experimental results surprised many chemists of his day, and ushered in an era of optimism for the prospects of a scientific answer to life's origin. However, the results generated by Miller have had a fluctuating status, and it is not at all clear that they can be used to generate a viable origin scenario. Some still take the experimental results to be as relevant as ever, because they indicate the ease with which some biomolecules – including amino acids – can be synthesized in certain conditions. However, it has long been questioned whether the gaseous conditions necessary for the synthesis of those molecules accurately reflect those present on the early earth, and many atmospheric scientists consider the Miller results as wholly inapplicable to origins of life research. This controversy is an important case demonstrating the lack of principled epistemic resources for origins of life theorists to coordinate their theories.

My fourth chapter investigates the relationship between origins of life research and evolutionary biology. The boundary between these two sciences is not at all clear. On the one hand, "life" is often defined within this field as that which is capable of Darwinian evolution, and if that is the case then the *origin* of that entity would not involve processes subject to Darwinian evolution themselves. That being the case, there is a lot of incautious talk about any process leading to life being an "evolution", and about "prebiotic evolution" (Bada and Lazcano 2009) where such processes are not related to contemporary evolutionary theory. One begins to get the sense that all organic changes leading to life are sometimes lumped under the term "evolution", but that is to empty the term of any precise theoretical significance. Contemporary evolutionary theory requires a particular kind of replication (based on the template mechanism of DNA copying) in order to achieve Darwinian natural selection. At some point in the past those mechanisms must have been different from – and much simpler than – what they are now. What *kind* of replication is needed for something to evolve, then, and what sorts of physical entities can instantiate it? The relationship between units of life and units of evolution still requires further analysis.

In this chapter I show how origins of life research has both integrated ideas from evolutionary theory, and in turn contributed to a richer understanding of evolutionary processes. While the science of life's origins has had (at best) mixed success in answering its central question – how life first emerged from an abiotic environment – it has, in fact, contributed to theorists' growing sense of the need to refine an evolutionary theory whose fundamental explanatory resource is natural selection. I focus on four concepts – self-organization, neutralism, symbiosis, and autonomy – that have been deployed in the science of origins of life, and which have at least indirectly influenced the reception of the synthetic theory of evolution. The case study provides valuable insight into the ways that subdisciplines intersect with broader scientific projects, and the ways in which science can progress in unexpected directions.

My final chapter is an analysis of one of the heuristic principles appealed to in origins of life research called the principle of continuity. I briefly review other heuristic principles that are cited in the field, and which are meant to serve as methodological guides to research and as epistemic principles that help secure knowledge of this historical event.

The principle of continuity has a long history in natural philosophy, and I begin by tracing some of its various incarnations in Leibniz, and its later associations with both Lyell's and Darwin's gradualism. I proceed to argue that, in origins of life research, the principle is employed either in very non-specific ways that end up being trivial, or, when it has been used to entail more specific consequences, that it constitutes an unacceptable *a priori* hindrance to research. That is the case in origins of life research, and it has occurred in previous episodes in the history of biology as well. I conclude that the principle of continuity lacks the probative power that is often attributed to it, and that if origins of life must seek other heuristic principles if it is to integrate its diverse strands of research into a coherent narrative of life's emergence.

The disciplinary identity of origins of life has yet to be settled. My dissertation is therefore about an endeavor that is presently in the making. Unlike, for example, a science studies project on physiology, the identity of which is now firmly coalesced around particular figures, theories, and techniques, my focus on a young science has allowed an alternative perspective on the methods and principles enrolled in the acquisition of scientific knowledge. Those methods and principles are under contention, but appear to be needed to bring a sense of disciplinary focus and progress. This has permitted what I consider an important view into a field of science in the making.

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CHAPTER 1

J.B.S. Haldane between holism and mechanism

"J.B.S. loved his father, but spent his life trying to prove him wrong."
--John Maynard Smith

1.1. Introduction

Louis Pasteur's swan-neck flask experiments, conducted and communicated within three years of Darwin's 1859 publication of *Origin of Species,* purported to banish spontaneous generation from the biologists' explanatory toolkit. Those experiments, combined with some masterful rhetoric from popularizers like T.H. Huxley, convinced many scientists that life must always arise from previous life: *omne vivum ex ovo*. An apparent question then remained about how life may have arisen in the first place. Pasteur and Darwin both abjured on this point: it was evidently not a topic to be considered. Darwin was explicit about the potential for speculating about any such prebiotic "warm little pond." In an 1871 letter to Joseph Dalton Hooker, he wrote, "It is mere rubbish thinking at present of the origin of life; one might as well think of the origin of matter." For many decades, there was no research program dedicated to life's origins.[2]

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[2] One noteworthy exception was the work of Felix d'Herelle, whose work on the bacteriophage was recognized upon his 1928 appointment as the (first and only) Chair of Protobiology at Yale University. Other scientists in this period articulated various versions of *panspermia*, the notion that life had existed previous to its appearance on

Scientists' thoughts on the topic went beyond the agnosticism of Pasteur and Darwin. Several have been positively critical of any such search for how life itself might have began. Niels Bohr, for example, conceived of life as a fundamental property of matter, and something recalcitrant to analysis. "The existence of life must be considered as an elementary fact that cannot be explained, but must be taken as a starting point in biology" (Popa, p. 1). Francis Crick was sufficiently skeptical of the likelihood of the current theories could actually account for life on Earth that he wrote an article and a book postulating that intelligent aliens may have spread life here (1981). The philosopher Joseph Henry Woodger, a leader of the Cambridge Biotheoretical Group that included members such as Karl Popper, was also wary about investigating life's origins. Woodger's 1929 book *Biological Principles* was an indictment of many reductionist methodologies, especially within the new field of biochemistry. In that text Woodger endorsed the view of vitalist Hans Driesch that "the question about the so-called primary origin of life is… incapable of being discussed." Woodger's argument seems to be that, in order to arrive at a consistent just-so story, the biologist must posit into existence the relevant environmental and chemical conditions that they were in principle supposed to be investigating. "Such 'explanations,'" Woodger wrote, "have no place in serious scientific literature and would be unheard of in physics" (1967, 408).

Today the intellectual landscape has quite different contours. Scientists investigating origins of life might write grant applications to numerous federal funding agencies, and the question has a legitimate standing within scientific discourse. Though

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Earth, and that it has been deposited here by meteors. However, this theory was thought of as a way to avoid the question about origins rather than an attempt at an answer.

it lacks a common disciplinary home, origins of life, with its own society and journal, is on its way towards institutionalization. The object of research is commonly cited as "the fundamental question of biology" (Delbruck 1986, p. 31). But it is not just that scientists think of origins as an *important* question; they additionally believe it to be an *accessible* one. Freeman Dyson writes, "The subject is moving away from the realm of philosophical speculation and into the realm of experimental science" (1999, 11). What forces and events, then, are responsible for this transition?

In what follows I hope to shed light on this question through an analysis of John Burdon Sanderson Haldane's 1929 treatise "The Origin of Life", which is today regarded (along with Oparin's 1924 article by the same title) as the "starting point" (Fry 2000) in contemporary discussions of origins. I argue that Haldane's adoption of a mechanistic explanatory framework was largely a result of the influence of Frederick Gowland Hopkins, and that this philosophical stance informed Haldane's research and writings at certain points in his career, including, crucially, his theory of origins. This is based on historical considerations of those who influenced Haldane, and on a close textual reading of his theory of origins, in the context of his broader corpus. I will then distinguish my own view of Haldane's philosophy of science from other influential interpretations of his work.

1.2. Context

The early 20th century saw landmark developments in many areas of biology. Biology and its subdisciplines were achieving varying degrees of institutionalization, and were amassing an impressive storehouse of knowledge. Through many of these advances, however, there was sometimes perceived to be a relative lack of systematicity, framework, and theory. J.B.S. Haldane wrote that "At present much of biology is in the stage of measuring and waiting for the idea" (1928, 140). Woodger was concerned that the state of biology in 1929 could come to resemble, invoking Whitehead's memorable phrase, "a medley of *ad hoc* hypotheses" (1967, 27).

A variety of theoretical debates, some quite old, structured the many biological findings. These included an ongoing dispute between vitalism and mechanism. Both terms would be used as labels for a wide spectrum of beliefs, an analysis of which is beyond the scope of this essay, but there was some continuity in the notion of mechanism and mechanistic explanation, which involved an analysis of an organism's component parts and their interactions. Mechanists would often trace their intellectual roots back to Descartes, whose understanding of corporeal substance was thoroughly mechanistic. Mechanistic analyses could signify either epistemological or ontological commitments: insisting that the best explanations of biological systems are mechanistic ones, or that living systems just *are* complex mechanisms themselves.

One widely influential mechanistic scientist in the early 20th century was German-born physiologist Jacques Loeb. Loeb used his scientific work on artificial parthenogenesis in sea urchins to support a philosophy that all life processes "can be unequivocally explained in physico-chemical terms" (1912, 3). He argued that laboratory analysis would eventually reduce all aspects of living organisms to the laws of physics and chemistry. That implied both a *reduction* of biological phenomena to the level of ultimate particles, plus the emphasis on certain *techniques* of analysis, namely the experimental methods of physics and chemistry. This attempt to reduce all biological processes to matter in motion reflected the earlier 19th century mechanistic materialism of

Loeb's German teachers, such as Julius Sachs and Wilhelm Roux, who looked to physics and chemistry as model sciences that biology should be emulating (Allen, 1975). Loeb's interpretation of mechanism also had consequences for researching the origin of life: "chemical reactions which take place in living organisms can also be repeated at the same rate and temperature in the laboratory…Nothing indicates, however, at present that the artificial production of living matter is beyond the possibilities of science" (1912, 5).

A host of objections were launched against such mechanistic interpretations of biological entities, and vitalism was among the most potent and long-lived. Vitalists insisted that mechanist explanations were at best incomplete, and at worst fundamentally misguided. Vitalism could serve as an in-principle opposition to mechanism, or alternately as a check to the progress of the mechanist's program, which was sometimes hasty in its simplification of complex systems (Bechtel and Richardson 1993). But the burden of evidence was often thought to reside on the side of the vitalists to show what other forces or principles were involved with life that the mechanist story left unaccounted. By the early 20th century, the multiple revisions that were forced upon the vitalists by new experimental evidence eventually proved embarrassing to the program, and the flag of vitalism would sustain decreasing numbers of advocates.

But even after vitalism was waning there appeared new concerns over mechanism, based on the need to analyze organizational relationships between parts, rather than just parts in isolation. Scientists in various locations would emphasize the need for new methods that did not destroy the phenomena of interest through the inevitably lethal experimental techniques of biochemists. These concerns are present in the work of Claude Bernard in the late 19th century, and were also articulated by

Lawrence Henderson and Walter Cannon in the 20th century. The latter considered themselves disciples of Bernard, and both investigated the organization of systems of dynamic equilibrium – processes that could bring about the efficient regulation of a body's interacting parts. Both believed that the interactions and processes of biology must be understood in the context of larger, organized systems, and shared the view that reductive, mechanistic science overlooked that point. The concern was that some higher level interactions were not derivable from lower-level processes.

Often, the diverging views on the prospects of mechanistic biology corresponded to different research programs. Holists often studied higher levels of organization, either organismal biology or physiology, while mechanistic materialists often studied biochemistry and biophysics. But the different philosophies were not just a difference in the level of analysis. For example, Henderson focused on the chemical level, but not in a mechanistic fashion, and it was also possible to study the whole organism in a mechanistic way. Mechanism and holism were different philosophies, not just analyses of different levels of nature.

One of many scientists initially attracted to vitalism was the Oxford physiologist John Scott Haldane, a member of a prominent Scottish aristocratic family who, by the end of his life, was remembered nearly as much for his philosophy as he was for his contributions to the understanding of respiration. Haldane was an Oxford physiologist whose best-known scientific work was on the role of carbon dioxide on the regulation of breathing. Among his many innovations were improved gas masks used in the World War, the decompression chamber for divers and submarine officers, and bringing canaries into coal mines, whose high metabolisms made them useful indicators of unsafe mining damps.

Haldane's deepest ideological commitments appeared to be a political sensibility seeking the welfare of all, as well as favoring collective responsibility over the individualistic philosophies that had underwritten many of the developments in 19th century laissez faire political economy. Haldane was committed to a higher social reality that transcended the individual, and was suspicious of both utilitarian moral theories that prioritized the individual, as well as the empiricist philosophy that was implicated in the use and spread of utilitarian ideals (Sturdy 1987, 33). Several of Haldane's peers expressly opposed the thought of both Herbert Spencer and liberal economic doctrines arising from the Manchester School (Seth and Haldane, 1883, 126-158, 187-213, 214-245).

When other frameworks and systems of belief came into conflict with his social and political ideals, they were rejected. According to Steven Sturdy (1987), that is how J.S. rejected both a common Scottish interpretation of Darwin and organized ecclesiastical authority together at once. Darwinism was quickly assimilated in Scotland's Calvinist climate, where, far from engendering atheism, the new evolutionism was often taken to bolster religious belief. The progressive tendencies generated through the mechanism of evolution, it was argued, revealed the pre-established moral and physical order of God's universe. Haldane was disillusioned with the capacity of orthodox religious movements to seriously treat the pressing issues of his time. Haldane saw the forces of religion and mechanistic science together as complicit in the problems manifest in contemporary Scottish culture, and working against the social good. While rejecting the church, Haldane also rejected the kind of science implied by this world view.

While Haldane briefly supported vitalism, he soon relinquished that support due to his recognition that experimental work seemed to obviate the need to posit any vital principle. But mechanistic materialism was hardly the only other option on the table.

Through his education and training, J.S. Haldane became aligned with Scottish Idealism, that movement denoting an uptake of the German idealists – specifically Kant and Hegel – in Scotland. With his brother Richard, J.S. published an article on Kant's epistemology in the anthology later considered the manifesto for Scottish Idealism, the (1883) *Essays in Philosophical Criticism*. The preface to that volume was penned by the most prominent and influential representative of Scottish idealism, Edward Caird, who praised the abilities of this transplanted German philosophy "to meet the questions of the day" and "to be brought to bear on the various problems of science, of ethics, and of religion, which are now pressing upon us" (1883, 3). The anthology was dedicated to the recently deceased T.H. Green, whose own brand of idealism, and critiques of empiricism, were widely read by the Scottish idealists.

In their contribution to the *Essays*, the Haldane brothers did not deny that biological processes were fundamentally material processes, but argued that separate categories of knowledge were required to assess differences between living and non-living reality:

> The distinction between what lives and what is mechanical substance is a distinction of point of view and not of objects in space… When we see a house and a man we may certainly distinguish them as inanimate and animate. But this only means that the man is naturally considered in a way in which the house is not (1883, 52).

Kant's thinking, especially as recorded in the second part of the Critique of Judgment, was widely influential for many biologists. There, Kant develops the thesis that, in addition to perceptions of causal processes taking place in space and time, categories of experience also include distinct judgments of beauty and of purpose. J.S. Haldane was critical of several of Kant's tenets, but he did argue throughout his career that self-preservation and self-expression were essential aspects of biological phenomena. In virtue of their teleological nature, then, organisms could not be understood mechanistically. For J.S., there was a wide gulf between the interpretation of inert material mechanisms, and the interpretation of organisms. Thinking about organisms in terms of mechanisms was a category mistake.

J.S. found one source of inspiration from Immanuel Kant, and another from more recent trends in philosophy and biology under the title of "holism." He adopted and further articulated his own version of teleological holism. Haldane cited South African General Jan Smuts as the originator of the term, which Smuts used to articulate a complex process by which parts come together to form larger entities.[3] According to this doctrine, whole organisms could not be understood merely in virtue of their component parts; any study of individual parts that was divorced from an understanding of the whole organism would be in vain. The idea here is often summarized as, "the whole is more than the sum of its parts." Furnished with the ideas of teleology and holism, Haldane had found principled objections to mechanism without the need to posit any vital principle.

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[3] As a charter member of both the League of Nations and the United Nations, the connections between Smuts' biological theories and his political ideals is intriguing.

The living world was distinguished not by any metaphysical "magic spark" or *élan vital*, but its objects fell under a different category of human perception. Haldane's teleological holism was rooted in an epistemic pluralism which could accept ontological monism.

For J.S., the problem with construing organisms as additive mechanistic systems was that significant biological knowledge would elude the scientist. The causal processes traced out by mechanistic accounts were inadequate because they failed to see that the processes under consideration were *for* the overall preservation of the organism's functional unity. The transfers of energy between organism and environment that were (successfully) elucidated under the mechanistic view were not random, but *directed* for the organism's good. Life was a coordinated maintenance of the self, and that active principle was constitutive of the living state, no matter how simple or complex the organism: "This is just as true of the most complicated actions of the human body as of the movement of the amoeba towards a source of nourishment" (1883, 54). It didn't matter what *kind* of activity was under consideration; *all* processes of life shared in common this feature, and it was entirely separate from the dynamics of inert physical and chemical bits of nature.

For the purposes of this paper, it is crucial to note that J.S. Haldane's philosophy of biology precluded the analysis of life as something that originated from chemical precursors. "The rejection of the mechanistic conception of life carries with it the rejection of the theory that life has originated out of mechanical conditions" (1935, 72). A cumulative change from non-life to life would, for Haldane, amount to acknowledgement of just the sort of materialist, mechanist conception of life he was at pains to deny. Life was, for J.S. Haldane, manifest as "persistent coordinated maintenance of structure." The "persistent coordination," as a function of the entire organism, drew on holism. The "maintenance of structure" may be recognized as a teleological principle closely related to function or adaptation, and as an organism's striving to express itself.

> It has been suggested by certain philosophical writers… that when a certain stage of physical and chemical complexity has been reached, living organisms "emerge" as something new. I confess that, as a physiologist, I can attach no meaning to this suggestion. Not complexity, but persistent co-ordination, is what distinguishes biological from physical interpretation. An "emergence" of life out of a Newtonian world would be a quite unintelligible miracle (1931, 36).

It is not clear that J.S. Haldane would have been interested in the notion of life's genesis generally, or if he believed life was an eternal principle, and it is equally unclear what his vision for such a research program might look like. But he was very clear that any research program assuming inert matter as the starting point for the emergence of life was in-principle objectionable. "There is and can be no origin of life out of mechanical conditions. Such an origin is inconceivable" (1930, 12). For J.S. Haldane, life's existence was a brute fact that was not resolvable through further analysis. "There is no explanation of life: it just appears to us as a fact in our experience, and a fact which is essentially inconsistent with a mechanical conception of our visible and tangible experience."

The denial that life is a more complex version of inert matter, severely constrains an investigation of life's appearance. J.S. seems to have found a principled reason to believe there just *is* no further explanation for life, while the mechanistic outlook sought further explanation for life. Whatever its worth as a philosophical stance, the mechanistic perspective allows and encourages a further scientific research program. In the case of the origin of life, J.S. Haldane's teleological holism, not unlike theistic special creationism, puts a limit to the reach of scientific research. At least among scientists, these disputes have played out largely along the lines of pragmatism rather than as specific philosophical resolutions. One perspective allowed research while the other didn't; the very *ability* to extend empirical investigation has been highly valued by scientists – whether or not such investigation provides the "answers" that were being sought.

1.3. Transition

J.S. exerted a dominant influence, both personally and intellectually, on his son, John Burdon Sanderson. J.B.S. Haldane took on many of his father's attitudes towards science, its role in social change, and even his relentless self-experimentation (both were their own favorite guinea pigs, regularly inducing loss of consciousness, wheezing, and vomiting, ostensibly for the sake of more accurate physiological experimentation). J.B.S. seems to have adopted a substantive portion of his father's views on theoretical biology as well. We have an early view into his philosophy of science in his 1923 book *Daedalus, or the Future of Science*. This speculative, forward-looking volume was a product of the post-war left-wing intellectual circle that included the likes of Bertrand Russell, T.S. Elliot, and Julian and Aldous Huxley. Aside from the book's speculations about controversial technologies, there is also considerable philosophical reflection, in particular concerning the limited prospects of mechanical materialism.

> Kantian idealism will become the basal working hypothesis of the physicist and finally of all educated men, just as materialism did after Newton's day… A time will come when physiology will invade and destroy mathematical physics… we are in for a few centuries during

> which many practical activities will probably be conducted on a basis, not of materialism, but of Kantian idealism (in Dronamraju 1995, 27).

Commenting on this book several decades later, Freeman Dyson considered this excerpt "the weakest section of the book." If the measure of "strength" is predicting the future, perhaps that is true, but the passage is nevertheless revealing of Haldane's conception of science. J.B.S.'s scientific work prior to this period primarily dealt with human physiology, the science of his father. J.S. thought of physiology as distinctively biological, while "bio-physics" or "bio-chemistry" rested on a misunderstanding of the unique nature of biological organisms. There seemed, then, to be an appropriate fit between Kant's distinct categories of perception and the Haldanes' notions of the place of physiology as a distinctively biological science, appealing to a separate class of explanations than those used in physics or chemistry.

From 1923 to 1932, J.B.S. held a position as Sir William Dunn Reader in Biochemistry at Cambridge under Frederick Gowland Hopkins. When J.B.S. accepted his new academic post, he remained more or less under the direct influence of his father's views. In 1922 he wrote to Hopkins: "It is only fair to say that most of the ideas which I have on physiology are really [my father's]. I tend to think of physiological questions primarily in terms of 'milieu interieur' rather than metabolism, thanks to him and Claude Bernard, and this enables one to see problems which from the point of view of method, are much simpler than many metabolic ones" (cited in Sarkar 1992). But J.B.S. would shortly relinquish much of his anti-mechanistic thinking while working alongside Hopkins.

F.G. Hopkins' institutional contributions to the discipline of biochemistry were paramount; he did as much as anybody to consolidate and promote his nascent field of research. He was an outspoken advocate of the burgeoning field, and he argued for its methods' applicability to biological processes. Against the claims of detractors, who argued that biochemistry was a circumscribed exercise into mere chemical facts, Hopkins sought to show that biochemical research can yield what is "essential to the fundamental manifestation of life."

> There are biologists with philosophical leanings who still suspect that biochemical facts are of chemical interest only… the chemist on the other hand hopes to gain real understanding from his own standpoint of whole organisms through his study of their parts (1936, 260).

It is no mystery to whom Hopkins was alluding here: he in fact cites J.S. Haldane frequently as a recalcitrant opponent of mechanism. Hopkins took Haldane's reservations seriously, but also sought to show why the latter's opposition to mechanism (and, indeed, to biochemistry in general) was misguided.

> In all cases structure, internal events and functions must be thought of as inseparable if we are to grasp the essential nature of such systems; but as every one will admit we could hardly obtain that grasp if we had no knowledge of the parts to subserve our mental synthesis (Needham 1949,186).

In this passage, from "A Lecture on Organicism", Hopkins concedes to the holist that it is indeed the aim of the various biological sciences to understand the nature of the whole organism, and to not lose sight of that aim throughout its various activities. But a requisite part of that goal, Hopkins argued, is the study and comprehension of the individual parts from which the organism is composed.

Such a reply still leaves plenty of room for dispute with an interlocutor such as J.S. Haldane: Is any such "mental synthesis" from parts to whole possible? And if so, how? Further, the notion that we can put the pieces together to comprehend the whole may still rest on controvertible assumptions about the nature of living things, such as the nature of complex causes or mechanisms that may very well be altered by their context as part of a larger whole. On the other hand, Hopkins' reply echoes with that of other mechanists before and since his day: mechanism allows for further research to proceed, and other philosophies (they argued) only tend to impede the progress of science. The program of the holist, goes the claim, is in fact no research program at all. It was not always a principle to be argued against, so much as an impediment to overcome simply by extending mechanistic research beyond a point that others had achieved.

Working under Hopkins had a profound affect on J.B.S. Haldane, whose science and philosophy would both undergo modification during his time at Cambridge. Not only Hopkins' influence, but also the results of his own science, would eventually secure a more favorable view of mechanistic explanations for Haldane, just as the role of adaptation, as a central unifying or "coordinating" characteristic of organisms, was attenuated. The reasons for this are manifold. For one, Haldane was then pursuing genetic studies of the quantitative effects of natural selection, so that eventually he came to see the central principle of biology, if there were one, to be natural selection, rather than any teleological or holistic notion of organisms.

Also, the fact of Haldane's success at using relatively simple mechanistic explanations within his own work further confirmed the possibilities of mechanism (Sarkar 1992, 390-394). In particular, his work on the kinetics of enzyme action adopted a biochemical perspective that was easily amenable to a mechanistic interpretation. With Briggs (Briggs and Haldane 1925), he proposed an important modification to the Michaelis-Menten theory of enzyme kinetics. Briggs and Haldane dropped the assumption that the enzyme-substrate reactions were in *equilibrium*, which meant that the rates of forward and backward reactions were equal, and that the net reaction rate was 0. Instead, they proposed that enzyme-substrate reactions were in a *steady state*, where the rate of change of reactions was constant (the overall reaction rate need not be 0). There could be many processes going on in the system, but there is some sort of flow through the system securing the constancy of certain variables. This work does not mean that Haldane was committed to mechanistic materialism, but did instill a tendency to think that biochemical explanations were of ultimate significance in biology.

By 1927, J.B.S. remained sympathetic to Kant's biological views, but seemed to express them in terms of the intuitions of the physiologist. The notion of adaptation was a *heuristic* principle, but it hardly held the axiomatic significance that his father would have bestowed upon it (or to the related notion of teleology). Haldane wrote that mechanistic investigations had so far been successful, but that the processes illuminated by such investigations are indeed coordinated in a way that is characteristic of living things.

> Thus we cannot avoid speaking of the function of the heart, as well as its mechanism… However mechanistic [the physiologist's] standpoint, he must use the idea of adaptation at least as a heuristic principle… At present, with Kant, we are compelled to leave open the question 'whether in the unknown inner ground of nature the physical and teleological connection of the same things may not cohere in one principle; we only say that our reason cannot so unite them' (1928, 128).

The physiologist will still feel an intuitive tug to focus on the significance of adaptation, but experimentation guided by mechanistic thinking could proceed unhindered.

Only a short while later, J.B.S. would be reminding students that, if they weren't a pupil of Hopkins, they may be likely to forget that "the only precise account of the most fundamental phenomena which he studies must ultimately be a biochemical account" (in Clark 1984, 100).

It was in this context, and with a newfound willingness to countenance a mechanistic understanding of life's basic principles, that Haldane formulated his seminal theory of the origins of life.

1.4. A "modern" theory

J.B.S. Haldane's interests were not limited to physiology and biochemistry. Indeed, his formal education at Oxford comprised degrees in mathematics and classics, which is just one indication of the breadth of his intellectual pursuits. He was also interested in broader cosmological ideas, and he was particularly well-suited to address the topic of life's origins. He did so in an 8-page treatise published in 1929 within *The Rationalist Annual*. It is important to note that at the time there was no research program to speak of devoted to life's origins. The discussions that J.B.S. would have been familiar with arose from essays on the consequences of various philosophies of biology. Although it may appear as a one-time bit of speculation simply for the sake of popular writing, it is impossible to separate Haldane's hypothesis from those discussions of vitalism, mechanism, and holism.

In order to motivate a solution to the problems of life's origins, J.B.S. proposed an analogy. (As it happens, this was in accord with his own instructions from his article, "How to Write a Popular Science Article."[4]) Just as Dubois' discovery of *Pithecanthropus* remains – the purported "missing link" – showed that our categories between human and ape were fluid rather than fixed, so also did Felix d'Herelle's discovery of the bacteriophage show that the boundaries between life and non-life are not absolute categories, but rather degrees on a spectrum. The bacteriophage was part of the category that had earlier been known as "filter-passers" – entities so small that they could survive filtration through ceramic filters. Haldane went on to outline the debate about whether or not the bacteriophage should have been admitted to the status of the living.

> On this view the bacteriophage is a cog, as it were, in the wheel of a life-cycle of many bacteria. The same bacteriophage can act on different species and is thus, so to say, a spare part which can be fitted into a number of different machines… A great many kinds of molecule have been got from cells, and many of them are very efficient when removed from it (Bernal 1967, 245).

Haldane is here highlighting the fact that parts of cells can be understood as retaining independent function even when physically removed from their context within the organism – an unsavory proposition to a holist, indeed. The repeated use of mechanistic language testifies to his theory's philosophical presuppositions. Based on the unsettled debate over the phage's status, and also our ability to conceive of the phage as a "cog in a machine," Haldane thought it safe to "legitimately speculate" on the question of life's origins.

For his environmental constraints, Haldane draws especially on the work of English chemist E.C.C. Baly, whose work had shown that, in atmospheres without

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[4] In that (1927) essay, Haldane recommended writing in short, declarative sentences, writing in an active voice, making the material personal, and making it relevant to peoples' lives by linking it with contemporary news items.

oxygen, the exposure of certain chemicals to ultraviolet radiation could produce organic products. Haldane's insight was to connect this research with a new hypothesis about life's origin. An early Earth devoid of photosynthesizing organisms would have lacked oxygen. Without a protective ozone layer, high amounts of UV radiation would have bombarded the surface as the planet was still cooling.

> When ultra-violet light acts on a mixture of water, carbon dioxide, and ammonia, a vast variety of organic substances are made, including sugars, and apparently some of the materials from which proteins are built up. Before the origin of life they must have accumulated until the primitive oceans reached the consistency of a hot, dilute, soup… The first precursors of life found food available in considerable quantities, and had no competitors in the struggle for existence. As the primitive atmosphere contained little or no oxygen, they must have obtained the energy which they needed for growth by some other process than oxidation – in fact, by fermentation… The first living or half-living things were probably large molecules synthesized under the influence of the Sun's radiation… The cell consists of numerous half-living chemical molecules suspended in water and enclosed in an oily film. When the whole sea was a vast chemical laboratory the conditions for the formation of such films must have been relatively favourable… (Bernal 1967, 247).

The result, where organic products slowly accumulated in an ocean, is what Haldane dubbed a "hot dilute soup." It has proved to be a memorable and long-lasting metaphor -- nearly a term of art now used to designate any class of theories that postulates the widespread accumulation of organic products on the early earth. Because of his reference to reproducing viruses, Haldane's scenario is a prominent early example of what might now be referred to as a "gene-first" tradition in origins of life theorizing, contrasting with alternative "metabolism-first" theories. Haldane made specific predictions to constrain his chemical hypothesis, which were adopted by later scientists for quite some time (Miller 1953), included the presence of a reducing atmosphere that included carbon dioxide, a heterotrophic (rather than autotrophic) organism that derives its energy needs from other organic substances in the environment, and an early organism as a *universal* ancestor which could explain the uniquely asymmetric macromolecules found in (nearly) all living things.

A crucial theme that Haldane emphasized in these passages is that of *continuity* between non-life and life. It states that there is no metaphysical or epistemic gap separating the chemical world from the biological; living processes are to be accounted for in terms of the organized complexity of chemical components. This continuity, it seems, would be ruled out by any philosophy which construes a biological organism in a fundamentally different category from other types of matter. In order to accomplish that, Haldane needed to clear a hurdle presented by, of all people, Pasteur. That's because Haldane took one the side-effects that Pasteur's ostensible triumph over spontaneous generation to be an increase the sense of a separation between matter and living systems (in Bernal 1967, 242).[5] That categorical discreteness had to be broken down for Haldane to posit this emergence of life from a chemical world.

Yet another contemporary intellectual strand of discreteness was associated with the 1926 Nobel Prize awarded to Jean Baptiste Perrin for experiments leading to the acceptance of atomic theory, positing the discontinuous structure of matter. Always

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[5] It is not clear whom Haldane had in mind with this statement that Pasteur had given solace to those who strove to separate mind and matter. A good guess, though, is that he was referring to someone like Herbert Carr, who in another 1929 article uses Pasteur to articulate the vitalistic thesis that "life is original and matter derived." Carr argued that since all life inherits its active power from previous life, there is a fundamental gap between inert matter and living matter. "[T]here is the undoubted scientific fact that every living thing is generated from a living thing and no single instance is known of life arising spontaneously out of inert matter, or of matter itself producing life. The biologist accepts the principle that life can only originate from life as axiomatic."

attentive to size and its significance to biology, Haldane used matter's discreteness to focus in on a more precise metric for life's origin. He reasoned that, before the size of the atom was known, there was no reason to doubt Jonathan Swift's nursery rhyme:

> Big fleas have little fleas
> Upon their backs to bite 'em;
> The little ones have lesser ones
> And so ad infinitum.

Following the updated atomic theory, though, there was a principled way to stop such a regress. Following the size scale downward from flea to bacillus to bacteriophage, one next arrives at the atom, "and atoms do not behave like fleas…The link between living and dead matter is therefore somewhere between a cell and an atom" (Bernal 1967, 244). Haldane noted that the phage displayed some crucial properties of life but not others, and so the *scale* of minimal life was proposed to be near, or just above, that of a phage virus. When it comes to functional traits such as reproduction and metabolism, size matters, and Haldane helped focus attention on the biochemistry of what a minimal organism would be able to accomplish.

Haldane acknowledged that evolution could not explain life's origins. Yet one can recognize in Haldane's theory the influence of a slow, steady progression, which could, over time, result in complexity. This progression included *intermediate forms* that were between mere chunks of organic material and thriving organisms. It would have a familiar ring to a generation of scientists freshly working out the details of natural selection. Just as intermediate forms would help break down the categories of immutable species, the intermediate forms enlisted by Haldane were supposed to break down the even more rigid categories of life and non-life.

How could that be done? One crucial step made by Haldane was to separate scientific work on life's origin from philosophical problems about mind's relationship to matter. Haldane frequently portrayed himself as "anti-philosophical", and this is one case where he wanted to set philosophical problems aside in order to advance a research program.

> Personally, I regard all attempts to describe the relation of mind to matter as rather clumsy metaphors. The biochemist knows no more, and no less, about this question than anyone else. His ignorance disqualifies him no more than the historian or the geologist from attempting to solve a historical problem (Bernal 1967, 249).

The philosophical question about the nature of mind did not immediately impinge upon the biochemist's right to pursue this important historical question: the biochemist could safely shed this particular concern and focus on the chemical details – details made increasingly clear through Haldane's elucidation. Haldane sought to remove much philosophy from the question of life's origin. The problem was essentially *historical* and it was a problem for scientists to work on. The vernacular for the topic of origins was more about organic chemistry than about mind or consciousness.

My reading of Haldane's attempt to secure a domain of mechanistic research in biology separate from the domain of speculation about mind and consciousness is supported by another passage from his (1932) book, *The Causes of Evolution*. "The world is full of mysteries. Life is one. The curious limitations of finite minds are another. It is not the business of an evolutionary theory to explain these mysteries." Evolutionary theories weren't necessarily attempts to resolve the "mysteries" of human minds. Mathematical, biochemical, and evolutionary investigations were best applied only to particular parts of the world, and not necessarily to consciousness. This reading helps to preserve a space for some agreement between J.B.S. and his father. While there were significant disagreements about how to investigate living entities, J.B.S. appeared to share his father's suspicions about any reductive, mechanistic approach to the mind. The differences between father and son were not as severe as some have supposed.

Nowhere in his account did Haldane address the question of chance versus necessity, which today provides for a great deal of philosophical speculation (see Fry 2000, White 2007). He was attempting a merely plausible naturalistic scenario by which prebiotic conditions eventually gave rise to primitive organisms, in a manner consistent with the view that "a thousand million years ago matter obeyed the same laws that it does today" (Bernal 1967, 249).

Haldane accomplished several ends with his theory. He employed a mechanistic understanding of biological systems as a tool with which to formulate the problem of life's origin. Life was to be understood as the organized arrangement of chemical parts: "…a suitable assemblage of elementary units was brought together in the first cell" (Bernal 1967, 247). Haldane established the topic of life's origin as one for biochemists to work on; and he marshaled recent microbiological and atmospheric evidence to inform his theory. Further, he strove to divorce the discussion of life's origins from related controversies dealing with the relationship between mind and matter.

J.B.S. Haldane's combination of scientific acumen, sweeping range of interests, willingness to speculate, and ability to use the popular press to communicate significant scientific ideas, all contributed to his particular theory, and has led to his stature as what J.D. Bernal calls an "originator of the modern view" of life's origins. I have sought to show how and why his philosophical stance on debates over the nature of life and its explanation has contributed our knowledge of life's origins.

1.5. Origins and Haldane's philosophy of science

My account has situated Haldane's theory in a milieu of debates over mechanism and its discontents, and I have suggested that it is best to understand Haldane's theory as one whose primary philosophical basis lay in mechanistic materialism. My thesis is at odds with those who have noted that both "fathers" of modern origin scenarios – Oparin and Haldane -- were committed Communists, and further that their theories were products of their familiarity with the Marxist tenets of dialectical materialism. In what follows I argue that evidence for Marxist inspiration in Haldane's theory is lacking, and that, in any case, Haldane was surrounded by ample intellectual resources that would account for his own formulation of "The Origin of Life." I will begin with a brief summary of what I take to be the salient features of what is referred to as "Marxist philosophy of science."

In *Anti-Duhring* and *Dialectics of Nature*, Engels argued for a materialism grounded in specific episodes in the history of science. In particular, Engels cited the discovery of the cell, demonstrating the unity in the living world; the law of conservation and transformation of energy, revealing a kind of continuity in nature's processes; and the discovery of biological evolution, establishing the natural origins of human history. But against other versions of materialism, Engels wanted to distance himself from any metaphysics that construed nature as static, rigid, or fixed. He believed that previous versions of materialism held matter as a passive or inert substratum, only set in motion by an external impulse.

Against that materialism emphasizing separateness, Engels desired to emphasize the motions, interrelations, beginnings, and endings of things. Engels believed the "new" sciences (especially evolution) were about development, process, and the interrelations of things. This new science demanded a new philosophy to replace the kind associated with 18th century sciences, which he understood as sciences of classification and collection of discrete entities. The philosophy he derived centered on three "laws." These laws were more general than those applying to one particular science – they were prevailing patterns in nature that all of history and thought were supposedly subject to.

The first law was the transformation of quantity into quality and vice versa. Against reductionism, this principle stipulates the emergence of novel properties at certain critical points. It was a way to make sense of qualitative differences without problematic distinctions of the kind Kant needed in biology. An example here was supposed to be the periodic table of elements, where (quantitative) differences in atomic weight had ramifications for the organization of all matter through its distinct chemical properties. The second law was the interpenetration of opposites. This entailed that matter was not an inert and homogenous mass, but contained within it organizing principles and active powers: development was inherent to matter itself. The third law of dialectics was the law of the negation of the negation, which was supposed to describe the synthetic nature of dialectical movement. While everything carried within itself the conditions of its own destruction, there was also a simultaneous regeneration of the new.

The relationship between the Oparin-Haldane hypothesis and Marxist philosophy has been a point of controversy ever since C.H. Waddington remarked that future historians would necessarily notice that the two men who advanced theories describing life as a natural development of the physical world – what amounted to a "revolution in thought" – were both Communists (1968). The suggestion implicit here, and made explicit by other historians, is that the tenets of dialectical materialism were a helpful way to resolve the apparently conflicting aspects of life represented in longstanding debates over mechanism and vitalism or idealism.

Most notable in this tradition is the work of Loren Graham, who has stressed that both Haldane and Oparin became dialectical materialists and declared that Marxism played an important role in their biological work.[6] Graham writes that both Haldane and Oparin were already under the influence of Marx when they formulated their theories, and became increasingly devoted to Marxism over time.

While not identifying as a Marxist himself, Graham takes his analysis to be a corrective to a historiographical tradition which has discounted the significance of dialectical materialism. In particular, Graham regrets the recurrent association of Marxist science only with the disastrous Lysenko Affair – referring of course to Stalin's support for Trofim Lysenko's Lamarckian breeding program, and the subsequent (lethal) suppression of scientists espousing classical genetics, deemed contrary to Soviet principles for its association with biological determinism. Graham notes, with good

[6] Graham's thesis is cautious. He notes that both Oparin and Haldane were *primarily* influenced by Marx only *after* the formulation of their origin of life theories. That still leaves room to think that Marxist principles were somewhat influential to their theorizing, insofar as the ideas were "in the air." My own argument may not be in strong tension with his, but nevertheless disputes the extent of that Marxist influence. In any case, Graham focuses on Oparin, whose "materialism developed in parallel with the predominant philosophical views of his society," and my focus is on Haldane. Insofar Oparin and Haldane are very frequently lumped together on this and other issues, Graham is still pertinent to my thesis.

reason, I think, that Marxist philosophy's contribution to the history of science is typically relegated only to the Lysenko affair:

> This tendency to explain an acknowledged calamity in science as a result of Marxist philosophy while assuming that a brilliant page in the history of biology had nothing to do with Marxism is a reflection, at least in part, of the biases and historical selectivity of anti-Marxist journalists and historians (1972, 258).

I sympathize with Graham's project, and I don't doubt that many political ideologies may have contributed to the historical neglect of Marxist science. But, recognizing the legitimacy of such a project, it is important not to go beyond the evidence for the case at hand. There is scant evidence that J.B.S. Haldane based his theory of life's origin on principles of dialectical materialism. The best evidence is based on his 1928 trip to the Soviet Union, upon which he later reported (1938c) that he encountered Marxist philosophy of science. The little evidence that exists about that 1928 trip and its immediate effects upon Haldane is not very supportive evidence of Graham's theory. Haldane wrote of his encounter with Russian thought in 1928: "It was hardly love at first sight" (1938c). That indicates that his 1928 trip to the Soviet Union didn't cause any gestalt switch. No scales fell from Haldane's eyes in 1928. Even though this recollection came a decade after the fact, there would have been little reason for Haldane to diminish the effects of Marxist philosophy of science upon his early encounters. There is no reason to doubt the statement that it took some time for Marxist thought to take root in Haldane's projects. The topics of his published writings suggest just that – the ardent support for Marxist thought appears in his writings only after 1930. By 1938 he wrote: "I think Marxism is true" (1938b, 16), and also that "I have only been a Marxist for about a year" (1938b, 13).[7] He joined the Communist Party of Great Britain in 1942.

Noting Loren Graham's own cautious thesis, Helena Sheehan extends this analysis of Haldane and how Marxism affected his 1929 theory. "The evidence for Graham's interpretation is even stronger than what Graham has himself set out…" The extra piece of evidence Sheehan invokes is disputable. It is found in a 1940 essay called "Why I Am a Materialist", where Haldane says that his stance fifteen years before (that is, in 1925) was that he had been a materialist in practice but not in theory. At that point he was aware of problems with materialism, but unwilling to accept idealism, and, as the 1940 essay reads, studying Engels and Marx allowed him to resolve those difficulties.

> Had these books been known to my contemporaries, it was clear that we should have found it easier to accept relativity and quantum theory, that tautomerism would have seemed an obvious hypothesis to organic chemists, and that biologists would have seen that the dilemma of mechanism and vitalism was a false dilemma (1938a).

Sheehan takes this as compelling evidence to believe Marxism played a formative role in Haldane's theory – she calls it "extremely significant" (1985, 320).

Sheehan's inference is problematic on at least two fronts. A close reading of this 1940 essay makes it clear that Haldane is not offering any precise dates for when exactly he was studying Engels. The essay simply tells a narrative about how dialectical materialism aided particular philosophical problems he had encountered in the 1920s, but

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[7] There is reason to think that the latter statement is an understatement; it came early in a public speech when he was concerned to establish the point that he was no professional philosopher. As such, it is not necessarily a reliable source to date any particular "beginning" of Marxist commitment to 1937. My own estimate would posit a gradual transition to Marxism in the early 1930s.

he does not say anything specific about what happened in 1925. There is no reason to think he was trying to offer a specific chronology of his own life. Secondly, there seems to be an equivocation when she implies that being a "materialist" in 1925 is tantamount to being a dialectical materialist. Of course materialism, with its roots in ancient philosophy, is a broad category, and it has flourished in distinct versions. This is just to say that all "materialism" is not necessarily the Marxist version, and the materialism that Haldane was wrestling with in the 1920s was *not* Marxist. (This is not a question-begging point because, in his manuscripts and publications from the 1920s, he isn't discussing dialectical materialism at all.)

I suggest it is best to avoid attributing anything other than an extremely weak Marxist influence upon Haldane's theory. My arguments above are negative – that there is little evidence that Marxism made an impact on his work in the 1920s. But I would further contend that Haldane's location, surrounded by diverse intellectual projects on the nature of life that *were not* products of Marxism, made for more than enough resources to stimulate Haldane's own theory of origins. For example, the (1927) textbook that Haldane composed with Julian Huxley contains several citations of Henry Fairfield Osborn's book, *Origin and Evolution of Life*, which advances an obscure "energy theory of life" alongside much analysis of contemporary chemistry. The scientific figures Haldane explicitly cited in "The Origin of Life" included E.C.C. Baly, Felix d'Herelle and Herman Muller, whose respective contributions discovering organic products from UV radiation experiments, discovering the bacteriophage, and likening the phage to a gene, show no signs of being influenced by dialectical materialism.

But to make this point even further I will cite a few articles from the 1929 volume of *The Realist*: *a Journal of Scientific Humanism*, for which Haldane served on the editorial board. He most likely would have been aware of all of these papers, and they will suffice to show the broad interest in scientific disputes over the nature of life. They were not products of Marxist thought, and it is reasonable to think that they collectively helped to inspire Haldane's own theory.

A.E. Boycott's article on "Invisible Viruses" notes that viruses are capable of variation and adaptation, traits associated with life, but yet still ponders whether viruses are too small to be alive. In his conclusion he writes:

> We may… think that live and dead form one continuous series extending from the hydrogen atom at one end to man at the other, the members of the series differing from one another quantitatively more than qualitatively… 'Live' and 'dead' are in short not the only possibilities: a thing may be partly one and partly the other…" (22).

Such a challenge to the categorization of life and death may have been just the thing to prompt Haldane's own theory, which similarly employed viruses to break down the fixity of those categories.

The philosopher Dorothy Jordan Lloyd has an entry that first reviews some of the history of biological theorizing, including the disappointments of vitalism and of the failure to discover "proteid" – what had been supposed would constitute "living matter" (43). At the same time, she documents a growing discontent with mechanistic materialism that is rooted in social factors, most significantly the horrors of the World War. "Probably the catastrophe of the war more than anything else of recent happening inclined men to doubt the absolute value of the materialistic philosophy that had so long been paramount in science." Lloyd proceeds to use recent developments in quantum mechanics to support Eddington's new philosophy of nature, which, she argues, means that the observations of scientists "are connected with a mystical, all-permeating background, the nature of which will probably escape for ever from capture by the experimental methods of natural science" (47).

Julian Huxley's "The size of living things" is an analysis of the significance of scale for biological processes. He notes that a certain size is requisite both to achieve "emancipation from passive slavery of the forces of environment" (1929, 57) and to achieve any serious degree of complexity. Philosopher Herbert Carr's "Life and Matter" is a succinct articulation of teleological holism, modeled after the recent philosophy of J.S. Haldane. "Life is not a stuff which can be separated by chemical analysis… Life is not a sensible quality or set of sensible qualities, supervening on molecular combinations. It appears, when it appears, as an independent activity directing material forces in view of the accomplishment of some definite purpose or end" (1929, 185). Carr's argument that life constituted a "potentiality" or "directing power" appealed to several homeostatic physiological examples, including the regulation of blood temperature. J.S. Haldane's own entry in the 1929 *Realist* likewise articulated holism as a way to frame living processes that was neither vitalistic nor mechanistic.

These few articles from *The Realist* sampled from the year that Haldane penned his theory of life's origin illustrate the flurry of activity addressing both scientific and philosophical aspects of the nature of life. Haldane was aware of a rich set of resources that was more than sufficient to account for his own interest in origins, as well as account for the particular intellectual genesis of his own theory.

I have argued that Haldane's theory of origins is best understood in light of a mechanistic and materialistic framework. But that does not entail an understanding of Haldane as an ardent proponent of mechanism. A close reading of "The Origin of Life" suggests that Haldane was searching for a more robust philosophical basis for formulating the relationship between life and non-life.

> Some people will consider it a sufficient refutation of the above theories to say that they are materialistic, and that materialism can be refuted on philosophical grounds. They are no doubt compatible with materialism, but also with other philosophical tenets (Bernal 1967, 248).

This cautious qualification that his theory is not necessarily materialist makes it clear that Haldane was hardly an enthusiastic partisan for mechanism. At most he seemed to regard it as the best program going for making novel empirical predictions. He explained that we only know about life in connection with certain arrangements of matter, "of which the biochemist can give a good, but far from complete, account" (249). If the mechanistic explanations of biochemistry were *incomplete* accounts of the living world, it may be best to understand Haldane's commitment to mechanism as attenuated. It was not an overarching philosophy, but a useful posit for certain chemical experiments, which he desired to make susceptible to experiment. Later comments suggest just this reading: that mechanism was a useful "workplace" philosophy with pragmatic benefits, but that outside of that context, he was not so certain. The 1940 essay "Why I am a Materialist" (cited above on the separate topic of dating Haldane's Marxist thinking) reflected back on his philosophical stance many years earlier: "although I was a materialist in the laboratory, I was a rather vague sort of idealist outside" (1940).

Both Graham (1972) and Sheehan (1985) interpret the above passage from "The Origin of Life" about compatibility with multiple philosophies as a conciliatory gesture that was intended to enlist as much support for his view as possible. They read Haldane as a committed materialist at this point, who is avoiding the risk of putting off potential allies who might take his ideas seriously if not for the materialistic assumptions. There are two problems with their interpretation: For one, Haldane never shied away from offending others' sensibilities. He was the furthest thing from a consensus-builder; his bombastic personality and writings testify to his unflinching willingness to alienate others. But more importantly, in an essay from the very next year, called "Reflections on materialism" Haldane repeated several times, "I am not a materialist," even though he conceded the "element of truth" it contains. Given this context, I believe the best way to interpret the philosophical passage at the end of "The Origin of Life" is as a reflection of Haldane's own philosophical uncertainty.

The uncertainty evinced in his 1929 theory helps *explain* why he might become attracted to dialectical materialism, but I do not believe there is evidence that dialectical materialism played a significant role in forming his theory.[8] I think this period around 1930 was when Haldane was acquiring his taste for Marxism, and it would be an influential part of his work through the 1930s and '40s, but it just wasn't present in 1929.

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[8] On my understanding of this episode, some philosophy serves as a necessary condition for scientific theorizing. However, it is not always the case that a pre-existing philosophy or ideology is what shapes the science. The causal arrows aren't one-directional, because the science, too, leads to the desire for new philosophical understandings. I think that was the case with Haldane, who took from his science the desire for a philosophy that refuted idealism and vitalism without being reductionist.

I should also distinguish my reading of Haldane's changing philosophical commitments (such as they were) from others in the literature. Sahotra Sarkar has offered a periodization of Haldane's thought, documenting the changing philosophies to which Haldane adhered. The problem here is that J.B.S. just didn't adhere very strongly to *any* philosophy. J.B.S. held a much looser grip on philosophies than did his father, and his only consistent commitment was a kind of scientism which attributed to science the ability to solve all social problems.

While there is much worth in these efforts at charting out Haldane's changing views (and I have also just discussed a sort of transition under the influence of Hopkins), I also think that any attempt to impose a strict categorization on Haldane's philosophy is probably doomed to fail. That is because, with the possible exception of dialectical materialism, which he was captivated by from the late 1930s into at least the mid-1940s, there was never a period where Haldane sought to align himself with a particular philosophical view. That doesn't mean he wasn't employing philosophical presuppositions or even explicit statements, but just that he didn't retain an overarching philosophical stance from which he sought to derive consistent views. But the boundaries that Sarkar discusses are neither as sharp as he imagines them, nor do they demarcate periods of genuine philosophical allegiance.

For whatever period of philosophical categorization proposed, there are counter-examples among Haldane's writings. But one need not even look that far: Haldane explicitly distanced himself from philosophy as an explicit pursuit alongside science. It is possible that he found his father's conversations unfruitful for scientific purposes, and it is also reasonable to guess that he thought explicit commitment to any philosophy to be an undue restriction on empirical research. He wrote: “In my opinion the details of all metaphysical systems have been incompatible with certain observed facts” (1932, 158). To the extent that all scientific proposals depend on background assumptions that are often not subject to empirical warrant, and which include metaphysical assumptions, then any interpretation of Haldane as actually anti-philosophical (as he would sometimes suggest), is also problematic. Philosophy can be given up as an explicit pursuit in itself, but not as a component of scientific thought. All theories and interpretations of experimental data are underwritten by philosophical presuppositions, however unarticulated or taken for granted they may be. J.B.S.’ adoptions of particular philosophies was often pragmatic and in the service of investigating scientific problems.

I have told the story of Haldane’s origin of life hypothesis as the product of a period of philosophical uncertainty. In the mid and late 1920s Haldane found scientific success through his work in population genetics and enzyme kinematics, pursuits most easily described as the sort of quantitative and reductive projects of mechanists. Haldane’s “The Origin of Life” is best understood as a mechanistic and materialistic proposal for life’s genesis, and it opened up a route for empirical investigation that was proscribed by the teleological holism of his father. As such, this type of theory maintains a kind of pragmatic advantage over theories that would block further investigation. It simply allows further questions to be asked, without a commitment to their being the right questions, or securing the right answers.

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CHAPTER 2

Haldane, expertise, and the popular press

"Man is a theorising animal. He is continually engaged in veiling the austerely beautiful outline of reality under myths and fancies of his own device. The truly scientific attitude, which no scientist can constantly preserve, is a passionate attachment to reality as such, whether it be bright or dark, mysterious or intelligible."

-- J.B.S. Haldane, *The Causes of Evolution*

2.1.

"I like controversy." That's how J.B.S. Haldane began a 1933 BBC broadcast of "The National Programme." In what follows I'll discuss how Haldane enrolled his peculiar relish for controversy, among other talents, via his writings in the popular press, in the creation of a distinctive vision for science's role in society.

John Burdon Sanderson Haldane inherited all of the privileges and high expectations that his illustrious name would suggest. He descended from a prominent aristocratic Scottish family: the Haldanes of Gleneagle. From his mother he gained early imperialist sympathies as a dutiful participant in the Victoria League and Children of the Empire. These he seems to have shed at the influence of his father, an Oxford physiologist whose scientific pursuits were closely bound to his pro-Labour politics, and with whom J.B.S. even as a child undertook dangerous respiratory experiments in mines.

J.B.S. grew up to become a prolific scientist and writer himself, and some have suggested that he would be remembered chielfly as a popularizer (Clark 1984). It seems to me that the scientific accomplishments of one of the founders of population genetics shouldn't be underestimated; moreover, his contributions to physiology, enzymatics, and biochemical genetics stand as substantial achievements in these respective fields. His scientific contributions indeed remain worthy of further scholarship. That said, a close analysis of his popular writings is a worthwhile pursuit, since in them we can see J.B.S. sorting out serious issues that were at the heart of science, its method, and its place in society. As a public man of science, his writings also molded much public opinion.

Haldane's popular writings were not, as Gary Werskey (1978) comes close to suggesting, merely sensationalizing. Nor were they just an outlet for his admittedly bombastic personality. Also, they were rarely a straightforward "dumbing down" of science. They included the proposal of creative ideas that were often quite serious, as, for example, his novel suggestion for a chemical pathway to life's origin (1929).

Haldane's broad corpus comprised news reports, science fiction writing, essays, political tracts, book reviews, and even a children's book, 'My Friend, Mr. Leakey', from which this illustration is taken.

Figure 1 – Illustration from *My Friend, Mr. Leaky.* It is perhaps surprising that such a serious, sometimes-overbearing personality like Haldane wrote a fanciful children's book replete with characters like this octopus – a servant who is happy to have many arms with which to serve the ingenious Mr. Leakey.

In the Preface to his classic 1927 essays 'Possible Worlds', Haldane writes:

> Many scientific workers believe that they should confine their publications to learned journals. I think, however, that the public has a right to know what is going on inside the laboratories, for some of which it pays. And it seems to me vitally important that the scientific point of view should be applied, so far as is possible, to politics and religion.

For J.B.S., popularization went hand-in-hand with his scientism: it was not just the political elite who needed the scientific point of view. Popularization was necessary to disseminate the scientific ethos to broader publics as well. This was a far cry from J.D. Bernal's (1969) vision of a scientific technocracy, with the public as an "ignorant but intelligently managed zoo." J.B.S. dearly wanted the assent and recognition of the public. Science wasn't a top-down application of knowledge claims produced by the cultural elite – he imagined a public who requested such science be performed in their own interests. Such a view was consistent with his own burgeoning socialist sympathies. Haldane's proselytizing on behalf of the "creed" of science came because, "in my opinion a society in which a creed was held by a 'select' minority, and kept from the vulgar herd, would inevitably become increasingly hypocritical, and probably grossly unjust" (Lunn and Haldane 1935). The guiding principles of scientific practice would, thus, need to be available to any and all.

Haldane's political stance could best be described as a "social technocracy" – as populist support for the scientific management of society, where that science was still understood to be the product of a small minority with special training, and indeed special virtues, enabling and legitimating their cultural authority to speak about what should be done.

J.B.S.' enthusiasm for popular writing was somewhat ironic given his initial censure of his friend Julian Huxley for the very same sin. A familiar Cambridge norm in the 1920s was that science is for laboratories; speculating about science's broader significance was unbecoming of professional scientists (Werskey 1978, chapter 1). That two of the prominent laboratory's leaders at Cambridge (Rutherford at the Cavendish and Hopkins at the Dunn) both had knighthoods must have lent a nearly feudal air to a place thoroughly steeped in aristocratic traditions. Aspects of such tradition in the Cambridge scientific subculture of the 1920s included isolation from the arts and an expectation of (relative) political inactivity. Political sentiment was largely expected to fall within the circumscribed values of the British ruling class.[9] Science was “pure,” “hard,” and experimental - a pursuit of truth for its own sake. Popularizing scientific results was thus exceptional behavior. Haldane’s break with tradition on this issue was the first of many during his lifetime, and some peers thought it was risky business. Crowther (1970) reported that the eminent chemist William Hardy had censured both Huxley and Haldane for damaging their careers by moving in the direction of popular writing.

Through the pages of many different periodicals, Haldane found his voice as a leading representative of science in Britain, and a peerless advocate of scientific socialism during the 1930s. Isiah Berlin called Haldane “one of the major intellectual emancipators” of the age. Haldane’s influence extended beyond educated progressives, as his manuscripts demonstrate his receipt of letters from various corners of Britain asking for his advice on, for example, whether one should decide to have children given certain concerns with disease (will my child have hemophilia?) and with race (will my children be dark-skinned?). Haldane went to great lengths to reply to such queries.

In the midst of European political strife and worldwide warfare, Haldane was convinced that “the future of Western civilization depends on whether we can assimilate the scientific point of view” (1932, 139). Clearly, Haldane shared his Cambridge colleagues’ confidence in the significance and vitality of science. But just what was this view? How exactly would science be a salve to the problems of the modern world?

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[9] It was not just that the University itself encapsulated aristocratic values; most of the practicing scientists descended from affluent backgrounds. Werskey cites an ex-Cambridge biochemist saying of his lab in the 1920s: “most, say seven out of ten of the staff and research workers had some inherited money… This gave them a feeling of security and independence” (1978, p. 22).

Not all scientism, or even all technocracy, is of the same sort, and it's worth looking at just what Haldane's own vision was. For the purposes of this paper, I focus especially on the years 1923-32, corresponding to his time at Cambridge as Dunn reader in biochemistry.

2.2. Into the spotlight

J.B.S. first appeared on the public stage in 1924, with his publication of *Daedalus, or science in the future*. This short tract depicted a future world increasingly under the dominion of scientific management. "No beliefs, no values, no institutions are safe" from an impending scientific order. It was an influential early publication: *Daedalus* even provoked book-length response from Bertrand Russell.

In *Daedalus*, Haldane discussed several controversial technologies, including birth control and a mechanized, extra-uterine vision of human birth. The latter idea, dubbed "ectogenesis" in Aldous Huxley's *Brave New World*, reverberated as a prophecy of a literally dehuman-izing technology. Ectogenesis also appeared in D.H. Lawrence's (1968) novel *Lady Chatterly's Lover*: a story whose thematic tension arises from the struggle between an industrialized and mechanized modernity on one hand, and an organic and holistic (not to mention sexualized) view of the world on the other. This division between mechanism and holism was a dichotomy quite familiar, both culturally and scientifically, to Haldane. Whereas Lawrence and many other artists were generally wistful for the latter, more Romantic view opposed to mechanization, Haldane, along with his Cambridge colleagues, was certainly more optimistic for science's prospects along those lines.

But for Haldane, that scientistic optimism emphatically did not render it unromantic. To the contrary, in *Daedalus* he asserts: “the biologist is the most romantic figure on earth at the present day.” He continues,

> I am absolutely convinced that science is vastly more stimulating to the imagination than are the classics, but the products of this stimulus do not normally see the light because scientific men as a class are devoid of any perception of literary form.

If scientists were gifted with such artistic perception, or alternatively, when artists finally began learning some science, their fruits would achieve heights comparable to Dante, Virgil, or even his favorite poet, William Blake. Haldane employed his training in the humanities as a means to communicate both the content of current science and his own broader ideals for science’s place in society.

Haldane’s Oxford education included degrees mathematics and greats: like his father, J.B.S. never took a formal degree in natural sciences. When Diane Paul (1983) writes of the Cambridge cadre who sought to replace England’s humanities-based education with a scientific education, J.B.S. remains a problematic example. While his vision for science’s cultural authority demanded public education in science, Haldane never wrote that instruction in the humanities should be excised in favor of the sciences. Haldane’s interest in poetry and the classics would color his writing for the rest of his life, while his love of mythology was closely connected with his understanding of moral education. A former student noted, “Life with Haldane required, among other skills, that one brush up one’s mythology” since he regarded myth as “the best form of moral instruction” (Maynard Smith, 1987, 8).

Science clearly stimulated Haldane's own fertile imagination. This entailed not only speculations of future possibilities, but also more pragmatic applications of that imagination, such as developing a motif of humankind's incipient "war" with viruses. Thus science could provide new narratives in which the quest for public health could influence our lives.

Haldane's popular articles adroitly weaved poetry, mythology, and artistic references into his accounts of science and its place in our lives. For all that it looks as if J.B.S. may be seeking to transcend the cultural divide between sciences and humanities articulated by his colleague C.P. Snow, he nevertheless upheld a view of science as distinct from other cognitive enterprises. For example, he would sometimes stress the superior knowledge that could arise from quantification, as opposed to the confusion engendered by philosophy and linguistic discourse. Also, his science fiction story "The man with two memories" involves a character who speaks two languages: one descriptive (described by its resemblance to Chinese), and the other "crass" and emotional (likened to Italian). These two tongues corresponded with different ends to which they were put, and science was conducted in the former descriptive, unemotional language.

Figure 2 - While *The Man With Two Memories* garnered little literary acclaim, one is nonetheless surprised to learn that even in the distant future, men will still be wearing kilts. Here, Haldane poses in his Black Watch kit from WWI. In addition to the apparent joy Haldane took from his experiences as a bombardier (where he earned the nickname "Bombo"), he wrote also of the joy of bicycle riding, along the front, in a kilt.

By 1926, J.B.S. had a steady stream of requests for his work in many periodicals on both sides of the Atlantic. Following a highly publicized divorce, he married journalist Charlotte Burghes, a writer from the *Daily Express* who coached him on popular writing techniques. They proved to be shrewd businesspeople and commanded a significant income from his popular writings. His occasional pieces for many dailies comprised the bulk of his income from writing, and beginning in the 1930s he was a regular columnist in the Communist *Daily Worker* (Bowler 2009). Despite warnings from colleagues, this pursuit of popularization did not seem to hamper his scientific accomplishments, which were numerous. Haldane composed quickly, and often wrote while in transit aboard trains. He joked that once he had gained his F.R.S., he could command fifteen guineas for an article instead of five (Clark 1984, 84).

2.3. Science & normativity

However often he might do it, Haldane's popular writing is much broader, and more interesting, than mere futurism. His treatment of science within the social order explicitly dealt with science's normative potential.

Haldane frequently wrote on the need for a "quantitative" ethics. He seems to have adopted a consequentialist ethical framework based on the maximization of human health rather than pleasure -- health being what he considered the more objective of the two categories. Haldane advocated sorting through relationships in our society and economy that affect peoples' health. That would demand the acquisition of statistical knowledge, in particular. He urged, "It is our duty to acquire the knowledge which will enable us to moralize our everyday actions" (1928, 32).

Haldane thought that scientific progress accompanies, and even generates, moral progress. As he argued in *Callinicus: a defence of chemical warfare*, chemical war seems preferable to conventional war on the grounds that it is both more humane and more scientific. Having suffered from both shell wounds and exposure to toxic gasses during World War, Haldane thought himself in a good position to launch this unpopular judgment. For Haldane, this overlap between moral improvement and technical advance was no accident, as scientific knowledge was most often broadly applied in the service of a more humane world.

At times Haldane makes it seem as though ethical principles are yet another topic amenable to scientific analysis, but he would often hesitate here at the metaphysical juncture, with statements that sound much like claims made by his philosophically astute father: “Ethical experience testifies to a super-individual reality of some kind” (1928, 35). Haldane also wrote of the capacity for “self-transcendence” that was inspired by ethical and religious practice (see below).

2.4. Eugenics

J.B.S. favored the essential commitments of the eugenics movement – the scientific creation of better society via state management of inherited traits. Throughout his life he retained some sort of belief in genetic and cultural inferiority of the lower classes. However, Haldane’s own solution to the problem of reproductive disparity between rich and poor (so much lamented by his contemporaries) countenanced the material conditions that he understood to generate it. While it was true that the lower classes reproduced at a greater rate than the upper classes (which J.B.S. called an “evil” (1927, 203)), Haldane proffered no Malthusian solution of withdrawing charity or state-sponsored welfare. Instead, the solution was to be found in economic incentives and rational persuasion.

Early genetics was a hotbed of eugenic enthusiasm; the pages of the journal *Nature* at the time were filled with eugenic recommendations. *Nature*’s editor, Sir Richard Gregory, supported negative eugenics and was also a vice-president for the Society for Constructive Birth Control and Racial Progress. Haldane by contrast appears as a rather lukewarm eugenicist, and he was opposed to any mandatory eugenic actions. There were two reasons for this: Eugenic solutions simply wouldn’t yield effective consequences for most traits, including "feeble-mindedness," a topic he discussed repeatedly. "Feeble-mindedness is fairly strongly inherited, but unfortunately it is generally inherited in such a way that the segregation or massacre of the feeble-minded, even if continued for several generations, would not stamp it out" (1928, 22). Second, Haldane was explicitly concerned with abuses of political power -- that mandatory eugenics would unfairly target the politically weak.

However, Haldane looked forward to the point when eugenics could be used without the travesties of justice that would inevitably accompany it in his own day. Thus in 1928 he maintained: "I am certain that it has a very great future as an ethical principle." His enthusiasm was sustained by hope for a coming age of classless society in which eugenics could be applied with fewer reservations about political abuses. In such circumstances, he imagined, eugenic principles would be adopted voluntarily for the good of society. Ironically, he believed it would be a Communist society which would be most conducive to eugenicist practices.

2.5. Religion

J.B.S. is today often remembered as a vehement atheist, and he certainly did enjoy his reputation as opponent of orthodox religion. McOuat and Winsor (1995) have even gone so far as to suggest that his interest in quantitative studies of natural selection was originally motivated by his desire to strike a blow against religious groups who gave the theory of evolution short shrift. While that particular etiology of Haldane's wide-ranging scientific career seems overstated, J.B.S. was concerned to bring science to bear on religion. Haldane began his classic book *The Causes of Evolution* with the quotation:

"Natural selection is dead" -- which he attributed to "Any Sermon." Such quips reflected both professional and popular uncertainty over the status of natural selection in the first decades of the twentieth century. This was, after all, the period which Julian Huxley referred to as "the eclipse of Darwinism." A major theoretical divide between Darwinian and Mendelian interpretations of evolutionary change was capitalized upon by some religious elements, using the lack of consensus as a way to manufacture doubt about the status of evolution more generally. Haldane's central role in bridging those two interpretations would not be confined to his specialized academic papers. Insofar as religion represented one of the few institutional loci of evolutionary skepticism, he would use the popular press to train his fierce criticisms there. Unsurprisingly, those religious criticisms would extend beyond the church's resistance to evolution.

Haldane was raised in a household that was, for its time, comparatively bare of religious observation. His sister Naomi wrote of their upbringing: "We had a set of strict ethical principles which were slightly harder to live up to because there was no supernatural sanction behind them" (Mitchison, 1968). At Eton boys' school Haldane cultivated a fierce disdain for religious instruction and belief; some subsequent encounters with allegedly timorous[10] chaplains in the trenches of the World War only reinforced his skepticism. He noted that he "developed a mild liking for the Anglican ritual and a complete immunity to religion" (quoted in Dronamraju 1995, p. 141).

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[10] This observation of others' lack of courage in the trenches should be qualified by the unusual delight Haldane himself displayed for trench warfare, which he famously thought of as "an enjoyable experience." See Clark 1984, chapter 2. In a letter to his sister dated February 1915 he insists: "Bomb-throwing is the next most exciting thing to being under fire." Repeated correspondence to his family confirms this enthusiasm: a note to his father read, in its entirety, "Brigade bomb officer is the best job ever."

Although a professed atheist whose professional identity was largely informed by religious skepticism, Haldane hardly avoided the topic of religion, and indeed he showed a lifelong interest in it. In a notebook from the late 1920s we find: “In mathematics we apprehend one class of invisible and eternal realities. Similarly there are probably invisible and timeless realities to which our ideas of beauty and goodness correspond” (Manuscript 20586, NLS). Such passages suggest a transcendent reality perhaps impervious to scientific analysis. Carlson (1995) recognizes Haldane’s “sympathy for the ultimate strivings” of religion. An understanding of those “ultimate strivings” needs to be developed in tandem with insight into what exactly he criticized in religions. Haldane’s spate with C.S. Lewis is one case study that may serve to illustrate his views.

Haldane locked horns with the prominent Oxford medievalist, fiction writer, and Christian apologist C.S. Lewis in a 1946 review of Lewis’ “Space Trilogy.” The science fiction series expressed Lewis’s concern that a contemporary scientific ethos – especially as embodied at Cambridge - was articulating the perpetuation and improvement of the human species as a supreme good. Lewis almost certainly had in mind here Haldane’s 1927 essay, “The Last Judgment.” Just as eugenic principles would be needed to solve the impending collapse of western civilization in *Daedalus*, Haldane’s romantic imagination had planetary exploration saving the human species following the collapse of the sun in “The Last Judgment.” Lewis thought that any “scientific hope of defeating death is a real rival to Christianity” (Adams 2000, 483), and in response wrote his fictional trilogy, in which Haldane was caricatured as a satanically-possessed physicist.

In a stinging review titled “Auld Hornie, F.R.S.” (that is, Satan, Fellow of the Royal Society – the way Haldane felt himself portrayed by Lewis), Haldane deployed his very favorite criticism: Lewis's books were filled with bad science. As examples he points out faults in the account of the spaceship's gravitational field and the composition of Martian atmosphere. It is not obvious how such attention to scientific details could apply to fiction in general or science fiction in particular, but Haldane sought to point out how such bad science was likely a *consequence* of Lewis's religious belief.[11]

Haldane's main point was to illustrate that Lewis' stories were thinly masked morality tales ultimately supporting a conservative political outlook. Just as bad, it was unfaithful to the best scientific standards of the day. Such unscientific writing exemplified, for Haldane, how the Christian worldview was too often yoked to long-expired science and to "inferior Greek philosophy," by which he meant mind-body dualism. His conclusion seemed to be that any religion with inflexible creeds, recalcitrant to change through their pretensions to timelessness and universality, could never be credible. Any viable religion would need to be flexible, tentative, open to revision by empirical circumstances. Religion was a buttress for conservative moral and political order; science posed the ineluctable need to refashion society in light of our best, newest, knowledge.[12]

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[11] Haldane's review constituted one of many volleys in the battles between intellectual and cultural traditions of Cambridge scientists and Oxford's religious humanists. Haldane's antagonism was reciprocated by Lewis, upon whose death was found a file titled 'Anti-Haldane.' Upon posthumous publication, this essay took on the more suitable title, "A Reply to Professor Haldane".

[12] Haldane's vision of science was at times explicitly destructive. "The conservative has but little to fear from the man whose reason is the servant of his passions, but let him beware of him in whom reason has become the greatest an most terrible of the passions.

Haldane's interest in religion continued through his later commitments to the Communist Party:

> The reality behind mystical experience is perhaps the perception of a unity which may have been a commonplace for a member of a primitive tribe… which even members of the socialist society of the USSR can only grasp in part. In a class society this reality can only be expressed in a highly mythological form (1942, p. 13).

With Marx, Haldane could affirm that religious beliefs are held due to material conditions and an unjust class society. But unlike Marx, he seemed to think the religious stories could be getting at something essentially right about a super-individual "unity". Again, Haldane seemed to find some potential for redemption in Christianity when he wrote that Marxism may have "given Christianity a new lease on life" (1946). What he meant in this passage was that he understood the precepts of Jesus to be best realized in a socialist society, and that western capitalism was impeding such just outcomes. What Haldane found most problematic about Christianity was not what he understood as its moral core, but rather the way it was enrolled to support particular political frameworks, especially those with capitalist or imperialist ends. He would often characterize early Christian religious practice in a positive light, contrasting its early "revolutionary" character with its later affiliations with neo-Platonist philosophy and imperial political projects (1939, 22).

Hence, Haldane was not wholly dismissive of religion's subject matter. He had a conviction that, even though fundamentally flawed, religions were all "concerned with something very important" (1962). With its emphasis on unity and reciprocal - even universal - moral obligation, religion was able to bind people together in a way Haldane found crucial to his understanding of the moral order. That religion did not always carry through on its ability to unify people – or worse, when it divided people – weakened what Haldane considered its already dubious standing.

2.6. Science policy

In the early 1920s, Haldane supposed that capitalism could best protect the freedom of scientific inquiry (1924), though later on he wrote that science would only be applied to war and other unseemly bourgeois interests if left in the hands of capitalism. Throughout his changing political views, Haldane retained the belief that "pure" scientific research demanded autonomy, and was not to be subject to state control. The value of research freedom later came into sharp relief during the Lysenko controversy, putting Haldane's alliance with the USSR to the test, and, eventually, the breaking point (Paul 1983).

Once again, notions of health occupied a central role in Haldane's views on policymaking. Numerous passages from this early period suggest that war-making distracts people from more universal human enemies, such as microbial pathogens. But if we would only focus on the latter problem, he supposed, related to our common commitment to human flourishing, then we would worry less about killing each other. Haldane seems to have a zero-sum notion of "struggle" here: more struggle with microbes meant less with each other. In any case, science was clearly relevant to lawmaking. In an article bearing a characteristically Haldanian title, "Biochemistry and Mr. Ghandi," Haldane writes of the differing physiological (and thus moral) consequences of a salt tax between England and India. The justice of any tax policy should be assessed through a scientific analysis of its consequences.

Yet J.B.S. makes it clear that science and science policy are not exactly the same thing. His view seems to construe the scientist a close confidant of the policymaker, a crucial voice in the decision-making process, but nevertheless, not exhaustive of the decision-making process. There remained a critical distance between scientist and lawmaker -- a distance that eases scientific responsibility somewhat, and which would end up, some years later, easing Haldane's retreat from Communism and eventually from England.

It is not clear whether Haldane considered the tension between his insistence on the scientific need to hold hypotheses only tentatively, and the politician's need to act, even under conditions of uncertainty. Haldane frequently contrasted the scientific perspective with the "creeds" of religion, emphasizing that science has neither need nor room for such guiding principles. In a highly publicized series of debates on the topic of science and religion (Lunn and Haldane 1935), Haldane addressed his interlocutor: "on the whole, my beliefs are a good deal more provisional than I imagine yours to be" (dated October 21, 1931). Haldane continued by pointing out how many of his own beliefs had been recently altered under the influence of new research. "I do not now believe all that I have myself written." In the absence of other guiding principles or broader values, it would seem that a policy based solely on scientific data would be subject to radical, and possibly quite frequent, updates.

2.7. Science, socialism & expertise

Haldane's early hopes for the Labour party, and especially its ability to satisfactorily address fascism and Nazism, were drying up by the late 1920s. And at least by the early 1930s he began acquainting himself with Marxism and with the principles of dialectical materialism, which (for philosophy) he found uncharacteristically impressive.[13]

Haldane looked East to find an alternative to the many problems he diagnosed at home; he thought the Soviet Union embodied the best example of a scientific society. His criticisms of England were, in the 1930s, increasingly framed as an unfavorable contrast with the Soviet Union. The Soviets, in his view, pursued education, state planning, scientific research, and much else in better accord with scientific principles. This included, in Haldane's view, ameliorating disparities between rich and poor. After years of writing and work on behalf of the left, he officially became a member of the Communist Party in 1942.

Yet Haldane was an influential insider with the British ruling class, and retained many of his own classist views. Were these not inimical to the spirit of Communism? To the contrary, he was fond of pointing out how they could be made commensurate with the guiding principles of communism: "I am a very strong believer in innate inequality" (1932, 137).

In the 1920s, unconstrained by the institutional strictures of Cambridge, Oxford, and his family's expectations, Haldane found a personal voice with which to criticize the establishment. In 1925, Haldane endured painful and highly-publicized divorce proceedings, in which he was actually fired from Cambridge for acts of "gross immorality." Cambridge found it unacceptable that he should marry a woman whose

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[13] Haldane consistently strove to present science as anti-philosophical, and therefore himself, too, as anti-philosophical. While he may not have consistently adhered to an explicit school of thought (before he found Engles's dialectical principles), that does not mean he did not work in accordance with an established pattern of more general beliefs.

previous marriage was dissolved for the sake of the new relationship. The ensuing appeal, which successfully overruled the original decision, marked something of a new day for Haldane.[14] He was increasingly willing to use his newfound freedom from institutional fetters as an instrument with which to criticize. He could simultaneously leverage the authority of Cambridge University, and flaunt its own conservative ideals. He sought to overturn some of the traditional moral restraints on men of science, and refashion those virtues. The virtuous man of science was not bound by Christian injunctions to chastity, but instead by injunctions to objectively and unemotionally collect and analyze data, and to apply those findings, come what may. He held little regard for the moral or social order that lead to his own prominent social standing.

Haldane's entrance into more radical politics was experienced very personally, in that it also signified a turning away from his own aristocratic family roots. This rupture curtailed communication with his family; disagreement with siblings and parents became the norm. His insistence on taking third class seats on the train literally separated him from his family during travel, as they choose the first class. His willingness to take coach travel became a topic he would mention often in print (1938, p. 183). Solidarity with the working class seemed to be a moral precept that long predated his official membership in the Communist Party.

But if he sought fraternity with the working classes, his aristocratic status was in a sense restored to J.B.S. through his centrality, visibility, and public service via science.

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[14] The Cambridge body for the investigation of such faculty problems had, unfortunately, long been designated the *Sex Viri*, or Six Men. Thus Haldane won a rhetorical battle by its inevitable re-labeling as the "Sex Weary." Following the proceedings, the group immediately changed its name.

His command of crowds at public lectures also contributed to his status, which he gave, according to one estimate, at the astounding rate of 100 per year.

For Haldane, his right to a high status arose not from membership in the aristocracy, but from the acquisition of skills permitting one to "put nature to the test" as it were. When explaining the scientific method, Haldane wrote that, far from humbly following wherever nature leads, the true scientist "does his best to take charge of the situation. Like a skilful barrister, he places nature in the witness-box and asks her simple questions, one at a time, being guided by his preconceived notions, but ready to give them up if they do not tally with the evidence" (1935, dated July 1932). While such aggressive, gendered images of science enjoy a long tradition, Haldane's choice of metaphors here imputed special skills to the investigator: not everyone can be a "skilful barrister." Because of their special ability to find truth, scientists remained the ones best qualified to be at the head of society. He construed these scientific standards as above and beyond the average abilities of other people:

> [S]cientists are less likely than any other group to sell their souls to the devil. A few of us sell our souls to capitalists and politicians… But on the whole we possess moral and intellectual standards, and live up to them as often as other people. I think we even do so a little more often, because we possess objective standards which others do not (1946).

Scientific acumen thus entailed skills and abilities that the general public lacked. But Haldane understood the rational public to still desire those special qualities to be used on their own behalves.

The standards of which Haldane boasted, in particular the moral variety, were not the traditional standards of a man of science. For example, the expectation of marital fidelity (or at least the expectation of silence about extramarital affairs) was undone through Haldane's much-publicized divorce proceedings. Instead, Haldane refashioned those scientific virtues. Making nature speak was an austere and self-denying pursuit. Much like his incredible record of self-experimentation, scientific inquiry was best generated through the strength, vigor, and utmost devotion of the scientist.

This understanding of the scientist's special virtues belies Steven Shapin's (2008) analysis of the moral equivalence that accompanied the secularization of scientific inquiry. No longer Priests of Nature, Shapin construes scientists of the 20th century in a morally distinct position from those "great men of science" who came earlier. Shapin writes that secularized inquiry, pursued by scientific naturalists largely inspired by Darwin, in fact held a distinct object of inquiry from previous scientists. In the words of Max Weber, the "de-magification" of the world meant the death of natural theology; the object of study went from Nature to mere nature. A corollary to this secularization, argues Shapin, was a switch in the understanding of scientists as morally superior (in virtue of their superior task), to scientists as morally equivalent to any other expert, with no special authority to pronounce on what ought to be done. In fact Shapin puts Haldane in the middle of this transition to moral equivalence, this "severance of a long-standing tradition causally linking scientific inquiry to person and public morality" (2008, 25). But just because Haldane was religiously irreverent and sexually promiscuous did not make him morally equivalent to other experts; his own self-conception located his commitment to objectivity as a virtue noticeably above the fray. The morals in question were just distinct from the previous ones thought to reside specially with scientists. And they were still public: Haldane's popular writings sought to transmit to the public just what it was that made science special and unique.

Elof Carlson's reading of Haldane's vision of science in society – at least from Haldane's early writings - was "science overriding any reflective social thought on the just or desired society" (1995, 94). This construal only gets it part right. Haldane did not intend science to override, but to be more thoroughly constitutive of reflective social thought. Science education would ensure part of that task, while public recognition of, and praise for, the skills of professional men of science should add to that public acclaim for science. Haldane's use of the popular press could further both ends.

Haldane was concerned with political threats to democratic self-governance from a young age. In a letter to his mother he reflected on the harmful influence of a over-reaching ecclesiastical power, and noted that such top-down influence was hardly confined to the church: threats to self-determination could come from other corners as well. "Nowadays doctors (and to a less degree other experts) tend to occupy a similar position. Such a calamity can only be averted by universal education in science" (Manuscript letter dated XMAS 1912). Of all pursuits, the young Haldane thought science had the peculiar ability to ensure that people were not shackled by one particular ideological pressure from above. Science was an emancipatory practice insofar as an objective commitment nature itself was able to override any one doctrine.

Haldane's participation in communist politics hardly entailed any universal equality. His own political authority, though, underwent an important transformation: from one based on wealth and hereditary right to authority based on knowledge and the specific values inherent to specifically scientific expertise. His dream of a "social technocracy" maintained a precarious tension between potentially radically revisionist scientifically-based policies, and a popular acclaim for just those very policies to be enacted in the public's own self-interest. Although the likelihood of such overlap was not high, Haldane understood scientific men and institutions to be the best, most disinterested custodians of public welfare. The public couldn't necessarily all reach the same ideals of rational inquiry, but they could come to appreciate and desire what it was that science provided.

Figure 3 - J.B.S. addressing a United Front meeting in Trafalgar Square, January 1937.

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CHAPTER 3

"Not just talk": the Miller-Urey experiment in context

In recent philosophy of science, more attention has been paid to the adoption, rejection, composition, and interaction of *theories*, while considerably less has been given to the role of *experiment*. Indeed, it has been noted that even the history of science is frequently written as a history of theory, with the theoreticians emerging as the victorious figures advancing knowledge, and experimentalists remaining the humble (and often anonymous) servants (Hacking 1983).

Perhaps this preference for theory over experiment is partly explained by a disciplinary disposition toward the abstract. In the seventeenth century, Francis Bacon sought to attenuate this exclusively speculative character of philosophy, insisting that the gap between scholasticism and craftsmanship must be bridged in order to pursue a successful science. This would mean that natural philosophers would have to use their hands, and their instruments would become central to the production of knowledge. Yet within most philosophical discussions of science we find a preference for the long-term over the everyday, for the linguistic over the performance, and for the logical over the messy business of mixing, staining, pipetting, and endlessly tweaking. Those preferences can sometimes occlude much of the important business of science from the analyses of philosophers.

The ubiquity of standard philosophical analyses of scientific *theories* has sparked something of a backlash, and has resulted in a competing philosophy of the so-called "new experimentalists" (see, e.g., Hacking 1983). This group focuses on the material practice of the sciences, and highlights, among the variety of scientific practices, the centrality and autonomy of experiments. The new experimentalists have noted that experiments are not always tied to theory, and to the contrary may come to have a "life of their own." At times there has appeared to be a tension between the rival camps, disputing the philosophical priority of theory and experiment.

In what follows I do not try to adjudicate between these competing conceptions of science, as I believe it is clear that theory and experiment interact with one other in distinct fashion in separate cases, and any adequate philosophical account will demand recognition of both. Rather, in an effort to better understand the nature and role of experimentation in the nascent field of origins of life research, I will review a historically significant experiment that is widely regarded as initiating a modern era of experimentation on the origins of life. A brief review of the Miller-Urey experiment will, I hope, prove illustrative of some of the ways that experiment can contribute to the development of a field of inquiry. Beyond its epistemic support for a particular thesis, the landmark experiment serves several crucial institutional functions. In a research environment where theory is difficult to come by, experiment rises to the forefront of the research agenda, and the primary strength of the Miller-Urey experiment is that it opened new avenues of experimentation.

Experimentation is about intervening in the world, yet in several areas of science, no intervention is possible in any direct sense. In contrast to, say, finding a biochemical pathway in an extant human cell, we have no immediate access to the phenomena of the origin of life, and scientists must reconstruct its occurrence in a way that best concords with their current understanding of other topics such as early atmospheric composition, evolution, and chemical reaction dynamics.[15] The intervention must take place on materials that are now available, in order to investigate a temporally and chemically distant set of events.

Early theorizing on the part of Oparin (1924) and Haldane (1929) provided a "conceptual breakthrough" (Fry 2000) but lacked immediate experimental support. In an October 1951 lecture at the University of Chicago, the chemist and Nobel Laureate Harold Urey spoke on the origin of the solar system, and suggested that atmospheric composition should be relevant to considerations about the origins of life. He thought that experiments should be conducted in the interest of pursuing questions about the relationship between atmosphere and potential chemical reactions. It is difficult to conclude the extent to which Urey saw his proposal as a "test" of the Oparin hypothesis.

Bada and Lazcano (2000) suggest that Urey wasn't even aware of Oparin's work until after his lecture, but their account is flawed. Their reconstruction makes Urey's proposal to test for chemical reactions under reducing atmospheric conditions seem unmotivated, and nearly borne of desperation. On the idea of testing for organic synthesis in a reducing environment, they write: "Urey remarked acerbically that 'if you have to go to these measures to get organic compounds, then perhaps a new idea is needed'" (2000, 109). ("These measures" was a reference to Melvin Calvin's quite

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[15] I want to allow for the view that our empirical "access" to various bits of the world may fall along a spectrum rather than into a dichotomy between "immediate" and "hidden." However, I think we may still contrast different poles of this spectrum. Cellular biologists have an easier time intervening on organelles than do cosmologists intervening on black holes.

involved 1950 experiments using a cyclotron to generate high-energy helium ions with which to irradiate carbon dioxide; the difficult method produced only trace amounts of formaldehyde, a first step in the synthesis of sugars.) The implication here is that the notion of organic synthesis in a reducing environment was an *ad hoc* suggestion for a new path of inquiry, rather than a theoretically-motivated hypothesis.

That account of the order of events is almost certainly wrong, given evidence that Urey was influenced by at least Oparin's work before that 1951 lecture. In a letter dated September 17, 1951, Urey suggested in written comments to colleague Oscar Riddle that Riddle incorporate Oparin's work.[16] The comments include reasons why a reducing atmosphere would be mostly likely on early Earth, in anaerobic conditions before the appearance of oxygen-generating photosynthesizers. Moreover, included in Urey's 1952 book, *The Planets: Their Origin and Development* under a section titled "Early Chemical Conditions" is a discussion which lists Oparin by name. Urey was thus clearly working with these ideas before the fall of 1951.

Stanley Miller, then a second-year graduate student, was part of the crowd at Urey's 1951 lecture and remembered the suggestion, but did not immediately pursue it. Urey's proposed experiment might perhaps have lain fallow:

> [Miller] did not immediately jump at the opportunity to do the experiment that Urey had suggested. According to Stanley, he 'found experiments to be time-consuming, messy and not as important', and as a result, he decided to concentrate on theoretical work (Bada and Lazcano 2000).

After little success in this short-lived theoretical endeavor, and an advisor who moved across the country, Miller eventually decided on Urey as an advisor and positioned

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[16] I am grateful to Matt Shindell for pointing out this letter to me.

himself to undertake Urey's suggested experiments. Urey initially counseled against the experiments, warning his new graduate student that the experiments were risky and unlikely to succeed (much less lead to a Ph.D. in just a few short years).

But Urey eventually granted a wary approval to the experiment. The apparatus was built to simulate likely geochemical conditions, and not necessarily to generate organic molecules. Water was boiled beneath a gaseous mixture of ammonia, methane, and hydrogen, which was subjected to a constant electric discharge to simulate the energy source potentially provided by lightning. With these ingredients, the primordial "ocean" and "atmosphere" were left to themselves under the influence of the energy source.

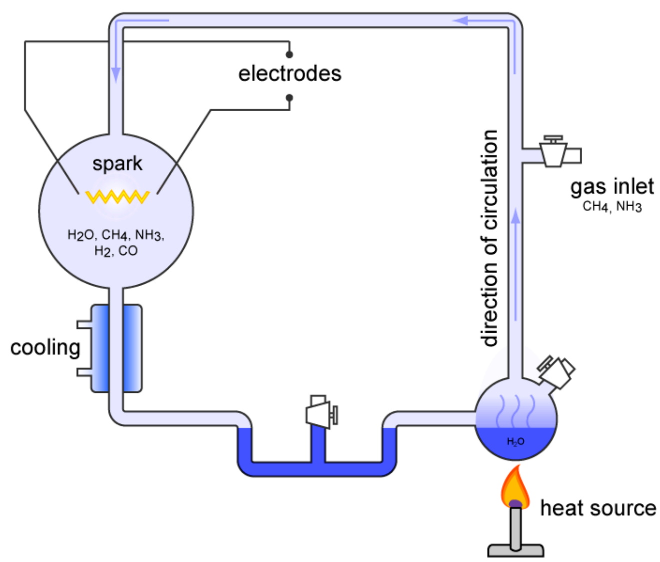

Figure 4 - A Diagram of Urey and Miller's experimental apparatus.

Miller suspected that fruitful results were forthcoming when, after only days, a change of color was affected in the liquid, with a reddish tint developing over time. In

1953, the 23-year-old Miller reported his results to an illustrious crowd of scientists at Chicago. After only a week of running the experiment, Miller detected a racemic mixture of several amino acids, the building blocks of proteins. He reported that 10% or more of the carbon in the system (from the gases) had been converted into organic compounds. The results were seen as validating the Oparin / Haldane hypothesis that inorganic precursors in a reducing atmosphere could generate organic compounds with the right energy input. The results were still surprising enough that publication in the journal *Science* was delayed because one of the reviewers simply didn't believe Miller's results, and the paper was only published with the imprimatur of Urey, the respected advisor.

The abiotic origin of organic compounds was not in itself news: chemists long knew of the Wohler Synthesis, in which urea can be synthesized from ammonium cyanate. Freidrich Wohler's 1828 discovery convinced many biologists that there was no metaphysical gulf between organic and inorganic, thus impinging on the vitalist belief of a principled separation between the realms of organic and inorganic worlds. But the generation of organic materials from inorganic in suspected geological conditions had remained unproven, and the Miller experiment had convinced the relevant community of scientists that these reactions were not only possible, but indeed relatively plausible given the experimental constraints.

While the construction and use of novel experimental apparatuses is never straightforward, there is a sense in which the Miller-Urey experiment is quite simple: there was no genuinely new technology involved, the chemistry is (retrodictively, at least) quite well understood, and the methods of analysis – paper chromatography – were relatively easy. Miller has later said that it is a virtue that the experiment is "so simple that a high school student can almost reproduce it" (Henahan, 1996).

Despite this simplicity, the experiment is widely regarded as the inauguration of the experimental study of prebiotic chemistry. Over and again one reads how the experiment "began the modern era of study." Other prebiotic simulation experiments soon followed in laboratories around the world, all with similar strategies: simple compounds are used to synthesize complex, biologically relevant polymers (Oro, 1961). Rather than attempting to *start* with known chemical reactions, this class of experiments would first specify an environmental context, construct a simulation, generate products, and later reconstruct the chemical intermediates that yielded them. The Miller-Urey experiment became a template for other experiments and a vision for new possibilities.

In this episode, it seems that experiment has both constrained, and, in a sense, overtaken theorizing. In one interview Miller recounted:

> The more important research are the experiments these days, rather than the trading of ideas. Good ideas are those that when reduced to an experiment end up working. Our approach is to do experiments and demonstrate things, not just talk about possibilities (Henahan, 1996).

This is a substantial change from his earlier assessment of experiments as "time-consuming, messy, and not as important." But Miller's statement reflects more than an operationalist impulse to "make things work." It also reflects a belief in the primacy of experiment over theory. In itself, however, Miller's experiment does not seem sufficient to substantially answer the questions many scientists were initially interested in: how life in fact began on Earth. Although accepted and even exalted, its scientific relevance is today hardly secure.

In the above quote, Miller doesn't address the question of how and why experiments "demonstrate things," or, moreover, how they demonstrate the *relevant* things. The conclusions of experiments do not stop with the description of some state of affairs that you brought about. They do not only extend our realm of experience. Significant experiments will also point towards something. Miller's original paper did not completely answer the question of what the experimental results are evidence *for*.

During Miller's initial presentation of his results to the Chicago audience, the discussion may not have gotten that far. As the story goes:

> At one point, someone – according to [James] Arnold, Enrico Fermi – politely asked if it was known whether this kind of process could have actually taken place on the primitive Earth. Harold Urey, Stanley's research advisor, immediately replied, saying 'If God did not do it this way, then he missed a good bet'. The seminar ended amid the laughter and, as the attendees filed out, some congratulated Stanley on his results (Bada and Lazcano 2000).

Amidst the jests, however, significant questions may have been passed over. The "good results," while laudable, here seem lacking without further interpretation. For the very "goodness" of the results may be thought to depend on some degree of fit with accepted theory. It is only evidence for the possible prebiotic organic reactions insofar as its assumptions are upheld.

The published version of his paper (Miller 1953) is an account of the experiment, not speculation about its implications. But the implications were clear: the experiment was relevant because of its promise that, *if the assumptions held*, amino acids could be generated relatively easily, and that those could conceivably result in biological activity. The paper's title made this explicit: "Production of Amino Acids Under Possible Primitive Earth Conditions."

But were those in fact the primitive Earth conditions? Atmospheric composition was questioned early on by Holland (1962) and and Abelson (1966), and more recently by Kasting and Catling (2003). Those studies suggested that the early atmosphere was not reducing (with large amounts of methane, ammonia, and hydrogen), but closer to neutral, and rich in carbon dioxide. Miller and Orgel (1974) wrote that, in the absence of certitude about the environment, their experiments actually provide evidence for the environmental problem.

> Arguments concerning the composition of the primitive atmosphere are particularly controversial. We believe that there must have been a period where the Earth's atmosphere was reducing, because the synthesis of compounds of biological interest takes place only under reducing conditions (Miller and Orgel 1974, 33).

The question that Urey rescued his student from answering (about whether the experimental scenario reflected a real possibility) is still debated, and most scientists are much less certain that reducing atmosphere, thought so crucial for the formation of biomolecules, in fact existed (Orgel, 1994). Atmospheric and geological data now indicate the early Earth possessed something closer to a neutral atmosphere, and without a sufficient amount of hydrogen, the chemistry of the Strecker synthesis (generating amino acids from HCN and aldehydes in the presence of ammonia) does not obtain.

This had led some researchers to dismiss Miller's results: "The atmosphere when life began was neutral. Miller's experiment only shows what might have happened if circumstances had been different" (Dyson 1999). Physicist Hubert Yockey turned up the rhetorical heat even further by accusing Miller and co-authors of reaching conclusions "based on faith" that are "appropriate in religious apologetics but not in scientific literature" (2005, 129). This has not stopped others from defending the plausibility of the

Miller-Urey low-temperature aqueous origin scenario. Notably, Miller's student Jeffrey Bada has argued, with others, that problems in other accounts of amino acid synthesis make the Miller-Urey scenario still the best theory of the origin of biopolymers (Cleaves et al. 2008). Their work has gone on to postulate special conditions that would have allowed for the same chemical reactions that took place in reducing environments. Note that the logic here is along the same lines as that expressed by Miller and Orgel in 1974: because biosynthesis takes place far more efficiently in these reducing environments, some such environment *must* have existed. Opponents to this line of thought, then and now, argue that environmental conditions must be established by independent atmospheric and geochemical data, which then must be used to constrain any further origins research. Depending on which science you prioritize, different conclusions can be reached.

While Bada's group admits that neutral atmospheres are not conducive to amino acid synthesis, they argue that the reason for this inhibition of synthesis is the oxidizing of organic compounds by nitrite and nitrate produced during the reactions. In the presence of oxidation inhibitors, such as ferrous iron, organic synthesis of amino acids in neutral atmospheres is still possible. Their research shifts the focus of attention to whether such oxidation inhibitors were present in the early earth environment, which they find "reasonable" given one other report from atmospheric scientists on the presence of dissolved iron in the early ocean (Walker and Brimblecombe 1985). Whether this "save" of the Miller-Urey scenario is viewed as *ad hoc* or a true advance remains to be seen, and will likely depend on which other theoretical commitments one holds.

In order for Miller's experimental conclusions to be relevant, then, it seems that "talk," too, will be needed. Much more theory, in fact, is necessary to fit the pieces together, but this, it seems, is just what is lacking in much contemporary research in the field (Orgel, 1994).

Experimentalists are opportunists about the tools they use: they will mostly use whatever they think will work. The significance of the Miller-Urey experiment is its inauguration of an experimental style: it established a new experimental template for further research, in addition to the surprising organic compounds it generated. Independent of the degree of epistemic support it provided for Urey's origin hypothesis, the experiment cleared a path for novel experiments to be undertaken. Immediate questions followed in the wake of Miller's experiment: what was the chemical pathway allowing for the incorporation of carbon into amino acids? Under what range of conditions can such syntheses proceed? Could other biomolecules, or even nucleobases, be formed in such contexts? It may turn out that the experiment's potentially faulty assumptions about atmospheric conditions make its results problematic with respect to the question that supposedly structures the field, but this does not detract from the impact of the generation of a research program.

The contribution of the Miller-Urey experiment was to consolidate a field around a methodology and an experimental procedure. It became known as a successful way to gain knowledge. The International Society for the Study of the Origins of Life (ISSOL) and its attendant journal, *Origins of Life and Evolution of Biospheres,* are replete with references to the experiment. The Society's award is the Urey Medal, and the group recently celebrated the 50th Anniversary of Miller's experiment with much ado. These groups recite the story as nothing short of a foundation myth for prebiotic organic chemistry, coalescing their identity and marking out a concrete event by which they can judge the progress, chronology, and very existence of their field.

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CHAPTER 4

Origins of life and evolutionary theory

4.1. Introduction

As a research program, origins of life has frequently had to fight to establish its own scientific legitimacy. It has been a distinctive outsider: both institutionally, as a late-coming, non-departmental subdiscipline funded largely though NASA's post-Sputnik biological space programs, and also conceptually, as an area of inquiry that did not neatly fit into extant disciplinary niches in organic chemistry, biochemistry, or the synthetic theory of evolution.

When compared to, say, molecular biology, the field of origins of life might seem like a trifling, and perhaps even suspect, science. After all, the field locates its experimental roots in a procedure that can easily be performed by high schoolers with simple glassware. The Miller-Urey experiment, widely considered to be the inauguration of the field (Fry 2000), was conducted by bootstrapping together funds (totaling less than one thousand dollars) from other grants. Dick and Strick (2004) have documented the close affiliations between origins of life research and astrobiology, the development of which explicitly promoted an interdisciplinary approach to research. The field consequently came to be perceived as "an odd borderland" (Dick and Strick, 47).[17] The field today retains a certain self-consciousness about whether it counts as a genuinely "hard" science (Joyce 1994).

Origins of life's institutional status as a fringe science is connected with its quite specific central question: providing an account of a potentially unique historical event. Origins of life began as a story of how evolution could have begun in the first place: an attempt to delineate just what happened in that primeval "warm little pond." As such, there was no obvious connection between its mandate and evolutionary dynamics.

In what follows I present a case in which innovative ideas from scientific fringes can gain significance in mainstream research. In §2, I discuss how origins of life has and has not historically fit into dominant trends in 20th century biology. In §3, I present four brief case studies which articulate how topics developed in the context of origins of life have in fact interacted with debates in evolutionary biology. My case studies sketch a picture of how a fringe research program has related to mainstream science. I conclude that even while origins of life has had scant success in "answering" its central, historical question, we might locate its positive contributions in the way it has interacted with distinct problems in theoretical biology.

4.2. Evolution and Life's Origins

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[17] The authors recount how grant applications to NIH and NSF have received lower priority if those organizations became aware that the researcher had been "NASAing around" – soliciting funding from an organization promoting such suspect science. (Dick and Strick, 52).

In her history of the evolutionary synthesis, Betty Smocovitis (1996) emphasizes the significance of unification as a regulating ideal in the structure of biological theory. Ostensibly answering the disapprobation of critics such as J.H. Woodger over biology's status as a fragmented, immature science, the "architects" of the neo-Darwinian synthesis strove for the construction of a principle that would both unify biology and grant it a fixed status among the hierarchy of the sciences. By the middle of the last century, writes Smocovitis, following the synthesis, it could be argued that biology had achieved this ideal:

> Not only had the discipline long thought to be disunified, in its infancy, and rife with metaphysics become a unified science, but now, too, it was an empirical, mature science, secure in its foundations and well positioned within the positivist order of knowledge – intermediate between the physical sciences and social sciences. Evolution, stretching from the gene to the human to human culture, would bind and link the mechanistic and materialistic frameworks with the human sciences. Reducible to the physical world of the gene and grounded in the fundamental mathematical principles of population genetics, the disciplines within the positivist ordering of knowledge stood independent yet united (1996, 169).

Such a message would be reinforced for quite some time to come; it is a common picture presented by textbooks: "Evolution is, indeed, the one coherent system of principles that unifies all of biology" (Futuyama 1979).

Smocovitis writes that the balance which the synthesis had achieved between the unity and autonomy of biological sciences was threatened with "destabilization" by two watershed events in 1953. One was the articulation of the macromolecular structure of DNA by Watson and Crick, and the other was Miller and Urey's famous origin of life experiment, the results of which suggested the biochemical basis of life. The story of molecular biology's success has been told many times; its relationship to physical science and its status vis-à-vis evolutionary biology have been extensively studied. In what follows I reflect upon the historical and conceptual development of the second event: the science of the origins of life that was launched subsequent to the Miller-Urey experiment. I want to emphasize the relationship, not between biology and the physical sciences, but among divergent trends within biology itself. In particular I will focus on the extent to which the science of origins of life has both borrowed from wider projects among the biological sciences, and in turn also destabilized the theoretical edifice of neo-Darwinian theory. I argue that the unique perspective afforded by origins of life research – one that is engaged with the "primal characteristics" of life – has enabled significant (if unexpected) critiques of standard views of evolutionary theory.

Fusing Mendelian genetics with Darwinian natural selection, the architects of the evolutionary synthesis upheld the centrality of natural selection to account for evolutionary change, and by the 1950s had provided an overarching formulation of evolution. Notwithstanding many important conceptual developments, it seems fair to say that biological orthodoxy has since remained relatively close to this picture.

The consensus arising from the evolutionary synthesis, then, was that biological science was most fundamentally about evolution. But what had been an epistemic measure of the structure of scientific theory could also provide a metaphysical perspective on life itself. It was not just biological science whose subject was evolution, but life itself has sometimes seemed to amount to evolution – more precisely, life was anything that evolved. And evolution, in turn, meant natural selection, the dominant view arising from the synthetic theory. Thus, one influential response to the old question, "what is life?" is to define as living "any population of entities possessing those properties that are needed if the population is to evolve by natural selection" (Maynard Smith and Szathmary, 1999, p3). [18] NASA astrobiologists searching for life in distant corners of the solar system also have an obvious concern with characterizing life – that perhaps foreign thing for which they are looking. NASA's explicit formulation of "life" reflects biologists' central focus on evolution. Theirs has been a search for "a chemical system capable of undergoing Darwinian Evolution" (Joyce 2002).

While evolutionary theory had become the centerpiece and unifying glue of biological science, there were also areas of research which lay outside of its purview. Research into life's origin, in particular, *seemed* to be excused from the explanatory aegis of natural selection. For whether evolution is conceived as the proposition that all life emerged from a common ancestor, or instead as change in gene frequencies within a population, it seems subsequent to, and dependent upon, the prior emergence of something to allow those developments. The central problem of origins of life research has been to develop a plausible naturalized account of biogenesis that could *lead to* the point where natural selection could "take over." One textbook announces: "The central question of research on the origins of life concerns the processes by which systems of molecules *led to* the earliest cells, capable of maintained metabolism, growth, reproduction, and evolution" (Deamer and Fleischaker 1994, emphasis added). The reach of the science was *pre*-evolutionary in the sense that biologists already had a satisfactory account of evolution, and the trick was to append an account of how that process actually

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[18] The authors concede that while other aspects of life (such as metabolism) may be necessarily present in living entities, a focus on evolution dictates their concern with the "potentiating" characteristics of multiplication and heredity.

developed – to supply a historical narrative of life's origin, up to the stage when the requirements for natural selection had been fulfilled. As I will show, however, this apparent divorce from evolutionary dynamics has not been borne out.

NASA astrobiologists are not the only ones asking "what is life." The question is central to other corners of research: notably in the sciences of origins of life and artificial life, whose very subject matters would seem to rely on a characterization of life's most basic features. And attitudes about what those most basic features are has been (unsurprisingly) a topic of debate. Since the advent of molecular biology, life, along with its essential connection to natural selection, has also been intimately associated with the role of DNA, the "informational molecule." The establishment of nucleic acids as life's "software" has been frequently juxtaposed with the cell's structural housing as "hardware." Information language and computational metaphors today pervade biology. This is an instance where technological mediation of our representations of the natural world has been significant. The relationship between human culture and natural science is pronounced at times, such as the passage in Maynard Smith and Szathmary's text where computer viruses are used as the benchmark to decide whether biological viruses should be considered truly alive (1999, 12-13).

Developments in molecular biology subsequent to the synthesis were attended by great – perhaps overzealous – enthusiasm and institutional support (Beatty 1990). And the science of life's origin has not been immune to the molecular avidity found throughout biological research throughout the latter part of the twentieth century. It is common to see information-centric accounts of life's origin (Maynard Smith and Szathmary 1999) and for the past several decades, the majority of researchers have favored a gene-first view, while a minority has held out for a metabolism-first account. In particular, much work has focused on possible 'RNA world' scenarios because of RNA's dual functions as both genetic template and catalyst. Many difficulties remain for such an account because of the highly improbable polymerization and concentration of RNAs in prebiotic environments. Nevertheless, it remains commonplace for theorists in this field to suppose that a self-reproducing "nude gene" (likely RNA) built up around itself a complex set of nucleic acids and protein molecules which made for a self-reproducing system coordinating a metabolic flow and capable of evolution.

The focus on natural selection and the primacy of the gene have become standard ones in theoretical biology. Few areas of research have been untouched by this focus, and origins of life has been no exception. In the following section, I present several case studies of research from origins of life. While they illustrate some ways that origins of life has incorporated ideas from other corners of research, they also demonstrate some novel contributions the science has made, and how those contributions have influenced the received understanding of evolutionary theory.

4.3. Case studies

Here I present four ideas that have been developed or deployed by researchers explicitly working on the origins of life. I single out these ideas not for their particular success at pinning down the *right* story of life's origin, but because of their apparent contribution to broader discussions in biological theory.

Stuart Kauffman's work on the emergence of order within complex systems has attracted considerable attention (and often perplexity!). Biological order has typically been understood solely as a product of an all-sufficient natural selection. Kauffman's alternative is an exploration of inherently self-ordering biological systems, and it constitutes a major step away from the selectionist paradigm. A central focus of Kauffman's work concerns life's origins. Rather than the highly improbable scenarios faced by earlier theories, Kauffman develops a body of theory in which life is "an expected, emergent, collective property of complex systems of polymer catalysts" (1993, 287).

At the root of Kauffman's theory lies a combinatorial grounding for a complexity threshold beyond which autocatalytic sets of polymers emerge spontaneously. In a random collection of catalytic molecules, Kauffman finds that the likelihood of autocatalytic closure (where members of the set catalyze each others' production) increases with the number of molecules in the set. When a critical complexity is reached, life can "crystallize." No genetic system is here required. Kauffman's conclusions seem to hold for a variety of assumptions about prebiotic chemistry and about the kinds of polymers involved, and would also help to explain why free-living systems exhibit a minimal complexity.

Origins of life is one manifestation of the issues of complex dynamical systems; for Kauffman, it is part of a larger project of developing a framework for spontaneous organization, or "order for free," as he sometimes calls it. Kauffman addresses similar problems in genetic regulatory networks. There, his "genetic program" is formulated as parallel distributed regulatory networks rather than serial genetic algorithms. Kauffman uses computer simulation to discover properties of some fitness landscapes in which one locus contains variations whose contribution to fitness depends on many alleles at other loci. This strong epistasis creates what Kauffman dubs a "rugged fitness landscape" – a metaphor that is meant to contrast with Wright's image of traits consistently moving an organism uphill to local adaptive optima.

As with origins of life, so also with genetic regulatory networks: it has been all too common to exaggerate the extent of design space available to natural selection. Kauffman's work seeks to demonstrate the surprising limitations to the range of evolutionary possibilities – over which natural selection has no sway.

Kauffman challenges the earlier (he thinks unnecessary) assumption that genetic variation leads to unbounded phenotypic variation, which leaves natural selection as the sole source of order. Kauffman's framework does not undermine natural selection, but embeds it in a theory of self-organization that both defines its preconditions and limits its influence.

While the extent of its usefulness remains to be seen, Kauffman's work is an enterprising attempt to explore the origin and structure of organizations. It has served to induce skepticism over the optimizing potential of natural selection (Dupré 1992), but has also sought to provide alternative scenarios to account for evolutionary change. The work has broader ramifications in biology: "Kauffman's treatment of the problem [of life's origin] leads to a new type of model that holds great promise for biology in general" (Wagner 1993).

The modeling itself is not the only contribution here: the perspective of a physicist, working on an abstract level and with explicitly mathematical concerns, has provoked renewed concerns over biology's relationship to physics and its disciplinary autonomy. "Current work on self-organizing systems may provide a stimulus not only for increased problem solving within the Darwinian tradition, especially with respect to origins of life…. but for deeper understanding of the very phenomenon of natural selection itself" (Weber and Depew 1996). Weber and Depew stress that self-organizational phenomena are dependent on stochastic processes, and want to reconfigure natural selection as a process rooted in probabilistic, rather than Newtonian, background assumptions. Weber and Depew use such constructions of self-organization to situate biological science as firmly rooted in universal (probabilistic) laws rather than earlier "narrativist," "anti-law" accounts cast in Ernst Mayr's mold of biological autonomy.

Kauffman's work is an analysis of order and the extent to which particular kinds of order are unique to living systems. If natural selection is the only source of biological order, then perhaps living systems are fundamentally different from nonliving ones (Oyama 1992). On the other hand, if biological order can be explained without selection, then living and nonliving systems may have important similarities, and Kauffman's "generic properties" of complex systems operate as high-level regularities explaining ordered states of *any* physical system, biological or otherwise. According to one standard view, biology studies the features of physical systems that contribute to the functioning of living systems (Bechtel 1993), implying that biology is a science of function rather than form. Kauffman, though, denies that characterization, and to that extent his work would re-orient biology as a study of form – a transformation which has far-reaching implications both for philosophers and biologists (Burian and Richardson 1996).

The physicist Freeman Dyson has developed an account of life's origin which, like Kauffman's, provides an alternative to more standard gene-first accounts of life. This theory provides the basis for the second and third ideas I want to discuss: neutral evolution and symbiosis. Dyson's account de-emphasizes the role of genes and selection, and instead displays a more holistic concern with a complex system's ability to maintain itself. He sees his theory as contrasting with dominant themes within evolutionary biology. Dyson considers "the primal characteristics of life to be homeostasis rather than replication, diversity rather than uniformity, the flexibility of the genome rather than the tyranny of the gene, and error tolerance of the whole rather than the precision of the parts" (1999, 90).

Dyson's two-stage hypothesis for life's origin begins not with replicating genes, but with a proto-metabolic cell. In the first step, an autocatalytic, metabolizing, boundary-maintaining system of proteins is established through a process similar to random frequency drift in a finite population. The assumption of a relatively brief time-span in his model leads to his use of drift: "Darwinian selection will begin its work after the process of genetic drift has given it something to work on" (1990, 20). Only later, within this metabolic context, does RNA appear as an intracellular, self-replicating parasite that eventually becomes integrated into the life-cycle of the host. This second stage of Dyson's double-origin hypothesis implies that nucleic acids are the oldest and most successful cellular parasites.

Dyson's theory is attractive for a number of reasons. It relieves the need to account for life's dual nature (as both a metabolizing and replicating system) as a single process, which seems, at present, exceedingly improbable. A dual-origin hypothesis is also favored because amino acids, the constituents of proteins, are capable of spontaneous formation in prebiotic environments, whereas nucleic acids are relatively much more difficult to synthesize. A protein-based metabolic system, then, would be more likely to develop independently than would self-replicating nucleic acids. Nucleotides, because they are such unstable molecules, would also have a much better chance of accumulating and polymerizing inside of a metabolizing host. Dyson's account is often considered one of the "most interesting and plausible models" for life's origin. (Moss 2003, 191).

Many theorists have taken notice of Dyson's theory, and not just those working in origins of life. Mootoo Kimura has enlisted Dyson's theory in support of the neutral theory of molecular evolution, according to which most evolutionary changes at the molecular level result from random fixation of selectively neutral (or nearly neutral) mutants through drift. Much fruitful work has resulted from the attempt to discern the relative significance and explanatory power of random drift. Kimura believes that his neutral theory accounts for the vast majority of evolutionary change, and therefore that emphasis on natural selection has been misleading. Kimura draws on Dyson's belief that "drift was dominant in the very earliest phase of biological evolution." Nodding to Dyson's theory, Kimura writes, "…the most prevalent evolutionary changes that have occurred at the molecular level since the origin of life on earth are those that were caused by random genetic drift rather than by positive Darwinian selection" (1992, 229). For Kimura, the (possible) temporal primacy of neutral evolution serves to help establish its conceptual significance as well.[19]

The second (symbiotic) stage of Dyson's theory has also garnered attention from theorists concerned with evolution. Symbiosis is an old idea that has found a modern

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[19] Dyson explicitly credits Kimura and Margulis as sources he relies upon in formulating his own "philosophical viewpoint" (21). I do not mean to imply, of course, that Dyson himself originated the ideas of neutral drift or symbiosis. It is not simply the original credit of the ideas that I'm interested in, but the fact that their articulation and deployment in a context of life's origins has in turn served to support their significance within evolutionary biology.

incarnation in Lynn Margulis's investigation of eukaryotic organelles as endosymbionts. (Margulis 1982, Sapp 2003). Margulis's own early work, incidentally, was funded through NASA's astrobiology program, as she could not obtain resources through the NSF. Symbiosis is no longer a heretical theory just of microbial associations and mitochondrial origins, and has gained widespread acceptance as an historical process. It is interesting, then, that Dyson now has appropriated this idea in his own formulation of life's origins. Lenny Moss (2003) credits Dyson with providing a novel, and important, "heuristic perspective" on the ability of symbiosis to account for evolutionary novelty.

Recent analyses of the human genome have shown the prevalence of transposable elements, called "parasitic" because they come equipped with their own promoters and enzyme templates to advance their own replication (Baltimore 2001, Lander 2001). The large quantity of parasitic transposable elements in the genome, far from being biologically irrelevant, is arguably a "motor-force of genomic innovation" (Moss 2003, 191). These elements can serve as important sources of innovation and reconstruction, as they can create new genes and new ways to regulate other genes (Federoff 1999). So the coding portion of the genome is hardly the only element of evolutionary significance: the evolution of *regulation* of that genome is also of primary consequence.

Due to this newfound appreciation of the diversity of genomic components, biologists have sought an understanding of how it is that ostensibly parasitic nucleic acid could come to be essential to the evolution of increasingly complex life forms. Dyson's theory of life's origin locates such parasitism at the very heart of life itself. The fact that ATP (a universal energy-carrier in modern cells) and AMP (one of the nucleotides composing RNA) are so chemically similar, and yet so functionally distinct, suggest to

Dyson that ATP had first become established in its metabolic energy-carrying role, and that it could have spontaneously polymerized into a kind of proto-RNA. Presumably, most cells would have been killed by this first "parasitic disease," but some molecules could have survived the invasion and turned the relationship into one of symbiotic mutualism.

For Moss, a better appreciation for the evolutionary significance of transposable elements enables us to avoid an improper understanding of a genome divided between non-coding "junk DNA" and all-powerful coding genes that operate themselves and can build anything. These considerations engender an altered perspective of evolution as "an ongoing symbiotic interplay between metabolic hosts and perennially short-circuiting segments of nucleic acid" (193).

A refined understanding of the primacy of selection in evolution is just one part of origin of life's theoretical contribution to biology; origins research has contributed in other ways as well, one of which I briefly note here. Varela and Maturana have formulated a theory of "autopoiesis" that conceives of life as a global network of relations establishing a self-maintaining dynamics in which action and constitution are one and the same thing for the system – a system in which the actions consist in the continuous regeneration of the processes and components that constitute the operational unit. This "operational closure," combined with individuality and an abstracted idea of metabolism, suffice for a definition of minimal life on Maturana and Varela's accounting. As a theory of life's fundamental organizational properties, the authors suppose their account to provide novel experimental tests, which could yield "potential keys for the origin of living systems" (Varela et al. 1974, 192). Autopoiesis is a property of a system rather than of individual molecules, and no genetic system is required. Other researchers in origins of life have also thought of autopoiesis as "life-defining." "Enactment of the autopoietic criteria would result in a minimal cell and would demonstrate the experimental recapitulation of life's Archaean origins" (Fleischaker, 1990).

Others have continued this line of research, taking their lead from autopoiesis, but stressing "autonomy" as the most significant contribution from Varela and Maturana. In one such accounting, membranes, catalysts, and energy currencies together constitute the components required to maintain a network of autonomous endergonic-exergonic couplings. Autonomy thus places greater stress on the minimal material and energetic requirements for life's emergence (Ruiz-Mirazo and Moreno 2004). This research is supposed by some of its proponents to have momentous theoretical import, as they are claiming to develop "a general theory of biology" (ibid., 235). Whether such ambitions could be, or need to be, fulfilled, autonomy has now been invoked in wider trends in biological theory, including systems biology (Bechtel 2006).

Work that developed within the context of origins of life has thus spurred research in many disparate directions. Contrary to the impression that origins of life was outside the domain of evolution, its mandate to formulate "general principles" establishes its relevance for biological theory on many levels.

4.4. Conclusion

Whether or not the above considerations could ever yield a biological theory to *rival* the synthetic theory of evolution, there has been a growing sense of the need to refine the standard view. There have been suggestions to "extend" (Wicken 1987), "expand" (Gould 1980), "finish" (Eldridge 1985), or "broaden" (Kauffman 1992) evolutionary theory. A certain malaise amongst theorists of biology has developed concerning a particular understanding of evolutionary theory which locates its essence in natural selection. Common to these concerns are the suspicions that evolution amounts to more than selection-driven changes of gene frequencies in isolated populations, and also that theories of evolution have been too concerned with maintenance, at the expense of origins of novel and diverse forms.

I am not suggesting that an otherwise-stolid acceptance of neo-Darwinism is being undone by insights gleaned only from origins of life research. Trenchant criticisms of the "received" (such as it is) selectionist view have indeed arisen from many quarters of biology, and have together contributed to a much richer understanding of biological processes than was on offer in the middle of the 20th century. My claim is more modest: I mean only to point out how some interesting developments that began with, or were developed within, the origins of life context have ended up contributing to arguments for the reconfiguration of the theoretical landscape of evolutionary biology.

The distinctive science of origins of life has provided accounts of life which go beyond just "those things that evolve by Darwinian selection." The origins of life research to which I have alluded not only mitigates the standard focus on replicators as our bedrock bio-ontology, but also challenges the "theoretical hegemony" (Oyama 1992) heretofore enjoyed by natural selection. It has additionally provided accounts of evolutionary novelty via symbiosis and more systemic accounts of minimal life, which can plausibly be put to use in areas like systems biology.

It is by no means clear if origins of life research is on its way to generating a complete accounting for life's genesis that suffices for continued evolutionary development. I am not alone in my skepticism. An "answer" to the question of life's historical origin does not appear forthcoming. Perhaps the science is not able to deliver much more than insight into how certain evolutionary processes that occurred were even *possible* (Dupré 1992). Nevertheless, it seems that origins of life is contributing to scientific inquiry in ways that were never part of its agenda, and likely could not have been foreseen.

Origins of life appears to be a distinctively modular pursuit: if the discipline were "completed," it would furnish the answer to its own organizing question – the historical genesis of life from an initially abiotic environment. This perspective on the science's aim is attended by a method for its evaluation: the inquiry is successful insofar as it moves toward answering this historical question. My case studies, however, suggest an alternative way of evaluating the fertility of this research program. The contributions of origins of life may instead be found in its interactions with *separate* areas of inquiry. The progressiveness of this program could be located not just within the purview of its own historical question, but in the development of a new and better ways of thinking about life's basic characteristics.

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CHAPTER 5

The Principle of Continuity in origins of life research

5.1.

In what follows I will introduce one of the reasoning strategies appealed to in origins of life research, called the principle of continuity (PC). Natural philosophy has a history of invoking nature's continuity – an apparently metaphysical precept that has acquired close ties with nature's lawfulness, intelligibility, and, more recently, with evolutionary thinking. I will provide some definitions and some examples of what scientists think PC is doing in their research. I'll make some suggestions about *why* it's invoked in origins research and offer a couple of case studies about how it is used and what it could possibly mean.

The philosophical questions I am treating include, what *sort* of principle is PC? If it is an a priori claim, is it legitimate, how can theorists responsibly use it, and what exactly might be its empirical consequences?

Through this discussion I will be suggesting that even though PC is regularly invoked in the field of origins of life, it lacks the probative power that is attributed to it. While it functions as a commitment of sort to naturalism, its usefulness as a guide to research is very limited indeed. More problematically, however, is its use to restrict hypotheses that may not accord with nature's continuity. Sometimes continuity is invoked to rule out particular empirical hypotheses. Yet the history of biology has provided reasons to be suspicious of that particular use of PC, as it may be employed in a way that unnecessarily circumscribes empirical investigation. History furnishes cases of empirical sciences advancing in the face of purported violations of this principle.

To make a diagnostic claim, it seems that one of the shortcomings in origins of life research is the lack of widely shared heuristic principles used to integrate the diverse findings of the field into a coherent or widely accepted scenario. Diverse findings from a wide swath of sciences must be coherently brought to bear on one another, and there are, to date, few commonly accepted principles by which to achieve that degree of coordination. A few such methodological strategies have been proposed, and here I'll focus just on one of those, known as the principle of continuity. Continuity is about the rate or type of changes involved in life's origin. PC is one among several heuristic principles, or perhaps constitutive principles, that is utilized in origins research.
Other heuristic principles discussed in the literature include the search for *definitions* of life; *top-down* and *bottom-up* approaches to deciphering life's basic physical constituents; a principle of *ubiquity;* and the *signature* principle (Deamer 1994, 9). These will be worth briefly describing.

Many theorists have offered definitions of life or the living state (Maynard Smith 1986, Fleischaker 1994, Luisi 1998, Cleland and Chyba 2002, Ruiz-Mirazo et al. 2004). The definitional approach seeks to generate confidence in one definition of life, and then to use the definition to probe for its satisfaction among possible chemical systems. Contemporary life shares several constituent subsystems in common, including a DNA-based genetic system engaged in replication, transcription, and translation; a metabolism with phosphate-based energy transduction and biosynthetic pathways; and lipid bilayer membranes. Yet most theorists would find it problematic to *define* life based on its current manifestation, especially insofar as they believe life must have been different at some point before current life forms or even the Last Universal Common Ancestor (LUCA).

While a broad array of empirical studies (including those rooted in physics and atmospheric chemistry) may fall under the rubric of origins of life science, much research investigates the specific properties of the first living organisms. Such research can often be categorized as either top-down or bottom-up.[20] Top-down implies that research begins with the simplest extant microbial life and moves "downwards" (invoking the direction on the phylogenetic tree[21]) towards even simpler components or functional units. This is generally recognized as an insufficient research strategy because even the simplest forms of life that now exist are so much more complex than anything able to be produced in a laboratory from chemical building-blocks. Most researchers recognize the need to compliment top-down with bottom-up research, which involves chemical syntheses that attempt to build up the structural or functional units of life from chemical precursors that may have existed in early Earth environments.

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[20] These are not exhaustive categories. Work on the RNA world often begins with the postulated intermediate stages between early chemistry and later life.

[21] I use this complex metaphor only as an explanation of the language involved in the science, and not as an endorsement of its use. Recent studies have suggested that the "tree" metaphor is complicated by phenomena such as lateral gene transfer, and that a massively reticulated phylogenetic "bush" is a better image, contrary to Darwin's suppositions and modern phylogenetic methods. Phylogenetic inference in origins of life is compounded by extremely high error rates of replication, suggesting that there were no well-defined lineage-like relations among early gene-bearing entities (Woese 1998).

Ubiquity is a principle that favors origin scenarios taking place within common or widespread environmental conditions over highly specialized or rare environments. Miller-Urey style amino acid synthesis can only take place in reducing atmospheres, and once it was realized that those conditions were unlikely to be very widespread on the early Earth (Kasting and Catling 2003, Abelson 1966), a strong commitment to the ubiquity principle would seem to suggest abandoning Miller-Urey approaches to biopolymer synthesis. On the other hand, if those reaction pathways are considered to be the far the most efficient way to produce organic products, some scientists de-emphasize ubiquity and postulate special isolated conditions such as volcanoes, hydrothermal vents, or the presence of oxidation-inhibitors that may have allowed the reactions to proceed (Cleaves et al. 2008).

The signature principle asserts that prebiotic processes should leave a signature – some causal trace -- in contemporary biochemistry. Morowitz considers the signature principle a "general principle" about the origin of life (1992, 154), and for him it is actually a consequence of the principle of continuity. "The signature principle is a powerful heuristic tool because it provides a protocol for going from richly detailed current knowledge to hypotheses about early life" (27). The high degree of biochemical connectivity in, e.g., metabolism, makes it very difficult to change elements of the system once the network is in place. Appreciating such difficulties "makes the study of current-day biochemistry a rich mine of information about pre-biochemistry" (155). Thus, knowledge of contemporary biochemistry is able to *travel* backwards in time: knowledge about the present applies to the past as well. Notions of "leaving a mark" play an important role in origins of life theorizing, where the aim of the science is to decipher the long-past structures and processes. Those past structures and processes may be very difficult to decipher without some specific downstream effects. Philosopher Carol Cleland has argued for a fundamental methodological difference between historical science (such as origins of life) and experimental science that is based on the historical sciences' need to explain traces of long-past events. "Traces provide evidence for past event just as successful predictions provide evidence for the generalizations examined in the lab" (Cleland 2002, 480). She notes that the difficulties in origins of life research such as the Miller-Urey style experiments is that:

> the logical relation between the hypothesis actually tested in the lab and the target hypothesis (about the origin of life on Earth) is very convoluted, winding through numerous highly speculative assumptions, ranging from conditions on early Earth to biochemical possibilities for producing amino acids and whole cells. This is fairly typical of experimental work associated with hypotheses about the remote past (2002, 484).

While these various heuristic principles each merit attention, I will train my attention primarily on the principle of continuity.

5.2.

The idea of a 'principle of continuity' enjoys a long history in natural philosophy. Conceptually, continuity implies some kind of unity or plenum; its opposite, discreteness, implies plurality.[22] Leibniz took it to mean (at least) that no physical change happens through a leap, and employed it in his philosophy of physics to argue against atomism. Since collisions between perfectly hard, inelastic atoms would entail that their speeds and directions must change instantaneously (and discontinuously), all bodies must be elastic,

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[22] Continuity has a related life in mathematics, where it has evolved through increasingly specific uses as infinitesimals and then limits.

and such elasticity entailed, according to Leibniz, having parts that can move with respect to one another (Ariew and Garber 1989, 132-133). So, contrary to the central thesis of atomism, all bodies must have parts.

Famously formulated as *natura non facit saltus* – "nature doesn't make leaps" – Leibniz was insisting that natural processes were in some respect "unbroken" or "uninterrupted" or "without gaps." For Leibniz, the principle was not just a way to argue against atomism and Cartesian laws of motion, but a deeply held metaphysical belief used in support of a number of other tenets regarding conservation laws and the intelligibility of nature. It was "a contingent principle of order grounded not in brute necessity, but in divine benevolence" (Mcdonough 2008). God could have made the world differently, but that would not have been the best world.

This principle's invocation on the topic of origins of life makes sense given the long period of questioning whether the category "life" could really be on a continuum with "non-life." Especially once spontaneous generation had fallen out of favor, it may have been difficult to conceive of life as anything other than a discrete metaphysical category wholly apart from the mechanistic and law-governed world of physics and chemistry. Certainly that was the case for J.S. Haldane, though he was hardly the only one who doubted the satisfactory investigation of biological phenomena through physical and chemical methods. Both he and J.H. Woodger believed that the question of life's emergence from chemical precursors was unintelligible. On the other hand, given that a spirit of Darwinian gradualism was taking root by the 1930s and animating much of biological theorizing, the suggestion that life is on a spectrum with non-life would fit squarely within the trend of accounting for biological change via incremental steps. That this principle of continuity is now posited as a *condition* for scientific investigation into the phenomena of life's initial emergence testifies to the multiple ways it has been conceived and used in the history of science. Once an explicitly theistic principle, it is now utilized as a commitment to naturalism – a kind of stipulation that God isn't interfering with the normal order of things. For Leibniz, continuity was God's way of instantiating natural order; for contemporary scientists, it is simply nature's way.

What follows are some definitions of the principle of continuity as it is used in the contemporary scientific literature on origins of life. While there are some obvious differences that appear in these attempts at definition, my focus will not be on the differences between the explicit formulations. One reason is because scientists have not inherited philosophers' fixation with precise definitions. My sense is that shared terminology is very often a *product* of empirical inquiry that often isn't settled until problems have been pursued to a sufficient extent. I think it is not uncommon for nomenclature to admit some vagueness and imprecision, especially in early stages of inquiry. Yet these definitions each reveal something important about the term's currency in the scientific discourse, so my emphasis will be on how the term gets *used* in scientific practice rather than just how it is spoken of.

A contemporary undergraduate textbook on biology offers the following definition:

> Because life probably evolved from nonlife by a continuous, gradual process, any process in life's evolution that we propose should be derivable from preexisting states. In other words, we should not expect to find sudden major changes (Purves 2001, 451).

Notwithstanding the "in other words," this first definition seems to propose two quite distinct criteria: derivability and gradual change. These criteria may or may not be closely aligned, but in any case the basic idea is: No saltatory transitions in past events or in their reconstruction in present theory. The text expresses confidence in a scientific study of this past event because, by implication, our contemporary scientific "derivations" will mirror the actual events of the past, and insofar as the processes scientists propose in the present will be continuous processes, there were no discontinuities in the past, original events that would break this mirroring.

The principle appears not just in introductory texts but in specialized tracts on origins of life as well. After noting that "the foundations of fact and experiment… are uncomfortably thin in origins-of-life research," Deamer and Fleischaker note that "plausible arguments" must also be relied upon to arrive at an adequate understanding of past biochemical events. The first of their three such plausible arguments is the principle of continuity. They describe PC:

> Those models that most clearly demonstrate a continuous evolutionary pathway leading from proposed origins to extant life forms are considered to be more plausible than models requiring a discontinuity between origins and evolved form (1994, 9).

I call this a description rather than a definition because of its obvious problems circumventing its own terminology, employing its own words in its definition. In any case, the notion arising here is focused more explicitly on contemporary scientific models of life's origin rather than on metaphysical grounds of what must have happened in the past. It is about the preferential status of contemporary theories that are relatively completed models spanning the full explanatory breadth of the origin of life. Rather than one small transition that may have been relevant to the origin story, this definition just says that those models are preferable that could offer the more complete account. While piecemeal bits of the story may be cobbled together, the ideal account is one that accounts for the transitions all the way from origins to extant life forms.

Francis Crick provided the earliest use of the term I am aware of in contemporary molecular biological study. In a 1968 paper on the origin of the genetic code, he invoked the Principle of Continuity to argue against a sequential increase in the size of the reading frame from one base at a time (giving 4 codons) to two bases at a time (giving 16 codons) and then to our present triplet code (with 64 codons). Crick writes that PC makes it unlikely that such sequential development took place because any change in codon size "necessarily makes nonsense of all previous messages and would almost certainly be lethal" (372). Each new protein-coding system would have had to evolve from scratch three separate times on that formulation, with no cumulative advantage being gained from the previous system. (The term "principle of continuity" appears without explanation or citation in Crick's paper; it is capitalized as a proper noun in the first instance of use, but not in the second.)

In the very next article in the same journal, biochemist Leslie Orgel, who frequently co-authored with Crick, used the term as well. Orgel was most likely the one responsible for the principle's transport into origin of life studies, where he is counted as one of the field's luminaries. In a paper discussing the origin of the genetic code, his formulation differed from the ones above. He wrote that PC:

> requires that each stage in evolution develops "continuously" from the previous one. It is very difficult to see how a totally different biological organization could have undergone a continuous transition to the nucleic

> acid-protein system with which we are familiar. Thus, at least until such time as a reasonable detailed model of a novel system is suggested and a means for its evolution into the present system is proposed, I feel justified in supposing that certain features of the contemporary genetic system emerged very early in the development of life (1968, 381).

This definition contains more content: it stipulates a certain kind of biochemical continuity that is shared between all life forms. The biochemistry of today is most likely the biochemistry of early life. Because of continuity *plus* the lack of any account of how a "totally different" organization could have transformed into the contemporary nucleic acid – protein system, we can content ourselves with the study of the contemporary biochemical system rather than worry about other possible, non-actual biochemistries.

This is a significant constraint, because a major burden of the field, as James Griesemer (2009) has pointed out, involves characterizing life in a way that doesn't beg all of the relevant questions about what life *must* be like. Prima facie, it seems that life might have been very different than it in fact is now, and it might have originated in forms that it currently doesn't display. Orgel claims we're justified in investigating our nucleic acid-protein world because it emerged very early on: it couldn't be a late-comer. With no viable alternative models of genetic and metabolic processes, we can safely assume it is the only organization worth investigating.

The basic idea behind these various formulations is that PC is the sort of principle that is required to secure confidence in a scientific investigation of a historical event. PC is supposed to link the inquiries of contemporary science with the ostensible events of the past. Any metaphysical discontinuity in the actual genesis of life – any "magic spark" that animated the first microbe – would frustrate any present hopes to accurately reconstruct those events using naturalistic assumptions.

Broadly, I suggest that there are two sorts of uses that are sometimes proposed for this principle. PC is sometimes used to *guide* research and *restrict* hypotheses. In the former capacity, the hope is to use the idea as a kind of posit that helps you look for particular things – processes that connect in causally relevant ways with other chemical processes that we have reason to think are important to life's origin. It would allow you to align your research program with features that you are more confident in – e.g. contemporary protein synthesis, or an RNA world. The researcher begins with what she is confident in, and explores outwards from there. If confidence in an RNA world is a starting point, then investigating the possible synthesis pathways for ribose sugars (a constituent of RNA) is one natural route to proceed. But such methods hardly require their own principle or nomenclature, insofar as it will amount to nothing more than "being rational" in whatever fashion is deemed best by the contemporary standards of the science.

In the latter capacity, PC might be used to restrict research that doesn't live up to its standard. It could rule out of bounds some hypotheses that conflict with a central principle that seems to have been so useful in the history of evolutionary thinking. This could be a helpful tool in a field increasingly crowded with theories. Henceforth I will be arguing that PC fares well (though trivially) in its first capacity, but that its use in the second capacity is more problematic.

In what follows I will suggest a couple of reasons why I think PC is invoked in origins of life research. Unlike the explicit uses to which it is put that I outlined above, these following motivations are more often implicit and unarticulated, yet I believe both are reasonable hypotheses given the structure and history of the field.

5.3.

A first reason PC is invoked is because continuity between *theories* is a kind of ideal within an incredibly fragmented field. There is no "discipline" of origins of life. There is not a single department of origins of life. The community is a heterogeneous collection of scholars that may have little knowledge of the other diverse fields of inquiry that investigate origins.

Any widely gratifying "answer" to how life arose from a prebiotic environment would seem to require an impressive synthesis of diverse findings from equally diverse subspecialties. One of the most interesting social-epistemic aspects of the field is the extent to which each apparently simple claim in the origin scenario depends so strongly on other fields. When the biochemist wants to make a claim, she finds herself constrained by the atmospheric chemistry, the geology, and the myriad environmental concerns that constrain chemical possibilities on a prebiotic Earth. Hence "continuity" may find appeal as an ideal that not only describes nature's fundamental workings, but guarantees that theories will actually link up and that there will be a consistent scientific narrative that incorporates constraints and insights from several different fields of study.

A second reason why I think the principle of continuity is invoked is because of its particular history in biological theorizing. Figures and ideas can loom large over a field even when scientists themselves don't necessarily acknowledge those historical antecedents. But theoretical biologist David Penny at least does nod to history when he says of the principle of continuity "Basically, we aim to explain the past by, in Lyell's phrase, 'causes now in operation'" (2005, 637). It will be worth briefly reviewing what

Lyell was up to in order to better understand this zeitgeist of continuity that seems so attractive to contemporary origins of life theorists.

The theoretical topology of 1830s geology prominently featured the debate between two camps that Whewell dubbed *catastrophists* and *uniformitarians*.[23] Catastrophists like Cuvier argued that the past was interrupted by events quite unlike what were presently experienced. In practice, catastrophism was typically linked with some sort of directionalism: the idea that an overall direction could be discerned in Earth's history (simple to complex, lower life to human). Indeed it was the catastrophes that helped to bring about the world in its current state, which is demonstrably different than it used to be, based on fossil and other geologic evidence.

Against this background, Charles Lyell's *Principles of Geology* was the primary text arguing to the contrary that earth history was a gradual process with no apparent directional change at all, but rather a protracted geological oscillation of incremental natural changes.

To be more specific, Lyell seemed to have three distinct goals in mind (Ruse 1979). The first, *actualism*, involved explaining the past in terms of the *type* of causes now in operation. The second, *uniformitarianism*, involved explaining the past in terms of the same *degree* of causes now in operation. You could obviously have the first without the second, but Lyell sometimes conflated the two, often lumping them together. Jointly they contribute to the core of his Principles. Third, Lyell argued for a *steady-state* view of the earth, which implied a perpetual cycle of degradation and eruption, with no

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[23] Whewell seemed to have a knack for just the right neologisms. He coined the word "scientist" as well.

overall direction or progress in either the organic or inorganic world. (The one exception he made to the no progress rule was distinctly human mental capacities.) It seems that the first two goals are still held relevant in thinking about origins research.

Lyell's insight was that a massively extended time scale "flattens out" the apparently catastrophic events into long-term fluctuations about a mean, leading to his ability to postulate incremental, gradual history. But Lyell tried to label catastrophism as unscientific, and his interpretation has exerted tremendous influence on the history of science (Bowler 2003, 131) – including, perhaps, in origins of life research.[24]

Catastrophism wasn't a silly theory; it was not just based on miracles or satisfying literal readings of religious scriptures (though initially it may have been moreso). It was a fruitful area of science that helped to generate a theoretical basis for major developments in stratigraphy, the science of rock layering (Bowler 2003, 116). In a sense, Lyell and Cuvier were talking past one another when it came to the issue of the pace of change. Lyell could out-catastrophy anybody by just insisting that the event in question, say, that recent glacial retreats across Europe, weren't catastrophic at all – that it must have been slow and gradual, according to the dictates of his uniformitarian thinking that causes we now find in operation explain Earth history. In short, Lyell's account to accommodate nearly any example the catastrophist could offer merely by

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[24] Other parties also had a role to play in forging links between catastrophism and religious doctrines. The early 20th century self-instructed Adventist geologist George McCready Price referred to his Biblically-literalist flood geology as "the new catastrophism", which was then equated with "creationism" by his students (Numbers 1999).

stipulating that the geological event in question took place over a very long period of time and that it was part of a direction-less geological history.

I think this history matters to origins of life theorizing, largely because of what (and who) came after Lyell. Darwin not only appealed to Lyell's gradualism but considered it central to his own theory. Michael Ruse and many others have labeled Darwin an "extreme Lyellian", which puts Lyell squarely on the "right" side of history -- that is, with Darwin. Peter Bowler writes that "Darwin's theory of evolution is a classic expression of the principle of continuity in biology" where the principle is here taken to mean that "all changes are both natural and gradual" (2003, 9). However, Lyell's own commitment to a steady-state history would have precluded Darwin's insistence on a progressive evolution.

This legacy of affiliation with Darwin has placed Lyell in the pantheon of scientific theorizing for some. (Martin Rudwick referred to this historically reconstructed Lyell who handily appeared for Darwin's benefit the "Baptist to Darwin's Messiah" (1970).) But we've seen that Lyell himself made more of his theory than it required. Bowler writes of Lyell's uniformitarianism: "he extended it far beyond the limits accepted by modern science" (132).

Darwin himself rarely wrote on the topic of life's origin, but he seemed to have continuity on his mind when he wrote in 1882, near the end of his life, that "Though no evidence worth anything has as yet, in my opinion, been advanced in favour of a living being, being developed from inorganic matter, yet I cannot avoid believing the possibility of this will be proved some day in accordance with the law of continuity" (February 28, 1882 letter to Mackintosh). This passage followed a reference to Wohler's synthesis of urea, significant for its demonstration, for the first time, that organic products like urea could be synthesized from inorganic materials: there was no metaphysical gap between the inorganic and the organic realms. Shortly later, Darwin penned "the principle of continuity renders it probable that the principle of life will hereafter be shown to be a part, or consequence of some general law" (March 28 1882 letter to George Charles Wallich). Darwin was aware of the apparent consequences of his theory of evolution for an account of life's initial appearance, but didn't believe that the time was ripe to investigate life's origin. He seemed hopeful for a future answer to the question provided within a secular, scientific framework (Pereto et al. 2009).

It is important to note how Darwin's sense of "continuity" may have affected his own work. In several passages from *Descent of Man* and *Formation of the Vegetable Mould*, Darwin supposes that mental faculties like love, imagination, curiosity, reason, and judgment must be found in nascent forms in "lower animals" including earthworms. The arguments are generally of the species that biological (including mental) powers are differences of degree and not kind. Such strong commitments to finding mind in other animals, and thus discounting qualitative differences between humans and other animals, may very well be a result of his commitment to this continuity.

These have been my attempts to say how and why the principle of continuity is so often invoked in origins of life research: that it represents a sort of hope that it might be used to coordinate disparate areas of research, and because of its history in evolutionary thinking, where Darwin's *Origin* (and by implication, subsequent evolutionary theory) are seen as grounded in a principle of continuity. Now I will present an example of how PC arises in origins of life research.

5.4.

To see how the principle of continuity has functioned in scientific research I will assess its use in a controversy over the proposal of a novel theory of life's origin, called the "clay" theory or "genetic take-over" theory.

Graham Cairns-Smith (GCS), a contemporary Glasgow University chemist, is well known for what is often dubbed his "genetic take-over" or "clay" theory (1982, 1985). Cairns-Smith began with an environmental consideration: Like many others, he was skeptical about the possibility of reducing conditions apparently needed for biomolecular accumulation. (Traditional Miller-Urey scenarios producing amino acids only take place in reducing atmospheres, but the chemistry underlying that process, called the strecker synthesis is inhibited by neutral or oxidizing atmospheres, which are generally favored by contemporary atmospheric scientists.) He noticed also that ultraviolet radiation is responsible for other processes that produce an oxidizing effect, also evidence against the traditional organic origin scenarios. So he postulated that the most likely environment for life's emergence is as far away from the sun as possible – perhaps in submarine hydrothermal systems, where reducing conditions are known to exist amidst the mineral-laden geothermal vents.

Cairns-Smith assigns a large role to this mineral chemistry, arguing that the usual organic chemistry is too complex to be suitable for origin scenarios. While some organic biomolecules like amino acids can be relatively easily synthesized, many other important components of life, most notably nucleic acids, are notoriously difficult to assemble in prebiotic environments (Bada and Lazcano 2009). Cairns-Smith's own calculations on the possibility of precise stepwise reactions of proteins, sugars, and nucleic acids make such a process vastly improbable. He writes: "There was not enough time, and there was not enough world" for that (1985, 47).

Cairns-Smith's focus on the mineral world allows what he thinks is a more probable route to organic life, but it is a route in which inorganic components serve as scaffolding for later, organic life. On his model, clay minerals serve as "crystal genes" which can evolve by natural selection. Clay minerals are complex crystals, and Cairns-Smith suggests important similarities between those minerals and other macromolecules. He contends that crystal growth and break-up can code for information based on the distribution of electrical charges on their surface. He argues further that such genes could replicate, direct the synthesis of a new layer on the mineral surface, thus contributing to the evolution of more stable and complex structures better suited to their environment. During this process, organic compounds could be synthesized on the surface of these structures, providing distinct survival advantages in the development of novel organic structures. Eventually, organic genes – some kind of nucleic acids – "took over" the structure, the mineral scaffolding was no longer required, and life as we know it later developed [Fig 1].

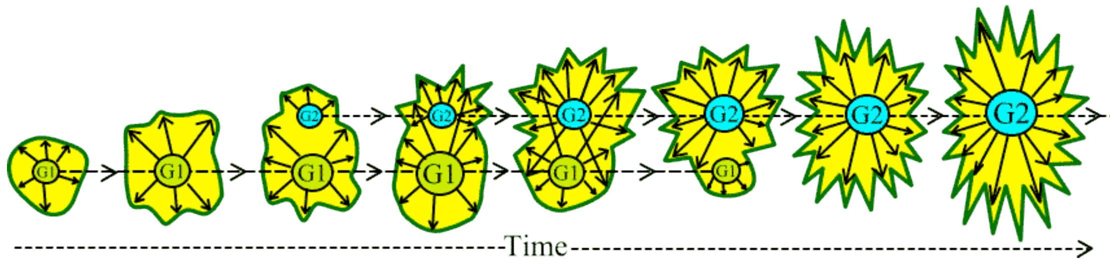

Figure 5 - Diagram of the genetic takeover. G1 represents the original crystal gene. This primordial inorganic gene helps to catalyze the organic gene, G2, which eventually takes over the phenotypic expression, as represented by the arrows. Image taken from the website: http://originoflife.net/takeover/

Cairns-Smith emphasizes the intricate, circular nature of biochemical organization, where "everything depends on everything". How could such systems develop without a designer? Richard Dawkins (1996) likened the naturalistic solution to this problem to an arch of stones representing biological organization. Could this kind of architecture develop one stone at a time in nature? Not in midair, on its own, but it could arise with scaffolding – in a pile of rocks which built up the arch, and which then slowly dissolved, leaving the arch in place. The scaffolding is initially necessary, but later on unnecessary.

Cairns-Smith writes: "before the various components of contemporary biochemistry could lean on each other, they had to lean on something else." That "something else" – the scaffolding – is the clay crystals. Other organic replicators were introduced later on, culminating in DNA, which proved to be so much more efficient at replication that the original replication system was left behind.

Genetic takeover theory was received well by the theoretical biology community; it was used by Richard Dawkins to exemplify "the properties that any satisfying theory of the origin of life must have" (1996, 149) and to explain why apparently improbable theories of life's origin must be precisely what scientists hope to find. (He argues there that an event such as life's emergence that happened roughly once in a billion years would appear to our all-too-human, naturally-selected brains as unlikely. We would intuitively consider any such event 'miraculous,' because our brains were made to assess probabilities against the background of timescales we can easily imagine, namely a few decades.) As a novel solution to the problem of life's origin, Cairns-Smith's theory has been widely noticed, but for a variety of reasons is less popular now than was in the past twenty years. Empirical evidence does not strongly favor the theory, but that does not distinguish the clay theory from other approaches, which are likewise vastly underdetermined by the evidence. What's interesting is that responses to Cairns-Smith are not always empirical in nature, but often, conceptual.

In addition to other objections, Harold Morowitz notes that contemporary organisms show "no vestiges" of clay intrusions, and their historical role in biogenesis would be difficult to decipher. Morowitz thinks that takeover theory fares poorly by the lights of continuity and is effectively "ruled out" as are other theories positing a role for mineral catalysis of organic ingredients to life.

> The principle of continuity may be introduced to critique proposed theories asserting that microstructures of clays or other minerals such as pyrite were essential elements in the transition to life. Since no clay structures or vestiges of clay structures exist in contemporary cells and since nothing in the logic of clay chemistry is unique, the introduction of the clay hypothesis violates continuity without persuasive arguments for the logical necessity for such a violation. The introduction of clays or

> pyrites… needlessly complicates origins of life theory (Morowitz 1992, 27).

This is a revealing passage for several reasons. It follows Morowitz's introduction of PC in which he likens it with Ockham's Razor. It is clear that adherence to PC is a methodological principle that is applied antecedent to scientific investigation, closely connected with "theoretical virtues" such as simplicity. His sense of the principle's significance is indicated by its placement alongside other esteemed metaphysical doctrines. Morowitz writes: "Continuity is a special aspect of the metaphysical principles of connectedness, simplicity, and causality" (27). Also, it seems that there is a high burden for any proposal to override the principle of continuity. Something like "logical necessity" is required for a theory to offset continuity. Lacking strong evidence to the contrary, continuity selects against theories that complicate the story of life's emergence.

In a later discussion directed specifically at clay theories, Morowitz repeats his claim that the transitions discussed by Morowitz (and another theory based on pyrite surface metabolism) violate continuity, and should be ruled out.

> The role of clays in biogenesis does not, however, hold up well to the criterion of continuity….The same argument developed for clays based on the principle of continuity can be directed to the theory of pyrite surfaces (Wachtershauer 1988). Again a persuasive case has been made for catalytic surfaces, but this in itself seems insufficient for the principle of continuity (Morowitz 1992, 91).

Likewise, Maizels and Weiner (1999) write that there is good reason to believe that a theory like Cairns-Smith's is simply looking in wrong place. Because molecular evolution is incredibly conservative in nature, the bits that remain now can be seen as fossilized clues to ancient past. They write that any sufficiently sophisticated structure would be very likely to be preserved, and that it would be very risky to change. Since the number of interactions within cell components is so high, a change in one molecule requires compensatory changes in many others. “When a change in one molecule would entail an impossibly large number of simultaneous compensatory changes in other molecules, then the necessity of coevolution can effectively freeze a molecule in time” (81). Thus evolution does not “obliterate its own tracks” – and as a consequence, we have reason to investigate the origin of components of *contemporary* biochemistry: that is, organic proteins, lipids, nucleic acids. If the mineral genes worked so well, they would have left some trace, if not stuck around altogether. Conversely, the ubiquity of organic replication materials we find today mean they were around from the beginning.

This idea that continuity secures the universality of protein-nucleic acid biochemistry has been put to use by other researchers as well. Citing the work of Maizels, Kunin (1999) uses PC to validate the view that the mutual catalytic dependence of RNA and protein “was a primary feature of the very first living systems” (461), thus also ruling out any pre-nucleic acid accounts of origins.

Cairns-Smith responds to the conceptual challenges such as those of Morowitz and Maizels by arguing that the principle of continuity must be discarded, because without his proposed inorganic origin, the conventional organic origin scenarios need to appeal to miracle-like low probabilities or singular events not covered by scientific explanation. He argues that positing the “unity” of biochemistry falters to the extent that it’s not providing fruitful results that answer how life could have emerged. He interprets research along those lines as stagnant. In support of his theory, Cairns-Smith points to several decades of mostly failed attempts to synthesize key macromolecules like RNA and proteins in plausible prebiotic conditions (Hazen 2005, 161).

Cairns-Smith explicitly concedes that he's giving up on biochemical continuity of the sort proposed by Orgel. While he accepts his departure from a kind of genetic or biochemical continuity, he still sees himself as playing by the "rules of the game." He doesn't see his work as unscientific and he is certainly not invoking any gods or other metaphysically suspect entities. Indeed, there is a real sense in which, according to his own logic, Cairns-Smith is the one who can claim the mantle of continuity. The reason is that he puts more emphasis on the problems with the origin of the RNA world "from scratch." Conventional accounts of an RNA world have been very popular, but still have little evidence for how the RNA world came to exist in the first place. This leaves a "gap" between the pre-RNA world and the simplest organisms we can conceptually postulate. Cairn-Smith's own theory is an attempt to bridge just that gap. He cautions: "This gap can be seen more clearly now. It is enormous" (1985, 4).

Mineral catalysis theories such as those of Wachterhauser and Cairns-Smith are attempts to account for the origin of the organic, nucleic-acid based systems of replication that probably appeared early in the development of life. Other accounts for the origin of those (typically RNA-based) systems have been plagued by chemical difficulties assembling the constituent subunits of RNAs. Upon surveying that research, Cairns-Smith declares that the probabilities for the natural chemical assemblage into self-reproducing RNAs is fantastically low – low enough to propose an alternative genetic system that could have been built up from inorganic components. Other theorists, including Morowitz, take the latter move as one that violates the principle of continuity. It is methodologically suspect to the extent that it introduces a complex narrative of inorganic mineral chemistry into what had been a (arguably simpler) narrative of a gradual increase in the complexity of organic chemistry.

Part of the stalemate here arises from the need for theorists to evaluate the work of other scientists. Origins of life is a distinctly multi-disciplinary pursuit, and very few theorists are familiar with all the disparate areas of research that compose the field. Different scientists have different expectations for the likelihood of success in other areas of study. This matters when it comes to clay theory: Cairns-Smith believes that organic accounts are plagued by intractable problems, and that his account is worth investigation. Many of those who work on organic accounts see their field as progressive and likely to bear fruit, and if that is so then the appeal to non-organic genetic systems seems quite radical indeed. In both cases, the community needs to assess the prospects of *other* areas of study.

If this debate is not dispute about the empirical merits of Cairns-Smith's model, I think it is actually more about the signature principle than about continuity. Recall that Morowitz believed in a tight link between the two principles, arguing that continuity entailed the signature principle. The signature principle seems to have much more concrete empirical consequences than does continuity – it is a much stronger principle. Although they frame their criticisms under the label of a violation of continuity, several of the comments of Morowitz and Maizels make it seem as if the real objection to theories like Cairns-Smith's is over the lack of a trace of any mineral or clay structures in current biochemistry. Their working assumption is that life's origin came about through a gradual increase in complexity of organic ingredients – just the ones that are pervasive today. Their research programs are delimited according to that initial assumption.

The signature principle seems to be in danger of making unwarranted assumptions about evolutionary history. In particular, it assumes that prebiological process must somehow be manifest in contemporary biochemistry, which can be investigated in the laboratory. It further assumes that the ingredients of current life are the ingredients that constituted ancient life. It construes biogenesis as a gradual process of construction from the same materials that eventually became highly interdependent and now evolutionarily conserved. Of course evolution rarely works the way that human engineers would. Evolution doesn't have an "end" in view, and it has no plan for the finished system. Evolution's "tinkering" complicates the way that we might think life was formed. Life could very well have emerged with different components than it has now.

Perhaps the dangers posed by the signature principle are outweighed by some sort of methodological benefit from its use. The signature principle may not be committed to any specific evolutionary histories, but only to a methodological restriction on what makes sense for scientists to investigate. Without it, maybe scientists would be at a loss for what to study, given the myriad alternate possibilities. I don't know whether the alternate possibilities really are myriad or whether it's true that scientists need to restrict their scope in just this manner. These concerns about the signature principle await further analysis. My point so far is only that PC is being used in a way such that its defenders believe it has real empirical significance. In this debate between defenders of inorganic surface catalysis and defenders of organic origins, it seems that PC is actually not doing the work that its advocates believe it is doing.

The concern is that a principle is being upheld for its own sake while prohibiting research into the merits of an alternative genetic system. Recall that Orgel's formulation of biochemical continuity was qualified: he claimed that *insofar as* scientists lacked "reasonably detailed" models of alternative genetic systems, he was justified in restricting his focus to contemporary genetic systems of DNA and protein. To then invoke this principle as a *reason* not to investigate other possible genetic systems would be an unfair restriction on empirical research – research that appears badly needed in this field. My point here is not to argue in favor of Cairn-Smith's particular model of genetic takeover, but to elucidate how the principle of continuity is used and some attendant dangers of that use.

5.5.

In its first capacity, as guide to research, PC seems to function as a pragmatic aid to practice, and its use is very old indeed. Origin of life scientists typically search for incremental changes that could have allowed novel mechanisms to emerge from simpler biochemical constituents. But if modern science will successfully appeal to this kind of stipulation, it must be sensitive to new empirical knowledge. Scientists routinely revise what they thought was possible or even "continuous" in light of learning new things about the world. For example, witnessing spontaneous crystallization in sodium acetate may very well evoke the sense that you had just witnessed a discontinuous jump. You might see this spontaneous switch from liquid to solid and see a "saltum" – at least, that is, until you acquire the cognitive and empirical resources to understand a few things about supersaturation or crystallization.

In its second capacity as a decision criteria and standard by which to judge results, PC is too weak to do its job because of the number of background assumptions that do not seem to be shared in common between different researchers. Aside from whether they even share the same definition of PC, the relevant assumptions for the application of PC include the assessment of fruitfulness of *other* areas of inquiry, and the permissiveness towards "alternative" life forms that differ from contemporary life.

David Penny writes: "it is still hard to imagine and experiment with completely different forms of life" (2005, 638). Part of the question, then, is What can you imagine? In Kyle Stanford's (2006) terms, what are the 'unconceived alternatives' to the universal biochemistry of today? It seems like what you can imagine is a cognitive and empirical question, the exploration of which should not be ruled out by an a priori stipulation.
PC is not sufficiently precise to serve as standard, and it seems bound to be either too strict or too loose to do much heavy lifting in origins of life research. If it is formulated precisely as to have specific empirical consequences, then it is too strict and will prohibit research that could be successful. If it is a loose commitment to naturalism, then it amounts to nothing other than just "being scientific" or even "being rational". I think there are compelling reasons not to be too committed to PC, where that idea has any strong empirical content. There are good historical precedents for such a worry.

One of Lyell's many gifts to Darwin's theorizing was a principled reason to think that the fossil record was incomplete. After all, the search for fossils was pretty young yet, and becoming a fossil is not at all easy: it requires a very particular process, and entails that many ancient living forms may never have achieved this fossilized state. That insight helped Darwin to answer questions about why we found so few transitionary forms.

But that was the 1840s. In the 1970s, Eldrigde and Gould (1972) shocked the biological community when they suggested taking the fossil record at face value: while some transitionary forms had been discovered (such as archaeopteryx, an intermediary between reptiles and birds), the fossil record mostly shows long periods of stasis followed by relatively fast bursts of new morphological divergence. The conclusion reached by Gould and Eldridge was to model their theory after the data: the result is what they called punctuated equilibrium.

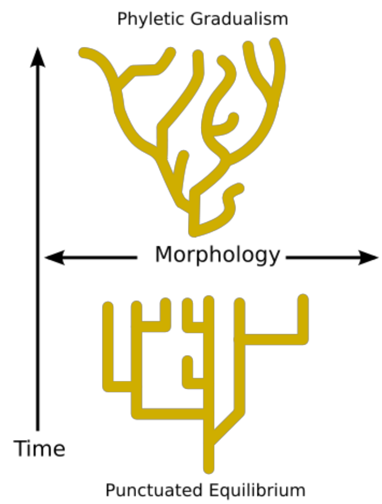

Figure 6 - On the top is a representation of the standard model, called phyletic gradualism, where the x axis is morphology space. On the bottom is punctuated equilibrium.

Even though their theory had built upon some well-known tenets of speciation first established by Ernst Mayr, punctuated equilibrium generated quite a controversy, attracting criticism for the fact that it violated Lyellian gradualism (see Sterelny 1992 for a review of the controversy). Some critics claimed that these fast bursts of morphological innovation were contrary to our understanding of slow, continuous evolutionary change. Eldridge and Gould responded that their evolutionary bursts of innovation might be "fast" in geological time, but that such changes accrued over tens of thousands of years, and hardly represented any miraculous jumps. Geologically fast can still be biologically slow.

I take it that this important episode from biopaleontology exemplifies the ways in which continuity can be used to unnecessarily restrict scientific advance. The intuition that some changes are just "too fast" carries little empirical content or warrant on its own.

So far I have shown two examples from origins of life research and likened it to another example from evolutionary theory; perhaps the principle of continuity emerges relatively unscathed from these few arguments to the defender of PC. But it gets worse for continuity, for reasons that have little to do with the genetic takeovers or punctuated evolution. That's because Cairns-Smith is right that the idea of takeover (depending on how exactly that's construed) is hardly inimical to spirit of evolution. Standard evolutionary theorizing accounts for many cases where traits evolved for one function, and then ended up serving a different role. Gould and Vrba (1982) dubbed such changes, "exaptations". Exaptations are characters that are fit for their current roles, but were not selected for those roles – they were selected for some other function (or not selected at all), and were then co-opted for a new role. Common exaptations include bird feathers, which initially evolved for insulation but then aided flight, and bones, which initially served as storehouses of calcium phosophates needed for muscular metabolism before they served their structural roles for terrestrial vertebrates.

These exaptations are a kind of *functional takeover*, and very few theorists doubt that those are ruled out of bounds by standard evolutionary theory. James Griesemer (2009) highlights a case of exaptive research traditions *within* contemporary origins of life debates that have to do with the origin of the ribosome. Griesemer discusses two competing accounts of the origin of the ribosome, the molecule responsible for translating the genetic code from messenger RNA into a peptide chain of amino acids. It is unknown how this molecule originated, and the two accounts propose different mechanisms. Both hypotheses involve fairly serious alteration in function – not just "change" in function – the way that many Darwinian processes are described, but exchanges or transfers in function. Griesemer distills their logic into categories of functional *exchange* and functional *transfer*:

•Exchange
A does F, B does G
A does G, B does F

•Transfer
A does F to B
B does F

The question is whether these changes fit within the confines of the continuity principle. Both hypotheses on the origin of the ribosome are hard-won products of empirical labor; both are open to varieties of evidence for or against them, and neither should be ruled out based on the tenets of what counts as continuous. Exchange or transfer of function may prove to be the kind of takeover that Morowitz rejects, but they seem to be within the spirit of a weaker formulation of continuity – the kind that proposes naturalistic causes and incremental transitions. The point is that lacking any specific conditions that would discriminate between unacceptable "jumps" and other well-established instances of evolutionary functional exchange or transfer, PC appears insufficiently precise. It lacks the rules needed for its own application.

5.6.

PC is widely recognized as a heuristic principle in origins of life research. It can be interpreted strongly, in a way that has real empirical import, or weakly, in a way that merely stipulates that each natural change has a cause that is recognized as possible in light of whatever contemporary science admits as legitimate.

Exaptation cases suggest PC has difficulties if constructed in a specific or strong sense, because there are myriad cases of functional takeovers in evolutionary history. Furthermore, if construed in any specific sense, it may be used to rule out important possibilities for further research.

The history of science shows that notions of precisely which phenomena count as "continuous" might be subject to change or to empirical revision. If one is inclined to salvage PC through these various revisions, then it ends up being a broadly applicable yet mostly empty dictum. It would seem to exclude little other than spontaneous, non-natural events with no explanations.

PC is being invoked as if it has serious probative or justificatory power. But it doesn't. It would appear to be a boon for scientists if it did, as the field of origins of life is lacking shared heuristic principles that would allow for the synthesis of diverse empirical data. But if the field is going to find reasoning principles common to different subspecialities, it will have to look for something other than continuity.

What then is PC doing in this literature? I am compelled to look to history: Lyell was using it to keep his science separate from his religion. In fact, the original intention of Lyell's Principles of Geology was to be a popular work entitled "Conversations in Geology" (Rudwick 1970). While the form of the work changed into a more detailed treatise for experts, it retained its original rhetorical intention of combating those who were using science to establish the possibility of a literal interpretation of the Mosaic narrative.

Invoking PC is one way to put forward your naturalistic credentials: it's a bulwark against non-naturalism. This is not insignificant given the social and epistemic space occupied by the science of life's origins. But in terms of contributing to the details of the research itself, it's just not that informative. Today's research landscape is hardly haunted by fears of ecclesiastical opposition, as it may have been 150 years ago. Conversely, few if any scientists are tempted to look for any "magic sparks" in the biochemical origin of life – such prohibitions appear now nearly built in to "methodological naturalism," (Tanona 2010), and scientists hardly need an additional "principle" that operates to serve just those ends.

The concern is that this principle could generate unacceptable constraints on scientific inquiry. That's not just a hypothetical worry: it has happened in scientific practice.

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CONCLUSION

Two centuries ago, when spontaneous generation was widely accepted, life's origin was relatively less of a problem. Today, scientific knowledge has certainly increased, but with it, some conceptual problems have become acute. While the conclusions reached in my various chapters have been cautious and sometimes critical, they have been made in the spirit of clarifying and critically analyzing an ongoing scientific project. The immature – perhaps "pre-paradigmatic" – nature of the contemporary science allows a perspective on the inside of a science under construction, where methods, techniques, and principles are all unsettled.

I have been concerned to address the epistemic principles enrolled by the science to help secure knowledge of life's emergence. I suggested that the principle of continuity was not up to the task that is often assigned to it. It is also unclear just what kind of epistemic principle natural selection might be for origins of life research; the field's relationship with evolutionary theory requires further articulation. Darwin's theory of evolution by natural selection has proved an enormously successful theory of transformations, but it has fared less well as a theory of origins. Ernst Mayr (1964) noted that, despite its title, Darwin's treatise failed to give much of an account of the *origin* of species. One of several origin scenarios not obviously accounted for by evolutionary theory is the origin of life itself. Whether the origin of life *could* even have an evolutionary explanation is a topic of serious debate: if life is considered that which is capable of evolving through natural selection, then the steps that led to its emergence would appear to be, as a conceptual matter, pre-evolutionary. Although selectionist thinking is common in theorizing about prebiotic chemistry, the current evolutionary theory of replicators presupposes an enormously complex system of DNA and enzymes that must have been absent in the initial steps to life's origin. Therefore, if life itself (often equated with the *ability* to undergo natural selection) evolved, then that could require adjustments to the contemporary theory of selection.

In chapter one I suggested that J.B.S. Haldane's adoption of a mechanistic framework of life's origin was not based fundamentally on a settled philosophical position, but was a pragmatic adoption that allowed further research. The view implied here is that scientists often do not wait to settle philosophical debates before getting on with their research, but that if one way of thinking precludes further study, then it becomes less desirable than alternative avenues that prohibit that inquiry.

Do the scientists themselves struggle with these epistemic principles? Not always. One piece of evidence is particularly worth noting here. It suggests that scientific progress in the origins of life is not tied very directly to answering the historical question of life's emergence. A recent (2009) Orgel Memorial Lecture at the Salk Institute was given by Thomas Cech, the biochemist who was awarded the Nobel Prize for his discovery of enzymatic RNAs. Those molecules are now thought to be one of the most promising lines of research into the origin of life due to their dual ability to catalyze their own replication, thus achieving a resolution of the chicken-and-egg problem that had plagued the field: sufficiently long stretches of nucleic acids require enzymes to replicate, but enzymes require fairly long stretches of nucleic acids to code for their complex structure. At that lecture Cech spoke of his relationship with Leslie Orgel and the latter's attitude about the reach of their research.

> I once asked [Orgel] if he really believed all of this stuff about being able to recapitulate early evolution, and he told me something that stuck with me and was very reassuring. "Well, it's an interesting framework – an interesting structure -- to sort of arrange your experiments around. But it doesn't really matter ultimately if you have recapitulated these ancient prebiotic events or not, because as long as you're doing really rigorous chemistry and interesting biochemistry, the results will have value and will have meaning and importance in the scientific community." And so I really appreciated that comment. In fact I found as the years went by, that I would very often cite his papers as providing key fundamental insights about, you know, the relative nucleophilicity of a 3' hydroxyl… or something like that, which was really quite… chemical observations that were quite independent of whether this had a profound influence on the first self-replicating system or not (Cech 2009).

This sentiment that the value of chemical inquiry is (at least in principle) independent of its applicability to the historical question of life's origin is in line with my suggestions from chapter four. There I argued that origins of life research has indeed furthered the progress of science, but not primarily in ways that contributed to the problem of origins. If evaluated only for its contribution to solving the historical origin of life, its success must be viewed as thin. But when evaluated on a broader scale in terms of its diversity of empirical advances and contributions to *other* areas of science – including evolutionary theory – then it may be credited with much more success.

One cannot help but notice that the language in Cech's recounting – asking the senior scientist whether he "really believed all of this stuff" about life's origin – indicates a fascinating attitude toward the "ultimate" question that their science is, ostensibly, in the business of investigating. Cech implies that the origin of life is a useful "framework" – a heuristic tool itself! – for simply doing good chemistry. That is to say, the important science could be done without any hope of resolving the historical question. In Cech's story, that ultimate question about origins sounds like a transcendent ideal. It sounds as though hushed tones were appropriate when asking Orgel if he dared to believe in that all-too-distant reality of abiotic emergence. The answer from the elder researcher was that no such faith was needed just to do the science.

Chapter two concerned the social, historical, and literary setting for J.B.S. Haldane's formulation of a theory of life's origin. That theory was articulated in the popular press, a venue Haldane often used to convey his peculiar brand of scientism, the view that many social and political problems will be resolved by improved scientific thinking. I convey some of the most significant themes of Haldane's popular science writing, and show how those themes elucidate Haldane's view of science's place in society.

My third chapter assessed the significance of the Miller-Urey experiment in terms of establishing a standard experimental model for future research into the origins of life. Its primary contribution, even if its main empirical finding is sometimes judged irrelevant, was to create an experimental scenario in which plausible pre-biotic conditions are recreated, and in which unknown, sometimes unpredicted, chemical reactions can be created and explored.

Chapter four explored the complex connections between evolutionary theory and alternative explanatory principles enlisted in origin of life science, including Kauffman's spontaneous self-organization theory, neutral evolution, symbiosis, and autopoiesis. Far from being wholly independent of evolutionary thought, such explanatory principles have often been assessed within a framework that is critical of the central assumptions of standard neo-Darwinian theory, and many of the proponents of the alternative explanations I cite have indeed been at pains to show how such explanations' relevance to life's origins further give credence to their role in theoretical biology more generally.

My final chapter critically analyzed the role of the principle of continuity in origins of life science, and suggested that it was not up to the task of providing an organizing framework for the diverse empirical findings relevant to life's origins. While it is often cited as an important constraint or condition on scientific exploration, the principle in fact does no serious work in the actual science, and demonstrates the danger of unnecessarily circumscribing scientific research by placing *a priori* constraints on the science.

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