Constant Factors and Hedgeless Hedges: On Heuristics and Biases in Biological Research

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How does a complex organism develop from a relatively simple, homogeneous mass? The usual answer is: through the (context-dependent) execution of species-specific genetic instructions specifying the development of that organism. Commentators are sometimes skeptical of this usual answer, but of course not *all* commentators, and not always for the same reasons. Here I attempt to lay bare the logical structure of the usual answer through an extended analysis of the heuristics and methodological principles at play in the exploration and explanation of development—and also to show a critical ambiguity that renders the usual answer suspect.

1. Introduction. Biological phenomena are a messy lot. Though this may often be true in other domains as well, in biology, at least, a staggering number of simplifying assumptions must be made just to get a research program off the ground. Historically, the most significant simplifying assumptions (or heuristics) employed in genetics and developmental biology have resulted in the elision of the organism as both nexus and nadir of developmental interactions. Very often, these heuristics are well justified; they are, at least, widely accepted. And yet differences in how they are interpreted and applied generate differences in what we can claim to know about development.

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The central problem of developmental biology is to understand how a relatively simple and homogeneous cellular mass can differentiate into a relatively complex and heterogeneous organism closely resembling its progenitor(s) in relevant respects. This is not a new, or newly recognized, problem. It has been with us since Aristotle, at least. But it is only recently that we have established a handle on how possibly to solve it. I am not convinced that we have yet grasped the right handle, though.

My strategy here is to explore five methodological principles used in biology; the first two of them are general, the next three used specifically in the context of understanding development. I briefly discuss arguments, abstracted from the biological and philosophical literature, for both the use of heuristics as such (the first principle) and for the use of particular heuristics (the second principle). For rhetorical purposes, I interpret the five principles as premises in an argument aimed at explaining development. I then illustrate how variance in the interpretation and application of the second principle yields inconsistent results and biases our biological knowledge in various ways. I argue in favor of an unorthodox reading of one of the heuristics, but a reading required by the imperative to take development itself seriously as the primary explanandum of developmental biology.

2. Heuristics. Let us define "heuristics" as *simplifying strategies to be used in situations of cumbersome investigational complexity* (Wimsatt 1980, Wimsatt 1986; Gigerenzer et al. 1999). There are at least twenty reductionistic heuristics in widespread use today, including those used in conceptualization, model building, theory construction, experimental design, observation, and interpretation.

One crucial caveat about heuristics is that they are purpose-relative. As Wimsatt notes, "all instruments in the natural, biological and social sciences are designed for use in certain contexts and can produce biased or worthless results if they are used in contexts that may fail to meet the conditions for which they were designed" (Wimsatt 1986, 297). Examples might include: the use of analysis of variance as a surrogate for the analysis of causes (Lewontin 1974; Sober 2000); or the use of linkage analysis in psychiatric genetics where the conditions of successful linkage (single gene of major effect, clear diagnostic criteria, known pattern of inheritance, clinical homogeneity amongst affected family members) are not met (Robert 2000a). In using heuristics, then, we must be careful to select the right one(s).

That notwithstanding, without the use of heuristics, we would be much further from solutions to pressing biological problems than we currently are. Here, then, is a first premise of biological research:

1. Simplifying strategies and assumptions, as such, are absolutely necessary in biological science.

This is an heuristic dealing with the use of reductionistic heuristics. Unlike Laplacean demons, human investigators of all stripes have limited intellectual, computational, temporal, and financial capacities. Any biological system to be studied must be simplified in various ways to make it tractable for agents like us. We build simplified models because we are limited beings, and most of the systems we want to understand are too complex in their natural state. So we abstract from them what seem to be the most important or the most easily manipulated variables in order to generate a manageable representation of their workings.

A common heuristic strategy is to simplify the *context* of a system under study. If we want to learn about *intrasystemic* causal factors, we build a model or design an experiment wherein the context of the system is simplified, rather than the system itself. Of course, we sometimes have to do both, especially if the system of interest is particularly complex; in such a case, we might use another kind of reductionistic strategy. But a golden rule of experimental design is: simplify the context first. Hence, a second general principle of biological methodology:

2. Simplifying the context of a system is advantageous if we want to learn about intrasystemic causal factors.

The strategy of context simplification is extensively employed in investigations of the role of genes in development, usually in the form of "environmental control." Here, one holds environmental variables constant across experiments or, worse, actually believes that the environment simply is invariant. One standard approach is to vary genetic factors against a common, invariant background of environmental factors—a standard environment.

Context simplification, instantiated as environmental control, is the basic methodological framework of many researchers creating and employing genome sequence data, for instance. Sequence data are produced by isolating strands of DNA, cloning them, and employing a variety of techniques to ascertain the order of nucleotides and their physical relationship to each other. Genomes, or even individual strands of DNA—the systems under study—do not exist in isolation from natural environments except in the pristine artificiality of the lab; moreover, there are good reasons to believe that even the structure (let alone the functions) of strands of DNA cannot be understood in isolation from their organismal context. Nevertheless, the environments, broadly construed, of DNA were abstracted away and held constant in the effort to generate the sequence of the human genome. (The same is true, of course, of the genome sequences

of model organisms, such as the mouse and the nematode worm.) The context was simplified, the experimental work proceeded, and draft versions of the genome sequence are now at hand.

For the most part, and despite occasional slips to the contrary, biologists are careful in employing the strategy of context simplification. For instance, with rare but notable exceptions—such as Hamer and Copeland 1998; but see Hamer 2002—very few scientists or commentators would today suggest that either nature (genes) or nurture (environments) is singularly decisive in organismal development. Despite the standard use of experimental or interpretive techniques to partition causation into internal (natural, genetic) and external (nurturing, environmental) components, techniques that may be unable by their very design to detect interactions between genes and environments (Wahlsten 1990; Sarkar 1998), many scholars grant that phenotypic traits arise from complex, possibly non-additive, interactions between multiple factors at many hierarchical levels. But not all varieties of interactionism are equivalent, and a vigorous debate has arisen over which varieties in fact take interaction seriously, and which simply pay "lip service" to interaction in a reflexive refrain masking secret adherence to the old nature/nurture debate (Robert 2003).

3. Exploring Development. Let me now briefly spell out three additional premises, again universally granted, which are employed as additional steps, beginning with the first two premises, in (roughly) a chain of argument putatively leading to a conclusion about development. The third premise, already alluded to, states that:

> 3. Genes by themselves are not causally efficacious, as genes and environments (at many scales) interact (differentially, over time) in the generation of any phenotypic trait.

While, once upon a time, biologists and commentators may have been happy to claim that genes determine organisms—body and mind alike—just as other scientists (mainly social scientists) and commentators were happy to claim that the organism is a kind of tabula rasa to be inscribed, shaped, and structured entirely by experience, no one seriously (or, at least, no one justifiably) entertains either of those perspectives today. There are no (overt) genetic determinists these days, even though some environmental determinists persist (usually in an effort to ward off the spectre of genetic determinism). Rather, as Russell Gray has put it, "nowadays it seems that everybody is an 'interactionist'" (1992, 172). So much so, in fact, that those perceived to be stirring the ashes of the nature/nurture debate are called apparently nasty names and relegated to the periphery of accepted scientific practice. This is the legacy of the "interactionist consensus" on development, the view that both genes and environments

(on many scales) are required for the production of some particular phenotype (Kitcher 2001; cf. Sterelny and Griffiths 1999).

The fourth premise is designed to permit investigation of interacting variables in development (in line with premises one and two):

> 4. We decide to focus on the causal agency of genes against a constant background of other factors, for pragmatic or heuristic reasons.

Experimental tractability is a core scientific desideratum. It is nice to imagine the world as full of interconnected parts not meaningfully separable from each other; but just try to analyze the world so imagined, and science grinds to a halt. It turns out that genes are much more experimentally tractable than a wide range of other interacting factors and agents.[1] Though the details cannot be explored in the space permitted, there are at least two scientifically well-regarded philosophical analyses justifying (some version of) premise four, namely, those of Schaffner (1998) and Gannett (1999).

Generally speaking, what we identify as a cause has its causal effects only in combination with additional necessary conditions (which, for pragmatic or other reasons, might have themselves been identified as causes). This idea is epitomized in a fifth and final premise, one that may seem more controversial than the first four but is nonetheless widely acknowledged:

> 5. A trait x is caused by a gene y only against a constant background of supporting factors (conditions), without which x would not be present (even if y is present).

Prima facie, given premises two and three, this fifth premise is well justified. Consider that variations on the fifth premise have been employed as definitions of a "genetic trait," as in Sterelny and Kitcher's sophisticated treatment:

> An allele A at a locus L in a species S is for trait P^* (assumed to be a determinate form of the determinable characteristic P) relative to a local allele B and an environment E just in case (a) L affects the form of P in S, (b) E is a standard environment, and (c) in E organisms that are AB have phenotype P^* (Sterelny and Kitcher 1988, 350).

In other words, as long as that particular allele, in genetic and standard environmental context, is associated with the relevant phenotypic outcome, then that particular allele may be deemed an "allele for" that phenotype. Given the necessity of simplifying assumptions (premises one and two), as

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1. This may be, of course, simply because we have spent so many decades perfecting techniques for genetic manipulation, and because huge amounts of money are available for such activities compared with others (Griffiths and Knight 1998, 255; Robert 2001b).

long as we recognize the critical contextual qualifications (premise three), and also that we focus on allele A for heuristic and pragmatic reasons (premise four), then we may deem premise five to be a plausible singling out of a gene as a cause in organismal development. So far, so good.

The five premises taken together are usually thought to justify the conclusion that:

> 6. Therefore, organismal development is a matter of gene action and activation, as particular alleles have their specific phenotypic effects against standard environmental backgrounds.

This conclusion coheres nicely with the usual explanation for why organisms develop as they do: there is a program or set of instructions for development inscribed in the genes. Of course, genes alone do not an organism make; they must be activated or "triggered"—there is no un-moved mover in the world as we know it; and the DNA must be suitably housed in appropriate cellular and extracellular contexts, which may themselves be very complex, in order for development to proceed. But, given these caveats, the specificity of development—the reliable, trans-generational reconstruction of form—is widely held to be best explained as a matter of gene action and activation.

But is that in fact true? Is development in fact *explained* in terms of gene action and activation? My argument is that it is not, though we may all happily agree, at least in the abstract, with the five premises thought to generate the conclusion above. Are we then illogical or, worse, illogical because ideologically motivated? Or is it rather the case that the five universally acknowledged premises do not actually generate the inference to the usual conclusion? I interpret the inference to the orthodox con-clusion as invalid: The conclusion does not follow from the premises we have before us, because there are two mutually exclusive possible readings of the second premise detailed above, only one of which could be taken to support the conclusion.[2]

4. A Flawed Heuristic? Recall that premise two stipulates that "simpli-fying the context of a system is advantageous if we want to learn about intrasystemic causal factors." Context simplification is usually achieved by holding certain factors constant while solving for others, and decisions about what to hold constant and what to investigate are pragmatically

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2. But even were the second premise perfectly straightforward—as it does, indeed, seem to be—and even were we therefore justified in asserting the conclusion on the basis of the five premises, we would be mistaken to interpret the conclusion as specifying an *explanation of development*. It is, rather, an explanation, or partial explanation, of gene regulation in development.

motivated, as explained in Gannett (1999), for instance. But the pragmatic dimension of these decisions renders the second premise crucially ambiguous: What counts as a system is not a matter of objective determination, but is itself influenced by pragmatic factors, such that what counts as intra-systemic or extrasystemic is decided by a range of considerations and not, as it were, thrust at us by nature. Accordingly, our results are constrained by the experimental design and not the facts of nature.

Several systematic problems (what Wimsatt calls "biases") are associated with environmental control as a context simplifier. First, context simplification is biased toward lower explanatory levels, so simplifying the environmental context stems from, and leads to, focusing on simple components of a system. Higher-level components of systems, and higher-level systems, are legislated out of existence in favor of lower-level systems and their components. Consequently, an investigator who simplifies the context in line with premise two may well be guilty of simplificatory asymmetry (Wimsatt 1986, 300–301). Secondly, we may be prone, should we forget or fail to appreciate the gravity of the simplifying assumption, to draw unjustified causal inferences; it is remarkably easy to fall into the trap of generating causal stories about genes against a constant environmental background (which itself exists only in the laboratory)—hence our fifth premise. We must be eternally vigilant, in simplifying the context, not to exaggerate the conclusions we draw.

I suggested above that premise five strikes us as entirely justified by appeal to premises one through four. But there is no necessity in my particular formulation of premise five, nor in Sterelny and Kitcher's instantiation of this premise. Consider that, by parity of reasoning, we might just as well have (again for some pragmatic reason) postulated, not an "allele for" P^*, but rather an "extracellular environment for" P^* given standard allelic, cytoplasmic, and other environmental contexts (Gray 1992; Smith 1992; Mahner and Bunge 1997; Robert 2000b). That we do not postulate such "extracellular environments for" does not imply that they do not exist; rather, we have decided, for whatever reasons, that "alleles for" are more important to establish. We are thereby guilty of explanatory asymmetry inasmuch as we construe, a priori, the relevant system in strictly reductionistic terms, thereby inviting inference to the conclusion that development is a genetic affair.

This result is fostered by only one of the two possible interpretations of premise two. Both interpretations involve heuristics. I shall refer to the suspect one as the "hedgeless hedge" heuristic (HHH); the other, to be explored and defended below, is the "constant-factor principle" heuristic (CFPH).

The phrase "hedgeless hedge" is owed to Roger McCain, who diagnosed hedgeless hedging as a major limitation of early sociobiological

thinking (McCain 1980; see also Neumann-Held 1999). The notion, though, is more broadly applicable. A typical definition of "hedging" is: protecting oneself from loss or failure by undertaking a counterbalancing action, as in hedging one's bets by not placing all one's eggs in a single basket (an awkward mixture of metaphors, to be sure!). "Hedgeless hedging" is a win-win strategy, denoting a fail-safe type of hedging: one puts virtually all one's faith in *A* and relatively little in *B,* and then attempts to establish *A* but not *B;* but betting on *B* at all (say, by publicly announcing that *B* is true, likely, or possible) provides a measure of safety just in case *B* and not *A*. Less formally, in proceeding according to the hedgeless hedge heuristic, "one admits the existence of an anomaly or problem of theory and then proceeds as though one had not. If one is then accused of neglecting the anomaly, one then produces the admission of its existence as conclusive evidence of one's innocence of the charge" (McCain 1980, 126). The hedgeless hedge is well-characterized as a simplifying assumption, in particular a simplification of context: One admits the implausibility of the simplifying assumption, but proceeds with the simple model nonetheless, generating results inadequate to the reality of the situation; when challenged, one refers back to the original admission of implausibility for exoneration.[3]

The hedgeless hedge heuristic shares with all heuristics the property of fallibility, which is a function of the cost-effectiveness of heuristic use. But the failures of heuristics tend to be systematic rather than random, such that we might identify their failures and correct for them (often by applying a new heuristic). That is, thanks to the systematic biases of simple heuristics, we are able to learn from our false models in generating truer, more complex theories (Wimsatt 1987). What is unique about the hedgeless hedge is that the limitations of the heuristic are so obvious that, even though a hedgelessly hedged model may initiate the production of more adequate models, such models will themselves be so drastically different from the original model that its catalytic role may be overestimated. More-

3. McCain's example of this strategy is sociobiologists' treatment of inheritance. While complexes of many genes are involved in the generation of traits, for purposes of tractability early models of sociobiological inheritance—such as that advanced in Wilson (1975)— reverted to one-locus theory, according to which we assume that one and only one gene is associated with a given inherited trait. As Wilson's mathematical models depend so heavily on one-locus theory, and the assumption of single loci is so inadequate to the reality of both inheritance and development, the model is rendered immediately suspect. McCain observes that Wilson is well aware of his simplifying assumption, and Wilson notes that future models will have to take polygenism into consideration. But to take polygenism into consideration is so completely to undermine the model on which Wilson's treatment of sociobiology rests that the one-locus model itself is virtually worthless. And yet admitting the limitations of the model functions as a hedge against the probability that the model is in fact not at all a good one.

over, the hedgeless hedge heuristic wears its bias on its sleeve, implying that its putative openness is sufficient to make the heuristic appear honest and true. Unlike other context simplification heuristics, the HHH contains within itself the additional mechanism of theoretical exoneration, thereby providing an excuse for denying, say, complexity while nonetheless admitting the existence (and importance) of such complexity.

There are abundant examples of hedgeless hedging in biological research.[4] Elisabeth Lloyd has explored a curious phenomenon, one that she refers to as "ritual recitation" (my "reflexive refrain," above), whereby investigators favorably cite the papers of those who challenge the investigators' theoretical framework, but then proceed as if there are in fact no problems with the framework. According to Lloyd, there is "a peculiar disconnect between what the authors explicitly acknowledge as serious theoretical and evidential problems, and how they actually theorize and evaluate evidence" (Lloyd 1999, 225).

In illustrating this claim, Lloyd discusses the emerging field of evolutionary psychology. According to Lloyd, central texts in evolutionary psychology are rife with footnotes citing, for instance, Gould and Lewontin's paper on the limits of adaptationism (Gould and Lewontin 1979), indicating awareness of problems of pan- or hyperadaptationist evolutionary theory, and sometimes acknowledging the need to avoid committing the errors Gould and Lewontin warn against. But, as Lloyd shows, these citations are smuggled into monographs expressly giving adaptation by natural selection an exclusive role in the evolutionary origin of phenotypic traits. Accused of naive adaptationism, the authors may simply point to the references as putative evidence of their innocence. The issue here, as elsewhere, is "a matter of the *actual weight given in practice*—not in lipservice" to the *B* term of the hedgeless hedging heuristic (Lloyd 1999, 226).

5. Beyond the Hedge. The difficulty with the hedgeless hedge heuristic in the context of development is that it amounts to paying lip service to development rather than taking it seriously. But what would it mean to take development seriously? I suggest that what we need is a better, less suspect variant of a context simplification heuristic, a more honest one, one more adequate to investigating biological reality, and one less likely to yield inference to an inappropriate conclusion about development. Following J. H. Woodger (1952), I refer to this alternative interpretation of the second premise as the "constant-factor principle" heuristic (or CFPH).

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4. For an extended discussion of hedgeless hedging (though not referred to by that epithet) in the case of genetic research regarding the aetiology of schizophrenia, see Robert (2000a).

For Woodger, as for others, the assumption of constant factors is often a useful simplifying strategy in order to achieve experimental tractability. In attempting to understand how genes function, for example, we may assume that the environment is a constant factor; against a constant environmental background, we may then solve for phenotypic differences by exploring the genotype (the variable factor). Where such differences are found, we may account genetically for the existence of variations. The heuristic assumption of constant factors is methodologically commonplace, but it is by no means infallible, as should be evident from the discussion thus far. Nonetheless, I will urge here that Woodger's "constant-factor principle," interpreted as an heuristic, works against the particular biases of the hedgeless hedge heuristic, and so is a more legitimate simplification heuristic and offers a more appropriate interpretation of our second premise.

The most encompassing problem with simplification heuristics, especially as instantiated in hedgeless hedging, is the tendency to downplay or simply neglect the causal significance of those factors held constant. Consider loss-of-function experiments. A typical loss-of-function experiment is one in which, against a constant background, a particular gene is manipulated so that it is not expressed at the right time and place; the investigators then observe the phenotypic outcomes and conclude that the outcomes are caused by the misexpressed gene. But often investigators will, in the absence of a complementary gain-of-function experiment, draw an additional, unwarranted conclusion, namely that the gene, when properly expressed, is itself causally responsible for the correct phenotypic outcome. But this latter inference simply does not follow.[5]

Holding factors constant is a good and necessary part of proper science. *But effacing their causal importance is not.* It is for this reason that we should prefer the constant-factor principle heuristic to other simplifying strategies as a methodological heuristic in making and interpreting experimental assumptions.

The CFPH asserts that, "if, in a series of experiments, certain factors are constant, not necessarily in the sense of unchanging in time, but in the sense of being of the same kind in each experiment, then *nothing can be asserted on the basis of those experiments about the role of such constant factors in the production of the observed result*" (Woodger 1952, 186; italics added). Prohibited assertions, according to the CFPH, include claims that the constant factors "'play no part' in the processes involved,"

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5. In a review of loss-of-function experiments involving homeobox genes in the mouse skeleton, Smith and Schneider (1998) are highly critical of a number of studies in which such an illicit inference is drawn, usually as part of a more general claim of the evolutionary role of the homeobox genes in producing skeletal novelties. See also Robert (2001a).

or that they play only a supportive role. Different experiments, perhaps even different sorts of experiments, are required to establish the latter results; they cannot be inferred from scenarios in which the constant factors are never varied.

Immediately, then, we see that the usual conclusion (6, above) cannot be validly inferred if premise two is interpreted according to the constant-factor principle heuristic. As long as premise two is interpreted as an invitation to hedge hedgelessly, then our near-universal presumption that genes are more causally relevant than other factors in development generates the conclusion that development is best explained as a matter of genes operating against a constant background of supportive conditions. But if premise two is interpreted along the lines of the CFPH, then we are free to imagine (and explore) other scenarios for premise five, and are thus less likely to imagine the validity of inferring the orthodox conclusion.

The second premise, now more satisfactorily interpreted according to the CFPH, reads as follows:

2a. Simplifying the context of a system (the definition of which is admittedly contingent) is advantageous if we want to learn about intrasystemic causal factors, but we must not neglect the possible importance of those contextual factors we abstract away.

Accordingly, we are invited to infer the following from premises (1) through (5), having replaced (2) with (2a):

6a. Therefore, against standard background conditions, aspects of organismal development may be partially a matter of gene action and activation, and it remains to be determined whether (and how) extragenetic factors make a specific causal contribution to ontogenesis.

Due to the limitations of the sorts of experiments undertaken thus far, we just do not know enough about development to conclude that the specificity of development is a matter of gene action and activation; and given any detailed analysis of the science of development, we will often have good reason to be suspicious of any such claim (Robert 2001a). An appropriate interpretation of premise two, coupled with appropriate variations on the fifth premise, demands further, broader explorations of causal factors in development.

The constant-factor principle heuristic is more satisfactory methodologically than either context simplification simpliciter or hedgeless hedging just because it provides grounds to avoid the biases of context simplification, and moreover because it guards against the particular biases of hedgeless hedging. In cautioning against interpretive folly even while promoting the necessity of simplification, the CFPH is a superior guiding principle.

6. Heuristic Superiority. If nothing can be inferred about the causal contribution of those factors held constant in a particular experiment, then we are compelled to undertake different sorts of experiments, varying other factors serially and then integrating the results of the serial experiments. But in conducting such serial experiments, we must be wary of the kind of simplificatory asymmetry Wimsatt (1980, 1986) cautions against in the use of particular heuristics. For as long as the factors to be varied are restricted solely to the class of systemic or intrasystemic variables (against a constant environmental or extrasystemic background), a systematic bias in favor of the model system's independence of the environment may emerge—and yet go unnoticed. So a full application of the CFPH requires appreciating the insight that "what one must control is a function of what relationships one is studying" (Wimsatt 1986, 303) and also what we count as comprising our particular system.

If one is interested only in causal relationships independently of environmental context, then one conducts experiments in which the environment is held constant—which is fine, as far as it goes, although the CFPH cautions that interpretation of the results must be constrained by admission of the limits of the experiment. Such constrained interpretations are few and far between, though, as evinced in recent discussions of what we can expect now that the human genome has been sequenced. But if one is interested in more complete causal analysis, the kind of analysis affording fewer and less onerous interpretive constraints—the kind of analysis legitimately yielding interpretations of real-world significance—then environments cannot be universally held constant, and their possible causal efficacy cannot be universally elided.

What, then, went wrong with premise two? Even in applying a well-chosen heuristic to a particular problem, a crucial caveat to bear in mind is that the application of the heuristic may transform the initial problem into a new one for which an answer is available. Yet, as the new problem is "nonequivalent but intuitively related" to the original problem, *we are no longer in fact solving for the original problem* (Wimsatt 1986, 295). When the transformation goes unnoticed, we may believe we have indeed solved the original problem. But we have not.

It is for this reason that the core problem of development is not, *pace* Rosenberg (1997), entirely solved by modern developmental genetics. The translation of development's hard problem (how a specific complex organism arises from a single, relatively homogeneous cell) into a problem about gene action and activation generates explanations at the level of genes; but these explanations solve (or, rather, begin to solve) the subsidiary problem of the role of genes in development, not the problem of development as such (Robert 2001a). The trick is to integrate these explanations with other developmental (cellular, environmental, ecolog-

ical) explanations within a larger organismal framework, rather than to assume that we understand development because we are beginning to grasp gene function. Multileveled, multidisciplinary analysis—appropriately heuristically informed—is the surest route for generating results adequate to the complexity of development, though from a comparatively simple, tractable, starting point.

To take development seriously is to take development as our primary explanandum, and to resist the substitution of genetic metaphors for developmental mechanisms—though some, perhaps many, developmental mechanisms will indeed be genetic mechanisms, others will be irreducible to genetic substrates. It may well turn out, even if we focus on development as such, that genetics will be our explanans. But we should not assume this a priori, and neither should we blindly aim for this result.

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