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Rethinking Woodger's Legacy in the Philosophy of Biology

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Abstract. The writings of Joseph Henry Woodger (1894–1981) are often taken to exemplify everything that was wrongheaded, misguided, and just plain wrong with early twentieth-century philosophy of biology. Over the years, commentators have said of Woodger: (a) that he was a fervent logical empiricist who tried to impose the explanatory gold standards of physics onto biology, (b) that his philosophical work was completely disconnected from biological science, (c) that he possessed no scientific or philosophical credentials, and (d) that his work was disparaged – if not altogether ignored – by the biologists and philosophers of his era. In this paper, we provide the first systematic examination of Woodger's oeuvre, and use it to demonstrate that the four preceding claims are false. We argue that Woodger's ideas have exerted an important influence on biology and philosophy, and submit that the current consensus on his legacy stems from a highly selective reading of his works. By rehabilitating Woodger, we hope to show that there is no good reason to continue to disregard the numerous contributions to the philosophy of biology produced in the decades prior to the professionalization of the discipline.

Keywords: J. H. Woodger, History of philosophy of biology, Logical empiricism, Axiomatization, Bauplan, Organicism, A. N. Whitehead, Theoretical Biology Club

> [T]here is little if anything of value in Woodger's work, and [...] therefore the time has now come to draw a decent veil over a biological dead-end.
>
> – Ruse, 1975, p. 2

> [T]he only way to understand a philosophical writer is by reading his own works, not by reading what other people say about him.
>
> – Woodger, 1929a, pp. 47–48

Whence Philosophy of Biology?

Philosophers of biology consider their discipline a relatively young field of research, having only emerged in the last third of the twentieth century. Its inception is usually traced back to the publication of two influential textbooks, Michael Ruse's *The Philosophy of Biology* (1973) and David Hull's *Philosophy of Biological Science* (1974), as well as the appearance of a series of articles by Kenneth Schaffner (1967, 1969a, 1969b) and William Wimsatt (1972a, b, 1974).[1] In their textbooks, both Ruse and Hull openly decried the fact that previous generations of philosophers had paid little attention to the life sciences. Ruse asserted that "the author of a book on the philosophy of biology need offer no excuse for the subject he has chosen, since few areas of philosophy have been so neglected in the past 50 years" (Ruse, 1973, p. 9). Likewise, Hull noted that his book would "take a closer look at that area of science which has been passed over in the rapid extrapolation from physics to the social sciences" (Hull, 1974, p. 6). A decade later, Alexander Rosenberg published another textbook on the philosophy of biology (Rosenberg, 1985), which was explicitly intended as an update of Ruse and Hull's introductions to the field. Like Ruse and Hull, Rosenberg remarked that until recently philosophical discussions of biology had been "usually an afterthought to discussions of physics" (*ibid.*, pp. 6–7). Similar claims can be found in the writings of many other philosophers of biology, usually in the preface or introduction of monographs and edited volumes (e.g., Sober, 1984, pp. 6–7; Brandon,

[1] Ernst Mayr (1961, 1969) and Marjorie Grene (1968, 1974) are also frequently acknowledged to have played an instrumental role in the formation of the philosophy of biology as an academic discipline. When the International Society for the History, Philosophy, and Social Studies of Biology was established in the late 1980s, Mayr and Grene were elected its 'Honorary Presidents'.

1996, xii–xiii; Kitcher, 2003, xv; Matthen and Stephens, 2007, pp. xi–xii). Ruse, in particular, has taken great pains over the past 40 years to reiterate that before the late 1960s, the philosophy of biology "really did not exist as a subject" (Ruse, 1979, p. 785), and remind us that "David Hull is the father of modern studies of biology from a philosophical viewpoint" (Ruse, 2008, p. 3). (See also Ruse, 1988, p. 1; 1997, p. 120; 2006, p. 37; Hull and Ruse, 2007, xix–xx; Takacs and Ruse, 2011, pp. 1–2.) As a result, this understanding of the history of the philosophy of biology has become deeply entrenched and is now simply taken for granted among practitioners of the field.

Of course, no philosopher of biology – including those cited above – has been careless enough to suggest that no work whatsoever was conducted prior to the professionalization of the discipline. Nevertheless, what everyone seems to agree on is that the few philosophical discussions of biology that *did* take place in the earlier decades of the twentieth century were misguided or unproductive, and do not stand in an ancestral relation to the contemporary discourse. Recently, however, this widely held view has come under scrutiny. In 2007, Jason Byron published the results of a bibliometric survey which revealed that nearly one tenth of the articles published in the first philosophy of science journals – *Erkenntnis*, *Philosophy of Science*, *Synthese*, and the *British Journal for the Philosophy of Science* – between 1930 and 1959 were devoted to topics in the philosophy of biology. Even though Byron's survey was restricted to four periodicals, and did not consider monographs or edited volumes published during the same period, his "first-pass, rough-grain analysis of some relevant evidence" (Byron, 2007, p. 413) suffices to show that the philosophy of biology played a far more prominent role in early twentieth-century philosophy of science than has been hitherto acknowledged by contemporary authors. Byron also argued that that the majority of articles published before the professionalization of the field were similar in content to the modern literature, noting that "[e]ven a cursory glance through the [titles] reveals a large number of articles that any contemporary philosopher of biology would recognize as 'real' philosophy of biology" (*ibid.*, p. 416). Although we agree, bibliometric data alone cannot establish this claim. To do this, one must engage directly with the literature produced in that period: analyze its content, evaluate its arguments, and consider its connection and possible relevance to contemporary philosophy of biology. This paper is intended as a first step in this extensive and ambitious undertaking.

The publications of Joseph Henry Woodger (1894–1981) are frequently cited as proof that early twentieth-century philosophy of biol-

ogy was a failed enterprise. In fact, a casual reference to Woodger's works is usually the only way that the claim in question is supported. In the late 1960s and early 1970s, those like Hull and Ruse who sought to establish the philosophy of biology as a professional academic discipline were quick to attack Woodger (who had by then retired) in order to distance themselves from his particular mode of theorizing. However, their criticisms were anything but informed, or even-handed. In fact, as we will see shortly, they are better described as *ad hominem* taunts than as carefully reasoned arguments. Subsequent newcomers to the field simply restated the remarks of the earlier generation without bothering to consult the relevant primary sources, and as a result, a number of myths about Woodger have become generally accepted. Because these misconceptions are frequently used to justify the claim that no interesting or worthwhile philosophy of biology was produced during the first half of the twentieth century, rethinking Woodger's legacy will go a long way toward refuting the received view of the history of the field. Restoring Woodger to his rightful place in the pantheon of twentieth-century philosophers of biology constitutes the first chapter in the long overdue project of rehabilitating the various generations of neglected thinkers who fruitfully worked at the intersection of biology and philosophy in the decades prior to the professionalization of the discipline.

Woodger's Legacy: The Current Consensus

Woodger is rarely mentioned in the philosophy of biology today, but when his name is invoked it is usually followed by a dismissive remark. The alleged shortcomings of his writings are seldom described in detail, but commentators generally agree that there is little if anything of import in his work, and hence it can be safely cast aside (as Ruse declares in the first epigraph of this paper). This view constitutes the current consensus regarding Woodger's legacy in the philosophy of biology, and it consists of four distinct claims.

The first and most frequently repeated criticism of Woodger is that he was a fervent logical empiricist whose attempts to impose the explanatory gold standards of the physical sciences onto biology were hopelessly misguided. Ruse, for instance, has stated that Woodger was an "empiricist of the most naively dogmatic kind" (Ruse, 1988, p. 1) who "insisted on putting everything into rigorous deductive systems, with absolutely disastrous consequences" (Ruse, 2000, p. 478). Along the same lines, Werner Callebaut notes that "John [*sic*] Woodger, a Cambridge [*sic*] embryologist turned logician, tried to force biological

theories into the logical-empiricist deductive corset'' (Callebaut, 2005, p. 104). Similar assertions of Woodger's supposed allegiance to logical empiricism can be found in Smart, 1963; Roll-Hansen, 1984; Rosenberg, 1985; Thompson, 1989; Sarkar, 1996; Wolters, 1999; and Cain, 2000.[2]

The second imputation associated with the current consensus is that Woodger's philosophical ideas were completely disconnected from the biological science of his time due to his lack of interest in, and arrogant contempt of, empirical research. This view has been stated most emphatically by Hull, who writes that ''Woodger pursued logical snarls with great enthusiasm, no matter how irrelevant they might be to biology, but dismissed empirical issues with an indignant snort, no matter how central they might be to the science that he claimed to be professing the philosophy of'' (Hull, 1994, p. 375). In a similar vein, Ruse mockingly describes Woodger as a ''mathematical magician and biologist'' (Ruse, 1984, p. 453) who published ''incomprehensible formalisms'' (Ruse, 1979, p. 785), and whose philosophy ''had little connection with real science, real biological science or real physical science for that matter'' (Ruse, 2000, p. 478). Nils Roll-Hansen has added to this that Woodger employed a priori arguments to reject ''a whole series of fruitful ideas from genetics, embryology, and evolution'' (Roll-Hansen, 1984, p. 427).

The third criticism of Woodger is that he possessed no real scientific or philosophical credentials, and consequently his work at the intersection of these fields can only be charitably described as second-rate. Roll-Hansen has been quite explicit on this point, asserting that ''[a]s a biologist, Joseph Henry Woodger [...] did not have the stature of earlier antireductionist methodologists such as Bernard, Haldane, or Oscar Hertwig. Nor was he a philosopher of the first class like Høffding, Broad, or Whitehead. But he was representative of the philosophy of science of his period, more specifically of the movement called logical empiricism'' (*ibid.*, pp. 415–416). Ruse has also questioned Woodger's biological credentials, arguing that he misconstrued the fundamental concepts of genetics (Ruse, 1975) and that he ignored ''the basic message of modern evolutionary biology'' (Ruse, 1984, p. 453).

The final, and perhaps most disparaging, claim made about Woodger is that even in his own lifetime, he was not well known, respected, or read by his peers – either in biology or philosophy – and consequently his ideas only exerted a marginal influence. Joe Cain has stated that

[2] Following standard usage, we do not distinguish between logical empiricism and logical positivism, and so we discuss together authors who have referred to Woodger as a logical empiricist as well as those who have referred to him as a logical positivist.

"Woodger can scarcely be described accurately as well known or prominent among biologists" (Cain, 2000, p. 539). "All evidence", insists Cain, "suggests Woodger was a minor figure within biology" (*ibid.*, p. 540). Marjorie Grene and David Depew make a similar claim in their monograph on the history of the philosophy of biology, remarking that "J. H. Woodger produced in the nineteen thirties what was supposed to be a statement of the principles of biology; but apart from a few followers in Great Britain, his effort had little influence" (Grene and Depew, 2004, p. 290). The widespread belief is that Woodger's work "was generally spurned by biologists" (Roll-Hansen, 1984, p. 416) and that, as a result, he "became progressively more isolated in his work and tragically resulted in talking to no one" (Haraway, 1976, p. 131). Hull, once again, puts it most poignantly:

> When I began studying philosophy of biology, J. H. Woodger, an embryologist turned logician, was one of the few people who had published in this field. As a result, I was forced to work my way through all of his publications, and I have never gotten over it. Some of the points Woodger made were worthwhile, but the amount of labor necessary to discover these kernels of wisdom far outweighed their value […] *More importantly, very few biologists or philosophers give any indication of ever having read Woodger*. Perhaps the axiomatic method is useful in uncovering and resolving logical puzzles, but it is not a very effective way of transmitting these resolutions to others. (Hull, 2000, p. 68) [Emphasis added]

The preceding quotations are typical of the way Woodger's oeuvre is treated by contemporary philosophers of biology. Thus, it seems only natural that scholars not familiar with Woodger's writings would choose to ignore them upon reading such remarks. We suspect that this is why the current consensus on Woodger has never been challenged. In what follows, we will show, through a detailed examination of Woodger's body of work, as well as a number of relevant historical sources, that the current consensus on Woodger is unfounded and misleading.

A Reappraisal of Woodger's Oeuvre

The present disregard for Woodger's writings stems from the fact that nowadays he is known almost exclusively for his attempts to axiomatize biological theories. However, the truth is that this research program occupied him for only one portion of his multifaceted career. Before his

formal turn, Woodger published a number of non-formal works on philosophy of biology, and prior to that, he was an experimental embryologist and cytologist. The best way to dispel the myths associated with Woodger is to carefully trace the course of his intellectual development. Only by considering his entire corpus in detail and situating it in its appropriate historical context will we be able to evaluate the significance and relevance of his ideas. As no systematic account of Woodger's body of work exists in the literature, we shall attempt to provide it in this section.

Woodger's scholarly career may be divided into three fairly distinct periods: an initial empirical period, an intermediate critical period, and a final formal period.[3] Let us consider each of them in turn.

Empirical Period (1914–1925)

Woodger graduated with honours in Zoology and Comparative Anatomy from University College, London (UCL) in 1914, taking embryology as a special subject and physiology as a subsidiary subject. His academic performance earned him the College prize in Zoology, as well as the Derby research scholarship, but his postgraduate research was interrupted by the outbreak of the First World War. Woodger enlisted in the army and was drafted to Mesopotamia, where he served for a term with the second battalion before being appointed protozoologist to the Central Clinical Laboratory in Amarah in 1917 (Floyd and Harris, 1964, p. 2). There he studied plague rats and investigated the possibility of a relationship between jaundice and hook worm infections. He also analyzed the possible carriage of amoebic dysentery by house flies – research which he later published as Woodger, 1921. Following his discharge from the military in 1919, Woodger returned to UCL, where he conducted embryological research with J. P. Hill and cytological studies with J. B. Gatenby. With the latter he published work on the formation of Golgi bodies during oogenesis (Gatenby and Woodger, 1920) and spermatogenesis (Gatenby and Woodger, 1921) in guinea pigs. Woodger also conducted empirical research on his own, specifically on the origin of germ cells in fowl (Woodger, 1925).

In 1922, Woodger left UCL to take the new Readership in Biology at Middlesex Hospital Medical School (now UCL Medical School), where he remained until his retirement in 1959. Soon after his appointment, he was faced with a very heavy teaching schedule, being responsible for delivering all the lectures for the Biology degree, as well as supervising

[3] Cain, 2005 proposes a similar division of Woodger's intellectual work.

the laboratory work for medical courses like human histology (Floyd and Harris, 1964, p. 2). Despite this, he managed to find the time to carry out the aforementioned research on the germline in birds, become fluent in German, and write and illustrate a five-hundred page textbook for his students, *Elementary Morphology and Physiology for Medical Students* (1924), which went through two subsequent editions (published in 1935 and 1943). Reviews of the book (Anon., 1925, 1936, 1944; H. L., 1944) praised its illustrations and established Woodger's reputation as a skilled and thoughtful teacher. *Elementary Morphology and Physiology* also came to be known for its advocacy of new goals for the medical curriculum, such as the tenet that medical students ought to memorize less and critically reflect more.[4]

The interest of *Elementary Morphology and Physiology* lies in the fact that it presents Woodger's earliest attempt to formulate a comprehensive view of the biological enterprise. The presentation of the empirical materials is arranged around the concept of organization, with successive chapters progressively moving across different 'grades', 'types', and 'plans' of organization. Organisms that display the same organizational plan constitute a specific type of organization, and each type can be further subdivided into different grades of organization. In this way, the concept of organization provides a means of theoretically structuring Woodger's empirical examinations of the different morphologies and physiologies found in the living world. Another interesting aspect of Woodger's discussion is the thesis that the triad of form, function, and environment constitutes the fundamental nexus which any explanation of organismic phenomena must attend to. Speaking of natural selection, for instance, he remarks that:

> This hypothesis involves all three aspects of the threefold relation of form, function, and environment. Selection is the result of the operation of environment in the widest sense of the term, but of itself it can do nothing. The organism supplies the raw materials for selection to work upon in the shape of variations of either form or function. It is the special merit of Darwin's theory that it took into account all three aspects of the problem, as all theories of the organism must do (Woodger, 1924, p. 465).

Elementary Morphology and Physiology ends with a chapter entitled 'Theoretical Biology and the Method of Science'[5] where Woodger

[4] As we will see later, Woodger returned to this concern at the end of his life, making it one of the central themes of his last book, *Physics, Biology, and Medicine* (1956a).

[5] We suspect that this is one of the first usages of the term 'theoretical biology' in the English language.

examines the nature of scientific theorizing and distinguishes between analogical, inductive, and deductive inferences. The chapter concludes with a brief discussion of philosophical problems specific to biology, such as the limitations of reductionism in the study of the organism (*ibid.*, pp. 500–501).

Critical Period (1926–1930)

In 1926, Woodger was granted a semester's leave to learn the experimental techniques developed by Hans Przibram at the Institute for Biological Research (informally known as the *Vivarium*) in Vienna. He had initially planned to work on transplantation in annelids, but owing to problems with the animal model he was forced to abandon his research almost immediately. Having no empirical work to occupy himself with, Woodger spent most of his time talking to Przibram and his students about the philosophical foundations of biology. Through these discussions Woodger realized that there were unanalyzed presuppositions underlying the majority of biological theories and that his scientific training had not equipped him – nor was it likely to have equipped any other biologist – to identify or examine them (Floyd and Harris, 1964, p. 3).

Although he would go on to publish several more empirical papers (de Beer and Woodger, 1930; Woodger, 1931b; Woodger and Hill, 1938), Woodger's visit to Vienna marked a turning point in his career. Upon his return to England, he embarked on an intensive study of analytic philosophy, and began to devote himself almost exclusively to the critical examination of theoretical and methodological issues in biology. As he described it, the driving force behind his decision to abandon empirical research for philosophical work was the fact that:

> [N]o one had attempted to do for biology anything analogous to what Galileo had done for physics, and Boyle had done for chemistry. No one, that is to say, had undertaken a systematic critical study of the fundamental properties and special requirements of this science in relation to the most advanced metaphysical, epistemological and logical notions of the day (Woodger to University of London Registrar, 6th January 1930).

This he set out to do, and in 1929 he published what he referred to as the 'tentative results' of his research: a five-hundred page treatise entitled *Biological Principles: A Critical Study*, for which UCL awarded him a D.Sc. degree in the Principles, Methods, and History of Science.

Biological Principles is an exercise in what Woodger calls 'critical biology', which involves the examination of the epistemological foundations of biological knowledge through the analysis of its central concepts. According to Woodger, a critical study of biology is necessary because it is an extremely fragmented science. The process of subdivision into specialized branches that characterizes progress in any science has not been supplemented, in the case of biology, by generalizations that knit the findings of its various branches into a unified whole. Instead, it has spawned ever increasing divergences in theoretical outlook between exponents of the different branches. These divergences reflect fundamental dichotomies lying at the heart of biological thought. Woodger views biology as "a science of antitheses" (Woodger, 1929a, p. 11), and it is the persistence of these antitheses that prevents the harmonization of its facts. The six core biological antitheses that he identifies are: mechanism versus vitalism, structure versus function, organism versus environment, preformation versus epigenesis, causation versus teleology, and body versus mind. For Woodger, these antitheses are not really 'out there' in nature, but result primarily from the ways propositions drawn from empirical data are systematized into a body of knowledge. Strictly speaking, they do not belong to the subject matter of biology (i.e., to the nature of organisms), but to the nature of biology *as* knowledge. The problem, Woodger points out, is that biologists tend not to be interested in such matters: "[t]hey are concerned with *getting* knowledge *about* animals and plants. They are interested in such knowledge, but not in it *as* knowledge" (*ibid.*, p. 14). It was Woodger's hope that an inquiry into the nature of biological knowledge would help biology become a more unified science.[6]

To aid him in his critical examination of biology, Woodger drew on the new view of nature that was emerging from the revolutionary developments taking place in physics at the time. Woodger became acquainted with the philosophical implications of relativity and quantum mechanics by studying the work of Bertrand Russell (1912, 1918, 1921), Alfred North Whitehead (1919, 1920, 1922, 1925), and C. D. Broad (1923, 1925). Whitehead, especially, exerted a profound influence on Woodger, for it was he who emphasized more forcibly than anyone else that the developments in theoretical physics would require a fundamental revision not only of the foundations of physics but of natural science in general. Woodger makes Whitehead's conviction his own in the opening paragraph of *Biological Principles*:

[6] Woodger's efforts to bring about the theoretical unification of biology are documented in Smocovitis, 1992, 1996.

> Modern natural science may be likened unto a crab which has grown too fat for its shell. The process of ecdysis is slow and painful. The old shell, which has matured and hardened for some three hundred years, has done good service. No wonder the crab is loath to part with it. But it has already begun to crack, and some bits have even dropped off. What is to be done? Should the crab go on getting fat and take no thought for the raiment of the morrow? Or should it resolutely face the situation, heave off the remains of the old shell with a sigh, and set about making a new one in earnest? There is a great deal of uncertainty about the precise form of the future new shell. But the evil day cannot be delayed much longer, and if it is put off too long the process of growth may suffer, or the whole may fall to pieces for lack of support. (Woodger, 1929a, p. 1)

The data of natural science constitute the crab's meat. The crab's shell represents the general framework upon which the data have been systematized, namely the mechanistic philosophy of nature of classical physics, with its emphasis on reductionism, determinism, and machine thinking. Scientists were understandably reluctant to renounce these mechanistic intuitions, as they had served them so well in the past, but the deployment of new ones seemed inevitable if further scientific progress was not to be compromised. Whitehead had been developing a new organicist philosophy of nature to supplant the older mechanistic worldview, and through his reading of Whitehead's writings Woodger surmised that such a philosophy was as suitable for biology as for physics. This is because it is a view which emphasizes processual change, internal organizing relations, and systemic irreducibility – notions more familiar to biologists than to physicists, as they had been central to the vitalist traditions in physiology and embryology for more than a century before. Although only a short time earlier such concepts had been viewed with suspicion and derision by the majority of biologists (who subscribed to the mechanistic paradigm), the collapse of mechanicism in physics opened up a new way of thinking about biology that denied its reducibility to physics whilst retaining a common overarching view of nature. It was such an organicist philosophy of biology that Woodger sought to develop in *Biological Principles*.

Biological Principles is divided into two parts. Part I deals with general epistemological problems involved in the systematization of data into scientific knowledge, and Part II examines the difficulties of biological knowledge in particular, namely the aforementioned biological antitheses. The first part begins with a detailed exposition and cri-

tique of phenomenalism, the radical form of empiricism which argues that physical objects cannot justifiably be said to exist in themselves, but only as perceptual phenomena or sensory stimuli. Woodger considers the fundamental error of phenomenalists like Ernst Mach and Karl Pearson to be that they presupposed the very knowledge of the real world they believed to be unattainable – knowledge about brains and sense organs as physical objects in the world. But what troubled Woodger the most was the tacit and uncritical endorsement of phenomenalism by prominent biological thinkers such as Max Verworn (1899), Lloyd Morgan (1923), and Jakob von Uexküll (1926), which invariably lead to a muddled understanding of the nature of scientific knowledge. Woodger proposes a realist epistemological alternative to phenomenalism that regards perception as an interpretive process involving both sensing and thinking, and which by various modes of abstraction results in the perceptual objects of common-sense knowledge as well as the more abstract concepts and propositions of natural science. Woodger also analyzes the categories of substance and cause and argues, following Whitehead, that both of them give expression to our experience of permanence in nature; the former expresses the stability of spatial characterizations without (intrinsic) change, and the latter expresses regularity in the mode of change of our characterizations.

Part I of *Biological Principles* ends with an examination of the subjective dimension of scientific thinking, which consists in a discussion of factors that are "capable of 'furnishing a motive for research', are in some sense a priori, are liable to be used blindly and uncritically, and, as we shall see, are of great importance for the study of the biological antitheses" (Woodger, 1929a, p. 203). These factors include the desire for monistic interpretations, the refusal of arbitrary breaks in nature, the attempt to reduce all science to physics, the demand for simplicity in explanation, the desire for atomistic interpretations, the demand for verification, the demand for predictability and unequivocal determination, and the postulate of the validity of inductive generalizations. Woodger views these subjective factors as conservative habits of scientific thought not primarily based on reason, but which induce scientists to persist in their adherence to existing explanatory frameworks instead of seeking to formulate new ones. Woodger does not argue that we should reject these factors, but simply that we should recognize their influence as unacknowledged intellectual convictions which may not be necessarily fit for the task of understanding the complexity of nature.

Part II constitutes the core of *Biological Principles*, as it is here where Woodger subjects the fundamental antitheses of biology to critical examination employing, when appropriate, the insights of Part I. The first antithesis he discusses, mechanism versus vitalism, deals with the nature of the organism: is a living system a machine (albeit a very complex one) fully explainable in mechanistic terms, or is it a different kind of system altogether, requiring different modes of explanation? In Woodger's view, the chief problem that prevents a resolution of this antithesis is that mechanists and vitalists appeal to convictions rather than reason, and end up talking past each other. His lucid diagnosis of the unfruitful state of this dispute deserves to be quoted at length:

> Both parties have made up their minds and decided the problem in advance, and there is no possibility of reconciliation between them. *One* of them is certainly wrong and both *may* be wrong. Each party will, of course, profess to base its claim on experience. The mechanist will be able to point to a long series of triumphs in the past and to the short period during which active research on his lines has been pursued. Being a firm believer in the 'uniformity of nature' his final success will seem to him to be a foregone conclusion. He will wonder how any reasonable man can possibly fail to share his opinions and will conclude that his opponents cannot be reasonable men. He will accuse them of being the victims of prejudice and other 'subjective factors' never dreaming that he may also be a victim of them himself. The dogmatic vitalist, on the other hand, will contend that living things, since they are not yet explicable in mechanistic terms, and since they exhibit peculiarities which are not encountered in the inorganic world, belong to a different order of being. His faith is not shaken by his opponent's success because he has long and complicated arguments which (in his opinion) place those successes in their proper perspective. But his opponent's faith is equally unshaken by such replies – chiefly because he does not read or understand them. Thus the vitalist concludes that his opponent is a man of crude sensibilities and inferior intellect, and the dead-lock is complete. (*ibid.*, pp. 230–231)[7]

Besides the importance of recognizing the aforementioned subjective factors at play in the thinking of both sides, Woodger argues that the deadlock can only be overcome by removing the confusion generated by

[7] Forty years after *Biological Principles*, Needham would note that Woodger's insightful examination of this antithesis marked the end of disputes between 'dogmatic' vitalists like Driesch and the mechanistic orthodoxy, which had featured prominently in the philosophy of biology in the early twentieth century (Haraway, 1976, p. 131).

the ambiguity of the basic terms used in the dispute. For instance, he notices that the term 'mechanistic explanation', as used in biology, has four different meanings: (a) an explanation in accordance with the laws of mechanics, (b) an explanation that uses physicochemical concepts, (c) an explanation based on the machine conception of the organism, and (d) a causal explanation in general.[8] Likewise similar problems plague the term 'vitalism', as it does not distinguish the wide variety of anti-mechanistic views. For example, it fails to distinguish between the metaphysical holism of Hans Driesch (who described himself as a vitalist) and the naturalistic anti-reductionism of J. S. Haldane (who considered himself an organicist, but was frequently referred to as a vitalist by his critics). In the end, Woodger concludes that the mechanistic standpoint is a convenient form of abstraction that, although heuristically useful in empirical investigation, does not accurately reflect the aspects of reality it enables us to study. The vitalists are right in asserting that the organism is not a machine, but the mechanists are correct in showing that it can be profitably studied as one. It is in this partial reconciliation of the two positions that Woodger's organicism lies.

The next antithesis Woodger examines, structure versus function, refers to the age-old question of the primacy of structure or function: does structure determine function, or is it function that determines structure? Biological thought has been harassed by this antithesis throughout its history (see Russell, 1916; Appel, 1987; Gould, 2002, pp. 251–341).[9] Woodger argues that this antithesis results from the artificial separation of space and time. As the new physics had shown, space and time are not independent from one another but are abstractions from a four-dimensional continuum from which they derive. Woodger reasons that an organism should not be conceived as a concrete object with a particular temporal activity, but as a spatiotemporal event that is both spatially *and* temporally extended and differentiated.[10] Anatomy and

[8] A similar analysis of the concept of mechanism and the expression 'mechanistic explanation' in the context of contemporary philosophy of biology is attempted in Nicholson, 2012.

[9] In his influential book, *The Cell in Development and Inheritance*, the cytologist E. B. Wilson writes: "Whether structure or function is the primary determining factor in vital phenomena is a question that has been a subject of debate for many generations of biological philosophers […] Thus has arisen a dilemma which belongs to the fundamental philosophy of biology and may here be left aside as practically insoluble" (Wilson, 1925, p. 670).

[10] This organicist view of living systems has been experiencing somewhat of a revival of late in the philosophy of biology, particularly in recent work by John Dupré (2011). See also Nicholson, 2010a.

physiology study the organism through different modes of abstraction. The former abstracts the organism from time, studying it as a desiccated, pickled, or fixed cross-section whereas the latter centres its attention on functional activity, relegating structure to a subordinate role. Neither of them confronts the organism as it exists in nature. To forget that we are dealing with abstractions in our anatomical and physiological investigations is to fall into the Whiteheadian fallacy of 'misplaced concreteness' (Whitehead, 1925, p. 51). Through his analyses of the concepts of function and structure, Woodger is able to resolve the antithesis by regarding them as interdependent and interdefining aspects of the four-dimensional process that is the organism: "What is required is an enlargement of our concept of 'structure' so as to include and recognize that in the living organism it is not merely a question of spatial structure with an 'activity' as something over against it, but that the concrete organism is a spatio-*temporal* structure and that this spatio-temporal structure *is* the activity itself" (Woodger, 1929a, p. 330).[11]

As to the antithesis of organism versus environment, Woodger points out that it is empirically impossible for there to be an organism without an environment, so it makes no sense to consider one in isolation from the other. The two are inextricably intertwined as they influence and determine one another. To an important extent, "[t]he characters of the organism are really characters of the organism *and* its environment" (*ibid.*, p. 346).[12] For this reason, the organism-environment complex constitutes the basic phenomenon that the biologist is confronted with in his research.

The antithesis of preformation versus epigenesis gave Woodger a lot of trouble, as reflected by the long (almost one hundred pages) and convoluted discussion of it in *Biological Principles*. This antithesis concerns the nature of the developmental process, with preformation conceiving it primarily as the result of a progressive increase in scale,

[11] After *Biological Principles*, other organicists proposed to reconcile structure and function along similar lines. J. S. Haldane (1931, p. 22), for instance, remarks that "[s]tructure and functional relation to environment cannot be separated in the serious scientific study of life, since structure expresses the maintenance of function, and function expresses the maintenance of structure". For his part, Ludwig von Bertalanffy (1941, p. 251) argues that "[t]he old contrast between 'structure' and 'function' is to be reduced to the relative speed of processes within the organism. Structures are extended, slow processes; functions are transitory, rapid processes".

[12] This seems to correspond to the outlook which has come to characterize much of the work of Richard Lewontin (1983, 1996, 2000), as well as that of the many philosophers of biology he has deeply influenced.

and epigenesis viewing it primarily as the result of a progressive increase in complexity. Woodger defines development as the serial change in the mode of characterization of an organism in the course of its history, or more accurately, as the serial changes exhibited by the mode of characterization of the spatiotemporal event that is knowable as an organism. These changes are dependent on environmental conditions, and on immanent factors in the organism itself. Woodger begins by asking what develops in development: what is it that persists and what is it that changes? His answer is that what persists is the cell-type of organization, which undergoes both spatial and temporal 'repetitions', and what changes is the mode of organization of the event that constitutes the organism as a whole – from its inception as a fertilized egg to when it becomes a differentiated multicellular adult.

In Woodger's view, the key to resolving the antithesis of preformation versus epigenesis lies in elucidating the relation between embryology and genetics. Embryology examines the differences in successive temporal slices of a given organism as its becomes more differentiated, whereas genetics deals with differences in the characterization of parts of an organism as compared to the corresponding parts in other organisms. Genetics teaches us that every cell, in virtue of its organization and of being part of a particular species, has a certain 'immanent endowment', which consists of 'Mendelian factors'. But what role do these factors play in development? Geneticists at the time, such as William Bateson, T. H. Morgan, W. E. Castle, were keen to draw an analogy between the role played by atoms in chemistry and the role played by genes in biology (see Nicholson, 2010b). Woodger rejects this comparison because cells are not made of genes in the way that molecules are made of atoms: "How can we speak of genes *making* cells? Cells are never *made*, they merely persist by division and elaboration […] Genes are concerned with characters not with 'making' cells" (Woodger, 1929a, p. 410). The role of genetic factors lies in determining, in conjunction with the environment, the *character* of the parts of an organism (as opposed to the parts themselves), and it is to this extent – and to this extent only – that development is preformed. At the same time, the genuine increase in the complexity of the organism's organization means that development is also an epigenetic process. Ultimately, Woodger felt he had only scratched the surface of the problem, viewing this antithesis as an expression of more general metaphysical conundra: "we are brought to the conclusion that this puzzle of preformation and epigenesis is but an aspect of the much wider question of the relation

between the events of nature which pass and the persistences of nature, and between what is actual and what is possible and how possibilities become actual'' (*ibid.*, pp. 423–424).

In his treatment of the antithesis of causation versus teleology, Woodger sets out to explain why it is that despite the fact that the concept of teleology tends to be considered unscientific, biologists of all creeds and inclinations cannot refrain from appealing to teleological language in their descriptions and explanations of organismic phenomena. Why are teleological modes of expression (e.g., 'in order to', 'aim', 'good', 'end'), so difficult to avoid when dealing with organisms? According to Woodger, teleology is viewed with suspicion because it is often used anthropomorphically to describe animal behaviours in analogy with conscious human intentions. Such teleological locutions ought to be banished from biology. But teleology is also used non-anthropomorphically in the description of physiological processes, and similarly when the workings of organisms are compared to the workings of machines. Machines may be said to be purposive in that they are means to the ends of their human designers and users. Are organisms purposive in the same way? Woodger argues that organisms exhibit a teleology of a different kind; one that is internal rather than external. ''A machine is *made* to realize some conscious human purpose. Its parts work together to secure *that* purpose, not to secure its own persistence. An organism *is* a mode of persistence'' (*ibid.*, p. 436). An organism is an organization which acts solely for the purpose of ensuring its continued existence. Unlike a machine, an organism acts on its own behalf. It is in the context of describing such physiological activities that teleological expressions are required in biology.[13] With regards to causation, Woodger denounces the fact that while teleology is deemed unscientific, causation is generally perceived by biologists as being entirely respectable, not realizing that the latter notion is as beleaguered with philosophical problems as the former. Woodger's conclusion is that both causation and teleology are necessary notions for biology. The key to resolving the antithesis resides in making their meanings precise so as to free them from anthropomorphic connotations.

The last antithesis that Woodger discusses is that of body versus mind. He suggests that this conflict stems from the difficulty of placing

[13] Woodger's analysis of organismic teleology anticipates the way in which this problem has been addressed in the recent philosophy of biology literature (cf. Kauffman, 2000; McLaughlin, 2001; Weber and Varela, 2002; Thompson, 2007; Mossio et al., 2009; Toepfer, 2012; Nicholson, In press).

on the same epistemological footing the 'public' facts of neurological science and the 'private' facts of psychological experience. He argues that it is as precarious to make inferences about psychological states from observations of the nervous system as it is to make inferences about the nervous system from knowledge gained by reflection. The key point is that discussions of matter and mind are 'extra-biological' in nature. Biologists should remember that these belong to the domain of metaphysical speculation, not of scientific investigation. The biologist's ''hands are full enough with difficulties and unresolved muddles in his own sphere, and if he devotes his energies to his own affairs he will have little time left for amateur psychology and metaphysics. All he needs to know about these topics is just enough to save him from introducing them into books which profess to confine themselves to biology'' (*ibid.*, pp. 470–471).

The book ends with a chapter on 'The Future of Biology' in which Woodger argues that every branch of natural science has three equally important aspects: the investigatory (concerned with empirical discoveries), the hypothetical (concerned with theories and explanations), and the critical (concerned with epistemological issues). In the case of biology, these have not developed in parallel. The investigatory aspect is the most advanced, the hypothetical aspect has been overshadowed by notions borrowed from other sciences, and the critical aspect remains completely underdeveloped. This is why, in Woodger's mind, biology remains fragmented and disunified: ''so long as [biologists] are divided into conflicting sects which conduct their debates like rival missionaries – appealing to 'subjective factors' by means of *ad hominem* arguments, rather than reason, so long will biology continue to be a 'science of antitheses', and effort which might be helpfully employed exploring *other* possibilities will have to be devoted to removing needless artificially created difficulties'' (*ibid.*, pp. 481–482).

Overall, *Biological Principles* is a towering achievement – remarkable both for its breadth and for its depth. Through its detailed discussion of the views of the major biological authorities of the time, it offers valuable insight into the state of biological thought in the first third of the twentieth century. More importantly, it presents one of the first systematic treatments of the philosophical problems of modern biology in the English language. It is also notable in that it arguably succeeds in resolving several longstanding conceptual puzzles in biology by applying a new philosophy of nature. *Biological Principles* is simultaneously a pioneering contribution to theoretical biology and to philosophy of biology.

Reviewers praised the book's "great erudition" (A. K., 1930, p. 911) and its capacity "to stimulate the biologist to thought" (Needham, 1930, p. 226), though they also complained about its length (Russell, 1930, p. 126) and the inclusion of grammatical errors (Sheldon, 1931, p. 384). The anonymous reviewer of *Nature* noted that "no biologist who really wishes to face fundamental problems should omit to read it" (Anon., 1929, p. 909).

While *Biological Principles* was in press, Woodger read a paper before the Aristotelian Society (Woodger, 1929b) in which he summarized some of the book's major claims. Months earlier he had also published an essay called 'Science and Metaphysics in Biology' (Woodger, 1928), which caught the attention of American biologist Raymond Pearl, who as a result invited Woodger to publish in his journal, *The Quarterly Review of Biology*.[14] Woodger accepted the invitation and wrote a triad of papers entitled 'The "Concept of Organism" and the Relation Between Embryology and Genetics' (Woodger, 1930a, b, 1931a), where he took the opportunity to "develop what was said in *Biological Principles* about organization, and also to improve upon Chap. IX [preformation versus epigenesis] […] which I felt was very muddled" (Woodger to Harding, 1st April 1930).

In these three papers Woodger argues that the concept of organism has not been adopted by the majority of biologists in the past owing to the heuristic success of the mechanistic conception of living systems as machines. Some of the inadequacies of this view had been pointed out by Driesch and J. S. Haldane, but neither had developed a satisfactory conception of the organism *as* an organism (i.e., as a self-organized system) because they did not pay sufficient attention to the nature of the living organization. Woodger goes on to elaborate his Whiteheadian understanding of the organism as a spatiotemporal event laid out in *Biological Principles* in order to provide a systematic theory of organismic organization.[15] As to the antithesis of preformation versus epigenesis and the relation between embryology and genetics, Woodger develops his critique of genetic preformationism, denying the reduction

[14] Pearl was particularly keen to commission Woodger for his journal because he shared Woodger's conviction that what biology needed was a greater development of its 'critical' aspect. In a letter to Woodger, he regretted that most biologists "are mainly working automatically and are doing very little thinking of a philosophical or any other kind" (Pearl to Woodger, 16th November 1928).

[15] Woodger has subsequently come to be recognized as one of the first modern biologists to attempt to come to theoretical terms with the organization of living systems (see Dunlop, 1944, p. 171; Zylstra, 1992, p. 111; Trewavas, 2006, p. 2422; Stebbing, 2011, p. 209).

of a cell to its genes (Woodger, 1930a, p. 13) and rejecting August Weismann's conception of chromatin as the central directing agency of the organism (*ibid.*, p. 18).[16] Woodger also criticizes attempts to dissociate the determining influence of both hereditary and environmental factors in developmental explanations: "Is there any sense in asking how many centimeters of a man's nose are "due to heredity" and how many are "due to environment"? Is it not like asking how much of the volume of a gas is "due to" pressure and how much "due to" temperature?" (Woodger, 1931a, p. 204).[17] Ultimately, however, Woodger could not hide his disappointment with the primitive state of theoretical biology, which, in his mind, prevented the clear elucidation of the relation between embryology and genetics. He concluded by caustically remarking that discussing "embryological topics and their relation to genetics with our current conceptual and linguistic apparatus is like performing a modern surgical operation with a pair of nail scissors and a potato peeler" (*ibid.*, p. 207).

Formal Period (1931–1978)

Woodger's frustration with the vagueness and ambiguity of scientific terminology, which he exposed in his analyses of the biological antitheses, continued to grow to the point where he no longer believed that natural language could supply an adequate theoretical framework for biology. In *Biological Principles* he had complained that a "great number of the words used in biological books are nothing more than ink marks without a clear meaning, and it is because no one considers it necessary, either to assign a clear meaning to such terms, or to abandon them altogether, that much biological controversy is apt to be so fruitless" (Woodger, 1929a, p. 482). In the process of writing the 'organism papers', Woodger concluded that what was needed to make

[16] As Woodger (1930a, p. 18) puts it, in Weismann's conceptual scheme, "Chromatin takes the place of Descartes' God as the 'controlling' mechanic"'. This view lives on in Richard Dawkins' (1976) theory of the 'selfish gene', as well as in the reductionist appeals to a 'genetic program' that 'directs', 'controls', or 'instructs' the developmental process. These conceptions have been the subject of intense criticism by many contemporary philosophers of biology (e.g., Webster and Goodwin, 1982; Nijhout, 1990; Moss, 1992; Keller, 2000; Morange, 2001; Robert, 2004). Woodger's critique of preformationism prefigures many of these subsequent criticisms.

[17] Woodger's rejection of the primacy of genes in character determination is strongly reminiscent of the claims recently put forward by proponents of 'Developmental Systems Theory' (see Oyama, 2000; Oyama et al., 2001).

biological ideas *clear* and *precise* was a rigorous means of expressing and relating propositions of the kind afforded by symbolic logic. To this end, despite the fact that he lacked any formal training in logic or mathematics, Woodger set to work to master the three volumes of Whitehead and Russell's *Principia Mathematica* (1910, 1912, 1913) in the hope that it could be adapted for his purpose. This marks the beginning of the final period in Woodger's career, characterized by a concern with the formal articulation of biological theories and concepts with the goal of strengthening the foundations of biological knowledge.

The preliminary results of Woodger's efforts to apply logical analysis to biological problems can already be found in the second and third 'organism papers', where he developed a notion of 'hierarchical order' using some of the concepts and notation of the *Principia Mathematica*. He followed this with a paper that attempted to formulate the axioms responsible for the deterministic and atomistic character of scientific theories, which was read before the Aristotelian Society (Woodger, 1932). During this time, Woodger came into contact with Karl Popper, who introduced him to Alfred Tarski. The two became close friends, and Woodger came to regard Tarski as his logic mentor. In 1935, he arranged a visit to Poland to discuss his ideas with Tarski and other members of the Lvov-Warsaw School of Logic.

Woodger published the fruits of his logical labours in his third book, *The Axiomatic Method in Biology* (1937). This book, which Woodger himself described as an 'experiment' to emphasize its tentative character, employs the tools of symbolic logic and pure mathematics to reconstruct the language of biology. As he explains in the preface:

> [T]he aim of this book […] is to provide an exact and perfectly controllable *language* by means of which biological knowledge may be *ordered* […] Because if we have a perfect language we need not dispute, we need only calculate and experiment. From the standpoint of recent investigations into the nature of the exact sciences [e.g., *Principia Mathematica*] we can see that the application of their methods to biology […] consists in creating a scientifically perfect language in which calculation is possible – one in which we cannot deceive ourselves or others because nothing is concealed and only scientific ends are served (Woodger, 1937, vii–viii).

In the following pages Woodger develops such a 'bio-logical calculus' in the form of an axiom-system constructed on the basis of ten primitive (i.e. undefined) 'biological constants': **P** (*part of*); **T** (*before in time*); **org**

(*organized unity*); **U** (*reproduced by cell division or cell fusion*); **cell** (*cell*); **wh** (*whole organism*); **m** (*male gamete*); **f** (*female gamete*); **env** (*environment of*); and **genet** (*genetic property*). Woodger then uses these constants to redefine in precise terms a number of notions from genetics, embryology, and taxonomy, and employs the logical apparatus of the *Principia Mathematica* to derive some of their basic relations.

The reception of *The Axiomatic Method in Biology* was mixed. Some reviewers commended it for being "of sufficient scientific importance to be of interest to both the logician and the biologist" (Fitch, 1938, p. 43) and applauded Woodger for his capacity "to speak as a logician to men of natural science and at the same time to speak as a man of natural science to logicians" (Strauss, 1940, p. 372). Others went further, dramatically proclaiming that "*The Axiomatic Method in Biology* stands as the inceptor of a new biological science, and can in this sense be compared to Harvey's *De Motu Cordis* or Wolff's *Theoria Generationis*" (Rosinger, 1938, p. 273). However, although Woodger asserted in the preface of the book that he presupposed no prior knowledge of symbolic logic – "[t]ime and patience, pencil and paper, are all that the reader need provide for himself" (Woodger, 1937, ix) – the same reviewer remarked (correctly, it would seem) that "this book will tax the resources of a trained logician, and will certainly terrify biologists to the extent that most of them will abstain entirely from reading it" (Rosinger, 1938, p. 273). J. B. S. Haldane (1938) wrote a rather disparaging review for *Nature*, where he criticized some of the biological assumptions Woodger had made in constructing his biological axiom-system. An altogether more balanced assessment was provided by C. H. Waddington (1938, p. 192), who asserted that *The Axiomatic Method in Biology* "has clearly the status of a forerunner, but its ultimate importance will depend on whether the method which it introduces can be developed into a valuable part of biology".

In 1938 Woodger received a scholarship from the Rockefeller Foundation to travel to the United States, where he interacted with logicians at the University of Chicago and psychologists at Yale University (Cain, 2000, p. 539). In the latter institution he worked closely with Clark Hull on the formalization of his psychological claims, publishing some of this work later that year (Woodger, 1938). Hull's book, *Mathematico-Deductive Theory of Rote Learning* (Hull et al., 1940), was heavily influenced by *The Axiomatic Method in Biology*, and although it was written with Woodger's assistance, Woodger asked his

name to be removed from the published version after not being able to agree with Hull on the nature of the axiom-system.[18]

Upon returning to England, Woodger wrote a short monograph entitled *The Technique of Theory Construction* (1939), which presented a simplified version of the bio-logical calculus expounded in *The Axiomatic Method in Biology*. In *The Technique of Theory Construction* he focuses on the general methodology of formalizing scientific theories, illustrating it with the construction of a 'specimen theory' belonging to the axiom-system he had developed in *The Axiomatic Method in Biology*. The book was published as a volume in Otto Neurath's *Encyclopedia of Unified Science*, and indeed Woodger emphasizes that his technique can contribute to the unification of the sciences because it reveals the structural similarities of different scientific theories. For Woodger, formalization provides a common means of communication that enables the comparison of different theories.

> Only when two theories are formalized […] can we properly compare them, because only then are the essentials upon which a comparison rests laid bare – only then do they possess a definite structure. The neglect of this precaution is chiefly responsible for the misunderstanding, confusion, and barrenness which frequently infect methodological [i.e. philosophical] discussions (Woodger, 1939, p. 71).

Reviews of *The Technique of Theory Construction* were published in a number of journals (E. N., 1940; Mills, 1940; Strauss, 1940; Wohlstetter, 1940; Braithwaite, 1941), and although many lauded its demonstration of the power of logical analysis in the systematization of scientific knowledge, the general impression was again that such efforts demanded a familiarity with symbolic logic that very few biologists had or were willing to acquire.

With the outbreak of the Second World War, Woodger devoted most of his time to dealing with the problems of teaching under the difficult

[18] Woodger appears to have become increasingly disappointed with Hull, who (in Woodger's view) seemed anxious to employ the notation of the *Principia Mathematica* without wanting to devote any attention to ensuring that the formalized theory satisfied the requirements of clarity and precision of an axiom-system. In a heated letter to Hull, Woodger bitterly declared that "as far as your theory is concerned my visit to New Haven has been a complete failure because I have not succeeded in teaching you *anything*". Woodger emphasized that "the 'symbolic' aspect of symbolic logic *is the least important thing about it*. Consequently the contribution which logic has to make to your theory does not consist simply in translating it into a language in which 'symbols' appear without 'words'. What I wanted to do, […] and what I have failed in persuading you to do, is to construct your theory […] so that it will be a genuine postulate system" (Woodger to Hull, 21st January 1939).

conditions involved in the temporary relocation of his Department to Leeds (Floyd and Harris, 1964, p. 5), but he nevertheless managed to continue his research. In 1940 he published two articles in which he attempted to explain once more, in even simpler terms than in *The Technique of Theory Construction*, the potential rewards of formalization in theoretical biology (Woodger, 1940a, b). Towards the end of the war, Woodger contributed to a *Festschrift* in honour of D'Arcy Thompson. His essay, entitled 'On Biological Transformations' (1945), has become somewhat of a classic in comparative morphology, as it proposed a new theoretical understanding of homology. Woodger begins by noting that comparing two systems usually involves setting up a one-to-one correspondence between their respective component parts, and stating how the corresponding parts resemble or differ from each other with respect to specific properties. In morphological comparisons, he contends that the correspondence should be established on the basis of *relations* holding among the anatomical parts of the compared systems, rather than on intrinsic properties of the parts themselves. Using a set-theoretical framework, Woodger provides a means of codifying topological information about anatomical parts using only three grouping rules, or designations: 'being immediately distal to', 'being proximal to', and 'being articulated with'. In this way, two parts from different organisms (or from different time-slices of the same organism) are in a state of correspondence if they share the same designations – that is, if they occupy the same relational position with respect to other parts in the system. Isomorphic systems of relations instantiate a '*Bauplan*'. As Woodger explains, Bauplans reflect uniformity of organization in different organisms. They are causally rooted in ontogeny, and they are evidence of common descent.[19]

In 1947, the University of London recognized Woodger's accomplishments by conferring on him the title of Professor of Biology. Two years later, he was invited to deliver the Tarner Lectures on the Philosophy of the Sciences at Trinity College, Cambridge, whose

[19] Woodger, 1945 is responsible for introducing the term 'Bauplan' in Anglophone biology. Brian Hall (1999, p. 94) suggests that Woodger's 'Bauplan' became the preferred term over 'Type' or 'Archetype' because it lacked the metaphysical connotations given to those terms by pre-Darwinian idealistic morphologists (but see Hull, 1988, p. 110). Today, 'Bauplan' is the standard technical term used to "express the idea of a homologous structural [i.e., organizational] plan underlying evolutionary transformations within a taxonomic group" (Raff, 1996, p. 196). A well-known example of a Bauplan is the pentadactyl limb skeleton in vertebrates, which Woodger discusses in his essay. For a more detailed account of the influence of Woodger, 1945, see Hall, 1999, Chap. 6, and Rieppel, 2006.

philosophers – Whitehead, Russell, and Broad – greatly influenced his earlier work.[20] These lectures were published as a book, Woodger's fifth, entitled *Biology and Language: An Introduction to the Methodology of the Biological Sciences including Medicine* (1952a). Woodger begins by recovering the investigatory-hypothetical-critical distinction of scientific activities he introduced in *Biological Principles*, stressing the importance of unambiguous terminology as a condition for the development of the critical aspect. Competent use of language, Woodger writes, is as indispensable to biology as competent use of microscopes. Conversely, understanding "the pitfalls to which a too naïve use of language exposes us is as necessary as some understanding of the artifacts which accompany the use of microscopical techniques" (Woodger, 1952a, p. 6; see also Woodger, 1948 for an elaboration of this analogy). The obvious departure from *Biological Principles* lies in the conviction that characterized his formal turn in the 1930s, namely that conceptual analysis is not enough. What is needed is a metalanguage in which to discuss the language of biology. It is in this context that Woodger reiterates his appeal for using the insights of the 'Boole-Frege' movement in the analysis of biological knowledge.

Biology and Language is divided into three parts. In Part I, Woodger outlines a logical scheme for ordering biological propositions into theories. Biological propositions are of two kinds: observation-records and theoretical statements. Theoretical statements that are generalizations of observation-records are termed 'zero-level hypotheses'; theoretical statements explanatory of those are termed 'first-level hypotheses'; statements explaining these in turn are termed 'second-level hypotheses', and so on. A theory is characterized as a hierarchical system of hypotheses, each of which is derivable from one or more of the others.[21] In Part II, Woodger supplements his ordering scheme with the tools of symbolic logic to sketch a metalanguage for genetics which he then uses to tackle a number of theoretical disputes in that field. For example, with regards to the debate over the developmental influence of genetic versus non-genetic factors, Woodger suggests to replace vague natural

[20] Before Woodger, previous Tarner Lecturers had included not only Whitehead, Russell, and Broad, but also G. E. Moore, F. R. Tennant, A. D. Richie, Arthur Eddington, C. A. Mace, and R. B. Braithwaite.

[21] In an appendix to Part I, Woodger uses this ordering scheme to exhibit the logical structure of William Harvey's theory of blood circulation as expounded in *De Motu Cordis*. The different types of propositions are classified as zero-level, first level, and so forth in a fashion that enables the theory to be clearly and elegantly presented. One reviewer remarked that "[t]the book is worth having for this [appendix] alone" (Wisdom, 1954, p. 340).

language terms like 'inheritance' and 'acquired characters' in favour of a formal classification of phenotypes in relation to their relative sensitivity to environmental influences.[22] Finally, in Part III, Woodger offers some tentative suggestions for the formal treatment of linguistic puzzles in neurology, where he provides a logical characterization of the concept of reflex, and in medical psychology.

Reactions to *Biology and Language* were considerably more positive than those to *The Axiomatic Method in Biology* and *The Technique of Theory Construction*. According to one reviewer, this was due to the fact that Woodger had "greatly simplified and clarified his procedures", though he also remarked that "[r]eaders too lazy to decipher a formula will still be excluded", before concluding that *Biology and Language* is "clearer and more persuasive than any of Woodger's previous writings" (Miller, 1955, p. 157). Others went further, describing the book as "one of the most important on the philosophy of biology to have appeared in many years" (Martin, 1954, p. 333). Philosopher John Wisdom (1954, p. 339) was "doubtful about the intrinsic relevance of Woodger's symbolization" while at the same time maintaining that "every philosopher of science will find a great deal to attract him". As for the biologists, John Gregg (1953, p. 279) praised it in *The Quarterly Review of Biology*, expressing the hope that "biologists will have the imagination and industry to benefit from Woodger's enormous creative efforts".

Throughout the 1950s, Woodger wrote a number of papers in which he developed in greater detail the arguments and methods advanced in *Biology and Language* (Woodger, 1951, 1952b, 1953, 1956b, 1958, 1959).[23] In 1956 he published his last book, *Physics, Psychology and Medicine: A Methodological Essay*, based on three lectures to medical students at the Middlesex Hospital Medical School. In this short treatise, Woodger returns to a topic he first broached in *Elementary Morphology and Physiology* 32 years earlier, namely the need for reform of the medical curriculum (on this subject, see also Woodger, 1955). Woodger argues that in the pre-clinical training of most medical students there is an overwhelming preoccupation with the concepts and

[22] In an appendix to Part II, Woodger addresses conceptual problems in evolutionary theory, specifically in taxonomy, where he appeals to set theory to reconcile the gradualness in evolutionary changes with the demand that passage from one taxonomic category to another must take place in a single generation. This was an attempt to solve what Peter Medawar subsequently branded 'Woodger's Paradox' in his philosophical dictionary of biology (see Medawar and Medawar, 1983, pp. 281–282).

[23] Woodger's article 'What Do We Mean By 'Inborn'?' (1953) elicited a response from J. B. S. Haldane (1955), who again criticized Woodger's formalizations. Woodger responded to Haldane's criticisms in a subsequent paper (Woodger, 1956b).

methods of physics, despite the fact that two fifths of patients that physicians encounter in their practices exhibit illnesses that are not classified as physical, but as mental. Woodger emphasizes the need to incorporate psychology and other areas of the social sciences into the medical curriculum, lest doctors forget that they are dealing with persons embedded in a social milieu, not with machines. As Woodger puts it, medical students are taught to "regard human beings as complicated pieces of machinery whose parts can go wrong in various ways. But being miserable is not a recognized way in which a machine can go wrong. Its correction seems to call for a different approach" (Woodger, 1956a, p. 2). *Physics, Psychology and Medicine* also discusses more briefly a wide variety of philosophical problems, including the status of explanatory hypotheses, the role of abstraction in science, the subject-object distinction, and the linguistic apparatus of psychological science. It even features a few whimsical – though not altogether frivolous – philosophical suggestions, such as the idea that we ought to be most sceptical of scientific theories when they are old, not when they are new. The book received mixed reviews in several philosophical journals (e.g., Nagel, 1957; Hochberg, 1957; Kapp, 1957; Muncie, 1957; Uytman, 1958; Cohen, 1958).

In 1959, Woodger retired from teaching, becoming Emeritus Professor. He attended numerous conferences and was offered visiting Professorships at three American Universities, but he declined all of them, preferring instead to continue his research from his country house in Epsom Downs. From there he wrote several more papers (Woodger, 1960, 1961, 1962, 1965, 1968). Figure 1 shows Woodger late in his life. His final years were devoted to revising and augmenting *Biology and Language* for the occasion of a Spanish translation of the book, which was eventually published as *Biología y Lenguaje* (1978). Woodger died in 1981.

Refuting the Claims of the Current Consensus

Having examined Woodger's entire oeuvre in detail, it is no longer necessary to rely on highly selective readings of his formal works to assess his place in the history of twentieth-century philosophy of biology. We are now in a position to reconsider the current consensus on Woodger's legacy, and show how problematic it actually is. Earlier in the paper we outlined the four major criticisms that contemporary philosophers of biology have made of Woodger. We will now demonstrate, using the insights gained in the previous section, as well as further textual evidence from Woodger's corpus, that every one of these claims is misrepresentative, if not entirely false.

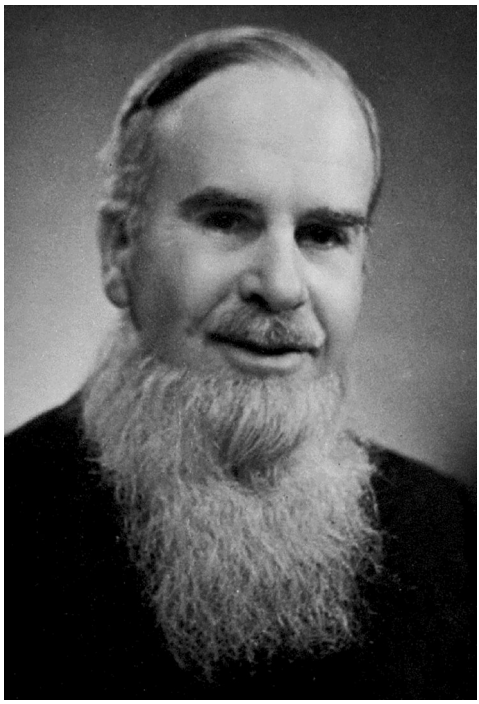

Figure 1. Portrait of J. H. Woodger in his later years (John R. Gregg Papers)

Woodger and Logical Empiricism

The first claim of the current consensus is that Woodger was an ardent exponent of logical empiricism. Because the emergence of philosophy of biology as an academic discipline is often connected to the demise of logical empiricism (see, e.g., the exchange between Sober, Rosenberg, and Callebaut in Callebaut, 1993, pp. 73–74), associating Woodger with this movement has provided contemporary authors with a convenient excuse for ignoring his work. Although Woodger did have long-lasting ties with some of the most prominent logical empiricists, his own relation to the movement is complex and rather difficult to characterize. What is clear, at any rate, is that he was not the kind of mulish logical empiricist that recent commentators have made him out to be. In fact, many of his philosophical views are directly antithetical to the ones commonly associated with logical empiricism.[24]

[24] We recognize that logical empiricism was a very heterogeneous philosophical movement, and that there are probably no doctrines which were upheld by all of its members. Here we focus on the familiar, overly simplistic view of the movement, as this is precisely the view that recent commentators have attributed to Woodger in order to discredit his work.

Consider, for instance, Woodger's stance on metaphysics. Logical empiricism is often characterized by the conviction that metaphysical statements are meaningless, and should be banished from philosophical and scientific discourse. It is evident that Woodger did not hold such a view. As we saw in the previous section, Woodger's main motivation for writing *Biological Principles* was to use Whitehead's new metaphysics of nature to develop an organicist philosophy of biology. Woodger explicitly addresses the place of metaphysics and its relation to science in the book, remarking that "[t]here seems to be a tradition among men of science that there is something disreputable about metaphysics – something which makes it a topic which it is desirable to avoid" (Woodger, 1929a, p. 23). He observes that among scientific writers a claim is often classified as 'metaphysical' when the author finds it unpalatable, and it is termed 'science' when it is deemed acceptable.[25] Although Woodger does not specify a criterion by which to demarcate scientific statements from metaphysical ones, many logical empiricists did so, famously proclaiming that whereas the former are actually or potentially verifiable, the latter are incapable of being observationally verified, even in principle. Woodger rejects such an analysis: "The term metaphysics or metaphysical is applied by some authors to any theory which attempts to go beyond the immediate data of sense […] The expression, 'immediate data of sense', is ambiguous, but, in the sense in which [Mach and Pearson] understand it, most if not all our scientific theories as commonly understood would be metaphysical" (*ibid.*, p. 29). Science, according to Woodger, cannot rid itself of metaphysics, nor should it attempt to do so, for the two are "mutually complementary" (*ibid.*, p. 24). Commenting on physiologists who seek to purge metaphysics from their discipline, he declares that those who suppose themselves to be above metaphysics are actually "only a very little above it – being up to the neck in it" (*ibid.*, p. 246). For Woodger, metaphysical statements only become problematic in scientific discourse when "they are entertained unconsciously, or their metaphysical character is not understood" (*ibid.*, p. 27).

Even though Woodger's views during his critical period bear no resemblance to the ones contemporary authors frequently ascribe to logical empiricism, the question remains as to whether he developed affinities with the movement following his formal turn. After all, it is during this part of his

[25] Woodger makes this point even more forcefully in *Physics, Psychology, and Medicine* (1956a, p. 58): "If you do not like some particular doctrine, and you are unkind enough to wish to embarrass the person who is defending it, it suffices to declare with emphasis that it is metaphysics. This has the double advantage of being obscure and derogatory. It is obscure because it is difficult to say what it means and for that reason it is difficult to rebut. It is denigratory because it is widely believed that, in some obscure way, metaphysics is disreputable".

career that he attempted to axiomatize biological theories, and this is the work that has given many commentators the impression that he was a logical empiricist. Cain (2000, p. 537), for example, explicitly states that Woodger's formal work was motivated by his commitment to logical empiricism: "To be sure, Woodger was *the* scholar in the nineteen thirties and forties working to apply logical positivism to the biological sciences. In print, this began with his 1929 *Biological Principles* […] but found a more sophisticated, formal expression later". In light of the preceding discussion, it seems clear that, contra Cain, there are no grounds whatsoever for claiming that *Biological Principles* was a logical empiricist treatise.[26] But can the same thing be said of Woodger's formal works, namely *The Axiomatic Method in Biology*, *The Technique of Theory Construction*, and *Biology and Language*?

Woodger drew on symbolic logic in these books in order to construct a metalanguage for biology that would help organize biological statements and uncover connections between different biological theories. This is strongly reminiscent of the goals Neurath set for his 'unity of science' program, which concerned "building up a uniform scientific language with a uniform terminology" for the purposes of "connect[ing] statements and terms of different disciplines for the deduction of individual predictions" (Neurath, 1983, p. 133). We suggest that it was by virtue of his affinities with Neurath's unity of science program that Woodger chose to publish his *Technique of Theory Construction* as part of the latter's *Encyclopedia of Unified Science*.[27]

We should emphasize, however, that Woodger's interest in the unity of science was not fuelled by a desire to reduce biology to physics. It is rather unfortunate that in the second half of the twentieth century the unity of science thesis came to be interpreted as a form of inter-theoretical reductionism – meaning that the unification of the sciences is to be achieved by explaining all theories of the so-called 'special sciences' in terms of the fundamental theories of physics – because this constitutes a radical departure from Neurath's original conception.[28] For

[26] It should be remembered as well that Woodger devotes much of Part I of *Biological Principles* to criticizing the philosophy of Ernst Mach, who was the intellectual godfather of logical empiricism, and even gave the organization that coordinated the activities of the Vienna Circle its name ('Ernst Mach Society').

[27] It is also worth mentioning that Woodger served on the advisory board of the *Encyclopedia*. He even entered, and won, an essay contest on the unity of science organized by Neurath (Reisch, 2005, p. 302).

[28] For influential defenses of the reductionist reinterpretation of the unity of science, see Oppenheim and Putnam, 1958; Nagel, 1961; and Hempel, 1966. For influential critiques, see Fodor, 1974; Dupré, 1993; and Rosenberg, 1994. Neurath's conception of the unity of science has been recently defended in Potochnik, 2011.

Neurath, unification does not imply reduction. Rather, the unity of science involves the coordination of the different sciences, none of which is taken to have privileged epistemic status. This was also Woodger's position. Consequently, it is inappropriate to suggest that because Woodger supported the unity of science program (as Neurath conceived it) he also sought to refashion biology to make it more like physics. In fact, the opposite is true.[29] Woodger remained a staunch defender of the autonomy of biology throughout his life, and although he considered physics and chemistry to be at a more advanced stage of development than biology,[30] he repeatedly argued that biologists had nothing to gain from slavishly adopting the models and theories of the physical sciences. Instead, scientists in every discipline should be "free to try any hypothesis which their data and their bright ideas may suggest, regardless of what other sciences may be doing" (Woodger, 1956a, p. 11).

What is one, then, to make of Woodger's appeal to formalization? If he did not intend to remodel the life sciences along the lines of the physical sciences, why did he try to axiomatize biological theories? As we have already explained, Woodger turned to symbolic logic in order to construct a precise, controllable language. His goal was to limit the use of natural languages because "the richness of their vocabularies and the arbitrariness of their syntactical rules militate against their sustainability for scientific purposes" (Woodger, 1939, p. 2). Woodger came to believe that formalization could help biology achieve the level of theoretical sophistication of the physical sciences, but he never doubted that such formalizations would need to cater for the specific epistemic requirements of biology. Thus, in the first chapter of the *Axiomatic Method in Biology* he remarks that "[i]n addition to making what use we can of existing abstract systems (which all owe their origin, historically speaking, to the demands of the physical sciences) it seems to me to be desirable that we should try to construct our own systems in accordance with the requirements of biological data" (Woodger, 1937, p. 16). Similarly, in *The Technique of Theory Construction* he notes that by developing a mathematical logic for biology, we will free ourselves

[29] Pnina Abir-Am (1987, 1991) and Betty Smocovitis (1992, 1996) appear to be the only contemporary scholars to have realized this.

[30] In one of the most oft-quoted passages from *Biological Principles* (1929a, p. 483), he writes: "Charles Darwin has been called the Newton of biology, but it will be time enough to talk about the Newton of biology after our science has found its Galileo. To suppose that Darwin was the Newton of biology is to suppose that biology has already reached a degree of theoretical development comparable with that of physics in the eighteenth century, and that surely is preposterous. We only make our great men ridiculous by putting them in fancy dress in this way".

"from the accidental restrictions of traditional mathematics, i.e., the mathematics which have arisen to meet the needs of physics" (Woodger, 1939, p. 39). It is this fundamental facet of Woodger's project that has been completely lost on his critics when they assert that Woodger "attempts to treat a biological discipline on the model of a close-knit physical theory" (Smart, 1963, p. 50), that he "insisted on putting everything into rigorous deductive systems, with absolutely disastrous consequences" (Ruse, 2000, p. 478), or that he "tried to force biological theories into the logical-empiricist deductive corset" (Callebaut, 2005, p. 104). Interestingly, Woodger was perceptive enough to anticipate such criticisms:

> [I]t seems to be felt in some quarters that the deliberate use of a technique of theorizing involves (in the case of biology) "fitting the facts of life" into some rigid predetermined scheme. Nothing could be further from the truth. Far from making facts conform to a scheme (which in any case would be impossible) we deliberately construct the theoretical system in such a way that it will as faithfully represent the facts as possible (Woodger, 1939, p. 74)

Woodger clearly does not fit the familiar view of logical empiricism that so many contemporary authors have attributed to him (e.g., Roll-Hansen, 1984; Rosenberg, 1985; Thompson, 1989; Sarkar, 1996; Wolters, 1999; Cain, 2000). He had no antipathy towards metaphysics, he rejected the verificationist criterion of meaning, and he was an unwavering anti-reductionist. His interest in logic after *Biological Principles* does of course resonate with logical empiricism, but his connection to the movement – to the extent that there is one – is best understood in relation to Neurath's non-reductionist unity of science program.

Woodger and 'Real' Biological Science

The second imputation of the current consensus is that Woodger's work was completely disconnected from the biological science of his time due to his lack of interest in, and arrogant contempt of, empirical research. The first difficulty with this claim is that Woodger was a practicing biologist for the first part of his career, and he remained in close contact with biomedical science throughout his 37-year tenure at Middlesex Hospital Medical School. The charge that Woodger's work "had little connection with real science, real biological science" (Ruse, 2000, p. 478) is difficult to square with the fact that his first book, *Elementary Morphology and Physiology*, was a five-hundred page survey of the state

of the art in biology. Woodger's subsequent critical writings, while not arising directly from empirical investigation, were thoroughly informed by biological research, as illustrated by the extensive analyses of numerous biological texts in *Biological Principles*. The problems that Woodger addresses in that book (i.e., the biological antitheses) are the fundamental theoretical problems of early-twentieth century biology, and they are very much connected to 'real' biological science. As to his formal work, the key point to remember is that it does not constitute a departure from his critical work. In both cases, his aims are precisely the same. They concern the problem generated by the "contrast between the brilliant skill, ingenuity and care bestowed upon observation and experiment, and the almost complete neglect of caution in regard to the definition and use of the concepts in terms of which its results are expressed" (Woodger, 1929a, p. 3). Although this remark is taken from *Biological Principles*, it serves as an equally accurate précis of *The Axiomatic Method in Biology* and *Biology and Language*. It is, of course, true that his philosophical tools changed after *Biological Principles*, but his objectives did not. As Donna Haraway (1976, p. 131) correctly observes, Woodger's "extreme preoccupation with logical issues was rooted in a lifelong attention to maximum clarity of scientific and philosophical concepts".

The stinging accusation that Woodger "dismissed empirical issues with an indignant snort" (Hull, 1994, p. 375) severely misrepresents his attitude towards empirical biology. Hull, Ruse, and others have been keen to portray Woodger as a logician – a "mathematical magician", to use Ruse's (1984, p. 453) derisive phrase – with a brazen disdain for practicing biologists, and who thought of himself as superior on the basis of his logical expertise. However, there is no evidence to support such a disparaging characterization. Woodger became interested in logic *through* his concern with biological problems, not the other way round. In his formal works, he speaks as a biologist interested in theory, not as a logician peering into biology.[31] Strangely, Woodger's critics seem to be oblivious of this. Roll-Hansen (1984, p. 427) goes as far as to claim that Woodger rejected "a whole series of fruitful ideas from genetics, embryology, and evolution" using a priori arguments! Roll-Hansen lambasts Woodger for doubting the material basis of the gene and being sceptical of empirical research on the origins of life in *Biological Prin-*

[31] Consider his use of the pronoun 'we' in the following passage: "If we are willing to take endless trouble, and exercise unlimited patience, in order to attain precision in experiment, it seems strange that we should grudge the effort needed for a precise analysis of our logical procedure, and for the accurate expression of our results" (Woodger, 1931a, p. 206).

ciples, mistaking Woodger's criticisms (which in 1929 would not have been unreasonable) for categorical denunciations made on a priori grounds. But Woodger was not dogmatic – especially not when it came to scientific matters, which he regarded to be subject to continuous revision. In the preface to *Biological Principles*, he warns the reader: ''Never mistake the *latest* word on any topic for the *last* word. As we do not *know* what discoveries are in store for us we do not *know* what is the last word on any topic'' (Woodger, 1929a, xiv). Overall, the portrayal of Woodger as a conceited logician who was dismissive of empirical science is a baseless caricature. Woodger turned to logic because he thought it would help him solve biological problems, and in his philosophical work he was never too far from the concerns of biology.[32]

Establishing Woodger's Credentials

The third claim of the current consensus is that Woodger possessed no real scientific or philosophical credentials, and consequently his work at the intersection of these fields can only be charitably described as second-rate. In light of the body of evidence already presented against this claim, it will not be necessary to consider it in detail. It suffices to remind ourselves in relation to the question of scientific credentials that Woodger was trained as an experimental embryologist and cytologist at UCL, that his academic performance earned him prizes and scholarships, and that he conducted and published empirical research in embryology, cytology, physiology, and protozoology. It may also be recalled that he collaborated with distinguished biologists like Hill and de Beer – both fellows of the Royal Society – while he was developing the organicist philosophy of biology he laid down in *Biological Principles* (for which he was awarded a D.Sc. degree). In 1930 Pearl described Woodger ''[u]nhesitatingly, and without reservation'' as ''the biologist of the greatest promise – in the long career – of any of his generation in Great Britain known to me'' (Pearl to University of London Registrar, 1930).

With regards to the question of philosophical credentials, it is true that Woodger had no formal training in philosophy or logic. Still, he taught himself these subjects so successfully that he appears to have

[32] A rare instance of recognition in the contemporary philosophy of biology literature of the practical biological relevance of Woodger's work is provided by Jason Scott Robert (2004, pp. 15–19), who praises Woodger's *Biology and Language* for its discussion of methodological heuristics in experimental genetics.

been one of very few people in the world – logicians and mathematicians included – to master all three volumes of the *Principia Mathematica* (Woodger to Gregg, 2nd February 1973). Later, Alfred Tarski confided in Woodger to translate his papers, which were published as *Logic, Semantics, and Mathematics* (1956). Finally, one could recall that UCL offered him a personal chair in 1947 and that he was invited to deliver the Tarner Lectures on the Philosophy of the Sciences at Trinity College, Cambridge in 1949. He also declined three visiting professorships in the United States upon his retirement. Bearing all these facts in mind, we can reject this particular claim about Woodger.

The Historical Influence of Woodger's Work

The final criticism of the current consensus is that even in his own lifetime he was not well known, respected, or read by his peers – either in biology or in philosophy – and as a result his ideas had virtually no influence. The best way to refute the allegation that "Woodger was a minor figure within biology" (Cain, 2000, p. 540) who can "scarcely be described accurately as well-known or prominent" (*ibid.*, p. 359) is to consider his interactions with other biologists. In 1929 Woodger began corresponding with the biochemist and embryologist Joseph Needham, who shared his enthusiasm about the prospects of articulating an organicist philosophy of living systems that would overcome the epistemological limitations of mechanistic biology (see Needham, 1928a, b). Their exchange eventually resulted in the forging of a select group of biological intellectuals interested in building theoretical bridges between biochemistry, embryology, genetics, and zoology. This group came to be known as the 'Theoretical Biology Club', and it met regularly at Woodger's house in Epsom Downs and other locations between 1932 and 1938. In addition to Woodger and Needham, the group counted experimental embryologist C. H. Waddington, crystallographer J. D. Bernal, and biomathematician D. M. Wrinch among its core members. Other participants included Dorothy Needham, Max Black, B. P. Wiesner, Dorothy Crowfoot, G. A. Barnard, L. L. Whyte, W. F. Floyd, J. B. S. Haldane, and Karl Popper. Meetings of Woodger's Theoretical Biology Club covered a wide array of topics, but they were collectively geared towards the development of a 'mathematico-physico-chemical morphology' that would enable an interdisciplinary engagement with the problem of biological organization at the supracellular, cellular, and

subcellular levels.[33] After the Second World War, Woodger reassembled the Club, and Peter Medawar was among its new members. Meetings were held up until the early 1950s. Overall, the Theoretical Biology Club that formed around Woodger was, quite simply, *the* major study group for discussions in the theoretical foundations of biology of its time.[34]

Was Woodger's influence an exclusively British phenomenon? Grene and Depew (2004, p. 290) assert that "apart from a few followers in Great Britain, his effort[s] had little influence". It is not difficult to show the problems with this claim. For example, in 1930 Woodger came into contact with Austrian theoretical biologist Ludwig von Bertalanffy, and the two collaborated closely on expanding the latter's *Kritische Theorie der Formbildung* for the occasion of its translation into English, which was published as *Modern Theories of Development: An Introduction to Theoretical Biology* (1933). This book draws heavily on Woodger's own *Biological Principles*, and stands as a perfect companion to the former with respect to the philosophical articulation of the organicist position in biology. Woodger and Bertalanffy remained close friends for many years.[35] Bertalanffy eventually moved to Canada, where he became increasingly interested in the trans-disciplinary research program he called 'General System Theory'. The definitive statement of Bertalanffy's organicism is his *Problems of Life: An Evaluation of Modern Biological and Scientific Thought* (1952). Woodger's influence on Bertalanffy is perhaps best illustrated by a remark of Needham in his review of Bertalanffy's *Problems of Life* for *Nature*, where he introduces the author by describing him as "the Woodger of Canada" (Needham, 1953, p. 1119).

Woodger's influence also extended to the international philosophical community. An examination of his personal letters reveals that he corresponded for decades with some of the most prominent philoso-

[33] Between 1935 and 1938, the Theoretical Biology Club negotiated with the Rockefeller Foundation the creation of an Institute for Mathematico-Physico-Chemical Morphology at Cambridge, but the Foundation, led by Warren Weaver, ultimately decided not to fund the project, preferring instead to support research into what Weaver baptized as 'molecular biology' (see Abir-Am, 1987).

[34] The history of the Theoretical Biology Club has been thoroughly documented in Abir-Am, 1987. For an account of Woodger's influence on Needham, see Abir-Am, 1991. For an account of Woodger's influence on Waddington, see Peterson, 2010.

[35] Woodger sent food parcels to Bertalanffy during the Allied bombing of Vienna, and after the war he invited Bertalanffy to stay with him in London so that the two could work together on problems in theoretical biology. (For an extensive biographical account of Bertalanffy, see Pouvreau, 2009.)

phers of the twentieth century, including Rudolf Carnap,[36] Alfred Tarski, Karl Popper,[37] and W. V. O. Quine, among many others. At an institutional level, Woodger's influence is illustrated by the fact that he served on the advisory board of Neurath's *Encyclopedia of Unified Science*, as noted earlier. He also contributed a volume to the *Encyclopedia* himself (i.e., Woodger, 1937) and in addition he translated Felix Mainx's *Foundations of Biology* (1955) for its inclusion. It is also worth mentioning that he served on the general editorial committee for *Synthese*, and devoted considerable effort to the promotion of philosophy of science in Great Britain. He was an early member of the 'Philosophy of Science Group' of the British Society of the History of Science, and when this group became the British Society for the Philosophy of Science, Woodger was appointed its second Chairman. In his obituary for Woodger in *The British Journal for the Philosophy of Science*, Popper declared that Woodger "stimulated the evolution of the philosophy of science in Britain and in the United States as hardly anybody else" (Popper, 1981, p. 328).

There is also a glaring lack of evidence in support of the contention that Woodger's work was ignored or treated with contempt by his contemporaries. Decades after its publication, *Biological Principles* continued to be considered a cornerstone of philosophy of biology. Ernest Nagel regarded it "the best book of its kind" (Martin to Woodger, 31st July 1951), and as late as 1965 Popper described it as "an excellent and indeed brilliant book – quite unique in its scope and treatment, and in my opinion the best book on the Philosophy of Biology" (Popper to Franklin, 25th September 1965). Moreover, it is interesting to note that Morton Beckner's *The Biological Way of Thought* (1959), which some contemporary philosophers of biology have erroneously identified as "[t]he first serious philosophical work that examined the foundations of biology" (Thompson, 1989, p. 23), was

[36] Woodger's interaction with Carnap does not appear to have been confined to philosophical matters. In 1959, he wrote to Carnap to apologize for having had an affair with his wife, Ina, several decades earlier during a summer holiday in Austria (see Woodger to Carnap, May 1959, and the letters from Rudolf and Ina Carnap to Woodger of the 10th January 1960).

[37] When Popper applied for the position at the London School of Economics, Woodger wrote him a reference. When he was appointed, he wrote to Woodger: "I am very thrilled about my appointment, and I am very much looking forward to seeing you soon: not only because you are the only person in England whose main interests run very closely parallel with mine but because I want to thank you for your help" (Popper to Woodger, 2nd July 1945).

conceived as a follow-up to *Biological Principles*, presenting an update of its major theses 30 years later (see Withers, 1961).

Now, it is possible that some commentators may have intended the criticism that Woodger's work "was generally spurned by biologists" (Roll-Hansen, 1984, p. 416) to apply exclusively to the books in which Woodger used symbolic logic. But even this more moderate version of the claim is problematic. It is undoubtedly true that Woodger's formal work divided opinions, and although it did not lack critics, there is evidence that several prominent biologists admired it and saw promise in the methods it introduced. For example, upon reading the manuscript that would become *The Axiomatic Method in Biology*, R. A. Fisher wrote to Woodger: "I can see [...] that you are laying the foundations of a logical system adequate to comprehend biological ideas in a way that is thorough and, if I may say so, masterly" (Fisher to Woodger, 18th May 1934).[38] Nicolas Rashevsky (1963) also paid tribute to *The Axiomatic Method in Biology*, comparing it to the pioneering work of Alfred Lotka and Vito Volterra in mathematical ecology, and connecting it to his own program in mathematical biophysics. To mention one more interesting example, shortly after the publication of *Biology and Language*, Richard Lewontin (then still a graduate student) wrote to Woodger: "I do not mean to intimate that I am out of sympathy with a linguistic approach to biology. On the contrary, I hope that quite soon biologists, realizing the revolution which has occurred in the physical sciences has been due to a reexamination of its fundamentals, will begin to apply the methods of linguistics to their own problems" (Lewontin to Woodger, 6th April 1953).

An even more important fact to take into consideration is that a number of biologists have been directly influenced by Woodger's formal program, and have employed his methods to pursue their own projects. John Gregg, for instance, heavily relied on Woodger's (1952a) set-theoretical concept of taxonomic hierarchy in his book *The Language of Taxonomy: An Application of Symbolic Logic to the Study of Classificatory Systems* (1954). Woodger's taxonomical ventures (as well as Gregg's) also had a considerable impact on Willi Hennig, the founder of cladistics. In his landmark work *Phylogenetic Systematics*, Hennig (1966, pp. 16–17) writes: "We consider the investigations of Woodger and Gregg extraordinarily important because they clarify, with methods that exclude all confusion and contradiction, the peculiarities of the hierarchic system, and so create exact prerequisites for investigating the

[38] On the affinities between Fisher and Woodger, see Allen, 1938.

questions of whether and why it deserves the favor it enjoys in biological systematics''.[39] Woodger's (1945) formal approach to morphology has likewise been influential. Apart from the general adoption of the term 'Bauplan' in biology, Woodger's conception of homology has influenced the work of biologists like Rupert Riedl (1978) and Diego Rasskin-Gutman (2003), as well as philosophers like Nick Jardine (1967, 1969) and Bruce Young (1993). Others who have been inspired by Woodger's formal methods include Mary Williams, who tried to axiomatize Darwin's theory of evolution (Williams, 1970), Martino Rizzotti and Alberto Zanardo, who provided an axiom-system for molecular genetics (Rizzotti and Zanardo, 1986; Zanardo and Rizzotti, 1986), and most recently, Giovanni Boniolo and his colleagues, who have been working to develop a formal language for molecular biology for the purposes of data-mining and prediction (Boniolo et al., 2010).

On the whole, there is no truth to the fourth claim of the current consensus. Woodger *was* well-known, respected, and read by his peers, both in biology and in philosophy. It is because of this reason that his ideas managed to exert an influence on these disciplines. The notion that as he got older ''[h]e became progressively more isolated in his work and tragically resulted in talking to no one'' (Haraway, 1976, p. 131) is a myth. In fact, a most impressive *Festschrift* was presented to Woodger on the occasion of his seventieth birthday (Gregg and Harris, 1964). The edited volume, intended to honour Woodger's scholarly contributions, consisted of 26 commissioned essays on topics as diverse as metaphysics, formal semantics, probability theory, animal growth models, hierarchical organization, and taxonomy (one reviewer acutely observed that Woodger himself was probably ''the only person qualified to review this book'' [Kneale, 1966, p. 160]). The remarkable roster of contributors included biologists like Rashevsky, Lewontin, John Tyler Bonner, Aristid Lindenmayer, and Leigh van Valen, as well as philosophers such as Quine, Popper, Beckner, Frederick Fitch, and Czesaw Lejewski.

A final point worth mentioning in relation to Woodger's philosophical influence is that he was a sufficiently prominent figure to warrant a personal entry in the *Encyclopedia of Philosophy* (published in 1967), as well as featuring conspicuously in the entry on 'Organismic

[39] For a detailed analysis of Woodger's influence on Hennig's phylogenetic systematics, see Rieppel, 2003.

Biology' [i.e. Organicism], as Smocovitis (1992, 1996) has pointed out.[40] When he retired after 37 years at UCL, *Nature* published a brief announcement, which stated that "Woodger's writing and thinking have had a quiet but deep and pervasive influence over a great deal of modern biological thought" (Anon., 1960, p. 75).

Reclaiming Woodger's Legacy

As previous sections have shown, the vast majority of criticisms that philosophers of biology have levelled against Woodger since the professionalization of the discipline are misplaced. The current consensus in the field regarding who Woodger was and what he accomplished needs to be supplanted by a more informed view. In this paper, we have taken the first steps towards the articulation of such a view.

At the risk of stating the obvious, we should clarify that in defending Woodger from his recent critics we do not intend to suggest that everything he did was valuable, or that he did not make errors of judgment. It is evident that not all of his work has stood the test of time. This is particularly true for *The Axiomatic Method in Biology*, which, while being a remarkable feat of logical analysis, clearly did not lead to the revolution in theoretical biology that Woodger hoped (and probably for good reason). Likewise, Woodger's axiomatic conception of scientific theories put forward in *The Technique of Theory Construction* seems misguided in retrospect, as does his insistence in *Biology and Language* on conducting all theoretical discussions in biology in a logical metalanguage. The crucial point is that these shortcomings in no way diminish the value and significance of the rest of his oeuvre, especially his critical work. *Biological Principles*, in particular, remains a monumental achievement of early philosophy of biology. As we have shown, its comprehensive treatment of the central philosophical problems of

[40] Woodger's influence in philosophy actually extended beyond philosophy of biology and general philosophy of science into debates in analytic metaphysics over the nature of identity and substance. This is reflected in the work of David Wiggins (1967, 1980). In the preface to his *Identity and Spatio-Temporal Continuity* (1967, vii–viii), Wiggins writes: "It gradually became evident to me in constructing this work that for the future of metaphysics no single part of the philosophy of science was in more urgent need of development than the philosophy of biology […] And it is a misfortune of present-day analytical philosophy that it has not inspired the production of any writings in the philosophy of biology which are […] worthy to succeed the seminal writings of J. H. Woodger".

biology is replete with arguments that prefigure a number of recent debates, as well as containing insights that are surprisingly pertinent to current discussions.

More generally, Woodger's understanding of the philosophy of science also remains relevant. Woodger conceives philosophical analysis as *part* of the scientific enterprise, belonging to what he calls the 'critical aspect' of scientific practice. Philosophy of science must be informed by empirical research, but should not dictate its direction. The philosopher of science ''has nothing to do with teaching investigators their business. He is concerned with interpretations: not with weighing empirical evidence upon which they are based, but with the most general assumptions, presuppositions, postulates, etc., which underlie them'' (Woodger, 1929a, p. 2). This non-prescriptive, naturalistic account of philosophical analysis as being directly complementary to scientific research, published in 1929, should resonate strongly with most contemporary philosophers of biology. There are many such lessons for the philosopher in Woodger's writings. Indeed, one of most valuable aspects of *Biological Principles* is the methodology it employs, in the way it resolves longstanding biological disputes by carefully analyzing the presuppositions upon which each of them rest – and doing so *without* resorting to symbolic logic. And if we are to understand Woodger's recourse to logic in his later years as part of a lifelong quest for the conceptual clarification of scientific knowledge (as we have argued in this paper), there is little reason to remain wary even of his formal writings.

We began this paper by emphasizing that the received view of the history of the philosophy of biology is seriously misleading and in dire need of revision. The widely held belief that the philosophy of biology arose *ex nihilo* in the last third of the twentieth century has – to the field's detriment – made contemporary practitioners oblivious to a huge body of literature. There are many other early twentieth-century philosophers of biology like Woodger waiting to be rediscovered and reappraised. For the time being, it suffices that we realize that the familiar caricature of Woodger as a dogmatic logician obsessed with axiomatizing biological theories, while playful and amusing, has resulted in decades of neglect of a very rich oeuvre. It may still be the case that the majority of philosophers of biology choose to ignore Woodger's work, but they will no longer have a convenient excuse for doing so.

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