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# Adolf Meyer-Abich, Holism, and the Negotiation of Theoretical Biology

## Abstract

Adolf Meyer-Abich (1893–1971; known as Adolf Meyer before 1938) spent his career as one of the most vigorous and varied advocates in the biological sciences. Primarily a philosophical proponent of holistic thought in biology, he also sought through collaboration with empirically oriented colleagues in biology, medicine, and even physics (including C. J. van der Klaauw, Karl K<sup>o</sup>tschau, Hans B<sup>o</sup>cker, Jakob von Uexk<sup>u</sup>ll, and Pascual Jordan) to develop arguments against mechanistic and reductionistic positions in the life sciences, and to integrate them into a newly disciplinary theoretical biology. He participated in major publishing efforts including the founding of *Acta Biotheoretica*. He also sought international contacts and worked for long stretches in Chile, the Dominican Republic, El Salvador, and the United States. His career straddled the Nazi period, which led him into a complex dance of support for and resistance to the regime. Despite the relative failure of his conceptual innovations (e.g., “holobiosis” and “holistic simplification”) to catch on, his ideas and writings sit squarely within the trajectory of thought and argument that has led to today’s reinvigoration of thought about conceptual integration in evolutionary developmental biology.

## Keywords

*Acta Biotheoretica*, Adolf Meyer-Abich, Bios, conceptual integration, development, evolution, holism, holistic biology, holistic simplification, holobiosis, theoretical biology

## Disciplines

European History | European Languages and Societies | German Language and Literature | History of Science, Technology, and Medicine

## Comments

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## Historical Essay

# Adolf Meyer-Abich, Holism, and the Negotiation of Theoretical Biology

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### Abstract

Adolf Meyer-Abich (1893–1971; known as Adolf Meyer before 1938) spent his career as one of the most vigorous and varied advocates in the biological sciences. Primarily a philosophical proponent of holistic thought in biology, he also sought through collaboration with empirically oriented colleagues in biology, medicine, and even physics (including C. J. van der Klaauw, Karl Kötschau, Hans Böker, Jakob von Uexküll, and Pascual Jordan) to develop arguments against mechanistic and reductionistic positions in the life sciences, and to integrate them into a newly disciplinary theoretical biology. He participated in major publishing efforts including the founding of *Acta Biotheoretica*. He also sought international contacts and worked for long stretches in Chile, the Dominican Republic, El Salvador, and the United States. His career straddled the Nazi period, which led him into a complex dance of support for and resistance to the regime. Despite the relative failure of his conceptual innovations (e.g., “holobiosis” and “holistic simplification”) to catch on, his ideas and writings sit squarely within the trajectory of thought and argument that has led to today’s reinvigoration of thought about conceptual integration in evolutionary developmental biology.

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“The goal of my scientific life [is] the promotion of theoretical biology. . .” (D30).<sup>1</sup> So wrote Adolf Meyer-Abich (1893–1971)<sup>2</sup> in a curriculum vitae prepared contemporaneously—likely in 1935—with his answers to the extensive questionnaire required of all civil servants in Germany by the Nazi government’s Law for the Restoration of the Civil Service of April 7, 1933. Despite the apparent simplicity of this formulation of his intentions, it reveals a central moment in Meyer-Abich’s understanding of scientific practice: “promotion” is an integral part of science, and must be pursued as an element of a “scientific life.” Throughout his career, particularly between 1930 and 1965, he pursued such promotion with great vigor—and with theoretical biology directly or indirectly implicated in all of his work—because he acknowledged no explicit boundaries between empirical investigation, theoretical structures, and the persuasive propagation of knowledge about the living world. For him, theoretical biology functioned as the turntable linking investigational practices and data in the life sciences with the sphere of concepts that could be deployed persuasively as reliable scientific knowledge. As a career-conscious scholar and colleague, he further attempted to make himself into the linchpin of several international networks of scientific investigators and institutions. In all these areas of his activity, Meyer-Abich’s successes remained ambiguous and his failures were many. His vision of theoretical biology also remained wedded to a limited number of his own conceptual innovations in holistic thought and argument, particularly the ideas of “holobiosis” (*Holobiose*), a holistic unification of symbiotic organisms from which emerges the potential for macroevolutionary change, and “holistic simplification” (*holistische Simplifikation*), according to which “simpler” physical and chemical laws and concepts can more easily be derived from higher-order biological theory rather than vice versa.<sup>3</sup> Recent historical literature on both theoretical biology and biomedical holism has noted Meyer-Abich’s contributions to various spheres of biological thought and practice, but his presence remains a ghostly one, poorly elaborated and without exploration of the scale and scope of his vigorous scientific and publicistic activity (Trienes 1988; Harrington 1996; Lawrence and Weisz 1998; Moynahan 1999; Laubichler 2001; Hossfeld and Olsson 2003).

Despite Meyer-Abich’s hopes that he could both demonstrate logically how holistic ideas made sense and discover empirical evidence for them, he ultimately persuaded relatively few colleagues of their significance. Nonetheless, he contributed energetically to the propagation of international biological thought and practice about evolution, development, theory-building, and conceptual change. He also reflected extensively and thoroughly on his own practice, and throughout his career kept scrupulous and elaborate records of his activities (Meyer-Abich 1969). He therefore deserves a central place in the contested and halting trajectory of the negotia-

tion of the practices and ideas that inform today’s evolutionary developmental biology. An analysis of his work provides evidence crucial to understanding the “acrimonious relations” in “conceptually isolated” biological disciplines during the mid-20th century (Laubichler 2007a: 21) and thus to the “conceptual block” and “dialectics” within and between dynamic fields of the life sciences (Allen 2007: 151). In many ways, Meyer-Abich’s difficulties were the difficulties of theoretical biology itself in the middle of the 20th century. Both his successes and failures point to central issues in the international construction of the field, issues that retain—especially with respect to the problem of conceptual integration within and across biological fields—a central place among the problems that theoretical biology is today striving with renewed vigor to surmount (Laubichler 2007b).

Among the biological scientists and scholars in the German-speaking world during the mid-20th century, Meyer-Abich pursued what he called scientific “promotion” with a collaborative energy and will that had few, if any, equals. Two approaches he used reveal important consequences for the analysis of scientific practice and the relationships between investigation, knowledge, and persuasion in the history of the life sciences. First, he dedicated the majority of his own published scholarly work to historical, logical, and (limited) empirical attempts to clarify and integrate the concepts through which biological knowledge is represented and communicated. Second, he tirelessly sought to develop new collaborative and international venues for the dissemination of theoretical biology through publication and teaching. One of these still exists: the journal *Acta Biotheoretica*, based in Leiden, the Netherlands, of which he was one of the three founding editors (Reydon et al. 2005). A second, the monograph series *Bios*, which was published by Barth in Leipzig under his editorship from 1934 until 1947, is equally revealing. Beyond these major publication efforts, the most interesting among his many scholarly and publicistic collaborations are his work with the medical propagandist Karl Kötschau on the so-called *Neue deutsche Heilkunde* (new German healing art) in the late 1930s, and with the physicist Pascual Jordan in the early 1940s on an abortive attempt to launch a new journal of interdisciplinary natural science called *Physis*. His institutional base from 1930 to 1958 was a professorship of history and philosophy of science in Hamburg that began as an unsalaried honorary position but eventually rose to the level of a full professorship. He also spent at least four lengthy stretches of his career in academic and research positions in Latin America (including Chile 1929–1932, the Dominican Republic intermittently 1935–1939, and El Salvador intermittently 1949–1952) and the United States (including stretches as a visiting professor at the University of Texas at Austin in 1960). In Chile and El Salvador he held professorships of theoretical biology (Meyer-Abich 1969).

The political upheavals of the 1930s and 1940s thwarted many of his goals, and led him into a complex and fascinating dance with the Nazi political system and its vast networks of complicity. Some of the most interesting documents of his relationship with the Nazi system emerge from his proposed appointment in 1936 to a new professorship of theoretical biology in Jena. His appointment was thwarted, however, by the opposition of high Nazi educational and propaganda officials including Karl Astel and Alfred Rosenberg, who were committed to race theory as the core of Nazi scientific and political ideology (Astel 1936; Rosenberg 1936; Harrington 1996: 195–198; Hossfeld 2004a). His troubled and troublesome relations with the Nazi system reveal how the promotion of biological science—because it was understood as part of science and a means of scientific practice in this period in the German-speaking world—held the potential to allow politics to dominate the processes of concept formation in biology, and thereby to subvert and supplant investigation. After the Second World War his collaborative energy and commitment to editorial projects flagged considerably, though he continued vigorously to write and to attempt to develop international networks and collaborations. Meyer-Abich's work therefore presents a fascinating case study of the ongoing complexities that make efforts at conceptual integration in biology such a difficult dance between scientific investigation and politics.

### Theoretical Biology Takes Form

By 1935, when Meyer-Abich asserted his dedication to it, “theoretical biology” had begun tenuously to develop into a small but contested subfield within the German-speaking life sciences. It is important here to identify a conceptual distinction that exercised players in the field at the time: theoretical biology and biological theory are not synonymous. For as long as there has been a disciplinary sphere identifiable as biology—that is, since around 1800—theoretical structures of explanation have accreted to the investigation of specific objects and phenomena. The history of 19th-century biology, after all, is rarely told without recourse to concepts like “cell theory,” “evolutionary theory,” “the theory of natural selection,” and “Mendelian theory.” These lastingly persuasive theoretical structures, however, were accompanied by numerous others that did not maintain their persuasive power. One need not look far to find theoretical statements, by some of the most well-respected historical figures in the biological disciplines, that proved ephemeral. Laubichler and Rheinberger (2006) discuss the work of one of these figures, August Weismann, in order to evaluate how, despite the overhauling of some of his conceptual innovations, his research remains part of the ongoing intellectual trajectory of biological science. They argue that Weismann's integrative approach to theory-building remains part of the process by which biological theory comes

to constitute something more disciplinarily and conceptually complex: theoretical biology. Laubichler (2001) has discussed how Darwinism (and especially its Weismannian selectionist variant) hindered the establishment of theoretical biology as a subdiscipline around and after 1900, due to its status as a particularly persuasive form of biological theory that nonetheless stood in tension with much of the German morphological tradition. Reif et al. (2000) have also begun the process of reinterpreting the significance of the work of early 20th-century German scholars—traditionally seen as outside the mainstream of Darwinian and genetic thought (see also Harwood 1993)—for the development of the Modern Synthesis of evolution and genetics in mid-20th-century Anglo-American biology. More recently, Laubichler (2007b: 132) has taken his arguments about the significance of the history of theoretical biology a step further, stating “that for several dimensions of theoretical biology—those connected to data analysis, model-building, conceptual analysis, and theory integration—the distinction between ‘history’ and ‘current work’ is meaningless.”

As a diverse quasi-discipline that seeks to make integrated, higher-order sense of theoretical material, theoretical biology further arose out of the widespread perception—especially in the German-speaking world—of a crisis in the biological sciences in the first decades of the 20th century. Numerous scholars with stated interests in theoretical aspects of the life sciences also gave parallel reasons for their invocation of this sense of crisis, their complaints focusing on the problem of adequate communication of conceptual content among, between, and beyond life-science fields. Charles Minot (1913: 103) spoke of “sundry disciplines more or less separated from one another,” Ludwig von Bertalanffy (1928: 14) of the “abandonment of any comprehension of biological phenomena,” and Emanuel Rádl (1930: vii) of “the living world” as “dominated by ideas.”<sup>4</sup>

Two scholars who contributed significantly to the propagation of theoretical biology as a disciplinary concept in Germany around 1920, Julius Schaxel and Jakob von Uexküll, wrote with palpable exasperation about what they perceived as the failure of the biological sciences to achieve a successful theoretical structure capable of integrating diverse practices and concepts. Uexküll (1926: xiii) condemned “dogmatic assertions, which contain a definite statement concerning Nature. . . . This is a mistake, because Nature imparts no doctrines. . . .” Invoking Kantian categories, he then (p. xv) drew the radical conclusion that “all reality is subjective appearance.” Schaxel (1922: 4) also recognized an ongoing crisis of biology, and bemoaned its roots in an “offensive lack of concern with the handling of concepts and a frivolous recklessness in word usage [which] heighten the degeneration of doctrines into unbearable convolution.” Schaxel made a three-pronged scientific effort to counter this tendency. He emphasized rigorous experimental work in his Institute for Experimental

Biology in Jena in the 1920s; wrote extensively himself about principles of theory formation in biology; and between 1919 and 1930 published 30 monographs in his edited series *Abhandlungen zur theoretischen Biologie* (Contributions to Theoretical Biology; see Laubichler 2001: 242–247). In 1921 and 1922 alone, he published nine volumes of the series, including works by close future collaborators of Meyer-Abich including Friedrich Alverdes and Emil Ungerer.

### Meyer-Abich's Vision of Theoretical Biology

Meyer-Abich entered the biological field in the midst of this period of conceptual and disciplinary ferment. He studied philosophy and natural sciences in Göttingen and Jena from 1913 to 1917, after which he worked for some years as a librarian before embarking on his career as a biological thinker from his base in Hamburg. He retained a conflicted interest in empirical investigation, and once even claimed that his choice not to emphasize such efforts in his own practice emerged from the same problem that plagued Weismann and Hans Driesch: eyesight damaged by overzealous microscopy as a young investigator. His early work drew primarily on his philosophical training in logic and the history of philosophy. His interest in conceptual integration and general biology was, however, already present by the mid-1920s, especially in his lectures. Although there is no evidence that he studied with Schaxel while in Jena, he had a correspondence with Schaxel beginning in 1919, in which he indicates that they also met personally (Bg6). An unpublished manuscript (1921), from his time as a librarian in Göttingen, bears the ambitious title “Empirics and reality: With special consideration of the relationships between physics and biology. A contribution to scientific theory formation.” He doubtless chose this title to echo the first edition of Schaxel’s 1919 *Grundzüge der Theoriebildung in der Biologie* (Principles of Theory-Formation in Biology). At this point in his career he also referred to himself as a “strict positivist” (A14). On March 23, 1922, Schaxel wrote to inquire, with some urgency, about Meyer-Abich’s plans to contribute a monograph to the *Abhandlungen* series (Ba70). Curiously, this is the last piece of correspondence with Schaxel in the Meyer-Abich papers. Given the political awareness and savvy that Meyer-Abich later demonstrated in his career, he may very well have felt that Schaxel’s leftist politics (including participation at high levels in the socialist government of Thuringia from October 1921 to October 1923) were enough of a liability that they outweighed Schaxel’s industriousness and connections (Hopwood 1997: 380–382).

Meyer-Abich concentrated on developing his philosophical standpoint in the mid-1920s. He called his 1926 Hamburg *Habilitation* monograph *Logik der Morphologie im Rahmen einer Logik der gesamten Biologie* (The Logic of Morphology in the Framework of a Logic of Biology as a Whole;

Meyer 1926). Ernst Cassirer sat on his *Habilitation* committee and retained substantial respect for Meyer-Abich’s work thereafter (Moynahan 1999: 562–563). Meyer-Abich also began developing the personal connections and international networks that would serve him well in his future career. He was never shy about contacting important scholars whom he thought potentially sympathetic. In 1928, for example, he traded letters with the young Joseph Needham, who was at the time preparing his *Chemical Embryology* (1931). In a letter to Needham dated November 24, 1928, he represented himself as a neo-mechanist—after Needham had expressed sympathy for organicism, but clear skepticism of its suitability as a guide to scientific practice (Ba57). In 1929 he accepted a professorial appointment to the University of Santiago, in Chile, where he taught philosophy and theoretical biology until 1932, when he returned to Hamburg to take up an unsalaried professorship. For some years thereafter he combined this honorary academic position with salaried work as a librarian. His position in Santiago may, quite by accident, have made his career path after 1945 somewhat easier: in order to limit Catholic influence, the university there had a requirement that all staff belong to a Masonic lodge. In 1935, during the campaign to Nazify the German civil service, Meyer-Abich was ordered to provide an explanation of his Masonic activities. It is plausible that this experience showed him that he would not be welcome as a party member despite his expression of nationalist feelings. He had also been a member of the bourgeois-liberal German Democratic Party in the early 1930s, which made him further politically suspect, though it did not cause him serious or ongoing persecution (A1; Bg16).

While in Chile he pursued two translation projects. The first was a translation into German of Henry Fairfield Osborn’s *The Origin and Evolution of Life* (1918). The second translation—of J. S. Haldane’s lectures published under the title *The Philosophical Basis of Biology* (1931), which Meyer-Abich had first encountered through a colleague in Chile (A8)—set him on the conceptual path that he would follow for the rest of his career. In the manuscript of the introduction to the latter translation, he credits Haldane with being the first to explore the idea that biological laws are in fact more general than physical laws—the simplest statement of his own “holistic simplification” (A8). As he also emphasizes in his introduction, Haldane drew many elements of his thought from branches of the German tradition in biological theory-building, for Haldane had studied with Haeckel and Strasburger in Jena, at Virchow’s Anatomical Institute in Berlin, and with Weismann in Freiburg. Around 1930, Meyer-Abich also began describing his own work extensively as theoretical biology. As early as 1921 he had explored, in manuscript form, arguments that theoretical biology inherently encompassed and linked empirical investigation and philosophy of biology (A14). While in Chile he wrote an extensive “program of lectures on

the philosophy and history of biological theories (theoretical biology)” (A8).

Interestingly, Meyer-Abich never published a monograph on theoretical biology, despite numerous halting attempts to formulate one throughout his career. The provisional finding aid of the Meyer-Abich papers in Hamburg lists at least eight collections of notes and manuscripts under variants of the titles *Theoretische Biologie* (Theoretical Biology), *Geschichte der biologischen Theorien* (History of Biological Theories), and *Grundfragen der theoretischen Biologie* (Basic Questions of Theoretical Biology). Given Meyer-Abich’s general rhetorical vigor, forthright willingness to invent new words and concepts in his writing (to the point that “-isms” proliferate in his work), and his unblinking willingness to face both political challenges and academic work in Latin America, this reticence is revealing. His correspondence gives some hints about its roots. At the simplest level, he was fully aware that theoretical biology was a multifarious and slippery concept that in fact might better have been used in the plural. He reflects directly on this issue of “multiple theoretical biologies alongside one another” in a short, undated manuscript from the early 1930s entitled *Beiträge zur Theorie der organischen Entwicklung* (Contributions to the Theory of Organic Evolution) (A12).

His long and occasionally conflicted relationship with Ludwig von Bertalanffy also contributed to his reticence about synthetic works on theoretical biology. In 1930, during his time in Chile, he corresponded with von Bertalanffy and Ungerer about writing a collaborative synthetic work on the subject. Though von Bertalanffy strongly supported the idea of such a collaboration, he went on to publish the first volume of his own *Theoretische Biologie* in 1932, and for a period thereafter the tone of his intermittent correspondence with Meyer-Abich cooled considerably (Ba9). Self-interested or not, however, von Bertalanffy’s courtly manner carried the day. In a letter to Meyer-Abich dated July 2, 1941, von Bertalanffy admits that they will likely continue to differ on some scientific matters, but adds that their disagreements need not cloud their personal relationship (Ba9). Meyer-Abich’s great respect for and close working relationship with Jakob von Uexküll (to whom, along with Hans Driesch, he dedicated Meyer [1934] as a mark of gratitude for their work as “pioneers of theoretical biology”) also likely made him unwilling to compete by producing a work with the same title. Uexküll also remained somewhat skeptical of Meyer-Abich’s logic-based arguments about holism, and explained to Meyer in a letter of July 10, 1943, that “in my view it is false to develop a theory logically, and only after the fact to demonstrate it with examples” (Ba80). This was not the only time that Meyer-Abich would hear such criticism from valued colleagues and collaborators.

Two important books with primarily philosophical-historical content bracket the most active period of Meyer-Abich’s career: his 1934 *Ideen und Ideale der biologischen*

*Erkenntnis: Beiträge zur Theorie und Geschichte der biologischen Ideologien* (Ideas and Ideals of Biological Knowledge: Contributions to the Theory and History of Biological Ideologies) and his 1963 *Geistesgeschichtliche Grundlagen der Biologie* (Intellectual-Historical Foundations of Biology). These two works demonstrate the many continuities in his thought and argument, and the centrality of reflection on the character and function of theory in biology. They further demonstrate Meyer-Abich’s reticence to represent his work as a fully integrated form of (or contribution to) theoretical biology. The most significant argument set forth in both works is the claim that facts and theories in biology are mutually constituted, that neither makes sense without the other. He does imply, in an argument congruent to Uexküll’s subjectivism, that theory inheres empirical reality in a cognitively more significant manner than vice versa (Meyer 1934: vii; Meyer-Abich 1963: 310). Logically (though fleetingly here), he invokes American pragmatism, particularly that of William James, to buttress this position (Meyer 1934: 65; Meyer-Abich 1963: 309).

Meyer-Abich derives two key insights from these arguments about the relationships between knowledge and theory-building: First, that theory is present in all biological practice in the form of what he calls *Erkenntnisideale* (ideals of cognition), and second, that all biology therefore inherently requires a kind of holistically determined and expressed “metabiology” that explores the systematics of the ideals of cognition deployed consciously or unconsciously in biological practice (Meyer 1934: 65, 117; Meyer-Abich 1963: 291–310). He does this in the recognition that “biology” is historically, conceptually, and disciplinarily polymorphous. He also argues for the primacy of biological theory over physical theory, invoking his principle of “holistic simplification” (Meyer 1934: 24–25, 47; Meyer-Abich 1963: 25, 39–40). The final conclusion of both books, borne out in similar form and rhetoric, is that biology as a sphere of scientific knowledge construction remains both historically conditioned and historically contingent. In 1934 he summarizes his arguments as follows:

One thus recognizes how closely phylogeny and physiology belong together. Today, however, the fields communicate with each other very poorly. In my opinion this comes from the fact that both have, up to now, pursued false ideals of cognition. Phylogeny wanted to be developmental mechanics (*Entwicklungsmechanik*). . . . And physiology strove to reach the mechanistic ideal of cognition. Nothing, though, is as ahistorical as the purely mechanical. Thus our two sciences had to talk over each other’s heads. I hope to have shown, however, that the new holistic ideal of cognition brings the divided fields back together. (Meyer 1934: 146)

By 1963 he had expanded his field of vision to include taxonomy, morphology, physiology, phylogeny, and ecology, but expressed his conclusions similarly, if with greater rhetorical flourish: “Biology taken as a totality can only be understood

historically. For, to say it yet again: biology as a science is not a systematic unity (*Einheit*), but a historical whole (*Ganzheit*)!" (Meyer-Abich 1963: 5). In 1934 he is confident enough to hope that his work might form a basis for the construction of a fully disciplinary theoretical biology—even if he (unlike von Bertalanffy, whom he invokes) knows that such a thing is in fact not yet possible (Meyer 1934: xi). By 1963 he has eliminated this claim from his work, but still believes that “theoretical endurance is historical endurance” (Meyer-Abich 1963: 310).

### ***Bios, Acta Biotheoretica*, and the Diversity of Theoretical Biology at Mid-Century**

The conceptual structure of Meyer-Abich’s philosophy of biological theory may have remained both highly consistent and somewhat idiosyncratic throughout his career. These writings represent, however, only a fraction of his effort. Especially in the 1930s and 1940s, he was a tireless traveler and correspondent, an active advocate of holism in scientific, journalistic, and political spheres, and engaged in numerous energetic and long-lasting collaborations in research, publishing, and institution-building. The two most important of these collaborations were publishing efforts that produced major documents of international theoretical biology in the mid-20th century. These were the monographs published beginning in 1934 by Johann Ambrosius Barth in Leipzig under the series title *Bios: Abhandlungen zur theoretischen Biologie und ihrer Geschichte, sowie zur Philosophie der organischen Naturwissenschaften* (*Bios: Contributions to Theoretical Biology and Its History, Including the Philosophy of the Organic Natural Sciences*) and the journal *Acta Biotheoretica*, published in Leiden beginning in 1935.<sup>5</sup>

In the *Bios* series Meyer-Abich produced largely theoretical works centered on epistemological questions. He also clearly sought—though there is no evidence that he acknowledged this publicly or privately—to take over Schaxel’s place within the publicistic structure of German theoretical biology and biological thought. The subtitle of the *Bios* series, of course, recapitulates verbatim the title of Schaxel’s own series of monographs, which had ceased publication in 1931. Schaxel went into exile in the Soviet Union in 1933 (Hopwood 1997), and by 1935 Meyer-Abich was himself bound up in political controversies in Jena (see below). Meyer-Abich’s own *Ideen und Ideale (Ideas and Ideals, 1934)* was the first volume in the *Bios* series, and he went on to publish several of his friends and collaborators, including Hans Petersen, Jakob von Uexküll, Hans Driesch (twice), Friedrich Alverdes, Emil Ungerer, and Karl Friedrichs. Driesch, Alverdes, and Ungerer had also published monographs in Schaxel’s series during the 1920s, as Meyer-Abich himself had hoped to do. Uexküll’s *Bedeutungslehre (Theory of Meaning, 1940)* is the monograph

from the *Bios* series that has developed the greatest scholarly significance, for it remains the foundational document in the field of biosemiotics (Kull 2001; on the relationship between Meyer-Abich and Uexküll, see Mildener 2007: 163–178). The production of the series also taught Meyer-Abich early on how to navigate successfully the increasingly complex Nazi systems of scientific and journalistic censorship, publication oversight, and paper rationing. Remarkably, the *Bios* series survived the end of the war; in all, 20 volumes were published through 1947 (see Laubichler 2001: 247–251). It likely died out largely because the Leipzig publishing industry came under increasing pressure from the Soviet occupation authorities in the late 1940s.

The journal *Acta Biotheoretica* began publication in Leiden in 1935, supported by a small endowment named for the longtime Leiden professor of zoology Jan van der Hoeven (Reydon et al. 2005: 3). Meyer-Abich’s closest collaborator and correspondent in Leiden was C. J. van der Klaauw (1893–1972), also a professor of zoology (about van der Klaauw and his relationship with Meyer-Abich, see Trienes 1988; Reydon et al. 2005; Dubbeldam 2007). Van der Klaauw and Meyer-Abich (who co-edited the journal in its early years along with J. A. J. Barge) shared a common sense of the close relationship between conceptual and historical knowledge and biological investigation, and also of the fundamentally international character of scientific work.

Van der Klaauw had been reading and citing Meyer-Abich’s work as early as 1931. Trienes (1988) discusses this at some length, but unfortunately without sufficient appreciation of the historical dynamics of their long and fruitful intellectual relationship after the early 1930s. The earliest evidence in the Meyer-Abich papers of their correspondence comes from May 1933, when they discussed what was thought to be a letter between Richard Owen and Alexander von Humboldt, apparently discovered by van der Klaauw in the course of his study of the correspondence between Owen and van der Hoeven (Ba45; Dubbeldam 2007: 13). In the end, they published only one paper together—a study of Kant’s teleological thought and its relationship to ecology (Meyer and van der Klaauw 1935). Despite some differences over the significance of Meyer-Abich’s principle of holobiosis for interpretations of evolution and development, Meyer-Abich and van der Klaauw remained on warm personal terms for decades after the Second World War. Their shared hope that their work might allow the “founding of the organizational possibility of a theoretical biology as a real discipline in empirical biology” sustained them through numerous hardships (van der Klaauw to Meyer-Abich, March 19, 1936; Ba45). Neither was their relationship substantially clouded by the fact that after the war, which enforced a hiatus in the publication of *Acta Biotheoretica* in 1944 and 1945, Meyer-Abich was no longer listed as one of the chief editors of the journal.

Meyer-Abich clearly hoped during the late 1930s that he could use *Acta Biotheoretica* to advance the international cause of biological holism. The first few volumes of the journal contain numerous papers drawn from the key figures in holistic biology, including Haldane, F. G. Donnan, Ungerer, Uexküll, Alverdes, and Meyer-Abich himself. Yet the editors knew that the journal would suffer internationally—especially in such ideological times—if it was seen only as the organ of a specific philosophical approach, so they also published many essays by figures critical of the tenets of holism, including Driesch, Herman Jordan, Karl Sapper, and Erwin Bünning. Furthermore, van der Klaauw always retained some skepticism of Meyer-Abich’s philosophical and terminological enthusiasms, and therefore worked as a countervailing influence to give the journal a more mathematical and empirically grounded character. Reydon et al. (2005) note these tensions between work on philosophical foundations and on mathematical approaches, but also miss a key theme in the journal’s editorial policy, a theme that played a mediating role between the two extremes. From its inception, *Acta Biotheoretica* regularly published work—including that of Meyer-Abich and van der Klaauw—that sought, in its broadest implications, to explore the relationships between evolution and development, even if it did so from outside the mainstream of the Anglo-American, Darwinian-selectionist thought that led to the Modern Synthesis (Reif et al. 2000; Laubichler 2001). The history of the *Acta Biotheoretica* group therefore deserves much closer scrutiny for its significant attempt to fill the disciplinary gaps that historians of evolutionary developmental biology now recognize as predominant in the mid-20th century.

*Acta Biotheoretica* also published three supplementary series (Reydon et al. 2005: 3), of which the most significant was the monograph series *Bibliotheca Biotheoretica*. The *Bibliotheca* appeared irregularly between 1941 and 1972 under an inscrutable volume-numbering system. Of the approximately 15 monographs produced, Meyer-Abich authored two, published as volumes 5 and 10, in which he made his most significant—if largely unsuccessful and unheeded—contributions to the holistic study of the relationships between evolution and development (see below; Meyer-Abich 1943, 1950, 1964). From the outset in 1935, Meyer-Abich appreciated that establishment of the *Bibliotheca Biotheoretica* would put him in a position of conflict of interest with respect to his own *Bios* series. He considered bringing the *Bios* series to E. J. Brill in Leiden for publication, but van der Klaauw felt this unnecessary (Bd9). Unsurprisingly, the advisory boards of *Acta Biotheoretica* and the *Bios* series overlapped considerably, with von Bertalanffy, Hans Böker, Driesch, Needham, E. S. Russell, Hans Spemann, August Thienemann, Uexküll, Ungerer, W. M. Wheeler, and J. H. Woodger serving on both boards in 1935. Nonetheless, the two series did remain separate in conception and content, with *Bios* taking over the more philosophical-historical

treatments, sometimes after Meyer-Abich and van der Klaauw had consulted about the better fit for certain manuscripts, for example, one by Hans Petersen, a retired professor of anatomy from Würzburg (Ba45; Petersen 1937).

Remarkably, perhaps in subconscious atonement for their intellectual and rhetorical debts to Schaxel, Meyer-Abich and his colleagues published Schaxel’s last monograph—on theories of ontogenetic determination—as volume 1 part 3 of the *Bibliotheca* (Schaxel 1942). Publishing the work of a self-identified socialist working in the Soviet Union during the Nazi occupation of the Netherlands and at the height of Nazi power was an oddly brave thing to do, but it was representative of the forms of resistance in which van der Klaauw apparently often engaged. In late 1942 the latter wound up in a Nazi internment camp, and Meyer-Abich wrote a strongly worded letter to the Nazi military governor of the Netherlands on his behalf, using the term “hostage,” but at the same time shrewdly arguing that theoretical biology was primarily a German field of science (a position that van der Klaauw himself had articulated to Meyer-Abich in 1940), but that international collaboration was crucial to its success (Ba45). This exchange is characteristic of Meyer-Abich’s relationship to the Nazi state. He often presented himself as dedicated to the state’s goals at the same time as he worked against some of its ideologically, politically, or militarily powerful elements. His career and correspondence therefore bear powerful witness to the multiple moral paradoxes through which the Nazi state’s strategies for the generation of complicity functioned in the scientific sphere.

### Scientific Practice, Theory, and Politics in the Nazi Period

On June 19, 1936, Uexküll wrote to Meyer-Abich with the following less-than-total vote of confidence: “Hopefully you will not end up burnt at the stake” (Bd17). A year later, on April 12, 1937, Petersen echoed the phrase almost uncannily: “. . . I have little leaning to put myself in such unmediated proximity to the burning stake” (Bd25). Many of Meyer-Abich’s relationships and collaborations, like that with von Bertalanffy, had moments both of quiet friction and open conflict. At the same time, Meyer-Abich appears to have been capable of impressive savvy and subtlety in the development and maintenance of scientific, professional, and even personal relationships. His extraordinarily extensive correspondence displays an ambitious man interested in professional success and recognition, but never with a preponderance of cynicism, and never at the expense of the personal aspects of scientific relationships, which he maintained through the depredations of Nazism and war with great loyalty.<sup>6</sup> He also put himself at some risk during the Nazi period to defend both German and non-German colleagues who were denigrated by Nazi ideologues, or even directly threatened by Nazi policy or reprisals. He was generally

willing to publish work critical of his own position in his journals and monograph series, and to accept highly critical remarks from both close and distant colleagues with equanimity. He did, however, seek political means of realizing his goals, and showed himself willing—in collaboration with important Nazi party-affiliated figures—to put his ideas and efforts at the service of the Nazi state. He failed at these attempts, but had both the cleverness and the good fortune never to have linked his own professional position directly to the Nazi party or his work directly to Nazi ideology.

In 1935 and into 1936 Meyer-Abich came in direct contact with the consolidating centers of Nazi administrative and political power as a result of his highly contested and ultimately unsuccessful appointment to a newly created chair of theoretical biology at Jena, mediated by an important collaborator at the time, the Jena medical professor Karl Kötschau. The controversy was particularly significant because it demonstrates how Nazi political rhetoric and organization motivated adversarial relationships between interest groups. This drove scientists to invest more and more effort in forms of persuasive practice, and further to have to strive to ingratiate themselves with certain party and administrative offices while they battled others. Working scientists were thus driven, through the party's demands for ever-proliferating forms of persuasive practice, into a combination of complicity and resistance that left no space for an apolitical or value-free science. Meyer-Abich had met Kötschau in the early 1930s, likely as a result of his work with the Hamburg physician, professor, and clinic director Ludolph Brauer on a comprehensive catalog of research institutions in the German-speaking world (Meyer et al. 1930). Brauer and Kötschau were academically trained physicians who belonged to the always-significant branch of German medicine that supplemented its so-called "school medicine" training with what would now be called "alternative" approaches. In late 1935 Brauer also wrote to Meyer-Abich to remind him that "the German university has always been political" (Ba13). They, like many German biologists and physicians trained around 1900, were not convinced that mechanistic physical and chemical principles could adequately explain the complexity of living systems like cells and organs with their contingent processes and their nontransparent kinds of pluripotency, varying regenerative capacities, and proliferating pathologies.

Kötschau believed that Meyer-Abich's attempts to re-ground the biological sciences in holistic epistemology could simultaneously provide better grounds for his nondeterministic medicine. Kötschau had developed—authorized at first by the Nazi hierarchy for—an initiative called the *Reichsarbeitsgemeinschaft für eine neue deutsche Heilkunde* (Reich Working Group for a New German Healing Art), and brought Meyer-Abich into it. He promulgated and propagated the principles of his Reich working group widely and vigorously, producing reams of correspondence (Proctor 1988; Kater 1989; Reich

2001).<sup>7</sup> During the peak of the Jena controversy, he wrote to Meyer-Abich alone as many as three times a day, and appeared to relish the private and public interest group jousting that Nazi political organization required. The two developed a vigorous collaboration beginning in 1935, one that survived by several years the failure of Meyer-Abich's appointment to Jena, Kötschau's subsequent departure for the directorship of a branch of the city hospital system in Nuremberg, and the closure of the Reich working group. Together, they came to call their attempted synthesis of holistic biology with non-deterministic medicine "biological medicine." The most significant result of their collaboration was a book titled *Theoretische Grundlagen zum Aufbau einer biologischen Medizin* (Theoretical Foundations for the Construction of a Biological Medicine; Meyer and Kötschau 1936). Meyer-Abich never got directly involved in the running of the Reich working group, but he remained in close contact with Kötschau throughout this period.

The controversy about Meyer-Abich's Jena appointment was a central moment in the solidification of race theory—precisely in conflict with and contradistinction to holistic biology and medicine—as the dominant form of persuasive practice sanctioned by Nazi scientific policy. The controversy began in January 1935 when Meyer-Abich was invited to Jena to speak to the Medical-Scientific Society about his holistic biology. Several Jena professors, including Kötschau and the holistic anatomist Hans Böker, supported the lecture as a foot in the Jena door for Meyer-Abich. But three other powerful Jena professors, Karl Renner, Max Hartmann, and Karl Astel, were not convinced. Astel made particular trouble. He was the holder after 1934 of a new chair in "human breeding" (renamed "human genetics and racial politics" in 1935), the director of the Thuringian State Office on Racial Character, and from 1939 to 1945 rector of the university. He was also fond of carrying on scientific and administrative controversies and disciplinary arguments on the pages of newspapers. He had close contacts with the Reich Office for the Protection of German Writings; with Alfred Rosenberg, Hitler's special deputy for intellectual matters; and with party political journals like the *National Socialist Monthly* (Hossfeld 2004a). In mid-1936, despite Astel's opposition, Kötschau and Böker had engineered Meyer-Abich's call to the new chair of theoretical biology (Harrington 1996: 195–198). The first indication that the Nazi race theorists at Jena would not tolerate his appointment came in a paper highly critical of Meyer-Abich by Astel's assistant, Lothar Stengel von Rutkowski, in the May 1936 issue of *National Socialist Monthly* (Ba27; Hossfeld 2004b).

But Astel and the other race theorists reserved their big guns for the December 1936 issue of the *National Socialist Monthly*, which was headlined by parallel papers by Rosenberg on "Worldview and Science" and by Astel on "Higher Education and Science." Their parallel claims were that only

“a Germanic theory of value” and a “race science on the basis of exact, experimental, factual material” would qualify as National Socialist science (Astel 1936: 1118; Rosenberg 1936: 1070). Meyer-Abich’s appointment was thereafter moot. His appeals to authorities—including Hans F. K. Günther, the Berlin professor and leading popularizer of race theory before and during the Nazi era (who had taught “social anthropology” in Jena from 1930 to 1935)—fell on deaf ears, even as he claimed that holistic biology could provide a better grounding for Nazi ideology than could mechanistic biology. He was savvy enough to be aware that his appointment was a political football, and admitted as much in a letter to Günther of May 25, 1936:

I naturally know that this tremendous interest in theoretical biology has much less to do with my own person or with the holistic biology I champion than with the always strong ideological struggle and the particular lay of the fronts in Jena.

Nonetheless he insisted—after an extensive critique of the “Bolshevist” “mechanistic” biology of Goldschmidt and Schaxel, and comparison to the biology pursued by Hartmann among others—that he retained the hope that holistic biology might come to be seen as central to the Nazi project:

The representative of military science in Jena declared to me . . . that holistic biology lies within the sense of the National Socialist worldview. Prominent representatives of the party tell me that time and again. I am pleased about that, but it is entirely clear to me how far I myself trail behind this great ideal of our time in my work. Only through combined efforts of both biological- and antimechanist-thinking researchers can it be realized. (Ba27)

This attitude represents not false modesty, but rather a savvy appreciation of the kinds of relationship-building required within Nazi political, academic, and scientific institutions.

It took some years for the controversy to die down, for Meyer-Abich and his colleagues, including Uexküll, were called on by Rosenberg’s Office for the Defense of Nazi Writings to substantiate their claims. As late as 1939, Meyer-Abich and Kötschau required the assistance of both SS medical officers like Joachim Mrugowsky and sympathetic members of the staff of the Deputy *Führer*, Rudolf Hess, to help them support their position when they were invited to Berlin for a “discussion” with party authorities at a police villa in Grunewald (Ba70). Kötschau and Meyer-Abich came to call their ideological battle with the race scientists the “Jena Academy Debate,” linking it to the great evolutionary controversy in the first decades of the 19th century between Cuvier, Lamarck, and Geoffroy de Saint-Hilaire in the French Academy of Sciences.

Meyer-Abich’s relationship with the Nazi state was made even more complex by another form of persuasive work. He spent a great deal of his professional time during the war as

a regular speaker to German military units. He gave dozens of lectures on questions of scientific epistemology to military organizations all through Germany and occupied Europe. A lecture series was even planned for Japan in 1941, but did not come to pass. It was in this manner—though apparently not under the explicit authority of the military command or the Nazi party—that Meyer-Abich visited and gave a lecture at the IG Farben facility at Auschwitz-Monowitz in 1944 (Bg38). This variety of persuasive work shows particularly clearly the contingencies of Meyer-Abich’s position in the Nazi system for the generation of complicity. His papers give little evidence of his motivations for investing so much time in this lecturing, but at the very least, service to the military hierarchy was one way to both curry favor with and resist the coercive intrusions of the Nazi party hierarchy (which always retained a somewhat tense relationship with the military officer corps).

Meyer-Abich’s interactions and collaborations with Mrugowsky and Pascual Jordan provide further evidence of the dynamics of his work during the Nazi period. These two colleagues were, like Kötschau, highly ambitious scholars who became party members while still struggling to profile themselves within the party’s complex web of prestige and influence. Meyer-Abich collaborated with them in the same manner he did with Kötschau, i.e., by seeking to profile himself as both serving the Nazi system in innovative ways and yet maintaining a certain distance from it. Mrugowsky became the most successful of Meyer-Abich’s collaborators within the Nazi hierarchy, rising to the position of director of the *Waffen-SS* Institute of Hygiene in Berlin. In September 1944 he received the title of professor from the University of Berlin, but he was to pay for his allegiance to the régime. Tried with the 23 Nazi physicians in late 1946 at the “Doctors’ Trial” in Nuremberg, Mrugowsky was executed by hanging on June 2, 1948 (Mitscherlich and Mielke 1949: 148; Kater 1989: 131, 321).

Meyer-Abich and Mrugowsky began corresponding by early 1937, after Mrugowsky wrote to Kötschau in 1936, expressing interest in defending the position of Meyer-Abich’s work, holism, and Kötschau’s medical projects in relation to Nazi ideology (Bd17). They continued to correspond regularly until Mrugowsky’s execution. Although they did not publish together, two variously lasting contributions resulted from the collaboration: some of the thinking that went into Meyer-Abich’s later work on Alexander von Humboldt (Meyer-Abich 1949, 1967) and the development of the Hippokrates publishing house of Stuttgart (which had been linked to the working group for a New German Healing Art) into a general publisher of nonspecialist works on medical-scientific themes, including two books by Meyer-Abich himself—*Natural Philosophy on New Paths* (1948) and *Biology in the Age of Goethe* (1949). The Meyer-Abich papers even contain a series of wrenching exculpatory letters by Mrugowsky to Meyer-Abich that were written from his prison cells in Nuremberg

and Landsberg/Lech in the months before the execution. They provide fascinating evidence of the kinds of psychological bargains struck by Nazi perpetrators. “You also know,” Mrugowsky wrote on August 29, 1947, “that I did not serve any ideology or any person, but rather my fatherland.” On October 27, 1947, he spoke of being “far away from any thought of violence and disdain for humanity, that I was never an anti-semitic in the political sense . . .” (Bd32).

Meyer-Abich worked and corresponded extensively with Jordan from before 1941 until after the Second World War. This collaboration resembles in kind Meyer-Abich’s work with Kötschau: a dialectic of productivity and tension between two scientists with overlapping but hardly identical personal and intellectual interests. Jordan, as one of the founding figures of quantum mechanics, was not shy about attacking strains in Nazi physics that he perceived as anti-modern. At the same time, he was willing enough to serve the Nazi party’s goals that he became a party member early (on May 1, 1933), a fact that he reported to Meyer-Abich in a letter of January 2, 1946 (Ba38; Bd38; see also Hoffmann 2003). During the war, he carried on, with Meyer-Abich’s assistance, a wide-ranging attempt to revive the political fortunes of quantum mechanics. Part of this project was a conceptual expansion into the sphere of “quantum biology,” a term that fascinated Meyer-Abich but never gained much investigational or theoretical traction (Beyler 1996). Jordan’s forceful defense of his own interests, both during and after the Nazi period, led him to describe his detractors to Meyer-Abich on September 22, 1941, as “scientific gangsters.” He was also happy to describe himself and his scientific goals to party comrades as—in contrast to the work of his detractors—fundamentally anti-Semitic, which he did, for example, in a letter of November 26, 1941, to a colleague that he copied to Meyer-Abich (Ba38). At the same time, Jordan’s psychology could allow him to describe himself as fundamentally anti-Nazi in the letter of January 2, 1946:

In the long view I am thoroughly optimistic that enough qualified evaluators will be found who can attest that this [Nazi party] membership only served to make my personal fight *against* National Socialism possible within the scientific sector. (Bd38; emphasis original)

Jordan succeeded in this exculpatory strategy. He retained his interest in politics, and became a member of the West German parliament (*Bundestag*) in the late 1950s (Schirmacher 2007). The personally and politically complex collaboration between Meyer-Abich and Jordan had one tangible result: two issues of a new journal with the title *Physis: Contributions to Natural Scientific Synthesis* (Beyler 1996: 268–269; Laubichler 2001: 48). A number of Meyer-Abich’s other collaborators remained suspicious of Jordan’s motives because they saw the self-interest in his position. Karl Friedrichs, for example, warned Meyer-Abich as early as July 3, 1941, that Jordan was “a positivist and in many respects our

opposite.” Nonetheless, Friedrichs recognized that Jordan’s association with quantum mechanics rather than holism could benefit the new journal, because it would demonstrate that they did not intend the journal to be a “one-sided” organ of holism (Ba26).

### Meyer-Abich, Evolution, and Development

In parallel with his extensive publicistic work during the Nazi period, Meyer-Abich also pursued what he hoped would become his most significant theoretical contribution to biological science—an exploration of the relationships between holobiosis and evolution, a project that occupied him throughout the second half of his career and became the basis for numerous publications. Meyer-Abich’s work on this material bore relatively little fruit as the basis for further theoretical or empirical work in the biological sciences, but it nonetheless represents and refigures several of the key themes in evolutionary and developmental biology during the mid-20th century that have recently come to renewed attention.

The failure of Meyer-Abich’s appointment as professor of theoretical biology at Jena in 1936 precipitated a lengthy period of residence outside Germany, and also his own most significant period of close association with empirical work in the biological sciences. Between late 1936 and August 1939 (when he returned hurriedly to Germany by a hazardous sea voyage that included a short internment in Iceland upon the outbreak of hostilities), he and Böker spent a considerable portion of their time in the Dominican Republic, where they built up the German-Dominican Tropical Research Institute near Ciudad Trujillo, as the capital was then called. The institute sought empirical evidence, especially in marine organisms, of holistic principles, and supported geological work about the island of Hispaniola. It also became something of a political and propaganda football as relations between the United States and Germany deteriorated around the outbreak of the Second World War in Europe (Weyl 1941). Meyer-Abich, of course, also remained active in discussions of scientific policy, and despite his physical separation from the centers of German research and politics in this period, he again showed his willingness to address major figures in search of support for his interests. Several letters attesting to the significance of the institute’s work for German-Dominican relations passed between him and Rafael Trujillo, the Dominican strongman (Bg56).

Meyer-Abich’s collaboration with Böker provided the focus for his research and writing in the late 1930s. Böker, who had been educated in Freiburg im Breisgau under Wiedersheim, came to Jena in 1932 as professor of comparative anatomy (about Böker, see Hossfeld 2002; Hossfeld and Olsson 2003). His major theoretical statement is found in a surprisingly meekly titled two-volume *Einführung in die vergleichende Anatomie der Wirbeltiere* (Introduction to the

Comparative Anatomy of Vertebrates). Ernst Mayr was impressed enough with this work to grant Böker substantial recognition for having avoided the key failing of many of his morphologist colleagues, which was to ignore the question of adaptation:

Almost the only exception was Hans Böker . . . who in a superb functional-evolutionary morphology asked all the right questions, as seen in hindsight, concerning the adaptive value of structures and their changes, but based his interpretations unfortunately on the wrong evolutionary philosophy (neo-Lamarckism). As a result his visionary study failed to have any effect. (Mayr 1982: 468)

Receiving another position in Cologne, Böker resigned his Jena professorship in 1938 as part of the aftermath of the political fiasco of Meyer-Abich's appointment, but he died soon after. Meyer-Abich prepared a small memorial volume that focused on Böker's theory of *Umkonstruktion* (Restructuring; Meyer-Abich 1941).<sup>8</sup> By the mid-1930s Böker had developed an approach to anatomy that focused on developmental questions with evolutionary consequences. As Hossfeld and Olsson (2003: 119) describe it, "Böker tried intensely to overcome the differences between phylogenetic research and genetics as well as between morphology and physiology by means of a holistic view, his theory of 'perfection' (*Vervollkommnung*)."<sup>9</sup> No wonder, then, that Meyer-Abich saw in Böker an important potential collaborator who could contribute to the conceptual unification of theoretical and empirical approaches.

The major results of this period of research in Meyer-Abich's own work are found in the various *Acta Biotheoretica* series (Meyer-Abich 1943, 1950). Many of these ideas and interpretations are also recapitulated in his most significant late monograph, *Geisteswissenschaftliche Grundlagen der Biologie* (Intellectual-Historical Foundations of Biology; Meyer-Abich 1963); in his *Naturphilosophie auf neuen Wegen* (Natural Philosophy on New Paths; Meyer-Abich 1948); and in his only major English-language publication, a set of lectures developed from his period as visiting professor at the University of Texas at Austin in 1960, that bears the stilted title *The Historico-Philosophical Background of the Modern Evolution-Biology* (Meyer-Abich 1964). In all these works he hoped to demonstrate generally that only a holistic understanding of the relationships between development, function, and taxonomy could account for the character of historical change in biological systems. More specifically, he sought evidence of the meta-symbiotic unification of types—holobiosis—in organismal structural and functional change. In the earlier forms of this work he built outward from Böker's *Umkonstruktion* and Austin H. Clark's concept of eogenesis ("simultaneous formation of the basic forms in the animal phyla"; Clark 1937: 192; see also Clark 1930)<sup>9</sup> to link organismal change with evolutionary change. Through the course of the 1950s and 1960s, as Böker's and Clark's concepts appeared to have failed to

gain traction, he emphasized them less and less. Nonetheless, the general tenor of Meyer-Abich's holobiotic approach to development and evolution remained consistent throughout the second half of his career.

In all of these works, Meyer-Abich struggled to develop and articulate an adequate conceptual framework with which to synthesize developmental and evolutionary questions. In Meyer-Abich (1943) he constructs and elaborates what he calls, in a refiguration of Haeckel's "biogenetic [basic] law" (*biogenetisches Grundgesetz*), the "typological basic law," and immediately makes clear in his title that he is interested in the consequences of this "law" for developmental physiology. Meyer-Abich's definition of the "typological basic law" represents a simple, even obvious, moment that affects taxonomy, physiology, and natural selection: "In no natural group of organisms is there any form existent in reality which shows all trait-tendencies of the group exclusively in the most primitive or only in the most differentiated final phase" (Meyer-Abich 1943: 18). Meyer-Abich apparently makes this argument primarily about groups of higher and lower taxa, but his language lacks clarity, seeming also to refer to single-species populations. His primary point is that any group (or population?) contains organisms that might appear to exist at different evolutionary stages. He also develops and explains a technique for drawing extensive "type circles" (*Typenkreise*) of genera within families, which purportedly show how "primitive" or "differentiated" characters correlate with one another across taxa (Meyer-Abich 1943: 9–10, Figures 2–5, 7). Type circles appear in all his major writings from then on. He appends a number of arguments to develop these principles into a set of conclusions about both evolution and development. The most important is that only juvenile organisms have the capacity to undergo changes of the sort that Böker described as *Umkonstruktion*, i.e., functional changes in organ systems. This argument represents one attempt to integrate Böker's views, with their Lamarckian tone, as a means of integrating and surpassing Weismannian orthodoxy about soma and germplasm (Meyer-Abich 1943: 43–44). Meyer-Abich's broadest hope for his integrative theory was that it might "provide on the basis of the principle of *holobiosis* the foundation for a new *evolutionistic* theory of phylogenesis" (Meyer-Abich 1943: 48). He also reflects extensively on how this theory differs from the evolutionary orthodoxy of his day, concluding that his ideas, based as they are on holobiosis, eogenesis, and *Umkonstruktion*, reflect developmental questions much more significantly because they focus on change in juvenile organisms, and thereby navigate around many of the traps of traditional "epigenetic" evolutionary theory, such as "missing links" and "hypothetical originary forms" (Meyer-Abich 1943: 61).

Meyer-Abich (1950, but originally submitted to *Acta Biotheoretica* in 1944) extensively elaborates the principles stated in 1943 around the concept of holobiosis. He explores what

he calls the “eogenesis of archetypes,” and links it to his reflections on juvenile forms from 1943 to conclude that “the progressive potentials that still exist in any given phyletic species can be realized only in embryogenesis, never in connection with adult form” (Meyer-Abich 1950: 19). The remainder of the work develops a complex set of arguments about the relationships between “bioses”: parabiosis, symbiosis, and holobiosis (see Meyer-Abich 1950: 39). In a large chapter he explores the questions of cellular biology, particularly surrounding the nucleus and mitochondria, that in fact presage remarkably the revival and elaboration in the 1970s of the endosymbiotic theory of the development of eukaryotes. He cites the pioneering works on this problem by Mereschkowsky, Wallin, and others, and engages their work critically (Meyer-Abich 1950: 75–121). In a lengthy final chapter he explores the botanical evidence for his holobiotic concepts.

His English-language work, *The Historical-Philosophical Background of the Modern Evolution-Biology* (Meyer-Abich 1964), provides an extensive recapitulation of these arguments and expands them with a chapter on “meta-biology” (a name he also gave to the final chapter of Meyer-Abich [1963]) in which he attempts to describe the logical structure of biological concepts. Here he returns to the language of his early conceptual works to argue that not only is biology itself a field that is defined by conceptual synthesis, but that such “dialectical synthesis” is the only adequate, guiding “ideal” of biological knowledge-creation. He describes his holobiotic concepts as just such a synthesis (Meyer-Abich 1964: 159–163).

However, even Meyer-Abich’s closest collaborators knew that his work was not quite enough. Even after the political and ideological turmoil of the 1930s and 1940s had cooled, he found it difficult to place himself at the center of innovation in either biological or philosophical fields. His work continued to navigate uncomfortably between and across disciplinary boundaries, and therefore remained suspect to many colleagues. C. J. van der Klaauw, who ironically remained one of his most sympathetic interlocutors, put this perhaps most bluntly to him in a letter of September 15, 1940, just as Meyer-Abich was beginning to formulate his extensive reflections on holobiosis and type synthesis:

The pure disciplinary zoologists, and I am one of them, will surely almost all be of the opinion that we have not—or not yet—come far enough to use something like this. It appears to be a form of theoretical biology that lies outside of disciplinary-scientific biology. . . . Once you are lined up with the pure philosophers who write and speculate about biological topics, it will no longer be easy to get into biology. (Ba45)

Meyer-Abich’s reputation was sufficient that he continued to publish widely and receive invitations to institutions in the United States and Latin America. But he never convinced even

his own colleagues in Hamburg that his work was central to their institutional profile: his professorship was converted to a fully funded chair (*Ordinarius*) only in 1956, two years before his official retirement.

Meyer-Abich may have failed to provide fully adequate or persuasive answers to the many biological and philosophical questions he broached, but his questions and methods were nonetheless critically thorough and even prescient with respect to the problems of integrating evolution and development. His work contains uniquely extensive and valuable reflections upon numerous investigational, conceptual, and persuasive transitions that took place in several biological subfields during the mid-20th century. Meyer-Abich therefore remains a fascinating case study of both the historical tensions in 20th-century biology and the possibilities and pitfalls inherent in all attempts at conceptual integration in the biological sciences—including those now being vigorously explored in evolutionary developmental biology.

### Archival sources

Adolf Meyer-Abich papers, State and University Library, Hamburg, Germany.

### Notes

1. References to manuscript materials in the Meyer-Abich papers (collection NAMA), State and University Library, Hamburg, Germany, are made with the appropriate alphanumeric manuscript box (single letter) or correspondence folder (double letter) call numbers. All translations from German sources are by the author.
2. Through 1937, Meyer-Abich wrote and published under the name “Adolf Meyer.” He then officially chose to add the name of his maternal family, likely to differentiate himself from both the Swiss-American psychiatrist Adolf Meyer (1866–1950) and the Bauhaus-affiliated architect of the same name (1881–1929). The mastheads of *Acta Biotheoretica* and the *Bios* series list him as Meyer-Abich beginning in 1938. While his hyphenated name is always used in the body of this paper, the references list his publications in the form his name is given on them. Library catalogs (including OCLC WorldCat) and periodical indexes are highly inconsistent, so searches for his work must be made under both names.
3. The term “holobiosis” appears to be Meyer-Abich’s coinage and to be limited largely to his usage. It does, however, very occasionally appear in recent work on macroevolutionary theory. For a recent study that mentions “holobiosis,” though not Meyer-Abich, see Reid (2007: 98).
4. Meyer-Abich thought highly enough of Rád’s *Geschichte der biologischen Theorien* (History of Biological Theories) that he wrote a short preface to the reprint published in 1970 (Meyer-Abich 1970).
5. Laubichler (2001) provides detailed discussion of the *Bios* series and Schaxel’s *Abhandlungen* (and their relationships to Wilhelm Roux’s series of monographs on *Entwicklungsmechanik*), but because his article focuses only on German theoretical biology, it does not discuss the close links between these publication efforts and *Acta Biotheoretica*, which was based in the Netherlands. Trienes (1988), Reydon et al. (2005), and Dubbeldam (2007) note that Meyer-Abich mediated between forms of theoretical biology in Germany and the Netherlands, but do not discuss it further.

6. The Meyer-Abich papers fill 56 boxes totaling some 10 linear meters in the manuscript department of the State and University Library, Hamburg. The sorted correspondence alone fills 23 boxes.

7. These scholars recognize Köttschau's historical significance, but none has examined the aftermath of Köttschau's departure from Jena as a result of failure of his, Meyer-Abich, and Böker's moves to represent holism as a better basis for Nazi ideology, biology, and medicine than was race theory, and the subsequent shut down of the Reich Working Group (which is noted but not explored in Proctor (1988: 235)).

8. See Hossfeld and Olsson (2003: 120) for a clear discussion of the complex concept of *Umkonstruktion*, which is perhaps best translated as "restructuring" or "refunctioning," for it indicates the evolution of new anatomical formations through developmentally mediated change in homologous structures.

9. Clark's concept of eogenesis, and even Clark himself (1880–1954; he was curator of echinoderms at the United States National Museum from 1920 to 1950), appear to have vanished from the history of evolutionary theory, despite his energetic popularization work and the huge and well-organized collection of papers housed in the archives of the Smithsonian Institution (<http://siarchives.si.edu/findingsaids/FARU7183.htm>; accessed October 22, 2008). Despite Clark's Harvard background and many resonances between eogenesis and punctuated equilibrium, neither Mayr (1982) nor Gould (2002) mention him. Additionally, neither the Clark collection nor the Meyer-Abich papers appear to contain correspondence between the two men or their close associates (personal communication, Tad Bennicoff, Smithsonian Institution Archives, October 23, 2008).

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