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ORIGINAL PAPER

Neither logical empiricism nor vitalism, but organicism: what the philosophy of biology was

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Abstract Philosophy of biology is often said to have emerged in the last third of the twentieth century. Prior to this time, it has been alleged that the only authors who engaged philosophically with the life sciences were either logical empiricists who sought to impose the explanatory ideals of the physical sciences onto biology, or vitalists who invoked mystical agencies in an attempt to ward off the threat of physicochemical reduction. These schools paid little attention to actual biological science, and as a result philosophy of biology languished in a state of futility for much of the twentieth century. The situation, we are told, only began to change in the late 1960s and early 1970s, when a new generation of researchers began to focus on problems internal to biology, leading to the consolidation of the discipline. In this paper we challenge this widely accepted narrative of the history of philosophy of biology. We do so by arguing that the most important tradition within early twentieth-century philosophy of biology was neither logical empiricism nor vitalism, but the organicist movement that flourished between the First and Second World Wars. We show that the organicist corpus is thematically and methodologically continuous with the contemporary literature in order to discredit the view that early work in the philosophy of biology was unproductive, and we emphasize the desirability of integrating the historical and contemporary conversations into a single, unified discourse.

Keywords History of philosophy of biology · Logical empiricism · Vitalism · Organicism · Theoretical biology

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[W]henever one does decide to publish, it is necessary to reckon with the great 'paper memory of mankind', the conserved experience of other workers who have loved and investigated the same things. It then becomes a duty to study the 'literature of the subject', if only for the purpose of bringing the new work into intelligible, organic relation with the old. Failure to do this may be justly interpreted as carelessness, sloth, ignorance or conceit. — (Wheeler 1906, p. 349).

1 Introduction

The philosophy of biology is a lively and well-established academic discipline. It boasts its own specialist journals, textbooks, conferences, and professional organizations. The discipline is in excellent shape, but according to the standard account of the history of the field, this is a very recent state of affairs. It is generally agreed that philosophy of biology only really took form in the last third of the twentieth century with the pioneering work of David Hull (1965a, b, 1972, 1974) and Michael Ruse (1969, 1970, 1971, 1973), as well as that of Kenneth Schaffner (1969a, b, 1974), and William Wimsatt (1971, 1972, 1974). These authors focused on a core set of problems, which set the agenda for subsequent discussions. From the 1970s onwards the field grew rapidly, becoming consolidated as a fully-fledged discipline in the 1980s, and ultimately developing into the prosperous area of research that it is today.

Of course, no one has suggested that nothing was written on the subject prior to the late 1960s and early 1970s. Indeed, a quick browse through the back issues of any of the older philosophy of science journals reveals that philosophy of biology articles have *always* been prominently featured (see Byron 2007 for a detailed bibliometric survey). Nevertheless, there is nearly universal agreement that most of this earlier work was unproductive and does not stand in an ancestral relation to the contemporary discourse.[1] To say that the early literature is not looked upon highly by current practitioners would be an understatement. As Ruse (perhaps the most outspoken of the discipline's founders) has put it, "[o]nly those who were there at the time—around the late 1960s, early 1970s—can know just how bad was much that passed then for the philosophy of biology. […] It was dreadful stuff, marked by an incredibly thin knowledge of biology" (Ruse 2000, p. 467). What allegedly distinguished the work that began to appear at that time is the fact that it took biology seriously. Unlike their immediate predecessors, the generation that began to turn their attention to biology during this period grappled with genuine conceptual problems raised by actual empirical research, instead of resorting to simplistic, textbook presentations of biological facts in order to advance prior philosophical agendas (see Hull 1969).

Two intellectual movements are said to have been responsible for the wrongheadedness of early twentieth-century philosophy of biology: logical empiricism and vitalism. The logical empiricists, we are told, devoted most of their efforts to analysing the logical structure of physical theories and cared little about the so-called 'special sciences' such as biology. When they did engage with

[1] This is reflected, for instance, in the *Stanford Encyclopedia of Philosophy*'s main entry on the subject (Griffiths 2014), in which work completed before the last third of the twentieth century is summarily discussed under the heading 'Pre-history of Philosophy of Biology'.

biology, it was only in ill-conceived attempts to make biological theories more like their physical counterparts by forcibly fitting them into rigorous deductive systems. The vitalists, on the other hand, sought to protect the autonomy of biology from the threat of physicochemical reduction by invoking obscure supernatural agencies acting within organisms. Dichotomizing the literature into two pervasive traditions—one of which sought to muscle biology into an idealized framework borrowed from physics, and the other which was intent on pushing biology out of the realm of science altogether—has led many to conclude that "for much of the earlier part of the twentieth century, philosophy of biology was in a sorry state" (Takacs and Ruse 2013, p. 6). This understanding of the history of the philosophy of biology is articulated quite explicitly in the opening pages of Ruse's textbook, *The Philosophy of Biology Today*:

> [P]hilosophers of science in the twentieth century have focused mainly on the physical sciences, and any spare effort has tended to be directed toward the social sciences. What little attention has been paid to biology has been generally directed to one extreme or another. At one end of the spectrum we have those who were overly impressed by the turn-of-the-century formalisms of the logicians and mathematicians, and who wanted to do likewise for biology. Since they—especially their leader J. H. Woodger […]—were simultaneously empiricists of the most naively dogmatic kind, their efforts tended to go unread […]. At the other end of the spectrum we have those who feared and loathed materialism, and who were determined to prove that an understanding of organisms demands reference to vital forces or spirits—*elans vitaux* or entelechies—forever beyond the grasp of conventional science. (Ruse 1988, pp. 1–2)

In this paper we will argue that the standard account of what the philosophy of biology was, as exemplified by the preceding quotation, is almost entirely false. As Jason Byron (2007) anticipated in a bibliographic analysis—which demonstrated that philosophy of biology was an active area of research for much of the twentieth century—a completely new account of the development of the field is needed. In earlier work (Nicholson and Gawne 2014), we took the first step towards the articulation of such an account by showing that the most maligned figure of early philosophy of biology, Joseph Henry Woodger, has been grossly misunderstood by modern scholars. The present paper takes a further step in the same direction by attempting to provide an alternative characterization of the state of play in the philosophy of biology during the first half of the twentieth century—well before the field as we know it today became established.

In the following sections, we will attempt to show that logical empiricism and vitalism were of minimal importance to the philosophy of biology during the first half of the twentieth century. The most prominent intellectual movement of early philosophy of biology was the organicist school that developed concurrently in Great Britain, Continental Europe, and the United States in the years between the First and Second World Wars. As we will see, organicism constituted a distinct discourse with a core set of topics that were addressed by an international

community of scholars who read, discussed, and responded to one another's work. In an important respect, the philosophy of biology was already a clearly defined field of research well before the generation that began working on the subject in the late 1960s and early 1970s was even born. Finally, we will offer a compelling reason for interesting contemporary philosophers of biology in the history of their discipline, namely that there is a far greater degree of continuity between the old and the new literatures—both in terms of content and in terms of method—than has hitherto been acknowledged. By setting the historical record straight, we hope to remove the stigmas that are unjustly associated with the early twentieth-century literature so that the unique perspectives and forgotten contributions of this period can be productively assimilated into the modern discourse.

2 Early philosophy of biology and logical empiricism

Because the advent of contemporary philosophy of biology is often tied to the demise of logical empiricism (see, e.g., Callebaut 1993), associating the early twentieth-century work in the field with the positivist tradition has provided a convenient excuse for ignoring it. That is to say, early philosophy of biology has regularly been dismissed as futile because it has been associated with (what many take to be) a discredited research program. A representative example of this argumentative strategy can be found in a paper by Gereon Wolters, which sets out to establish that early work in the field was fundamentally flawed because it was infected by the misguided ideological preoccupations of logical empiricism. According to Wolters:

> 'Antimetaphysics', 'reduction', [and] 'physics as model science' are key concepts with which logical empiricism set the stage for more than three decades of stagnation in the philosophy of biology. Antimetaphysical needs […] were imposed on philosophy of biology in the form of reductionism and in taking physics as model for biological science. Logical empiricism envisaged through its normative conception of a reductionist biology a biological science that had only [a] slight resemblance to the one actually practised by biologists. Preconceived ideas about what biology ought to be prevented the unprejudiced analysis of what biology, in reality, was. (Wolters 1999, p. 195)

In this section, we will dispute the alleged connection between early philosophy of biology and logical empiricism by showing that the two philosophers of biology who may be most readily associated with the Vienna Circle, namely Woodger and Ludwig von Bertalanffy, did not endorse *any* of the three logical empiricist theses singled out by Wolters. As we will see, neither of these individuals showed a disdain for metaphysics, both rejected the view that biological explanations are reducible to physical explanations, and neither believed that physics is the ideal science which biology should model itself after.[2]

[2] We focus on Wolters' theses, not because we regard them as necessary and sufficient criteria for determining whether a particular individual was a logical empiricist, but because they effectively

Let us start with Woodger. Whenever the terms 'logical empiricism' and 'philosophy of biology' appear in the same sentence, a reference to Woodger invariably follows (see, e.g., Roll-Hansen 1984; Rosenberg 1985; Thompson 1989; Sarkar 1996). "To be sure", says historian Joe Cain (2000, p. 537), "Woodger was *the* scholar […] working to apply logical positivism to the biological sciences". More derisively, it is frequently remarked that Woodger "tried to force biological theories into the logical-empiricist deductive corset" (Callebaut 2005, p. 104). Indeed, dismissals of early philosophy of biology often consist of brief allusions to the wrongheadedness of Woodger's enterprise. In a previous paper (Nicholson and Gawne 2014), we provided a comprehensive overview of Woodger's oeuvre in order to demonstrate that the current consensus regarding his work and influence is troublingly inaccurate. Here, we will focus specifically on Woodger's stance concerning the three theses outlined by Wolters.

Although it is undeniable that Woodger maintained close personal connections with prominent logical empiricists, such as Otto Neurath and Rudolf Carnap, and participated in some of the movement's professional activities, his intellectual relation to logical empiricism is far less clear-cut. Consider, for instance, the verificationist criterion of meaning so commonly associated with logical empiricism. This commitment supposedly prompted members of the movement to reject all metaphysical statements as meaningless, and accordingly, to maintain that all traces of metaphysics should be purged from the sciences. By examining his work, it is easy to show that Woodger displayed an attitude toward metaphysics that is completely at odds with such a position.

Woodger's five-hundred-page magnum opus, *Biological Principles: A Critical Study*, was explicitly conceived as an attempt to use Alfred North Whitehead's new metaphysics of nature to resolve the major antitheses of biological thought, such as mechanicism versus vitalism, preformation versus epigenesis, and causation versus teleology. More tellingly, Woodger asserted in that book that there are no absolute standards by which a particular statement can be judged to be metaphysical or scientific in character. He noted that biologists often categorize a claim as 'metaphysics' if they happen to disagree with it, whereas claims that they find congenial are classified as 'science'. Woodger also openly denied that science should attempt to rid itself of metaphysics, arguing instead that the two fields are "mutually complementary" (Woodger 1929a, p. 24). As to the question of whether it is possible to purge metaphysical statements from science, Woodger responded by quipping that those who "suppose themselves to be above 'metaphysics' […] are only a very little above it—being up to the neck in it" (ibid., p. 246). For Woodger, the real, and perhaps only, danger with metaphysics is that scientists do not always

Footnote 2 continued
encapsulate the caricaturized view of logical empiricism that has so frequently been lambasted by modern philosophers of science. We are well aware that logical empiricism was actually a highly heterogeneous movement, and that there were probably no doctrines that were upheld by all of its members (see, e.g., Creath 2014), but this does not really matter in the present context. Our only aim here is to show that early work in the philosophy of biology does not instantiate the flaws and prejudices that have come to be associated (rightly or wrongly) with the logical empiricist program (cf. Sarkar under review).

realize that their interpretations of empirical data can be tainted by unanalysed metaphysical presuppositions.

On the issue of reductionism, Woodger's stance was equally clear: he emphatically rejected the possibility that biology could be reduced to physics and chemistry. For Woodger, biological explanations are not reducible because they address the reciprocal relations between parts and whole in living systems in ways that mechanical explanations couched in physicochemical terms cannot. While conceding that organisms are solely physicochemical in their constitution, he believed that an explanatory approach that exclusively focuses on physical interactions, chemical reactions, and chemical composition runs the risk of glossing over the very feature that makes organisms distinctive, namely their hierarchical, self-maintaining organization. Woodger complained that:

> Biologists, in their haste to become physicists have been neglecting their business [by] trying to treat the organism not as an organism [i.e. an organized system] but as an aggregate. And in doing so they may have been good chemists but they have not been good biologists, because they have been abstracting from what is essential to the biological level. (Woodger 1929a, ibid., p. 291)

What is 'essential to the biological level' for Woodger is organization. To understand this phenomenon, biologists must develop their *own* concepts and tools of analysis. Woodger himself devoted a considerable portion of *Biological Principles*—as well as a triad of subsequent papers (Woodger 1930a, b, 1931)—to elucidating the nature of organismic organization and articulating a corresponding theory of biological explanation.

Woodger was also explicit in his opposition to the view that physics is the model science that biology should try to emulate. In fact, a theme that runs throughout his entire oeuvre is the conviction that biology must be encouraged to find its own way forward if it is to achieve the theoretical sophistication of physics. That is to say, biology should be allowed to develop in the directions that enable it to confront its subject matter on its own terms. According to Woodger, biologists have nothing to gain from dogmatically adopting the models and explanatory standards of the physical sciences, as this only "engenders a feeling of inferiority in biologists. It makes them feel that they are wasting their time unless they too become biophysicists or biochemists. It thus retards the search for explanatory hypotheses on the biological levels" (Woodger 1952, p. 336). Elsewhere he remarked that "to regard the traditional 'physico-chemical' ways of thinking as indispensable and as 'natural science *par excellence*' is the apotheosis of biological stupidity" (Woodger 1929b, p. 358). It amounts to crying out: "Stop thinking, folks! The physicists have done all the thinking necessary" (Woodger 1956, p. 10). More forthright assertions of the autonomy of biology can hardly be found.

In light of these considerations, why have so many modern commentators insisted on labelling Woodger the foremost champion of logical empiricist philosophy of biology? We suspect it is because critics have been extremely selective in the parts of Woodger's corpus they have chosen to discuss, focusing almost exclusively on his attempts to axiomatize biological theories. This selective

treatment has led some of Woodger's formal works, especially *The Axiomatic Method in Biology*, to acquire an almost mythical status among contemporary philosophers of biology. In Hull's words, "[a]s with *Finnegans Wake*, everyone seems to have heard of Woodger's *The Axiomatic Method in Biology* (1937), but very few have ever read it, and the few who have have not come away very impressed" (Hull 1988, p. 105). The goal of Woodger's formal project was to construct a meta-language for biology that would help organize biological statements and uncover connections between distantly related theories. Due to its heavy use of symbolic logic, this project has often been misinterpreted as an "attempt to treat a biological discipline on the model of a close-knit physical theory" (Smart 1963, p. 50). For the same reason, Woodger himself has repeatedly been accused of "insist[ing] on putting everything into rigorous deductive systems, with absolutely disastrous consequences" (Ruse 2000, p. 470).

Although Woodger did come to believe that axiomatization could help biology achieve the level of theoretical sophistication found in the physical sciences, he never doubted that such formalizations would need to cater to the specific needs of biology. For instance, in the first chapter of *The Axiomatic Method in Biology* he asserted 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 noted that translating theories into a formal meta-language would free biologists "from the accidental restrictions of traditional mathematics, i.e., the mathematics which have arisen to meet the needs of physics" (Woodger 1939, p. 39). In that book he also perceptively anticipated the criticisms that commentators would make of his project decades later:

> [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. (ibid., p. 74)

It is clear, then, that even if one focuses exclusively on his axiomatic works and wilfully ignores *Biological Principles* and all of his other non-formal publications, Woodger still does not fit the cartoonish view of a crusading logical empiricist that has often been attributed to him in order to discredit his work. Throughout the various phases of his career, Woodger forcefully rejected all three theses that Wolters picks out as distinctive of logical empiricist philosophy of biology. His growing interest in logic from the 1930s onwards does, of course, resonate with aspects of the logical empiricist program, but this alone does not justify depicting him as a physics-revering, anti-metaphysical reductionist.

The other early twentieth-century philosopher of biology that might be associated with logical empiricism is Bertalanffy (see, e.g., Rieppel 2003; Sarkar under review). Born into *fin-de-siècle* Vienna, Bertalanffy was raised in the

intellectual milieu that gave rise to 'The Scientific Conception of the World' of the Vienna Circle. As a student at the University of Vienna, Bertalanffy studied under Moritz Schlick—the founder of the Vienna Circle—and interacted frequently with many other members of that community, including Viktor Kraft and Friedrich Waismann. After finishing his doctoral thesis, Bertalanffy attended the meetings of Carnap's 'Study Group for Scientific Cooperation', and had regular contact with Hans Reichenbach, leader of the Berlin Circle (Pouvreau 2009). Bertalanffy also contributed the longest article (i.e. von Bertalanffy 1930b) to the first issue of *Erkenntnis*—logical empiricism's official vehicle of publication—and received several invitations to attend the movement's conferences on the unity of science (Hofer 2002). Given this track record, it is tempting to conclude that Bertalanffy was a committed logical empiricist. This, however, would be an extremely misleading characterization.

Although he admired Schlick personally, and grieved when Schlick was murdered by a former student in 1936, Bertalanffy was actually a lifelong critic of logical empiricism. In the foreword to the 1962 edition of his early classic *Modern Theories of Development: An Introduction to Theoretical Biology* (first published in English in 1933), he explained that his "philosophical education took place in the classic Vienna Circle of logical positivism", and that his longstanding opposition to the movement stemmed from "early-felt limitations of this philosophical attitude" (von Bertalanffy 1962, p. vi). In his writings, Bertalanffy frequently objected to the movement's radical empiricism, indicted its commitment to scientism, and considered its rejection of metaphysics sectarian and naïve.[3] While Bertalanffy was critical of the highly speculative biological theorizing that vitalists tended to indulge in, his entire body of work in the philosophy of biology was geared towards the articulation of an 'organismic' standpoint that would provide the metaphysical foundation for a scientifically-grounded, empirically-informed 'theoretical biology'.

Bertalanffy's efforts to establish a theoretical framework proper to biological phenomena was driven by the conviction that biology is an autonomous field of inquiry that cannot be reduced to physics and chemistry. Although physicochemical analyses are necessary in the scientific study of life, they are not sufficient. Reductionistic investigations that catalogue the parts and processes of living systems and characterize them in isolation cannot yield truly biological explanations because they provide no information about how these parts and processes are organized and functionally integrated into the complex coordinated whole that we refer to as an organism. For Bertalanffy, "the chief task of biology must be to discover the laws of biological systems to which the ingredient [physicochemical] parts and processes are subordinate" (ibid., p. 65). The following passage effectively summarizes Bertalanffy's stance on the question of physicochemical reduction:

[3] In one of his last works, he wrote: "You, the reader, may wonder about that old-fashioned term 'natural philosophy' in the title of this essay. Is it not the hallmark of modern science that it got rid of obsolete philosophy? Have modern positivists—including the Vienna School where I myself started more than 40 years ago—worked in vain and am I going to reinvoke the ghosts of metaphysics? […] A slightly mischievous answer would be that science and philosophy never got rid of metaphysics and that the metaphysics of positivism is a particularly naïve and superficial one" (von Bertalanffy 1967, p. 55).

> If we investigate vital processes physically and chemically we shall never reach a process which runs contrary to the laws of physics and chemistry. In this sense life is only 'a combination of physical and chemical processes'. But it is possible that such a point of view does not touch at all the real biological problem—and in *this* sense life *is* more than a mere heap of physical and chemical processes and has its 'own laws'. The physico-chemical explanation of single phenomena in the organism does not, therefore, suffice for the foundation of theoretical biology. For the essential characteristic of living things as such—the arrangement or organization of materials and processes—it gives no explanation, and offers no possibility of setting up laws for this characteristic. The view that simply by means of a knowledge of the physics and chemistry of the materials and processes of the organism biology will become a branch of physics and chemistry, and so render a theory of the organism superfluous, is thus quite untenable. (von Bertalanffy 1933, p. 35)

Having said this, Bertalanffy did not doubt that "a fusion of the realms of physical and biological laws will ultimately be achieved", though he qualified this statement by adding that "the very inclusion of biological problems and fields leads to an expansion of the system of concepts and laws of physics" (von Bertalanffy 1952, p. 157). For Bertalanffy, the problem of the reduction of biology to physics is to a certain extent a semantic one, as it depends on the extension one bestows on the term 'physics'. Biology can only be said to be reducible to physics insofar as we are willing to expand our conception of physics to accommodate it.

Be that as it may, Bertalanffy did not believe that biology would advance by emulating physics. He held that biology's progress was dependent upon its ability to articulate its own theoretical apparatus to deal with its distinct domain of phenomena. Theoretical biology cannot be derived from theoretical physics. Instead, it must consist of an independent set of concepts, principles, and laws that can play the same roles in biology that theoretical physics does in physics. Bertalanffy attempted to realize his vision of theoretical biology in the two volumes of his *Theoretische Biologie* (von Bertalanffy 1932, 1942). In these works, he drew on the thermodynamically open nature of biological systems (which exchange both energy and matter with their environment in order to maintain themselves far from equilibrium) to formulate general organismic principles that could function similarly to Newton's three laws of motion. Bertalanffy then attempted to use these principles to bring together various branches of biology under a common theoretical umbrella.

On the whole, neither Woodger nor Bertalanffy endorsed any of the theses Wolters takes to be characteristic of logical empiricist philosophy of biology (i.e. 'antimetaphysics', 'reduction', and 'physics as model science'). Both authors unequivocally rejected them, and did so by appealing to very similar arguments. This convergence is not due to chance. Woodger and Bertalanffy held similar views because they belonged to the same intellectual movement, namely the organicist school that blossomed during the interwar period. As we will see later, this discourse developed completely independently from logical empiricism.

The closest that anyone got to defending Wolters' theses in a systematic way was the Prague-based geneticist Felix Mainx, who attracted the attention of Carnap and Philipp Frank. Mainx was asked to write the entry on biology for the *Encyclopaedia of Unified Science* (see Hofer 2013 for the details of this commission). The resulting contribution, an eighty-five-page booklet titled *Foundations of Biology*, bears all the hallmarks of what Wolters would expect from a logical empiricist treatise on biology: it advocates the verificationist criterion of meaning, condemning biological works that attempt to pass off tautologies and metaphysical claims as meaningful statements, it denies any autonomy to biology, asserting that its delimitation as a natural science is a purely pragmatic exercise, and it is strongly critical of abstract theorizing in biology—which Mainx mockingly dubs 'parabiology'—concerning problems such as organization, teleology, and wholeness. Accordingly, Mainx explicitly rejects the analogy between theoretical physics and theoretical biology, arguing that the biological sciences have no need for such a field:

> A deliberate separation of a 'theoretical biology' would today mean an intellectual decline or even the encouragement of speculative tendencies which would not promote the development of the science. A purely theoretical biology would be unable to make any scientific assertion which would say more than the statements of the special branches about living things. (Mainx 1955, p. 58)

Mainx's disagreement with Woodger and Bertalanffy concerning the prospects and desirability of developing a theoretical biology is indicative of the ideological gap that existed between them. For organicists like Woodger and Bertalanffy, the single most pressing task in biology was the development of the science's theoretical and philosophical foundations, and it was the numerous contributions made by members of this school—rather than Mainx's short book—that set the intellectual agenda of the era. Mainx's *Foundations of Biology*, while fitting the image of early twentieth-century philosophy of biology constructed by Wolters and others, can hardly be considered representative of the work conducted at the time.[4]

3 Early philosophy of biology and vitalism

Logical empiricism is widely regarded as a discredited research program, but the scorn that has been heaped upon this tradition seems insignificant when compared to the level of contempt that is persistently shown towards vitalism. The word 'vitalist' has become corrupted to such an extent that biologists and philosophers today use it only as a term of abuse to disparage and ridicule views they judge to be irrational, supernatural, or mystical. Indeed, to publically accuse someone of harbouring vitalist views amounts to condemning them to the scorching fires of scientific hell. There is something rather perverse about this state of affairs, as some of the most

[4] This should not be taken to imply that there is nothing of value in Mainx's book. Our point is simply that, at the time of its publication, Mainx's *Foundations of Biology* had very little impact. For a detailed analysis of the book's contents, see Sarkar (under review).

celebrated figures in the history of biology were committed vitalists.[5] How vitalism has come to be regarded as the quintessential *bête noire* of modern biological thought has to do with complex historical circumstances that need not concern us here. For our purposes, it suffices to realize that labelling early twentieth-century philosophers of biology as vitalists has been enough to ensure that their contributions are relegated to the dustbin of history. Why should contemporary scholars bother to study the work of authors who, in the aforementioned words of Ruse (1988, p. 2), "feared and loathed materialism, and who were determined to prove that an understanding of organisms demands reference to vital forces or spirits—*elans vitaux* or entelechies—forever beyond the grasp of conventional science"?

Ruse's allusion to the *élan vital* and the entelechy is, of course, a reference to the vital principles postulated respectively by the two most famous—or rather *in*famous—vitalists of the early twentieth century: Henri Bergson and Hans Driesch. When contemporary philosophers of biology mention these two men, they write as if they were the only authors who were writing on the subject at the time. Ernst Mayr, who played a fundamental role in the establishment of modern philosophy of biology—both through his own pivotal contributions (e.g. Mayr 1961, 1969, 1988) and through his mentorship of Hull (see Hull 1994)—recounted his first encounter with the subject as a young man in the 1920s:

> When I inquired (ca. 1926) which philosophers would be most helpful to a biologist, I was told Driesch and Bergson. When I left for New Guinea one and a half years later, the major books of these two authors [i.e. Driesch 1908 and Bergson 1907] were the only books I dragged around with me in the tropics for two and a half years. In the evenings […] I would read these two volumes. As a result, by the time I returned to Germany, I had concluded that neither Driesch nor Bergson was the answer to my search. Both authors were vitalists and I had no use for a philosophy based on such an occult force as the *vis vitalis*. (Mayr 2004, p. 2)

Whoever advised Mayr on what to read during his travels was seriously misinformed, as the writings of Bergson and Driesch are only the tip of the iceberg of what was going on in philosophy of biology at the time. The possibility then suggests itself that the reason why Bergson and Driesch are the authors most often remembered today is that they were the leaders of a vitalist community that was especially prominent during the early decades of the past century. However, this too is incorrect. Bergson and Driesch were salient figures at the time, not because they were the guiding luminaries of a thriving intellectual movement, but because they were the last outspoken advocates of a dying creed. Their influence, which was nevertheless considerable, needs to be understood in this light.

Of the two, Driesch had a far greater impact on philosophy of biology. It is true that Bergson's *L'Évolution créatrice* (1907) was widely read upon its publication,

[5] Thankfully, systematic re-evaluations of the place of vitalism in the history of biology are beginning to appear (e.g. Reill 2005; Normandin and Wolfe 2013). For an example of how vitalism can continue to play a useful role in contemporary biological theory, see Kirschner et al. 2000.

especially by philosophers. However, most biologists felt that the vitalist metaphysics it advocated was not relevant to their theorizing, as it was based on subjective anthropocentric notions such as intuition and sympathy (see, e.g., Schaxel 1913). Woodger (1929a, p. 299) observed that "biologists in general seem to have a rather low opinion of Mr. Bergson" and this may help explain why most of them did not bother to discuss his views—not even to criticize them.[6] The case of Driesch is quite different. Unlike Bergson, Driesch was already a well-respected experimental embryologist before he ventured into philosophy. His arguments for vitalism, which he developed on the basis of embryological investigations that he himself conducted, immediately caught the attention of those interested in the philosophical foundations of biology.

Driesch's formulation of a vitalistic theory of life was motivated by the conviction that Wilhelm Roux's *Entwicklungsmechanik*—the dominant experimental research program in biology at the time—was incapable of explaining the plastic, self-regulatory dimensions of development. In his experiments, Driesch had found that the developing embryo adapts to external perturbations by regenerating and reorganizing its parts; it constitutes a 'harmonious equipotential system' that, he argued, cannot be conceived in mechanical terms. To account for the distinctive non-mechanical character of organisms, Driesch postulated a vital principle, which he termed the 'entelechy'. Driesch conceived the entelechy as an immaterial, regulating agent that determines which of the various potentialities resident in the developing organism become physically realized, and which are restrained. The entelechy acts as a kind of buffer that protects the inbuilt tendencies of the embryo from being disrupted by adverse environmental conditions. According to Driesch, the entelechy is non-temporal, non-spatial, and non-psychic, and consequently does not contradict any natural laws (Driesch 1908, 1914).

The crucial fact to bear in mind when considering the reception of Driesch's ideas by his contemporaries is that support for vitalism in general had been steadily declining since the mid nineteenth century, and that by the turn of the twentieth century appeals to vital principles had become disreputable (cf. Coleman 1977; Singer 1989; Hunter 2000). In a sense, Driesch's arguments for vitalism were already anachronistic by the time they were published. As mentioned earlier, they represented a concerted effort to revive a dying biological tradition rather than extend an existing one (which is why Driesch's views were often characterized as '*neo*-vitalism'). However, Driesch's attempt to resuscitate vitalism was a resounding failure, as his ideas were immediately met with violent opposition.

For example, browsing a special issue of *The Philosophical Review* devoted to the mechanicism–vitalism dispute (volume 27, number 6), which was based on a 1918 meeting of the American Philosophical Association that brought together philosophers and biologists, we find that contributors differed from one another in their respective adherence to mechanicism, but all unequivocally rejected Driesch's neo-vitalism. Criticisms of Driesch by his contemporaries took a range of forms, but

[6] An interesting exception is Julian Huxley, who in the preface to *The Individual in the Animal Kingdom* expressed his debt to Bergson: "It will easily be seen how much I owe to M. Bergson, who, whether one agrees or no with his views, has given a stimulus (most valuable gift of all) to Biology and Philosophy alike" (Huxley 1912, pp. vii–viii).

a prime target was his notion of entelechy. Some criticized the inexplicable fact that the entelechy does not operate in the same way across taxa, e.g. a 'salamander-entelechy' is able to regenerate a limb, whereas a 'human-entelechy' cannot. Others directed their criticism towards the concept of entelechy itself, arguing that it does not constitute an explanation, but simply another name for the problem at hand: "Driesch transfers the complexities of development to entelechy, leaving them in exactly the same need of analysis and explanation as before" (Jennings 1918, p. 581). Some argued, despite's Driesch's claims to the contrary, that the operation of an entelechy necessarily violates the laws of physics:

> [A]n entelechy can, without the performance of work, guide or coordinate toward a definite end processes which themselves require the performance of work. This view implies that in the organism, molecular movement can be directed, retarded, or accelerated at the will of the entelechy […] [However,] It is physically impossible for any agency to modify the processes in any material system without modifying the energy-transfers in that system, and this can be done only by […] the performance of work. One is forced to conclude that all such attempts at the solution of biological problems are based on fundamental misunderstandings. (Lillie 1914, pp. 843–844)

So while it is true that Driesch's neo-vitalism shaped philosophical discussions of biology at the beginning of the twentieth century (on this point—and on this point alone—we find ourselves in agreement with Ruse's analysis), it is also true that his ideas were categorically rejected by his peers upon their publication.[7] Already by 1911, Driesch's ideas were said to have "justly aroused vehement protest on the part of scientific investigators" (Elkus 1911, p. 358). By the end of the 1920s, any remaining neo-vitalist defences had all but disappeared from the literature. In a 1929 review essay titled 'Present Tendencies in Biological Theory', the renowned myrmecologist William Morton Wheeler observed that:

> [V]italism has many more lives than the proverbial cat and […] it periodically invades and confuses biology, introducing its metaphysical entities for the purpose of resolving theoretical conflicts, just as the gods were brought down in the machine to straighten out the plots of the Greek drama. That we have again entered on a period that abhors such artifices is shown by the literature, in which references to Driesch's entelechism, the most serious and elaborate attempt ever made to provide biological *dei ex machina*, are becoming increasingly rare. The same is true of Bergson's 'élan vital,' which naturally played a greater role in philosophy. (Wheeler 1929, p. 104)

What is most characteristic about the generation of authors who began writing on the philosophy of biology in the 1910s and 1920s is that they attempted to transcend the traditional conflict between mechanicist and vitalist conceptions of life by carving out a middle ground between the two positions that would retain what they

[7] As with any historical generalization, if one looks closely enough one can find exceptions, such as James Johnstone (1914), Eugenio Rignano (1926), and L. Richmond Wheeler (1939), all of whom defended Driesch-inspired forms of vitalism.

deemed valuable in each position, while avoiding their respective defects. Thus, they heartily agreed with the vitalists in their recognition of the systemic nature of living systems, and in their repudiation of mechanical interpretations of biological phenomena, but they also fully agreed with the mechanicists in their unwavering commitment to materialistic explanations, and in their adoption of physicochemical methods in biological investigation. The vitalists had been right to defend the autonomy of biological theory, but they had been wrong to ground it in the supposition that the characteristic features of organisms are derived from the activities of unknowable directive agencies. On the other hand, the mechanicists had been right to insist that organisms are subject to the same laws that govern the operation of non-living systems like machines, but they had been wrong to conclude that such laws suffice to account for the character and behaviour of organisms. Biological explanations are not reducible to mechanical ones because organisms are not machines, but this does not mean that the distinctiveness of organisms cannot be understood in purely naturalistic terms, i.e. as a consequence of their self-organizing and self-regulating capabilities. In this way, proponents of this new school of thought—which, as we will see later on, came to be known as *organicism*—sought to steer a middle course that would avoid the Scylla of mechanicist reductionism and the Charybdis of vitalist mysticism.

Organicist philosophy of biology arose at a time in which "the vast preponderance of active biological workers are mechanists" (Needham 1925a, p. 235). As a result, many organicists had to endure repeated accusations of vitalism because they refused to accept the mechanicist orthodoxy of the day. The noted physiologist John Scott Haldane (father of J. B. S. Haldane), who was one of the most prominent exponents of organicism, is a case in point. In his philosophical writings, Haldane tended to develop his argument in three stages: first he would discuss mechanicism and vitalism in turn, then he would draw on his own physiological work to point out what he considered to be the fatal flaws in both of these doctrines, and finally he would propound his own organicist views (see, e.g., Haldane 1921, 1930, 1931). Nevertheless, his emphatic denunciation of the machine conception of the organism meant that he was sometimes lumped with Driesch and dismissed as another misguided vitalist. This is an appellation he strongly objected to: "I am not, and never have been, a vitalist, although simply because I am unable to accept the traditional mechanistic biology of the last few decades I am often regarded as a vitalist. Vitalism in any form has the same fundamental defect as the mechanistic theory of life" (Haldane 1931, p. 31). As early as 1908, Haldane had pointed out that the vital force was "simply a convenient resting-place" for the facts about the organism that mechanical explanations could not explain (Haldane 1908, p. 693). Later he would also state that vitalism is "of little use as a working hypothesis in actual investigation" (Haldane 1917, p. 112), adding that its major merit lied in its "destructive criticism of the mechanistic theory" (ibid., p. 111). By 1932 he proclaimed that "biologists have almost unanimously abandoned vitalism as an acknowledged belief, and I do not think that they are ever in the least likely to return to it" (Haldane 1932, p. 54).

Similar dismissals of vitalism can be found in the works of all the other major organicists. The marine biologist Edward Stuart Russell, for instance, devoted his

first philosophical paper to criticizing vitalist theories for postulating unintelligible agencies (Russell 1911). In his 1924 treatise *The Study of Living Things: Prolegomena to a Functional Biology*, Russell castigated Driesch for injecting in a chain of physical processes a psychical agency endowed with "faculties of a very high, even miraculous order" (Russell 1924, p. 21). "It is as if", Russell continued, "one postulated the existence of a little infinitely wise god, situated somewhere inside the organism, […] interfering now with this process now with that, in such a way as to regulate and harmonize them all" (ibid.). Russell went on to author two additional monographs that presented his organicist outlook (Russell 1930, 1945), which shared Haldane's holistic vision of the organism, but which also emphasized the intrinsic purposiveness of organismic activities. In dealing with the thorny issue of organismic teleology, Russell, like Haldane, was careful to insulate himself from the possible charge of vitalism:

> Our conclusion that life processes are essentially and fundamentally directive and creative may be rejected as 'metaphysical' or 'mystical'. It is of course nothing of the sort. I make no hypothesis as to the philosophical basis or 'ground' of directiveness or creativeness. I merely accept the patent evidence that they are characteristic of living things and of them alone. (Russell 1945, p. 192)

Another influential advocate of the organicist position was Woodger, whose views we already examined in relation to logical empiricism. On the issue of vitalism, Woodger chided Driesch's negative argumentation for failing to realize that disproving mechanicism does not by itself establish the validity of vitalism, given that the two standpoints do not exhaust all theoretical possibilities (Woodger 1929a, chap. 5). Bertalanffy, whom we also discussed in the previous section, was equally critical of vitalism, asserting that "[t]he interpretation of the puzzling purposefulness of life by a still more puzzling active entity offers merely a mythological treatment of biology" (von Bertalanffy 1933, p. 43; see also von Bertalanffy 1952, pp. 7–8).

Aside from Haldane, Russell, Woodger, and Bertalanffy, an additional proponent of the organicist viewpoint was the chemical embryologist Joseph Needham, who published a series of important papers on the philosophy of biology (Needham 1925b, 1928a, b, 1930a, b, 1932, 1943). Needham considered that the obsolescence of the neo-vitalist school was best illustrated by the observation that "[t]he more its special entities immanent in living beings have been considered, the less necessary they have seemed to be in relation to the facts and the less valuable they have been found as inspirations to research" (Needham 1928b, p. 88).

The leading American promoter of organicism was the zoologist William Emerson Ritter. Like his European counterparts, Ritter resolutely condemned vitalism, describing it as "a walled city with the gates locked and the keys lost beyond recovery" (Ritter 1909, p. 184). In his gargantuan, two-volume work *The Unity of the Organism, or the Organismal Conception of Life*, he declared that "all supernaturalism, no matter what nomenclatorial garb it takes on, must be repudiated by the sciences of organic beings. Ideas, or psychoids, or entelechies, or 'principles' of any kind conceived as independent of, or even separable from, sensible objects

are quite […] repugnant to me" (Ritter 1919b, p. 149). Ritter was also critical of Driesch in particular, whom he blamed for corrupting Aristotle's original conception of 'entelecheia' (see Ritter 1932).

In conclusion, what emerges from the preceding analysis is that it is deeply misleading to use the term 'vitalist' to characterize early twentieth-century philosophy of biology. The vast majority of authors who wrote on the subject (not just those who belonged to the organicist school) criticized vitalism, and explicitly rejected any possible associations with theories such as the one proposed by Driesch. Regarding Driesch himself, it is undeniable that he had a considerable impact on discussions in the philosophy of biology at the start of the twentieth century, but not for the reasons that are frequently given. Driesch's ideas never caught on, and were widely rejected upon publication. By the 1920s, most philosophers of biology had brushed vitalism aside and had moved on to discuss issues related to the organicist standpoint. It was organicism, not vitalism, that dictated the direction of the field well into the 1950s.[8]

4 Organicism and early philosophy of biology

As we have seen in the preceding sections, the claim that early philosophy of biology was dominated by the agendas of logical empiricism and vitalism is unfounded. During the first half of the twentieth century, it was the distinct concerns of organicism that shaped discussions in the field.[9] But where does the term 'organicism' actually come from? According to Needham (1928a), the first biologist to use it was Yves Delage (1903), but it acquired an entirely new meaning in 1917 when it was

[8] For example, Ernest Nagel began his 1951 paper on biology by remarking that "[v]italism of the substantival type sponsored by Driesch and other biologists during the preceding and early part of the present century is now a dead issue in the philosophy of biology" (Nagel 1951, p. 327). Instead, Nagel concerned himself with what was the dominant theme at the time, namely the adequacy and applicability of the organicist conception of life. Morton Beckner, who was Nagel's student, followed this trend in *The Biological Way of Thought* (1959), which presented a detailed reappraisal—and defence—of organicism. Beckner's book also addressed other topics (such as the nature of selection, the logic of taxonomic classification, and functional analysis) that would later be picked up with a vengeance by the new generation of philosophers of biology that went on to establish the discipline in the 1970s and beyond.

[9] Despite its importance in the historical development of modern biological thought, organicism has received surprisingly little attention from philosophers and biologists, and even from historians of science. For decades, the only detailed study of organicism available was Donna Haraway's *Crystals, Fabrics, and Fields: Metaphors of Organicism in Twentieth-Century Developmental Biology* (1976), which, despite its restricted scope, remains the standard reference work on the subject. More recently, Maurizio Esposito's *Romantic Biology, 1890–1945* (2013a) has provided a far more expansive survey of the organicist movement, but the impressive breadth of this study often comes at the expense of depth, and the author's quest to yoke organicism to the Romantic tradition in German biology—seemingly at all costs—is an unwelcome distraction. Aside from these book-length treatments, some noteworthy papers have examined aspects of the organicist movement, including Beckner (1967), Hein (1969), Abir-Am (1987, 1991), Smocovitis (1992), Gilbert and Sarkar (2000), Allen (2005), Etxeberria and Umerez (2006), Bruce (2014), and Peterson (2014).

appropriated by Haldane to describe his own views.[10] Although Haldane did much to popularize the word, not all of those who shared his general outlook made use of it in their writings. Ritter, for instance, seemingly unaware of Haldane's adoption of 'organicism' two years earlier, coined the more cumbersome term 'organismalism' in 1919.[11] Russell called it 'the organismal point of view', whereas Bertalanffy preferred to speak of the 'organismic conception'. Such terminological differences notwithstanding, all of these authors converged in their views, in spite of their diverse biological backgrounds, as well as their disparate geographic locations.[12]

Although there was no official compendium of beliefs upheld by all organicists—in fact, they often criticized one another—there are a number of recurring themes in their writings that enable us to legitimately speak of an 'organicist school' or an 'organicist movement'. At a most general level, as we have already noted, the organicists sought to dissolve the longstanding conflict between mechanicism and vitalism by advocating a 'third way' that would offer the best of both worlds. Organicism combined "the insistence of vitalism on the real complexity of life with the heuristic virtues of the mechanistic practical attack" (Needham 1936, p. 9), thereby vindicating "the autonomy of life, denied in the mechanistic conception, and remaining a metaphysical question mark in vitalism" (von Bertalanffy 1952, p. 20). More specifically, what united the organicists was a shared commitment to three general ideas, which we shall discuss in more detail below: (a) the centrality of the organism concept in biological explanation; (b) the importance of organization as a theoretical principle; and (c) the defence of the autonomy of biology.

4.1 The centrality of the organism concept in biological explanation

Organicists believed that the living organism constitutes the starting point of all biological theorizing and thus "the unit to which all biological concepts and laws must relate" (Russell 1930, p. 173). As Bertalanffy (1933, p. 49) put it, "the concept of organism occupies an analogous central position in biology to that which the concept of energy occupies in physics". Drawing inspiration from thinkers such as Aristotle, Immanuel Kant, and Claude Bernard, the organicists conceived the organism "as a specific whole, in which the parts and actions are essentially related to one another" (Haldane 1930, p. 97). They emphasized that the parts of an organism are "mutually constitutive of each other" (Ritter and Bailey 1928, p. 308)

[10] At the beginning of his Silliman Lectures at Yale University, published as *Organism and Environment as Illustrated by the Physiology of Breathing*, Haldane wrote: "It has been suggested to me that if a convenient label is needed for the doctrine upheld in these lectures the word 'organicism' might be employed" (Haldane 1917, p. 3).

[11] In relation to his coining of 'organismalism', Ritter remarked: "On behalf of this unauthorized and rather bungling word, I make no plea. […] My only concern is for the idea. If that survives and flourishes I shall be satisfied, no matter under what name it becomes known" (Ritter 1919a, pp. 28–29).

[12] As a final note on terminology, it is worth pointing out that the term 'holism' is sometimes used interchangeably with 'organicism' in the secondary literature. We think this is unhelpful, as it does violence to the etymology of these terms, and introduces unnecessary ambiguities. The word 'holism' was coined by Jan Smuts in his book *Holism and Evolution* (1926), and the views he expounds in that volume do not correspond to those of the organicists. Smuts' holism actually belongs to another school of thought of the same period, emergent evolutionism, which we will briefly discuss at the end of this section.

and that their activities "are directed to the maintenance, production, or restoration of the wholeness of the organism" (von Bertalanffy 1933, p. 8).

This general conception of the organism has two major methodological implications, which Russell described as the two 'laws of biological method'. The first law is that "[t]he activity of the whole cannot be fully explained in terms of the activities of the parts isolated by analysis" (Russell 1930, p. 147). That is to say, the organism exhibits properties that are missing when all of its parts are examined independently, and which are only visible upon consideration of the whole. This is another way of expressing the old dictum—often attributed to Aristotle—that 'the whole is more than the sum of its parts'. The organicists emphasized, however, that there is nothing mysterious about this assertion, as the term 'more' does not denote anything added *to* the parts (such as an entelechy), but refers simply to the complex organizing relations *between* the parts (see Needham 1936, p. 7).

The second law of biological method is that "[n]o part of any living unity and no single process of any complex organic activity can be fully understood in isolation from the structure and activities of the organism as a whole" (Russell 1930, p. 147). The reason for this is that "the whole enters always into the determination of the activities of the parts" (Woodger 1929a, p. 247), and consequently "[t]he organism in its totality is as essential to an explanation of its elements as its elements are to an explanation of the organism" (Ritter 1919a, p. 24). In other words, as the parts of an organism are mutually dependent upon one another, and "work together for the good of the whole" (Russell 1930, p. 168), their activities can only be understood in relation to their position in the whole. This accounts for why "the behaviour of an isolated part is, in general, different from its behaviour within the context of the whole" (von Bertalanffy 1952, p. 12; see also Woodger 1929a, p. 451).

The organicists' commitment to these two laws of biological method shaped their theoretical outlook. Among other things, it prompted them to reject particulate theories of inheritance, including the chromosome theory of the gene developed by Thomas Hunt Morgan (1919, 1926) and others at the time. In accordance with the first law,

> If we hold fast to the principle that the whole cannot be *completely* explained in terms of its parts, that the modes of action of higher unities may be conditioned, but cannot be *fully* accounted for, by the modes of action of lower unities, it follows that no substance and no sub-cellular unities can be invoked as *sufficiently* accounting for the phenomena of development and heredity, which are essentially phenomena manifested by whole organisms. (Russell 1930, p. 192, emphasis added; see also Ritter and Bailey 1928, pp. 320–324)

Moreover, in accordance with the second law, if the parts are subordinate to the whole, and their activity is determined by their relations to the whole, "there is absolutely no place for independent and isolated material units [within an organism] which represent or determine certain characters or groups of characters [of the whole]" (Russell 1930, p. 83; see also Haldane 1921, pp. 54–60; Woodger 1929a, pp. 361–370). For the organicists, hereditary units such as genes are "pure

abstractions" (Russell 1930, p. 150) which cannot be deemed responsible for the ontogenic generation of the properties and activities of the organism as a whole.[13]

Ultimately, the organicists believed that heredity would *not* be primarily explained with genetics (which deals with the statistical distribution of phenotypic variations across populations), but with development (which addresses the regular re-production of the phenotype in the individual organism, *taken as a whole*). And it is because they were acutely aware of the openness and plasticity of the developmental process (which they understood results from a confluence of internal and environmental factors) that they could not conceive it as being the result of the action of fixed or 'determined' hereditary particles (see Esposito 2013b). "[T]here is no need", Woodger declared, "to regard the zygote as a mystery-bag of 'potencies' which, at their appointed hour, proceed to blossom out into the 'characters' which they are said to be 'for'" (Woodger 1931, p. 203). In place of the preformationist view of the geneticists—where "chromatin takes the place of Descartes' God as the 'controlling mechanic'" (Woodger 1930a, p. 18)—the organicists attempted to provide an epigenesist account where organizational relations between the parts emerge progressively during ontogeny.

Organicism's emphasis on the integrated unity of the organism was also reflected in considerations of individual cells. An important organicist we have not yet mentioned, but who perceived more clearly than anyone else the systemic nature of the cell, was Paul Alfred Weiss.[14] Weiss viewed organisms, and cells in particular, as complex unitary organizations that tend to preserve their character in the face of external perturbations. In various writings, he discussed the two 'laws of biological method' in the specific context of cells. In an essay delightfully titled '$1 + 1 \neq 2$' (Weiss 1967), Weiss analysed the aforementioned dictum that 'the whole is more than the sum of its parts', noting that though the term 'more' is often interpreted as an algebraic addition, a living cell certainly does not have more content, mass, or volume than is constituted by the aggregate mass of molecules which comprise it. The 'more', Weiss argued, refers to the need to supplement the sum of statements

[13] This did not mean, however, that the gene concept should be dispensed with altogether, as it could still provide a useful service as a heuristic device in the explanation of development. What was important was to realize that "[t]he gene is a word, which enables a complicated happening to be briefly denominated" (Dembowski, quoted in Russell 1930, p. 67). As Russell explained, "[s]o long as the gene is regarded as a purely hypothetical concept invented for the specific purpose of explaining certain complicated facts of inheritance, no great harm is done. But when it is treated as a real existent body all [sorts of] factitious and gratuitous complications […] tend to set in" (Russell, p. 157; see also Woodger 1929a, p. 370).

[14] In fact, Weiss was, according to Bertalanffy, one of the first to introduce the concept of 'system' into biology in 1924 (Koestler and Smythies 1969, p. 47). But although Weiss developed his organicist outlook early on in his career (see Weiss 1925, 1926), in the ensuing decades he remained reticent to discuss his philosophical views in his publications, preferring instead to centre his attention on his empirical investigations (there are, of course, exceptions, such as Weiss 1940). As Weiss himself would later recount, for many years he chose to restrict his role to that of an "experimental explorer, interpreter, and integrator, for whom the 'system' concept remained simply a silent intellectual guide and helper in the conceptual ordering of experience" (Weiss 1977, p. 18). It was only toward the end of his career, from the 1960s onwards, that he resumed work on publically propounding his organicism (by then in the context of the burgeoning reductionistic claims of molecular biology). This is why many of his philosophical papers date from this period, despite the fact that most of the ideas that underlie them had been formulated almost half a century earlier.

that can be made about the individual parts by any such additional statements as are required to describe the collective behaviour of the parts, when taken as an organized whole. Cells cannot be fully described in terms of their molecular constituents, as this procedure fails to differentiate between a live cell and a dead one, or even between a dead cell and a homogenate of a physically disintegrated cell (Weiss 1963). Weiss also maintained that the whole cell exerts an influence over its parts, and cautioned against reductionistic assertions concerning the 'molecular control of cellular activity'. When we carefully scrutinize the empirical basis for such assertions, Weiss claimed, we "discover that the 'controlling' molecules have themselves acquired their specific configurations, which are the key to their power of control, by virtue of their membership in the […] organized cell, hence [these molecules are in fact] under 'cellular control'" (Weiss 1962, pp. 66–67).

4.2 The importance of organization as a theoretical principle

In addition to highlighting the central place of the organism in the explanation of biological phenomena, another common thread that runs through the organicist corpus is an intense preoccupation with the notion of organization. This is not surprising, as organisms are what they are by virtue of their organization.[15] It is organization—not matter—that is the very basis of life itself (Ritter and Bailey 1928, p. 318; Woodger 1929a, pp. 308–309; Needham 1936, p. 117). The term 'living substance' is a misnomer; "a pure abstraction, to which nothing corresponds in Nature" (Russell 1930, p. 165). Bertalanffy agreed: "[t]here is no 'living substance' because the characteristic of life is the organization of substances" (von Bertalanffy 1933, p. 48). The organicists also criticized the notion of 'protoplasm'—a term then popular among cytologists referring to the 'stuff of life'—for the same reason (e.g. Haldane 1921, p. 53). There is no such thing as protoplasm in nature. To proclaim that cells consist of protoplasm and leave it at that is to wilfully neglect any organization above the chemical level. Woodger illustrated this with a memorable analogy:

> If a large bomb is dropped upon a populous town we might apply the term 'town-plasm' to the debris which remained, but it would be a little absurd to say that towns were *composed* of such town-plasm, and that from a sufficient knowledge of such debris it would be possible to gain an adequate knowledge of the organization of towns. (Woodger 1929a, p. 294)

Weiss made a very similar claim decades later when he asserted that there is no phenomenon in a living system that is *not* molecular, but there is none that is *only* molecular either. "It is one thing not to see the forest for the trees", wrote Weiss referring to the cellular organization, "but then to go on to deny the reality of the forest is a more serious matter; for it is not just a case of myopia, but one of self-

[15] The organicists took this to be a self-evident assertion. As Woodger rhetorically remarked, "Is it not a bare analytical judgment (in the Kantian sense) to say that organisms are organized? Is not organization the very back-bone of the concept [of the] organism?" (Woodger 1929a, p. 290).

inflicted blindness" (Weiss 1969, p. 11). Organization, the organicists argued, is inescapable in any consideration of living systems.

Although all organicists stressed the importance of the concept of organization for biology, there is an important difference among them regarding the way in which they made use of this concept. The earlier generation (i.e. Ritter, Haldane, and Russell) considered it to be a sufficient explanation of a biological phenomenon to attribute it to the organization of the system in question. The later generation (i.e. Woodger, Weiss, Needham, and Bertalanffy) refused to take comfort in the mere invocation of organization, and considered it of the utmost importance to investigate what organization actually consists of. Needham explicitly drew this distinction in his organicist treatise *Order and Life*, in which he described the thought of the older generation as "'dogmatic' organicism", and branded the work of the younger generation (to which Needham himself belonged) "'legitimate' organicism" (Needham 1936, p. 18).

This distinction explains why Needham was so often critical of Haldane (e.g. Needham 1925a, b, 1928a, 1930b, 1936). In an essay Needham wrote in honour of Haldane after he died, he stated that "Haldane's great service to biological theory was the way in which he persistently called attention to the special form of organisation existing in living things. On the other hand", Needham continued, "his great failure consisted in his defeatist wish to accept this principle of organisation as axiomatic" (Needham 1943, p. 124). Indeed, in *The Philosophical Basis of Biology*, Haldane had dubbed the existence of the living organization "the fundamental axiom of biology" (Haldane 1931, p. 31), which "we cannot define in terms of anything simpler" (ibid., p. 110). Along very similar lines, Russell claimed that the "fundamental unity of the developing organism […] is not susceptible to complete explanation, and must be accepted as an irreducible postulate of biology" (Russell 1930, p. 6; see also Russell 1933, p. 155). Unsurprisingly, Needham did not find Russell's views on organization all too congenial either.[16]

But Needham was not the only one to protest that organization was "a problem to be solved and not a magic word conferring upon biologists a licence to cease from experiment and from thought" (Needham 1932, p. 92). All of the younger, 'legitimate organicists' emphasized the need to treat organization as the *explanandum* rather than as the *explanans*. Woodger observed that "[i]f the concept of organization is of such importance as it appears to be it is something of a scandal that biologists have not yet begun to take it seriously", adding that "[t]he first duty of the biologist would seem to be to try and make clear this important concept" (Woodger 1929a, p. 291). Likewise, Bertalanffy asserted that organization is "neither a mysterious entity nor a refuge for our ignorance, but a fact that can and must be dealt with by scientific methods" (von Bertalanffy 1952, p. 20). Weiss, for his part, asserted in the context of cell biology that "[t]here are now available practical and constructive approaches to the gradual replacement of symbolic

[16] In private correspondence with Woodger, Needham wrote: "Your letter made me feel what I always feel about Ludwig [von] Bertalanffy, namely, that in all probability we could come to agreement after a good verbal discussion—a feeling I never have, for instance, about E. S. Russell" (Needham to Woodger, 12th October 1929, Needham papers, Cambridge University Library).

references to 'organisation' by true insight into the dynamics involved" (Weiss 1962, p. 67).

One important aspect of biological organization that all of these organicists emphasized is its hierarchical nature (Woodger 1930a, b, c, 1931; Needham 1936; Weiss 1939, 1971; von Bertalanffy 1952). The hierarchical order of organisms enables us to distinguish—as we progressively move down the scale of organization—parts composed of cells, the cells themselves, parts of cells, molecular assemblies of parts of cells, and so on.[17] Organicists argued that the principles which govern the behaviour of the parts at each level of organization cannot be deduced from principles that apply lower down (or higher up) the hierarchy. The methodological implication of this is that each level of organization needs to be investigated on its own terms. To take a concrete example, in order to understand the behaviour of cells during morphogenesis, it is necessary to develop a theory about how *cells* interact during development and not be satisfied with an account that deals exclusively with relations between molecules. Moreover, organicists recognized the importance of understanding how the spatial hierarchy changes as the organism develops through time. They understood that only by considering the progressive development of biological organization would it be possible to come to terms with the characteristic hierarchical order of living systems.

4.3 The defence of the autonomy of biology

A third defining theme of the organicist movement was its concern with defending the autonomy of biology with respect to the physical sciences. Earlier in the paper, in our analysis of Wolters' theses of logical empiricism, we showed that Woodger and Bertalanffy both maintained that biology constituted an independent field of inquiry that would only progress by developing its own theoretical foundations. They were not alone in this regard. All of the organicists shared the conviction that the distinctiveness of organisms demanded a unique set of theoretical tools for their elucidation.

The organicists' unwavering commitment to the autonomy of biology is the reason why most of them made the machine conception of the organism of mechanicism a primary target of their criticisms (e.g. Haldane 1884; Russell 1924, pp. 2–8; Woodger 1929a, pp. 451–452; von Bertalanffy 1933, pp. 36–38; Weiss 1973, pp. 38–41). Although organisms can sometimes be studied *as if* they were complex machines, they are not *really* machines, as they exhibit capabilities such as growth, reproduction, and regeneration—all by virtue of their distinct kind of organization—that have no counterpart in the machine realm. Consequently, they cannot be fully explained in mechanical terms. Mechanicists may, say, investigate hormonal secretion by measuring its mass, volume, or chemical composition.

[17] The organicists noted, however, that not all parts belong to an organism's hierarchy simply by virtue of being parts. Woodger made this point by distinguishing between an organism's 'components' and its 'constituents' (Woodger 1930b, p. 457). Components are functionally defined parts of the spatial hierarchy (e.g. the nucleus is a component of a cell) whereas constituents are either parts which lie outside the hierarchical order (e.g. the extracellular matrix) or arbitrary parts taken without regard to the hierarchical order (e.g. a beef steak).

Similarly, they may investigate muscular contraction by measuring the rate and extent of contraction, or by the accompanying heat production. But in all of these instances, the phenomena observed are always mechanical or physicochemical. A *biological* understanding of these phenomena requires that they are interpreted in the context of the organismic organization that makes them possible, and this can only be done through the introduction of concepts not found in classical physics and chemistry.

The earliest organicists, such as Haldane and Ritter, articulated their defence of the autonomy of biology by flatly denying the possibility of a physicochemical explanation of life. In 1908, Haldane stated that "[t]hose who aim at physico-chemical explanations of life are simply running their heads at a stone wall, and can only expect sore heads as a consequence" (Haldane 1908, p. 696). Ritter echoed Haldane's sentiment a year later, remarking that "[t]he presumption that biological phenomena may be adequately treated in terms of chemistry and physics […] leads inevitably to a forcing of evidence" (Ritter 1909, p. 177).[18] Russell made the case for biology's autonomy slightly less dogmatically, conceding that:

> [W]hile many of the phenomena presented by living things are […] to be explained as the direct result of simple physical and chemical relations, there still remain a vast number of facts of life which cannot be explained by any direct reference to chemical laws. They present truly biological problems which can be solved only by biological laws. (Russell 1911, p. 332)

Russell's view, then, is not that physicochemical principles do not apply to biological phenomena, or even that they are not useful in biology; in fact, "no one would dream of denying the validity and value of biochemical and biophysical research" (Russell 1945, p. viii). It is simply that they do not suffice in providing a complete explanation of what makes organisms distinctive. As we saw earlier, this was precisely the position of Woodger and Bertalanffy as well. It is what prompted both of them to call for the articulation of a 'theoretical biology'—a call also made by Needham (1932, p. 84)—on the basis of what empirical biology tells us about organisms.[19] The same commitment to the autonomy of biology was expressed by Weiss many years later, when he asserted that "[b]iology must retain the courage of its own insights into living nature; for, after all, organisms are not just heaps of molecules" (Weiss 1969, p. 400).

[18] The similarity between these two thinkers was noticed by Roy Wood Sellars in his *Evolutionary Naturalism*, where he observed that "Haldane and Ritter, who would not call themselves vitalists, […] challenge the adequacy of physics and chemistry, as these are ordinarily understood, as means of explaining biological processes" (Sellars 1922, p. 325).

[19] Note that the organicists' usage of the term 'theoretical biology' is quite far removed from the way it is understood today, and corresponds more closely to our modern conception of philosophy of biology. This is because in the early decades of the twentieth century no clear distinction existed between 'theoretical' and 'philosophical' examinations of biology. For the organicists, the two always went together—the task of systematizing biological knowledge through an analysis of its conceptual foundations was simultaneously a theoretical *and* a philosophical undertaking. It was only later (particularly after the Second World War) that the term 'theoretical biology' lost its philosophical undertones and began to be used almost exclusively in the context of the mathematical modelling of biological processes.

4.4 The organicist community in the early twentieth century

Having completed our examination of some of the major themes of organicist thought, it is worth considering in a bit more detail the organicist *community* as it existed in the early decades of the twentieth century. In geographical terms, it is possible to distinguish three major groups of organicists: one in Great Britain, another in Austria and Germany, and a third in the United States.[20]

The British faction was spearheaded by Haldane, whose pivotal contributions to the physiology of respiration at the turn of the twentieth century enabled him to speak with considerable authority on philosophical issues in biology. Russell was deeply influenced by Haldane, and so were Woodger and Needham, who regarded their respective contributions to biological theory as a further development of (and, particularly in the case of Needham, also a reaction to) Haldane's ideas. Haldane, in turn, was familiar with the writings of the younger generation.[21] In the 1930s, Woodger and Needham became instrumental in promoting organicism in Great Britain. Together they founded the 'Theoretical Biology Club', an exclusive collective of biological thinkers inspired by Whiteheadian philosophy (especially Whitehead 1925), which held regular meetings between 1932 and 1938 to discuss organicist themes from a variety of disciplinary perspectives (see Abir-Am 1987, 1991; Peterson 2014). Aside from Woodger and Needham, other core members included embryologist Conrad Hal Waddington, crystallographer John Desmond Bernal, and biomathematician Dorothy Maud Wrinch. Out of these, Waddington in particular would revisit organicist themes during much of his later career, especially in the context of his visionary efforts to bring about the theoretical unification of development, genetics, and evolution (see Peterson 2011).

Bertalanffy and Weiss, both Viennese, are the most familiar of the German-speaking organicists (at least among Anglophone scholars). Vienna was an important centre of organicist biology, owing in large part to the groundbreaking work conducted at the *Biologische Versuchsanstalt* under the directorship of Hans Przibram (see Coen 2006; Logan and Brauckmann 2015). However, the roots of German organicism can be traced back to the Jena-based embryologist Julius Schaxel. Following the outbreak of the First World War, Schaxel was forced to abandon experimental research, and he began analysing the basic presuppositions of competing biological theories; an enterprise he dubbed 'critical biology'.[22] Schaxel published the results of his philosophical examinations in his *Grundzüge der Theoriebildung in der Biologie* (Schaxel 1919), where he criticized both Roux's *Entwicklungsmechanik* (for presupposing a machine conception of the embryo on a priori grounds) and Driesch's neo-vitalism (for postulating an unknown agent that

[20] There were, of course, supporters of organicism in other countries, such as France and Italy, but the intellectual movement was primarily developed by authors belonging to these three groups.

[21] For example, Haldane's *The Philosophical Basis of Biology* included a supplement with detailed, and mostly favourable, reviews of Woodger's *Biological Principles* and Russell's *The Interpretation of Development and Heredity*. Haldane concluded his review by noting that "[t]he books of Mr. Woodger and Dr. Russell represent critical and constructive efforts to refashion biology on a more secure theoretical basis" (Haldane 1931, p. 165).

[22] This term was later adopted by Woodger (1929a) and Bertalanffy (1930a), among others.

obstructed biological understanding). Hoping to recruit others to the cause of critical biology (which he rebranded as 'theoretical biology'), Schaxel started editing a book series titled *Abhandlungen zur theoretischen Biologie*, which between 1919 and 1931 published 31 monographs, many of which advocated an organicist viewpoint (cf. Woodger 1930a: pp. 5–6; Laubichler 2001; Reiß 2007). Indeed, it is no coincidence that Weiss and Bertalanffy both published their first books (i.e. Weiss 1926; von Bertalanffy 1928) as part of Schaxel's *Abhandlungen*. Other important proponents of organicism in the German-speaking world included the botanist Emil Ungerer (1926), the zoologist Friedrich Alverdes (1932), the neurologist Kurt Goldstein (1934), and the theoretical biologists Jakob von Uexküll (1926) and Adolf Meyer (1934), both of whom lectured at the University of Hamburg.[23]

At the other side of the Atlantic, the primary advocate of organicism was Ritter. The same year Schaxel completed his *Grundzüge* and began editing the *Abhandlungen*, Ritter published *The Unity of the Organism, or the Organismal Conception of Life* (Ritter 1919a, b). Ritter also promoted organicist principles through the research agenda of the Scripps Marine Institution (now Scripps Institute of Oceanography) he founded in San Diego, California. Another important proponent of organicism in the United States was the physiologist Charles Manning Child (1915a, b), who Ritter first met at the *Stazione Zoologica* in Naples in 1894. The two became close friends and developed converging philosophical views. As Ritter acknowledged in his magnum opus, "Child formulates views of the nature of organisms that agree very well with the organismal standpoint upheld in this volume" (Ritter 1919b, p. 111). Child spent most of his career at the University of Chicago, where an organicist approach had developed following the appointment of the zoologist Charles Otis Whitman in 1892. Whitman's writings—especially his essay 'The Inadequacy of the Cell-Theory of Development' (1893), which criticized atomistic accounts of morphogenesis and defended a position he described as 'the organism-standpoint'—inspired an entire generation of biologists at Chicago that included Child, the aforementioned Wheeler, and the brothers Frank Rattray Lillie and Ralph Stayner Lillie. While Frank was practically-minded and sought to implement organicist ideas in his embryological investigations into the process of fertilization, Ralph was a prolific theorist who published a large number of articles in specialist philosophy journals, including *The Journal of Philosophy* and, later, *Philosophy of Science* (e.g. Lillie 1926, 1932, 1934, 1937, 1940, 1942a, b, 1948).[24]

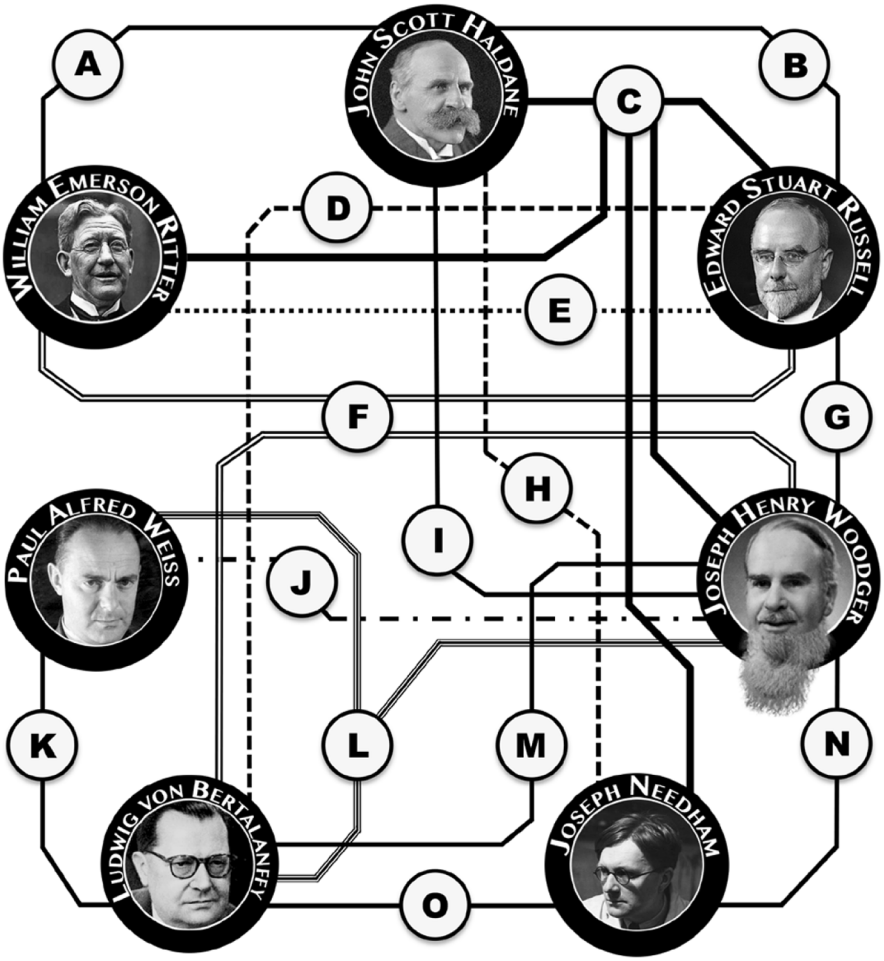
In spite of the geographical distance separating them, organicists belonging to different factions were well aware of each other's work, and interacted with one another in a variety of different ways: they engaged in extensive correspondence, collaborated on joint projects, discussed each other's ideas in their publications, and met privately and at conferences to discuss matters of common interest. Intellectually, they deeply influenced one another. Figure 1 maps the complex web of personal, professional, and intellectual connections linking the seven major

[23] The eminent philosopher Ernst Cassirer was also based in Hamburg during the same period. It is perhaps not surprising, then, that he wrote a monograph partly devoted to the philosophy of biology (i.e. Cassirer 1950) in which the influence of organicist ideas is quite apparent.

[24] For a detailed examination of American organicism, see Esposito (2013a, chaps. 5 and 6).

organicists we have discussed in this paper, namely Haldane, Russell, Woodger, Needham, Bertalanffy, Weiss, and Ritter. As the figure clearly shows, these authors did not work in isolation, but constituted a densely interconnected network. Organicism must therefore be understood as a distinct *discourse* with a core set of topics that were addressed by an international community of scholars who read, discussed, and responded to one another's work. Despite what modern commentators have frequently claimed, the philosophy of biology was already a well-established field of research a good half century before the generation that supposedly created the discipline started working on the subject.

Figure 1. The Organicist Movement in Early Philosophy of Biology: Connections and Influences



◀**Fig. 1** Social network detailing personal, professional, and intellectual connections between the major organicist philosophers of biology discussed in this paper. Each lettered node corresponds to a particular link, which is sometimes exemplified by an illustrative quotation:

(**A**) Ritter (1918, p. 127) praises Haldane's efforts "to raise the term organism to the level of a category", and describes Haldane's philosophical interpretation of respiration as "pronouncedly organismal" (Ritter and Bailey 1928, p. 325).
(**B**) Russell (1924, p. vii): "I owe much to the writings of Dr. J. S. Haldane".
(**C**) In 1931 Ritter chairs a session at the second International Congress of the History of Science titled 'Historical and Contemporary Interrelationships of the Physical and Biological Sciences', whose participants include Haldane, Russell, Woodger, and Needham (Ritter 1933).
(**D**) Russell (1952, p. 748): "Bertalanffy's brilliant exposition of the organismal theory of life […] is indispensable for all those, whether biologists or philosophers, who concern themselves with the fundamental problems of organic life".
(**E**) Russell (1930, p. 177): "It will be seen that Ritter's point of view is essentially the same as that taken here, though he has arrived at it by a different route. I take this opportunity of acknowledging my indebtedness to Ritter".
(**F**) Bertalanffy to Ritter in 1931: "It is very nice on your part to bring to my attention Russell's *Interpretation of Development* [*and Heredity*]. I knew that book already, Dr. Woodger sent me a copy of it. While I was reading it, I was quite amazed by how similar the book is—in content and order—to my *Kritische Theorie der Formbildung* […] [T]his is for me a remarkable proof of the 'publicity of the case' and, again, it proves how much our 'organismic' train of thought is in the air today. You will be further convinced of the striking parallelism between Russell's book and mine once it comes out in English, which, as you know, will happen very soon" (quoted in Esposito 2013b, p. 168).
(**G**) In the preface of *Biological Principles*, Woodger (1929a, p. 9) thanks Russell "for reading the manuscript from the biological point of view and making a number of helpful criticisms".
(**H**) Though critical of Haldane's 'dogmatic' form of organicism, after Haldane's death Needham writes an essay in his honour acknowledging his influence (Needham 1943, chap. 6).
(**I**) Haldane (1931, pp. 149–150): "Mr. Woodger's book on *Biological Principles* is mainly critical, and as he carries me with him in nearly all his criticisms, and his references to my own writings are very friendly, I can only express the hope that his book will be widely read".
(**J**) Weiss (1969, p. 43) notes that the elucidation of the part-whole relation in organisms "started in the 'Woodger days' at Cambridge", adding that "[l]ogically he [i.e. Woodger] has been absolutely correct".
(**K**) Weiss and Bertalanffy meet in Viennese coffeehouses throughout the 1920s to discuss the need for systemic approaches in biology. As Weiss (1977, p. 18) later recounted, "I soon found that [Bertalanffy's] thinking and mine moved on the same wave-length—his coming from philosophical speculation, mine from logical evaluation of practical experience".
(**L**) In 1926 Woodger spends a semester at the *Biologische Versuchsanstalt* in Vienna, where he discusses the conceptual foundations of biology with Weiss. He also befriends Bertalanffy during his stay.
(**M**) Woodger and Bertalanffy collaborate closely on the English adaptation of the latter's *Kritische Theorie der Formbildung*, published as *Modern Theories of Development* in 1933. Years later Bertalanffy visits Woodger in London and the two work together on philosophical issues.
(**N**) In 1928 Woodger and Needham begin an intense correspondence. A year later Needham invites Woodger to speak at his Cambridge college, and following the publication of Woodger's *Biological Principles*, the two publically engage one another in the journal *Mind* (Needham 1930c; Woodger 1930c). In 1932 they jointly create the Theoretical Biology Club, which meets to discuss organicist themes.
(**O**) Needham, both in his publications and in his private letters (see, e.g., fn. 16), praises Bertalanffy's work. Bertalanffy (1930a) examines Needham's philosophical views in considerable depth. The two correspond extensively between 1928 and 1939.

Organicism reached its peak in the late 1920s and early 1930s. The movement began to lose momentum in the late 1930s for a variety of reasons. The older generation simply died out. Haldane's last book, *The Philosophy of a Biologist*, was published in 1935, and he died just one year later. Ritter died in 1944, but he had retired over a decade earlier. Russell continued to write on organicist themes after the 1930s (e.g. Russell 1945, 1950) but at a much slower pace. What is remarkable about the younger generation is that—with the exception of Weiss (see fn. 14)—they all drifted into other areas of research. Woodger became increasingly preoccupied with the axiomatization of biological theories, and redirected his attention towards the clarification of biological concepts using formal logic (e.g. Woodger 1937, 1939, 1952, 1959). Needham developed an intense fascination with China after meeting three visiting Chinese scientists in 1937, and immersed himself in the study of Chinese culture and technology, eventually becoming the world-leading historian of Chinese science. Finally, Bertalanffy grew ever more concerned with the elaboration of the transdisciplinary research program he called 'General System Theory', which sought to generalize the organicist principles acting on living systems to all the various kinds of systems studied in the natural and social sciences.[25]

Overall, this section has shown that there was a fully-fledged philosophy of biology discourse—as well as an international community of philosophers of biology—well before the fathers of the modern discipline were even born. Moreover, we have seen that this discourse was shaped by the distinct agenda of organicism. We wish to emphasize the fact that organicism is *not* a historiographical label we have concocted in order to conveniently lump together a host of thinkers under a single heading, but a term that was actually adopted by most of the historical actors we have considered, and which referred to a *real* intellectual movement that developed concurrently in Great Britain, Continental Europe, and the United States between the First and Second World Wars. The same can be said, incidentally, of the phrase 'philosophy of biology', which is often incorrectly assumed to be of recent coinage. A clear illustration of both of the preceding points is a review article published by Needham in 1928, revealingly titled 'Recent Developments in the Philosophy of Biology'. In this paper, Needham declares that "[t]he theoretical standpoint that can best be called 'Organicism' is probably the most important factor in these considerations at the present time" (Needham 1928b, p. 77). To mention just one more example, in his book-length survey of early philosophy of biology, William McDougall writes that "[t]here is now a large

[25] It is rather disconcerting that Woodger, Needham, and Bertalanffy are now remembered for their later pursuits, rather than for the instrumental role they each played in establishing an organicist philosophy of biology during the interwar years. Woodger is known—and derided—for his axiomatic work, but his incisive non-formal examinations of biology are almost never discussed (see Nicholson and Gawne 2014). Needham has become renowned for his encyclopedic multi-volume work *Science and Civilisation in China*, whilst his extensive philosophical writings (not to mention his scientific contributions) have been entirely forgotten. And Bertalanffy is often acclaimed as one of the founding fathers of systems theory, yet he is seldom recognized as the pioneering theoretical biologist and philosopher of biology that he was. We believe that the skewed legacy of these three authors (coupled with the unfortunate fact that Haldane's legacy has been totally eclipsed by that of his more famous son, J. B. S. Haldane) helps explain the almost complete erasure of organicist philosophy of biology from the collective memory.

number of biologists who give adhesion to what is called 'organicism' or the organicist or organismal or organismic doctrine" (McDougall 1938, p. 149).

Of course, not all early twentieth-century philosophers of biology were organicists. There were critics of organicism—such as Lancelot Hogben (1930)—as well as proponents of other schools of thought that also rose out of the mechanicism–vitalism dispute. One such school was *emergent evolutionism*. The chief advocates of emergent evolutionism, namely Samuel Alexander (1920), Conwy Lloyd Morgan (1923), and Smuts (1926), claimed that the organism concept does not apply exclusively to living beings, and that it should be adopted to refer to the entire spectrum of integrated unities existing in nature (Whitehead formulated a similar argument in his *Science and the Modern World*). For the emergent evolutionists, the very notions of life and mind ought to be abandoned in favour of the concept of whole, whose domain of applicability ranges all the way from inorganic substances to human societies.[26] An additional school of thought that sprung from the mechanicism–vitalism dispute was *dialectical materialism*, which drew on the writings of G. W. F. Hegel, Karl Marx, and Friedrich Engels, and argued that all of nature is in a state of perpetual transformation. In this view, the living domain represents a qualitatively new dialectical level that needs to be examined on its own terms. Up until the 1920s, dialectical materialism developed mostly in the context of Soviet biology, but it later became known in Britain after a delegation of Soviet scientists expounded the doctrine at the second International Congress of the History of Science in London in 1931.[27] Prominent exponents of dialectical materialism included Prenant (1938), J. B. S. Haldane (1938), and Aleksandr Oparin (1961).

5 Conclusions

For the past forty years, there has been near complete agreement that early twentieth-century work in the philosophy of biology was irreparably flawed. Indeed, this claim has become so common that it is generally treated as a truism that requires no corroboration. Those who have taken the time to provide evidence in support of this unflattering assessment have often suggested that the defects of the early twentieth-century literature can be attributed to the fact that the work carried out during this period was heavily influenced by two 'disreputable' intellectual movements: logical empiricism and vitalism. In this paper, we have attempted to undermine the belief that early philosophers of biology failed to produce anything of value by arguing that the most prominent intellectual tradition of the era was neither logical empiricism nor

[26] The organicists' reaction to the views of the emergent evolutionists varied considerably. Some, like Ritter and Russell, were broadly sympathetic (Ritter and Bailey 1928, pp. 333–334; Russell 1930, p. 179). Others, like Woodger and Haldane, were mostly critical (Woodger 1929a, pp. 105–110; Haldane 1931, pp. 38–39). Needham and Bertalanffy, for their part, adopted a neutral stance (Needham 1928a, pp. 33–35; von Bertalanffy 1933, p. 52).

[27] This congress was also attended by Ritter, Haldane, Russell, Woodger, and Needham (Ritter 1933; see also Abir-Am 1985). In fact, Needham subsequently became quite enamored with dialectical materialism, admitting in his *Order and Life* that "the standpoint which follows from its fundamental propositions is closely similar" to his own organicism (Needham 1936, p. 45). A few years later, Needham would write the foreword to the English translation of Marcel Prenant's *Biology and Marxism* (1938).

vitalism but organicism, which had little connection to logical empiricism, and was explicitly conceived as a *reaction to* vitalism. Given this disconnect, any negative connotations associated with logical empiricism or vitalism should not be transferred to early twentieth-century work in the philosophy of biology. Organicism must be judged according to its *own* philosophical and scientific merits, not those of intellectual traditions which were, at best, tangentially related.

The stigmatization of early philosophy of biology has had a number of unfortunate consequences, the most notable being that the intellectual labours of several generations of thinkers have been almost completely lost to history. The response we have offered in this paper to the various condemnatory remarks that have been made over the years is merely a single step in what will necessarily be a multi-stage reappraisal. It is our hope that, as a result of the analysis we have presented here, philosophers of biology will no longer feel inclined to dismiss the rich history of their discipline. The question remains, however, as to whether the writings of Haldane, Ritter, Russell, Woodger, Needham, Bertalanffy, and Weiss can be meaningfully integrated into the contemporary conversation. To put it slightly differently, could work produced three quarters of a century ago be of any relevance to current research? The short answer is 'Yes!'. As this paper has demonstrated, there is palpable, even obvious, thematic continuity between the organicist discourse and the contemporary literature (for an illustrative review, see Nicholson 2014). In this concluding section, we want to draw attention to the methodological continuity which also exists between the old and the new philosophies of biology in order to quell any lingering doubts about the feasibility of the unification project we have proposed.

In a famous paper titled 'What Philosophy of Biology Is Not', which is now widely regarded to have precipitated the advent of the modern discipline, Hull (1969) reflected on what he took to be the dismal state of the field, famously suggesting that work completed by previous researchers should serve as an example of how *not* to philosophically engage with the life sciences. Hull made no attempt to hide his disappointment in the concluding remarks of his essay:

> In conclusion, there are many things that philosophy of biology might be. A philosopher might uncover, explicate, and possibly solve problems in biological theory and methodology. He might even go on to communicate these results to other philosophers, to scientists, and especially to biologists. He might show what consequences biological phenomena and theories have for other sciences and for philosophy or to show what consequences other sciences and even philosophy have for biology. These are some of the things which philosophers of biology might do. With rare exception, they have not. […] It must be admitted that thus far [philosophy of biology] is not very relevant to biology, nor biology to it. (Hull 1969, pp. 178–179)

These remarks paint a very bleak picture of the state of the subject, but they were constructive in that Hull's intention was to provide a direction for future work in the discipline. Writing in 1969, Hull took the philosophy of biology to be a field of unrealized possibilities. Most would agree that the ship has been righted since Hull's time. However, if this is the case, then the field has *not* undergone a radical developmental transformation, but has simply experienced a return to form.

The criticisms Hull developed in his paper are primarily methodological. His main objective was to show that contemporary work in the field was insufficiently informed by the most recent findings of biological science, or was communicated in a way which made it nearly inaccessible to the broader biological community. Interestingly, the organicists, with the exception of Woodger, receive no mention whatsoever. Without wanting to belittle the instrumental role that Hull played in the development of modern philosophy of biology, we cannot help but wonder that, had Hull made a greater effort to 'study the literature of the subject' (as per Wheeler's recommendation in the epigraph of this paper), he almost certainly would have come away with a decidedly different impression of the field's accomplishments.

We can exemplify this point by referring to another survey of the field written by Needham forty years before Hull's, the aforementioned 'Recent Developments in the Philosophy of Biology'. What is interesting about this paper is that Needham describes the field's methodology and its relationship to biology in ways that very closely resemble Hull's utopian image:

> The immutability of philosophical thought meets the changefulness of the science of biology in that difficult region, which is yet so attractive, the abstract aspects of the study of living things. The philosophy of biology, as it is usually called, is a most unsatisfactory name, for it implies that the philosopher may be defined as the reverse of the specialist who knows continually more and more about less and less. But philosophy is to be understood here […] as part of the theory of scientific investigation as a whole, [and] the self-criticism of the scientific method. (Needham 1928b, p. 77)

The organicists were leading authorities in biology who turned their attention to philosophical problems in their science, as opposed to philosophers with a passing interest in biology—which were Hull's main target in his paper. For this reason, there should be no question as to whether the organicist literature Hull omitted in his survey was scientifically informed. In his review, Needham described the philosophy of biology as a wide-ranging field of research, encompassing an array of different perspectives and approaches, but he made a point to emphasize that even those who choose to ply their trade in the most remote corners of the field must keep pace with the latest empirical research. In his own words, "[u]nlike philosophy proper, the theory [i.e. philosophy] of biology is by no means independent of experimental results" (ibid.). Needham (and all the other organicists, for that matter) would have heartily agreed that the philosophy of biology ought to be all of the things Hull mentions in the quoted passage above. However, Hull's failure to engage with their work has given subsequent generations of readers the impression that it can be safely cast aside with all the other refuse that fell under his rubric of 'what philosophy of biology is not'. We hope to have shown this to have been a very regrettable mistake.

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