

## General Introduction to the Symposium on Evolutionary Developmental Biology: Paradigms, Problems, and Prospects<sup>1</sup>

Co-organized by Richard Burian, Scott Gilbert, Paula Mabee, and Billie Swalla

RICHARD M. BURIAN<sup>2,3</sup>

*Department of Philosophy, Virginia Polytechnic Institute and State University,  
Blacksburg, Virginia 24061-0126*

**SYNOPSIS.** This symposium undertakes to examine some historical background relevant to the renaissance in biological studies linking evolution and development, to review the current status of research in this rapidly changing area (especially the problem of forging links between disciplines that have gone in divergent directions), to address the benefits and difficulties that arise from molecular studies of the relationship between evolution and development, and to help set the research agenda in evolutionary developmental biology in the next few years. Rather than introducing the individual contributions that follow, this paper aims to set some historical background for the topics they cover. I argue that old questions about the relationship of development to evolution, raised by such figures as William Bateson and Richard Goldschmidt, remain relevant to contemporary work, though they require major reformulation in light of subsequent developments. Many older questions, long set aside as intractable, remain open. Recently developed techniques may enable us to answer some of them. Accordingly, I suggest, it is worth reviewing the work of several historical figures in setting current research agendas.

The idea for this symposium grew out of a breakfast at the Denver meeting of SICB, at which three of the four organizers brainstormed about a symposium on the history of Evolution and Development. The symposium contributes to the consolidation of a flourishing new interdisciplinary field, informally known as “Evo-Devo.” The Society for Integrative and Comparative Biology (SICB) has provided a venue for a considerable amount of work in Evo-Devo by hosting relevant symposia; four recent examples are (Gilbert and Atkinson, 1992; Martindale and Swalla, 1998; McHugh and Hylanch, 1998; Olsson and Hall, 1999). A

group of three Evo-Devo symposia were presented at the SICB Meeting in January 2000. The other two, “Beyond reconstruction: Using phylogenies to test hypotheses about vertebrate evolution” and “Hox clusters and the evolution of morphology,” will be published in subsequent issues of this journal. These events helped inaugurate SICB’s new Division of Evolutionary Developmental Biology, which we hope will serve as a professional home for Evo-Devo biologists from all over the world. The papers in this symposium aim to provide a large-scale view of Evo-Devo, touching on its past, present, and future. We asked the contributors to address topics that would illuminate the long history behind the new field, its present state (including strengths, weaknesses, and analyses of key concepts and techniques), and its prospects over the near term.

Since I am an historian-philosopher of biology, not a biologist, my main tasks in this introduction are to set some historical background and to provide a conceptual context for the papers that follow. I begin with a few comments about the period from

<sup>1</sup> From the Symposium *Evolutionary Developmental Biology: Paradigms, Problems, and Prospects* presented at the Annual Meeting of the Society for Integrative and Comparative Biology, 4–8 January 2000, at Atlanta, Georgia.

<sup>2</sup> E-mail: rmburian@vt.edu

<sup>3</sup> Written while the author was a Senior Fellow in the Center for the History of Recent Science, Department of History, George Washington University, Washington, D.C. 20052. Support of the Center and of Virginia Polytechnic Institute and State University during a sabbatical leave for academic year 1999–2000 is gratefully acknowledged.

1880 to 1920. As of 1880, although the details were hotly disputed, a general account of the main features of mitosis was being worked out, and meiosis was still only dimly understood (Churchill, 1970; Baxter and Farley, 1979; Robinson, 1979; Duchesneau, 1987; Baker, 1988; Maienschein, 1990). During the next thirty years or so, flourishing lines of work were begun on cell division, early embryonic development, evolution of species, fertilization, heredity, and phylogeny. All of these topics were subjects of intense dispute. These problems were generally held to be intimately interconnected, so much so that many biologists considered them inseparable, forming a single nexus that covered what was eventually separated into cytology, embryology (later transformed into developmental biology), evolutionary biology, genetics, and reproductive biology (Maienschein, 1981, 1983, 1986, 1987). From a current perspective it is very difficult to understand the ways in which all of these problems were tangled together at the end of the nineteenth century.

An interesting example of the conceptual and terminological difficulties involved concerns the term “evolution.” Darwin did not use it anywhere in the first edition of his *Origin of Species* (Darwin, 1859)—and he had a good reasons for avoiding the term. Its primary biological meaning was then, and in some contexts continues to be, an unfolding, as in the unfolding (*i.e.*, development) of an organism from a fertilized egg or the unfolding of a fern from a fiddlehead. I have taken this last image from Gould (2000), published after the oral presentation of this introduction. Gould comments on Darwin’s use of the word “evolved” in the famous last sentence of the *Origin* (“Whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved.” [Darwin, 1859, p. 490]). He argues, rightly, that this sentence is meant to contrast the simple revolution of the planets (“cycling”) with the complex variational process of “descent with modification” by which the gloriously diverse forms of life have evolved—and to contrast the *variation-*

*al* basis of biological evolution with the simple unfolding or transformation of forms implicit in the then-current uses of the term “evolution.” Bowler (1983, 1988) provides scholarly reviews of the working out of these issues toward the end of the nineteenth century. Because of this root meaning, the term *meant* development in most biological contexts in the mid-nineteenth century, and was strongly associated with preformationism. This usage remained common well after Darwin died. Consider, for example, two brief questions, posed by Wilhelm Roux, a founder of *Entwicklungsmechanik*. In 1885, three years after Darwin’s death, he asked: “Is embryonic development epigenesis or evolution? Is it the new formation of complexity, or is it the becoming visible of complexity previously invisible to us?” (Roux, 1885, p. 414, as translated in Baxter and Farley, 1979, p. 156). Gilbert Bourne, writing in 1894 on “Epigenesis or evolution?” (Bourne, 1894) connected the developmental meaning of “evolution” as the unfolding of a preformed embryo with evolution of species. He distinguished between narrower and broader senses of the term “evolution.” The narrower sense is the ontogenetic one just discussed; it refers to unfolding from a prior structure in accordance with preformationist theory. The broader sense is phylogenetic, covering evolution and formation of species. According to Bourne, this broader meaning was grafted onto the narrower sense and used in support of orthogenetic theories of evolution, but he also argues in detail (*e.g.*, p. 117) that the broader usage is employed by such opponents of orthogenesis as Wilhelm Roux, August Weismann, and C. O. Whitman. This reflects the fact that their accounts of what *we* call evolution are intimately based on their accounts of development—and thus that their theories differ rather widely from current evolutionary orthodoxy.

Developmental uses of the term “evolution” were reinforced by Oscar Hertwig’s influential book *The Biological Problem of Today: Preformation or Epigenesis?* (Hertwig, 1896). Accordingly, near the turn of this century, many biologists who espoused or contested theories of evolution, meant, among other things, to support or under-

mine theories of the preformation of species according to a developmental plan. For them the issue often focused on the differences between the innovation required for making a new species and the constraints, imposed by the restraining power of heredity, that kept organisms within specific (*i.e.*, *species*-specific) boundaries. If all this is correct, the developmental plan [or, anachronistically, program] of a species had to be violated to create a new species—a violation that could be accomplished by evolution, understood in Bourne's broader, developmental, meaning.

Developmental uses of the term "evolution" remained in effect, among some embryologists at least, into the 1960s—*e.g.*, (Bounoure, 1964). However, most biologists abandoned the developmental connotations of the term by the 1920s, thanks (among other things) to the creation of separate disciplines to deal with the problems of embryological development, of evolution (*i.e.*, origin of species, macroevolution, biogeography, Darwinian selection, etc.) and of heredity. The corresponding separation of problems is often counted as a major accomplishment of the early part of the century. It was especially pronounced in the United States, where the rapid growth of biology in universities and agricultural institutions (Paul and Kimmelman, 1988; Kimmelman, 1990; see also Olby, 1990) favored the formation of new institutions and disciplines, including particularly T. H. Morgan-style genetics.

As we all know, this separation of disciplines also exacted a considerable price, including the erection of barriers separating practitioners of embryology, evolutionary biology, and genetics. The barriers were partly institutional, partly conceptual, and partly practical. With the techniques available before the widespread use of the tools of molecular biology (for practical purposes, at least until the 1950s), embryological studies were conducted mainly on organisms, (*e.g.*, marine invertebrates, anurans, etc.) utterly unsuited to traditional Mendelian genetics, and genetic research with organisms equally unsuited to traditional embryology (*e.g.*, *Drosophila*). For a useful treatment of the historical importance of

choice of organisms along these lines, see (Lederman and Burian, 1993). Furthermore, efforts to establish connections among these newly separated disciplines failed because of the difficulty of connecting the available knowledge of developmental and genetic mechanisms to plausible and testable hypotheses about speciation and long-term evolution and of testing evolutionary claims via findings of embryology or genetics.

Notoriously, the plethora of untestable speculations regarding phylogenies and the evolutionary relations of organisms, rampant around the turn of the last century, drove many major biological pioneers to abandon evolutionary in favor of experimental research. Thus, in their early days, the ideologies of *Entwicklungsmechanik* and genetics both dismissed evolutionary biology as far too speculative and their practitioners insisted on the need to make biological hypotheses experimentally testable. By the 1930s, embryology and genetics had developed sharply divergent conceptions of adequate testing, standards of evidence, and sets of problems whose solutions were considered mandatory for biology. Thus there was a separation of cultures—of practices, organisms, techniques, terminology, and fundamental assumptions. I prefer not to use Thomas Kuhn's dangerous term "paradigm," but it remains true that the deep commitments formed within the separated traditions of these three disciplines made it difficult to resolve the differences in the beliefs and expectations of their practitioners. (Difficult, but not impossible. Some scientists and philosophers interpret paradigm changes as irrational jumps in the face of the impossibility of resolving difficult problems. This is one of my reasons for disliking the term.) So, speaking loosely, the differences in paradigms among these disciplines presented barriers to unified accounts of the hereditary control of development and of the influence of development and changes of development on evolution.

The current flourishing of Evo-Devo represents a sharp reduction of the strength of these barriers, based in part on the availability of new concepts and tools that allow the integration of developmental, evolution-

ary, and genetic studies. The putative modules of development, some of which are discussed in this symposium, can be analyzed via genetic techniques; they contain genes and higher-level units as components, but are not “merely” genetic entities. Some of them, at least, are also putative modules of evolution and their features and histories are being revealed in part by comparative studies across enormous phylogenetic distances. One of the most notorious examples, a module for control of eye formation that is common to drosophila, mice, and humans (Halder *et al.*, 1995; Gehring, 1996; Maas, 1996; Nilsson, 1996; Burian, 1997; Callaerts *et al.*, 1997; Oliver and Gruss, 1997; Gehring, 1998) may stand as an emblem for laymen such as me, but it is a very small part of the story and may turn out to be atypical. The extraordinarily rich details regarding integrated units that function in development, with controls that are preserved through even hundreds of millions of years, provide us with the materials to bring developmental, evolutionary, and genetic techniques to bear on old, but transformed, problems. This triangulation, I believe, will be the key to the success or failure of Evo-Devo.

To succeed in this triangulation, we must revive and transform significant problems articulated in the late nineteenth century, but often in conceptually confused ways. To reunite the findings of the disciplines built up in the interim and to bring the powerful new molecular tools to bear on the transformed problems, we must reformulate those problems reasonably clearly, taking account of the conceptual revisions and the enormous amount of new knowledge gained in the last century. We must also learn to use the distinctive tools of all of the different disciplines involved with appropriate cross-checks—tools from phylogenetic systematics, the study of transcription factors, gene knockouts, and much more. We must find ways to reconcile or bypass the divergent terminologies and assumptions involved so that we can understand each other’s claims and reconcile the findings produced by use of different investigative tools.

To illustrate the sorts of reformulation

and transformation required, let me mention issues raised by two illustrious biologists, one near the beginning and the other at the middle of the twentieth century. As is well known, Wilhelm Johannsen and William Bateson, Mendel’s bulldog, both resisted the chromosomal theory of the gene as elaborated by the Morgan group. Although Bateson, in particular, formally conceded that Morgan might be right about genes in December 1921 (Bateson, 1922), he was never really persuaded that genes *could* be material entities arrayed on chromosomes. A major reason for his stance is not well known, partly because he never published the relevant arguments in full. These concerned the difficulty of supposing that *any* material particle or constellation of material particles could control the formation of a body—a dynamic process—in such a way as to respond to strong environmental insults and nonetheless yield a result that conformed to a specific Bauplan. He apparently thought that there must be some sort of dynamic process or dynamic equilibrium transmitted from generation to generation. In some informal jottings, he played with Chladini figures (the complicated tracings left by a pendulum dripping sand in a complex orbit) and with ideas about harmonic resonances. He was unable to produce any serious models or hypotheses along these lines, but he was convinced that meristic changes, such as changes in the number of vertebrae of the organisms within a lineage, were the consequence of a change in the number of nodes in a vibratory pattern. Thus, such patterns had to be preserved (or perhaps regenerated?) across generations, and there seemed to be no way that a linear sequence of distinct molecules—Morgan’s “genes”—could do such a thing. Implicit here is the requirement that a full-fledged theory of heredity must offer an explicit account not only of how particular parts are formed, but how they are put in the proper place and retained there in accordance with a Bauplan and, for any complex ontogeny, how the sequence of steps is kept in order. Not only did the Morgan group’s chromosomal theory offer no such account, but it also appeared that it was impossible in principle for it to do so. Apparently, Bateson

thought that form could be provided and maintained by transmissible stable harmonic resonances or some such wave-based phenomenon. Furthermore, stable harmonic resonances would be capable of dynamic response to environmental changes, occasionally yielding “mutations,” for example, by changing the number of nodes in a standing wave (Coleman, 1970; Cock, 1983).<sup>4</sup>

Bateson’s mechanisms are of no interest to us. But the desiderata he thought that theories of hereditary control of development should meet are still of great importance. We have acquired conceptual and experimental tools that might be capable of solving the issues he raised in the forty years since the operon model of gene regulation was proposed. One can reasonably hope that some combination of positional information, modularly built dynamic response systems, and spatio-temporal regulation of the behavior of those response systems and their effects can put pieces together to make an organism. This sort of account might help, for example, to explain the similarities and differences between regeneration and formation of an organism from scratch. It might make sense of the recycling of control modules, such as the use of transcription factors two or three times over in the course of ontogeny to initiate or regulate distinct processes. Still, it would be a good idea to ask how far we have come by cross checking our hypotheses and the tests we devise against the explanatory desiderata put forward by Bateson.

As always, God is in the details. An enormous amount of hard empirical work and serious rethinking of what sorts of things can legitimately count as modules in development is required to devise serious tests to determine whether hypothesized modules exist, how they interact, and whether they play the roles ascribed to them. This is one of the tasks that will be addressed in the symposium.

---

<sup>4</sup> For a general historical review of disputes about the nature of the gene, see Carlson (1966); see Burian (2000) for my views on the interplay between the concern with developmental issues and changing gene concepts in the first third of the twentieth century.

Richard Goldschmidt, notoriously, also raised worries about whether we were using the correct units of analysis in genetics and in our attempts to understand development. Late in his life, near the middle of the century, he went so far as to attack the gene concept on grounds (in my terminology, not his) that chromosomes are response systems with such powerful internal interactions that genes are mere abstractions reflecting the interactions of parts of chromosomes when they are in one stable configuration rather than another—*e.g.*, (Goldschmidt, 1950, 1951, 1954). He envisaged some sort of reshuffling of pieces or units within chromosomes that would yield systematically new behaviors, according to the contextual interactions of the parts. While the details of his scheme do not correspond to current knowledge of the so-called dynamic genome, the logic of his argument fits well with that of much recent work that makes developmental changes depend on regulatory changes of relatively stable pieces and processes, with physical reshuffling of parts often a key step in regulatory change. The roles played by the *rec-A* system in *E. coli* or by transposons and other such elements in reorganizing any number of genomes might well be seen as doing honor to the spirit, though not the detail, of Goldschmidt’s vision. And as Rudy Raff and Tom Kaufman, who dedicated *Embryos, Genes, and Evolution* (Raff and Kaufman, 1983) to the memory of Goldschmidt, have taught us, his vision is of major importance to ongoing work in development.

This background provides a setting for the topics taken up in this symposium, which will set forth and clarify some old questions (papers by Hall, Gilbert, Dietrich, Gayon, Donoghue, and Laubichler), clarify a some key concepts (papers by Gayon, Donoghue, Bolker, Laubichler, and Mabee), examine some current problems and the tools with which they can now be attacked (Carroll *et al.*, Mabee, Roth, Arthur and Wagner *et al.*), and argue about the directions in which Evo-Devo should evolve (Carroll *et al.*, Wagner *et al.*). The contributors seek to show that the history with which we deal is very much alive and to challenge workers in Evo-Devo to engage

with larger issues conceptual and issues as well as with the technical questions and techniques that, inevitably, occupy much of their time. By uniting efforts of all these sorts we hope to improve our understanding of the interrelations between development, genetics, and evolution. The contributors to this symposium hope to have erected some useful guideposts for this ambitious task. We will have done well if the mix of problems, historical and conceptual analyses, consideration of appropriate tools to use, and suggestions of directions for research help some readers of these papers in the pursuit of the issues explored in these pages.

#### ACKNOWLEDGMENTS

This symposium received major support from NSF Grant #IBN 9983454 and additional support from the SICB and its Divisions of Comparative and Cell Biology, Evolutionary Developmental Biology, Integrative and Comparative Issues, and Systematic and Evolutionary Biology. The Organizers would like to thank all these sponsors for their generosity. We also thank the contributors to the symposium for their creative responses to our demands and for their hard work and the many Evo-Devo researchers who attended the SICB meeting and helped make this symposium and the inauguration of the Society's new Division of Evolutionary Developmental Biology a success. Given this start, we expect the new division to flourish and look forward to following its progress in future meetings of SICB.

#### REFERENCES

- Baker, J. R. 1988. *The cell theory: A restatement, history, and critique*. Garland, New York and London.
- Bateson, W. 1922. Evolutionary faith and modern doubts. *Science* 55:1412.
- Baxter, A. L. and J. Farley. 1979. Mendel and meiosis. *J. Hist. Biol.* 12:137–173.
- Bounoure, L. 1964. La lignée germinale chez les batraciens anoures. In E. Wolff (ed.), *L'origine de la lignée germinale chez les vertébrés et chez quelques groupes d'invertébrés*, pp. 208–234. Hermann, Paris.
- Bourne, G. 1894. Epigenesis or evolution. *Sci. Prog.* 1:105–126.
- Bowler, P. J. 1983. *The eclipse of Darwinism*. Johns Hopkins University Press, Baltimore.
- Bowler, P. J. 1988. *The non-Darwinian revolution*. Johns Hopkins University Press, Baltimore.
- Burian, R. M. 1997. On conflicts between genetic and developmental viewpoints—and their resolution in molecular biology. In M. L. Dalla Chiara, K. Doets, D. Mundici, and J. van Bentham (eds.), *Structure and norms in science. Proceedings of the 10th International Congress of Logic, Methodology, and Philosophy of Science*, pp. 243–264. Kluwer, Dordrecht.
- Burian, R. M. 2000. On the internal dynamics of Mendelian genetics. *Compt. Rend. Acad. Sci., Paris, III. Sciences de la vie/Life Sciences* 324. (In press)
- Callaerts, P., G. Halder, and W. J. Gehring. 1997. PAX-6 in development and evolution. *Annu. Rev. Neurosci.* 20:483–532.
- Carlson, E. A. 1966. *The gene: A critical history*. W. B. Saunders, Philadelphia and London.
- Churchill, F. B. 1970. Hertwig, Weismann, and the meaning of reduction division circa 1890. *Isis* 61: 429–457.
- Cock, A. G. 1983. William Bateson's rejection and eventual acceptance of chromosome theory. *Ann. Sci.* 40:19–59.
- Coleman, W. 1970. Bateson and chromosomes: Conservative thought in science. *Centaurus* 15:228–314.
- Darwin, C. R. 1859. *On the origin of species by means of natural selection or the preservation of favoured races in the struggle for life*. John Murray, London.
- Duchesneau, F. 1987. *Genèse de la théorie cellulaire*. Bellarmin-Vrin, Montreal-Paris.
- Gehring, W. J. 1996. The master control gene for morphogenesis and evolution of the eye. *Genes and Cells* 1:11–5.
- Gehring, W. J. 1998. *Master control genes in development and evolution: The homeobox story*. Yale University Press, New Haven.
- Gilbert, S. F. and J. Atkinson. (eds.) 1992. Development and macroevolution. A symposium. *Amer. Zool.* 32:101–144.
- Goldschmidt, R. B. 1950. 'Repeats' and the modern theory of the gene. *Proc. Natl. Acad. Sci. U.S.A.* 36:365–367.
- Goldschmidt, R. B. 1951. The theory of the gene. *Cold Spring Harbor Symp. Quant. Biol.* 16:1–11.
- Goldschmidt, R. B. 1954. Different philosophies of genetics. *Science* 119:703–710.
- Gould, S. J. 2000. What does the dreaded "e" word mean, anyway? *Nat. Hist.* 109:28–44.
- Halder, G., P. Callaerts, and W. Gehring. 1995. New perspectives on eye evolution. *Curr. Opin. Genet. Devel.* 5:602–609.
- Hertwig, O. 1896. *The biological problem of today: Preformation or epigenesis?* Heinemann, London.
- Kimmelman, B. A. 1990. Agronomie et théorie de Mendel. La dynamique institutionnelle et la génétique aux Etats-Unis, (1900–1925). In J.-L. Fischer and W. H. Schneider (eds.), *Histoire de la*

- Génétique: Pratiques, Techniques et Theories*, pp. 17–41. ARPEM, Paris.
- Lederman, M. and R. M. Burian. (eds.) 1993. The right organism for the job, A Special Section. *J. Hist. Biol.* 26:235–367.
- Maas, R. 1996. Keeping an eye on eye development. *Nature Genet.* 12:346–347.
- Maienschein, J. 1981. Shifting assumptions in American biology: Embryology, 1890–1910. *J. Hist. Biol.* 14:89–113.
- Maienschein, J. 1983. Experimental biology in transition: Harrison's embryology, 1895–1910. *Stud. Hist. Biol.* 7:107–125.
- Maienschein, J. 1986. Preformation or new formation—or neither or both? In T. J. Horder, J. A. Witkowski, and C. C. Wylie (ed.), *A history of embryology*, pp. 73–108. Cambridge University Press, Cambridge.
- Maienschein, J. 1987. Heredity/development in the United States, circa 1900. *Hist. Phil. Life Sci.* 9: 79–93.
- Maienschein, J. 1990. Cell theory and development. In R. C. Olby, G. N. Cantor, J. R. R. Christie, and M. J. S. Hodge (ed.), *Companion to the history of modern science*, pp. 357–373. Routledge, London.
- Martindale, M. Q. and B. J. Swalla. (eds.) 1998. The evolution of development: Patterns and process. A symposium. *Amer. Zool.* 38:591–684.
- McHugh, D. and K. Hylanch. (eds.) 1998. Evolutionary relationships of metazoan phyla: Advances problems, and approaches. A symposium. *Amer. Zool.* 38:813–988.
- Nilsson, D. 1996. Eye ancestry: Old genes for new eyes. *Curr. Biol.* 6:39–42.
- Olby, R. C. 1990. Rôle de l'agriculture et de l'horticulture britanniques dans le fondement de la génétique expérimentale. In J.-L. Fischer and W. H. Schneider (eds.), *Histoire de la génétique: pratiques, techniques et theories*, pp. 65–81. ARPEM, Paris.
- Oliver, G. and P. Gruss. 1997. Current views on eye development. *Trends Neurosci.* 20:415–421.
- Olsson, L. and B. K. Hall. 1999. Developmental and evolutionary perspectives on major transformations in body organization. A symposium. *Amer. Zool.* 39:612–694.
- Paul, D. B. and B. A. Kimmelman. 1988. Mendel in America: Theory and practice, 1900–1919. In R. Rainger, K. R. Benson, and J. Maienschein (ed.), *The American development of biology*, pp. 281–309. University of Pennsylvania Press, Philadelphia.
- Raff, R. A. and T. C. Kaufman. 1983. *Embryos, genes, and evolution: The developmental-genetic basis of evolutionary change*. Macmillan, New York.
- Robinson, G. 1979. *A prelude to genetics*. Coronado Press, Lawrence, Kansas.
- Roux, W. 1885. Beiträge zur Entwicklungsmechanik des Embryo. *Zt. f. Biol.* 21:411–524.