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HISTORICAL ESSAY

That was the Philosophy of Biology that was: Mainx, Woodger, Nagel, and Logical Empiricism, 1929–1961

Sahotra Sarkar[1]

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Abstract
This article is a systematic critical survey of work done in the philosophy of biology within the logical empiricist tradition, beginning in the 1930s and until the end of the 1950s. It challenges a popular view that the logical empiricists either ignored biology altogether or produced analyses of little value. The earliest work on the philosophy of biology within the logical empiricist corpus was that of Philipp Frank, Ludwig von Bertalanffy, and Felix Mainx. Mainx, in particular, provided a detailed analysis of biology in the 1930s and 1940s in his contribution to the logical empiricists' *Encyclopedia of Unified Science*. However, the most important contributions to the philosophy of biology were those of Joseph Henry Woodger and Ernest Nagel. Woodger is primarily remembered for deploying the axiomatic method in biology but he also used semiformal methods for the analysis of many biological problems. While Woodger's axiomatic work was often derided by some later philosophers of biology (e.g., David Hull and Michael Ruse), this article defends both the biological and the philosophical significance of some of that work, for instance, those aspects that led to the recognition of the conceptual complexity of mereology and temporal identity in biological systems. Woodger's semiformal analyses were even more important, for instance, his explication of the concepts of the *Bauplan* and of innateness. Nagel's importance lies in his analyses of reduction and emergence in the context of all empirical sciences and his use of these analyses in a careful exploration of biological problems. While Nagel's model of reduction was generally rejected by philosophers of science in the 1970s and 1980s, particularly for biological contexts, it has recently been sympathetically reconstructed by many commentators; this article defends its continued relevance for the philosophy of biology.

Keywords Ludwig von Bertalanffy · Emergence · Formalization · Philipp Frank · Logical empiricism · Felix Mainx · Ernest Nagel · Philosophy of biology · Reductionism · Joseph Henry Woodger

Introduction

In his contribution to a Festschrift for philosophers of science Merilee and Wesley Salmon, Wolters (1999) argued that the logical empiricists' philosophy of biology was a case of "wrongful life" (1999, p. 187). According to Wolters: "the major congenital defects of logical empiricism's philosophy of biology are: (1) the wrong people who dealt with it; (2) the wrong general ('ideological') framework, they worked in, and consequently (3) the wrong questions they asked" (1999, p. 187). More recently Wolters (2018) has reiterated his earlier claims arguing that the logical empiricists were largely "ignorant of the biological sciences" (which is the new version of the "wrong people" claim); that "they concentrated on an unproductive ('ideological') framework (anti-vitalism, reduction) that they took to be the philosophy of biology," and "this prevented them from dealing with actual problems of biological science" (pp. 233–234).

These contentions are not uncommon: skepticism about the value of logical empiricism for the philosophy of biology goes back at least to Hull's (1974) influential textbook on the subject.[1] Callebaut (1993), besides being generally dismissive of logical empiricism, attributes to Sober the view that "issues in biology did not interest [logical empiricists] very much." Some biologists have also followed suit in

✉ Sahotra Sarkar
sarkar@austin.utexas.edu

1 Departments of Philosophy and Integrative Biology, University of Texas, Austin, TX, USA

[1] Hull's Hull (1974) explicit target was the logical empiricists' account of reduction but it is clear from the discussions that his skepticism (as noted by Wolters 1999) was not limited to reduction alone. Hull (1973), which is a review of Ruse (1973), explicitly rejects the relevance of all logical empiricist analyses of biology.

dismissing logical empiricism's relevance to the philosophy of biology—see, for example, Mayr (1988, 2004).

The *Cambridge Companion to Logical Empiricism* (Uebel and Richardson 2007a) ignores biology altogether: there is no chapter on logical empiricism and the philosophy of biology even though there is a chapter on logical empiricism and the philosophy of physics (Ryckman 2007), which is not a surprise, but also chapters on the logical empiricism and the philosophy of psychology (Hardcastle 2007) and the philosophy of the social sciences (Uebel 2007). The index has no entry for "biology." This is particularly surprising because (Rieppel 2003) had recently demonstrated the influence of Carnap's (1922) *Der Raum* and Woodger's work (see below) on Hennig's (1950) formulation of cladistics in systematics.[2]

The purpose of this article is to challenge this legacy of demeaning the relevance of logical empiricism to the philosophy of biology. It will suggest that, once we take theoretical debates amongst the biologists in the 1930s and 1940s into account, the contributions made by logical empiricists from the late 1930s till the late 1950s fall centrally within the conceptual landscape of contemporary work in theoretical biology (what Wolters refers to as "ideology"). Only slightly more polemically, it will also suggest that some of the logical empiricist analyses continue to provide insights even today.

These claims are no longer entirely novel. In particular, Hofer (2002, 2013), Byron (2007), and Nicholson and Gawne (2014, 2015) have defended related claims. I use and comment on Hofer's work below; while she focuses (mostly appropriately for her biographical purposes) on networks of personal interactions between the relevant figures, my concern here is with the intellectual content of the logical empiricist analyses of biology. Byron's contribution is to point out that there was a long and lively tradition within the philosophy of biology (and not just limited to logical empiricism) long before the late 1960s when Hull and a few others are often supposed to have founded the discipline. However, his methodology is entirely bibliometric; in contrast, the concern of this article is to trace and analyze the critical philosophical arguments. Nicholson and Gawne (2014) aim to rehabilitate Woodger. They ignore Nagel and, by and large, accept the view that logical empiricism was of little value for the philosophy of biology. (According to them, although they acknowledge the complexity of this issue, Woodger should not be treated as a logical empiricist.) Another difference between their analysis and this article is that, when they consider Woodger's formal work, Nicholson and Gawne's defense of Woodger is largely sociological, dealing with the reception of Woodger's work in its contemporary context; in contrast, this article critically examines the content of Woodger's most important formal contributions. Nicholson and Gawne (2015) defend organicism in contrast to logical empiricism as the appropriate philosophy of biology.

The debates amongst the biologists alluded to earlier, starting in the late 1920s and continuing through the 1930s and 1940s, were both scientific and philosophical. The most important philosophical debate was that between mechanism and organicism (and, also, to some extent emergentism), and it dominated theoretical discussions within much of biology into the 1950s (see the third section). Within the logical empiricist canon, this debate played itself out in the disagreements between Woodger on the side of the organicists and Nagel tentatively on the mechanist side. Nagel left a lasting legacy in his analysis of theory reduction and reductionism. Woodger's formal analyses revealed (1) the complexity of mereology in the biological context which was subsequently recognized by contemporary biologists, as well as (2) problems with the definition of temporal identity for biological systems. Woodger's semiformal analysis of a variety of biological concepts, especially those of the *Bauplan* and innateness, are perhaps even more important. Nagel also provided a telling critique of the doctrine of emergence that remains pertinent today. His analysis of teleology in 1961 was less important and changed radically in the 1970s in response to the advent of molecular biology—but post-1961 developments are beyond the scope of this article. (The year 1929 was chosen as the beginning of this story because that was when Woodger's first philosophical book, *Biological Principles*, was published. The year 1961 was chosen as the end because it saw the publication of Nagel's *Structure of Science*, which is widely regarded as the epitome of logical empiricist philosophy of science (e.g., see Suppe 1974).)

The discussion below is organized around Wolters's claims but that is not because they should be singled out for special criticism—as noted earlier, such views are commonplace. It is to Wolters's credit that he crystallized these views precisely even though they ultimately turn out to be indefensible. The second section will address the claim that the "wrong people" addressed biology within logical empiricism. It will show that almost all the relevant logical empiricists had impeccable biological credentials. The next section turns to the question of the "wrong framework." It will document the extent to which the debate between mechanism and organicism dominated philosophical discussions of biology in the 1930s and 1940s. Thus, the logical empiricists quite appropriately focused on this issue in the 1940s and 1950s. Section four will consider whether they asked the "wrong questions." It will describe in detail and critically defend

[2] In response, Uebel (personal communication, August 4, 2014) has pointed out that when the volume was first being planned around 1998 there was no extant literature documenting attention to biology on the part of the logical empiricists.

the contributions of Nagel and Woodger. The arguments of both the third and fourth sections have already been briefly rehearsed in the last paragraph. Section five will turn to the question of the legacy of these logical empiricist analyses for the philosophy of biology today. It will provide a qualified positive assessment of both Woodger's semiformal technique of conceptual analysis and Nagel's work on reduction. Final remarks occupy the sixth section which turns to the question why the logical empiricist contribution to the philosophy of biology came to be denied by the self-appointed "founders" of that discipline in the 1970s. Philosophers of science (including philosophers of biology) seem to be largely unaware of the history of the philosophy of biology from the 1930s till the 1960s, and this article is also intended to be a corrective to that problem.[3]

The Wrong People?

Did the wrong people address biology within logical empiricism? Before attempting to answer this question, two points should be noted:

1. This is an uninteresting question unless the relevant features of these persons were directly responsible for poor work in the philosophy of biology. This is the case that Wolters tries to make, noting *ad nauseum* that major figures amongst the logical empiricists (the figures he mentions are Carnap, Hahn, Hempel, Reichenbach, and Schlick), with the exception of Neurath, were trained in mathematics and physics, and not in biology. (Neurath had a background in economics and history.) Before questioning this claim (in the rest of this section), it is important to note that having formal training in biology need not be a prerequisite for making contributions to the *philosophy* of biology—the question should be whether the relevant logical empiricists had adequate familiarity with biology and that is the level at which I will examine this claim.[4]
2. Much depends on what the scope of logical empiricism is taken to be, in particular, because there is some reason to question whether Woodger's work should be seen as part of the logical empiricist corpus (Nicholson and Gawne 2014). Wolters includes within logical empiricism the Berlin group around Reichenbach besides those figures who were part of the Vienna Circle at some point in their careers. This already means that, as Wolters seems to recognize, the anti-metaphysical rhetoric of the (mainly early) Vienna Circle (in particular, that of Neurath) should not be viewed as being as central to the identification of the relevant figures as four other features of logical empiricism: (1) the emphasis on empiricism; (2) the deification of formal logical analysis as the instrument for philosophical progress; (3) the promotion of the unity of science; and (4) a linguistic turn, that is, a belief that philosophical (and, possibly, scientific) progress can be achieved through a reformation of language. After World War II, when logical empiricism largely came to be a U.S.-based movement, these are the features that distinguished it from other philosophical schools; in particular, the second feature serves to distinguish it from the largely allied program of home-grown pragmatism.

That said, it is time to turn to the content of Wolters's (and other similar) claims that only figures not competent to pursue the philosophy of biology worked on biological problems within logical empiricism.

Philipp Frank

As Hofer (2002, 2013) has pointed out, at least one of the figures within the inner circle of logical empiricism, namely Philipp Frank (1884–1966), did have formal training in biology at the University of Vienna, sporadically between 1902 and 1908, at times studying with Hans Przibram at the Prater Vivarium. As early as 1908, well before the Vienna Circle period, Frank (1908) had published a paper, "Mechanismus oder Vitalismus," in the *Annalen der Naturphilosophie* that was a careful treatment of the dispute, pointing out, among other things, that there was no logical argument against vitalism even though mechanistic assumptions may have greater heuristic value in biological research. His 1932 book, *Das Kausalgesetz und seine Grenzen* (Frank 1932), included

[3] Byron (2007) documents the extent of this problem. Note, for instance, Ruse (1973, p. 9): "The author of a book on the philosophy of biology need offer no excuse for the subject he [sic] has chosen, since few areas of philosophy have been so neglected in the past 50 years"; or Hull (1974, p. 6): "The purpose of this book will be to take a closer look at that area of science [biology] which has been passed over in the rapid extrapolation from physics to the social sciences"; or Cohen and Wartofsky (1976, p. v): "The philosophy of biology should move to the center of the philosophy of science—a place it has not been accorded since the time of Mach"; or Rosenberg (1985, p. 13): "In the last few decades, many philosophers have turned their attention to biology to assess the adequacy of a philosophy of science that has been drawn from an almost exclusive examination and reconstruction of physics." Nicholson and Gawne (2014) go even further by providing numerous self-serving quotations from Hull and, especially, Ruse, spanning decades, all designed to anoint themselves as the founders of the philosophy of biology. Many of these consist of ad hominem attacks on Woodger.

[4] In fact, much of modern molecular biology was created by physicists and chemists with no formal training in biology (Olby 1974; Judson 1979; Sarkar 1989). It would be ironic if the successful practice of biology did not require formal training in biology but the practice of the philosophy of biology does.

detailed criticism of Bertalanffy's defense of teleology in biology. Wolters ignores Frank altogether; figures such as Hull and Ruse seem to be unaware of his existence.

Ludwig von Bertalanffy

Perhaps even more surprisingly, Wolters ignores Bertalanffy except for a few remarks that summarize one paper, viz., Bertalanffy (1930). Ludwig von Bertalanffy (1901–1972), later known as the pioneer of what he called "General Systems Theory," came to Vienna (from Innsbruck) to work in philosophy with Schlick in 1924, the year that the Vienna Circle began its weekly meetings. Bertalanffy finished his dissertation (on Fechner) under Schlick in 1926 and then turned to biology as a profession (Davidson 1983). In 1928, he published *Kritische Theorie der Formbildung* (Bertalanffy 1928), a book that was translated into English by Woodger in 1933. From 1929 to 1934 Bertalanffy was part of Carnap's "*Studiengruppe für wissenschaftliche Zussamenarbeit*," a group promoting interdisciplinary work in the analysis of science (Hofer 2002). The group included another biologist, Wilhelm Marinelli (Stadler 2001).

Both Bertalanffy and Marinelli gave lectures to the Ernst Mach Society (which was dominated by members of the Vienna Circle) in 1930 (Stadler 2001). This should suffice to dispel claims that the early logical empiricists were not interested in biology or not in active contact with professional biologists. However, Bertalanffy seems not to have attended the weekly meetings of the Vienna Circle and, later in life, he rejected logical empiricism explicitly and claimed never to have endorsed it (Davidson 1983).

Bertalanffy's writings from the 1920s suggest a more complicated story. His 1928 book, *Kritische Theorie der Formbildung* (Bertalanffy 1928), cites Schlick (1925) three times but only in passing. However, when he published the first volume of his *Theoretische Biologie* in 1932 (Bertalanffy 1932), logical empiricist themes move to the foreground of the theoretical discussion in the first chapter. Carnap (1923) is quoted extensively along with Schlick (1925). When *Kritische Theorie der Formbildung* was translated into English and "adapted" by Woodger in 1933, some of these passages were included. In particular, Bertalanffy (and Woodger) summarize and explicitly endorse a passage from Carnap (1923), quoting him as saying:

> "In opposition to a widespread view it is without significance for physics whether we call the content of the first realm (sense–data), e.g. the perceived colour blue, mere phenomena, and that of the second, e.g. the corresponding electromagnetic vibrations, 'reality' in the realistic sense, or whether, on the other hand, in the positivistic sense, we call the first the 'really given', and the second as only consisting of conceptual complexes of those sense–data. On that account physics does not say: 'where this blue appears there is, in reality, such and such an electronic process to make calculation possible', but physics expresses itself quite neutrally with the help of purely formal co-ordinating relations, and leaves the question of further interpretation to a non-physical investigation." (Carnap, 1923)[5] (Bertalanffy and Woodger 1933, p. 20)

Bertalanffy goes on: "In any case the theoretical constructions must be so constituted that they are, in Schlick's phrase, 'unequivocally co-ordinated' with the perceptual world. If that is achieved, the fulfilment of the principal task of science—the exact prediction of future events—is possible with the help of natural laws. ...Scientific law does not consist, as is often said (Dubois-Reymond, Sigwart, Roux, and others), in insight into the 'causal necessity' of events" (Bertalanffy and Woodger 1933, p. 20). These claims would have made almost any contemporary logical empiricist happy. Recall that this is a period before Carnap and many other logical empiricists took the "linguistic" turn (Sarkar 2013). Bertalanffy's views do not depart from the core of the logical empiricism of that earlier period. Moreover, given that the quoted remarks are taken from a book devoted to biological development, Bertalanffy obviously found logical empiricist themes highly pertinent for biology.

Ultimately, the importance of Bertalanffy's work will depend on the question whether his "General Systems Theory" lives up to his proselytization. Its future does not seem particularly promising at present: if the claim that there was a robust and important logical empiricist tradition in the philosophy of biology is founded solely on Frank and Bertalanffy, it would not have a very strong basis. Luckily, starting in the 1920s, we have the work of Woodger and, starting in the 1930s, also that of Nagel, besides an important foundational work by Mainx.

Joseph Henry Woodger

Two factors brought Woodger and Bertalanffy together: a shared passion for biology and a dissatisfaction with mechanism—there will be a discussion of what "mechanism" means in the third section. Joseph Henry Woodger (1894–1981) was educated at University College, London (UCL) where he received an honors degree in zoology in 1914.[6] After serving during World War I, he returned to UCL where he continued embryological research until 1922 when he assumed the new Readership in biology at

[5] This is a quite liberal translation by Woodger but sufficiently faithful to the content of the original not to be corrected here.

[6] Biographical details on Woodger are from Floyd and Harris (1964).

the Middlesex Hospital Medical School (now the UCL Medical School). In 1924 he published a 500-page textbook, *Elementary Morphology and Physiology for Medical Students* (Woodger 1924) which included what may be the first use of "theoretical biology" in English (Nicholson and Gawne 2014). The organismic point of view—in contrast to a mechanistic one—dominates the theoretical discussions of this book.[7]

In 1926, Woodger obtained a semester's leave to learn experimental techniques from Przibram in Vienna. However, difficulties with the planned experimental system (an annelid model) led him to abandon this project shortly after arrival. Instead, Woodger began his lifelong obsession with the philosophical foundations of biology. Woodger seems to have met Bertalanffy in Vienna but there is no evidence of any direct contact with the Vienna Circle. There is no compelling evidence either of any influence from the logical empiricists in Woodger's work in the 1920s though, after returning to London from Vienna, he was preoccupied with writing his first philosophical book, *Biological Principles*, published in 1929, which systematically attempted to critique mechanism (Woodger 1929). However, during this period he appears to have begun to study modern analytic philosophy carefully (Floyd and Harris 1964).

By the time he had translated and adapted Bertalanffy's *Modern Theories of Development* in 1933, Woodger's work had taken a turn in which logical empiricist themes began to play a central role. It is important to distinguish between whether Woodger identified himself as a logical empiricist and whether his work should be regarded as being part of the logical empiricist corpus. In answer to the first question, there appears to be no evidence that Woodger ever identified himself as a logical empiricist. However, he participated in various logical empiricist projects. He was elected to the committee organizing the International Congresses for the Unity of Science at the First Congress for the Unity of Science in Paris in 1935 (although the speakers on biology were Frank and du Noüy) (Stadler 2001, p. 367) and participated extensively in the *International Encyclopedia for Unified Science*, starting as early as 1938 when he contributed a piece,"Unity through formalization" (Woodger 1938), to Volume 6 of the series *Einheitswissenschaft/Unified Science/Science Unitaire* edited by Neurath (1938). In the *Encyclopedia* itself, he contributed the volume, *Technique of Theory Construction* (Woodger 1939); the volume on biology was written by Mainx (1955) and translated from German to English by Woodger. Moreover, Woodger was constantly in touch with Carnap whose help was important while he wrote *Axiomatic Method of Biology* (Woodger 1937). Nevertheless, while Woodger would probably have aligned himself with "scientific philosophy," his chief philosophical interlocutor (presumably partly because he lived in the United Kingdom) was Popper. Through Popper he came to be acquainted with Tarski whose work he also translated into English (from German).

When it comes to Woodger's work, it would be idiosyncratic not to put it within the logical empiricist corpus. Indeed Wolters (1999) does put it there though Nicholson and Gawne (2014) hedge their bets. Critics such as Hull (1994) and Ruse (1984) also put it there though, given that they show minimal familiarity with the contents of that work, their views can be disregarded. Details of Woodger's work, starting with *Axiomatic Method of Biology* (Woodger 1937), will be taken up in the fourth section. Suffice it here to note those points that should determine an answer to the question whether it belongs within logical empiricism. In one important aspect it clearly does not: Woodger never eschewed metaphysics and emphasized Whitehead's influence on him throughout his career (although he was also prone to occasional anti-metaphysical exhortations). Beyond that, though, starting in the mid-1930s: (1) he was clearly an empiricist; (2) few have exceeded him in the deification of formal logic; (3) he was an ardent advocate of the unity of science though, unlike most logical empiricists (see below), he viewed axiomatization rather than the adoption of a physical language as the path to unification; and (4) by the late 1940s (see, e.g., Woodger (1952)) he, too, had taken the linguistic turn. Finally, Woodger's work was regarded as falling within the logical empiricist tradition at that time: J. B. S. Haldane (1938a) titled his review of Woodger's (1937) *Axiomatic Method in Biology* "biological positivism"; there appears to be no record that Woodger complained.

Ernest Nagel

There can be no doubt about Ernest Nagel's (1901–1985) position within logical empiricism; what is odd is that he finds no mention in Wolters's diatribe. Nagel was born in what is now Slovakia but emigrated to the United States at the age of ten; his entire professional career from his undergraduate days till his retirement was spent at Columbia University in the City of New York.[8] Nagel attended meetings of the Vienna Circle in the mid-1930s and reported on its work at length to a U.S. audience in the *Journal of Philosophy* in 1936 (Nagel 1936a, b). He attended the Preliminary Conference of the First International Congress for the Unity of Science in Prague (August 31–September 2, 1934) followed by the Eighth International Congress of Philosophy (September 2–7); at the latter he presented a paper, "Reduction and

[7] For a critical discussion of this book, as well as Woodger (1929), see Nicholoson and Gawne (2014).

[8] Biographical information on Nagel is from Suppes (1994).

Autonomy in the Sciences," that will be discussed below. Though Nagel's philosophical background was in American pragmatism (and at the 1939 Fifth International Congress for the Unity of Science, he spoke on Peirce (Stadler 2001, p. 387)), he became associated with logical empiricism because, like Morris (1938), he found the areas of agreement between pragmatism and logical empiricism far more extensive than potential areas of disagreement. He was part of the editorial core and also a prominent contributor to the *Encyclopedia of Unified Science*, contributing the volume, *Principles of the Theory of Probability* (Nagel 1939).

After World War II, Nagel emerged as one of the most visible proponents of logical empiricism—and one of those who still paid attention to recent developments in the special sciences (from which, Carnap, for instance, had become increasingly divorced). For a generation of students, his *Structure of Science* (Nagel 1961) was the introduction to logical empiricist philosophy of science. Nagel's chief contribution to the philosophy of biology during this period came from his interest in questions of reduction and emergence. I will turn to that work in detail in the fourth section. Nagel was not formally trained in biology but his work indicates that he read it thoroughly[9]; at Columbia University, his interest in biology led to regular discussion meetings with the biologists there (Schaffner, personal communication, 2015).

However, Nagel's legacy in the field is not entirely positive. By the early 1940s, Nagel had succumbed into an uncritical anti-Marxism, probably under the influence of Sidney Hook who emerged as one of the most prominent and visible Cold Warriors in the 1950s (Reisch 2005). Neurath had proposed Lancelot Hogben as the potential author of the philosophy of biology entry for the *Encyclopedia of Unified Science*. Even though Hogben was one of the most prominent biologists of his generation (Sarkar 1996), Nagel rejected him because he was a Marxist. Writing to Charles Morris, one of the editors of the Encyclopedia, Nagel observed:

> Why has Hogben been selected to do the pamphlet on biology? I admit the Encyclopedia could have made a worse selection—but the real point at issue is whether it couldn't have been a much better one. For my part, I don't enjoy the prospect of having the foundations of biology class-angled.[10]

This quotation is symptomatic of political bias so unfounded on any deep knowledge of either Hogben's scientific or political writings that it gives some credence to Wolters's theses. Hogben's Marxism, though deep, was neither doctrinaire (the contrast here is with his fellow biologist, Haldane (1939) nor visible as "class-angled" in any of his work including such popular classics as *Mathematics for the Million* (Hogben 1937). Nagel thus rejects Hogben on political grounds even though the latter was one of the two most prominent defenders of mechanism, what Nagel rechristened as reductionism (Hogben 1930; see below in the third section).

Felix Mainx

Finally, before turning in detail to the contents of Nagel's and Woodger's work, it is worth taking a good look at Felix Mainx (whose biography is treated at length by Hofer (2013)) because he contributed *Foundations of Biology* (Mainx 1955) to the *Encyclopedia of Unified Science*. Wolters (1999) notes that Mainx's work fully falls within the logical empiricist corpus and also admits that it received some attention from biologists in the US in the 1950s. The critical question is the quality of Mainx's analysis, which Wolters denigrates. What follows will challenge that assessment.

Hofer (2013) summarizes Mainx's career: He was born in Prague in 1900, studied at the Botanical Institute there under Pringsheim and Czurda, and became interested in the evolution of sex determination in algae and protozoa. By 1932 he had established himself in the profession through his genetical work and his attempts to integrate genetics and evolution. (Along with Pringsheim, Mainx was actively involved in a debate about the genetics of sex determination with Hartmann and Moewus, one in which Pringsheim and Mainx ultimately prevailed—the debate was marked by blatant anti-Semitic rhetoric on Moewus's part.[11]) A "positivistic" demand for adequate data in support of theoretical claims was part of Mainx's rhetoric against Hartmann. Mainx was forced to resign from his position at the Botanical Institute because of his anti-Nazi activism. Logistical problems resulting from the outbreak of World War II prevented his emigration to the UK (where he had been offered refuge at the John Innes Horticultural Institution which had already accommodated Pringsheim). In the ensuing years, Mainx is known to have performed acts of sabotage against the Nazis;

[9] See also his archives https://urldefense.com/v3/__http://www.columbia.edu/cu/lweb/eresources/archives/rbml/Nagel/__;!!HoV-yHU!uRaX0aCZp1Dr07zIGJ1C2y9B9UKjc5cvmKxSqdjWA6s8QmAT6moOH3GCIXJVe_dPJuy6J1mu7dhFABB_A6MZWCnndjEb.

[10] Nagel to Morris, November 16, 1944. Quoted from Reisch (2005, p. 206).

[11] See Hofer (2013) for details.

he survived the war to emigrate to Vienna in 1946 and stayed there for the rest of his career.

The question of who should author the entry on the foundations of biology in the *Encyclopedia of Unified Science* was debated by Neurath, Frank, and Carnap in the late 1930s and 1940s, with Frank preferring Mainx and Carnap initially preferring Bertalanffy, with the possibility of Hogben or Woodger as an alternative lurking in the background (Reisch 2005; Hofer 2013). It appears that Mainx was preferred to Bertalanffy because his work was (correctly) viewed as being closer to contemporary biology because of its emphasis on genetics and evolution. Because of the long delays in the publication of the *Encyclopedia* induced by the war, *Foundations of Biology* only appeared in 1955 even though it was largely completed a decade earlier. Since the content of this work has entirely been ignored by recent commentators (including Hofer) some detail is provided below; it should become apparent that it deserves far more attention from philosophers of biology than can be devoted to it here.

Foundations of Biology began from a thoroughly logical empiricist perspective. While biology is a special science, it is so because of the "observed peculiarity of its object and ...the development of its own methods of research and points of view," and not because of metaphysical (including ontological) commitments, which are denigrated along with all efforts to build a "system" of biology (Mainx 1955, p. 2). Mainx endorsed a criterion of demarcation (testability, interpreted as either verifiability or falsifiability) for the empirical statements of biology but, departing from some logical empiricist dogma, also endorsed the view that all descriptive statements are implicitly theory-laden. Ultimately all that mattered was a statement's ability to make predictions.

As hypotheses emerge from descriptive statements (which requires scientific imagination), new hypotheses should ideally be consistent with the "permanent structure of science" (Mainx 1955, p. 6) and be incorporated into it. A remarkable discussion followed:

> If the incorporation does not succeed without contradiction, then, by a thorough logical analysis of the contradictory statements and their elements, we must investigate whether the contradiction is not merely apparent and whether it cannot be removed by a logical change. In other cases the contradiction can be bridged over by means of accessory hypotheses which restrict or extend the validity of the hypothesis and which, in turn, must satisfy the requirements of testability by experience. Naturally, they must not be introduced only *ad hoc*, i.e., they must not be merely tautological or formulated without any connection to the rest of the system of the relevant science, because in that case they would in principle be removed from any testability. On account of their heuristic value they can often give birth to a new development in science. If a far-reaching contradiction persists between old and new hypotheses, this leads to a "crisis" in the empirical sciences concerned. This, in turn, leads to a revision of the system of statements hitherto in use. (1955, p. 6)

Ideas later typically associated with Kuhn (his *Structure of Scientific Revolutions* (1962) was also part of the *Encyclopedia*) were clearly in the air within the logical empiricist ambit.

Turning to the content of biology proper, Mainx distinguished between "elementary" and "complex" points of view, the subdivision being "purely practical" and for convenience of exposition (Mainx 1955, p. 7). These points of view are methodological in the sense that they represent approaches taken to the study of biological systems. Somewhat loosely (and Mainx would not have put it this way), the elementary points of view were consistent with contemporary mechanism; less loosely, the complex points of view were those typically invoked by the emergentists (see the third section).

There were three elementary points of view, depending on whether attention should be directed to visible structure, behavior, or "significance as a member of a reproductive chain":

1. *The Morphological Point of View*: Under this, Mainx discussed the process of classification and noted that it involves theoretical commitments (e.g., claims of homology) that can be tested. Classification was hierarchical; at the highest level was the "construction plan." This is what Woodger had in mind when he introduced "*Bauplan*" as a technical term in Anglophone biology (almost simultaneously to the date when *Foundations of Biology* appears to have been written, *ca*. 1945; see below).
2. *The Physiological Point of View*: Physiology was "the theory of the functions of the organism, of its organs and tissues, or, better, ...the theory of the processes which take place in the organism and between the organism and the environment" (Mainx 1955, p. 15). Mainx's treatment of it was desultory. Only two points are particularly salient: (1) There was an extended treatment of "blanket statements" (*Pauschalaussage*; 1955, p. 17) such as those about the response of an organism to a stimulus which are apparently simple statements with very complex backgrounds and applicable to many situations. (2) He noted the special problems for physiology posed by the human "inner experience" (1955, p. 19) but left it for the psychologists to address.
3. *The Genetical Point of View*: Mainx identified this with the use of population genetics, especially Mendel's

"rules,"[12] to explain and predict the transmission of traits. Even failures of these rules (and Mainx has linkage in mind because it violates Mendel's second rule (of independent assortment)) were indicative of the value of those rules: "the heuristic value of the Mendelian hypothesis has proved itself precisely in those cases which seemed to falsify the hypothesis. The setting-up of testable auxiliary hypotheses has empirically disclosed new connections and ...has led to a significant increase of knowledge" (Mainx 1955, p. 22).

There were five complex points of view, that is, ways in which "statements about complex states of affairs are formulated in biology" (Mainx 1955, p. 24). The subdivision was supposed to be "quite arbitrary and in no way exhaustive" (1955, p. 24):

1. *The organism as an open system*: Mainx emphasized that, though non-living systems could be open, they could often be analyzed as closed systems; in contrast, this option was not available for living systems which must always be treated as open. Consequently every biological claim must implicitly include an organism's environment to some extent. Mainx listed some characteristics of organisms as living systems: self-regulation, dynamic equilibrium, and so on. The discussion included probably the first philosophical account of the use of models in biology (models can be "experimental arrangements or theoretical constructions").
2. *Growth, development, reproduction: the historical character of the organism*: Mainx observed that growth and development were irreversible but highly ordered processes in which past states, in some sense, determined the present one which in turn similarly determined future ones. He emphasized that the sense of this determination remained to be fully explicated because of the complexity of the developmental process, understanding which would require a synthesis of all points of view. However, he also noted the particularly useful role genetic analysis can play by explicating the causal factors of development.
3. *Organic diversity and its structure*: Here, the focus was on taxonomy and Mainx observed that modern techniques of classification, by relying on a multiplicity of carefully selected traits, had produced a robust system that covered all organisms with considerable success, and that this was true even when taxonomy was considered distinctly from evolution (as, indeed, it was before the theory of evolution emerged in the late 19th century). Much of Mainx's discussion centered on difficulties in formulating an exact concept of species. Mainx noted how standard attempts to define the concept led to problems.
4. *The population as the natural form of existence of living beings*: Here, Mainx forcefully articulated a position that philosophers of biology typically associate with Mayr (e.g., Mayr 1988). He observed that species exist as populations, "the real form of existence of the species in question at a particular moment of time" (Mainx 1955, p. 40). The genetics of populations views the entire population as a "genetical system" (1955, p. 41) on which the environment acts through selection. The complexity of analyzing this process, how it depended on the mode and pattern of reproduction, was taken into account. Finally, because populations are collectives, Mainx argued that claims about them must be statistical in nature and mathematics became a necessary tool for the analysis of population genetics.
5. *The history of organisms*: Finally, Mainx turned to evolution and observed: "No subdivision of biology is to such a degree choked up with unrestricted theorizing or fogged by fanciful speculation or made the battleground of extrascientific differences of opinion as this" (Mainx 1955, p. 43). The emphasis was on the empirical basis of evolution. Three types of problems were distinguished: (1) claims about the past processes of evolution which can be directly tested by paleontology; (2) claims about phylogeny; and (3) claims about the mechanisms of evolution in the past and present. The latter two types of claim could be indirectly tested using (a) paleontological research; (b) biogeographic research; (c) the comparative method applied to anatomy and embryology; and (d) population genetics. Each of these was then briefly described. The discussion of adaptation was sophisticated, emphasizing the importance of precise environmental conditions and changes in the environment that must be known to understand the genesis of complex adaptations (such as the eye) (1955, pp. 48–49). Mainx also differentiated between two claims: that of the complications of "higher" organisms compared to protists and that of the greater adaptation of the former compared to the latter. Insightfully, he took the second claim as unproven: "The biological survival value of a species in its proper environment is the only scientifically usable measuring rod" (1955, p. 49). It was impossible to say definitively that "higher" organisms are better adapted than protists by this criterion. Mainx noted that this observation brought into question the very idea of evolutionary progress.

[12] It is striking that Mainx used "rules" rather than "laws" in future concordance with many post–1970 debates in the philosophy of biology—see, for example, Sarkar (1998).

The second major part of the book turned to the significance of speculation in biology. Mainx did not deny the value of the heuristic role that speculation can play in generating new testable hypotheses. But most of his effort was spent on an elaborate dismissal of what the Vienna Circle would have called pseudoproblems in the philosophy of biology. Mainx does not use the term "pseudoproblem"; rather, he classified the problematic views as "parabiology." Starting with the dispute between vitalism and mechanism, he argued that both make either tautologous claims or ones that are too ambiguous to be testable. The case against vitalism was easy (and emergentism, organicism, etc., are later subsumed under vitalism). Against mechanism, he proclaimed (perhaps somewhat oddly): "When the mechanist ascribes properties to his 'biomolecules,' when he assumes structures in the fertilized egg from which all vital and developmental processes necessarily follow, this amounts to no more than a pure tautology, in which what is to be explained is already put into the definition of the concepts serving for explanation" (Mainx 1955, pp. 60–61). What Mainx surprisingly missed was the fertile heuristic role this assumption was playing at the time—this is exactly the kind of "speculation" that he had earlier endorsed. It appears that Mainx had not been following developments in biophysics that would shortly become a core component of the emerging molecular biology of the 1950s. This is perhaps the greatest weakness of Mainx's analysis—but that is probably obvious only in retrospect with the advantage of a half century of hindsight.

What is clear from the discussion in the book is Mainx's strong commitment to a systems view of biology. He continually referred to organisms as open systems and also noted:

> No biologist will assert that the results of the physics and chemistry of inorganic nature will alone suffice to elucidate scientifically the relations of living things. ...Without the knowledge of living things we should never have had examples of such structures, and an essential part of what is given in nature would have remained unknown to us. ...Nevertheless, no man [sic] doubts that even within organisms the same general physicochemical laws hold as in inorganic nature. (Mainx 1955, p. 64)

Arguably, this is a form of emergentism in disguise.

Issues connected with teleology, holism, subjectivity, and so on, were similarly dismissed as "parabiological." Following logical empiricist doctrine, ontological claims in general were treated as having no empirical content. Teleology, in the sense of the ascription of functions, was acknowledged as having "high heuristic value" (Mainx 1955, p. 68) in generating new research. (Functions were defined using processes contributing to natural selection.) Holism was dismissed because of the vagueness of terms such as "whole" and "sum"—but Mainx's discussion did not reach the level of sophistication that Nagel had achieved (see below) of which Mainx seems to have been unaware. Mainx rejected a project of "theoretical biology" as being dangerously removed from the empirical work that was the core of contemporary biology. Mathematical population genetics was exempt from this criticism because, according to Mainx, it remained sufficiently close to empirical work. The discussion ended with Mainx celebrating his Catholicism but noting: "From the statements of empirical science not a single decision in matters concerning a philosophy of life or valuation can be reached. Biology as an empirical science can therefore never give an answer to those 'great questions of life' which move men [sic] from within" (1955, p. 83). The invocation of the "is-ought" fallacy (Mainx does not use this terminology) was intended as an argument against all attempts to frame ideologies (with normative content) from an alleged biological basis which, according to Mainx, had been attempted not only in Hitler's Germany but also in Stalin's Soviet Union (in the latter case, as demonstrated by the ongoing Lysenko affair).

This precis should indicate that Mainx's book did what it was supposed to do: lay out the philosophical foundations of biology comprehensively in a manner concordant with the basis tenets of logical empiricism. The book merits more attention from philosophers of biology today than it has been accorded: in particular, Mainx's treatment of evolution, as noted earlier, was quite sophisticated though the discussion of mechanism did not reach Nagel's level of sophistication (see below).

Where does this leave us? With the sole exception of Nagel, all the figures treated in this section had formal training in biology; three of them, Bertalanffy, Woodger, and Mainx, were professional biologists for at least part of their career. There is thus little to complain about the background in biology of those writing on that topic within the logical empiricist corpus. It is, of course, still possible that the work they produced had little merit. The arguments of the last few pages have already provided a defense of Mainx; it is time to turn to the others, especially Woodger and Nagel.

The Wrong Framework?

Wolters claims that the logical empiricists relied on a physicalist framework and that this led to their being reductionists (1999, p. 193). Noting (correctly) that physicalism was supposed to hold the key to the unity of science, he goes on to claim that the logical empiricists aimed to achieve this unity through the reduction of all the sciences to physics (1999, p. 193).

On two points Wolters is correct: (1) by the 1930s most logical empiricists, following Neurath and Carnap, had endorsed physicalism; and (2) this was supposed to lead to the unity of science (Carnap 1934). But Wolters's account of what physicalism meant for the logical empiricists is entirely inaccurate. He claims: "physicalism in the first place meant reduction of any scientific talk to talk about the given, be it in a phenomenalist language, as Carnap had proposed in the *Aufbau*, or in a 'thing–language,' i.e. a language characterized by spatio-temporal reference, as was first advocated by Neurath" (Wolters 1999, pp. 193–194). He proceeds to claim: "But this general physicalist idea was soon interpreted as a reductionist research program" (1999, p. 194). These claims require careful examination because they form part of the basis for the indictment of logical empiricism:

1. To the best of my knowledge, reduction to a phenomenalist language (what Carnap (1928) in the *Aufbau* calls the autopsychological language), was *never* called physicalism by any of the logical empiricists. (Wolters quotes nobody.)
2. By the 1930s, representing observations in a physicalist language was *not* a reduction to the given: physicalist "protocol sentences" could arguably be subject to revision, a possibility that was carefully debated by Neurath and Carnap (Uebel 2007).
3. The physicalism of the 1930s required observation reports to be formulated in a physical language. But this does not, in any sense, constitute a reduction to physics. As we shall see below, starting in the 1930s, Nagel had begun to formulate what became the standard logical empiricist model of reduction which required *explanation* beyond representation in the language of the reducing theory. Carnap's and Neurath's physicalism *never* required explanation. Wolters claims that the putative reductionism is a matter of interpretation—it seems more to be a matter of a very fertile imagination.[13]
4. By the late 1930s Carnap (1939) had realized that a "thing language" must be distinguished from the language of fundamental physics for the obvious reason that, given quantum mechanics, the theoretical entities of fundamental physics were hardly similar to the ordinary things of everyday life (see, e.g., Sarkar 2013). Physicalism became the thesis that observation reports must be formulated in a "thing language" with terms referring to macroscopic objects such as microscopes or measuring apparatus. Physicalism was no longer a thesis about the language of physics.
5. The main reason for adoption of a physicalist language was to ensure that observations, which provide the foundation for knowledge (according to empiricism), were reported in a public intersubjective language. So, it should come as no surprise that Carnap's (1963) final characterization of physicalism only required intersubjectivity of the language of observation reports.

Thus, the program for the unity of science through physicalism had nothing to do with reductionism as it has been understood since Nagel's seminal analysis starting in the 1940s, which will be fully discussed below. In fact, the unity of science through reductionism seems to have been explicitly defended only once in the logical empiricist corpus—and that was in 1958. That defense by Oppenheim and Putnam (1958) is problematic insofar as it relied on a nonstandard model of reduction; nevertheless it also showed full cognizance of recent developments in biology. I will return to it in the fifth section.

Ironically, what Wolters misses is that the issue of reductionism provides exactly the correct framework to discuss some of the most important developments in biology in the 1920s and 1930s. It was relevant to biochemistry, physiology, genetics, cytology, developmental biology (what was then still called embryology), and (obviously) to the emerging discipline of biophysics—and the biologists viewed it as such.[14]

The discussion here will be short and geared only to demonstrate the importance of reductionism within the biology of the period in which logical empiricism emerged as the dominant project of the philosophy of science.[15] That is all that is required to put the work of Woodger and Nagel in context. What is now called reductionism was called mechanism within biology and in philosophical discussions of biology in the first half of this century. Nagel (1951) followed (Loeb 1912) in interpreting it as only requiring that all living phenomena can be "unequivocally" explained in physicochemical terms. Though there is a long philosophical history of this thesis, it only became experimentally tractable towards the end of the 19th century with developments in organic chemistry, physiology, embryology, and what became the discipline of biochemistry in the early 20th century.

[13] The only notable exception to this claim appears to be Feigl (1963) with which Carnap (1963) explicitly disagreed.

[14] It is perhaps debatable as to how important it was to evolution. If we take the incorporation of classical genetics (in particular, the work emanating from the Morgan school as well as biochemical genetics) into evolutionary biology as being important, as Haldane (1932) and Wright (1934) clearly did, the issue of reductionism becomes important even in that context. If evolution is taken to be largely comprised of population genetics and systematics (before the molecular turn of the 1960s) the question of reductionism is largely irrelevant. But evolution is not the sole area of biology, and evolutionary biology since the 1950s has also had to engage with the material basis for heredity and diversification.

[15] For more detailed histories of the debate between mechanism and its alternatives, see Gilbert and Sarkar (2000) and Allen (2005).

Mechanism of this sort was forcefully defended by many prominent biologists besides Loeb. Before him, it was defended by Roux; in the 1920s and 1930s it was defended at book length by Wilson (1923) and Hogben (1930) but, most importantly, it provided the basis for the research programs on many rapidly growing subdisciplines within biology, including biophysics and biochemistry (Roll-Hansen 1984; Sarkar 1992a). Opponents were legion. Originally these were vitalists of different hues, with Driesch (e.g., 1914) being perhaps the last prominent one; he seemed initially to have believed in some ontological factor differentiating living and non-living matter. But vitalism, especially after the rise of the new biochemistry in the 1920s, was largely a dead issue. Rather, the alternative to mechanism was seen as an endorsement of some version of the view that explaining features of organized wholes such as living organisms required recourse to laws that were not only those that governed the behavior of their parts independent of all reference to the wholes.

In the 1920s and 1930s, versions of this view came to be called emergentism, holism, and organicism (Gilbert and Sarkar 2000; Allen 2005). Book-length defenses came from many, including J. S. Haldane (e.g., 1931, among many other books), Henderson (1917); Needham (1936); Russell (1916), and Spemann (1943), besides, of course, Bertalanffy and Woodger. Finally, dialectical materialism provided yet another alternative to mechanism that also claimed the existence of novel laws of matter at each level of organization—this was applied to biology in many ways, perhaps most notably by J. B. S. Haldane (1939; see also Sarkar 1992b). Even some early molecular biologists such as Delbrück (1949), following the complementarity arguments of Bohr, denied the possibility of mechanistic explanation of all of biology (Sarkar 1989). The prominence of these anti-mechanists was such that Allen (1975) claimed that they (through their biological work) were responsible for the complete transformation of biology in the first half of the 20th century.

This was the context in which Woodger and Nagel turned to the philosophy of biology in the 1930s. An excellent summary of these disputes was provided by Beckner (1959) in a dissertation written under Nagel's supervision at Columbia University; this appears to be the first dissertation on the philosophy of biology written in English.[16] It follows that Woodger and Nagel showed insight, and an appropriate awareness of the contemporary situation in theoretical biology, when they chose to focus on reductionism. As we shall see in the next section Nagel's analysis of the doctrine of emergence showed considerable insight.

The Wrong Questions?

The previous section was intended to show that, by focusing on reduction and emergence, Nagel and Woodger were addressing issues regarded as fundamental in theoretical biology and its philosophy through the early 1950s. But were the specific questions they asked appropriately selected? The next subsection will now argue for the contemporary importance and continued relevance of Nagel's analysis of reduction and emergence (and also note that Woodger independently proposed a closely related model of reduction). The subsection following that will move beyond reduction and emergence and analyze what Woodger achieved through his formal approach to biology.

Reduction and Emergence

Nagel's work on reduction is well-known, having been both intensively criticized in the 1970s and 1980s and recently defended by a variety of authors.[17] It spanned over thirty-five years of Nagel's career. As mentioned earlier, he first presented an analysis of reduction (and autonomy) in 1934 and published part of that analysis (excluding the material on autonomy) in 1935 (Nagel 1935).

The problem, as Nagel saw it in 1935, was the following: "An entity is exhibited as a complex of constituents, whereby some phases of its behavior can be shown to be related in terms of their relation to phases of the behavior of its constituents" (Nagel 1935, p. 47). Note how weak this claim was: all that was required is that the relation in question be exactly specified—there was no requirement that reduction be a form of explanation. Nagel distinguished between five types of reduction: (1) *constitutive* reduction, which requires no more than the existence of the relations mentioned earlier; (2) *characteristic* reduction, which permits a theoretical inference of the properties of the entity from those of its constituents; (3) *complete* reduction, which is even stronger insofar as the inference reaches the joint properties of several entities (at the reduced level); (4) *formal* reduction, when the relations involved in a reduction are formally (that is, mathematically) specified; and (5) *epistemic* reduction, which is a reduction to "sensuous experience," the nature of which was left unspecified. Perhaps the most crucial contribution of this early paper was an explicit recognition that it was pointless to claim one science could be reduced to another

[16] Thanks are due to Ken Schaffner for drawing my attention to Beckner's case. There will be no detailed discussion of Beckner's work because it does not fit well within the logical empiricist canon in spite of Nagel's involvement.

[17] Sarkar (2015) provides a critical review of this burgeoning literature and a partial defense of Nagel's model of reduction.

without referring to a particular time, that is, historical stage of each science's development.[18]

By the time Nagel returned systematically to the problem of reduction some 14 years later, one crucial development had taken place: the concept of a deductive–nomological explanation and its logical structure had been carefully explicated by Carnap (1939) and, especially, Hempel and Oppenheim (1948).[19] Nagel now presented a model of reduction as an explanatory relation between theories and applied it to the question of the reduction of parts of thermodynamics to the kinetic theory of matter. He distinguished between two situations: homogeneous reductions in which the two theories share all relevant concepts (and, therefore, all relevant terms when the theories are formally or "linguistically" formulated) and inhomogeneous reduction in which they do not (that is, the potentially reduced theory introduces new concepts not found in the reducing theory). Nagel's favored example for homogeneous reductions, which he took to be philosophically simpler than inhomogeneous reductions, was the reduction of Galilean mechanics to its Newtonian counterpart. Reduction then consisted simply in the derivation of the reduced theory from the reducing one—this criterion came to be called the "condition of derivability." In *Structure of Science*, Nagel (1961) took these derivations to be logical deductions in which the premises include the laws of the reducing theory and appropriate boundary or initial conditions, and the conclusion consists of the laws of the reduced theory. While there is no historical evidence that indicates that Nagel explicitly had the deductive–nomological model of explanation in mind when formulating his account of reduction (Schaffner 2013), it is clear that his model assumes such a structure with the *explanans* itself being a law (given that theories are sets of laws) rather than an individual fact.[20]

In the case of inhomogeneous reductions, because the reducing and reduced theories have terms that are not in common, appropriate connections must be established between the relevant terms before a deduction can be attempted. In 1949, Nagel (1949) assumed that all terms of the reduced theory must be definable using terms of the reducing theory—this criterion was his "condition of definability." However, this requirement proved to be too strong; by 1951, when he first attempted to analyze the question of reduction of biology to a physicochemical basis, Nagel (1951) replaced it by a "condition of connectability" (as it was called in *Structure of Science*). By 1951 he had realized that these connections could be lawlike synthetic claims; thus, unlike definitions, they could formally be conditionals or biconditionals. Eventually, Nagel espoused a pluralism about the nature of these connections, allowing them to be logical connections, conventions, or factual claims (Nagel 1961).

Nagel's model was finally fully presented in *Structure of Science* in 1961. What is important here is that this account emphasized that satisfaction of the two formal conditions for reduction (connectability and derivability) was not sufficient to make a reduction valuable. Nagel introduced two sets of "nonformal" conditions that distinguished scientifically valuable reductions from those that are not. These nonformal conditions, which have often been ignored by critics (as emphasized by Sarkar (1989) and Waters (1990); see also Sarkar (2015) for a review of recent attention to these nonformal conditions), were crucial to Nagel's analysis of reduction in biology; they will merit sustained attention. The first set of nonformal conditions detailed epistemic virtues of the reducing theory, and of a putative reduction, if that reduction were to have scientific significance; the second set introduced contextual historical constraints that also help adjudicate the value of reductions.

For Nagel (1961), there were five epistemic virtues:

1. Nagel observed: "If the sole requirement for reduction were that the [reduced theory] is logically deducible from arbitrarily chosen premises, the requirement could be satisfied with relatively little difficulty" (1961, p. 358). For that reason, the reducing theory must not consist of any *ad hoc* set of assumptions. At the very least, its "theoretical assumptions... [must] be supported by empirical evidence possessing some degree of probative force" (1961, p. 358). Note, here, the similarity to Mainx's (1955) strictures against *ad hoc* assumptions (see above).
2. The evidence in favor of the reducing theory should be independent of the evidence in favor of the reduced theory (1961, p. 358). This condition was clearly met in the potential reduction of biology to physics and chemistry.
3. Perhaps the most telling substantive condition that Nagel introduced was that a reduction should help the further development of the reduced theory (1961, p. 360). There was thus no question of the reducing theory replacing or eliminating the reduced one—eliminativism is against both the spirit and the letter of Nagel's analysis, a point that he repeatedly emphasized later (Nagel 1970). The potential reduction of biology to physics and chemistry in no way challenged the autonomous methodologies of biological research.

[18] As Nagel (1935, p. 48) observed: "Although chemistry may in some sense be reducible to contemporary physics, it is not the case that it is reducible to the physics of the early nineteenth century."

[19] On Carnap's earlier exposition of this model, which is often not recognized (unlike the case of Hempel and Oppenheim), see Sarkar (2013).

[20] Carnap (1939) and Hempel and Oppenheim (1948) had noted this case but had not connected it to the problem of reduction.

4. Good reductions should lead to new predictions (1961, p. 361). The new predictions that Nagel had in mind could include the formulation of what he called "intimate and frequently surprising relations of dependence," not only between the reduced and reducing theories but also, potentially, between the laws of the reduced theory.
5. If reduction led to some degree of new unification between the reduced and reducing theory, that was an added virtue (1961, p. 360). However, unification was neither the goal nor a necessary (or, for that matter, sufficient) condition for a scientifically significant reduction.

In general, Nagel's discussion suggests that the last two virtues in this list are not as important as the first three; however, there was no explicit statement to that effect.

The temporal context of a reduction was important because theories were not static entities but changed over time (Nagel 1961, pp. 361–363). Nagel emphasized that theories "must be at appropriately mature levels of development if the reduction is to be of scientific importance" (1961, p. 363).

1. In particular, whether or not there is a reduction between theories is itself temporally indexed, that is, it is illegitimate to ask whether one theory is reducible to another without specifying at what time stage of development each of them is being considered (1961, pp. 361–362).[21] Nagel repeatedly emphasized this point in his discussion of the potential reduction of biology to physics and chemistry.
2. The time at which a reduction is attempted should be appropriate (1961, p. 362). If the theory to be reduced is at a stage when much of its fruitful developments is exploratory, that is, consists of attempts to expand its domain, its reduction may not only be scientifically irrelevant but may harm its progress. As Nagel put it: "a discipline may be at a stage of active growth in which the imperative task is to survey and classify the extensive and diversified material of its domain" (1961, p. 362). In either case: "Attempts to reduce the discipline to another (perhaps theoretically more advanced) science, even if successful, may then divert needed energies from what are the crucial problems at this stage of the discipline's expansion" (1961, p. 362).

Subsequently, Nagel only altered his analysis of reduction in two ways (Nagel 1970): (1) he allowed the reducing theory to correct the reduced one[22]; and (2) he acknowledged that the derivations of the latter from the former would often involve approximations and other subtleties.

The subsequent history and current status of Nagel's analysis of reduction, which was just summarized, has recently been reviewed in detail by Schaffner (2013) and Sarkar (2015)—see also the fifth section. It will suffice here to restrict attention to Nagel's remarks about biology. There are two relevant papers: the one mentioned earlier from 1951, "Mechanistic Explanation and Organismic Biology" (Nagel 1951), and one from a year later, "Wholes, Sums, and Organic Unities" (Nagel 1952). These discussions were reproduced with no substantive changes in *Structure of Science*, even though biology had been transformed by molecular techniques in the intervening debate—this issue will be discussed below.

In the first of these two papers, Nagel took it for granted that mechanistic explanations are reductionist explanations par excellence. He clearly distinguished two positions—whether there was any *in principle* reason to argue for the irreducibility of biology to a physicochemical basis and whether, at present, biology had been so reduced. In both papers, drawing on many examples from physics and chemistry, he argued that there was no such *in principle* reason. Many claims about specific heats of solids could not be derived from 19th–century physics but could be derived from quantum mechanics in the 20th century. Nothing precluded the possibility that the same development may happen for any set of biological claims. Further, Nagel emphasized that the hierarchical organization of biological systems was not a necessarily insurmountable obstacle for such a reduction given that many non-living systems were also hierarchically organized without challenging the potential reduction of claims about them to physics and chemistry. This observation was supposed to answer claims of "emergence" based on hierarchical organization; and Nagel concluded that "none of the arguments advanced by organismic biologists establish the inherent impossibility of physicochemical explanations of vital processes" (1951, p. 337).

At the same time Nagel took it to be trivially true that the physicochemical characterization of almost any part of biology was too crude at that time for any significant reduction to take place. He included genetics in this category (and apparently did not change his mind over the subsequent decade—see below). As he put it: "organismic biologists are on firm ground if what they maintain is that all biological phenomena are not explicable thus far

[21] The same point was later emphasized by Hempel (1969) within the logical empiricist canon.

[22] Though Schaffner (1967a) is not cited, this point was emphasized by him. Given that Schaffner's work formed part of a dissertation written under Nagel's supervision at Columbia University, the former should receive at least some credit for this refinement of Nagel's analysis.

physicochemically, and that no physicochemical theory can possibly explain such phenomena until the descriptive and theoretical terms of biology meet the condition of definability" (1951, p. 330); *ipso facto* there was no question of the satisfaction of the condition of derivability. Throughout, and I take this to be a virtue of Nagel's analysis, he was liberal in his construal of what constituted a biological "law"—any significant generalization would suffice with significance indicated by the scientific context. What deserves emphasis is the flexibility of Nagel's approach.

The second paper was a careful analysis of claims of emergence, particularly of whether sense could be made of the emergenticists' claim that "the whole was greater than the sum of the parts." Much of it was spent on spelling out the ambiguity of the terms, "whole" and "sum," and how critical their construal was to determine the status of the emergenticists' claim. Nagel distinguished eight senses of "whole" and four senses of "sum." If the term "sum" is used to refer to "organized systems of dynamically interrelated parts" (Nagel 1952, p. 25) as, indeed, organismic biologists seemed to use that term, then he saw no *in principle* reason why the behavior of such "wholes" could not be explained using properties of their parts.

Finally, he pointed out that the same "whole" can be decomposed into parts in more than one way: in effect, identifying parts of a whole constituted making a theoretical claim. It was the question whether "a system can be *analyzed in terms of a theory* concerning its assumed constituents and their interrelations" (Nagel 1952, p. 28; emphasis in the original). Moreover, "the distinction between wholes which are sums of their parts and those which are not is *relative to some assumed theory* ...in terms of which the analysis of a system is undertaken" (1952 p. 26; emphasis in the original). If these issues are taken into account, organized wholes such as organisms must be analyzed differently than unorganized ones such as a container of gas. Once that was done, Nagel saw no *in principle* problem with showing that an organic "whole" may be a "sum" of its parts; but, in biology any such claim was unproven at that time and could only be proved by future empirical work. Given what was said in the third section, above, the centrality of these discussions to ongoing debates on emergence should be obvious.

In passing, it should be recorded that Woodger (1952, pp. 271–272) also independently provided a somewhat similar account of reduction. The problem was set out in the same way as that of Nagel's "inhomogeneous" reduction. However, Woodger required each new term in the reduced theory to be connected to terms in the reducing theory by a biconditional; moreover, he required these biconditionals to be theorems of the reducing theory. This was a step backwards from Nagel (1951) and will merit no further attention here.

What Formalization Achieved

Within the philosophy of biology, Woodger is primarily remembered—and often criticized—for his attempts at the axiomatic formalization of biology. In the 1930s Woodger was part of a "biotheoretical gathering" that was influenced by Whitehead and tried to establish a broadly autonomous discipline of theoretical biology (Abir-Am 1987). Woodger's turn to formalization was as much a result of the influence of Whitehead and Russell's *Principia Mathematica* as it was of the logical empiricists (who were equally influenced by *Principia*). In contrast to most logical empiricists (including Carnap and Neurath), who sought the unity of science through physicalism, Woodger explicitly sought this unity through formalization (Woodger 1938). Though he was aware of more recent developments in logic, he initially adopted the notation of *Principia*.

The formalism for axiomatizing biology was systematically developed in *Axiomatic Method in Biology* (Woodger 1937). His purpose was to "provide an exact and perfectly controllable *language* by means of which biological knowledge may be *ordered*" (1937, p. vii; emphasis in the original). Carnap and Tarski are thanked for help and, indeed, the focus on an "ideal scientific language" (Woodger 1937, p. viii) had a strongly Carnapian flavor. The system had ten undefined symbols: **P** (the relation "part of" interpreted both spatially and temporally), **T** (the relation "before in time"), **org** ("organized unity"), **U** (the relation between members of **org** denoting how one entity is produced from another), **cell** (the class of cells), **m** (the class of male gametes), **f** (the class of female gametes), **wh** (the class of whole organisms over some extent of time), **Env** (the relation "environment of"), and **genet** (the class of genetic properties). A complex set of axioms and theorems follow, first about the (spatial and temporal) hierarchical structure of biological systems, next for cell division and fusion, then for division and fusion involving gametes, for genetics, and for embryology. In an appendix Tarski presented a simplified axiom system by reinterpreting **T** as "before in time or at the same time." Overall it was an ambitious attempt. But it is not surprising that the use of the ponderous notation of *Principia* prevented it from being appreciated by a wide audience.

Part of Woodger's axiom system was developed by Carnap in his *Einführung in die symbolische Logik* in 1954 but using modern notation (Carnap 1954), and (in the interest of intelligibility) the discussion here will use Carnap's version. (Carnap also introduced a new set of axioms for kinship relations; for brevity, this part of Carnap's work will be ignored here). What is striking from Woodger's axioms is how complex part–whole relations and temporal identity can be. Consider just one axiom: transitivity. Using P (an undefined term) to express "part of," that is, Pxy means x is a part of y, this axiom can be stated as:

$$Pxy.Pyx \supset Pxz \tag{1}$$

Now, define "sum," that is, x is the sum of the class F, written as $Su(x,F)$, as elements of F are parts of x and for each part y of x there is an element z such that y and z have at least one part in common:

$$Su(x, F) \equiv (u)(Fu \supset Pux).(y)[Pyz \supset (\exists z)(\exists w)(Fz.Pwy.Pwz)]$$

Now introduce an axiom that states that every non-empty class has a unique sum:

$$(\exists u)(Fu) \supset (\exists x)(y)(Su(y, F) \equiv (y = x)) \tag{2}$$

From these two axioms, it follows as a theorem that P must be reflexive, that is,

$$Pxx$$

This means, contrary to what may have been expected, that every entity is a part of itself. It also follows as a theorem that:

$$Pxy.Pyx \supset (x = y)$$

This means that if two entities are parts of each other, they must be identical.

Mereology in the sense of the parts–whole relation has subtleties—and Woodger could have emphasized them more than he did (and, here, even Carnap's discussion is cursory). Temporal identity turns out to be equally complicated. There are many more complex results including explicit definitions of hierarchies introduced by different relations. For instance, let Tr be the undefined symbol interpreted as "being (temporally) earlier than" in Carnap's notation (corresponding to **T** in Woodger's version). With Woodger's axioms, it can be shown that $Tr(x, y) \supset \sim Pxy$; moreover, $Tr(x, y).Pzy \supset Tr(x, z)$. These theorems are not entirely unexpected in retrospect but surprising in the context of the axioms that were explicitly introduced.

The same theory was presented in part (with no essential change) in *Technique of Theory Construction* (Woodger 1939), which was Woodger's contribution to the *Encyclopedia of Unified Science*. By this time, he had abandoned the already archaic notation of *Principia* and the new presentation used minimal symbolism. The later *Biology and Language* (Woodger 1952) (comprised of the 1949–50 Tanner Lectures) presents a notationally much-simplified system than that in *Axiomatic Method in Biology*; the new system was not fully formalized and makes extensive use of set theory. The book included an interesting axiomatization of Harvey's theory of the motion of the heart (Woodger 1952, pp. 45–49, 75–92) designed to show the power of the axiomatic method. The core of the book consisted of an axiomatic treatment of parts of genetics. First, Mendel's rules were aximomatized for a one–locus n–allele system (1952, pp. 112–115). Next, multiple loci and linkage were introduced (1952, pp. 115–124) though only the two–locus case got explicit attention. Throughout, claiming to follow J. B. S. Haldane, Woodger defined genotypes as classes of organisms that cannot be further decomposed using breeding experiments (1952, p. 99). In an interesting move, development was contextualized to the environment in explicit axioms. There was a cursory treatment of evolution. A final section looked at neurology.

Biology and Language was less ambitious than *Axiomatic Method in Biology* in two ways: (1) full formalization was eschewed in favor of a more didactic approach which eased the problem of understanding Woodger's aims, at least for biologists; (2) the biological concerns were more limited. Nicholson and Gawne (2014) have documented that *Biology and Language* got a somewhat better reception than the earlier book. Nevertheless, there does not appear to be any particularly novel insight comparable, for instance, to the complexity of mereological relations exposed by the axiomatic treatment in *Axiomatic Method in Biology*. Even the axiomatic treatment of genetics was remarkably incomplete. In retrospect, the highlight of the book was the analysis of Harvey's theory—and that merits attention even today.

In fact, the semiformal method of *Biology and Language* had already been deployed much more fruitfully in a slightly earlier paper, "On Biological Transformations" (Woodger 1945), which was Woodger's contribution to a Festschrift for D'Arcy Thompson. This paper is well–known because it introduced the term "*Bauplan*" to Anglophone biology. Here Woodger first sketches an explicit relational definition of morpohological structure similarity:

> S is a morphological maximum identity correspondence between a set γ of parts of some life[23] and a set δ of another life, with respect to a set κ of morphological relational properties, if S is a one–to–one pairing of the members of γ with those of δ; if S brings a maximum number of parts into correspondence so that if x is any member of γ paired by S with a member y of δ, then there will be parts x' and y' also paired by S and belonging to γ and δ respectively and such that either x is a part of x' and y of y', or x' is a part of x and y' of y; and finally if, whenever any member of γ has any property belonging to κ, then the member of δ paired by S with it also has that property. (1945, p. 104)

A series of definitions followed (1945, pp. 104–105):

1. A part x is in *morphological correspondence* with a part y if and only if $x \in \gamma$ and $y \in \delta$ and x is paired with y by

[23] By "life" Woodger meant "a single organism throughout its whole temporal extent" (1945, p. 96).

some S which, with γ and δ satisfies the conditions given above with respect to a set κ of properties.
2. A set γ of parts is *isomorphic* with a set δ if and only if there is a pairing S that satisfies the conditions given above with respect to some κ.
3. Given a set of isomorphic lives, a complete set of sets of isomorphic parts is a *Bauplan*.
4. A life u *exhibits* a *Bauplan* λ if and only if u possesses a set of parts that is a member of λ.
5. A *Bauplan* λ *determines* α if and only if every life that exhibits λ is a member of α.
6. A *taxonomic group* is any set of lives determined by a *Bauplan*.
7. A *Bauplan* λ overlaps a *Bauplan* μ if and only if, $\forall\gamma \in \lambda$, and $\forall\delta \in \mu$, there is a $\theta \subset \gamma$, $\theta \neq \gamma$, that is isomorphic with δ such that all members of γ that are not members of θ are members of parts of θ.

Woodger noted that "the word [*Bauplan*] is used in preference to 'structural plan' because of its brevity, but also and chiefly because a *technical* term in needed having just the significance given to '*Bauplan*' " (1945, p. 104n; emphasis in the original).

Woodger listed five consequences (1945, pp. 105–106):

(a) If any part of one "life" is in morphological correspondence with a part of another "life," the two "lives" in question will exhibit a common *Bauplan*.
(b) If two "lives" exhibit the same *Bauplan*, then there will be a morphological correspondence between some set of parts in each.
(c) If one *Bauplan* overlaps another, then every "life" exhibiting the former will also exhibit the latter.
(d) If a "life" exhibits a *Bauplan*, then it will also exhibit every other *Bauplan* that overlaps the first.
(e) If one *Bauplan* determines a taxonomic group, α, and overlaps some other *Bauplan* that determines a taxonomic group, β, then α is included in β.

Woodger went on to show how this analysis provided the basis for a more precise account of homology and similar concepts than the alternatives that had thus far been used. In particular, in the case of homology, using the concept of morphological correspondence alone would provide a definition in which shared ancestry was not required between homologous entities (as, for instance, urged by Huxley (1942)). The problem of defining homology in stricter ways becomes one of choosing what criteria should be added beyond the requirement of morphological similarlity.

In the 1950s, Woodger (1953) used the semiformal method to explicate a developmental definition of "inborn." Starting with a primitive expression $\mathbf{dlz}(x, y, z)$ interpreted as the relation that the zygote x develops in the environment y into the phenotype z, Woodger first defined this relation for a set of "lives." He then defined a set of "lives" P as consisting of those that are obtained only from members of a specified zygotic range X in an environmental range Y ($X = \mathbf{Z}(P)$ and $Y = \mathbf{E}(P)$). Let $\mathbf{E}(P)$ be the restriction of the set of "lives" to a time equal to that of the attainment of adulthood by members of P. He then defined "inborn" in terms of environmentally insensitive sets of "lives" using the condition $\mathbf{E}(p) \subset \mathbf{E}(P)$. A more complex formulation removed the reference to adulthood. Similarly, "acquired" was defined in terms of environmentally sensitive sets of "lives."

Woodger's piece provoked a reply from Haldane (1955) who found the treatment not precise enough—and also suggested that complete precision was not only probably impossible but also of not much practical relevance to biology. Woodger (1956) claimed to have been misunderstood and, indeed, there is some reason to think that Haldane had been careless in reconstructing Woodger's formalism. In retrospect, what is important is that the exchange showed that, though Woodger had recognized some of the difficulties with the concept of innateness (which is what "inborn" was supposed to capture), the problems were even worse than what he had shown. Haldane had even less sympathy: "I certainly do not defend the term 'inborn character', and try to avoid using it" (1955, p. 245). Woodger had raised a problem that continues to trouble philosophy of biology even today (see below).

Teleology

Given the popularity of cybernetics in the sciences of the 1950s it is only to be expected that Nagel considered teleology and goal–directed processes in *Structure of Science* in 1961. In non-biological cases Nagel's discussion is not without interest. After a typically incisive analysis of the multiple, vague ways in which some biologists had invoked "irreducible" teleology for biological systems, Nagel noted that the use of variational principles in physics constitutes a certain kind of teleology. He goes on to show that such teleological processes can be given a non–teleological formulation (through conditions on systems of dynamical equations incorporating ordinary causal claims). This is supposed to show that, in principle, teleology in biological systems can also be given non–teleological formulation—thus, there was no *in principle* irreducible telelology in biology. The hierarchical organization of biological systems does not change this situation.

While this may be true, Nagel nowhere noted the critical role typically played by natural selection in producing biological goal–directedness or function. (Here Nagel's treatment was a step backwards from that of Mainx (1955)—see above.) Consequently, these discussions remained orthogonal to the main trends in the analysis of teleology in philosophy of biology starting around 1970. Nagel returned to the question of teleology at length in his Dewey Lectures of 1971 (Nagel 1977a, b) and the discussion there finally broached issues raised by the new molecular biology. An assessment of that analysis will be left for another occasion—post-1961 developments are beyond the scope of this article.

The Legacy

The final question to consider is that of the significance of this work for the philosophy of biology not only as biology was practiced in the 1940s and 1950s, but especially as the philosophy of biology developed in the post-1961 period, eventually becoming a major subdiscipline within philosophy of science. In the case of Nagel's work on reduction, this is relatively straightforward. The first point to note is Nagel's caution about the possibility of a reduction of biology to the physics and chemistry of his day. Somewhat strangely, this is where there is ground for criticism. Between 1952 and 1961, Nagel saw no reason to change his relatively negative assessment of the possibility of the reduction of biology to current physics and chemistry. The events of 1953 and the creation of a successful molecular biology seem to have passed him by (Sarkar 1989).[24] However, this criticism cannot be leveled against all within the logical empiricist corpus. In 1956, Kemeny and Oppenheim (1956) produced an alternative model of reduction. According to this model, the reduced theory can explain all the phenomena explained by the reduced theory (or more) and is better "systematized."[25] Deploying this model, Oppenheim and Putnam (1958) published their well-known defense of the unity of science through reduction in 1958.

Oppenheim and Kemeny (1958, p. 9) recognized six levels between which potential reductions could occur: (1) elementary partcles; (2) atoms; (3) molecules; (4) cells; (5) (multicellular) living organisms; and (6) social groups. They then described what they interpreted as successful reductions between these levels while emphasizing that most of them remained incomplete and that much work remained to be done. Leaving aside details of their analysis (which is marred by the nonstandard model of reduction that they use), Oppenheim and Putnam provided a clear account of the significance of molecular biology in producing a reduction between levels (4) and (3) (1958, pp. 20–22). In particular, they give a clear account of the double helix model for DNA, note its informational interpretation, and also how the surface shape of molecules is responsible for the specificity of biological interactions. They endorse (correctly) a molecular interpretation of semi-conservative replication leading to gene duplication but (incorrectly, in retrospect) also support Delbrück's quantum transition model of a mutation (Timofeeff et al. 1935) which had become implausible by that time. This seems to have been the first philosophical discussion of molecular biology: it is remarkable how accurate the discussion of molecular biology was and, from the perspective of this article, what must be emphasized is that Oppenheim and Putnam (at that time) worked within the logical empiricist framework.

In the 1960s, models similar to Nagel's began to be deployed to make the case for reductionism in molecular biology starting with Schaffner's work (e.g., Schaffner 1969).[26] In the 1970s, these analyses were rejected by Hull (1972, 1974) on the grounds that the relations between biological phenomena and physicochemical mechanisms were "many–many." Within the emerging subdiscipline of the philosophy of biology, Hull's arguments came to be widely accepted even though subsequent work showed them to be flawed (Wimsatt 1976; Sarkar 1989): a mechanism in a given molecular context (and reductionists following Nagel could not ignore the context) gives rise to a specific event—nothing in molecular biology departs from such a determination. Meanwhile, Wimsatt (1976) produced an alternative model of reduction. In the 1980s, while Kitcher (1984, e.g.) accepted Nagel's model as a correct model of reduction, he rejected any claim that reduction was taking place in molecular biology—and this was generally taken to be a problem for Nagel's model of reduction. (Note the irony here: Nagel himself had remained extremely cautious about a reduction of biology to physics and chemistry. The critics ignored Nagel's caution.) There emerged a near–consensus among philosophers of biology that Nagel's analysis of reduction was of little value in the philosophy of biology. The anti-reductionist consensus began to be challenged in the late 1980s (Sarkar 1989, 1992a; Waters 1990). There has been no consensus since. The question whether there is reduction in biology—and not only in molecular biology—is a live topic in philosophy today (Brigandt and Love 2012), and it all goes back to Nagel and the logical empiricists. Moreover, since about 2000, in contexts other than biology Nagel's analysis of reduction has received a remarkable

[24] The same criticism can also be leveled against Beckner's (1959) dissertation, written under Nagel's supervision, which also ignores molecular biology altogether.

[25] As many commentators have pointed out, this model is more one of theory replacement than reduction—see, e.g., Schaffner (1967b) and Sarkar (1989, 1998) for further detail.

[26] Schaffner's original interest was in reduction in physics. Nagel explicitly required him to include genetics as a case study in his dissertation at Columbia University in the 1960s which was written under Nagel's supervision (Ken Schaffner, personal communication, 1989).

revival though these developments are beyond the scope of this article.[27]

What has largely disappeared, though, is much discussion of emergence, as a contrast to reduction, that occupied so much of Nagel's attention. However, what this shows, is the extent to which molecular biology has transformed all of biology. Before molecular biology, claims that biology would be reduced to physics and chemistry were little more than promissory notes—in spite of their great heuristic value in directing research (as Roll–Hansen (1984), for instance, has emphasized). Since molecular biology, these claims have become much more plausible (what Nagel (1961) failed to recognize). However, discussion of emergence has not entirely disappeared (Gilbert and Sarkar 2000; Allen 2005), a point to which I will return in the next section. In these discussions, Nagel's care in distinguishing different senses of key terms remains as invaluable now as it was in the 1950s.

Woodger's legacy is more contested. Nicholson and Gawne (2014) have documented at length the many disparaging remarks about Woodger that Hull (e.g., 1994) and Ruse (e.g., 1984) have made over decades. These remarks are almost entirely directed against Woodger's use of formal techniques but there is no evidence that Hull or Ruse ever understood them.[28] More pertinent are critics such as Haldane (1938b, 1955) who did follow Woodger. Haldane's (1938a) review of *Axiomatic Method in Biology* was largely negative, but, written at the height of his proselytization of dialectical materialism (Sarkar 1992b), a negative assessment is perhaps only to be expected of what, to Haldane, appeared to be a quintessentially logical positivist work. He did raise some serious technical objections to Woodger's formal characterization of the fertilization process—but these were largely a result of the simplifications that Woodger had been forced to introduce and (as Haldane explicitly noted) were supposed to be tentative. Haldane concluded that the book's "main importance may be to lay bare erroneous assumptions rather than to serve as the basis of a further theoretical construction" (1938, p. 266). What Haldane had in mind here was the complexity of defining mereological relations in biology (which was emphasized above). Haldane's dispute with Woodger on innateness in the 1950s can similarly be seen in a more fruitful light—though, by then, Woodger had begun to use semiformal rather than full-fledged axiomatic techniques. Woodger's treatment of innateness is an important precursor to the many explications of that confusing concept in recent philosophy of biology though no one seems to recognize his priority (see, e.g., Griffiths (2002), which does not acknowledge Woodger's work).

The importance of these semiformal techniques is perhaps best seen in Woodger's explication of the concept of *Bauplan*, which was treated in some detail above. Here, Woodger is typically given full credit within biology, especially in evolutionary developmental biology—see, for instance, Hall (1999). But even the axiomatic method cannot be entirely dismissed. In developing cladistics, Hennig (1950) cited Woodger's axiomatic treatment (as well as Bertalanffy's work) extensively, in particular, on the question of temporal identity.[29] Woodger's (1945) influence was also strongly felt in Hennig's discussions of homology. Finally, should there ever be a theory of developmental evolution—what Raff (1996) has called the "shape of life"—Woodger's explication of the concept of a *Bauplan* will likely emerge as his most important theoretical contribution in biology; at present, this can only be a matter of speculation.

Final Remarks

Most of the conclusions that emerge from this exploration were already drawn in the last section. The additional remarks here will be limited to a discussion of two questions:

1. *Why was the earlier logical empiricist work ignored in the 1970s when philosophy of biology emerged as an independent subdisicpline within philosophy of science*? This question was raised on every occasion that the argument of this article has been presented to an audience and it deserves at least a tentative answer. At least four factors seem to have played a role:

    1. The first point to note is that the late 1960s and 1970s saw an almost complete rejection of logical empiricism within the philosophy of science following the influential criticism of figures such as Paul Feyerabend, Norwood Russell Hanson, and, especially, Kuhn. What logical empiricism was supposed to be was reduced to a cartoon by Suppe (1974), possibly the single most widely used source for logical empiricism used by students entering philosophy of science during that decade. Logical empiricist philosophy of biology fell by the wayside during this general rejection of logical empiricism. Woodger was regarded as the quintessential logical empiricist because of his interest in formalization.

[27] See, especially, the recent critical reviews mentioned earlier, by Schaffner (2013) and Sarkar (2015).

[28] Ruse (1975) pretends to engage with Woodger's formal work but does not do so with any subtlety.

[29] Two recent papers by Rieppel (2003, 2006) are particularly important for the reconstruction of philosophical influences on Hennig's notoriously obscure writings. Besides Woodger and Bertalanffy, Rieppel notes the apparent influence of Carnap.

Nagel was self-admittedly an unreconstructed logical empiricist. It is likely a new generation of philosophers of biology ignored Woodger and Nagel without ever having read them.

2. In Woodger's case, the deployment of formalism put much of the work beyond the competence of philosophers of biology of the period. It was easier, and perhaps more professionally expedient, for them to dismiss this work than to understand it.
3. As noted earlier, Nagel's model of reduction was considered irrelevant partly because the possibility that molecular biology provided a reduction of parts of biology to physics and chemistry was itself rejected by figures such as Kitcher (1984) who were then quite influential. Moreover, those who still continued to be interested in reduction, especially in the philosophy of mind, came to largely regard it as an ontological issue rather than an epistemological one as emphasized by Nagel (and Hempel) (Sarkar 2015). Given also that Nagel's analysis of teleology in 1961 left much to be desired in the context of biology (see above), it is not entirely unreasonable that there was a general impression that logical empiricism had little to offer to the philosophy of biology. The sympathetic reconstruction of logical empiricism in this article critically required a prior defense of Nagel's analysis of reduction (Sarkar 2015).
4. At a more individual level, Hull's (1974) blanket rejection of logical empiricism in his *Philosophy of Biological Science*, probably the most used textbook for the philosophy of biology from the period, played a major role. Hull, in turn, seems to have been influenced by the rejection of logical empiricism expounded by Michael Scriven under whose direction he wrote his dissertation at Indiana University.

2. *Leaving history aside, do the logical empiricist analyses of biology continue to be of relevance today*? It was pointed out above that the questions of reduction and emergence were transformed by the emergence and establishment of molecular biology in the 1950s and 1960s and that much of the discussions of these questions from earlier decades became largely irrelevant. However, molecular biology in the post-genomic era may have given new life to the question of emergence which, in any case, had never entirely disappeared, particularly in developmental biology (Gilbert and Sarkar 2000). In molecular biology today, gene regulatory networks (GRNs) have come to the forefront of research and may offer some promise for a better understanding of development (Davidson 2010). The possibility of emergence in such networks remains an open question that may well benefit from a Nagelian analysis of what is meant by the term. The possibility of emergence has also emerged in a somewhat different form in individual–based models (IBMs) which show promise in theoretical ecology (Sarkar 2005). It is widely recognized that the concept of emergence in ecology has multiple meanings; this aspect of the concept is believed to be productive in spite of the imprecision (Railsback and Grimm 2011). A Nagelian analysis may well prove to be of value here should it be performed with the same care and open–mindedness of Nagel's original effort from 1952. The defense of Woodger's semiformal methodology, if not of his axiomatizations, is even more straightforward. Indeed, formal work in philosophy of biology is commonplace today—in particular, in the context of the foundations of evolutionary theory (e.g., Sober and Wilson (1999); Kerr and Godfrey-Smith (2002); Sarkar (2004, 2008, 2014); Okasha (2006, 2008); Plutynski (2006)) but not limited to it (e.g., Sarkar (1998); Griesemer and Wade (2000); and Stegmann (2009)). Woodger should be regarded as one of the pioneers of this approach. In biology, proper, development has once again become a source of theoretical interest, as noted earlier, and Woodger's explication of the Bauplan remains the most interesting such explication to date.

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