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Resisting Dichotomies: Causal Images and Causal Processes in Development

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*We need to find a way of thinking about things
that is not just a way of thinking of alternatives to them.*

Adam Phillips

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Chapter 1

Introduction: Parts, Images, and Processes in Biological Explanations

1.1 General overview

This thesis is about resisting dichotomous ways of thinking, understanding, and explaining. It is not, however, about resisting to make distinctions. We encounter binary distinctions everywhere in the philosophical literature: subject/object, ontology/epistemology, theory/practice, particular/universal. While such dualisms may be methodologically useful in certain contexts, it is the grounding of binary distinctions on dichotomous thinking that needs to be resisted. Fitting distinctions into dichotomous frameworks is imposing a static view on an otherwise dynamic world. In the land of opposition antithesis prevails, and it endangers our understanding. Here I explore the dangers of dichotomous explanatory frameworks in developmental biology, particularly its contribution to explanatory fundamentalism. I do so by examining two theoretical perspectives that exhibit distinct ways of approaching developmental phenomena: a mechanistic approach offered by Genetic Determinism, and a process-based approach offered by Developmental Systems Theory (DST). Each perspective frames the decomposition of biological systems by way of (at least) two theoretical commitments: a commitment to a certain unit (or units) of explanation, and a commitment to a certain image of causation. I argue that the reasons to prefer the process-based approach over the mechanistic approach derive from the choice of an image of causation, rather than from the choice of a unit of explanation. I argue as well that Susan Oyama and James Griesemer both offer compelling ways of articulating explanations of developmental phenomena that integrate two or more explanatory units, thus counteracting explanatory fundamentalism.

1.2 Motivations for the focus on ‘parts’ as a main explanatory category

Looking at contemporary works on explanation, I find that philosophers of science tend to distance themselves from the “Received Doctrine” about scientific explanation, which consists of derivation from laws.¹ Discomfort with this view has its roots in a set of related problems that have proven very difficult to solve. There are concerns with causation, with the explanatory role of contingency and *ceteris paribus* conditions, with giving an account of natural necessity, with a long-standing neglect of the context of discovery and actual scientific practices, with distinguishing laws from non-laws, with laws as a demarcation criterion, and so on.² Instead of attempting yet another possible solution to these problems, some philosophers turn to the so-called “special sciences” (biology, psychology, and economics) in search for an alternative and more widely applicable account of explanation. Cartwright’s (1989) shift from an ontology of laws to an ontology of capacities, Woodward’s (2003) manipulationist account of explanation, and Mitchell’s (2003) continuum account of pragmatic laws, are contributions in this direction.

Within philosophy of the biological and the cognitive sciences, those who distance themselves from the talk of laws address mechanisms, processes, and, to a certain extent, capacities,

¹ For evidence that Hempel and Oppenheim’s (1948) Deductive-Nomological (D-N) model of explanation is the doctrine that has dominated the scene both as an ideal of scientific explanation and as object of study for those concerned with the problem of explanation see Salmon’s *Four Decades of Explanation*, a book devoted to philosophical analysis of the D-N model and its successors.

² In addition to the discussion on what a law is, there is an ongoing discussion on whether there are laws in biology. Volume 96 of *Philosophy of Science* is a collection of works that aim to answer the question “Are there laws in biology?” and to explore the implications of the existence (or non-existence) of laws for biological explanations. For an analysis of the role of Mendel’s “laws” in evolutionary explanations (and one possible answer to the question above) see Beatty (1995). I do not address these quandaries here.

as units useful for the study of explanation. Examples of those who focus on *mechanisms* in the biological sciences are Bechtel and Richardson (1993) and Machamer, Darden, and Craver (2000). Griesemer (2000) focuses on the *processes* of reproduction and development (as we will see, without excluding mechanisms or capacities), while Cummins (1983) speaks of the ascription of *capacities* to functional components in his treatment of psychological explanation. What these views have in common is an emphasis on *parts* as units of explanation (whether the relevant parts are mechanisms, processes, and/or capacities).³ In the same vein, Haugeland (1998) argues in favor of systematic explanations of objects that are composed of distinct parts, where “specifying interactions [of the parts or functional components] is crucial to the explanation” (p. 13). It is important to note, however, that the part-based ontology of objects (or systems) is not given. Rather, the elucidation of the component parts of a system is *achieved* through guided analysis and decomposition of the objects or systems of interest. In the following section (1.3) I provide a preliminary characterization of *units of explanation* that captures this idea.

1.3 Theoretical perspectives and their resources

The idea that a good way to explain the behavior of a biological system is by decomposing the system into parts and then providing an account of how these parts are organized or articulated to produce the phenomenon of interest was first introduced into the modern discussion in the works of Kauffman (1971) and Wimsatt (1974, 1980). The “articulation of parts” move on explanation does not claim that the sum of parts constitutes the whole. Rather, it takes into account that “parts acquire properties by virtue of being parts of a particular whole,

³ Mechanisms have also played an important role in physics-inspired accounts such as Salmon’s causal/mechanical model of explanation (1984, 1998). Although my main interest here is in developmental biology, I do not want to restrict the focus on *parts* to this discipline.

properties they do not have in isolation or as parts of another whole” (Levins and Lewontin (1985, p. 3). Kauffman’s main thesis is that a system can be viewed from a number of different perspectives, each of which yields different decompositions of the system into parts and identifies different properties for the parts. Wimsatt further elaborates this idea by emphasizing that explanation of the behavior of a given system depends on the decomposition/articulation heuristics specified by a theoretical perspective, rather than from idealized deductive accounts derived from theories. Wimsatt characterizes heuristics as non-algorithmic procedures that do not guarantee correct solutions. Moreover, he recognizes that heuristics produce systematic errors that constitute the *biases* of the heuristic, which are specific of a theoretical perspective.

Recently, Griesemer and Winther have followed Wimsatt in arguing that theoretical units frame the analysis of biological systems by providing the prescriptive biases of how to decompose the system into parts. According to Griesemer (2000), theoretical perspectives coordinate the ways in which we decompose, model, and represent phenomena, but they do not offer specific models to represent, interpret, and explain phenomena. This does not mean that theoretical perspectives do not influence the way explanations are constructed; they do so by encouraging theoretical commitments which, once they are made, constitute resources for explaining. In Chapter 2, I provide a characterization of theoretical perspective that involves two resources:

1. *Image of causation.* An image of causation is not a full-blown metaphysical account of causality. It is, rather, a causal schema that reflects a certain understanding of the general part-whole structure of a biological system. Theoretical perspectives and images are related in the following way: “Images express theoretical perspectives in that they specify preferred lines of abstraction from phenomena of interest and also prioritize principles in

terms of which models may be constructed to represent phenomena” (Griesemer 2000, p. S349).⁴ In other words, it is through images that theoretical perspectives provide guidelines for modeling and explaining phenomena. The “central dogma” of molecular biology, according to which the arrows of causality run from DNA to RNA to protein (and not the other way) is an example of an image of causation. Images of causation constrain our explanatory configurations.

2. *Units of explanation.* I understand the practice of system-decomposition as the practice of identifying units of explanation. For example, we may analyze (decompose) a system’s behavior in terms of mechanisms or processes.⁵ As units of explanation, mechanisms and processes sustain explanatory patterns. Mechanisms sustain mechanistic patterns of explanation while processes sustain process-based patterns of explanation.⁶ As Winther notes, however, mechanisms and processes, as well as other aspects of biological systems, such as structures or functions, may be considered parts. *Partitioning frames* determine “whether some aspect, such as a particular mechanism, should be considered a part or a relation be-

⁴ Although Griesemer does not explicitly refer to images as *images of causation*, I think that his characterization of the way in which perspectives and images relate fits with my idea of an image as a causal schema—an explanatory resource provided by theoretical perspectives.

⁵ I do not state capacities as an example of units of explanation because I want to keep in mind that capacities obtain at all levels of organization as well as in all forms of system-decomposition (i.e., mechanisms and processes may exhibit capacities). Capacities, in this sense, are on a different ontological level than mechanisms and processes. I thank James Griesemer and Andrew Hamilton for pointing this out to me.

⁶ It is important to note that what is understood as *mechanism* or *process* (and even whether we can draw a distinction among these) varies historically, and is specific of a theoretical perspective. Moreover, depending on how processes are understood, they may sustain different explanatory patterns (e.g., mechanistic, law-based, etc.). In section 3.3 I briefly explore how processes sustain a “law-based” pattern of explanation.

tween parts” (Winther forthcoming), and in this sense they can guide the identification of second-order units of explanation (i.e., a mechanism that is composed of a set of mechanisms or a process that arises from the relation between mechanisms or other processes).

From this sketch it should be clear that images of causation and units of explanation are closely related, that is, they come together as parts of a theoretical package. Another way of saying this is that they are theoretical co-commitments.⁷ In this project I examine two types of images of causation: foundational and relational, as a means to work out precisely what the relationship is between them.

- A *foundational* image of causation is the causal schema according to which the behavior of a biological system can be explained in terms of *one* fundamental unit of explanation that holds across all levels of (biological) organization.
- A *relational* image of causation is the causal schema according to which the behavior of a biological system requires to be explained in terms of *at least two* units of explanation that may operate in different levels of organization, and neither of which is more basic than the other.

There are several instances in which theoretical perspectives that embrace a foundational image of causation privilege mechanisms as units of explanation. Genetic Determinism, a perspective that embraces the “central dogma” of molecular biology as an image of causation, confers genetic mechanisms a fundamental causal role in the development of biological form. This, however, need not be the case. Privileging a certain unit of explanation (e.g., the

⁷ I borrow this term from Elihu Gerson (1998) and Rasmus Winther (pers. comm.), who have employed the notion of co-commitment for the analysis of concepts in their broader sociological context.

privileging of mechanisms instead of processes) *does not* derive from the appropriation of a foundational image of causation. For example, not all mechanistic explanations possess a foundational character. It is the amalgamation of a single unit of explanation with a foundational image of causation within a reductive account that generates erroneous (or at least suspicious) explanations. As I argue in Chapter 2, the image of causation adopted by DST allows the integration of processes and other units in explanations of development. In Chapter 4, I present Griesemer's Reproducer Perspective as another view that embraces an image of causation suitable for the integration of several different explanatory units.

1.4 Resisting dichotomies

In Chapter 2, I characterize the philosophical project of DST as a critical and constructive approach to explaining. DST's dissatisfaction with the dichotomous explanatory framework set forth by Genetic Determinism constitutes part of its critical project. There are two side effects of Genetic Determinism and its commitment to a foundational image of causation: causal disparity and explanatory asymmetry. Both of these are instantiated in the dichotomous explanatory categories that DST resists: gene/environment, biology/culture, nature/nurture.

These dichotomies and their asymmetrical implications present themselves in many guises. One is a variant of the argument from design. First we recognize regularities in our surroundings. Then "We formulate...a descriptive rule, which is a form of knowledge, and infer from it a prescriptive rule, separate from the processes we see and controlling them" (Oyama 2000a, p. 12). We end up claiming that our nature is created by a genetic plan, the metaphor of the prior plan becoming not only harmless but also useful. Dichotomies are also present in what Oyama calls the modern version of preformationism. For eighteenth century preformationists, form was concretely material, miniaturized and encapsulated; for modern geneticists form is inscribed

in a molecule, it is “initially material but cryptic, then manifested in the phenotype” (Oyama 2000a, p. 29). Genetic causes are thus seen as that which “has the power to effect change without being changed” (Oyama 2000a, p. 30)—a modern version of the unmoved mover of Aristotle and Aquinas.

These dichotomies are even more patent when genetically encoded behaviors are distinguished from those that are said not to be genetically encoded, the inherited are told apart from the acquired, and ontogeny and phylogeny are seen as “*alternative processes whereby information enters the organism*” (Oyama 2000a, p. 15). Dichotomous thinking, claims Oyama, “is the very frame on which our endless nature-nurture disputations are woven” (Oyama 2000a, p. 15). It is, however, also the frame that leads to ingenuous interactionist views about development. Because “It is a truism that all traits are influenced by both genetic and non-genetic factors ... this “interactionist consensus” is little better than the nature-nurture dispute” (Oyama, Griffiths and Gray 2001, p. 2).

Conventional interactionism, which has been a common attempt at solving the nature/nurture dispute, reinforces a dichotomous explanatory framework and justifies “building theories of development and evolution around a distinction between what genes do and what every other causal factor does” (Oyama, Griffiths and Gray 2001, p. 3). This is precisely what DST’s parity thesis wishes to resist. DST does not claim, as I argue in Chapter 3, that all the sources of causal influence on development play the same role, independently of the way we group causal influences. Rather, it claims that “differences between the *particulars* of the roles of causal genes and [other] factors” does not legitimize the construction of explanations that presuppose *in principle* causal disparity (Oyama, Griffiths and Gray 2001, p. 3). Critics and sympathizers of DST have addressed both the parity and the symmetry theses. I present Kitcher’s critique of DST and my version of Oyama’s defense of such criticisms in Chapter 3.

1.5 The scientific practice of explaining

The object of study of DST is the activity of explaining the development of organismal form, that is, the activity of generating biases and providing guiding assumptions as well as explanatory resources to formulate explanations. In his Foreword to the second edition of Susan Oyama's *The Ontogeny of Information*, Lewontin writes: "There are two difficulties with the current trend of explanation of development. The first is that the wrong question is being asked; the second is that, even to the extent that we are concerned with the answer to that question, the wrong answer is being given" (vii). But explaining, as DST's object of study, is not so much about asking questions and answering them as it is about *identifying and contesting the assumptions (or commitments) that underlie the types of questions asked and the types of answers given*. DST's project is importantly different from the more standard philosophical exercise of giving accounts of explanation. DST, as I read it, is concerned with the underlying constraints and possibilities on explanation (of various kinds) that result from committing to a unit of explanation and an image of causation. Putting this another way, theoretical perspectives bias the kinds of explanations offered. The DST project is to force these biases to the fore so they can be examined.

This concern with the examination of the biases inherent in working from a theoretical perspective is what motivates Oyama's resistance to dichotomies in explanation. In the Afterword to her book's second edition, Oyama writes: "My principal quarrel with those who speak of genes that foresee, recognize, or organize is not so much that figurative language is used for molecules as that it is used for some molecules and not others, and not only to enliven the narrative, but to *explain* how the living world works" (Oyama 2000a, p. 201). Oyama points to the many dangers of assuming dichotomous explanatory frameworks. These present themselves in different spheres, and have important implications that are not restricted to the field of developmental biology.

For example, nature-centered explanations of behavior were paradigmatic of E. O. Wilson's sociobiology, which emphasized the genetic basis of human behavior. Critics of sociobiology, in turn, underlined the importance of environmental influences above biological causes. DST shows that the inclination toward oversimplified polarities can yield only vacuous explanations. Another danger, of which Lewontin, Rose, and Kamin (1984) warn, is that advancing "a reductionist explanation of human life in which the arrows of causality run from genes to humans and from humans to humanity...is more than mere explanation: It is politics" (p. 18). Even as sociobiology and politics do serve to demonstrate the importance of how explanations are structured, here I do not deal with these topics, but with other pressing problems in the philosophy of science, which DST also helps to reconsider.

While DST does not advance an account (or model) of explanation, DST *does* set the parameters for the discussion about what sorts of "part-based" accounts of explanation (to borrow Winther's terminology) are useful for development. As I argue in Chapter 3, Kitcher's criticism of DST on the ground that it does not provide a useful account of explanation is wide off the mark, albeit in an interesting and instructive way. Even though theoretical perspectives constrain our explanatory configurations of development, they do not commit us to one particular account of explanation. DST's explanatory openness is about maintaining the possibility of providing different part-based accounts—or better, of integrating different explanatory patterns—within a single pluralistic perspective. As I see it, DST is not the only process-perspective pursuing this objective, and I do not mean to advertise it as *the* better alternative. Several objections can be made to DST. I point to these in Chapter 4, when I present Griesemer's Reproducer Perspective.

This plurality of part-based accounts leads to some questions about the relationships between them. In Chapter 4, I take up these issues in a preliminary way through an examination of Griesemer's Reproducer Perspective (RP). A look at RP helps to

shed light on both the philosophical project of DST, and on how process-perspectives tackle part-based explanations. A particularly interesting feature of Griesemer's approach is that he uses several kinds of explanatory patterns (by incorporating several units of explanation) to provide a new perspective on development and reproduction. My treatment does not tie up all the loose ends on these topics. On the contrary, it points to several interesting new questions about the extent to which explanatory patterns compete or can be integrated, and about whether and how several theoretical perspectives can be pulled together into a "patchwork" to form a more complete picture of developmental systems. In a final section of this work, I identify these and other issues raised by my discussion that demand further clarification and research.

Chapter 2

Philosophical Project of Developmental Systems Theory

2.1 Aim and scope of DST

Developmental Systems Theory (DST) is a critical stance towards “received” explanatory accounts of biological phenomena, more specifically, of genocentric explanations of biological development. While DST has the specific aim of scrutinizing such accounts, it is heterogeneous in its component ideas. In this section I provide a general overview of DST. First I review *some* of the ideas that have historically influenced DST. Then I present Susan Oyama’s founding document, *The Ontogeny of Information*. Underlying DST’s intellectual project is a principled resistance to dichotomies such as gene/environment, nature/nurture, biology/culture. Here I focus on the critical motivations of DST, which greatly delineate its aim and scope as a theoretical perspective. In section 2.2, I characterize DST’s constructive approach to explanation.

2.1.1 Historical origins of DST

The first part of *Cycles of Contingency* (2001), the most recent collective work on developmental systems, provides a history and outlines the sources influences on DST. It presents views from fields as diverse as embryology, ethology, developmental psychology, ecology, and evolution, all of which had in common a critical treatment of binary distinctions. One of these is the inherited vs. acquired opposition, a categorization that was commonly accepted to explain the origins of behavior in early twentieth century psychology (Johnston 2001). According to Johnston, inherited behavior was coupled with instinct, and was understood to be an inherent, unalterable or essential part of the individual’s makeup that resulted from the evolutionary history

of the species to which it belonged. In one of the inherited/acquired opposition's most far-reaching formulations, instinct became a determinant of behavior, the foundation on which all behavior was based. Learning, environmental resources, and experience were regarded as subsidiary influences on behavior. When Konrad Lorenz advanced his theory of instinct during the first half of the twentieth century, "he could consistently speak of behavior as being "innate" or "inherited" as though these words surely referred to a definable, definite, and delimited category of behavior" (Lehrman 2001, p. 26).

Anti-instinct movements and other critical views contested antithetical formulations such as the inherited/acquired opposition (e.g., Dunlap, Kuo, Carmichael, Lashley, Tinbergen, and Lehrman). This dichotomy was criticized on the theoretical ground that appealing to instinct—where instinctive behavior equals inherited behavior—as an explanatory category explains away behavior and blocks further investigation into its ontogeny. It was also criticized for lacking empirical evidence to support it. Lehrman's critique of Lorenz's theory of instinct focused on Lorenz's conviction that it would always be possible to identify purely innate and purely acquired behaviors on the basis of whether the development of a behavior is determined by genes or the environment. Johnston (2001) writes that in his quest of a coherent alternative to instinct theory,

[Lehrman] provided both theoretical arguments and empirical evidence to show that behavior cannot be neatly divided into the categories of learned and innate. Instead, he argued, we must analyze the development of every pattern of behavior in terms of a continuing interaction between the organism and the environment (*not* between the genotype and the environment, as is sometimes proposed) (p. 18).

The idea that we should seek explanations of development in the interactions that occur within the developing organism and between the organism and its environment, in addition to informed repudiation of the innate/acquired, genetic/envir-

mental distinctions, are initiatives proposed by developmental psychology as well as by several areas of the cognitive, social, and biological sciences. These initiatives constitute one of the points of departure of DST.

Another important influence is Levins and Lewontin's criticism of the idea that "the genes propose and the environment disposes," an idea often claimed to be central to Darwinism (Levins and Lewontin 1985, p. 88). DST aims at eradicating this idea, pervasive in both developmental and evolutionary biology, that the inside and outside of an organism are separate domains of causation that work autonomously and independently. This traditional view is grounded on two fundamental metaphors: the genetic program and the selecting hand.⁸ The first depicts development as "an unfolding or unrolling of an internal program that determines the organism's life history from its origin as a fertilized zygote [*sic*] to its death," the other asserts that evolution is an autonomous selecting hand that shapes species to fit the requirements of an external environment" (Lewontin 2001, p. 55). Developmental systems advocates claim that replacing such views with a systems view in which causal factors are both interdependent and mutually constraining, and which depicts development and evolution as interrelated processes of dynamic entities, may help to reformulate the relationship between development and evolution. But perhaps most significantly—at least with respect to the philosophical problem that concerns me here—DST may help to reformulate causal explanation en route to a more integrative account of different explanatory pattern. As I argue in sections 2.2 and 2.3, DST's repudiation of causal dualisms results in the proposal of a relational image of

⁸ There is a considerable amount of literature devoted to the history of the concept of genetic program and other metaphors in the explanation of development. For example, Evelyn Fox Keller (2003) identifies three historical periods in the explanatory framework of genetics that introduced, each, particular concepts for framing the problem of development: gene action, genetic programs, and positional information (see Part Two of her book). See also Donna Haraway (2004) for a study on the "metaphors that shape embryos," and Lily Kay (2000) for a treatment of informational metaphors.

causation, one which allows the integration of several units of explanation within a single account.

2.1.2 *Founding document of DST*

The founding document of DST is Susan Oyama's *The Ontogeny of Information*, originally published in 1985 (revised second edition: 2000). Oyama begins this book by criticizing a set of shared assumptions that underlie explanations about how organisms acquire their forms. The orthodox way of explaining, she says, is to assume that form somehow preexists its development and appearance in the organism, and that preexisting form is causally powerful and explanatory. While eighteenth-century preformationists postulated that biological structure was encapsulated within the organism, information is the source of form in the modern version of preformationism.⁹ According to her, this doctrine is not very different from the modern variant of the argument from design, whereby "our nature is created by a genetic plan, an intelligence in the chromosomes, which was in turn created by natural selection" (Oyama 2000a, p. 12). Because no modern scientist is willing to testify against the existence of non-genetic causal influences (if these do not exist, what does genetic information regulate, direct and control?), a dualistic conception of developmental causation arises: we can draw a distinction between genetic causes and "other types" of causes. This causal dualism "undergirds the opposition of biological to cultural processes, the mare's nest of biological determinism and the whole nature-nurture complex" (Oyama 2000a, p. 13).

The image of causation that is at work within this framework is foundational, one in which "the arrows of causality run from genes to humans and from humans to humanity" (Levins and Lewontin 1985, p. 18). What this image depicts is the privileging

⁹ According to Clara Pinto-Correia too, molecular biology is "the final successor of preformation," and "is currently threatening to take over the entire field of developmental biology" (1997, p. 309).

of a single unit of explanation: genetic *mechanisms*. Dressed in explanatory guise, some of the most outrageous instances of “the gene for” locution grasp this foundational image: there are genes for aggression, genes for sexual preference, genes for infidelity, even genes for religious faith!¹⁰ Genes (that is, genetic mechanisms) are the canonical causes of the development of form; “other causes” (e.g., mechanisms that involve environmental factors) are either relegated to the background or responsible only for altering the normal course of development, for meddling with the “default phenotype” (notice that even when conceding non-genetic factors a more relevant causal role, the idea that genetic mechanisms play a central causal role retains explanatory import).

According to Oyama, combatting Genetic Determinism requires acknowledging that “information is developmentally contingent in ways that are orderly but not preordained,” which in turn requires altering “many of our ways of thinking about the phenomena of life” (Oyama 2000a, p. 3). Combatting Genetic Determinism requires acknowledging, to use Haraway’s words, that “[f]ar from connoting a fixed type, form is a formative process” (Haraway 2004). How does DST recommend that we resist the gene/environment, nature/nurture oppositions? A standard attempt is what Oyama (2001) has dubbed “conventional interactionism.” One version of it claims the following: because nobody denies that all phenotypes are the joint product of genes and environment, the real debate is not about whether a particular trait is due to nature or nurture, but rather how to partition causal responsibility for the trait into additive

¹⁰ Dean Hamer, author of *The God Gene: How Faith Is Hard-Wired Into Our Genes*, is convinced that his studies show the primacy of “nature” over “nurture,” for the people carrying the God gene—a vesicular monoamine transporter that regulates the flow of mood-altering chemicals in the brain—are more likely to develop religious faith, independently of the environment they grow in. Hamer’s claim about having identified the “God gene” is as suspicious as his assertion, more than a decade ago, that he had identified a DNA sequence linked to male homosexuality (see Elizabeth Day 2004).

components. For example, everybody knows that intelligence is neither innate nor acquired, but we can still figure out whether intelligence is 50 percent or 70 percent genetic; “[t]he nature/nurture debate is thus allegedly resolved in a quantitative fashion” (Oyama, Gray, and Griffiths 2001, p. 1).

Another version of conventional interactionism relies on a faint distinction between “privileged” causes and “auxiliary” causes. It formulates explanations under the supposition that we should embrace nature *and* nurture, as every phenotype is the outcome of interaction between antagonistic causal factors (usually genetic mechanisms) and supporting causes. However deflationary it might be with respect to oppositions, conventional interactionism reinforces dichotomous explanatory categories; it combines “encoded nature” with varying doses of “contingent nurture,” therefore solidifying the nature/nurture debate instead of solving it. DST offers constructivist interactionism—the interactive and interdependent work of causal relations in the constructive development of an organism’s form—as a solution to the nature/nurture debate.

By focusing on the developmental construction of an organism’s life cycle, which “comes into being through the interactions between the organism and its surrounding as well as interactions within the organism” (Oyama, Griffiths, and Gray 2001, p. 4), DST supports the use of processes as explanatory units. This does not mean that DST rejects the use of mechanistic patterns of explanation. DS advocates believe that processes capture the causal, part-whole structure of developmental systems in a more appropriate way. Genetic mechanisms play an important causal role in an organism’s development as they become embedded in developmental processes.

2.2 Characterization of DST

In section 2.1, I reviewed the basic ideas that motivate the developmental systems project. But what exactly is DST? Robert, Hall and Olson (2001) describe DST as being “not so much a single

theory as a set of theoretical and empirical perspectives on the development and evolution of organisms” (p. 954). DST is also described by Oyama, Gray, and Griffiths (2001) as “a general theoretical perspective on development, heredity and evolution, a framework both for conducting scientific research and for understanding the broader significance of research findings” (p. 2). Bateson (2001), Nijhout (2001) and Neumann-Held (2001) use the term *approach* more or less interchangeably with *theory* and *perspective* when referring to DST. While most developmental systems advocates have no quarrel with using these terms interchangeably, some authors have developed characterizations that do require a more consistent terminology. Godfrey-Smith (2001) offers a dual characterization of DST as research program and philosophy of nature. In sub-section 2.2.1, I critically examine Godfrey-Smith’s interpretation of the developmental systems project as a means to providing my own operational characterization of DST. In sub-section 2.2.2, I consider Griesemer’s account of DST as a theoretical perspective.

2.2.1 DST as research program/philosophy of nature¹¹

In his contribution to *Cycles of Contingency*, Godfrey-Smith proposes that DST—a set of scientific and philosophical ideas—can contribute to two different intellectual projects: as research program DST suggests positive and negative heuristics¹² that affect biological investigation, and as philosophy of nature DST comments on the overall picture of the world that biological science has to offer us (Godfrey-Smith 2001, p. 284). Godfrey-Smith’s purpose is not to evaluate whether DST operates exclusively with respect to one intellectual project or the other. However, he does distinguish what we can expect from DST as

¹¹ I am grateful to Melinda Fagan for early discussions on this topic.

¹² There are several ways in which the term heuristic can be understood. Here I follow Wimsatt’s (1980) characterization of heuristic as a “a ‘cost-effective’ way of producing a solution” (p. 220).

research program from what we can expect from DST as philosophy of nature.

According to him, it is possible to evaluate whether DST the research program offers heuristics and strategies that facilitate empirical investigation (as Gray 2001 suggests) or that hinder it (as Kitcher 2001 asserts). But DST qua philosophy of nature need not be “a useful tool in the laboratory, or a good heuristic for guiding research” (Godfrey-Smith 2001, p. 289). Philosophy of nature, in his sense, comes after empirical science. It is also autonomous from science insofar as it formulates its own concepts and addresses its own questions. The goal of philosophy of nature, then, is not to contribute to the scientific endeavor in a significant way. It is, rather, to critically evaluate the way in which we approximate the world and to assess our descriptions of it.

While Oyama might agree with Godfrey-Smith that much of DST’s work derives from a dissatisfaction with the general picture of the world offered by contemporary biology (in particular, with what Godfrey-Smith calls the preformationist pattern of explanation), she would disagree with him that DST’s critique is autonomous from its ability to influence science-in-the-making, for “the nature of the critique...prohibits an absolute distinction between foundation and application” (Oyama 2000a, p.10). In employing the philosophy of nature/research program distinction to elucidate the intellectual project of DST, Godfrey-Smith falls victim to precisely that which DST struggles so hard to resist: dualisms and dichotomies.

Moreover, distinguishing between philosophy of nature and research program only to recognize that “existing DST writings tend to combine contributions to both projects” or worse, underlining that this distinction “should not be taken to deny an *interaction* between more philosophical and more empirical commitments within science” (Godfrey-Smith 2001, p. 285, emphasis added) simply fails to capture what DST is all about. Paraphrasing Oyama (2000, p. 7), Godfrey-Smith turns to interaction in search for a solution to a problem but ignores that

the solution is the problem. I do not mean to say that, in characterizing DST, we can do without the space for philosophical reflection created by what Godfrey-Smith calls philosophy of nature. I also do not mean to say that the best characterization of DST (if we are to reject Godfrey-Smith's distinction) is unstructured amalgamation of ideas and objectives.

My minimal claim is that the project of DST makes no sense if we characterize it in dualistic terms. DST is not about the possibility of influencing empirical research with philosophical ideas—DST the philosophy of nature crossing the heuristic border every so often to visit DST the research program. Accepting that DST's philosophical work "might well come to have an effect on the science itself" by changing "the hidden or overt philosophical commitments of the scientists" is of no use to characterize DST if we also assert that "the absence of such an effect on science does not rob the philosophical work of its value" (Godfrey-Smith 2001, p. 285). I find such an appreciation inadequate for a framework that would not consider its job done (or that would consider it an empty exercise) if it did not address all of the following: criticizing the picture of the world that scientists offer, identifying commitments and problematizing assumptions at the very time they are being invoked, and proposing a better way to redescribe the natural world. Godfrey-Smith (2001) is aware that "[m]aybe defenders of DST will not want to accept the strategy offered...they might think that "philosophy of nature"...is empty or at least dubious" (p. 289).

While I do sympathize with DST, I do not discard Godfrey-Smith's characterization because I am a "defender." I do so because I am convinced that it fails to capture what I take to be the philosophical project of DST: a critical and constructive approach to the scientific practice of explaining. What is at stake then, for DST, is not so much a revision of the research strategies employed in biological investigation, but a revision of our understanding of the significant biological causes and entities, which is also a revision of what we consider to be the appropriate units of explanation.

2.2.2 DST as theoretical perspective

Also in search of some clarification of the developmental systems project, Griesemer (2000) distinguishes between developmental systems as a *theory* and as a *theoretical perspective*. For Griesemer, there are important differences between theories and perspectives. Theoretical perspectives coordinate the ways in which we decompose, model and represent phenomena, but they do not offer what theories do, that is, specific models to represent, interpret and explain phenomena. In accordance with this distinction, what Oyama formulated in 1985 is a developmental systems *perspective* (DSP), not a developmental systems *theory* (DST).¹³

Because “perspectives do not explain” (Griesemer, pers. comm.), DSP does not provide explanations. Griesemer should not be interpreted as arguing that a theoretical perspective makes no contribution to the overall project of explaining. While theories yield models for explaining, theoretical perspectives yield guidelines for theorizing and for modeling. If theoretical perspectives do not provide explanations (finished products), they do provide guiding assumptions and encourage theoretical commitments that, once they are made, become resources for explaining.

Winther suggests that there is an ambiguity in the term *theoretical perspective* in the way Griesemer and other authors (e.g., Kauffman, Wimsatt, Gerson) use it: “depending on the purpose of the philosopher or sociologist of science, the term can be used as either a descriptor of scientific activity or the biases guiding such activity” (Winther 2003, p. 85). If we understand this distinction to be exclusive, then, as a descriptor of scientific activity, DST fits Godfrey-Smith’s category of research program.

¹³ According to Griesemer (2000), what Gray and Griffiths formulated in 1994 is a Developmental Systems *Theory* that *does* offer descriptive models to represent, interpret, and explain developmental phenomena. Here I address Oyama’s 1985 (2000a) book as the representative document of DST, so its characterization as theoretical perspective is more relevant to my work.

However, following Griesemer (and Wimsatt, before him), Winther (2003) employs the term *theoretical perspective* in a non-exclusive way, “in the sense of guiding biases *as well as* assumptions” that “direct scientific activity, in a discipline, in a consistent manner” (p. 85, also note 109). Whether I speak of DST’s critical or constructive approach to explaining, I refer to DST as a theoretical perspective in this sense —where causal images and identifying units of explanation influence scientific activity. Although here I focus concretely on the scientific activity of explaining, I also take into account that “for any particular perspective, such biases and assumptions are more compact and different in kind than the sum total of scientific activities” (Winther 2003, p. 85). I retain the acronym ‘DST’ only to avoid further contrasting and clarification.

2.3 A critical and constructive approach to explaining

In this section I return to Griesemer’s notion of theoretical perspective to provide a more detailed characterization of DST. I take theoretical commitments, in the form of an image of causation and a certain unit (or units) of explanation, to be the main features of a theoretical perspective. In sub-section 2.3.1, I concentrate on DST’s critique of the commitments made by Genetic Determinism. I then turn to DST’s own commitments and constructive project, in sub-section 2.3.2.

2.3.1 Resisting causal dualism, rejecting explanatory fundamentalism

DST rejects the image, offered by Genetic Determinism, of genes as privileged causal agents that direct the course of development. In section 2.1, I pointed out that Genetic Determinism is grounded on causal dualism: the conviction that we can distinguish between genes and “other types” of causes. Because such distinction results in a hierarchical arrangement of causal

factors, it leads to the identification of a fundamental unit of explanation (in this case, genetic mechanisms).

More particularly, Genetic Determinism advances:

- (a) a binary distinction between genes and “other types” of causes, where the first are more fundamental than the latter, and
- (b) that genetic mechanisms are “the primary causal explainers of development, heredity and evolution” (Griesemer 2000, p. S354),

while DST rejects:

- (a) “the dichotomy of genes vs. other developmental resources in favor of a causal democracy of many kinds of resources equally necessary to produce developmental outcomes” (Griesemer 2000, p. S350), and
- (b) “the privileging of genes over other developmental resources in causal explanations of development and evolution” (Griesemer 2000, p. S350).¹⁴

We might say that two types of theoretical commitments are at work within each perspective: (i) the commitment to an image of causation (ontological commitment), and (ii) the commitment to a specific unit (or units) of explanation (epistemological commitment). The point I want to make is not about whether the privileging of certain units of explanation derive from the appropriation of a certain image of causation (whether epistemological commitments derive from ontological commitments or vice versa).¹⁵ Rather, my claim is that these commitments are so

¹⁴ These two items are usually referred to as a) causal parity, and b) explanatory symmetry. As we will see in sub-section 2.3.2, causal parity and explanatory symmetry reject one sort of image and demand another, but they are not images themselves (in the way Weismannism is an image, for example).

¹⁵ Salmon (1989) refers to the idea that, for scientific explanation, ontology precedes epistemology, as a “bottom-up” approach to explanation (e.g.,

embedded in a theoretical perspective that they come in the same package (and in this sense the epistemological/ontological distinction is diluted).

Recapitulating in these terms, Genetic Determinism is a theoretical perspective that advances a foundational image of causation and promotes the privileging of genetic mechanisms as units of explanation. Genetic Determinism's foundational image of causation is one in which "information flows in only one direction," and only from one source, "from the genes to the structure of the proteins that the genes bring about" (Gottlieb 2001, p. 46). This image of causation corresponds to the central dogma of molecular biology, which in turn is a reformulation of Weismannism. Griesemer identifies important similarities between Weismannism and the central dogma: "Weismannism expresses the causal logic of germ and soma, gene and protein. Germ/DNA exhibit genetic continuity across generations and also are the causes in development of soma/protein" (Griesemer 2000, p. S354). Moreover, according to Genetic Determinism, this unidirectional flow of information occurs through a single type of means: genetic mechanisms. What becomes important, for explanatory purposes, is the accurate description of such mechanisms (the description of the activities of genes).

It is the contention of DST that information useful for the development of an organism is not only provided by genes. This does not mean that genetic mechanisms are unimportant for explaining biological development, simply that they are not the fundamental explanatory units of development. Indeed, within a

Salmon's own mechanistic account). He differentiates his view from a "top-down" approach to explanation, the idea that epistemology precedes ontology (e.g., Kitcher's unificationist account). In section 3.2, I argue that Kitcher's epistemological critique of DST is grounded on the conviction that a "top-down" approach to explanation is the better scheme. By failing to see that this distinction is weakened—indeed, removed from explanatory discourse in biology—by DST, Kitcher fails to engage in the more significant discussion of what is the appropriate image of causation and what are the appropriate units of explanation for biological development.

systems view there is no fundamental cause and no fundamental unit of explanation:

The roles played by the vast and heterogeneous assembly of interactants that contribute to a life-course are system-dependent and change over time. So DST creates an inhospitable context for moves that preempt the investigation of actual processes by identifying one type of source as controlling or directing the process, leaving other interactants to function as background conditions, raw materials, or sources of disturbance (Oyama, Gray, and Griffiths 2002, p. 5).

For DS theorists, genetic mechanisms play an important causal role in an organism's development as they become embedded in developmental processes, that is, as they become part of the "processes that collectively and successively constitute the developmental system" (Oyama 2000a, p. 169). For Oyama, processes are more or less stable configurations of causally relevant factors, that is, processes are notoriously regular or invariant, and this is the reason why she considers them the proper units of explanation for developmental phenomena (see Oyama 2000a).

While DS theorists resist the widespread talk of genetic blueprints, they do not dismiss genes as causal interactants. It is the concept of gene as "program" or "prior plan," which results in the identification of genetic mechanisms as fundamental units of explanation, that they reject. There are, however, different gene-concepts (or even different ascriptions of a same gene-concept, such as the developmental gene) that may be compatible with a process-perspective.¹⁶

Followers of the Developmental Systems approach have themselves brought forth new characterizations of "gene" whose aim is to emphasize explanatory anti-fundamentalism. One of these alternative notions is the *process molecular gene*, as proposed by Eva Neumann-Held in 2001. Another is Lenny Moss' (2001) Gene-D or *developmental resource gene*. The

¹⁶ For a historical and epistemological perspective on gene-concepts, see Beurton, Falk, and Rheinberger (2000).

process molecular gene is a relational concept defined as “the process that binds together DNA and all other relevant non-DNA entities—including a developmental environment—in the production of a particular polypeptide” (Neumann-Held 2001, p. 76). A Gene-D is defined by its specific molecular sequence and thereby functional template capacity, but it denotes only a resource from which different results may arise in different contexts. While Neumann-Held aims to extend the developmental systems approach to the molecular level of organization through a unified gene-concept, Moss relies on the possibility of embracing a developmental, instead of a preformationist notion of gene, in order to preserve “the efforts of DST to formulate a perspective which does not presume the causal (or ontological) priority of any particular kind of entity and thereby maintains an explanatory openness on all empirical fronts” (Moss 2001, p. 90). In Chapter 3, I argue that explanatory openness, rather than unification, better coheres with the philosophical project of DST.

2.3.2 Changing commitments: causal parity and explanatory symmetry

Up to now I have presented the critical aspects of DST. Here I outline the constructive contribution of DST by focusing on two of its theses: *causal parity* and *explanatory symmetry*. Although causal parity and explanatory symmetry are not listed as “central tenets” of DST in recent review articles (e.g., Robert, Hall and Olson 2001, as opposed to Schaffner 1998, who lists causal parity as one of the five tenets of the Developmentalist Challenge), I think that fleshing out these two positive theses is crucial to understanding the philosophical project of DST.

Causal parity denies that some causal factors (such as genes) *possess*, in principle, “special directive, formative, or informative power” (Oyama 2001, p. 178). Explanatory symmetry denies the *attribution* of a privileged role in explanations to some causal factors (such as genes)—in short, “the treating of some causes as

more equal than others” (Oyama 2001, p. 178). Because commitment to an image of causation and to a unit (or units) of explanation comes in the same theoretical package, DST insists that both causal parity and explanatory symmetry are at work when analyzing developmental phenomena. These theses demand, respectively, a non-foundational image of causation in which “[c]ausation is multiply contingent, and [causal] influences both select each other and determine each other’s effects” (Oyama 2000a, p. 24), and the explanation of a system’s behavior in terms of no fundamental unit.

It is important to note that the causal parity thesis “does not claim that all these sources of causal influence play the same role, nor that all are equally important (whatever that might mean)” (Oyama, Gray, and Griffiths 2001, p. 2). Also, causal parity does not aim to discard the possibility of distinguishing between different types of causes: “causal symmetry is neither a platitude about multiple influences nor a denial of useful distinctions, but a powerful way of exposing hidden assumptions and opening up traditional formulations to fruitful change” (Oyama 2000c). The thesis, then, is not that we cannot assign causal importance to various parts of systems, but rather, that in assigning causal importance we should not forget that assignments are often heuristic and may or may not reflect the nature of the relevant causes. Revising and justifying these assignments forces us to re-frame our causal notions in a way that brings the system back into view.

When analyzing biological systems, theoretical perspectives decompose, individuate, and group, and “different groupings of developmental [causal] factors are valuable when addressing different questions” (Oyama, Gray, and Griffiths 2001, p. 2) posed by different perspectives. Moreover, “things that are “the same” in one analysis won’t always be so in another, or at another time” (Oyama 2000c, p. S342). In Chapter 1, I said that I understand the practice of system-decomposition as the practice of identifying units of explanation. But identifying units of explanation in turn requires drawing lines around groups of causes.

These two activities—drawing lines around groups of causes and identifying units of explanation—are, respectively, where the parity thesis and the explanatory symmetry thesis of developmental systems operate.

Chapter 3

Epistemology and Metaphysics in Developmental Systems Theory

3.1 Epistemological criticism of DST

The idea that biological development is a constructive process involving interaction of many different causal factors echoes Levins and Lewontin's (1985) call for a "dialectical biology." Dialectical biology stresses the importance of a symmetrical view of causation by noticing that the organism is itself a *cause* of its own development, in other words, that an organism alters the world (e.g., by determining which causal factors are relevant) through its own life activities at the same time it is being altered by the world. For dialectical biology and developmental systems advocates, Genetic Determinism—with its foundational image of causation—is blind to this systemic property. While authors like Oyama, Levins, and Lewontin believe that this framework, as well as the image of causation it endorses, ought to be reconceptualized, in Kitcher's judgment no such reconceptualization is needed: "Genetic Determinism persists not because of some subtle error in conventional ideas about the general character of biological causation but because biologists who are studying complicated traits in complex organisms are prone to misapply correct general views" (Kitcher 2001, p. 397). In this section I present Kitcher's critique of DST and his defense of Genetic Determinism, particularly through his treatment of the concept of norm of reaction. I also show that the consequences of endorsing a foundational image of causation are nothing but *subtle*.

3.1.1 Kitcher's defense of Genetic Determinism

According to Kitcher, those who endorse symmetry theses believe that "an organism's environment should not be thought of

as identifiable prior to the organism and its distinctive forms of behavior,” which is a methodological claim (Kitcher 2001, p. 400). They also believe, he says, that “the singling out of genes as causal factors is an unwarranted abstraction from a complex causal situation wrongly giving priority to some determinants of the phenotype,” which is an epistemological claim (Kitcher 2001, p. 400). Kitcher believes that DST advocates confuse epistemological and methodological claims when they maintain that an organism alters itself and the world through its own life activities, at the same time as it is being altered by the world, and that an organism is itself a cause of its own development. Kitcher translates these ideas into a principle of causal dependence that states the following: *C* cannot be a causal factor in the production of *P* if *C* is dependent on *P* (Kitcher 2001, p. 400; see also Oyama 2001, p. 182).

If Lewontin and Oyama understand causal dependency in this way, then for them—Kitcher concludes—“the idea of a norm of reaction, with its partitioning of causal variables along different axes, is confused” or nonsensical (Kitcher 2001, 401). Kitcher believes, *contra* Oyama, that there is a non-problematic way of speaking about Genetic Determinism (i.e., there is a way in which asymmetrical explanations are non-problematic and useful to biology). For Kitcher, claims that “genetic causes take priority” or possess a privileged explanatory status are “of little use for reconstructing the debates about Genetic Determinism” (Kitcher 2001, p. 397). For him, understanding Genetic Determinism as a claim about the shape of a norm of reaction is coherent, and it illustrates his point about the lack of reconceptualization needed for speaking soundly about Genetic Determinism.

Kitcher’s criticism of DST’s understanding of Genetic Determinism rests upon his conviction that DST is trying to give an account of explanation. As I said in Chapter 2, (especially section 2.3), this is mistaken. The project of DST is the analysis of the underlying theoretical commitments of Genetic Determinism, particularly its commitment to a foundational image of causation and the privileging of a single explanatory unit. Kitcher refuses

to engage in the “subtleties” of images of causation and argues, instead, that any errors in Genetic Determinism are methodological. His defense of Genetic Determinism rests upon the conviction that it is possible to apply the methods correctly and explain an organism’s form in terms of near-deterministic genetic mechanisms. I say ‘near-deterministic’ because it would be incorrect to suppose that Kitcher endorses the same kind of explanatory fundamentalism than genetic determinists do. Kitcher is not willing to accept that there can be *one* fundamental unit of explanation that holds across all levels of (biological) organization. However, he does rely on genetic mechanisms as privileged units of explanation to support a unificationist account of explanation. That his unificationist project is incompatible with the philosophical project of DST is true, but we must be careful not to confuse the aims of the two projects.

3.1.2 Enter the norm of reaction

A *norm of reaction* can be understood as “the reaction of genotypes in development to the environment” (Lewontin et al., 2001, p. 28). Reaction norms can be represented graphically as a curve on which the phenotype is plotted as a function of the environment for two or more genotypes. For Kitcher, Genetic Determinism (in its crudest form) is a claim of a flat norm of reaction, one in which the phenotype is completely stable across all the environments with respect to which it is plotted (see also Oyama 2001, p. 180) (See Figure 3.1). Kitcher maintains that this sort of graph is hardly ever obtained (this is a reflection of the near-deterministic character of genetic mechanisms) and, moreover, he says that because genetically determined traits are “causally close to the immediate biochemistry in which DNA is involved—they will not be the characteristics for which we wonder about the rival contributions of nature and nurture” (Kitcher 2001, p. 397).

Kitcher suggests that we focus instead on the fact that there can be slight variations in the graphical representation of a norm

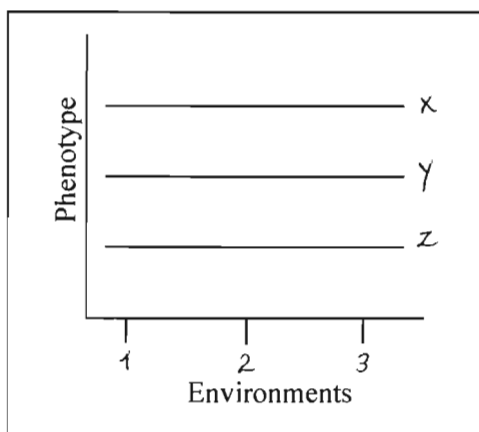


Figure 3.1. Genotypes (x , y , and z) manifest the same phenotypes under three changes in environment. Environment does therefore not influence the phenotype. If the environment were an influence, we would see non-flat genotype lines, with angles relative to the environmental axis indicating degree. Flat norms of reaction denote Genetic Determinism.

of reaction. In other words, he believes that understanding Genetic Determinism as a matter of degree (with the help of norms of reaction) is more helpful in reconstructing the nature/nurture debate than opposing determinist theses—something an “underdeveloped transinteractionist biology,” like DST, is prone to do (Kitcher 2001, p. 399). In order to make his point about the usefulness of understanding Genetic Determinism in terms of norms of reaction, Kitcher requires characterizing “environment” in a way in which it can be mapped on an axis to assess genotype-environment interactions.

Kitcher’s strategy to do so is by combating Lewontin’s claim that no matter how we characterize environment, it is not possible to predict a phenotype from the analysis of genotype-environment interactions. Not even exhaustive knowledge of the genes of a developing organism and its environment would allow prediction of the phenotype (see Lewontin 1991, p. 26; also Kitcher 2001, p. 402). In support of this claim, Lewontin “notes that fruit flies typically have different numbers of bristles at the

left and right sides of their thorax,” and “that the difference cannot be explained by a difference in genotype and is not traceable to differences in the environment” (Kitcher 2001, p. 402).

Kitcher thinks that the important question to ask whenever claims, like Lewontin’s, about the implausibility of speaking of genetically determined features arise is, “what is being counted as part of the environment?” (Kitcher 2001, p. 402). Generally speaking, an organism’s environment (non-genetic causal factors) can be divided up into factors that are causally relevant in the determination of a phenotype, factors that are not causally relevant in this way, and factors whose causal relevance we do not know. Kitcher admits that, because we have only a rudimentary ability to identify the “functional environment” (i.e., factors that are causally relevant in the determination of a phenotype), to draw a norm of reaction should not be taken to indicate that we fully understand “how to order environments along the axis” (Kitcher 2001, p. 401). While we should acknowledge that when using norms of reaction “our ignorance affects the pictures and the conclusions drawn from them” (Kitcher 2001, pp. 401, 402, also note 3), Kitcher is convinced that the concept of norm of reaction is “perfectly well defined,” and non-problematic if applied correctly (Kitcher 2001, p. 402).

3.2 Epistemology of constraints and epistemology of possibilities

According to Kitcher, then, the concept of norm of reaction is coherent and useful insofar as there is an in-principle possibility of classifying *all* environmental factors into causally relevant and causally irrelevant ones, that is, as long as we can eliminate the “factors whose causal relevance we do not know” category (one that can neither be mapped on an axis nor excluded).¹⁷ Moreover, because he believes there is an in-principle possibility of identi-

¹⁷ Kitcher adopts the in-principle possibility of classifying the *total* environment as a theoretical pre-assumption, but he does not—to my knowledge—provide a strategy for the full identification of such factors.

fyng *all* the “bits and pieces” of the environment that are causally relevant to the developing organism—that is, of *totally* identifying its *functional* environment—Kitcher thinks that we can do without Lewontin’s notion of developmental noise (that is, the random source of variation which brings about differences that cannot be accounted for either by genetic or by environmental differences). To make his point clear, Kitcher states that “the form of the phenotype can be viewed as fixed by the genotype and the environment provided that we conceive of the environment in the proper (total) fashion. There is no need to invoke developmental noise or to think that the notion of a norm of reaction breaks down here” (Kitcher 2001, p. 403).¹⁸

Kitcher’s reaction toward Lewontin is indicative of what Falk calls an “epistemology of *constraints*,” a line of thinking that regards the phenotype to be highly dependent on the genotype, even if it responds to “varying-though-predictable” environmental circumstances (Falk 2001, p. 134). Assuming that, in a genotype-environment setting, the environment constrains the extent to which the phenotype depends on a given genotype, builds upon a form of causal disparity: the assumption that we can readily identify genetic and non-genetic causes, and that—insofar as non-genetic causes are classified as part of “the environment”—they become secondary causes that merely constrain the phenotype’s dependence on the genotype. Interpretation of a norm of reaction in terms of an epistemology of constraints also entails explanatory asymmetry: depending on the shape of the curve we obtain, one type of causal factor is assigned a privileged explanatory status.

Lewontin and Oyama are skeptical about thinking of constraints as restrictions on the production of phenotypic variability. For them (especially Oyama), speaking of genetic causes and environmental constraints, where the former are given predominant explanatory roles, supports the sort of dichotomies that sustain Genetic Determinism (see Oyama 1993). For this reason, subscribing to an epistemology of constraints does not allow us

¹⁸ For a defense of Lewontin’s position over Kitcher’s, see Pigliucci (2001).

to step outside the problem of Genetic Determinism; it prevents us from understanding it and hinders progress towards a proper comprehension of developmental processes. Kitcher is correct in saying that Lewontin's and Oyama's basic idea that the functional environment "depends on (in the sense of varying with) the properties of the developing organism" is compatible with attempts to draw norms of reactions (Kitcher 2001, p. 401). However, he is mistaken about their rejection of the concept of norm of reaction, for Lewontin "points out the open-ended non-predictability, the epistemology of *possibilities*, that is provided by the NOR [norm of reaction] notion" (Falk 2001, p. 134). Oyama (2000) also celebrates an epistemology of possibilities by stating that "One salutary consequence of dropping our inference to, and explanation by, information in genes or in environments is that developmental continuity, stability, and uniformity, as well as discontinuity, lability, and variability, can be given the theoretical and methodological scrutiny they deserve rather than being imputed willy-nilly on the basis of nature-nurture assumptions" (p. 173).

In endorsing an epistemology of constraints, Kitcher reifies the gene/environment dichotomy through an idealized characterization of environment, according to which the obtainment of the 'same' outcome depends on whether we can identify the 'same' initial conditions. He employs near-deterministic genetic mechanisms in the service of the idea that the object of good science is to collect and classify patterns (regularities) that permit us to unify phenomena. It is not clear, however, how he can articulate genetic mechanisms as parts within a unificationist account of explanation, that is, how such mechanistic explanations "form part of a systemic picture of the order of nature" (Kitcher 1989, p. 430). Kitcher's invocation of norms of reaction is a strategy for defending a single view of explanation to the exclusion of others. Kitcher is not just being stubborn: his defense of a deflationary version of Genetic Determinism, in the face of DST's arguments in favor of explanatory openness, is in the service of a view of unity. Insofar

as geno-centric explanations work toward simplicity and generality, they fit well with his unificationist account of explanation. From the DST point of view, however, the price Kitcher is willing to pay for unity is too high, because it means reifying dichotomies and failing to examine base theoretical commitments. In Kitcher's case it also means imposing unnecessary limits on the number and kinds of explanations one can offer. While Kitcher argues in favor of a unificationist account that is not incompatible with theoretical pluralism, his account *does* have a flavor of explanatory fundamentalism: by forcing specific situations into "common patterns" it reduces the types of facts we can explain, and it reduces the number of explanatory units we can explain with.

3.2.1 Resisting dichotomies is not rejecting distinctions

Kitcher accuses advocates of DST and dialectical biology of advancing the "blanket charge" that "any kind of separation of causal factors does violence to the causal complexities of development" (pp. 400- 404). For Kitcher, "[c]omplex causal situations do not demand that we perform the impossible feat of considering everything at once; rather they challenge us to find ways of making these factors manageable" (p. 404, also note 4). Advocates of DST and dialectical biology are not in disagreement with this assertion. Also, they do not advance the blanket charge of which Kitcher accuses them. In fact, "[t]he single metric implied by accusations of blanket sameness... is precisely one of the things that DST denies" (Oyama 2000c, p. S342).

Kitcher believes that Oyama and Lewontin differ from him with respect to what they consider to be adequate ways of analyzing the complex processes of development. While he is sympathetic toward interactionism, which regards the identification of norms of reaction for genotypes, or the discovery of genes "for" traits as *one* among many ways of analyzing the complex processes of development, according to him Lewontin and Oyama deny the legitimacy of norms of reactions as representa-

tions of the relative invariance of a phenotypic trait, given a particular genotype across a “manageable range of environments” (Kitcher 2001, p. 406). He says, moreover, that “Lewontin has miscast the important methodological point about the difficulty of settling questions of concern [which for Kitcher are the shapes of norms of reaction] as an incorrect conceptual point about the incoherence of a norm of reaction” (Kitcher 2001, p. 402).

But DS theorists and dialectical biologists do not disagree with Kitcher with respect to *what* “hasty generalizations” are being drawn from *which* analytic methodology, as Kitcher puts it. The main source of disagreement is, again, grounded on Kitcher’s reification of dichotomies: “Though anyone, if pressed, admits that any “genetic” character requires the proper environment to appear, this is quite beside the point, since, as we have seen, the idea of dual processes relies not on genes or environment as sufficient *causes* but rather as alternative *sources* of form” (Oyama 2000a, p. 175, emphasis added). DST’s concerns about norms of reaction are not methodological (or just interpretative), but regard the theoretical assumptions and commitments that Genetic Determinism brings forth when constructing norms of reaction.

For Oyama (2001), “[t]he problem [with norms of reaction and analysis of variance] lies less with the analytic technique per se than with the preanalytic assumptions that guide its use, and thus the meanings that are attached to the results” (p. 181). For Kitcher, conversely, the questions of concern are primarily methodological but can be conquered—as we saw—with correct identification of the total environment; the theoretical pre-assumption that such identification is possible (if only in principle) is correct. From a developmental systems perspective, identification of total environment is both in principle and *de facto* a bad commitment to make (or even an impossible commitment to keep). From the point of view of an epistemology of possibilities, the correct theoretical assumption to make when explaining development is *implausibility* of identifying total environment. DST’s strategy is identification of partial environment, with par-

ticular attention to the context and causal processes of which this environment is part.

I said before that, according to Kitcher, one of the reasons why the concept of norm of reaction is nonsensical for DS theorists is that they endorse causal dependence. Oyama emphasizes that, to her knowledge, none of them have said, “the idea of norm of reaction is itself ill formed” (Oyama 2001, p. 180). Oyama also stresses that the form of causal interdependence that DST supports has nothing to do with Kitcher’s characterization of causal dependence, an account that is ultimately reducible to a foundational image of causation.

Furthermore, Oyama points out that “[t]o reject a special executive role for the DNA is not to deny that all sorts of distinctions can be made among factors and among the ways they impinge on development,” something that—as Kitcher himself notes—is not incompatible with the concept of norm of reaction (Oyama 2001, p. 178). DST’s claim is not that “genetic effects on organisms cannot be identified, but that the genes have their effects by being affected by other factors...and these often include the very processes they influence” (Oyama 2001, p. 182). The sort of constructivist interactionism that DST promotes is not opposed in principle to the possibility of developmental stability or regularity (Oyama 2001, p. 179). It is, however, opposed to the standardized, deterministic, way of explaining such regularity: “[d]evelopmental biologists have tended to be impressed by regularity, but [as an epistemology of possibilities suggests] there is more than one way to explain it” (Oyama 2001, p. 187).

For DST, the shape or degree of flatness of a norm of reaction is not the best way of accounting for phenotypic regularity because it confuses issues of regularity of *outcomes* with the nature of the causal *processes* producing those outcomes (see Oyama 2001, p. 179). When interpreting a norm of reaction one should ask: “Under what conception of developmental processes are genes more important in traits that emerge reliably in many environments than in those that vary across them?” (Oyama 2001, p. 180). As it turns out, having reliable patterns of outcomes is

coupled with Genetic Determinism when a foundational image of causation guides analysis of developmental causes. Such an image confounds the analysis of interacting causes with the task of delineating alternative causes (Falk 2001, pp. 131, 132).

3.2.2 Downside of a “top-down” approach to explanation

There is an underlying philosophical discussion regarding the nature of causal explanation that lurks behind Kitcher and Oyama and Lewontin’s discussion. The way Kitcher frames the problem of explanation commits him to the view that at least in an important sense all explanation is deductive (as Kitcher (1989) acknowledges).¹⁹ According to him, to explain is to derive descriptions of phenomena from general argument patterns, and the fewer the better. To explain is to accommodate a great variety of phenomena and events within a coherent general framework, that is, to unify apparently dissimilar phenomena according to a restricted number of (deductive) explanatory patterns (Kitcher 1993, Chapter 5).

Kitcher characterizes the “explanatory store” as the set of argument patterns that maximally unifies *K*, the set of beliefs accepted at a particular time in science. Whether a given derivation is a good or bad explanation is a matter of showing that it belongs (or not) to *K*. This unifying deductive structure is the main normative structure characterizing scientific knowledge. Kitcher views explanation (which is, according to him, part of the project of unification) as prior to causation. Whatever claim we can make about causal relations in the world is, for him, derived from epistemology. What is considered a correct causal claim will depend on the way the claim fits with considerations as to what is the current way of understanding the unification of nature. There is no causal order “out there” to which our models and

¹⁹ To reinforce this claim, Salmon (1989, p. 182) describes Kitcher’s unificationist project as “the form in which the epistemic conception of scientific explanation [developed by Hempel] can flourish today.”

explanations refer beyond the causal order that is distilled from our unification attempts, which in turn are characterized in terms of epistemological constraints on deductive systematization. This epistemological stand requires, in particular, that whatever we make of “context” or “history,” it has to be captured in terms of constraints on deductive patterns of explanation. To that extent, Kitcher’s view is an example of the sort of epistemology that (as I mentioned before) Falk calls “epistemology of constraints.”

When Lewontin and Oyama defend an epistemology of possibilities, more than rejecting the existence of constraints on our explanatory patterns, they should be interpreted as saying that constraints do not have to be understood as constraints on *deductive* patterns of explanation. I take DS advocates to suggest that an image of causation can constrain what we consider to be acceptable explanatory configurations, that is, they can constrain what we consider to be units of explanation (see sections 2.2 and 2.3). This, however, does not mean—contrary to what Kitcher claims—that explanation precedes causation.

Kitcher commits to a “top-down” approach to explanation, which is deductive in the sense that events are explained by fitting them into a larger explanatory pattern. In his view, causation derives from explanation, that is, epistemic relations are prior to causal relations. For Kitcher, if we were to commit to a thesis of explanatory symmetry, this could not be understood at some intermediate level of explanation; the symmetry should be understood as flattening out all causal differences. This, contrary to what Kitcher thinks, is not something advocates of DST claim: “Notice that [causal] parity is not a matter of pronouncing all factors to be “equal” or “the same” in the sense that they cannot be distinguished” (Oyama 2001, p. 183). What is important is to show that Kitcher is committed to the view that a thesis like the symmetry thesis of DST has to be understood in terms of a general pattern of explanation, and thus (according to him) in terms of a unificationist project. To that extent the symmetry thesis can only be understood as a thesis in principle. For a unificationist, commitment to symmetry (or asymmetry) can only come from

the “top.” For DS theorists, if a causal image is adopted that entails a symmetry thesis, this symmetry can be an expression of a dependence on context, or history, and to that extent it should be understood, as Oyama points out, as an unprincipled claim about symmetries (unprincipled here means “in no predetermined way”).

From a developmental systems perspective, a single causal factor “might under certain circumstances be “determinative” in the sense of being a good predictor across some range of cases” (Oyama 2001, p. 181). However, as Oyama points out, a good deal of mischief comes from confusing this sense of prediction with foundationalist claims: issues of regularity of outcomes and causal processes “come together when *genetically determined* is used for organisms or characteristics rather than for patterns of outcomes” (Oyama 2001, p. 181). Oyama is concerned that collapsing these issues leads to genetic essentialism, which is one reason why she considers “the widespread usage of *genetically determined* to indicate a phenotype with a flat norm of reaction [even if it represents one end of a continuum that is hardly ever obtained] to be so terribly unfortunate” (Oyama 2001, p. 181). This concern is not addressed by Kitcher’s critique of DST. Because of his commitment to a “top-down” account of explanation, Kitcher fails to engage in the discussion about what is the appropriate image of causation and what is the appropriate unit (or units) of explanation for development.

3.3 Causal image and explanation

A causal image can be thought of as the implicit metaphysical notion of causality that leads to or constrains what a theoretical perspective considers to be acceptable models of causal configurations. Like Levins and Lewontin (1985), Oyama, Gray, and Griffiths (2001) “think that the fundamental problem [behind explanation of development] lies in the way causation is viewed in biological systems” (p. 6). For them, attention to constructivist interaction and interdependence counteracts one-way causal sto-

ries. As a theoretical perspective, DST demands the substitution of a foundational image of causation with a systemic or relational image, not without reminding us that “although we may use our methods of randomization and control to “isolate” the effects of a factor, we do so precisely because our factors are abstracted from a dense causal complex” (Oyama 2001, p. 182).

In this section I present Lewontin, Rose, and Kamin’s account of a systemic image of causation, which inspires DST. I also explore the epistemological implications of this image of causation, as well as its relation with the search for laws in Goodwin’s structuralist perspective. Finally, I point to DST’s recognition of the explanatory tension that is generated by what I call Psillos’ metaphysical problem.

3.3.1 Relational causation revisited

A systemic or relational image of causality (characterized first by Lewontin, Rose, and Kamin in 1984) is one in which the actions and effects of causal participants are interdependent, and outcomes are attributed to a distributed set of causal participants instead of being attributed to actors neatly separated from that on which they operate (Oyama 2001, p. 178). Oyama finds such an image suitable as a resource for explaining development, but she does not reject the separation of causes or the identification of causal influences. As I said in section 3.2, to resist dichotomies is not to resist distinctions, but to resist the grounding of distinctions on a dichotomous framework. What Oyama stresses is the implausibility of separating causes *with respect to distinct boundaries*; for her, “[c]ausation is multiply contingent, and influences both select each other and determine each other’s effects,” (Oyama 2000a, p. 24). Within a relational image of causation, causal factors *construct* the constancy of form that development exhibits, and they do so through an interactive ontology (I have previously referred to this idea as ‘constructivist interactionism,’ see sub-sections 2.1.2 and 3.2.1).

As Cor van der Weele (1999) notes, however, an image of causation that emphasizes relations in causal thinking is not exclusive of constructivist approaches (such as DST). Goodwin's structuralism also focuses on relations.

Like Goodwin, constructionists emphasize that causation should be conceptualized in a relational way, in order to represent the processes, as opposed to the entities, involved in development. As Gray expresses it: "If information and causation are our focus then it is relationships not entities that count" (Gray 1992, p. 194), while Gilbert Gottlieb says: "The cause of development—what makes development happen—is the relationship of the two components, not the components themselves" (Gottlieb 1992, pp. 161, 162). This should direct attention to process-aspects of development instead of static causes (van der Weele 1999, p. 65).

DST demands a relational image of causation and emphasizes causal processes as units of explanation. Goodwin also stresses the importance of related processes, but there are interesting differences between the ways that Goodwin's structuralism and DST's constructionism consider that a relational image of causation informs explanation.

Goodwin's structuralism is a theoretical perspective that shares with DST the resistance to causal asymmetry and, like DST, endorses a relational image of causation. While Goodwin's structuralism is also, in a sense, a process-perspective, what Goodwin understands as 'process' sustains a very different pattern of explanation than the one DST commends. The major task of process structuralism is to seek organizing principles, to find laws that describe generic outcomes of developmental processes (see Griesemer 2004). For Goodwin, understanding developmental processes as functional causal relations that can be described in mathematical terms is the best way to accomplish this explanatory ideal. In other words, Goodwin's notion of causal process sustains a "law-based" pattern of explanation.

For DS advocates, development is a result of the contingent endeavor of rich, interdependent, causal configurations, not the result of law-abiding causal processes. In van der Weele's words,

In contrast to Goodwin, finding laws is not [DST's] explanatory ideal. In the constructionist perspective, interactive contingency reigns the world. "Relation," for [DS theorists], is more or less synonymous with "interaction." The causal network involving genes and many other factors, including a particular environment, is what [DST] authors have in mind when they emphasize that developmental causation is relational (van der Weele 1999, p. 65).

While there may be many difficulties with Goodwin's conviction that he can arrive at developmental laws, my purpose here is not question his explanatory ideal. The aspect that most interests me about Goodwin's position is that it exemplifies the explanatory openness derived from endorsing a relational image of causation. According to my two-element characterization of theoretical perspective, one can choose explanatory units flexibly and this choice does not depend on (or derive from) the choice of an image of causation. Relational images do not favor some explanatory patterns and oppose others; Goodwin's process structuralism illustrates this aspect of my characterization.

Summing up, causal images, together with units of explanation, work both as resources and constraints for devising particular causal explanations in biology. An important aspect of a causal image is that commitment to it does not entail commitment to a specific unit (or units) of explanation.

3.3.2 *Explaining sin techo, sin piso*

Although the primary concerns of DS theorists are not metaphysical, I take DST to have a genuine concern for the conceptualization of a relational image of causation. For Oyama, to say that "the developmentally relevant environment depends on the organism" means that "whether, and how any aspect of the surround is involved in producing an organism is a function of that

organism's characteristics and its activity" (Oyama 2001, p. 189). It does not mean, as Kitcher claims, that according to DST causes cannot be separated or causal influences cannot be identified because "any kind of separation of causal factors does violence to the causal complexities of development" (Kitcher 2001, pp. 400- 404).

A difference between DST's image of causation and Kitcher's interpretation of DST's image of causation is that DST aims to include context "not only as container but as constitutive of developmental processes" (Oyama, pers. comm.). However, it is not only top-down approaches to explanation, like Kitcher's, that conflict with understanding DST's project. A bottom-up approach like Salmon's (1989, 1998) causal-mechanical model of explanation is also problematic for DST. According to Salmon, the causal structure of the world is implicitly displayed at the "bottom," and the "top" constitutes only the explicit form of that order. Moreover, a unificationist explanatory scheme like Kitcher's (1993), as well as Salmon's causal-mechanical model, ignore that the diversity of disciplines in biology demands a diversity of patterns of explanation. For example, population genetics formulates explanations that are very different from the ones formulated by evolutionary developmental biology.

While causal-mechanical explanation proceeds, apparently, in the opposite direction to unificationist explanation, both share unification as an ultimate goal of scientific explanation as well as a foundational style: Salmon's "bottom" is as enduring as Kitcher's "top." Even if we choose the "bottom-up" approach, Salmon could say, the goal of unification can promote our understanding of the world. As Psillos (2002) notes, the important issue at stake here concerns the role of causation in explanation. "[i]n particular, it concerns the important metaphysical question: what comes first, explanation or causation?" (Psillos 2002, p. 283).

I take DST to maintain that biological explanations have no "top" or "bottom." What is at the "top" or at the "bottom" is defined contextually, depending on the phenomenon to be ex-

plained (i.e., at what level of organization it occurs, in terms of what level of organization—or unit of explanation—it is to be explained), and depending on the theoretical perspective from which we approximate the phenomenon. One could ask if, from a developmental systems perspective, a pattern of explanation that can properly explain biological developmental requires solving Psillos' metaphysical problem.

If the way of solving the metaphysical problem requires appealing to a relation of dependence between causality and explanation, that is, defining once and for all what is at the “top” and what is at the “bottom,” as well as the direction in which explanation occurs (like Kitcher and Salmon do), then for DST, solving this problem is a useless enterprise. Solving the problem cannot inform a perspective that considers processes as units of explanation or one that aims at integrating various units. This does not mean that Psillos' metaphysical problem is irrelevant to biological explanation of development. Recognizing the existence of the problem and acknowledging the explanatory tension that it entails is, according to my interpretation, the philosophical challenge that DST exposes. To invoke Oyama's own metaphorical language, to explain development *sin muros*, that is, a phenomenon without predefined causal boundaries, we require explanation *sin techo*, *sin piso*, that is, an account of explanation without predetermined limits and without foundations.

Chapter 4

Conclusion: Achievements and Unsettled Issues

4.1 Summary

Throughout this manuscript I have relied on a specific characterization of the notion of a theoretical perspective for my analysis of the philosophical project of DST. This characterization, which identifies causal images and units of explanation as constitutive elements, has been useful for:

- a. understanding DST's rejection of dichotomies, as well as its criticism of Genetic Determinism
- b. showing that Godfrey-Smith's dual characterization of DST is misleading
- c. identifying the types of theoretical commitments that are at stake in the activity of explaining developmental phenomena
- d. defending the explanatory openness of the DST project, and in this way defending DST from Kitcher's criticism
- e. recognizing the explanatory tension that arises from the unavoidable relation between ontological and epistemological aspects of explanation

In order to combat Genetic Determinism, DST provides an alternative package of theoretical commitments that favors a relational image of causation over a foundational one. It is clear that DST emphasizes processes as units of explanation. However, it is not clear how its focus on this unit escapes the inclination to use processes as (fundamental) explanatory units that hold across all levels of biological organization. If my analysis of DST is correct, saying that in order to understand development "we must look at its processes, not only at its constituents, and be ready to meet them on as many levels as their

complexity justifies” (Oyama 2000a, p. 27), is not to reconceptualize explanatory fundamentalism in terms of processes. The problem lies, rather, in the difficulty of saying what a process is, from a DST point of view. In sub-section 2.3.1 I pointed to two gene-concepts that aim at incorporating genetic mechanisms into broader developmental processes. However, it should be clear that processes need not take the place of genetic mechanisms as units of explanation. One of the most significant contributions of process-perspectives, for philosophy of biology as well as for philosophy of science in general, is that they illustrate how endorsing a relational image of causation allows theoretical perspectives to integrate two or more explanatory units. If DST’s focus on processes has not adequately illustrated this, a brief look at another process-perspective that more explicitly integrates mechanisms and capacities, as well as processes, can help to shed light on the philosophical project and contribution of DST.

4.2 Mechanisms, capacities, and processes as units of explanation

For Griesemer (2004), “A process perspective in science takes *processes* to be the primary phenomena to be described (observed, represented, explained, predicted, understood, experimented upon, measured, conserved, destroyed, engineered)” (p. 361). The Reproducer Perspective (RP) is a process perspective that aims at understanding development, inheritance, and evolution, by way of analyzing the processes that take place throughout “an evolutionary hierarchy of levels of productive organization” (Griesemer 2004, p. 359). RP differs from DST in several respects. For example, the type of explanatory parity or symmetry that it pursues has to do with the processes of development and inheritance, rather than with developmental resources: “RP treats heredity and development as equally relevant and entwined parts of a complex process of reproduction” (p. 366). Evolutionary processes also take part in this integration insofar as devel-

opment is understood, from an evolutionary point of view, as the acquisition of a capacity to reproduce. Moreover, a commitment to a hierarchy of levels of organization and a commitment to a dynamic image of causation allow the integration of what I call explanatory units. For example, factors that play a causal role in mechanisms of development acquire the capacities they display via the process of development (which is the acquisition of a capacity to reproduce).

In this manuscript I have presented DST as a perspective that can replace Genetic Determinism, and its share of theoretical commitments, as an adequate perspective on development. While I do believe that there are better and worse ways of approaching developmental phenomena, a point that Griesemer makes and that should not be underestimated, is that we must not turn to process perspectives for competitive replacement of other perspectives (whether these focus on processes or not). Rather, we must view the multiplicity of perspectives as a means to urge “trans-perspective cooperation in the service of comparative analysis” (Griesemer 2004, p. 365).

4.3 Further research

I have argued here that DST offers good reasons to resist certain wide-spread dichotomies that are encouraged by the genocentric view of development. Also, I have presented the DST process-perspective as one possible alternative to this view. Several issues raised by the discussion here demand further clarification but cannot be treated in a project of this scope. For example, my use of processes as units of explanation raises several issues about what processes are, how they differ from mechanisms, and how they are identified and bounded. There is also a set of related issues surrounding the metaphysics of causation. These include a more robust characterization of foundational and relational causation, and a clearer account of the contingency claims of process-perspectives.

Traditionally, the idea that contingent aspects of the world can play a role in scientific explanations has been considered nonsense (e.g., Hempel and Oppenheim's 1948 deductive-nomological account of explanation). However, recent approaches to explanation in the philosophy of science (e.g., Beatty 1995; Cartwright 1989, 1999; Woodward 2003; Mitchell 2003) advance interesting claims in favor of accounts of explanation that involve contingency in a crucial way. Because DST's notion of development is one in which "contingency is central and constitutive, not merely secondary alteration of more fundamental, "preprogrammed" forms," (Oyama 2000b, p. 116), DST may provide important insights on how to constructively develop an image of causation that moves away from explanatory asymmetry—something that current, non-dichotomous accounts of explanation also seem to be doing.

Framing my characterization of DST's philosophical project within the theme of causal explanation has opened several lines for future investigation. Important among these is a more complete exploration of non-dichotomous accounts of causal explanation in the philosophical literature. For example, Woodward's (2003) manipulability conception of causation, on which causal explanations are understood to answer continua of invariance for interventions on otherwise regular causal relations. In this vein, it would also be valuable to explore the relevance of Nancy Cartwright's anti-fundamentalism and patchwork of laws theses (Cartwright 1999). Cartwright's work has affinities with the DST approach in that she insists that our best science points to heterogeneity of various systems, rather than simple or unified relations between them. But perhaps more pressing than these is the development of a taxonomy of "part-based" explanations, given that "part-based" accounts in terms of mechanisms, capacities, and processes have all become common in the philosophical literature.

Finally, DST exposes the challenge of acknowledging the explanatory tension that arises from what I call Psillos' "metaphysical problem". Here I hoped to include this problem in

the general discussion, rather than solve it. The solution, from a DST point of view, is that there just is no universal answer, but that each process will have its own peculiar features that will have to be examined in context in order to find out how its causal story relates to explanation. In other words, according to a process-perspective there is only temporary relief for the onto-epistemological tension—another dichotomy is resisted.

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