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## On Conceptual Change in Biology: The Case of the Gene\*

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The current situation in philosophy of science generally, and in philosophy of biology in particular, is most unsatisfactory. There are at least three general problems that many philosophers thought themselves near to solving twenty years ago, only to find that the anticipated solutions have come unglued. These are (1) the problem of characterizing and understanding the dynamics of conceptual change in science; (2) the problem of understanding the interrelationships among theories including particularly the reduction of one theory to another; and (3) the problem of scientific realism (i.e., the problem of how seriously to take the claims of theoretical science or, at least, of some theoretical scientists, to be describing the world literally--in terms of such theoretical entities as genes and protons, DNA molecules, and quarks). This general situation has significant effects on the philosophical study of particular sciences. In philosophy of biology, for example, although one finds a large number of elegant studies of particular topics, the sad fact is that there is no generally satisfactory large-scale synthesis in sight. We have no agreed-on foundation, no generally acceptable starting point from which to delimit and resolve the full range of theoretical problems of interest to scientists and philosophers regarding biology.

This chapter provides a preliminary report on a new approach to conceptual change, together with a sketch of its application to important biological subject matter. The approach offers some promise of providing satisfactory framework, compatible with scientific realism, for detailed, studies of particular scientific developments.

Before I sketch in some of the relevant philosophical background, it will be useful to indicate how various concepts of the gene will enter the discussion. It is now 84 years since Mendel's work was rediscovered. Within fifteen years of that event, say with the publication of *The Mechanism of Mendelian Heredity* by T. H. Morgan and his coworkers in 1915, the main elements of the classical theory of the gene were fairly well established. A nearly constant series of improvements and refinements in that theory grounded in good part in laborious but fascinating experimental work, resulted in considerable revision of that theory. Indeed, the accumulated changes run so deep that some have characterized this historical process as one in which Mendelian genetics was replaced by a series of improved successors which can be grouped under the label *transmission genetics*. (For one example, cf. Hull, 1974, chap. 1.) This extended process, both in its theoretical and its empirical aspects helped prepare the way for what is usually considered to be (to use the vogue label) a scientific revolution brought on by the

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advent of molecular genetics. For present purposes, we may mark that advent by the publication of the justly famous solution of the principal structure of DNA in Watson and Crick (1953).

It will not be possible to review the relevant history in this chapter-- the task is simply too large.<sup>1</sup> Instead, I will single out a couple of moments from the history of genetics to illustrate the character of conceptual change in that discipline and to support the claim that the conceptual changes examined fit well with the larger philosophical views put forward in this chapter. I will show that the approach which biologists took to the conceptual changes in question had a significant effect on their practice, a result which suggests that the proper handling of conceptual change ought to be of real concern to working scientists. Finally, I will suggest that a full-scale study of the concept of the gene is a singularly appropriate vehicle for working out a general account of conceptual change in science.

### **Continuity and Discontinuity in Scientific Theories**

A very crude description of the present impasse in the theory of conceptual change will suffice for present purposes.<sup>2</sup> In spite of an immense variety of refinements; there are two main views to be considered. One of these, associated with the names of Paul Feyerabend<sup>3</sup> and (perhaps mistakenly) Thomas Kuhn<sup>4</sup>, might be labeled the discontinuity view. It claims that there are, at least occasionally, genuine conceptual revolutions in science. Revolutions are to be understood in a radical way; when they occur, those scientists who are separated by a revolution in a given field end up working with concepts and theories that are mutually incommensurable. This is to say that the pre- and post-revolutionary theories and concepts do not share a common denominator, and so are not interdefinable in any useful way. Discontinuity theorists usually add that the recording of scientific observations requires some sort of conceptual apparatus and that the concepts involved in gathering and reporting scientific observations are theory-laden in some way or other. With these additions, they argue, it follows that observations worked up to support or test one of the theories in question need not (and in difficult cases will not) serve in any direct way to support, test, or undermine the competing theory. Otherwise, the relevant observational concepts could be used to provide at least partial interdefinitions of the concepts drawn from the competing theories. But those theories were supposed, by hypothesis, to be incommensurable.

This result is counterintuitive. At first glance, at least, it seems simply incorrect to claim that evidence gathered under Newtonian or Mendelian auspices must automatically be thrown out or recast if it is to have a bearing on quantum mechanics or on molecular genetics, respectively. Not surprisingly, such claims have been highly controversial, with some thinkers supporting them and others arguing that they reduce discontinuity views to absurdity. Indeed, the best-known theorist of scientific revolutions, Thomas Kuhn, has backpedaled a long way from this sort of reading of his work in an attempt to avoid the "absurdities" that result from extreme interpretations of incommensurability. (Cf. Kuhn, 1983b.) But such extreme interpretations are not easily avoided. The difficulty is that people (including Kuhn, who, as will be shown in n. 6,

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<sup>1</sup> The best source for this history is Carlson (1966), which in part inspired this essay. Cf. also Whitehouse (1965) for a complementary approach.

<sup>2</sup> A useful elementary survey of the background is Brown (1977); a more advanced and detailed survey is Suppe (1977).

<sup>3</sup> Cf. Feyerabend (1962, 1965, 1970, 1975).

<sup>4</sup> Cf. Kuhn (1962, 1970, 1977).

even now cannot escape the central difficulty) arrived at discontinuity theories honestly, by means of a series of persuasive arguments which have not been adequately answered.

Discontinuity theories arise from a set of seemingly commonsensical commitments regarding the nature of scientific language, concepts, and theories. Among these is the notion that a scientific theory is adequately represented as an interconnected body of statements about some domain of phenomena, and that this body of statements typically employs characteristic theoretical concepts. Theoretical concepts, in turn, cannot be properly understood in abstraction from the relevant theory. While it may, arguably, be true that one need not employ the whole of a theory to understand or delimit the relevant concepts, one does presuppose at least some central core of the theory in question when utilizing those concepts. Thus, to measure the mass of a body, one must presuppose at least Newton's second law ( $F = ma$ ), and probably the third law as well. And for 'mass' to refer, i.e., for bodies to genuinely have mass, bodies must in fact behave in accordance with Newton's laws; in short, the fundamental laws of Newtonian mechanics must be true if their theoretical terms are to refer to objects or properties in the world.<sup>5</sup> Similarly, to determine whether an organism is heterozygous for a recessive Mendelian gene, say a gene for albinism, a classical geneticist must presuppose various theoretical principles. One of these principles claims, for example, that, other things being equal, an organism that has a recessive gene in double dose will, in the right circumstances, exhibit the trait (in our example, albinism) in terms of which the gene is identified. Obviously, on any reasonable account of the structure of classical genetics, further principles concerning the transmission of genes from parents to offspring must also be presupposed in setting forth the workings of heredity. And organisms will possess genes only if the relevant Mendelian laws are true (or, perhaps, only if some fundamental subset of those laws is true).

These considerations show that a holistic account of theoretical language has important consequences. According to a holistic account, the proper employment of a theoretical concept requires one to mobilize some fairly large entity--an entire theory or paradigm or conceptual scheme or conceptual framework or theoretical language or world view, to borrow some of the terms current in the literature. Such holism brings radical incommensurability with it. What has led (or sometimes forced) people to swallow discontinuity theories has been their acceptance of some form of holism about theoretical concepts. And they have been forced to this holism, in turn, by recourse to wholly inadequate semantic theories for theoretical languages.

The reason for claiming that Kuhn, like the rest of us, has not solved this tangle of problems is that he has not shown how to integrate his account of conceptual change into a nonholistic theory of theoretical language. What is required is some way to spring concepts like 'mass' and 'gene' sufficiently loose from the fundamental principles of the theories with which they are allied that it becomes apparent how one could seriously use those terms as a scientist without presupposing the truth of the corresponding principles. Only then will we be able to make sense of debates between theorists over which of their theories, built on incompatible principles, is a (or the) correct theory of mass or of the gene. It is not enough to wish to reject extreme versions of incommensurability; until philosophers show how to get around holistic

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<sup>5</sup> Kuhn's original articulation of these ideas (1962, pp. 101-102) has been altered in various ways, but as his 1983a (pp. 566-567) shows, he still accepts the central claim that if the fundamental laws or postulates of a theory are not true, the leading theoretical terms of that theory do not refer. See the following note for an elaboration of this point.

theories of the language of theoretical science, they will be vulnerable to arguments forcing them into acceptance of radical incommensurability.<sup>6</sup>

For the sake of completeness, I shall deal briefly with the second class of theories of conceptual change in science, namely continuity theories. At the moment, most extant continuity theories are discredited, for they are committed to the idea that some core of scientific concepts (for example, so-called observational concepts) provides a permanent (though perhaps expandable) base for the spinning out of new theoretical concepts and theories. One reason for the disrepute into which continuity theories have fallen is their close connection to traditional philosophical accounts of the reduction of one theory to another. These accounts, alas, are a total failure. They claim that when a theory (say Mendelian genetics) is reduced to (or by) another (say molecular genetics), one can deduce the central claims of the theory which is reduced from those of the new, more fundamental theory.<sup>7</sup> The deduction uses the principles of the fundamental theory together with suitable definitions and statements connecting the concepts of the two theories, and descriptions of the circumstances in which the reduced theory obtains. Were this account of reduction correct, the concepts of the reduced theory would be, in effect, definable within the more fundamental theory and the claims of the reduced theory would be a subclass of the claims of the fundamental theory. In fact, as is now generally recognized, reduction of this sort virtually never occurs in science. One cannot even deduce Kepler's laws for our solar system from Newtonian mechanics. Given that there is more than one planet, what one deduces is the claim that the orbital formulae obtained from Kepler's laws are approximately correct though literally false; the calculable perturbations of Mars from its supposed elliptical orbit, for example, were observable within the limits of accuracy with which Kepler was working. Worse yet, when one comes to cases like statistical versus phenomenological thermodynamics or, as we shall see, Mendelian versus molecular genetics, discontinuity theorists have put forth quite convincing arguments to show that the concepts of the theory to be reduced simply cannot be reproduced within the successor theory. (Hull, 1974, chap. 1.) This claim, as I shall argue, seems to be entirely in accord with the facts.

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<sup>6</sup> Kuhn's latest attempt to escape the consequences of holism turns on restricting the interconnections among terms and concepts to a local context--hence his term local holism. Yet, as the following quotation shows, Kuhn has serious trouble in accounting for disagreements over the reference of theoretical terms: "Nevertheless, I take the Second Law to be necessary in the following languagerelative sense: if the law fails, the Newtonian terms in its statement are shown not to refer." (Kuhn, 1983a, p. 567.)

If this text is taken literally, Kuhn cannot properly parse a debate between an Einsteinian and a Newtonian physicist in which each individual argues that the theory he prefers gives a proper description of what, allegedly, they both refer to by the term 'mass'. This difficulty is a consequence of Kuhn's claim that if the Einsteinian term 'mass' is well-grounded, then the phonetically and lexicographically identical Newtonian term cannot refer at all. It follows that one cannot coherently argue that what is referred to by the Newtonian term is properly described within Einsteinian mechanics, for, while accepting Einstein's mechanics, one cannot coherently hold that the Newtonian term refers at all. Yet such disagreements are absolutely commonplace and give no sign of incoherence. What has gone wrong is the mistaken linkage of reference to meaning. Even if holism is correct about the meanings of theoretical terms (as it surely is), such holism is mistaken when applied to reference. This point undercuts all standard treatments of incommensurability, including the weakened versions which Kuhn currently advocates while seeking to escape the consequences of radical versions of incommensurability.

<sup>7</sup> Cf., e.g., Nagel (1961). The extended debate among Hull (1974, 1976), Ruse (1976), Schaffner (1969, 1976), Wimsatt (1976, 1979), and others shows the sorts of difficulties which this approach encounters.

Over and above the connection between continuity theories and discredited theories of reduction, there is fairly broad consensus that they are not true to the facts. There simply is no timeless and secure set of observational concepts, let alone non-observational concepts, strong enough to be employed as the base for building up the concepts of theoretical science.

How do all these abstract considerations bear on the workings of real science? What on earth is to be accomplished by dismissing both continuity and discontinuity theories of conceptual change? For those who wish to surmount difficulties, not just to find them, the next steps may provide some relief. I shall argue that the standard theories of conceptual change are vitiated by a mistaken presupposition that presents their having real contact with live science. An examination of the concept--or rather the concepts--of the genes provides an ideal vehicle for bringing about a new start on a much more satisfactory spotting.

### **The Gene: Development of a Concept**

This is time to make a first pass at the various concepts of the gene. The question that we will pursue is this: to what, if anything, does a scientist refer when he uses the term 'gene' or one of its cognates? For present purposes this question will provide enough of a guiding thread to lead us through what will someday have to be turned into an immensely complex discussion.

Honesty compels me to remind the reader how complex a full treatment of the history of the gene will have to be. A single sentence from Carlson (1966) makes the point:

The gene has been considered to be an undefined unit, a unit-character, unit-factor, a factor, an abstract point on a recombination map, a three-dimensional segment of an anaphase chromosome, a linear segment of an interphase chromosome, a sac of genome's, a series of near sub-genes, a spherical unit defined by target theory, a dynamic functional quantity of one specific unit, a pseudoallele, a specific chromosome segment subject to position effect, a rearrangement within a continuous chromosome molecule, a cistron within which fine structure be demonstrated, and a linear segment of nucleic acid specifying structural or regulatory product. (p. 259)

To cut through all this complexity, I shall make four fundamental points by use of one rather simple example. The first point is that it is possible for scientists to exercise strong controls to ensure that they are referring to the same entity or entities in spite of very large differences in viewpoint, terminology, concepts, and theoretical commitments. The episode in terms of which I will make this point concerns William Bateson, one of the founding fathers of genetics--the one, in fact, who coined the very term *genetics*.<sup>8</sup> Bateson never was fully persuaded that genes could be localized on chromosomes or that they could be mere molecules or material particles. (Because of the dynamics required for them to achieve their effects, he thought that they would have to be stable harmonic resonance or something of the sort.) He usually employed the term 'factor' rather than the later coinage 'gene', and he used 'character' or 'unit character' for traits which he counted as the effects of the presence, absence, or, rarely, alteration of a single factor. He preferred to talk of 'discontinuous variation' rather than 'mutation' and he distinguished sharply between such discontinuous variations (which he thought were the real stuff of

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<sup>8</sup> Useful secondary sources on Bateson include Cock (1983), Coleman (1970), and Darden (1977). For the invention of the term genetics, see Carlson (1966, pp. 15-16).

evolution) and the small continuous variations, which he took to be the target of Darwinian natural selection.

Bateson (1916) is a review of the Morgan group's definitive book, *The Mechanism of Mendelian Heredity*, published by Science.<sup>9</sup> Now Bateson and Punnett had discovered linkage between genes (which they called 'gametic coupling') in 1906. Ironically, linkage (which, by the way, requires violation of Mendel's law of independent assortment) served as one of the cornerstones for the Morgan group's arguments that chromosomes are the bearers of the hereditary material and the relative locations of the factors or genes may be pinpointed quite precisely by means of maps based on the degree of linkage between them. The point about the Bateson review is very simple: in spite of all the differences between his views and those of Morgan and his colleagues, there simply is no difficulty about the reference of the relevant terms. Without explicit consideration, Bateson considers it to be established that certain traits (e.g., certain eye color traits in *Drosophila* which had been shown in experimentally controlled mating to be transmitted to offspring in well-established particulate patterns) are indeed the consequence of the presence (or perhaps, sometimes, the absence) of a particular factor. The main disagreements in question concern not whether there are factors or genes acting in such cases, but rather what kinds of things these factors are and how strongly the available evidence supports the view that they are material particles or some such) located at the relative positions on the chromosomes worked out by the Morgan group's mapping techniques. In the much later operationalist terminology of L. J. Stadler (1954), Bateson has retreated to an operational definition of the gene, which he shares with Morgan's group; his disagreement with them concerns the best theoretical account to offer of the behavior of the operationally defined gene. In Stadler's terminology, this disagreement concerns the hypothetical gene. (I will return to Stadler's distinctions briefly at the end of this chapter.)

The second point to be drawn from this episode is that the procedures involved cannot be confined to Bateson and Morgan's groups; they are already the firmly established property of a larger community. It is, of course, true that the procedures by which factors (or genes) are identified and individuated can be refined and improved in ways which may shift the reference of particular terms; such refinements can change altogether the set of entities to which a community refers by use of such terms as 'gene' and 'factor'. But the terms are, in a certain sense, community property. There are experts who are in a position to adjudicate questions regarding whether or not Morgan and his colleagues met the conditions required for them to be referring, at least *prima facie*, to particular genes and to determine whether or not any objections to their claim so to have referred are cogent or not. Bateson, himself one of those experts, knew that the Morgan group had unassailably demonstrated that they were dealing with single genes by the standards then available.

This point about the social character of the referential use of scientific and prescientific terms is rather stronger than it looks. As a concrete example which, in spirit, goes back to the work of Hilary Putnam (1975, pp. 223-229ff.) I am able to use the terms 'mole' and 'mole rat' to refer to distinct mammals in spite of the fact that if someone were to bring a few of each of them to me I could not tell which were the moles and which the mole rats. The reason that my lamentable ignorance does not prevent me from using these terms to refer correctly to distinct animals is that my usage is interlocked quite deliberately with that of a larger community, and particularly with that of experts, of whom I am prepared to defer, who can tell moles from mole

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<sup>9</sup> Cock (1983), who places considerably less emphasis than I do on Bateson's vibratory theory of heredity, has an extended discussion of Bateson's review of Morgan et al. at pp. 41ff.

rats. The matter is, of course, not so simple when it comes to questions on the forefront of knowledge; all of the experts may be (and have on certain issues been) wrong in their central beliefs about genes. Nonetheless, as the very example of the Bateson review demonstrates, when the social controls work correctly and the world cooperates (neither of which can be counted on), the experts' procedures allow them to secure reference or theoretical terms like 'factor' and 'gene' even in the face of considerable disagreement, even in the face of pervasive and fundamental error in the foundations of their theories.

The third point suggested by consideration of the Bateson review is that terms like those under discussion are used by the community in a way which does not require that their reference be fully specified. To put the point differently, the account we give of the reference of terms like 'gene' ought to allow radically different descriptions of the things which genes might turn out to be. We often secure reference for theoretical terms and concepts while using deeply mistaken theories. It is clear, I hope, that if the world were appropriately different and if the genes in question in the Bateson review were in fact stable harmonic resonance rather than segments of chromosomes, nothing would need to be changed in what Bateson or Morgan group wrote, but the reference of some of their terms would have been different. To use a term from Kitcher (1978, 1982), we must consider not only the reference, but the reference potential<sup>10</sup> of a term to understand how it is used. One can argue that, as of 1916, the terms used to refer to, say, the vermilion eye color gene of *Drosophila melanogaster* included stable harmonic resonance in their reference potential. Not long afterward, thanks to the Morgan school's success, that reference potential shifted; some genes, at least, had to be localized on chromosomes. Accounting for such changes is an important part of a satisfactory history of the concept the gene.

The fourth point is more or less a corollary of the third. It is sometimes possible to resolve disputes regarding the reference of theoretical terms in the light of subsequent investigation. No matter what Bateson thought he was referring to, when he spoke of the *Drosophila* genes, he was in fact referring to segments of *Drosophila* chromosomes. Sometimes, at least, the world is well enough behaved to allow a clean resolution of such disputes. At the same time, to gain a proper appreciation of Bateson's writings, we must not let the truth of the matter run away with us. Bateson's beliefs inevitably affected his terminology; in this he is typical of all working scientists. Sometimes he used the term 'factor' in the attempt to refer to the entities which his own undeveloped, theory, if correct, would have described. In such instances (since there are no such things) the term does not, in fact, refer at all. At other times we used the term 'factor' to refer to those entities, whatever they might turn out to be, which he, the Morgan group, and many others I singled out by their experiments. The unwary reader who fails to distinguish these different uses of the same term will be unable to evaluate Bateson's claims correctly.

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<sup>10</sup> My encounter with this notion in Kitcher (1978) was crucially formative; this chapter grew out of thinking through how to apply the apparatus developed there to the history of the concept of the gene. His development of this notion in his 1982, pp. 339-347, is particularly useful. I am also grateful to Kitcher for helpful discussions concerning our work in progress; the overlap of interest and approach is unusually strong.

### Theories of Reference and the Gene

Let us collect our results to date. From a philosophical point of view they concern the theory of reference and the proper analysis of theoretical concepts. I shall take each of these in turn.

At the moment there are two fairly standard theories of reference to cope with. The traditional theory,<sup>11</sup> whose roots lie in the work of Frege holds that kind terms like 'gene' refer, in the first instance, to any and all objects which fit the underlying description or descriptions with which the speaker is prepared to back up his or her use of the term. Those descriptions are said to spell out the sense of the term in question. According to the traditional theory, the reference of a kind term is fixed by its sense and by the world; a kind term refers to just those object which fit the description or descriptions implicit in the sense of the term.<sup>12</sup>

In the context of the present investigation, this traditional theory can be seen to be allied with the holistic theories of theoretical language rejected above. Its natural application to the Bateson case employs Bateson's mistaken and unarticulated theory of factors to determine what the sense of 'factor' is and thus, in turn, to fix what he referred to in his primary uses of that term. The consequence is that the traditional theory claims that, precisely because Bateson's theory of factors (or genes) was fundamentally mistaken, Bateson did not refer to anything when he used whichever term for genes. On at least some occasions, in contrast, the Morgan group did refer to certain chromosome segments by use of such terms as 'gene' and 'factor'. But this contrast is wrong and wrongheaded. One small symptom of its erroneousness is that, taken seriously, it does not allow one to construct a plausible construal of Bateson's review.<sup>13</sup>

Although there are a number of moves that one can make to try to save the traditional theory in application to this case, this is not the occasion to explore them. I shall simply assert, dogmatically, that the traditional theory is wrong and ask its defenders to present their arguments to the contrary.

The alternative theory, various versions of which have been developed by Donnellan, Kripke, Putnam,<sup>14</sup> and others, is sometimes known as the causal theory of reference. (The label is a misnomer, but the issues involved are not of immediate importance to the matters at hand.) At first sight, the causal theory fares rather better in dealing with our case. It holds that what a kind term like 'gene' refers to depends on the earlier uses back to which present uses can be traced. 'Gene' refers to whatever natural kind (assuming that there is one) in fact entered into the relevant causal interactions when Mendel performed his experiments on garden peas and baptized the factors which determined the patterns of inheritance which those peas exhibited. Thus since, in the tradition we are exploring, the term 'gene' can be traced back to Mendel's uses of the German words *Charakter*, *Element*, *Faktor*, and *Merkmal* as means of describing the determinants of particulate inheritance, the causal theory says that both Bateson and Morgan were referring to the determinants of particulate inheritance, whatever their true nature.

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<sup>11</sup> A useful brief account of this theory may be found in Schwarz (1977).

<sup>12</sup> The power of this position is nicely illustrated by Kuhn's tacit reliance on it in the argument criticized in n. 6. If it were not for the supposed unique connection between sense and reference, what ground would there be for holding that, given the way the world is, the reference of a theoretical term or concept is fully fixed by its place in the fundamental laws of that theory?

<sup>13</sup> The point is entirely parallel with the objection to Kuhn put forward in n.6.

<sup>14</sup> Cf., e.g., Donnellan (1966, 1970), Kripke (1972), and Putnam, (1973, 1975).

Prima facie this theory of reference can handle the Bateson review. As usually developed, however, like the Fregean theory it is a closed rather than an open theory of reference.<sup>15</sup> By this I mean that the reference of such kind terms as 'gene' is treated as being completely fixed once those terms are properly established within the tradition. This closure, I believe, is yet another reflection of a holistic theory of theoretical language. Indeed, the fundamental presupposition shared by continuity and discontinuity theories of conceptual change is just idea that the reference of theoretical concepts is closed. Closure of reference makes conceptual change into an all-or-nothing phenomenon.<sup>16</sup> When we turn to the history of genetics for the second time, we shall see that not only the reference potential but also the actual reference of the term 'gene' has changed in a controlled way during, the development of the discipline of genetics.

But before turning again to cases, a few words are needed about reference potential. Following Kitcher (1978, 1982), though with important modifications,<sup>17</sup> I maintain that reference potential provides the central tool by means of which to analyze theoretical concepts. In the Fregean tradition, the concept mobilized by the use of a term is equated with the sense of the term, that is, with the underlying descriptions which speakers would employ to back up their use of the term. The alternative account that I am about to sketch is designed to reflect precisely those characteristics of the referential use of theoretical kind terms which give the Fregean tradition the greatest difficulty. Specifically, we need to take account of the ways in which the practice of scientists makes three things likely: first, that their fundamental theoretical terms will pick out some natural kind even when their theories about that kind are badly mistaken; second, that different scientists will be able to refer to the same natural kinds even when their theories about those kinds are in radical disagreement; and, third, that the precise referential use of such central theoretical terms as 'gene' can, in good cases, be brought into line with experimental results and with the theoretical commitments of the relevant community once that community achieves full consensus on those commitments.

### Referring to Genes

The price that must be paid for theoretical discourse to have these characteristics is the referential openness of theoretical concepts and systematic ambiguity in the referential use of theoretical terms. Some will consider this price very high, but it has always been paid, for it unavoidable. The history of genetics nicely illustrates the openness and systematic referential ambiguity of theoretical terms as well as some of the ways in which these are controlled. (The phenomena involved, though, are found in all of the sciences.)

Given the success of the Morgan group, the term 'gene' typically refers, in fact, to a segment of a chromosome which, when activated or deactivated, performs a certain function or has a characteristic effect. But how much of a chromosome? And what functions or effects?

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<sup>15</sup> Kitcher makes a similar point in different terminology in his 1982, p. 345.

<sup>16</sup> Closure of reference with respect to theoretical terms is the fundamental move which undermines "local holism." So long as the fundamental laws of therelevant theory or the particular linguistic practices of a founding individual or community are thought to fix the reference of theoretical terms for once and for all, theory change which adjusts the reference of the resulting theoretical terms will be impossible, and referential change regarding theoretical entities will be an all or nothing phenomenon.

<sup>17</sup> Among the modifications: Kitcher seems to think that reference potential is an extensional concept. On my reading, it is not. Thus, he claims to be an extensionalist. I am not.

Much of the effort that went into mapping genes may be viewed as an attempt to answer the first question; much labor was expended on the determination of which part of which chromosome contained which genes. In the process, certain criteria were developed for telling one gene from another. According to one of these, if two mutations affecting the same phenotypic trait--say two eye color mutations--could be separated by recombination, then they belonged to separate genes; if they could not be so separated, then they belonged to the same gene. (That is, they were counted as alternative alleles at the same genetic locus.) This way of individuating genes was suggested by Sturtevant (1913a and 1913b), who suggested that two closely linked eye color mutations (called 'white' and 'eosin') that Morgan and Bridges had been unable to separate in an experiment using 150,000 flies (Morgan and Bridges, 1913) should be considered to be two alternative abnormal alleles at a single locus, the locus which had already been located in a specific position on the X chromosome. Now the more closely two genes are linked, the more difficult it is to separate them by recombination, and the larger the number of flies that must be used to execute the test. Thus it should be no surprise that such claims are sometimes wrong and that it was established many years later that, in this very case, one can separate the two genes in question if one performs a truly gigantic recombination experiment.<sup>18</sup> (Cf. Carlson, 1966, p. 64; Kitcher, 1982, p. 351.)

Consider the problem this creates when one asks what is referred to by subsequent uses of such terms as 'the gene for white eyes' or 'the eosin locus'. If one conforms to the usage established on the basis of Sturtevant's results, one refers to that portion of the chromosome which contains both the white and the eosin genes. But if one is working with the recombination criterion for theoretical purposes, one may refer, instead, to the smaller portion of the chromosome containing one, but not both of these genes. This is to say that two rather different segments of the chromosome belong to the reference potential of these rases. Very often it makes no difference which portion of the chromosome one refers to. (They are, after all, virtually inseparable by ordinary techniques.) But occasionally it may matter whether one purpose or the other--conformity to established usage in order to accomplish coreference with other scientists or correct application of the criteria separating genes from one another--dominates one's usage. For a long time, the ambiguity was inescapably built into the mode of reference which was available in discussing these genes.

Indeed, at various stages in the history of genetics, it became a theoretical and practical necessity to distinguish between different gene concepts each of which picked out different segments of the chromosome or employed different criteria of identity for genes--remember Carlson's list! For example, in the 1950s Seymour Benzer pointed out that many geneticists had assumed that the smallest unit of mutation with a distinct functional effect coincided with the smallest unit of recombination-- and he performed some elegant experiments which showed that this claim is false.<sup>19</sup> As a result, in some circumstances it became necessary to choose between the unit of function (which, for reasons which need not concern us, Benzer called the *cistron*), the unit of mutation (which he called the *muton*), and the unit of recombination (which he called the *recon*). This particular result showed that there had been hidden openness in the reference potential of the term (and the concept) 'gene' and that, in some arguments, though not in general,

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<sup>18</sup> Cf. Carlson (1966), chap. 8, for a discussion of the conceptual importance of Sturtevant's analysis which provided the key step in recognizing that mutation often involves alteration rather than loss of genes.

<sup>19</sup> Cf. Benzer (1955, 1956, 1957).

it was necessary to divide the reference of that term (concept) according to the separable modes of individuating genes.

The actual history is, of course, much richer than I have let on here, particularly when one pursues the story into the present, where one encounters split genes with separately movable subunits, transposable control elements, parasitic ("selfish") DNA, and so on. But enough has been said to show that there are at least four ways in which the reference of a particular use of the term 'gene', or one of its cognates, might be specified.<sup>20</sup> Which one of these is relevant will turn on the dominant intention of the scientist and the context of the discussion. One such intention is conformity to conventional usage. Taking Sturtevant's experimental result for granted, conformist usage would refer to the same segment of the X chromosome whether one spoke of the white or the eosin locus. Another, sometimes conflicting, intention is accuracy in the application of the extant criteria for identifying the relevant kinds or individuating the individuals of those kinds. When accuracy is the dominant intention, 'white' and 'eosin' refer to different segments of the chromosome. Thus Sturtevant's mistake expanded the reference potential of the term 'gene' by adding a compound chromosomal segment to the items potentially referred to by that term. In some, but only a few, contexts it provided terribly important to take the resultant long-recognized ambiguity of reference into account in order to understand the actual use of the relevant terms and to reconcile conflicts between competing descriptions of the outcomes of experiments. What is at stake here is the precise roles that one's theoretical presuppositions and accepted experimental results play in fixing the reference of one's terms. Although this discussion has not provided a general resolution of that difficult problem, it has given some indication of the proper apparatus to employ in carrying out case by case analyses.

The Benzer case illustrates a third way in which reference may be fixed: once an ambiguity (such as that between 'cistron' and 'recon') becomes troublesome, it is sometimes necessary to stipulate as clearly as possible which of the available options one is taking as a way of specifying the reference of one's terms. Even at the risk of total failure to refer--which might happen if one's analysis is mistaken--one fixes one's reference to all and only those things which fit a certain theoretical description. The result is clarity, and when clarity is the dominant intention, reference is fixed in much the way that Frege thought that it is always fixed. A sense is determined by a description, and reference depends on whether or not anything, in fact, fits that description. Finally, one may operate with a dominant intention which Kitcher calls naturalism, to wit, the intention to refer to the relevant effective natural kind occurring or operating in a certain situation or in a certain class of cases. Though the matter needs to be argued on another occasion, I suspect that one must have recourse to naturalism over and above conformity, accuracy, and clarity in order to put forth a successful account of the grounds on which Mendel, Bateson, Morgan, Benzer, and all the rest may be construed as employing concepts of the same thing--the gene.

### Concluding Remarks

To set up the conclusion of this chapter, it will be useful to make some very small comments about the relationship between Mendelian and molecular genetics. As the reference of the term 'gene' became more tightly specified during the development of Mendelian (or, if you prefer, transmission) genetics, in a large range of central cases the concept of the gene became

<sup>20</sup> Compare Kitcher's prior discussion in his 1982, pp. 342ff.

that of a minimal chromosomal segment performing a certain function or causing a certain effect. The relevant effect was known as the phenotype of the gene. Not surprisingly, a major part of the history of the gene, not addressed here, concerns the interplay between what one counts as genes and how one restricts or identifies the phenotypes which can be used to specify individual genes. But when all this is said and done, a great variety of phenotypes can legitimately be used to single out genes. As will soon be made clear, one's very concept of a gene depends on the range of phenotypes one considers.

Thanks to the advances made in molecular genetics, it is now possible to examine changes in the DNA (mutations!?! ) fairly directly. In some cases, at least, it is also possible to track the effects of those changes rather exactly. It is now well known that some changes in the DNA are silent. That is, they have no effect on any other aspect of the structure, the development, or the composition of the organism. Effectively, such changes in the genetic material do not amount to changes in the function of any gene, though, when suitably located, they do constitute changes in the structure or composition of the relevant gene. Other changes in the DNA do, of course, change the organism, but some of them do so in ways which, arguably, are of no importance to its structure, development, or function. For example, some so-called point mutations result in the substitution of one amino acid for another in some particular protein manufactured in accordance with the information contained on the gene in question. Many such substitutions have very drastic effects. But some of them, so far as can be told, do not alter the way the protein folds and do not alter its biological activity or function in any significant way. In such cases, I suggest, there are strong reasons for tolerating in perpetuity important ambiguities in the concept of the gene.

The reason for this is that phenotypes at different levels are of concern for different purposes. Consider, for example, medical genetics. If one is concerned with PKU and allied metabolic disorders, the phenotypes one deals with will range from gross morphological and behavioral traits down to what turns out to be the heart of the matter--enzyme structure and function.<sup>21</sup> With respect to all of these phenotypes, both silent changes in the DNA and those changes which have no effect on enzyme structure or function will not (and should not) count as mutations, i.e., as changes in the relevant gene. It does not matter whether or not these changes occur within that segment of DNA which constitutes the gene of interest; because they have no relevant functional effects, the gene counts as unchanged. The reason for this is clear: the concept of the gene is coordinate with the concept of the phenotype. And the phenotype of concern is not defined biochemically at the level of DNA, but (if it is defined biochemically at all) at the level of protein or via some functional attributes consequent on the biochemistry of the relevant proteins.

It is important to recognize that there are legitimately different interests which lead us to deal with different sorts of phenotypes. Evolutionists, for example, may be interested in the rate of amino acid substitutions in proteins or of nucleotide substitutions in DNA. That is, the phenotypes they are concerned with might be defined by amino acid or even nucleotide sequence, not protein function. Accordingly, their definitions of the phenotype and of the gene may be discordant with those of the medical geneticist. And it is not a matter of right or wrong, but simply a matter of legitimately different interests. The point is a fairly deep one, allied to Putnam's point, discussed briefly above, about the linguistic division of labor. I have written, until now, sloppily, as if there were only one community of biologists, or, rather, geneticists. But this is simply false. There are large and important specialized subcommunities with legitimately

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<sup>21</sup> There is a helpful discussion of PKU in Burian (1981-82, pp. 55-59).

different interests, interests which lead them to deal with legitimately different phenotypes. As the example introduced in this paragraph shows, there are serious cases in which there is no real question but that those differing phenotypes correspond with different concepts of the gene and different criteria for individuating genes.

Work in molecular genetics may well show that, like Bateson's, some contemporary attempts at establishing gene concepts are ill-founded. Indeed, I believe that there are clear cases (for example in sociobiology, cf. Burian, 1981-82, but perhaps much more generally) in which certain gene concepts will simply have to be abandoned in light of some of the findings of molecular genetics. But molecular genetics cannot discriminate among well-founded gene concepts. There is a fact of the matter about the structure of DNA, but there is no single fact of the matter about what the gene is. Even though their concepts are discordant, the community of evolutionists concerned with the evolution of protein sequence and the community of medical geneticists working on metabolic disorders are both employing perfectly legitimate concepts of the gene. This provides strong, concrete support for the claim that the concept of the gene is open rather than closed with respect to both its reference potential and its reference.

A dangling thread provides a moral for biologists to consider. Recall Stadler's distinction between the operational concept of the gene and the various hypothetical concepts of the gene. Stadler is right that proper use of an operational concept can ensure conformity and protect against the pernicious effects of certain theoretical errors. But, as the example of white and eosin genes shows, operational criteria (here, specifically for the individuation of genes) are themselves theory-laden and quite often erroneous. Furthermore, there is no single operational concept (or set of operational criteria) for the gene. In the end, as the brief discussion of molecular genetics in the last few paragraphs suggests, the best arbiter we have of the legitimacy of both operational and hypothetical concepts of the gene comes from molecular analysis. The latter, in turn, cannot be extricated from what Stadler would have considered a hypothetical concept, namely that of the structure of the DNA molecule. It follows that genetic concepts (and theoretical concepts generally) are inescapably open in the ways I have been describing.

For philosophical readers, I add only that the results of this extremely sketchy treatment of the case of the gene, if they withstand scrutiny, will prove to be of immense consequence for our understanding of reduction and of scientific realism.

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