Philosophers have always sought general theories.¹ We have sought general theories of truth, knowledge, justice, and beauty, to mention just a few. In The Principles of Morals and Legislation, for example, Bentham says that ‘By the principle of utility is meant that principle which approves or disapproves of every action whatsoever, according to the tendency which it appears to have to augment or diminish the happiness of the party whose interest is in question’. He goes on to emphasize, ‘I say of every action whatsoever; and therefore not only of every action of a private individual but of every measure of government’.² Likewise, Tarski’s theory of truth was intended to be general in virtue of its formality, and he makes it clear that he thinks it important that a semantic theory applies, directly or indirectly, to psychology, sociology, linguistics, physics and biology.³ For whatever reasons, in many different fields, it has seemed important to aim for general accounts. This predilection for general theories has played an important role in philosophy of science. Philosophers have sought general theories of confirmation, explanation, reduction, and rationality, and, of course, of theories themselves. It has seemed important, for example, to produce an account of explanation extending across the entire range of the sciences, and it was regarded as a serious challenge when some philosophers came to contend

¹There are at least four dimensions to generality: (1) universality of form; (2) no dependence on, or reference to, particulars; (3) unrestricted validity; and (4) wide (as opposed to narrow or specific) range of application. We offer no account of how these are related, or how they should be balanced.

²Chapter 1, Section 2; originally published in 1789.

that historical sciences embodied a radically different sort of explanation. It was a central goal of Hempel’s initial work on scientific explanation to show, contrary to writers such as Collingwood, that historical and physical explanation had essentially the same structure, however much they might differ in subject matter. The goal remains central in more recent work on explanation.

Unrestricted generality is an important ideal. But generality is not the only virtue, either in science or philosophy. An acceptable theory must not only present a body of unrestricted truths. Acceptable theories must also meet a number of traditional epistemic desiderata. Some, such as precision and testability, we will not discuss. We will appeal to some pertaining to justification and support in passing. Good theories should be susceptible to multiple independent lines of investigation and confirmation. They should also enable us to cover a large territory with little equipment, and this should be accomplished without appealing to ad hoc devices. That is, generality ought to be accompanied by conceptual simplicity. The combination of generality and simplicity has methodological teeth. It suggests, for example, that one should be able to grasp the essentials of the theory of evolution by concentrating on a few key notions, such as adaptation, fitness and drift. Stepping up a level, in philosophy of science this same point suggests that a good theory of, say, explanation should apply equally well to quantum mechanics and evolutionary biology, or to history and economics.

Some philosophers, like many scientists, have reacted against the ideal of generality as the cardinal virtue of theories. Working independently and from different perspectives, each of us has repeatedly argued that there is not much generality in biology. The material treated in genetics, systematics, and evolutionary biology should be, and is, covered by many loosely interconnected theories that, taken

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together, require much richer conceptual equipment than would a corresponding single general theory. Biology, at least, proceeds primarily from local theories, as we will illustrate in the case study below.

Philosophers of science need to recognize the importance of such theories in philosophy as well as in science. Whether or not there are well-grounded 'universal' theories, one cannot understand the development of science except in terms of the interaction of partially conflicting local theories. Historically, there is room to argue that no general—or even supposedly universal—theory is epistemologically well-founded in the abstract, apart from its local applications. Though our argument does not depend critically on this claim, it is our view that the drive toward universality and unification stems from an expansion of the domain of local theories and the attempt to integrate them with one another. We suspect that there would be a considerable gain in our understanding, even of supposedly universal theories, by treating them in terms of their local roots. Philosophy, too, needs local theories; in particular, we will argue that it is a mistake to start from a general theory of meaning, reference or truth, abstractly grounded. We begin instead with variation in usage, with changes of meaning and reference as theory and context change, and with variation in the conditions for truth as meaning and reference shift. Philosophy can profit from a switch to a more scientific style, employing models of limited scope, drawn from a number of overlapping and partially competing perspectives and disciplines. Ours is an image drawn from sciences concerned with complex systems, especially the biological sciences. We will draw on the history of genetics in order to undermine attempts to employ general theories of meaning and reference. Although our discussion focuses on the concept of the gene, the moral applies equally well to such concepts as mass in physics, element in chemistry, and utility in economics.

II

Our central purpose is philosophical, so we shall not develop our central example— which involves the history of the concept of the gene—in great detail. But
a brief thumbnail sketch of some relevant issues will be useful at this point. Genetics is supposed to provide an account of inheritance, of the transmission of traits from parent(s) to offspring. Genes, of course, are what is actually passed on. Though there was much work on heredity during the last half of the nineteenth century, there was not much that we would recognize as genetics until some time after 1900, with the ‘rediscovery’ of Mendel.\textsuperscript{8} Throughout the period following publication of the \textit{Origin of Species} (1859), theories of heredity were developed by a variety of figures, including Ernst Haeckel, Herbert Spencer, Hugo de Vries, August Weismann, and, of course, Charles Darwin himself. There were large differences between the views they proposed, though they were generally committed to particulate theories which treated hereditary transmission as due to particles contained in the germ plasm passed on physically from parent to offspring. The goal of all these theories was to explain a variety of hereditary and developmental phenomena. Indeed, during this period, hereditary and developmental phenomena were not clearly distinguished; the \textit{explananda} encompassed a wide array of effects, including the commitment to a certain \textit{Bauplan}, the resemblance of offspring to parents, and the occasional reversion to ancestral type, as well as some pseudo-phenomena such as the tendency of female offspring to resemble the mother more than the father and the inheritance of acquired characters. Nineteenth-century theories of inheritance thus aimed to explain a wide array of phenomena, many of them developmental in character; they were certainly not limited to what subsequently came to be regarded as the domain of genetics. In terms of methodology, however, much of the work that eventually led to twentieth-century Mendelian theories was based on hybridization and breeding experiments that followed the distribution of clearly marked traits in the offspring of parents with clearly marked differences. Soon after 1910, this work was reinterpreted in light of the experiments with \textit{Drosophila}, including, notably, those begun by Morgan, Bridges, Sturtevant and Muller at Columbia in 1910.

It was at least twenty years after the ‘rediscovery’ of Mendel before a clear distinction between genes and the traits by which they are identified was worked out. Indeed, the term ‘genetics’ was not introduced in print until 1906, when Bateson used it to encompass the study of variation and heredity.\textsuperscript{9} Many influential geneticists—

\textsuperscript{8}It is somewhat ironic, though not surprising, that Mendel was not engaged in what we now recognize as Mendelian genetics; instead, he was concerned primarily with demonstrating the instability of hybrid species. This is tangential to our concerns in this paper. See R. Olby, \textit{Origins of Mendelism}, 2nd Edn (Chicago: University of Chicago Press, 1985), and ‘A Retrospect on the Historiography of the Life Sciences’, in J. D. North and J. J. Roche (eds), \textit{The Light of Nature} (Dordrecht: Martinus Nijhoff, 1985), pp. 95–109.

among them Bateson and de Vries—thought of the unit of inheritance as the ‘unit character’. The latter expression was used indifferently for the gene and for the trait by means of which it was identified. Even this may be misleading, for no clear and consistent distinction was drawn during the period between the observed character and its material basis. It was in 1909 that Johannsen set forth a sharp distinction between the phenotype—the total set of characters which can be measured and observed—and the genotype—the inherited potential. The phenotype is the result of an interaction between a developing organism and the environment in which it lives and grows. Genes in turn became components of the genotype.\textsuperscript{10} It took at least a decade more for the distinction between genes and the traits they underlie to enter widely into the Mendelian literature and displace the terminology of ‘unit characters’, but by the 1920s, most Mendelians treated genes as distinctive underlying causes of organismal traits. Genes were generally considered to be localized on chromosomes, following the results of the Morgan group and others, but they were of unknown constitution and their mode of action was also unknown. Thus, \textit{faute de mieux}, a gene was identified functionally as the transmitted cause, whatever it might be, of a particular trait or complex of traits. In slightly different words, genes were \textit{indefinitely} described entities, individuated by way of their effects and their pattern of transmission. Other aspects of gene concepts concern the material transmitted and the way this material is organized. For ease of exposition we will collectively label these as \textit{structural} aspects of the gene or of gene concepts. The differences in the way genes may be identified and described will play an important part in what follows.

The use of functional means of identifying genes is readily illustrated. In the work of the Morgan school, there are genes for eye color, each of which causes some effect on eye pigmentation; they are distinguished from each other by their separability in transmission from one generation to the next. A. H. Sturtevant, in the very first demonstration of the linear arrangement of genes on a chromosome identified the six ‘factors’ he analyzed \textit{only} in terms of their phenotypic effects: one for body color, three affecting eye color, and two concerning wing structure.\textsuperscript{11} Nearly ten years later, writing about the origin of variation, H. J. Muller said this:

\begin{quote}

The chemical composition of the genes, and the formulae of their reactions, remain as yet quite unknown. We do know, for example, that in certain cases a given pair of genes will determine the existence of a particular enzyme (concerned in pigment production), that another pair of genes will determine whether or not a certain agglutinin shall exist in the
\end{quote}


blood, a third pair will determine whether homogentisic acid is secreted into the urine ('alkaptonuria'), and so forth.  

Muller allowed that nothing was known at the time about the structures that underlie genetic mechanisms. Consequently, he approached the problems entirely in terms of functional capacities of genes, including their mutagenic, catalytic and autocatalytic properties. Finally, in the classic work of George Beadle and E. L. Tatum, mutant strains of Neurospora were identified solely in terms of the biosynthetic reactions that they could sustain. These are all clearly functional identifications of genes, at different levels.

The concern with structure is also easily illustrated. In 1902, on the basis of cytological results, Walter Sutton suggested that the Mendelian law of heredity, by which he seems to have meant what we would call 'independent assortment', was due to the pairing of maternal and paternal chromosomes and their separation at meiosis. Genes, or allelomorphs, are identified as parts of chromosomes. Carl Correns independently noted the association between chromosome behavior and plant hybridization experiments, while Theodor Boveri provided experimental proof of the individuality and hereditary potentialities of visibly indistinguishable chromosomes. Although there was considerable doubt during the first two decades of the century about the relations of genes and chromosomes and, indeed, about whether what was transmitted was a material entity or, for example, a stable resonance that could account for pattern formation, by the 1920s there was widespread consensus that genes are material entities, typically—perhaps necessarily—located on chromosomes. This, together with other considerations, occasioned a search for the material of heredity, the substance out of which genes were composed. At least some theories suggested that one could define genes in terms of this (putative) substance.

By the 1930s and into the 1940s, the orthodox view was that genes are—and, indeed, had to be—composed of protein. As is now familiar, this proved to be wrong. The genetic material is nucleic acid. But whatever genes are

12 Variation Due to Change in the Individual Gene, American Naturalist 56 (1922), 32.
13 See, e.g. 'Genetic Control of Biochemical Reactions in Neurospora', Proceedings of the National Academy of Sciences 27 (1941), 499–506; for a discussion of this work, see Burian, 'Unification and Coherence'; and Bechtel and Richardson, Discovering Complexity, ch. 8.
15 Boveri’s experimental work on this topic concerned sea urchin development. His experiments showed that a sea urchin egg fertilized by multiple sperm had to have a balanced complement of chromosomes in order to develop normally. This provided the basis for an argument that the chromosomes were the carriers of the genes and has led to the standard labelling of this theory as the Sutton–Boveri or Boveri Sutton theory. See Boveri, 'Über mehrpolige Mitosen als Mittel zur Analyse des Zellkerns', Verhandlungen der physikalischen-medizinischen Gesellschaft zu Würzburg 35 (1902), 67–90; and 'Zellenstudien VI. Die Entwicklung dispermer Seeigeliecher. Ein Beitrag zur Befruchtungslehre und zur Theorie des Kerns', Jenaische Zeitschrift für Naturwissenschaft 43 (1907), 1–292.
17 Substantive reasons favoring this view included the seemingly infinite variety and controllable stability
composed of, it was also important to ascertain whether they are indivisible particles; whether they are organized in one, two, or three dimensions; whether, given a linear model, they are contiguous or separated into discrete pieces; whether they overlap one another or not; and so on. All of these positions about gene structure have been occupied at one time or another and have entered in important ways into the effort to define genes.

It is of central importance to our argument that structural and functional considerations have been intertwined to varying degrees in dealing with genes and gene concepts. Genes were originally characterized by their effects. They were the indefinitely described hypothetical entities responsible for the transmission of phenotypic properties from parent(s) to offspring. They came to be understood as material particles with an unknown structure, no longer regarded as hypothetical, but responsible for complex functions. There is a long and fascinating history involved in the effort to determine how one could properly refer to such entities, define them, or delimit them. One aspect of this was the effort to ascertain whether reference to functional aspects alone, or to both functional and structural aspects, are necessary in defining genes.18 As the discipline developed, it became increasingly clear that a knowledge of the structure and composition of genes was necessary in order to understand their functional properties. The evolving problems of genetics acquired and required multiple constraints. We will draw on additional fragments of that history in the discussion to follow.

III

It will be useful to look at how genes are defined in some more recent sources. In a recent textbook, Douglas Futuyma remarks that ‘Except in RNA viruses, the genetic material of all organisms is DNA’, and goes on to describe various differences in the organization of the genetic material. A few pages later, he says ‘To a first approximation, one may think of a gene as a DNA sequence that is transcribed into an RNA transcript that codes for a single polypeptide’, with the caveat that genes are

17continued


18Lindley Darden provides valuable insights into the interaction between these two modes of characterizing genes and the genetic material in her exploration of the Morgan school’s exploitation of the interaction between structural considerations, based largely on the observed behavior of chromosomes, and functional ones, based on the transmission of traits, in developing their account of the gene. Darden nicely illustrates the influence of such ‘interfield connections’ in shaping and constraining theory change. See L. Darden, Theory Change in Science: Strategies from Mendelian Genetics (New York: Oxford University Press, 1991), especially ch. 7. These issues are central to part III of Bechtel and Richardson, Discovering Complexity.
'exceedingly difficult to define'. In a somewhat more ambivalent form, Wen-Hsiung Li and Dan Graur suggest that a gene may be defined as 'a sequence of genomic DNA or RNA that is essential for a specific function'; they include regulatory genes as well as genes specifying RNA or protein within the scope of their definition. A first point to notice is that such statements as 'Genes are strands of DNA' are vague and perhaps ambiguous. What we are given by Futuyma, or by Li and Graur, are certainly not standard definitions. They certainly differ in reference, and therefore in meaning; moreover, these characterizations are not unambiguously conceptual or empirical. Such statements may be used to fix terminology or concepts, or to state connections discovered or discoverable by empirical means in laboratory research. In fact, such statements are thoroughly ambiguous in many biological writings, seeming to be used in both ways in the same paper or book. The distinction between conceptual and empirical claims is usually not well drawn in biological discussions. Moreover, the definitions found in textbooks and journal articles are typically partly functional and partly structural with structural and functional considerations systematically interwoven. Genes are sequences of nucleic acids, but ones which code for RNA, or for proteins, or control transcription. This sort of ambiguity is even more strikingly illustrated in the following passage from Bruce Wallace, in which he offers this rather tentative definition of the gene:

I find it convenient to regard any chromosomal region that exhibits a coordinated relationship such as a structural gene together with its nearby (usually upstream) regulating elements (even those referred to as 'genes' by others) as a gene. In higher organisms such a unit exhibits Mendelian inheritance. It is, by definition, a unit of function—not one of mutation or recombination. Control genes that lie on other chromosomes or on the same chromosome but so far removed that recombination is common are referred to as other genes, together with all other, unlinked genes. Within the tightly linked region, however, the same structural gene (A) with either one or two cis-acting (i.e. same DNA strand of chromosome) essential controls (C1 and C2) become, in my view, two alleles of one gene—A1 and A2. If the structural gene cannot perform its mission without its control, and the controls of the two identical structural genes differ, I say the genes differ. In doing so, I virtually ignore the identity of the structural genes. By the same token, I say that sickle-cell and normal hemoglobins differ regardless of the identity that these two hemoglobins exhibit at all but 1 of nearly 300 amino acid sites.

In Wallace's hands, structural considerations virtually evaporate, and what matters is whether a unit exhibits Mendelian inheritance and affects the phenotype uniformly. This sort of ambivalence is just what philosophers with holistic inclinations would expect. A Quinean, for example, would consider the situation just described entirely

22 The Search for the Gene, p. 178.
natural: no one should expect a clear boundary, or a real distinction, between analytic and synthetic. We should expect the conceptual and empirical to blend. Likewise, holists about meaning—those who hold that ‘conceptual schemes’, ‘disciplinary matrices’, or ‘theories’, as wholes determine the meanings of terms—will find no surprise in the shifting conceptual content of a term like ‘gene’. Indeed, any theory of meaning that allows empirical presuppositions to be built into the conceptual content of a term or concept would predict the occurrence of phenomena like those on which we focus here. For example, depending on what is presupposed, the empirical content of ‘Genes are strands of DNA’ will vary. If we presuppose that genes code for polypeptides, then empirical questions will center on how this is accomplished, the character of intermediaries, and similar questions. If we depend on no such presupposition, then our empirical range is expanded to include questions concerning the mechanisms of regulation, among others. Without entering into technical disputes about the theory of meaning, we want to make two distinctions that are underemphasized in most discussions. First, theses about meaning, meaning change, and meaning invariance, can be construed as making global or as local claims. Second, in making claims about meaning, or the meaning of specific terms, it is important to distinguish descriptive from normative theses.

We are inclined to endorse some form of meaning holism, construed as a global, descriptive thesis. Almost anything can be included in, or excluded from, the meaning of a term like ‘gene’. Given an overview of the history of genetics, it is difficult not to conclude that conceptual and empirical characterizations of genes offered by researchers have been so thoroughly intertwined that they cannot be sorted into two discrete packages. If asked, in the abstract, whether the claim that genes are nucleic acid sequences is empirical or conceptual, or whether it is analytic or synthetic, the only sensible response would be to decline the question. It is empirical, and conceptual. It is neither analytic nor synthetic. Locally, however, in particular contexts and at particular times, with particular issues at stake, researchers did take the trouble to disentangle the conceptual and the empirical, or aspects of the conceptual and the empirical. We have already mentioned the distinction drawn by Johannsen between the phenotype and the genotype, between the genetic constitution and its expression in visible features of the organism. Johannsen’s initial interest was in testing Galton’s law of filial regression. What Johannsen found was that selection was capable of sorting for weight or length in a heterogeneous population, but that in a ‘pure line’ it was ineffective. Two individuals might be indistinguishable with respect to, say, height, and yet respond in entirely different ways to selection. This

23Feyerabend held such views in his earlier writings at least, as perhaps did Kuhn and Sellars. There is a seldom-noticed difficulty for such views. If the theoretical or conceptual framework is supposed to determine the meaning of key theoretical terms, how is one to account for cases in which there is no resolution, within the confines of a single work devoted to the development of a single major theory, of the conceptual content of the leading theoretical concepts involved? The resulting ambiguity of such claims as ‘Genes are strands of DNA’ is important to the evaluation of holist theories of meaning.
required the distinction between phenotype and genotype. The distinction between
the two brought considerable conceptual clarity to the issues of inheritance which
occupied Johannsen, though the motivation was clearly empirical. To shift to a more
recent example, though much work with *Drosophila* was inspired by the view that
recombination could not occur within genes, Seymour Benzer's work on the 'fine
structure' of the gene in phage showed that there were hundreds of detectable sites
of mutation and recombination within the gene. The result was a distinction between
genes as units of recombination, mutation, and function. As Benzer saw, the old
concept of the gene ceased to apply univocally when applied at the level of fine
structure.\(^{24}\) Once again, a conceptual distinction allows us to make systematic sense
of empirical results. Disentangling the conceptual and the empirical is often an
advisable stance, not least when there is a breakdown of terminological unity among
research groups working in the same area.

Conceptual matters are obviously separated out by researchers themselves when
they feel that the occasion demands it. By training, philosophers are more apt to focus
on such conceptual issues; they may do so even when the empirical issues no longer
seem to require it. Philosophical analysis, whether carried out by a philosopher or a
scientist, thus may allow for reconstructions that locally disambiguate statements
commingling conceptual and empirical components. At the very least, this may help
in understanding the issues in retrospect. It also provides for a normative stance,
however minimal. The value of retrospective reformatory analysis must be judged by
its results, by the extent of the clarification and understanding it provides, or the
guidance it offers in disentangling conceptual and empirical components of problems.

The general moral is not that the conceptual and the empirical are inseparable. We
often hold some issues in place conceptually, as it were, while discussing the bearing
of some piece of evidence on a claim.\(^{25}\) However, the location of the line between
the conceptual and the empirical can and does change, not only with time, but, at a
given time, with the disciplinary (or other) practices and assumptions operating in
the background. Present-day philosophy of science, partly in reaction against the
normative stance and the excesses of logical positivism, tends toward the descriptive.
We think that philosophy has a legitimate normative role, in addition to more limited
descriptive ones. We will suggest that separating descriptive and normative theses
here provides the philosopher with important tools. For the moment, a simple example
illustrates the point. We already noted the difficulty in interpreting such statements
as 'Genes are composed of nucleic acid'. At times such statements seem to be
definitional in character, at times empirical. For certain purposes, in certain contexts,
such statements should be clarified. The resulting local reconstruction of genetic discourse can separate empirical and conceptual issues in such a way as to facilitate research. We do not think there is just one way to reconstruct any particular scientific dispute or to distinguish the conceptual and the empirical. Neither do we think such reconstruction is neutral with respect to empirical issues. Our point is rather that reaching a resolution may depend on achieving a certain degree of consistency. If different researchers or research groups draw the line between the conceptual and the empirical in different ways, this may result in a confusion that hampers research or renders conflicts and questions incoherent. When this occurs, reconstructions that clarify the separation between the conceptual and the empirical or redraw the boundaries between them may benefit research.

IV

Let us return, then, to the concept of the gene. Simplifying rather radically, we are left with the following picture. Some characterizations of genes are wholly functional: genes—whatever their structure and composition—are the units transmitted from parents to offspring that bring about certain effects. Particular genes were identified as 'the factor or gene for X' or 'the [transmitted] unit character for X'. Some early debates, for example, between followers of Morgan and followers of Bateson, can be easily construed as empirical disputes about the structural correlates of these functionally specified genes—were they stable resonances, material particles on chromosomes, or yet something else? On the other hand, once the chromosome theory was accepted, there were cytologists such as John Belling who argued that chromomerers, the least visible microscopic particle along a chromosome, were the relevant physical entities and that it was the job of the geneticist to find out what the functional significance of the different genes, thus defined and identified, might be. This is a structural definition of the gene. Once we identify the general structure, or material, responsible for inheritance, it is open to us to try to identify its function. Contemporary molecular biology often does just this. This amounts to a search for functional correlates of an entity identified via a structural concept of the gene. In general, geneticists seem to have employed some form of vaguely delimited and imprecise 'mixed' concept with both functional and structural elements. We have already noted this tendency in contemporary definitions of genes in terms of nucleotide sequences. One unfortunate consequence of this imprecision, historically, was ambiguity about the questions being investigated. Historically, the sole item that


seems to remain unchanged is that genes are transmitted from parent(s) to offspring. This constitutes, as it were, the least common denominator of gene concepts.

We will present a simplified account, meant only as a first approximation, to facilitate our discussion of the issues raised by the multiplicity of options thus opened up. We employ two schemata that conveniently capture distinct ways of reconstructing the meanings of genetical terms or concepts. Strictly speaking, the schemata do not provide interpretations of the concept of the gene, but capture two general approaches to gene concepts, each elaborated differently at different times. In this sense, they capture two families of interpretations, differing in content. The first is functional:

R1. Genes can be defined in terms of function, indifferent to structure.

The earliest work of the Morgan school, for example, was limited to functional characterizations, as we have seen. Many different functions may be taken to be central to the concept of the gene—for example, their ability to code for proteins, or to control gene expression. In *Physiological Genetics*, Richard Goldschmidt treated the gene functionally in this sense, emphasizing that genetics so understood is concerned with the transmission of hereditary traits and the control of development. In employing a functional concept, he could identify genes in ways that were relatively indifferent to their physical realization. This indifference to physical realization, characteristic of functional concepts, is important, for when it is discovered that one structure rather than another realizes, or is the salient cause of, that function, the result is obviously empirical. Given a strictly functional concept, it will not matter conceptually whether genes are composed of proteins, DNA, or RNA, whether genes are linear or three dimensional, continuous or dispersed. A functional concept is invariant relative to such discoveries about structure, and debates about structure or realization count as empirical.

The second approach incorporates structure:

R2. Genes can be defined in terms of function and an associated structure.

For example, genes might be defined as segments of DNA with a specified function. If it is then discovered that RNA can, in some cases, also function as a hereditary material and it is acknowledged that there are RNA genes, the change will be

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28John Dupré develops a parallel analysis of gene concepts in terms of a dichotomy between structure and function. Though he builds an entire chapter on this dichotomy, which is similar to ours, he uses the dichotomy for very different ends. See *The Disorder of Things: Metaphysical Foundations of Science* (Cambridge, MA: Harvard University Press, 1993), especially pp. 124 ff.


30It is important to be circumspect about such a claim. Conceiving of the gene as the unit of heredity, Goldschmidt adopted a concept that is indifferent to physical realization, though he was intensely interested in the mechanism of gene action. Much of his discussion is concerned with detailing phenomena that he selected because they constrain the problem of realization. Functional definitions are not irrelevant to realization. They constrain it. See Burian, ‘On Conceptual Change in Biology’, for more discussion.
conceptual. To be sure, the change is empirically motivated, but it is a conceptual change nonetheless. Because the chromosome theory of heredity employed, in this sense, a structural gene concept (it fixed the physical realization of genes in a way that the earlier Mendelian ‘transmission’ genetics did not), the recognition of extra-nuclear (cytoplasmic) genes represented a conceptual change for proponents of various versions of the chromosome theory. This illustrates a point that will prove important in the sequel: concepts of the sorts we are exploring include empirical claims (‘presuppositions’) as part of their conceptual content.

A purely functional account tends to suggest greater continuity of meaning in genetics than one incorporating function and structure. Analyses requiring both tend to stress conceptual discontinuities. The discoveries that genes are composed of nucleic acids rather than proteins, or of RNA as well as DNA, yield conceptual discontinuities on R2, but yield empirical rather than conceptual changes on R1. It follows that philosophers whose treatment of meaning is continuist in spirit should favor accounts built on a schema like R1. Doing so allows them to claim that there is no problem of principle (though there may be in practice) in reidentifying ‘the same gene’ no matter how our empirical understanding of the structure of genes, or of the materials of which they are composed, changes. On such an account, it is only an empirical problem to reidentify, say, the gene for white eyes in *Drosophila melanogaster* that Morgan found in 1910, the gene which proved to be the crucial vehicle for converting him to Mendelism. On the other hand, philosophers whose account of meaning is discontinuist should favor schema R2, which builds the physical structure of genes and the material of which they are composed into the very concept of a gene. Given our theories about genes, we cannot reidentify Morgan’s white eye gene, for it does not exist. R2 thus favors discontinuity because it treats new hypotheses about gene structure and composition as generating wholly new concepts of the gene, concepts whose referents must have a certain structure.

The remainder of this article is devoted to drawing out the implications of our treatment of genetics for philosophy and, especially, the theory of meaning. To prepare the way, we emphasize our stance that philosophical texts should be treated in the same fashion as we have treated scientific texts. The gene concept can be reconstructed in various ways. Similarly for the concept of meaning. We will argue that a single concept of meaning cannot cover all of science or philosophy any better than a single concept of the gene can cover all of genetics. We will not attempt to provide an analysis of the concept of meaning any more than we have for the concept of the gene, for we believe, and hope to convince the reader, that such a goal cannot be achieved. In working toward this conclusion we sometimes speak, informally, of meaning and reference on the assumption that these concepts are clear enough locally (i.e. in the context of our discussion) for us to make ourselves understood. We do not mean to imply by this necessary tactic that there is a satisfactory concept or overarching analysis of meaning or reference on which we, or our readers, can draw.
We are now in a position to bring our historical sketch to bear on the philosophical issues with which we began, specifically, on the prospect of general theories of meaning. The history of genetics presents us with marked discoveries linked with conceptual change. On the conceptual side, developmental phenomena, for example, were given center stage in theories of heredity in the nineteenth century, and then excluded altogether in the early twentieth. Hybridization became the relevant paradigm of experimental method. The distinction between genes and the characters in which they are expressed, the role of chromosomes in heredity, their linear organization, and much more, were introduced. We shall argue that attempts to analyze the extent and depth to which these changes were conceptual show that no determinate and general answer to the question, ‘What is the concept of the gene?’ is satisfactory. Likewise, the history warrants no determinate and general answer to the question, ‘How should we separate change of belief concerning empirical matters from conceptual change in the development of genetics?’ As far as the history per se is concerned, we urge in this part that the same changes can be described from different perspectives, equally satisfactorily and legitimately, as changes in concept or as changes in belief. Put another way, distillations of factual from conceptual issues can be enforced for local purposes in some contexts, but the boundaries between the two are not stable across time or space. Local separations of conceptual from empirical issues are immensely valuable, as we will see below. It is often helpful to distinguish something as integral to the concept of the gene, and something else as peripheral but these distinctions are only helpful locally. Many ways of making such distinctions are permissible; in different contexts there are valid reasons for adopting now one, now another, of the different and distinctive demarcations. As a result, one should resist theories of meaning—or of reference—which prescribe uniquely determinate and general ways to partition factual and conceptual components. Any such theory will be unable to handle the development of the concept of the gene. We do not think the history of the gene unusual in this regard. The same is certainly true of central concepts in the history of physics and chemistry, and even of philosophy. Thus, we should regard general theories of meaning with suspicion.

Our immediate goal in this part is more modest than a full-fledged attack on general theories of meaning. Here we seek to show that historical constraints will not yield uniquely determinate and general concepts. We have seen in Part IV that different reconstructions of the concept of the gene favor conceptual continuity or discontinuity, depending on whether structural components are added to functional ones in the analysis of that concept. Discontinuist accounts, emphasizing changes in concept over changes in belief, typically raise problems of commensurability and

For ease of exposition, we shall not discuss additional cases (like that of Belling, see n. 27) in which a purely structural definition of a gene was put forward. To include these would complicate, but not alter, the main lines of our argument.
It is possible to argue that these changes are superficial, and mask stability of some core of meaning or of reference, and that that is all that is required for continuity. Since the advent of Saul Kripke's landmark essays, and in light of subsequent defenses of an allied view by Hilary Putnam, it has seemed obvious to many that any adequate theory of reference must at least have a 'causal', or 'historical' component. In an idealized form, a causal theory claims that the reference of, for example, a proper name or a natural kind term is determined by genealogy and genesis; that is, it is determined on the basis of how the term has been transmitted among members of the linguistic community, together with how the term, or its ancestor, was introduced into the language. Genealogy and genesis are sufficient. More traditional 'descriptivist' theories claim that the reference of a proper name or a natural kind term is determined on the basis of an associated description or set of descriptions; that is, the reference of the term is whatever is correctly described by the description or descriptions that are associated with the noun in question, perhaps delimited by the context of utterance or inscription.

These are ideal types. In practice, defenders are often more moderate, incorporating both descriptive and causal components in their analyses of meaning and reference. Thus, causal theorists often give descriptive content some leverage either in fixing the initial reference or in subsequent transmission. The obverse is no less true of descriptivist theorists. To take a classic example, Bertrand Russell viewed a principle of acquaintance as an integral part of his theories of reference and description. Since acquaintance is to be understood as immediate contact with the object of thought, this is at least a non-descriptive component of his theory; yet Russell is a descriptivist par excellence. Classical positivist construals, according to which the meanings of theoretical terms depend on implicit definition, are also recognizably descriptivist in orientation, but in these treatments, the abstract calculus is not disconnected, but 'anchored at the solid ground of the observable facts' by semantical rules. Again, these rules are 'causal' in a suitably broad sense. Thus, the differences between descriptivist and causal theories are largely matters of emphasis rather than


differences in kind; they turn on the weight accorded to, and the significance of, the descriptive content incorporated. To repeat, we regard causal and descriptivist theories as ideal types, defining poles along a continuum. At one extreme, descriptive content alone determines reference; at the other, descriptive content is altogether excluded and reference is determined by causal role alone.

Theories of meaning and reference positioned near the causal pole tend to be continuist in spirit. Descriptivist theories tend to portray changes as discontinuous, with respect to both meaning and reference, since they treat reference as following meaning. Different styles of reconstruction, each equally legitimate in different contexts, systematically favor different views of science. In our view, the history of genetics does not decisively favor causal or descriptivist theories of meaning and reference (and thus cannot resolve disputes over 'the' correct reconstruction of science), but it inclines one to lay considerable stress on descriptive components of meaning. To see why, examine how considerations of meaning and reference play out with reconstructions of meaning along the lines of R1 and R2. Approaches based on both of these schemata focus on descriptive content. The central difference between the two concerns the amount of empirical content built into the concept of the gene. Though both lie somewhere in the middle ground between the two ideal types, R2 lies further toward the descriptive end. That is, R2 incorporates greater descriptive content. The greater constraints imposed by reconstructions along the lines of R2 raise the prospect of incommensurability because, on R2, given the actual history, marked changes in the concept of the gene, and corresponding changes in reference, are common. Many philosophers would conclude that, to this extent, R2 is discredited. There were, after all, important empirical issues at stake in determining whether genes are proteins and, once it was clear that they are composed of DNA, whether or not they are composed of continuous sequences of nucleotides. It was also important to find out that RNA, in addition to DNA, could serve as the hereditary material for certain organisms. A treatment of the concept of the gene along the lines of R2 would convert these empirical matters into conceptual ones. Moreover, researchers who have made such discoveries as those just alluded to often maintain

37 One might hope to define a causal theory that left names, or natural kind terms, entirely bereft of descriptive content. We know of no one who has actually defended this radical a view, but in any case it seems unlikely that such a view could be true of any cases, as Peter Geach long ago recognized. See Geach, Reference and Generality (Ithaca: Cornell University Press, 1962).

38 It is, perhaps, worth underscoring the role of implicit definition elaborated by logical positivists and logical empiricists as a source of incommensurability. A reexamination of Feyerabend's earlier work, for example, shows that the implicit definition of theoretical terms provided one of his most important arguments for the incommensurability of like-appearing terms drawn from different theories. See Feyerabend, 'Explanation, Reduction, and Empiricism', in H. Feigl and G. Maxwell (eds), Minnesota Studies in the Philosophy of Science, vol. III (Minneapolis: University of Minnesota Press, 1962), pp. 28-97.

that their predecessors were referring to the same entities, but were mistaken about the composition of those entities—that it was *empirical*, not conceptual, investigations that drove the discoveries. At least sometimes, treating structure as part of the concept of the gene would compromise the seemingly empirical character of these disputes, making the history look conceptually discontinuous when it was not.

By electing the approach implicit in R1 rather than R2, we can accommodate these intuitions. On R1, the concept of the gene is functional; accordingly, conceptual changes are less marked. If we limit ourselves to a *purely* functionalist conception of the gene, we can shift the specific problems raised by differing views of the material of heredity away from the gene concept. R1 thus allows for continuity of meaning and reference where R2 does not. But functional concepts of the gene still have considerable descriptive content, and R1 cannot wholly eliminate the threat of incommensurability and discontinuity. For example, we already mentioned Goldschmidt’s functional concept of the gene as a unit of heredity. He required genes to be units of transmission that control development. In doing so, he built on a long tradition. Most nineteenth-century treatments of heredity—including those of Weismann and Darwin—coupled the concepts of transmission and development. Nonetheless, early in the twentieth century, genetics dropped the conceptual link between development and heredity. As Jane Maienschein, Jan Sapp, and others have convincingly argued, the development of genetics in the U.S. depended critically on restricting the study of heredity to the study of transmission and excluding a wide array of developmental phenomena.40 Indeed, the elimination of the requirement of explaining developmental functions directly made it possible to construct an empirically useful gene concept (given the available techniques and the need to study heredity primarily by means of breeding and cytology) and to develop practices appropriate to that more restricted concept.41 As this vignette shows, the empirical content of concepts developed (or analyzed) in accord with R1, even though it is smaller than that of concepts based on R2, is more than sufficient to generate incommensurable concepts of the gene.

It might, therefore, seem promising to shift to a wholly non-descriptive theory, purging the concept of the gene of all descriptive content whatsoever. This is the moral suggested by Philip Kitcher in discussing a variety of cases from the history of science.


41The complexities of development being what they are, it is not correct to ascribe control of development to single genes. (Strictly speaking, classical genetics ascribes a function to a gene only when differences in the gene (or its absence) result in differences in the phenotype.) Partly because of ‘position effects’ (in which the same genetic material yielded different effects according to its position in the genome) and partly on the grounds that the whole of the genetic material is involved in control of development, so that (for developmental functions) it could not be separated into functionally discrete units, Goldschmidt was, notoriously, skeptical of the reality of particulate genes. For two examples, see ‘The Theory of the Gene’, *Scientific Monthly* 46 (1938), 268–273, and ‘The Theory of the Gene: Chromosomes and Genes’, *Cold Spring Harbor Symposia in Quantitative Biology* 16 (1951), 1–11. As should be immediately clear, Goldschmidt’s concept of the gene would qualify, on many accounts, as incommensurable with most gene concepts of his time.
Kitcher's position deserves a much more careful and detailed treatment than we can provide here, but the general approach is straightforward. Kitcher thinks that there are a variety of modes of reference associated with expressions. He calls this 'compendium' of the modes of reference for a term the 'reference potential' of the term. In some cases reference will follow descriptive content, and we will rest content to refer to whatever it is that satisfies some description. In other cases this is not so, and reference can be to entities that are decidedly different from the way we believe them to be or the way we would describe them. Applying this account to the history of genetics, Kitcher concludes that, 'from the beginning, geneticists were talking about chromosomal segments', even if they were skeptical about the reality of genes or doubted that they were chromosomal structures. The result is a solution to the problems posed by Kuhn and Feyerabend over incommensurability. Kitcher's idea is that all we need is referential continuity to insure theoretical continuity and comparability, and that the referent for (some tokens of the term) 'gene' is whatever it is that 'figures in an appropriate way in a correct explanation of the production of the [linguistic] token'. For our purposes, it does not matter whether we follow Kitcher in suggesting that non-descriptive modes of reference are all that is needed to secure referential continuity and prevent incommensurability; a causal—or 'historical' or 'externalist', theory of meaning could secure the same result by purging meaning of all descriptive content and then letting meaning determine reference. In either case, one would strip the account of the gene of all descriptive content, leaving even the functional properties of the gene conceptually open.

The strength of a non-descriptive approach—whether for reference, or meaning, or both—is that it establishes continuity by abstracting altogether from the content of belief, by removing all descriptive content from concepts and from the determination of reference. It therefore eliminates problems with comparability and commensurability. The price this exacts is enormous: genes become the Ding an sich responsible for the phenomena of trait transmission. Perhaps even this offers too much descriptive content, since trait transmission too may not be definitive of genes. Genes become simply Ding an sich. Their status as particulate or not becomes conceptually indeterminate, as does their connection with cellular constituents, or even trait transmission. Such descriptively empty concepts cannot readily be connected with

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44'Genes', p. 344.
chromosomes, protein, DNA, or RNA. Thus they cannot be used to reconstruct the actual debates in the history of genetics.

Setting aside wholly non-descriptive theories, then, we return to the differences between R1 and R2. Is there any reason, based on the history of genetics, to prefer analyses of meaning based on one of these schemata over the other? According to R1, the ascription of a particular structure or material composition to genes is an empirical matter and according to R2 it is not. But this difference need not involve any disagreement over facts or over the phenomena needing to be explained.46 A commitment to a particular definition does not require one to deny, a priori, any specific empirical result. Defining genes in terms of chromosomal regions exhibiting a ‘coordinated relationship’ (as Wallace for example does), does not require one to deny that there are important regulatory elements controlling gene expression. These simply will not count as genes, but as parts of genes, when they occur within a functionally coordinated unit of DNA. Similarly, defining genes in terms of DNA does not require one to deny the importance of RNA in retroviruses, or even that RNA can serve as hereditary material. There are two types of hereditary material, after all, and given significant enough differences in function we might choose, indeed some have chosen, to reserve the term ‘gene’ for the DNA. The usefulness of definitions truly does depend on purpose. For some purposes it is useful to choose a more restrictive definition and for others to adopt a more liberal one. Such choices affect how elegantly issues can be discussed and conveyed, but they need not affect the range of facts that can be captured.

R1 and R2 thus provide alternative schemata for expressing the same facts, differing primarily in regard to the scope, power, and proper application of the gene concept. A discovery concerning the hereditary material which expands our knowledge concerning the structure or composition of genes will count as empirical according to R1, yet, according to R2, it will count as a discovery that we have mischaracterized genes, or, perhaps, that some other sorts of entities also perform functions that are performed by genes. Yet these are not disagreements over facts or over the historical record; they are differences over how to interpret that record and the actual (agreed on) findings. R1 and R2 can yield equivalent descriptions covering the same empirical data and encompassing the same historical changes, though they will disagree over how to partition factual and conceptual components of those changes. In this respect, it is a matter of indifference whether the discovery that DNA rather than protein is the genetic material counts as a conceptual change or not. We can describe the associated changes as changes in concepts or in beliefs. Under R1 it becomes a change in belief, and under R2 a change in meaning. In either case, we

46In spite of factual agreement, there remains disagreement over the descriptive apparatus, the terminology, and the allocation of empirical vs conceptual components of descriptions. The parallel point was made long ago by Poincaré regarding the use of coordinating definitions in physical geometry. A given line (or surface) can be described as curved or straight (planar) depending on one’s assignment of coordinates and distances—and there need be no empirical disagreement. See, for example, Science and Hypothesis, translated by J. Larmor (New York: Dover Reprints, 1952), chs 3–4.
must accommodate the same empirical issues, and recognize the same sweeping changes in our theories. Likewise, it is a matter of historical indifference whether the shift to a Mendelian conception of heredity is interpreted as an empirical or conceptual change. In this case too, we confront the same empirical issues, and are confronted with changes in our theories. From this perspective alone, there is no basis for a definitive or principled choice between R1 and R2.

This moral, at least, is general. It is not tied to scientific case histories. For example, consider the scholarly disputes concerning the authorship of Shakespearian drama. J. T. Looney argued that the author of the Shakespearian plays was Edward de Vere, the 17th Earl of Oxford.\textsuperscript{47} The Earl was a patron of the theater who produced some plays under his own name in his early years. The explanation offered for his using a different name was the social stigma attendant to the stage at the time. We may suppose the Earl even intended the public to believe that an actor did the writing; it was not a simple pseudonym. In any case, the suggestion is that he elected to use the name of an obscure actor, presumably foregoing credit for the plays altogether. Such theses are described, indifferently, as the view that Shakespeare did not write the plays; as the view that the Earl of Oxford used the name ‘Shakespeare’; or, again, that the Earl of Oxford was Shakespeare. The first of these—that Shakespeare did not author the Shakespearian works—is of course what a causal theory demands, for, however it is framed, the import is that the man baptized in the parish church at Stratford-on-Avon in 1564 is not the author of the works customarily attributed to him. The second and third formulations are in the spirit of descriptive theories: the third says that the Earl of Oxford and Shakespeare are the same person, and the second that ‘Shakespeare’ is a name for the Earl of Oxford. Again, we have alternative views, all of which adequately describe the facts; all of which divide facts, concepts, and referents differently; and any of which can be readily adopted. Even Shakespearian cryptographers have made nothing of such a distinction—and we certainly hope our comments will not revive the disputes, since we think there is no significant distinction here needing to be made. Whether we say that our beliefs about Shakespeare are predominantly erroneous, though they are true of another man, or that our beliefs about Shakespeare are predominantly correct, though Shakespeare was not baptized ‘Shakespeare’, the facts, whatever they are, remain unchanged. As Wittgenstein said in a similar context, ‘Say what you will so long as it does not hinder you from seeing what is the case’.\textsuperscript{48}

The same moral applies to the choice between descriptivist and causal theories. Causal theories allow for relatively greater continuity of meaning and reference than

\textsuperscript{47} "Shakespeare" Identified in Edward de Vere, the Seventeenth Earl of Oxford; and the Poems of Edward de Vere (Port Washington, NY: Kennikat Press, 1975).

\textsuperscript{48} Philosophical Investigations, translated by G. E. M. Anscombe, 3rd Edn (New York: Macmillan, 1958), Section 79. We are fully aware of the parallels of this case to the fictional use of Aristotle by Kripke in ‘Naming and Necessity’. 
descriptive ones by minimizing the role of descriptive content, or meaning, in determining reference. Purely causal theories will attempt to eliminate descriptive content altogether; but here, too, the choice is imposed by the philosophical program. We have shown that the same changes can be described, according to the purposes of our investigation, as differences in beliefs, as differences in concepts, or as some mixture of the two. More generally, given reasonable alternative theories of meaning or reference, we can still capture all of the differences between Weismann and Muller, or between Morgan and Futuyma. If there are reasons for preferring one approach over the other, it will not lie in the ability of that approach to accommodate the historical development of genetics. The best way to describe the behavior of concepts becomes clear only after a reconstruction of scientific debates, and depends on the purpose of the reconstruction. Therefore, sweeping philosophical theories that seek a uniform meaning for all the relevant uses of the corresponding term must look for justification not from the history, or from the science, but from the purposes they serve. As Kitcher recognized, we are faced with heterogeneity in the reference of different uses of the same term. Since alternative reconstructions tend to favor different philosophical views, reconstructed history provides evidence that is, at best, suspect. For all these reasons, we believe it is a mistake to presuppose that there is a uniquely determinate and general concept of the gene (or of meaning), to be read off the history. They are, instead, read into it.

Speaking directly to the case of the gene, one might assume—once again, incorrectly—that if one description accords better than another with the ways in which geneticists formulated their theories, then the testimony of the language users provides a yardstick by which to judge theories of meaning and reference. That is, one might assume that geneticists' usage will determine whether a gene concept along the lines of R1 or R2 is to be preferred. But this move is unsatisfactory. First, as we have already indicated, scientists typically use ambiguous formulations, which makes the yardstick a poor one at best. Second, when scientific usage is unambiguous, it sometimes fits well with a functionalist view and sometimes with a structural theory. Most fundamentally, though, the best reconstruction will depend on local issues at stake within the community, on the practices shaping those issues, on what is taken as settled and what is not. The usage and opinions of individuals will provide at best a dimension of the total picture.

If views according to which gene concepts were incommensurable are factually equivalent to alternative accounts on which they are commensurable, the sting is removed from the debates over commensurability. Those analysts who wish to maximize the power of the commitments of a theory or a scientist, will employ extremely inclusive reconstructions of the relevant concepts—at the price of making comparison with alternative theories difficult. Those analysts who wish to maximize meaning invariance and commensurability, will employ 'minimal' reconstructions of the relevant concepts—at the price of reducing the content of the theories. Similar flexibility pertains to scientists. For example, those scientists seeking to bridge
disciplines or theories with conflicting commitments, will retreat toward minimal conceptual content in their theoretical concepts. On the other hand, those seeking to enhance the problem-solving power of the theories will move toward greater conceptual power. The key point is that it is possible to manipulate the degree to which a relevant concept is theory-laden, and the specific content with which it is loaded. All of this yields a strong result: meaning invariance and commensurability in genetics (and, we maintain, in science in general) depend on choices regarding the meaning of terms, both within the context of ongoing science and in that of philosophical analysis of science. Accordingly, we find neither continuity nor discontinuity, neither commensurability nor incommensurability, within a discipline or its development. The dichotomies are artifacts of our own making.

VI

We have concentrated on scientific concepts, and their changing uses. We have urged that concepts such as that of the gene can be reconstructed in various ways, depending on a variety of local contingencies; further we indicated that this should have important consequences for theorizing in philosophy. Philosophical concepts are no more stable than scientific ones. Though we have used terms such as ‘meaning’ and ‘reference’ throughout as if these concepts, unlike that of a gene, are invariant across contexts, in fact we do not believe that philosophical concepts are any different. All the issues we brought out in our characterization of genetics carry over into philosophy. We have seen that different reconstructions of episodes in the history of science and of philosophy appear to fit different theories of meaning. The purposes we have in analyzing a concept and its context determine which reconstructions are adequate. Once we opt for a particular reconstruction, we use philosophical tools that fit the reconstruction. Considering only the concept of meaning, we will briefly indicate why one overarching concept will not do.

Let’s grant that the history of genetics does not allow a unique, determinate, and general partition between the conceptual and the empirical and, thus, that the status of such statements as ‘Genes are segments of chromosomes’ or ‘Genes are sequences of nucleic acids’ is ambiguous. There have been times when it was a matter of empirical dispute whether genes were segments of chromosomes and whether they were sequences of nucleic acids. At later times, these things were taken as conceptual givens. What concept of meaning allows a faithful description of these changes and of the changing epistemic status of the claims? What concept of reference reflects the shifting status of the gene and tracks the shifting content of the concept of the gene? The conceptual content reflects the scientific context, for which reason it is not stable. From the point of view of genetics alone, there is no single concept of the gene. Disambiguation comes only with reconstruction. After that we are able in principle to apply various concepts of meaning to the episode under scrutiny. The nature and
purposes of the reconstruction will determine which concept of meaning or reference is the most appropriate.

It is still possible to hope that some uniformity, some continuity, can be extracted amid the changing conceptual flux through the promise of pragmatic ascent.\(^4^9\) Since the instability of the scientific concepts is a consequence, in part, of the changing empirical and social context, it is natural to suppose that an adequate semantics must somehow account for contextual factors. Even if a structural concept of the gene conforms relatively well with what someone said or wrote, it may be that, given the context, a functional concept accords better with what 'gene' meant at the time. Here, again, we feel the draw of a general theory of meaning, applicable across changing contexts. We propose to resist it.

Although pragmatic concerns, as such, did not enter explicitly into our discussion, it is obvious that one could follow a strategy which embraces a theory of meaning and reference in some pragmatic form. The idea is simply that reference will be a function of the expressions and the context and that the semantic assignment for an expression will depend, correspondingly, on context. This has the virtue of keeping the semantic theory proper simple, even if the actual assignments to particular expressions are highly variable.\(^5^0\) Thus, though the meaning of, say, 'gene' changes across time, the corresponding suggestion would be that there will be a pragmatically based theory, with context as one parameter, which yields appropriate semantic assignments.

The obvious problem is that we lack the pragmatics. Given the wide array of factors that are constitutive of the pragmatic context—including empirical, theoretical, and social factors—we are not at all sanguine about the prospects for developing such a theory. We may have a simple semantics, but we have no systematic account of the contribution of context. Even if we set this aside for the moment, however, the move does not differ centrally from the possibility of externalizing meaning: as we have said, it is tempting to think that an 'externalist' theory of meaning, or a causal theory of reference, might preserve the univocity of such concepts as that of the gene. In both externalist and pragmatic theories, the goal is to enforce, in a principled way, a distance between what some scientists might have thought or said explicitly, and what, in the context, properly was meant. On this suggestion, the meaning of an expression would be determined by a function \(F\) for that expression \(e\) in \(L\) and the context \(C\). Schematically, if \(\mu_e\) is the semantic assignment for \(e\), then

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\mu_e = F (e, L, C).
\]

\(^4^9\)There are clear parallels, which we cannot explore here, between this problem and problems with ambiguity, which affect the attempt to give a truth conditional semantics for natural language in the Davidsonian style. But in this context it is especially worth looking at K. Parsons, 'Ambiguity and the Truth Definition', \(Noûs\) 7 (1973) 379–394; Parsons is concerned with ambiguity within a language at a time, we are concerned, in addition, with ambiguity across time.

\(^5^0\)William Lycan adopts an analogous maneuver in dealing with the context dependence of indexicals. See his \textit{Logical Form in Natural Language} (Cambridge, MA: MIT Press/Bradford Books, 1986), p. 53. Lycan would most likely be contemptuous of our loose talk of meaning; see \textit{ibid.}, p. 272.
(It is easy to see how this suggestion could be generalized within a Tarskian theory, relativizing the truth predicate to context as well as language.) The meaning assigned changes partly as a function of context. Stability is attained only at the more abstract level of a pragmatic theory.

Confronted with the question whether there is any reason to prefer a theory that thus offers semantic assignments for ‘gene’ that are relatively detailed but vary with context, as opposed to embracing a theory according to which the meaning of ‘gene’ is stable with respect to context but relatively abstract, we recognize no principled reason to prefer one over the other. The functional (R1) and functional cum structural (R2) construals considered in Part IV in fact provide us with two such alternatives. We see no principled reason to prefer R1 to R2 as the schema for constructing the concept of the gene, and see no principled reason to recommend pragmatic ascent as opposed to semantic dissent. Whether we adopt a pragmatic theory itself may be a pragmatic matter.

Of course, there are ‘deeper’ philosophical reasons, tied to particular philosophical concerns and projects, why philosophers accept or reject theories of meaning and reference, of commensurability and incommensurability. These do not affect the moral that we believe should be drawn from cases like those we examine here. As our argument has shown, the history of science (and of the uses of terms) cannot decisively favor one theory of meaning over another. ‘Evidence’ drawn from the history of science for or against a philosophical conclusion about meaning is highly sensitive to the way one reconstructs the science.51 One reconstruction will favor a richer conception of ‘content’, but another will favor a more meager one. One reconstruction of a given episode will favor R1, while an alternative reconstruction will favor R2. The historical record alone can provide no univocal answer as to what constitutes the concept of a gene (or the concept of meaning). Theses about change of meaning and reference are relative to the interpretation one favors; this is as true of the concept of meaning as it is of the concept of the gene. The choice of such an interpretation may—and should—depend on one’s philosophical and scientific purposes.

Although the locus of issues about the concepts of meaning and reference is different than the locus of issues about the concept of the gene, we are unable to uncover any argument why the concepts of meaning and reference should behave in strikingly different ways than the concept of the gene. That is, like scientific concepts, philosophical concepts (including those of meaning and reference) require local

analyses and are best employed in different versions for different purposes. Philosophers who have sought general concepts and theories of meaning have (thus far) failed to come up with concepts that enjoy widespread consensus. Our approach suggests a reason of principle for this fact: a global theory of meaning is a will-o' the-wisp. Instead, we need a plurality of concepts for different purposes, and we must abandon the hope of finding a single concept of meaning or reference that will serve all purposes.

VII

Thus far, we have conformed to the prevailing climate in the philosophy of science by taking a predominantly descriptive approach; the approach was at best weakly normative in the sense that it aimed at a better articulation of scientific parlance with the help of reconstruction. The prevailing descriptivist climate, however, stems, in part, from an overreaction to the normative excesses of logical positivism. At least sometimes, philosophy should provide the measure for science, and not vice versa. The spirit of the descriptive approach—broadly construed—is best captured by the idea that there are no privileged reconstructions of science. Indeed, the alternative reconstructions we presented appeared to be equivalent insofar as they are empirically indistinguishable in spite of differences in the dividing line between the conceptual and the empirical. However, the idea that there are no privileged reconstructions must be qualified. No reconstruction is privileged in a context-independent, global, way. In particular contexts, though, it is wrong to act as if anything goes. It is precisely this that warrants normative approaches. Whereas our previous concerns have focused on reconstructions that could be adequate in different contexts, we want now to turn to situations that suggest reform rather than reconstruction. We will briefly illustrate one way of 'going normative' using R1 and R2.

Suppose that R2—a structural description of genes—provides the best description of the way some researchers at some time, with some set of issues on the table, actually used terms and that they actually meant what they said and wrote. One might still argue that they sometimes did not choose the best conceptualization of their own work—either for their own purposes or for some further purpose. For example, suppose there are empirical issues concerning the structure of a transmitted factor that causes an organism to manifest a certain trait. Where not enough is known about the structure in question—or where new results cast doubt on previous beliefs about that structure—it is unwise to beg the empirical questions by embedding presuppositions about the relation of structure and function into the concepts employed or into experimental practices in the laboratory. 52 From this perspective, R1 conveys the sort of conceptualization that researchers should use (or should have used) for the purpose

of correlating structure and function, whatever their actual practice. R1 gives coherence to an historical episode by a perfectly sensible rational reconstruction.

One can make this point in another way. There are structural, functional, and mixed gene concepts. On the whole, mixed concepts are loaded with more theory than the others, and this can result in a confusion of matters of meaning with matters of fact. Given the possibility (emphasized throughout this paper) of manipulating the theoretical load borne by concepts, one can provide scientists with practical rules of thumb regarding the manipulation of that load. One such rule would be to reduce the conceptual burden of a concept when that will allow the empirical resolution of a seemingly conceptual dispute. Thus, where there are significant disagreements about the correlation of structure and function, or about the actual realization of genes, it is typically best to employ separate structural and functional concepts rather than mixed ones. This would decrease the chance of confusing facts and meanings and it would maximize the commonality of meanings and the commensurability of the claims made on opposing sides.

This does not mean we can simply settle for the more austere R1, uniformly minimizing conceptual load. There is a tradeoff between conceptual load and explanatory power. Reducing the conceptual load has the virtue of opening up the empirical arena, and increasing the ability to settle issues by empirical investigation. But if we strip the concept of the gene of empirical content, we minimize its explanatory power. At least sometimes, by imposing an additional conceptual load it is possible to sharpen issues by imposing greater constraints on the problem at hand. By increasing conceptual load and explanatory power, one removes certain empirical questions from the table and constrains the space of solutions to open questions. This may well turn out to be a virtue in particular cases, but the art of scientific investigation is, in part, the art of determining which cases of this sort are virtuous and which are a source of difficulty. What is clear, even from our minimal account of the relevant history, is that redescriptions of functions or structures have played a central role in the history of genetics. Such redescriptions made it possible, for example, to distinguish between genes that had not previously been recognized (or regarded) as distinct by switching from morphological to biochemical descriptions of phenotypes. This change allowed the separation of previously indistinguishable phenotypic functions and was crucial to the analysis of gene action and regulation. The provision of some sort of guidelines regarding the use of redescriptions of the relevant phenomena in revising the meaning of theoretical terms is an important task for methodologists and philosophers. One ought not, indeed, one cannot, remove all of the conceptual burden from concepts—for it is desirable that they have problem-solving and explanatory power. Nonetheless, there are clearly many contexts in which the prescription to avoid ‘mixed’ concepts provides a powerful clarificatory tool for

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53 For more discussion of the use of multiple constraints in scientific problem solving, see Bechtel and Richardson, Discovering Complexity, Part III.
resolving scientific disputes or for reconstructing what actually was accomplished
during such disputes.\textsuperscript{54} We consider the development of heuristic roles for the
manipulation of concepts to be a central task for the philosophy of science (or the
philosophy of the special sciences) in the coming decades.

For those who are skeptical concerning the desirability of a normative stance, we
provide a brief example from recent genetic work, with both theoretical and practical
implications.\textsuperscript{55} This case concerns the pervasive disputes concerning genetic versus
environmental determination of psychiatric disorders. Most of the disputes in question
are confused and misguided. From a methodological point of view, many of the
controversies can be dissolved by use of the elementary point that concepts such as
'genetic determination' and 'environmental determination' should be attached to
differences in features between organisms, not to features of individual organisms as
such. Many recent psychiatric journals and textbooks employ a commonly used
classification of etiological factors that may play a role in severe disorders. Two
overarching categories of factors are recognized: biological and psychosocial. An
analysis of details and the context shows that biological factors are assumed to be
internal to the organism, and ultimately genetic; hence, researchers who would
relegate environmental influences to a subordinate position often defend the idea that
psychiatric disorders are 'genetically determined'. Analogously, psychosocial factors
appear to be equated with external, environmental, ones. Hence the distinction
between biological factors and psychosocial factors, built upon separated practices,
is laden with assumptions that are easily overlooked under casual inspection.\textsuperscript{56} The
two dichotomies—biological versus psychosocial, and internal versus environmen-
tal—are in fact independent of one another. The result of amalgamating the two is
confusion. The researchers involved disregard the possibility that biological factors
in the environment could be implicated in the genesis of psychiatric disorders.
Research done outside mainstream biological psychiatry in fact suggests that this may
be so. Effects of diet provide one example. Because of this amalgamation of concepts
and presuppositions, we may be saddled with a grossly deficient view of psychiatric
disorders. In these circumstances, we maintain that a normative stance aiming to
unload the concepts is desirable. At this point we simply do not know what the results

\textsuperscript{54}Van der Steen has discussed a number of examples in which the strategy of unloading concepts in
this manner can be of immense value. See his 'Methodological Aspects of Migration in Fishes', 'Concepts
in Biology'; A Practical Philosophy for the Life Sciences; and 'Towards a Practicable Methodology for
Medicine'; see also Van der Steen and Sloep, 'Mere Generality is Not Enough'.

\textsuperscript{55}See Van der Steen, 'Towards a Practicable Methodology for Medicine'.

\textsuperscript{56}This is but a specific instance of a general point made frequently by Richard Lewontin. See, e.g. Inside
and Outside: Gene, Environment, and Organism (Worcester, MA: Clark University Press, 1994). See also
Richard Levins and Richard Lewontin, The Dialectical Biologist (Cambridge, MA: Harvard University
Press, 1985), especially chs 1–3, and R. C. Lewontin, Steven Rose and Leon J. Kamin, Not in our Genes
(New York: Pantheon Books, 1984), especially chs 7–8. As an anonymous referee has pointed out, Lewontin,
like many STS theorists, emphasizes practice more than we have in this article, which is focused
on concepts and theories. We concur with the referee regarding the importance of practice and the futility
of attempting to understand how scientists separate conceptual and empirical claims without focusing
closely on their practices.
of inquiry might yield, but insisting on the need to draw the relevant distinctions provides us with the conceptual tools to ask the pertinent question.

VIII

Our approach to meaning and reference applies with equal force to properly metascientific concepts, such as that of an explanation, theory, etc.57 Our suggestion may be brought out by concentrating on what some scientists, rather than philosophers, have to say about methodology. As early as 1966, Richard Levins pointed out that biological models and theories cannot simultaneously satisfy such diverse methodological criteria as generality, realism, and precision.58 He argued, convincingly, that the context—including especially one’s purposes in working with models and theories—determines which of the above-mentioned methodological criteria should have overriding importance. A theory used to calculate dosages of prescription drugs had better be precise. If precision of this sort can only be attained by use of highly specific rather than general theories, so be it. For other purposes, generality may be of far greater value than precision. As a consequence of the methodological divergences appropriate to the different uses of, and purposes governing, scientific work, scientific theories are a mixed lot. No single methodological characterization, no single set of methodological criteria, can cover them all.

This context dependence pertaining to scientific theories and methodologies should enforce a parallel context dependence in the philosophy of science. Context dependence undermines any search for ‘the’ nature of scientific theories or explanations, ‘the’ theory of meaning and reference, etc. Consistent with these strictures, we do not seek to replace the (now defunct?) ‘received view’ of theories with some other monolithic view—say, the semantic view. Neither do we have sympathy with those who advocate ‘cognitive’ as opposed to ‘social’ theories of scientific change, or vice versa. From our perspective, it is no wonder that such general positions are nearly always controversial and overdrawn, or that philosophers have always found counterexamples to them. What we reject is the common reaction to those counterexamples—namely, to search for a new and better theory that can handle all cases. Instead, we advocate trying to specify, by whatever means one can, the contexts in which, and the purposes for which, the theory under scrutiny is valid or useful and to devise further theories that apply outside those contexts. Many domains may require many models. Additionally, following Levins’ prescription, we advocate attempting to locate the overlap, the concordances and discordances, between neighboring models and theories as a means of specifying the loci requiring further work and the pressure points at which extant views come into conflict or need further

57See Van der Steen, A Practical Philosophy for the Life Sciences.
development and examination. For this purpose, many models may be better than a highly confirmed general theory even for a single domain.

As for theories, so also for scientific explanations. In population genetics there are explanations in which laws and lawlike statements play a crucial role. In other domains—for example, in molecular genetics—there is no obvious role for such laws.\(^59\) One may explain why certain organisms develop a certain feature while others do not by pointing to a salient genetic difference and tracing the detailed mechanisms leading from the genetic cause to the phenotypic effect. But these mechanisms may have little more that is lawlike to them than a Rube Goldberg device. In such a case, there may not be any general laws that deal with the sundry causal antecedents of the relevant trait, certainly not in practice, but perhaps not even in principle.\(^60\) Once again, the context—questions asked, issues at stake, purposes entertained—will determine which methodological features are desirable. So, too, for theories of meaning.

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\(^60\) In response to work by philosophers of history, Hempel responded in 'The Function of General Laws in History' (see n. 4) that there are laws necessary to historical explanation, though they may not be historical laws. Likewise, it is possible to maintain that development depends on laws, perhaps from physics or chemistry, though they are not specifically biological laws.