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"No General Structure"

No General Structure

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Abstract

This chapter introduces a distinctive approach for scientific metaphysics. Instead of drawing metaphysical conclusions by interpreting the most basic theories of science, this approach draws metaphysical conclusions by analyzing how multifaceted practices of science work. Broadening attention opens the door to drawing metaphysical conclusions from a wide range of sciences. This chapter analyzes conceptual practice in genetics to argue that the reality investigated by biologists lacks an overall structure. It expands this conclusion to motivate the *no general structure thesis*, which states that the world lacks a general, overall structure that spans scales. It concludes that the no general structure thesis counts as metaphysics because it says something very important and general about the world. This thesis informs science as well as philosophy of science, and it provides a useful perspective for societies that look upon science to help solve complex problems in our changing world.

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Introduction

Scientific metaphysics can be described as an attempt to theorize about the fundamental nature of the world by drawing upon the best scientific theories of our day.[1] The fact that the best scientific theories of the past turned out to be wrong about the fundamental nature of the world poses a challenge. Why, skeptics ask, should we expect today's theories to provide a secure basis to inform us about the world's fundamentals? In this chapter, I propose that instead of drawing metaphysical conclusions by interpreting the most basic theories of science, we draw conclusions by analyzing how multifaceted practices of science work. This opens metaphysical inquiry to areas of science that are not organized around efforts to articulate, develop, or extend the explanatory scope of a core theory.[2] I motivate and illustrate this approach for scientific metaphysics by analyzing what is one of the most successful scientific practices of today: the practice of using genetics to investigate, manipulate, and explain the workings of organisms.

This chapter begins by briefly discussing a traditional conception of scientific metaphysics. Next, it considers philosophical work aimed at identifying the fundamental nature of being in the biological world, such as being an organism, being a gene, or being a process of natural selection or genetic drift. Such work is not generally recognized by metaphysicians as an integral part of scientific metaphysics.[3] Perhaps this work does not seem sufficiently general because it is cast at the wrong scale. But I will argue that if we reconceive generality in terms of what one finds across different scales, then questions about what is true at intermediate scales (such as the scales of macromolecules, cells, organisms, and ecosystems) are not necessarily less general than

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[1] Niall Roe (manuscript) distinguishes between scientific metaphysics and the metaphysics of science. *Scientific metaphysics*, the topic of this chapter, can be thought of as metaphysically inquiry that is informed by science. One might pursue this approach and yet leave open the possibility that this is not the exclusive legitimate approach for metaphysics. The *metaphysics of science* can be thought of as inquiry into metaphysical assumptions, concepts, and principles embedded within science. This inquiry might be pursued using exclusively traditional metaphysical approaches. For example, many metaphysicians assume that scientific reasoning critically relies on reasoning about dispositions. Much research on the metaphysics of dispositions, however, completely ignores contemporary science. Such research could be thought of as falling under the "metaphysics of science" even though it is not scientifically informed. Inquiry into metaphysical concepts embedded within science might also (or alternatively) be pursued by appealing to the findings or methodologies of contemporary science. See Stanford, this volume, for a somewhat different way of distinguishing between the metaphysics of science and scientific metaphysics. I prefer Roe's classification because it acknowledges that some metaphysicians are trying to investigate the metaphysical assumptions, concepts, and principles embedded within science from an analytic perspective.

[2] This approach also opens the door to drawing metaphysical conclusions from areas of science whose practices are organized around core theories that cannot be interpreted as describing metaphysical fundamentals.

[3] Many, or least some, philosophers of biology conducting this work do believe it is an integral part of metaphysics. My point is that outside philosophy of biology this work is not generally recognized as being integral to metaphysics.

questions about what is true at the largest or smallest scales. I call this the *generality across scales thesis*.[4] This sets the stage for developing the central argument of this chapter.

My central argument starts by applying the traditional approach of scientific metaphysics to the science of genetics. But instead of applying this approach to today's genetics, I first apply it to the genetics of the 1930s. I draw upon the best scientific theories of the era to theorize about the fundamentals of heredity, development, and evolution. In doing so, I consider the gene concept of classical genetics. Although genes were central to classical genetics, genes were entities with unknown structure and unknown proximate effect. Neither the what nor the how of their individual contributions were understood. Nevertheless, geneticists could accurately identify causal connections between genotypes and phenotypes in terms of *the difference principle*. This principle held in experimental contexts because geneticists deliberately simplified causal situations by standardizing environmental conditions and removing genetic differences that might affect the phenotypes under study. This manipulation and control is what made the practice of classical genetics possible, and I argue that it has important implications about how we should pursue scientific metaphysics.

The next step in my central argument is to show that although one could have been (and still can be) a realist about the basic theory of classical genetics, attempts to interpret this theory for the purpose of drawing conclusions about the fundamentals of heredity, development, and evolution were doomed to fail. Furthermore, understanding how the basic theory was deployed in the context of experimental practice reveals the futility of trying to draw conclusions about fundamentals from this best theory. But what about the best theory of today's genetics? Might the best theories of contemporary biology offer a more secure basis for identifying the fundamentals of heredity and development?

I examine a central concept of contemporary genetics (the molecular gene concept) and explain how it is employed in practice. Although analyzing this concept in isolation of broader practice might lead one to conclude that it identifies the fundamental unit of heredity and development, examining how the concept is employed in practice reveals that it is not a category of being that "cuts nature at its joints". The problem with using this metaphor is that DNA has too many joints. I will argue that the practice of genetics utilizes a flexible gene concept because there is no overall structure, functional or causal, of the elements of DNA.

I expand my conclusion about genetics to motivate the *no general structure thesis:* the world lacks a general, overall structure that spans scales. It does not have a "*the* causal structure" that Salmon (1984) claims all explanations should fit into, and it lacks the kind of overall "definite and mind independent structure" that Psillos (1999) claims is an essential tenet of realism. This thesis is similar to the metaphysical views advanced by Cartwright (1983 and 1999) and Dupré (1995), though I motivate it differently. I close the chapter by arguing that the *no general structure thesis* counts as significant metaphysics.

[4] The generality across scales thesis can be viewed as a challenge to Ladyman's idea that physics is "the science that holds at the widest range scales" (Ladyman, this volume).

1. The traditional approach of scientific metaphysics.

Scientific metaphysics refers to the idea that metaphysics should be informed by science (Kincaid 2014). This characterization raises two difficult questions: (1) what is metaphysics? and (2) how should metaphysics be scientifically informed? One indication of the difficulty of answering the first question is the emergence of a subfield, meta-metaphysics, which is aimed at addressing questions about the nature of metaphysics as a subject area, including the question, "what is metaphysics?" (Chalmers et al 2009, Tahko 2016). For the purposes of this chapter, I will assume that metaphysics is an area of philosophy that seeks answers to questions about the general nature of reality, the general nature of the things, kinds, processes, and complexities in the world. One of the questions raised by this chapter concerns how we should conceptualize the sense of 'general' when we say that metaphysics seeks to answer questions about the general nature of reality.

The question of how metaphysics should be scientifically informed is also important, and this issue will be addressed in the present chapter. The literature on scientific metaphysics provides a somewhat ambiguous answer to this question. Some writings stress applying the *results* of science to answer metaphysical questions; other writings stress employing the *methods* of science. Philosophers emphasizing the results of science have focused on the most basic theoretical results of physics (e.g. Ladyman et al 2007; Maudlin 2007, Callender 2011, and French 2014). This approach can be motivated by the idea that metaphysics is about the most fundamental features of the world, which is also what the theories of fundamental physics are about. The rough idea is that metaphysical conclusions should be drawn by interpreting the most basic theories of physics in order to determine what these theories say about the fundamentals of reality.

Theoretical results of biological sciences have not played the kind of key role that theoretical results of physics have played in work explicitly recognized as scientific metaphysics. (I explain the qualification 'work explicitly recognized as' in section 2). Scientific metaphysicians who attend to the "special sciences" often assume that physics holds the trump cards for metaphysical questions and do not see a need to closely engage the actual theoretical contents of biological sciences to establish their basic metaphysical claims. For example, Ladyman et al (2007) and Ladyman (this volume) argue that the core project of metaphysics is to unify the theoretical findings of special sciences with the fundamental theories of physics. Their casual engagement with the contents of biological theories is markedly different than their close and serious engagement with theories of physics, and biology plays a different kind of role than physics in their argument. It's as if physics comes first, and biology had better agree. French (2014) gives more serious attention to the actual contents of biology, but the core thesis (ontological structural realism) is established on the basis of analyzing physics (beginning in chapter 1). Biology is not addressed until the last chapter of the monograph (chapter 12).

Writings about scientific metaphysics that emphasize the methods of science (rather than the results of science) often focus attention at the meta-level. For example, Maddy (2007) addresses questions about what methods metaphysics should employ, rather than directly employing those methods in metaphysical inquiry.[5] In contrast, the new experimental school of philosophy seeks to use scientific methods, usually from social psychology or cognitive science, to generate results that will directly bear on problems that are widely considered to be part of contemporary metaphysics. Philosophers have, for example, employed experimental methods to address the problems of personal identity (e.g. Bruno and Nichols 2010), time (e.g. Paul 2010), causation (e.g. Hitchcock 2012), and ontology (e.g. Smith 1999). The distinction between scientific metaphysics that emphasizes applying the results of science and scientific metaphysics that emphasizes applying the methods of science is not a sharp one. Some scientific metaphysicians attempt to both engage the results of science and apply the methods of science. While I acknowledge the scientific methods approach, for the remainder of this chapter, I use the term *traditional scientific metaphysics* to refer to the variety of metaphysics that attempts to inform metaphysical inquiry by interpreting theoretical results of empirical investigations that have already been conducted by scientists.

Scientific metaphysics, as described above, typically involves interpreting the basic theories of physics to determine what these theories say about the fundamental reality of the world. But, as several authors have warned, this can be problematic since the basic theories of physics analyzed by metaphysicians are themselves grounded in metaphysical presuppositions (Sklar 1981, Hawley 2006, Chakravartty 2014). This raises the possibility that what is indirectly read off the fundamental theories of physics are the metaphysical biases of physicists, not empirically grounded metaphysical truths. Psillos (1999) responds that the key to avoiding this problem is to consider only those parts of scientific theories that are critical to the predictive success of the science. But I prefer another response. We should not conceive of scientific metaphysics as a purely empirical inquiry or as an inquiry that is informed by a purely empirical source. It is inquiry that, like science, involves an interplay or entanglement of non-empirical and empirical reasoning. The basic strategy underlying scientific metaphysics, I suggest, is based on the idea that the world provides constraints on scientific inquiry, and that philosophers can inform metaphysics by investigating these constraints.

2. Questions about biological fundamentals.

Scientific metaphysics has directed attention primarily to questions about the "fundamental" features of the world, those features that are common to everything, always, everywhere. Work explicitly recognized as scientific metaphysics centers on fundamental physics because scientific metaphysicians assume that whatever is true of fundamental physics must be true of everything (and whatever is true of biology is not true of everything). However, there is considerable work in philosophy of biology that can be interpreted as a metaphysical quest to identify fundamentals. The fundamentals sought in these inquiries are the fundamentals of the living world (rather than of the universe at large). This work can be interpreted as scientific metaphysics even though it is not always recognized as such. Consider, for example, the questions:

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[5] The distinction breaks down if one conceives of scientific inquiry as an integral part of metaphysics because under this conception the methods of science are (of course) being directly employed by scientific participants in the project of metaphysics.

What is life?

What is an organism (or a biological individual, or a Darwinian individual)?

What is a species?

What is natural selection? What is drift?

What is fitness?

What is group selection?

What is a population?

What is a gene?

What is genetic/genomic/biological information?

What is a signal?

What is a mechanism?

What is a function?

Philosophers of biology have often analyzed and critiqued scientists' answers to these questions with an eye towards identifying the fundamental reality of life, or the fundamental reality of being an organism, being a gene, or being a process of natural selection or drift. My claim is that many philosophers of biology (but not all) pursue these questions as if they are seeking fundamental answers to questions about ontological categories of being. One indication of this motivation is the frequent appearance of the term 'fundamental' in their writings.

But, as mentioned in the introduction, this philosophical work in biology and philosophy of biology is not generally recognized as an integral part of scientific metaphysics (French 2014 is an exception). There are, perhaps, two reasons for this. One is that answers to these questions do not seem sufficiently general. For one thing, not everything in the universe is living or even a part of something alive. So there is a scope issue. In addition, biology is cast at a particular scale, so there is a scale issue. But if one reconceives generality in terms of what one finds at different scales (some readers might say different "levels"[6]), then features found at intermediate scales (e.g. scales of macromolecules, cells, organisms, and ecosystems) are not necessarily less general than features found at the largest or smallest scales. The theory of quantum mechanics might describe general, structural features at very small scales, but it does not itself describe structural features that exist at larger, intermediate scales. For example, it does not describe structural features of the complexities in ecological dynamics or gene regulation that are being investigated by biologists. Perhaps the kinds of complexities that biologists deal with are quite general (even in non-living parts of the world) and representative of complexities throughout much of nature and across different scales. If so, then *knowledge about the form of complexities in biology would have a kind of generality that the*

[6] I put term 'level' in quotation marks throughout this chapter because this terminology implies that nature is organized into levels. I do not assume that nature has an overall structure that is organized into levels, so I use the term 'scale'.

basic theories of fundamental physics do not. I call this admittedly contentious idea the *generality across scales thesis* and return to it in sections 7 and 8.

Perhaps another reason that metaphysicians do not generally recognize the questions presented above as metaphysical questions is that these questions do not yield neat, univocal answers. What is a species? According to many leading philosophers of biology, species are not any one kind of category or thing. As Ereshefsky (1998) puts it, the "various 'taxa' called species lack a common unifying feature." And things do not parse more neatly at the level of genetics. Philip Kitcher is even more skeptical about genes than Marc Ereshefsky is about species: "a gene is whatever a competent biologist chooses to call a gene" (Kitcher 1992, p. 131).[7] Nevertheless, these questions are often posed by philosophers of biology in fundamental terms, as if what it is to be a species or a gene should come down to a few principled essentials. When it turns out that the diversity of life cannot be neatly partitioned into species or organisms, or when it turns out DNA cannot be neatly partitioned into genes, philosophers become skeptical about the reality of such kinds and search for other kinds that will hold up to their philosophical (i.e. fundamentalist) ideals. Hence the shift in philosophical attention from species to populations, from organisms to Darwinian individuals, and from genes to discrete functional units in DNA. It is as if philosophers of biology are framing questions in ways that call out for metaphysical answers; that is, answers that would provide a basis for drawing conclusions about the fundamentals of evolution, development, and life in general.

It could be argued, by appealing to the reconceptualization of generality suggested above, that much research in philosophy of biology can be understood as scientific metaphysics. Under this interpretation, a central aim of philosophy of biology is to formulate basic questions in ways that answering them will identify the fundamentals of the biological world, for example the fundamentals of what it is to be a unit or process of heredity, development, or evolution.

3. What is a gene? The classical gene concept in scientific practice.

What is a gene? Contemporary geneticists employ multiple gene concepts that seem to offer conflicting answers. At one extreme is what I call the classical gene concept, which provides biologists with a blunt conceptual tool that works well in investigative and explanatory contexts in which precision is not available or useful (Waters 1994 and 2007). At the other extreme is what I call the molecular gene concept, which provides biologists with a remarkably precise and yet flexible tool for contexts in which precision

[7] Beatty (1995) convincingly argues that there is a reason why there are no fundamental laws of biology: biological generalizations are contingent on the historical process of evolution. Beatty's argument could be expanded to challenge the idea that their are fundamental biological entities, fundamental biological kinds, or fundamental biological processes as well. Whatever the explanation, unlike the basic theories in the parts of physics investigated by scientific metaphysicians, the basic theories across the biological sciences do not readily yield to the kind of fundamentalist interpretation associated with metaphysics. Of course, whether current or future theories will eventually yield to such an analysis is a contentious issue in philosophy of biology (e.g. Waters 2011, Okasha 2011).

is important (Waters 2000). The classical gene concept, as the name suggests, comes from classical genetics, the practice of genetics developed by T. H. Morgan and his collaborators in the late 1910s and 1920s. This practice continued until it was retooled to become contemporary genetics (Waters 2008a). In this section, I describe conceptual practice in classical genetics. In the next section, I consider how a traditional scientific metaphysician, if working in the 1930s, might have answered the question "what is a gene?"

It is important to understand that the classical concept of the gene was grounded in experimental practice, not in abstract theorizing. Experimental practice in classical genetics involved strategically constructing breeding regimens that produced distinctive inheritance patterns. These patterns were explained by a central theory, called the transmission theory. This theory was constructed to apply to artificial experimental situations and the experimental situations were constructed to instantiate the theory (Waters 2008b). This may appear to involve a problematic circularity or explanatory triviality. But these practices were aimed, not just at explaining transmission patterns, but at investigating a wide variety of biological processes, including hereditary, physiological, developmental, and evolutionary processes (Kohler 1994). When successful, this experimentation enabled biologists to manipulate processes they didn't understand and to thereby reveal new information about these processes (Waters 2004).

While it is important to understand geneticists' theoretical explanations in the broader context of their experimental practice, one cannot understand this practice without analyzing how the theoretical/explanatory reasoning worked within it. Geneticists' reasoning about genes invoked a conceptual division between the internal genetic makeup of an organism, called its *genotype*, and its outward character, called its *phenotype*. They explained patterns of *phenotypic* transmission produced in their experiments by following the transmission of *genotypic* differences from one generation to the next, and then attributing the presence of alternative phenotypic traits to the presence of alternative genotypes (i.e. to the presence of alternative forms of genes). These explanations were based on the transmission theory, which included the idea that genes are located in linear fashion on chromosomes, principles about the transmission of genes that were grounded in an understanding of chromosomal mechanics, and the principle that differences in genes cause differences in phenotypes.

Let us consider the concept of the gene standing behind these explanations. As units passed down from generation to generation, genes were conceived as stable entities, capable of being replicated, located at designated positions in chromosomes. By at least the 1920s, most geneticists thought of genes as having physical structure, and it was the physical make-up of genes that was presumed to provide their stability. Practically nothing, however, was known about the internal structure of the gene until quite late in the development of classical genetics. The idea that there is a linear ordering of genes in chromosomes was essential to complex explanations of gene transmission, but this ordering does not imply anything about the structure of the genes themselves. Morgan and others frequently stated that their theory made two assumptions about the internal structure of genes: (1) gene structure is relatively stable, and (2) the structure of each gene is replicated before chromosomal division (e.g. Morgan 1926, p. 27). Muller (1922) pointed out that classical explanations positing spontaneous mutation made an additional assumption: (3) mutations in the structure of a gene are also replicated.

Just as Darwin's *Origin of Species* contained a scarcity of information about the nature of species and their origins, Morgan's *Theory of the Gene* had little to say about the structure of genes or their individual contributions to phenotypes. Genes could be speculatively related to the developing form of an organism, but the connection between genotype and phenotype was spelled out concretely only in terms of what I have called *the difference principle*: gene differences cause phenotypic differences within the genetic and environmental contexts of particular populations (Waters 1994 and 2007). Strevens (this volume) describes a similarly simple model of predator/prey population cycles in ecology that also involves difference making. But, he analyzes the ecological model in terms of an abstract explanatory principle ("enion probability analysis") that is ultimately grounded in a metaphysical theory, whereas I analyze models of transmission in terms of a concrete principle grounded in investigative practice. In the case of genetics, at least, it is important to keep in mind that the explanatory principle is easily and very accurately applied in experimental contexts because geneticists deliberately simplified experimental situations by standardizing environmental conditions and removing genetic differences that might interfere with the expression or transmission of genes being used in an experiment.[8]

It is easy to exaggerate the knowledge (or claims) of classical geneticists by focusing on theoretical explanations in an abstract context (rather than in the context of experimental investigation) or by citing geneticists' speculations. For example, classical geneticists have sometimes been accused by later scientists, philosophers and historians as believing in a simple one gene / one character conception, a version of preformationism.[9] This misinterpretation might arise because geneticists' practice involved creating situations in which just one gene is the actual difference maker of a phenotypic difference in a laboratory population (Waters 2007). From a distance, it might appear that geneticists thought their explanations of experimental phenomena represented inheritance generally. But practicing geneticists understood that they were creating simple situations (which was obvious to them because they had to deliberately set up these situations to make their experiments work).

The relation between genotype and phenotype is not "one gene / one character", and classical geneticists knew this. As they clearly stated, one gene can affect a variety of characters and a single character can be influenced by a number of different genes (genes at different loci). Geneticists knew, for example, that eye color in *Drosophila* is

[8] I worry that Strevens' simple model cannot be applied to actual population cycles in an accurate way and that his analysis is too far removed from actual investigative practices in ecology to be useful. Stanford's critique seems to apply (Stanford, this volume).

[9] Examples of such exaggeration are too numerous to cite, but I take Moss (2000) and Keller (2003) accounts of classical genetics as representative of how historians and philosophers exaggerate the claims of classical genetics (claims of which they are very critical). Keller (personal communication) makes the compelling argument that geneticists' speculations reveal biases that had significant impact on their research.

affected by mutations at many different loci; by 1915, Morgan's group had already identified mutations at 25 separate loci that affected eye color (Morgan, Sturtevant, Muller, and Bridges 1915, p. 208). They knew that these mutations generally affect several different characteristics. The white eye allele (located on the X chromosome), for example, was associated not just with white eyes, but with a colorless sheath of the testes, sluggish behavior, and perhaps a shortened life span as well. Experimental practice included protocols for selecting just one such phenotypic difference for experimental purposes. A one gene /one character conception of the gene makes sense only from a decontextualized, abstract perspective.

An examination of Morgan's sophisticated reflections on genotype/phenotype relations reinforces this interpretation. In *Theory of the Gene* (1926), Morgan reported that embryology reveals that every organ of the body is the "culmination of a long series of processes" and he presumed that genes act on the steps along the way. If each step in the development of an organ is affected by many genes, he reasoned, then there couldn't be any single gene for the organ. Likewise, if one gene affects steps in the development of multiple organs, then there couldn't be any single organ associated with a gene. Hence, the many-many relations. Morgan elaborated:

> Suppose, for instance, to take perhaps an extreme case, all the genes are instrumental in producing each organ of the body. This may only mean that they all produce chemical substances essential for the normal course of development. If now one gene is changed so that it produces some substance different from that which it produced before, the end-result may be affected, and if the change affects one organ predominantly it may appear that one gene alone has produced this effect. In a strictly causal sense this is true, but the effect is produced only in conjunction with all the other genes. (Morgan 1926, p. 306)

Classical geneticists could only speculate about the immediate impact of genes (here, Morgan speculated that they produce chemical substances). This passage and Morgan's discussion of developmental processes suggest that the immediate impact of a gene is separated from characteristics such as eye color by a series of developmental processes that are also influenced by a number of other genes. This means that it would be impossible to specify a gene's contribution to phenotype in terms of characteristics such as eye color.

An abstract analysis of the transmission theory might make it appear that classical geneticists had a lot to say about genes, heredity, and development. But an examination of their theoretical knowledge in the broader context of experimental practice reveals that they did not have a lot to say about these matters. The real value of their science involved what they could manipulate and investigate, not what they could explain (and not even what their theory could *in principle* explain, see Waters (2004).

My aim in this section is to make two points. First, the structure of the world that geneticists were manipulating and investigating was not directly reflected in the structure of their concepts and theories. Second, one gets a better sense of the complexity of the reality that geneticists were engaging when one analyzes their investigative and manipulative practices in light of their concrete, local aims (instead of analyzing their theoretical explanations abstractly – as if all we need to consider is their aim to explain inheritance patterns or their alleged aim to explain all development in terms of genes). Scientific metaphysicians interested in complex reality should focus on scientific knowledge (including theoretical knowledge) in the context of scientific practices (broadly speaking), not in an abstract context in which theories can be viewed separately from material practices designed to advance investigative and manipulative goals .

4. Applying the traditional approach of scientific metaphysics to the best biological theories of 1930s.

Morgan was a meticulous empiricist and he tried to avoid letting speculation enter into his scientific writings. But scientific metaphysics is by its nature speculative, and it is fair to ask how hypothetical scientific metaphysicians in the 1930s might have answered the question, "What is a gene?" Could their answer to this question provide a basis for answering questions about the fundamentals of heredity, development and evolution?

Bold scientific metaphysicians of the 1930s (and it is fair to assume that these hypothetical metaphysicians would have been as bold as today's real ones) might have claimed that since presumably every stable inheritance pattern can be attributed to gene differences, that genes must be the *fundamental constituents of heredity*. In fact, many biologists, until quite recently have written in textbook expositions and glossaries that genes are the "fundamental units of inheritance". What does it mean to say the gene is the *fundamental* unit of heredity? Presumably it means that if you could identify every gene and every difference in every gene, and if you could trace the transmission of each gene and each gene difference from one generation to the next, then you would have a comprehensive basis for understanding everything about heredity.

Scientific metaphysicians of the 1930s could have gone further. Since differences in genes cause phenotypic differences by affecting the way organisms develop, genes might also be the *fundamental units of development*. What would this mean? It would mean that if you could identify every gene and the functional role of every gene, then you would have a comprehensive basis for understanding everything in development. A scientific metaphysician of the 1930s would not need to speculate about the role of genes, but they could draw on the success of the transmission theory to claim that genes must have functional roles of some kind, and that fulfilling these roles must provide the fundamental basis underlying the development of organisms.

Although this might appear excessively speculative, consider the point that many contemporary metaphysicians take to be a marker of metaphysical significance: unification (e.g. Ladyman et al 2007, Ladyman this volume). Identifying the gene as the fundamental unit of both heredity and development would unify their understanding of these phenomena. With the establishment of classical genetics, the science of heredity (genetics) and the science of development (embryology) had become disunified. But by going to the fundamentals, a metaphysician could have exclaimed, "this metaphysical interpretation reveals an underlying unity of nature!" This would be viewed as a basis for confidence. But why stop with the processes of inheritance and development?

Scientific metaphysicians of the 1930s might have wondered whether genes could also be *the fundamental units of evolutionary change*. Evolution involves the inheritance of changes in the forms of organisms over many, many generations. This must involve the inheritance of changes in the way organisms develop these forms. So, the metaphysical interpretation might go, genes must be the fundamental units of evolutionary change. Again, we could ask, what would this mean? Presumably, it would mean that if you could identify every gene and every difference in every gene that has occurred in evolutionary history, if you could identify the first appearance of each of these genes and gene differences, and if you could trace and explain the changes in frequencies of these genes and gene differences, then you would have a comprehensive basis for understanding evolution.

Scientific metaphysician in the 1930s could have appealed to the latest developments in evolutionary theorizing and drawn upon the abstract theoretical work of Fisher, Haldane, and Wright to substantiate this view. The unification of fundamental principles of evolution with the fundamental unit of heredity and development could have made this interpretation of the best scientific theories of their day extremely appealing.

Of course, I am not advocating the idea that genes really are the *fundamental units* of heredity, development, or evolution, or even the more modest idea that scientists and philosophers of the 1930s had good reason for believing this. After all, section 3 shows that analyzing the classical concept of the gene in the context of scientific practice (rather than abstractly) reveals that there was good reason *not* to view classical genes as fundamental units of inheritance. The aim of this section is to show that today's favored approach to scientific metaphysics is unreliable. We can see this because if philosophers of the 1930s used this approach to identify the fundamentals of life, and if they analyzed the transmission theory and explanations in an abstract fashion (i.e. not in terms of the role they played in the context of investigative practice), they could have erroneously concluded that genes are the fundamental units of heredity, development, and evolution.

5. What is a gene? The molecular gene concept in scientific practice.

How should contemporary scientific metaphysicians answer the question: "what is a gene?" Most philosophers weighing in on this question have concluded that trying to answer it is hopeless. For example, consider the prevalent idea that genes are units in DNA that "code for" polypeptides. (Some background: DNA is a macromolecule consisting of two complementary linear sequences of nucleotides; RNA is a macromolecule consisting of one linear sequences of nucleotides; a polypeptide is a molecule made up of a linear sequence of amino acids.) According to this prevalent idea, genes are subsequences of nucleotides in a strand of DNA. The basic story behind this conception goes as follows. A linear sequence of nucleotides in DNA comprises a gene, and this sequence is "transcribed" into a corresponding sequence of nucleotides making up an individual RNA molecule during RNA synthesis. Subsequently, the linear sequence of nucleotides in this RNA molecule is "translated" into a linear sequence of amino acids making up a polypeptide molecule during polypeptide synthesis. But it turns out that the syntheses of RNA and polypeptide molecules are far more complicated than this story suggests. These complications render this simplistic conception of genes ambiguous, vague, and exception-ridden.

On this basis, Evelyn Fox Keller (2003) argues that the term 'gene' has outlived its usefulness, and many philosophers of biology agree with Keller. Much of the philosophical literature on this topic implies that the fundamental units of genetics exist at smaller scales and are more varied than suggested above. Some philosophers have argued that the real "molecular-level" units of genetics are not genes, but what were once thought to be parts of genes: promoters, enhancers, exons, and introns. Some philosophers have also argued that genes exist, but only at the "higher level" of classical genetics. Other philosophers have proposed novel gene concepts that seem to depart significantly from conceptual practice. For example, one idea is that genes are processes rather than entities (Griffiths and Neumann-Held 1999). But for the most part, philosophers have decided that today's science tells us that there is no such thing as a gene at the "molecular level".

I have argued that an analysis of how contemporary geneticists reason when they use the term 'gene' reveals that they use a multiplicity of concepts (Waters 1994). Sometimes it is useful to be vague, and in such contexts biologists invoke a blunt concept akin to the gene concept of classical genetics (described in section 3). In other contexts it is important to be precise. When precision is important, biologists employ what I have called the *molecular gene concept* (Waters 1994, 2000).[10]

The molecular gene concept has placeholders. When the placeholders are filled, the concept can be used to pick out precise segments of DNA. So this concept is precise. But it is also flexible because the placeholders of the concept can be filled out in a multiplicity of ways, and the different instantiations that result can be used to pick out different segments of nucleotides in DNA. Some instantiations pick continuous segments, others pick out discontinuous segments. Some instantiations pick out segments that overlap segments picked out by other instantiations. The overall situation is very messy. But the flexibility of this concept enables biologists to pick out different precise segments of DNA that are relevant to different explanatory, investigative, and manipulative interests.

The molecular gene concept can be specified as follows:

[10] Paul Teller comments that chemistry employs different concepts of atom in a similar way. Some concepts are useful in certain contexts because they are blunt. Other concepts are useful in other contexts because they are precise. Although what I say here is directly about genetics and allied sciences, I believe it applies to sciences more generally.

a gene *g* for linear sequence *l* in product *p* synthesized in cellular context *c* is a potentially replicating nucleotide sequence, n, usually contained in DNA, that determines the linear sequence *l* in product *p* at some stage of DNA expression.

The referent of any gene, *g,* is a specific sequence of nucleotides. The exact sequence to which a *g* refers depends on how the placeholders *l, p, and c* are filled out.[11] As figure 1 illustrates, this provides biologists with the conceptual means to pick out precisely what DNA segments determine different linear sequences in different stages and contexts of DNA expression.

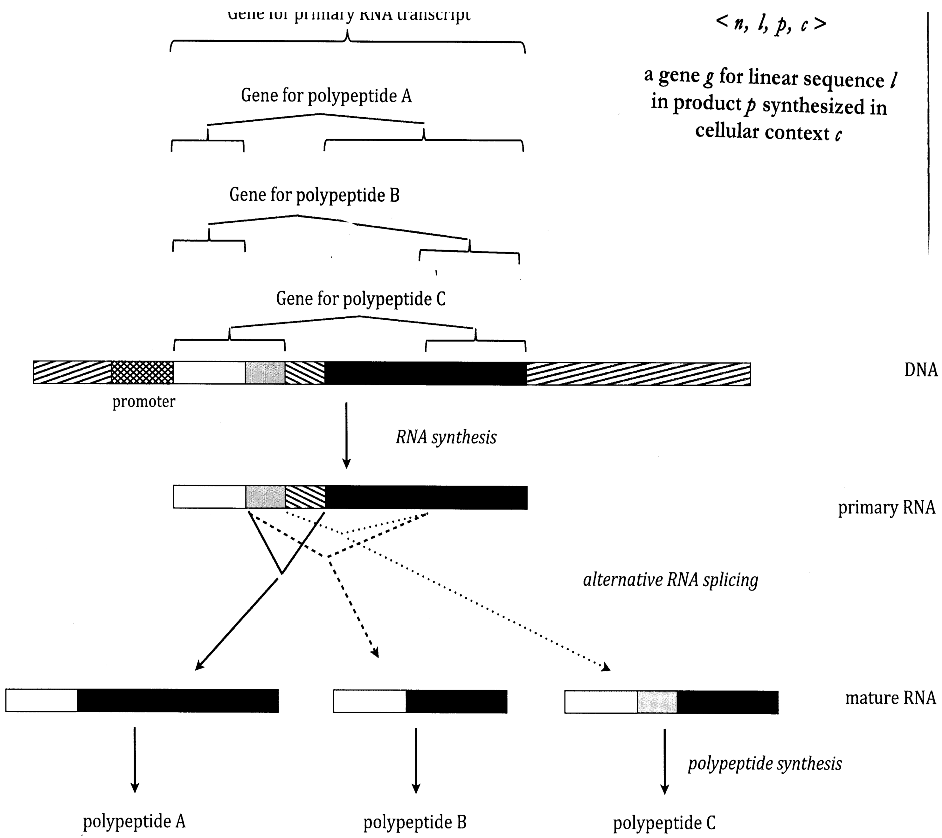

FIG. 1: The molecular gene concept enables biologists to partition DNA in multiple ways. With respect to this figure, if biologists are interested in the synthesis of the primary RNA transcript (see above), then the DNA segment covered by the bracket directly under 'gene for primary RNA segment' is the relevant gene (the continuous region of five segments in the DNA molecule starting with the solid white segment and ending with the solid black segment). If biologists are interested in the synthesis of

[11] The relevant sense of 'determine' is analyzed in Waters 2007.

polypeptide B occurring in some tissue at a particular stage of development, then the relevant gene is the discontinuous region marked by the brackets directly under 'Gene for polypeptide B' (the white and black segments in DNA and not the segments between).

The molecular gene concept is a remarkable conceptual tool. It gives biologists the flexibility they need to pick out DNA segments that line up with different causal chains (or processes) within the incredible complexities of DNA expression and development. It does so by providing the basis for partitioning the DNA molecule in a multiplicity of different ways.

In answer to the question, "what is a gene?", a contemporary scientific metaphysician adopting the traditional approach might answer that a gene is any segment of nucleotides that satisfies the analysis presented in this section. After all (trusting this analysis), this is what the best scientific theorizing of today employs as its gene concept.

6. Problems with applying the traditional approach of scientific metaphysics to genetics.

One problem with applying the traditional approach of scientific metaphysics to genetics is that the results are unstable. It leads to results that might seem plausible at one time, but turn out to be implausible afterward. For example, the fundamental interpretation of classical genetics set out in section 4 turns out to be simply false. Subsequent developments in genetics, developmental biology, and evolutionary biology have shown that genes are not *fundamental* units of heredity, development, and evolution. Genes are not the fundamental units of heredity and development because it is plainly false that knowledge about the functioning of each and every gene would provide the fundamental basis for explaining everything about heredity or development (Griffiths and Stotz 2013). Genes are not the fundamental units of evolution. Even if one could identify each and every gene that has occurred in evolutionary history, each and every difference in each gene that has occurred in evolutionary history, the first appearance of each of these genes and gene differences, and even if you could trace and explain the changes in frequencies of these gene differences, you would still not have a comprehensive basis for understanding all of evolution.

The attempt to interpret the best theories of the 1930s to identify the fundamentals of heredity, development, and evolution would have led to false conclusions (and the conclusions would not have been trivially false, they would have been false in significant ways). I wish to emphasize that the problem is with trying to interpret the transmission theory in a way that will reveal the *fundamentals*. The problem is not with the transmission theory as it was used in practice. The theoretical explanations, as sketched in section 3 (see Waters 2007 for more details), have withstood the test of time. Subsequent findings at the molecular level indicate that these explanations were correct. The transmissions of differences in functional units arranged linearly in chromosomes were indeed causally responsible for the patterns of phenotypic transmission produced in the laboratory. History does *not* undermine a realist interpretation of local explanations of classical genetics. But history does undermine an attempt to interpret the basic theory in a way that reveals the fundamental structure of the world (or the fundamental structure of inheritance, development, and evolution).

But perhaps the theories of genetics in the 1930s were not sufficiently developed to provide a basis for drawing stable metaphysical conclusions. Traditional scientific metaphysicians might respond that we should look to today's science, not yesterday's. And doesn't my analysis of the molecular gene concept support the idea that genes are fundamental units? After all, the analysis identifies a kind of internal structure and a kind of immediate functional role (in RNA synthesis) allegedly shared by all molecular genes. So this class of genes could be considered to be a natural (or fundamental) class. But this is a natural class (or kind) in a *weak sense*, in the sense of sharing an internal makeup and structure and having the same kind of proximate causal impact.

In a *stronger sense*, natural classes (or kinds) are supposed to provide the single correct parsing that reveals the joints of nature. Such classes would be *categories of being* that key into the fundamental structure of the world. If we looked at the gene concept out of the context of investigative and manipulative practices, it might appear to designate a natural class in this strong sense. But when we see how the concept is employed in practice, *and why it is a useful concept*, we see it designates a natural class only in the weak sense.

The molecular gene concept is useful not only because of its precision, but also because of its flexibility. The concept is relational. It is a *gene for* concept (Waters 2000). It can be applied differentially, to pick out different causal chains passing through DNA. These chains of causation extend from bewildering causal complexities of cellular functioning through different portions of DNA and then continue out into equally bewildering causal complexities of the cell. Multiple causal chains are entangled with one another and proceed through overlapping segments of DNA.[12] The molecular concept enables biologists to identify and manipulate individual chains in precise ways while other chains are stable (often systems are held stable via experimental manipulation). This enables biologists to slip and slide through causal entanglements that they do not understand in their natural complexity. In multicellular eukaryotes, differential splicing leads to different proteins in different functional contexts in different tissues at different stages of development. In some contexts, one set of exons plays a key role determining linear sequences; in other contexts other segments play key roles in determining linear sequences. The molecular gene concept enables biologists to specify the exons relevant to the synthesis of different proteins in particular cellular contexts without knowing much about the overall complexity of the natural situation.

[12] Griffiths and Stotz 2013 supports this account of the causal situation, though it does not argue for the kind of metaphysical view advanced in this chapter. In fact, its argumentation seems to assume a much different epistemology and metaphysics than the one being developed in this chapter and the corpus of work this chapter draws upon (Waters 1994 through Waters 2014).

The fact that this kind of conceptual practice works so well in genetics supports the idea that molecular genes are not a natural class in the stronger sense. That is, they do not provide a single correct parsing of DNA that reveals fundamental joints of nature. If molecular genes were a natural class in this strong sense they would provide the unique division of DNA that would line up with the uniquely correct and comprehensive partition of development processes. Parsing DNA at its joints would line up with a parsing developmental at its joints. But geneticists have not identified such a canonical parsing. So, the metaphor of dividing nature at its joints does not seem to fit. There are simply way too many "joints" in DNA and way too many useful ways to parse the molecular and developmental processes. I have already explained that some parsings of DNA key into particular processes in particular tissues at particular developmental stages, other parsings key into different developmental processes in other tissues and/or other developmental stages. But the same is true from the other direction. Biologists have not found a parsing of genetic control processes that keys into a canonical set of joints in DNA. In sum, what they have found is that just as DNA complexly impinges on a causal mess, a causal mess also complexly impinges on DNA. That is why conceptual practice takes the form it does. Biologists have designed the molecular gene concept so they can navigate through a mess.

My concern is not that individuating genes in DNA depends on employing a relational concept. Rather, my point is that the way this relational concept is employed in practice indicates that the overall situation is far too messy to apply the metaphor of cutting nature at its joints. The remarkable feat of biologists is that they have succeeded in developing conceptual and technical tools that enable them to maneuver within bewildering messy complexities without having an overall theory or understanding of these complexities. My metaphysical claim is that scientific practices in genetics and allied sciences take this form because they are adapted to a reality that has no overall structure. The reality has lots of structure, but no overall structure.

7. Drawing general conclusions from conceptual practice in genetics.

Scientific metaphysicians might respond to my skepticism about the idea that the intermediate-scale world that biologists engage lacks an overall structure by retreating from biology. "Perhaps biology is not ready for metaphysics. Perhaps geneticists haven't found nature's joints. But the joints *must* exist. If not in DNA, then elsewhere." I disagree. Genetics is sufficiently advanced to draw important metaphysical conclusions. But to draw them we must broaden our attention from a narrow focus on its theories and theoretical concepts, to how its theories and concepts are used in the context of broader investigative and manipulative practices. In addition, we should not assume at the outset that the world must have an overall structure, or that living systems and processes must have an overall structure at the scale being manipulated and investigated by biologists.

The practice of genetics enables biologists to control and manipulate a wide variety of phenomena and to gain new knowledge about the entities and processes of life. If the metaphysical ideas set out in section 4 were true, then the practice of contemporary genetics would presumably take a different form than it does. It would be based on a gene concept that provided a canonical parsing of DNA. But contemporary gene concepts do not provide such a parsing (or imply that such a parsing exists *even in principle*). Instead, biologists rely on gene concepts that enable them to parse DNA in a multiplicity of ways. Why does their practice take this form? The simple answer is "because it works". Practice has been adapted to work in the reality of the world that biologists are engaging.

A metaphor might be helpful here.[13] Biologists using genetics to investigate the workings of organisms are like newcomers trying to navigate in a strange city. Suppose they enter the city of Arles in southern France. The streets of Arles lack a general, overall structure. You can learn to navigate in one small part of the city, but that does not help you anticipate the layout of another part of the city. Contrast this with newcomers arriving in the western Canadian city of Calgary. The streets of Calgary are laid out in a grid with four quadrants. "Streets" are oriented north/south (they run from north to south or from south to north) and "avenues" are oriented east/west. Streets are numbered sequentially in each quadrant, starting from the center of the city. For example, streets in the northwest quadrant are named 1 Street NE, 2 Street NE, and 3 Street NE. Avenues are named in the same fashion, for example 1 Avenue NE 2, Avenue NE, and 3 Avenue NE.

The best strategy for learning to navigate in cities like Calgary is to key into the overall structure. Exceptions to the grid structure exist near rivers and railroad tracks, but knowing there is some general, overall structure will facilitate newcomers' exploration of the city. Newcomers investigating Washington D.C. would also be well-served by a strategy that rests on the assumption that there is some overall structure of streets, even though the structure of streets in D.C. is quite different than the structure of streets in Calgary. But newcomers to Arles should strategize their investigation differently because there is no overall structure to the street layout in Arles. My claim is that the domains being investigated by biologists using genetics are more like Arles than Calgary. We can infer this from the nature of the practices used in genetics. The world within organisms, like the street layout of Arles, is a mess. The practice of genetics has been adapted to navigate through the mess; the molecular concept of the gene is designed to enable biologists to investigate and control causal pathways within a messy, dynamic entanglement of interacting causal chains, an entanglement that they do not understand in its complexity.[14]

The idea that the reality within organisms lacks an overall structure can be generalized. It implies that the world in its entirety lacks a general overall structure. That is, it suggest that the world lacks a "the structure" that spans scales. But perhaps the domain (part and scale of the world) investigated by genetics is an exception. Perhaps the world is like the street layout of Calgary, and the living portion of the world at the

[13] I thank John Norton for suggesting this metaphor.
[14] The distinction and metaphor presented here is somewhat similar to Feynman's distinction between Greek and Babylonian approaches to theorizing (1967) as discussed by Wimsatt (2007). But Wimsatt's rich and provocative discussion seems to assume that the world itself has a fundamental structure even though physical theories do not.

scale being investigated by geneticists is akin to the portion of Calgary located near the Elbow River, where the grid breaks down and the street layout is messy.

In section 2, I described the generality across scales thesis, which states that features of the world at intermediate scales (such as the scales of macromolecules, cells, organisms, and ecosystems) are not necessarily less general than features at the smallest and largest scales. The generality across scales thesis implies that the feature of having no overall structure could obtain quite generally across scales being investigated by biological and social scientists, and by physical scientists in many fields as well. Why should we think that the universe has structure at every scale or that the principles (or laws) at the smallest scales provides an overall structure at larger scales?[15] Is the structure of the world a fractal?

There is much work to be done to clarify the widespread assumption that the world has a "*the* structure" and to articulate the contrary no general structure thesis. This work will require drawing upon traditional work in metaphysics as well as carefully analyzing a greater breadth of scientific practices across a range of sciences, biological, social, and physical. This chapter offers a start by arguing for the idea that the reality engaged by geneticists lacks an overall structure and by raising the question of whether the widespread metaphysical assumption that the world has a "the structure" is mistaken.

8. Does the no general structure thesis count as significant metaphysics?

Does the claim that the world lacks a general, overall structure that spans scales count as a *significant* metaphysical idea? I close this chapter by responding to four objections to the supposition that it does. But before proceeding with the first objection, it is worth pointing out that a denial of a metaphysical thesis is itself a metaphysical thesis. The assumption that the world has a "the structure" across scales, which is widely held by philosophers of science, is a metaphysical thesis. Hence, claims that deny this assumption are also metaphysical. So the claim that the reality within organisms lacks an overall structure at the scale at which geneticists engage, and more generalized versions of this claim, must also be metaphysical.

Objection 1: The no general structure thesis is too skeptical to count as metaphysics. After all, it amounts to an antirealist view about science, so it must be anti-metaphysics.

This chapter does not advance an antirealist view of science. Section 3 sketches classical geneticists' explanations of phenotypic transmission patterns for two purposes.

[15] The significance of the idea that the science of fundamental physics "holds" across scales, often expressed by philosophers of physics (e.g. Ladyman, this volume), is unclear. Does it imply that the laws or principles of fundamental physics structure the world across scales? This depends upon how we interpret 'holds'. I believe there is a way to interpret the idea that a set of principles "holds across scales" that is consistent with the no generality across scales thesis. This is an idea worth exploring, but I do not have the space to do so here.

First, to show that the theoretical explanations of classical genetics have stood the test of time. We should be realists about the geneticists' theoretical claims that gene differences caused phenotypic differences in the experimental contexts they constructed and that patterns of phenotypic transmission produced in experiments resulted from the transmission of gene differences from one generation to the next. Classical geneticists were right about the causal relationship between genotype and phenotype in their experiments, they were right about gene differences being located in linear fashion in chromosomes, and they were right about the roles that chromosomal mechanics in meiosis played in producing the patterns of gene transmission they produced in the laboratory.

The second purpose for sketching the theoretical explanations in classical genetics is to separate this realist account of the transmission theory from a fundamentalist interpretation of this theory (the former is presented in section 3, the latter in section 4). I agree with Cartwright (1999, p. 23) that fundamentalism, not realism, is the problem.

We should also be realists about the central theory of contemporary genetics, but again we should separate our realism from a fundamentalist interpretation of the theory. Scientific metaphysics should proceed from an analysis of the form practice in genetics takes, not from an analysis of its core theoretical concepts removed from the context of that practice. Simply put, metaphysics should be practice-centered, not theory-focused.[16] But practice-centered metaphysics does not ignore theory and it does not adopt an anti-realist attitude towards those theories. Hence, this metaphysics is not based on antirealism.

Objection 2: Can we really draw a conclusion about metaphysics from scientific practice? Doesn't this analysis simply reveal epistemological or cognitive limitations?

This objection expresses a worry that applies to all metaphysics. When metaphysicians employ methods of analytic philosophy to investigate how the world must be, might they just be investigating how *we* must conceive the world because of our cognitive limitations? When traditional, scientific metaphysicians interpret the theories and explanations of physics to investigate "the structure" of the world, might they just be reinforcing scientists' biases or theories that are partly shaped by our limited cognitive abilities? Might the apparent mess of causal interactions within organisms being investigated using genetics have an overall structure that humans cannot recognize because of cognitive limitations? Yes, yes, and yes.

Metaphysics is risky business. Scientific metaphysics is based on the idea that appealing to science to inform metaphysics will decrease the risks. Appealing to a broader analysis of science, one that examines the use of concepts and theories in the context of investigative, manipulative, and local explanatory practices will better inform metaphysics. I maintain that informing metaphysics by analyzing the concepts and theories of science from a narrow, theory-focused perspective is riskier than informing

[16] For more on the distinction between practice-centered and theory-focused, see Waters 2014.

metaphysics by analyzing the investigative and manipulative practices in which these concepts and theories are employed.

Objection 3: This conclusion is not sufficiently general to count as metaphysics.

A preliminary response to this objection is given in section 2. But it is worth re-stating with respect to the no general structure thesis. A critic advancing this objection might be willing to grant that the causal processes within an organism lack an overall structure at the scale at which geneticists engage. But the critic could still object: "So what if the world at this scale within organisms is a mess? This does not mean that the nonliving world at this scale is a mess (scope issue). And it does not even mean that the world within the organism is a mess (scale issue). To determine whether the world is a mess, one needs to consider everything in the whole world not just some entities within it (organisms). And to consider the world at this universal scope, one must consider it at the appropriate scale, a much smaller scale than the scale of causal interaction being investigated by geneticists. Fundamental physics is the only discipline that can reveal whether the world has an overall structure and what that structure might be."

In section 2, I respond to this kind of objection by suggesting that we should adopt a different sense of general. Instead of thinking of general in terms of applying to everything at some one scale, we should conceive of generality across different scales. Claims about an overall structure existing at very small scales are not *necessarily* informative about whether there are overall structures at larger scales. The *generality across scales thesis* suggests that the no overall structure idea applies quite generally across scales being investigated by biological and social scientists, and by physical scientists in many fields as well. Metaphysics can (and should) be thought of as concerning what is true across many scales, *especially* across scales we directly engage and experience as human beings.

Objection 4: This conclusion does not count as a significant metaphysical thesis because it is trivial.

Stanford (this volume) critiques traditional scientific metaphysics and argues that it adds nothing of value to philosophy of science. I believe Stanford's critique raises an important question: what is scientific metaphysics good for? Being informed by science, by itself, does not make a metaphysics important. Metaphysics should not only be informed by science; metaphysics should be informing science, the rest of philosophy, and society as well. I conclude this chapter by arguing that the no general structure thesis is a metaphysical doctrine of real importance.

With respect to philosophy, the no general structure thesis can inform our epistemology of science. Consider philosophy of biology. This thesis implies that the questions set out in section 2 should *not* be interpreted as fundamental questions. The distinction drawn with respect to classical genetics should be drawn with respect to these questions as well. In the case of classical genetics, the science provided accurate descriptions and explanations, but it should not be interpreted to yield a fundamental account of heredity, development, or evolution. Such interpretations rest on an assumption that the processes of heredity, development, and evolution have some fundamental structure such that it must be possible to find a basis of scientific understanding that keys into that structure. But the no general structure thesis implies that our interpretations of scientific knowledge should not be premised on the faith that such structures exist. This thesis has important implications for how philosophers of biology should interpret and answer questions such as, "what is a species?", "what is an organism?", or "what is a Darwinian population?" In addition, the no general structure thesis has implications for how we should interpret and answer meta-scientific questions such as, "what is a natural kind?"

The no general structure thesis can inform science as well. The quest for a comprehensive and unified, or even integrated, explanatory perspective can be a useful heuristic, but it should be viewed a heuristic, not as *the* aim of science. Practicing geneticists were right not to follow philosophers when we obsessed about questions such as 'what is fitness?' or 'what is a gene?' It might be heuristically useful to ask such questions, but when it is not useful, such questions can simply be dropped and the project of investigating and manipulating nature can resume. A philosophy of biology that asked, "what ways of conceiving of biological individuals could be useful?" and "in what contexts and for what purposes would they be useful?" would be much more informative to biologists than simply asking, "what is a biological individual?". The no general structure thesis is important to science. A philosophy of science that adopted this thesis would be more useful to scientists.

The no general structure thesis is far from trivial. It has important implications for how scientists should conduct their investigations and how we (philosophers of science) should conduct ours. But shouldn't metaphysics do more? Shouldn't it also inform society about how to conduct its affairs? The no general structure thesis can deliver on this desideratum as well. It suggests that society should admire science for its secure knowledge about the world, but that society should not interpret that knowledge as if it depended upon or provided an understanding of fundamentals. Research in genetics should be supported and funded, but not because it will "decode life" or reveal the magic keys to unlocking solutions to the complex problems societies face. The no general structure thesis applies generally across scales at which we experience the world. It has implications for how philosophers should understand scientific knowledge and for how scientists construct such knowledge. And it can inform the public understanding, support, and use of science. Such a thesis counts as significant metaphysics.

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