AN ALTERNATIVE TO KITCHER'S THEORY OF CONCEPTUAL PROGRESS AND HIS ACCOUNT OF THE CHANGE OF THE GENE CONCEPT

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Abstract

The present paper discusses Kitcher's framework for studying conceptual change and progress. Kitcher's core notion of reference potential is hard to apply to concrete cases. In addition, an account of conceptual change as change in reference potential misses some important aspects of conceptual change and conceptual progress. I propose an alternative framework that focuses on the inferences and explanations supported by scientific concepts. The application of my approach to the history of the gene concept offers a better account of the conceptual progress that occurred in the transition from the Mendelian to the molecular gene than Kitcher's theory.

Philosophical accounts of the change of scientific terms often focus on the reference of terms. Philip Kitcher goes beyond the mere study of reference by using his notion of the 'reference potential' of a term, which allows for a more fine-grained study of conceptual change. In fact, Kitcher's framework is probably the most sophisticated and interesting framework on conceptual change in the philosophy of science; and it includes an account of conceptual progress as well. Kitcher developed this approach in a series of publications (1978, 1982, 1993); and his main application to the change of a concept in a historical period is his paper on the gene concept (1982).

This paper criticizes Kitcher's account and his claim that conceptual change and progress is best studied as change in reference potential. Apart from criticizing Kitcher's semantic framework and its fruitfulness for the philosophy of science, I will propose my own approach to scientific concepts, which is a version of conceptual role semantics in that it defines conceptual content in terms of the inferences and explanations supported by concepts. I will apply my framework to the same concept that Kitcher uses — the gene concept. My goal is to show that my approach offers a better account of the progress that occurred in the transition from Mendelian to molecular genetics.

Mode of reference and reference potential

Kitcher introduced his notion of reference potential in his seminal paper 'Theories, Theorists, and Theoretical Change' (1978), and restated it with some clarifications in 'Genes' (1982). The book *The Advancement of Science* (1993) restates this account of conceptual change with a few modifications, and goes beyond former discussions by adding an account of conceptual progress, based on the notion of reference potential. Kitcher's main example to illustrate his ideas on reference potential is the

term 'phlogiston' and the difference between phlogiston and oxygen chemists (1978, 1993). The framework on conceptual change is applied to a longer historical period in 'Genes' (1982), where Kitcher discusses the early history of the gene concept until the emergence of the molecular understanding of the gene. This section of my discussion focuses on Kitcher's purely semantic notions such as reference potential and discusses some unsolved issues of Kitcher's account. The next section will address Kitcher's account of conceptual progress and the adequacy of his framework for the study of conceptual change and progress.

The basic notions and features of Kitcher's framework are as follows. Kitcher belongs to the tradition in philosophy of science reacting to Kuhn's work and the threat of incommensurability. The main reply to the challenge of meaning incommensurability was to focus on the *reference* of scientific terms, and to point out that scientists may very well refer to the same entities despite the fact that they have different theories or different beliefs about this referent. Once common reference across different theories is secured, the claims of these theories can be semantically evaluated and compared. In fact, as long as philosophers of science did not just talk about theoretical change in general, most studies of scientific concepts and their change were referential approaches to scientific concepts.[1] Kitcher (1978) starts out his discussion by addressing Kuhn's claim that a new theory (from a different paradigm) cannot possibly talk about the same thing as the old theory: "The idea that conceptual relativism is a thesis about reference has been cogently presented by Israel Scheffler." (p. 521). Consequently, Kitcher's first step is to point out that we need to study the reference of scientific terms: "The remedy is to begin with the notion of reference."

[1]Scheffler 1967; Putnam 1973; Martin 1971; Fine 1975; Devitt 1979; Leplin 1979; Levin 1979; Newton-Smith 1981; Hacking 1983; Burian 1985; Miller 1987; Papineau 1987; Sankey 1994; Devitt and Sterelny 1999; Psillos 1999; Andersen 2001; Boyd 2002.

(p. 522). Kitcher (1978) offers an improved account of reference by arguing that while a token or an utterance of a term has a unique referent, different tokens of a term type may refer to different things. For instance, the term 'phlogiston' as used by the phlogiston chemist Priestley was sometimes non-referential (nothing satisfies the associated description). On other occasions, for instance when Priestley described the effects of him breathing 'dephlogisticated air', he referred to oxygen (in accordance with the causal theory of reference). By taking term tokens as the units of analysis, Kitcher can make some sense of the claims of phlogiston chemists by pointing out that some of their statements referred to the world (and were possibly true).

In his second basic step, Kitcher goes beyond the mere study of reference. Even though scientists endorsing different theories may refer with the same term to the same entity, they still may associate a different meaning with this term. Scientists refer to the same thing, but they may refer in a different manner. Kitcher acknowledges this by introducing the notion of a *mode of reference*. This is the way in which a term token refers or they way in which reference is fixed for a term token. Stressing the insights of the causal theory of reference, Kitcher explains that there are *causal modes of reference*. Reference to an entity was originally established by an introducing event, and reference is passed along in a causal fashion, possibly over generations. When a scientist later utters the term, the production of this term token is the terminal event of a long causal chain, where the first event of that chain is the introducing or 'initiating event' that determines to which entity the term actually refers. In his earlier writings (1978, 1982), Kitcher assumed that a causal mode of reference is the initiating event only, while his latest account (1993) states that the mode of reference is the total causal chain.[2] Moreover, there are *descriptive modes of reference*. On

[2]Kitcher does not explain why he chose to use this revised account.

a particular utterance of a term, the referent of the term is that kind that satisfies the description provided by the speaker. Kitcher (1982) explains that the intention of the speaker at the point of utterance determines which situation obtains. If the primary intention is to refer to an object or natural kind that is present a causal mode of reference obtains. Similarly, if the speaker intents to conform to the prior usage of others, she causally inherits reference from others. In other situation, a person intends to refer to what she has specified or can specify. In such a case a descriptive mode of reference obtains.[3]

In the case of scientific concept, there are many descriptions that can be used to pick out a certain referent, and a there are different causal chains and initiating events that established reference to the same kind. So a term *type* is associated with many different modes of reference. Kitcher calls the totality of modes of reference of a term the *reference potential* of a term, and makes clear that the reference potential is usually heterogeneous. Kitcher's central idea is to identify scientific concepts with reference potentials (1978, p. 543). Thus *conceptual change is change in reference potential*. In the course of history, the reference potential of a term may enlarge if we acquire new ways of referring to or picking out a known entity or it may contract in case former modes of reference are deemed to be problematic. Studying the change in reference potential allows for a more fine-grained philosophical account than the mere study of reference of a term, because we can distinguish different ways of referring to the same referent.

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[3]Psillos (1997) critically discusses Kitcher's distinction between causal and descriptive modes of reference, in particular Kitcher's assumption that due to the use of different types of reference determination a term type may refer to different objects on different occasions (as in the phlogiston case). My discussion is not primarily about Kitcher's framework as an account of the reference of scientific terms. Instead, I focus on his approach as a theory of conceptual change and progress. I will indicate below when Psillos's critique of Kitcher bears on my discussion.

There are some unsolved internal issues that Kitcher's account raise, suggesting that his notion of reference potential has to be elaborated. One open issue arises from Kitcher's definition of conceptual progress (to be discussed in more detail in the next section). Conceptual change, i.e., a change of the reference potential of a term, is progressive in case of "adding a description that picks out the pertinent kind or by abandoning a mode of reference determination belonging to the [old] reference potential that failed to pick out the pertinent kind" (1993, p. 105). Note that this is *not* the same as the acquiring novel true beliefs and abandoning false beliefs about the referent. Kitcher's definition focuses on descriptive *modes of reference*; and not every description of or belief about the referent is a mode of reference. For instance, if in a particular situation the reference of the term 'gene' is fixed causally, the statement 'genes are protein molecules that are part of chromosomes' is a description of genes in the sense of a claim about genes, but this description does not fix the reference of the term 'gene'. Kitcher's account can be interpreted as follows. Some statements fix reference descriptively and are thus modes of reference. Others, however, do not fix reference. But they still refer, because reference is parasitic on prior statements or beliefs that are actually reference-determining. The first type of statements that fix reference may be called reference-analytic — they are (descriptive) modes of reference and we cannot abandon without potentially changing the reference of the term. Reference-analytic statements fix meaning in the sense of reference. The second type of statement that do not determine reference can be called reference-synthetic. Scientist can deny these reference-synthetic statements or disagree over them without influencing the reference of the term involved. As in this case reference is parasitic on reference-analytic statements, it is possible to make false statements that are still about the same referent, which is philosophically important. On Kitcher's account, conceptual progress is not change of the totality of statements accepted or utterances

made by scientists. Instead, it is about the subset of statements that are descriptive modes of references. In my terminology, conceptual progress is the acceptance or abandonment of reference-*analytic* statements. Thus in order to assess whether conceptual progress occurred we need to be able to tell reference-analytic statements apart from reference-synthetic ones.

Independent of Kitcher's definition of progress, his account of conceptual change in general is about change in the reference potential, i.e., about the addition and deletion of modes of reference. So we need to be able to tell whether a statement is a descriptive mode of reference, i.e., reference-analytic. The upshot is that Kitcher is committed to a distinction between reference-analytic and reference-synthetic statements.[4] His 1982 discussion actually mentions this at one point by stating that "not all community shared beliefs which use a particular term may be employed in fixing the reference of that term. ... I rely on a distinction between beliefs which are employed in reference-fixing and beliefs which are not." (p. 347). The unsolved issue is precisely that Kitcher has not yet offered an account that is elaborated enough to draw this reference-analytic/synthetic distinction. Kitcher points out that the speaker's intentions have some bearing on reference determination. But the appeal to the intentions or other background beliefs of the speaker alone will not do the job, because scientists may simply have false beliefs. It is important for an approach that includes causal factors of reference determination that false beliefs of a scientist need not interfere with reference. Stathis Psillos (1997) has previously criticized Kitcher's idea that in each situation there is a dominant intention of the speaker that settles

[4]As Kitcher identifies concepts with reference potentials and the notion of a mode of reference is Kitcher's proxy for the meaning or sense of a term, this distinction between reference-analytic and reference-synthetic is in fact a real analytic/synthetic distinction — it distinguishes meaning-constitutive from other statements.

the referent and the mode of reference. Psillos's point is that a scientists often has the intention to refer both in a causal and descriptive manner, and since she may have the strong but erroneous belief that her theoretical description in fact picks out the substance that she refers to in a causal fashion, these two intentions are not viewed as conflicting by the scientists and therefore there is no dominant intention.[5] Kitcher obviously assumes that it is a clear-cut matter of fact whether a description is a mode of reference or not. But so far Kitcher's theory is not elaborated enough to yield a notion of reference-analyticity that defines in virtue of what features something is a mode of reference. Without a precise definition of mode of reference, the notion of reference potential as the totality of modes of reference is not precisely explained as well.

Another unsolved issue emerges from Kitcher's most recent discussion. His account of concepts focuses on reference and modes of reference. Hilary Putnam (1975) prominently argued that if the meaning of a term is to determine its reference, then meaning cannot be inside the speaker's head. Restating Putnam's Twin Earth examples, Kitcher concludes: " 'What is in the speaker's head' does not therefore determine

[5] "Still, saying that Priestley was involved in different modes of reference on different occasions of him using the expression 'dephlogisticated air' is, if anything, an idealisation. Barring extreme cases, one's intentions to refer are so interwoven that they cannot be naturally broken up into two components, in particular into intentions to refer to a certain object — no matter what this object turns out to be — and intentions to refer to whatever satisfies a certain (possibly theoretical) description. Under normal circumstances, one's mode of reference is a function of both one's intentions to refer to an object (or, to a kind of object, an instance of which is present) *and* one's intentions to refer to *this* object as exemplifying one or more descriptions. ... More generally, I would like to note that attributing different dominant intentions to refer on different occasions makes no difference to the explanation of Priestley's judgements, arguments and assertions. Priestley would make (and in fact did make) the same judgements and assertions about the stuff he isolated regardless of whether he characterised it via theoretical descriptions or by its detectable qualities." (Psillos 1997, pp. 265, 267)

reference. I shall articulate my approach to scientific language by building on the recent insights about reference." (1993, p. 76). Consequently, Kitcher defines the mode of reference such that it includes events outside the speaker's head (e.g., the initiating event): the mode of reference is the total "complex causal chain that stands behind her [i.e., the speaker's] current vocalization" (p. 77). However, Kitcher's notion of a mode of reference is also supposed to be a sort of Fregean sense. And the point of Fregean senses is that they are sensitive to "differences in cognitive content" (p. 78). Even though two distinct modes of reference may refer to the same object, the rational agent may not know this and reason differently with one mode of reference than with another. Kitcher (1993) illustrates this with Frege's example, according to which the descriptions 'evening star' and 'morning star' are different modes of references for the same object. In some context the speaker may use 'evening star' but be unwilling to use 'morning star' because it is associated with a different sense.[6] To play this cognitive role, a sense has to be grasped by the speaker — using Frege's terminology. Consequently, based on his naturalistic account of cognition, Kitcher states that "acquiring the reference potential of a term consists in incorporating a set of propensities into procedural memory" (p. 78). In sum, one page after stating that a reference potential consists of causal chains *external* to the speaker, suddenly the reference potential is a set of propensities *inside* the head.

Kitcher has to find a way to resolve this tension. Given that he is most fundamentally after rebutting the incommensurability threat, a natural recommendation for him is to focus on the reference of terms. This suggest a picture according to which meaning and concepts are outside the head, so that Kitcher can stick with

[6]Kitcher's original discussion already viewed modes of reference as Fregean senses or modes of presentation: "The reference potential is akin to the second idea of sense as 'the manner in which reference is presented'." (Kitcher 1978, p. 534)

his definition of mode of reference which includes initiating events and other events outside the speaker's head. A contradiction can be easily avoided if Kitcher abandons the claim that a mode of reference is also a sort of Fregean sense that is sensitive to cognitive differences. In this case, Kitcher would still have an approach to conceptual change that goes beyond the mere study of reference, because modes of reference cut finer than extension. This strategy of resolving the contradiction has the drawback that we may wonder why we need the more fine-grained approach in the first place. The traditional motivation is that we ascribe different concepts (meanings, senses, intensions) to different speakers because they reason and act differently with their concept. The very reason to ascribe concepts to persons is to explain thinking and rational behavior. And this is important for the study of conceptual change in science as well. Researchers prefer to conduct different experiments if they have different concepts; and contemporary scientists reason differently about certain phenomena because they have different concepts compared to scientists from the 19th century. But Kitcher seems to be barred from using this standard motivation as the reason to make use of an approach to concepts that is more fine-grained than the extension of terms. He still needs to defend his identification of concepts with reference potentials.

I view the points raised in this section as showing that Kitcher has not spelled out in sufficient detail what a mode of reference and thus a reference potential is. But in the following discussion I will not deal with these purely semantic and internal issues, but focus on the promises of Kitcher's account for the study of conceptual change, assuming that there is a way to elaborate the notion of reference potential in the way Kitcher intends.

A critique of Kitcher's account of conceptual change and progress

I now turn to a discussion of the adequacy of Kitcher's framework for studying conceptual change and progress. My critique will prepare my alternative approach, which I will sketch in the next section and apply to the case of the gene concept. I have three basic points of criticism. My *first point* relates to the fact that Kitcher — like most of the recent tradition in the philosophy of science — starts out his study with an account of reference. He supplements his theory to get an account of conceptual change, which uses the notions of mode of reference and reference potential and thus is fundamentally based on the idea of reference as well. The standard motivation for the focus on reference is to address the threat of incommensurability. However, in my view there is more to the philosophical study of conceptual change than addressing issues relating to incommensurability; and I do not think that reference is the primary concern for an account that is intended to explain and evaluate conceptual change. For this reason, I want to challenge the motivation for the focus on reference. My point is that even if one is primarily after rebutting incommensurability, focusing on reference alone is insufficient.

In Kuhn's work incommensurability of meaning has problematic consequences because it is claimed to entail radical *epistemic* incommensurability. The challenge is ultimately of an epistemic nature — scientists are claimed not to be a position to rationally choose between different theories. Incommensurability of meaning (as well as incommensurability of standards) seems to make rational justification impossible. This is the fundamental challenge to rationality stemming from Kuhn's work. An account of reference by itself, however, has not much to offer on this epistemic issue. The purely semantic notion of reference allows the philosopher to verify that scientists

endorsing different theories refer to the same entity, and it allows her to assign truth values to the statements made by scientists. But pointing out that later theories were right where former scientists made false claims does not show that scientists had *reasons* to abandon former beliefs. Epistemic considerations are necessary to address this issue.

The implication for Kitcher's approach is that it not sufficient to merely spell out the notions of reference and reference potential. Kitcher would need an account as to whether and how scientists *know* (at least implicitly) about the reference and reference potential of the terms as used by them *and* by other scientists. Kitcher does not say much about this issue, and my impression is that he thinks that such an account follows relatively straightforward from his definition of reference and reference potential. This ambiguity can be illustrated when Kitcher (1978) states that his account of reference ensures that "the scientists in question will be able to *formulate* their disagreements" (p. 528). However, an account of reference as such can show that two scientists talk about the same entity and make statements with a different truth-value. But this does not entail that the scientists *know* about the fact that they actually refer to the same entity, and if they do not have some knowledge of the fact that they make contradictory statements, they actually cannot formulate their disagreements. More needs to be done than giving a definition of reference and reference potential, but Kitcher hardly discusses such issues. He very briefly address how phlogiston chemists can communicate with each other and how other oxygen chemists can understand phlogiston chemists (1978, pp. 541–542; 1993, p. 103). This is exactly the issue that needs to be discussed and pursued in detail in order to offer something against incommensurability. I do not claim that Kitcher could not develop such an account. My point is that while Kitcher takes the incommensurability

threat as the prime motivation for his focus on reference, an account of reference and reference potential alone is not sufficient to solve this issue.

This point can be put in a different way. Kuhn's argument surrounding incommensurability is structured as follows. Kuhn starts out with the observation that scientists from different paradigms have strongly different beliefs. His first step is to argue that this existence of different theories and paradigms implies that the same term can be used with very different meanings. The second step is the suggestion that meaning difference or meaning incommensurability brings about fundamental epistemic problems. Thus, overall Kuhn's argument starts with an epistemic point (difference in belief), and using a semantic detour (differences in meaning) he draws the epistemic conclusion that scientists cannot rationally choose between different theories. My stance on this argument is to reject the second step. The interesting point is that the standard referentialist tradition in the philosophy of science does not reject the second part of Kuhn's argument.[7] In fact, the idea that differences in meaning lead to incommensurability is the very motivation for talking about reference. However, the usual position is not to deal with Kuhn's inference from difference in meaning to incommensurability by *denying* that there are meanings (or senses) and that the only semantic property is reference. Instead, philosophers of science usually choose to focus on reference, and not to *talk* about meanings (or admit

[7]For instance, Burian (1985), in an article that endorses Kitcher's framework, states that people have often accepted "some form of holism about theoretical concepts," and that "holism brings radical incommensurability with it" (p. 24). Burian implicitly accepts the second step of Kuhn's argument, thus he views buying into the first step as the problem creating incommensurability. But if we reject the second step, then we can even accept the first step. On a certain holistic construal of meaning, every difference in belief may amount to some difference in meaning, but then these differences in meaning do not have any radical epistemic implications — insofar as de facto differences in beliefs do not imply radical epistemic incommensurability.

differences in meanings). However, meanings and differences in meaning are implicitly acknowledged. After all, if conceptual change is about change in concepts and not just about change in reference, then concepts do actually change. For instance, Howard Sankey's (1994) detailed discussion of incommensurability views an account of reference as a rebuttal of incommensurability. However, Sankey also agrees with Kuhn that perfect and literal translation between theories is sometimes impossible—while non-translatability was one reason for Kuhn to claim that scientists cannot rationally convince scientists endorsing other theories of their position. In the case of Kitcher's account, his notion of mode of reference is a proxy for the meaning of a term; and there are different modes of reference and reference potentials change over time. Thus, Kitcher is also explicitly committed to differences in meanings.

In sum, the Kuhnian inference from differences in meaning to epistemic incommensurability is usually not rejected and instead used as a motivation for the primary focus on reference instead of meaning. If this inference is accepted, however, then it is quite problematic that many philosophers of science do not or cannot really reject the notion of meaning, the existence of meaning differences, or the change of meaning in the course of history. Moreover, an account of *reference* does not address at all the idea that *meaning* differences entail epistemic troubles. Instead, we have to deal with the notion of meaning and show that meaning differences need not imply that scientists cannot convey their claims and arguments to their opponents. If the challenge of meaning incommensurability is understood in this way, then considerations about reference appear to be independent of this problem. Despite the popularity of invoking reference as a reply to the incommensurability threat,[8] reference alone

[8]Scheffler 1967; Putnam 1973; Martin 1971; Fine 1975; Devitt 1979; Leplin 1979; Levin 1979; Newton-Smith 1981; Hacking 1983; Burian 1985; Miller 1987; Papineau 1987; Sankey 1994; Devitt and Sterelny 1999; Psillos 1999; Andersen 2001; Boyd 2002.

won't do the job.

My *second critical point* is about the applicability of Kitcher's framework. In the last section, I pointed out that Kitcher does not have a fully elaborated account of when a statement or belief is reference-fixing — in other words, Kitcher has not explained what determines the difference between reference-analytic and reference-synthetic statements. The discussion so far assumed that this is a purely semantic issue. However, it also has a bearing for the practical study of conceptual change. For Kitcher's claim is that we should study episodes in the history of science based on his framework, and a philosophical account of conceptual change should be applicable to the philosophical study of science. What we must have is an account that helps us to detect and study reference potentials in concrete cases. Even though we do not need a theoretical account of the notion of reference-analyticity, we do need *criteria* for picking out reference-analytic statements. But Kitcher does not offer any criteria of how to pin down modes of reference and distinguish them from beliefs that do not influence reference. Given this, it is unclear how Kitcher's framework is to be applied to concrete cases.[9]

Let us take a look at the essay 'Genes' (Kitcher 1982), which is intended to study the reference potential of the gene concept from Mendelian to molecular genetics. The brief history of the gene that Kitcher discusses picks out very interesting points.

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[9]Kitcher offers some guidelines as to how to interpret historical episodes. His idea is that we should use Richard Grandy's 'principle of humanity', which is a variant of the principle of charity. Even though this principle may in fact be relevant for studying the history of science, this interpretative approach will not support the clear-cut and principled distinction between reference-analytic and reference-synthetic statements, which Kitcher is committed to. Psillos's (1997) critique of Kitcher's account of reference focuses on his reliance on the principle of humanity. Psillos argues that "that the application of the principle of humanity does not offer a principled way to show that the historical actors were involved in different modes of reference when they produced different tokens of an expression-type." (p. 259).

But it is hardly an application of his philosophical framework. Kitcher mentions only three modes of reference: Sturtevant's description that genes are segments within which recombination cannot occur, put forward in 1913 (p. 351); the criterion given by Benzer's *cis-trans* test, as spelled out in 1957 (p. 352); and Beadle's 1941 one gene–one enzyme hypothesis (p. 354). The first problem is that Kitcher does not say why he considers these statements to be modes of references — each statement could be nothing but a (potentially false) claim about genes while the reference of the term 'gene' occurring in them was fully fixed by prior statements.[10] As long as we do not have a *prima facie* idea of why we are dealing in these concrete cases with modes of references (rather than other utterances), Kitcher's framework is not really applied. In addition, Kitcher's three isolated examples alone do not give us a good idea as to how the reference potential of the term 'gene' changed (and it does not give us a comparison between the Mendelian and molecular gene concept). If the study of conceptual change is the study of reference potential, which is supposed to be quite heterogeneous in that it encompasses many modes of references even at a particular point in history, then we need a detailed account of how several new modes of references emerged rather than three isolated examples. In sum, Kitcher's framework that commits us to detect and study modes of references (but not other entities) appears to be hard to apply to concrete cases; and actually, Kitcher's 1982 historical study does not offer a detailed application of his own philosophical framework.

My *final critical point* challenges the idea that the study of reference potential actually addresses those aspects of concepts that are crucial for explaining and evaluating conceptual change. Kitcher's account is designed rebut the incommensurability

[10]Given that these descriptions do not pick out the class of genes as we understand it nowadays, it is not implausible to assume that they were actually false claims about genes, rather than reference-fixing descriptions that picked out a class of objects that we do not view as a natural kind nowadays.

thesis. But I think that there are additional goals for the study of conceptual change and Kitcher's approach is likely to fail to address these goals. As far as the study of conceptual change and differences is concerned, the notion reference potential alone is of limited use. Reference potential is Kitcher's proxy for the notion of 'concept' or 'sense of a term'. However, it does not allow us to keep different concepts apart that correspond to the same term. For instance, it is widely recognized that the term 'species' corresponds nowadays to different concepts. In the next section I will claim that currently we still use the Mendelian gene concept apart from the molecular gene concept (in population genetics rather than molecular biology). Sometimes in the course of history, a scientific concept may split into two concepts, and we should be able to take this conceptual difference into account. Kitcher, as his account stands, could in such a situation only note that the reference potential of the term 'gene' is highly heterogeneous, ignoring that some conceptual variation might be best viewed as being due to the existence of several concepts or senses being associated with the same term. Thus we need an account of concept individuation that permits us to track conceptual change and the emergence of novel concepts.

Even without a conceptual split, in the course of history the meaning of a term may change so substantially that we might want to consider this as the emergence of new concept, while the old concept (the meaning formerly associated with the term) is not used any longer. The emergence of the molecular gene concept out of the Mendelian gene concept could be viewed in this way. Recognizing substantial conceptual change (that justifies the postulation of a new concept) is important for studying conceptual progress, because not every change is equally important. While Kitcher counts every addition to a reference potential as progressive, I will later make use of an account of concepts that can reserve the notion of progress to substantial

conceptual changes that have an impact of the scientific discipline under consideration. More important is the fact that Kitcher does not tell us what is progressive about his notion of conceptual progress. Descriptions that are not modes of reference are not part of the reference potential and thus *by definition* do not contribute to conceptual progress. However, Kitcher does not offer a defense of this assumption. Why should additions of reference-analytic statements be necessarily more progressive than additions of reference-synthetic statements? Moreover, adding new modes of reference and eliminating problematic modes of reference may be all there is to progress in the change of *reference potentials*. But I think that this hardly exhausts conceptual progress in science. What is crucial about conceptual change is that certain changes facilitate scientific discovery in a better manner, permit scientists to justify new hypotheses, and to explain new ranges of phenomena. This is in my view the main impact on conceptual progress on science. Kitcher is not interested in this dimension of conceptual change, his account of conceptual change is independent of how new conceptual variants contribute to scientific discovery and explanation. In the next section I sketch my own account that focuses on these issues; and I will argue that the change from the Mendelian to the molecular gene concept is progressive precisely because the molecular gene concept supports certain types of explanations that the Mendelian gene concept cannot offer.

Kitcher (1993) focuses on two types of scientific progress: conceptual and explanatory progress. I agree with Kitcher that explanatory progress is of fundamental importance for science (and should be for the philosophy of science), and Kitcher's notion of explanatory schemata yields a good account of explanatory progress. However, Kitcher's account of conceptual progress and explanatory progress are completely unrelated. In my view, conceptual progress is a crucial factor in explanatory progress.

It is not easy to see how reference potentials can contribute to explanations.[11] But concepts definitely do, so the notion of reference potential is likely to miss an important aspect of concepts and conceptual progress. To illustrate this point, conceptual progress may occur in the case of the introduction of a completely new term, provided that this concept permits to explain new phenomena. For instance, Darwin's introduction of the concept of natural selection permitted the explanation of a range of previously unexplained (or improperly explained) phenomena. Kitcher's account of conceptual progress, however, just focuses on the change or the refinement of the reference potential of existing terms; but it does not and cannot count the introduction of a new term as progressive. For it is hard to see how the mere establishment of reference to a new entity or kind by itself contributes to explanation. Darwin's introduction of the term 'natural selection' did not just establish reference to a new process, but it played a fundamental role for the explanatory success of Darwinian evolutionary theory. Thus, we have to go beyond reference and modes of reference to account for the explanatory impact of a concept.

Conceptual change as change in inferential and explanatory potential

My last section questioned the applicability of Kitcher's framework to concrete cases and its fruitfulness as a framework for studying conceptual change and progress. Kitcher's reference-oriented framework is primarily designed to address the incommensurability problem. However, I think that the study of conceptual change involves

[11] This ultimately stems from the fact that a referential account of concepts takes a 'meaning is outside the head' approach. As already mentioned, it is unclear how concepts thus conceived can figure in scientific reasoning such as giving explanations and justifying hypotheses.

some further important tasks. These are 1) tracking conceptual change and the emergence of new concepts, which presupposes an account of concept individuation; 2) explaining why conceptual change occurred; and 3) assessing to which extent an instance of conceptual change was progressive. My main critique of Kitcher was that he does not offer a way of individuating different concepts (that correspond to one term) and his account of conceptual change and progress is independent of the way in which concepts contribute to research by facilitating discovery, justification, and explanation. My claim is not that Kitcher is debarred from adding to and modifying his framework to include these issues. However, in this case he would first start out with an account of reference, in a second step define reference potential and in a third step try to develop an account of the way in which change in reference potential contributes to giving explanations and aspects of scientific progress that go beyond adding and deleting modes of reference. Kitcher never addresses up front what the goals of a theory of conceptual change and progress are. Instead, he just assumes that reference is the crucial thing to focus on and then offers some additions to this framework without justifying for instance why adding and deleting modes of reference captures conceptual progress. I explained why I am skeptical about the necessity to focus primarily on reference when it comes to the study of conceptual change. Consequently, I prefer to offer an alternative account that focuses more directly on the explanation and assessment of conceptual change. The goal is to have an account of concepts that permits us to detect whether substantial conceptual change occurred, to explain why it occurred, and to evaluate whether the change was progressive.

My account of concepts is a version of *conceptual role semantics*. Conceptual role semantics is not a unique and specific theory, rather it is an approach encompassing various (sometimes quite different) semantic approaches in the philosophy of mind

and language (Block 1986; Brandom 1994; Field 1977; Harman 1973; Horwich 1998; Loar 1981; McGinn 1982; Wedgwood 2001) and in cognitive science (Carey 1991; Evans 1989; Miller and Johnson-Laird 1976; Woods 1981). The idea of conceptual role semantics — also called functional role or inferential role semantics — is that the meaning of linguistic symbols and the content of mental representations is at least partially constituted by the cognitive or inferential role they have for a thinker or community. Concepts have a specific role in thought, perception, decision making, and action. In the philosophy of mind, the notion of conceptual role is often explicated based on the language of thought hypothesis (Schiffer 1981; Harman 1982). The conceptual role of a syntactic symbol in the language of thought (i.e., its semantic content) is the set of causal relations it has to other mental symbols (including the causal relation to perceptual input and motor output). However, this paper is not concerned with the nature of mental content and related issues from the philosophy of mind. Instead, my semantic framework is intended as a heuristic tool to study conceptual change in science. For this reason, it is not necessary to endorse the language of thought hypothesis or a particular theory of the mind. Instead, I will define the notion of conceptual role based on public language, which fits with the fact that in the study of the historical episodes in science we have to rely on the verbal and written reports of scientists.

The labels 'conceptual role semantics' and 'inferential role semantics' are used synonymously, because conceptual roles are often defined as *inferential roles*. On my account, an inferential role is the set of inferences in which a term figures. An individual endorses various inferences, and the inferential role of a term T is the totality of accepted inferences between statements in which T occurs (at least once). The idea is that an individual makes a particular inference (reasons in a particu-

lar way) because of the content of the terms involved. We explain the fact that an individual reasons or behaves in this rather than another way by the fact that he entertains concept X rather than concept Y (that term T means for her X rather than Y). Thus, conceptual content is viewed as the set of inferences supported by a concept — in other words, as the inferential potential of a concept. While philosophical accounts along the line of conceptual role semantics have stressed the inferential potential of concepts, one needs to keep in mind that concepts — in particular scientific concepts — are also used for explanations. Thus I emphasize that my notion of conceptual role includes not just inferential role, but also the *explanatory role* of concepts. A particular concept might be crucial for explaining a specific class of processes or situations, while in order to account for another class of phenomena a different concept needs to be employed. Without the concept of natural selection, for instance, we would be unable to give a wide range of important explanations in evolutionary biology. It is not obvious how explanation relates to standard models of inference making. Salmon (1970) argues that explanations are not arguments (neither inductive nor deductive), so the inferential role of concepts need not encompass their explanatory role. In the case of statistical explanations, concepts may pick out a class and appropriate reference classes and link them to statistical relevance relations. In causal explanations a concept picks out a set of entities that are part of similar causal processes or governed by the same causal law. In the case study of the next section I will argue that the crucial difference between the Mendelian and the molecular gene concept is that the molecular gene concept supports explanations that the Mendelian gene concept cannot give.[12]

[12]Conceptual roles are often associated with narrow (as opposed to wide) content, i.e., the relationship between the mind and the world is not part of the conceptual role. It has been argued that versions of conceptual role semantics focusing on inferential role or narrow content alone cannot give

A fundamental feature of my account of concepts is that it assumes two levels of content. The first level focuses on individuals and their idiolect. Content is identified with the conceptual role of a term — the totality of inferences and explanations endorsed by a particular person in which this term figures. Given that probably no two individuals endorse the same set of inferences, their inferential roles of a term T will differ. My first level of content is holist, two individuals will often associate a different meaning with the same term. Content on the level of individuals can be viewed as the meaning of a term in the idiolect of a person, or as the mental representation this person associates with the term. Individuals may very well differ in their mental representations, and it is in fact important to acknowledge the interpersonal difference in content if mental content is to explain reasoning and behavior. If two scientists have a different conception of genes and thus on my account of meaning associate a different idiolect-meaning with the term 'gene', then due to their different conceptions they may make different theoretical claims and conduct different experiments.[13] But *concepts* have to be shared between individuals, so I use a second level of content that focuses on scientific groups and the meaning of terms in a public language. It is on this level that the study of conceptual change takes place. I view a concept as a cluster of similar individual mental representations. Taking a concept as a group-level entity abstracts from the interpersonal variation and focuses on the

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a satisfactory account of content, because they cannot account for the representational aspects of content, and thus for the possibility of misrepresentation and falsehood. Proponents of conceptual role semantics have reacted to this problem by including the mind-world relationship in the conceptual role (Harman 1987), or by saying that inferential role/narrow content is only one aspect of content to be supplied by an account of reference (Field 1977; McGinn 1982; Block 1986). I favor this second option, but since my goal is to study differences in certain scientific concepts rather than putting forward a general theory of content I am not concerned with this issue in the present paper.

[13]Because this semantic holism and difference in meaning is just a reflection of de-facto difference in belief, it does not immediately run into problematic incommensurability.

more substantial difference between different concepts. Thus, I follow Harman (1973) and Block (1986) in assuming that merely similarity, not necessarily identity in conceptual role is sufficient to share the same concept. In other words, I do not identify concepts with conceptual roles — not every difference in conceptual role amounts to a new concept. Instead I assume that conceptual content supervenes on conceptual roles — two persons using different conceptual roles may still count as possessing the same concept, but two persons can express different concepts with the same term only if they use different conceptual roles.

But how to individuate concepts? The question is which of the inferences and explanations a person may endorse are characteristic of and define a particular concept. That is, which inferences are meaning-constitutive in that a person possesses this particular concept if and only if she endorses these inferences (while people may still disagree about other inferences without counting as possessing different concepts)? We saw that Kitcher assumes that there is a unique matter of fact whether something is a mode of reference or not by claiming that there is a clear-cut and unique distinction between beliefs that fix reference and other beliefs. Kitcher is committed to a reference-analytic/synthetic distinction, even though he has not yet offered an account as to how to make this distinction. My framework, however, does not maintain that there is a unique distinction between meaning-constitutive and non-constitutive inferences (or between analytic and synthetic inferences). Instead, I assume that some concepts can be individuated in different ways, so that one term can have more than one meaning at the same time. The idea is that a particular term may be viewed as corresponding to a single concept (which is ascribed to every person from a whole scientific field). But at the same time, we can legitimately individuate in a more fine grained manner, i.e., this term can also be considered as corresponding

to two or several concepts (so that each of these concepts is attributed to a smaller group of persons only). The reason is that there are different philosophical and explanatory interests that underlie a particular study of the change of a scientific term. These interests determine how a concept is to be individuated; and as the same term can be subject to different philosophical studies and theoretical interests, its content may be individuated in different ways. I do not assume that there is a unique and principled distinction between real concepts and mere variants of a concept. Starting with the total conceptual role of a term endorsed by an individual (the idiolect of this person), we can study the interpersonal variation in conceptual role. This variation tends to be grouped around certain poles or in certain clusters, and we can pick out one of these clusters and consider it a concept. Such a choice is fruitful as long as it fits some of the philosophical interests that can underlie a particular study of conceptual change. As there are different possible explanatory interests, different ways of individuating concepts can be legitimate. I will illustrate this idea in the case study below.

To sum up my approach, rather than dealing with the reference potential of terms, I focus on the inferential and explanatory potential of terms. These are the inferences and explanations supported by concepts (i.e., those inferences and explanations endorsed by most scientists of a given community). Conceptual change is studied in terms of the change of the inferences and explanations supported by a term. While I questioned whether Kitcher's notion of reference potential is easily applicable to concrete cases, I take for granted that we have a handle on which inferences and explanations are endorsed by scientists. Conceptual progress is to be assessed in terms of how a novel range of inferences and explanations supported by a

new concept contributes to science.[14] Rather than studying additions and deletions of mode of references, I focus on some features of science activity that directly matter to science — inferring and justifying hypothesis and giving explanations. The final section applies this framework to the concept of the gene.

The Mendelian and the molecular gene concept

The point of this case study is not to apply my framework to the whole history of genetics or even some part of it — this is beyond the scope of the present paper. Instead, I will pick out a few selected issues that illustrate how my approach can be applied in practice. The focus is on characterizing the Mendelian vs. the molecular gene concept. I will explain what the conceptual differences between the Mendelian and the molecular gene concept are and in what sense we have a clear instance of conceptual progress.

During the history of Mendelian genetics, especially its early history, the views about genes changed relatively rapidly. And even at a particular time, geneticists differed widely as regards their views on the nature and function of genes. Some biologists viewed genes as parts of the chromosome, thus endorsing an inference from 'is a gene' to 'is a delimited part of the chromosome'. Muller (1926) was willing to make the bold inference 'has autocatalytic properties' from 'is a gene'. Others

[14] In another paper, I use this framework to *explain* the conceptual change of a different biological concept (reference omitted for purposes of anonymous review). The guiding idea is to explain conceptual change based on the goals of a scientific field. A particular research program uses its concept to pursue certain goals and to obtain a particular epistemic product — characteristic type of inferences and explanations. Conceptual change can be explained based on the emergence of new research approaches that have different goals. The current study focuses on conceptual progress, though.

such as Goldschmidt (1937) rejected the idea that Mendelian factors are parts of the chromosome, and assumed that genes are abstract entities or that the chromosome is the unit of physiological action. Stadler (1954) argued for an operational approach to the gene that prohibits inferences about the nature of the gene that do not directly follow from the experiments. At any particular point in time, different geneticists sometimes endorsed different inferences and explanations in which the term 'gene' figures. This difference can be viewed as a difference between the mental representations each individual has for the term 'gene'. Mental representations or mental contents are particulars, and individuals may differ in their representations. In other words, due to the widespread disagreement about the material nature of genes, the inferential and explanatory potential of terms such as 'gene', 'factor', and 'allele' — taken as properties of individuals — exhibited interpersonal variation.[15]

It is important to take this fine-grained view of mental content and recognize differences because the historically existing variation in individual conceptual roles explains why different scientists sometimes reasoned differently, chose to pursue different experiments, and criticized each other. In addition, this fine grained-level of meaning points to ways in which conceptual roles can change. The gene was viewed as the unit of function, the unit of recombination, and the unit of mutation. Not every scientists endorsed all of these three views together, but in the case of some geneticists their conceptual role of term 'gene' allowed for an inference to each of

[15]Carlson (1966), in his history of Mendelian genetics, puts the differences in views as follows: "The gene has been considered to be an undefined unit, a unit-character, a 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 genomeres, a series of linear subgenes, a spherical unit defined by target theory, a dynamic functional quantity of one specific unit, a pseudoallele, a specific chromosome segment due to position effect, a rearrangement within a continuous chromosome molecule, ..." (p. 259).

these characterizations of the gene. Originally, this did not create any contradictions at all. But as soon as new experimental and theoretical results suggested that the smallest unit of mutation, the unit of recombination and the unit of function need not be identical, adding these new inferences to an existing conceptual role created incoherencies. Benzer (1957) addressed this issue by suggesting three new terms 'muton', 'recon', 'cistron', so that the inferential and explanatory role of the term 'gene' is split into three conceptual roles and contradictions are avoided.[16] There are obviously different ways in which a conceptual role can change, and studying the total conceptual role of a term suggests ways in which it might change.

Despite these differences in individual mental representations, the crucial step in my account is to delimit the Mendelian gene concept by certain inferences and explanations in which the term 'gene' figured and that were widely endorsed by geneticists. On my account, *the conceptual role of the Mendelian gene is the prediction and explanation of Mendelian patterns of inheritance.* From the genotypes of the parental generation (the genes and alleles of the parents) we can infer the distribution of genotypes and thus phenotypes of the following generations (at least the statistical mean of the next generation, even though in case of a small sample the actual distribution will diverge from the prediction due to random effects). In fact, the existence of Mendelian patterns of inheritance and mendelizing traits was the crucial evidence for the existence of Mendelian genes. Whatever the material nature of genes, Mendelian geneticists agreed on the fact that patterns of inheritance are to be predicted and explained by the inheritance of factors or alleles. This prediction is possible because the inferential role of the Mendelian gene includes an account of how genes behave in processes such as segregation and linkage (which entails the genotype distribution of

[16]The terms 'muton' and 'recon' were not adopted. But the gene concept came to be largely identified with Benzer's notion of a cistron.

the next generation) and it makes reference to notions such as the dominance of alleles (which entails the phenotype of the filial generation). This aspect of the Mendelian gene can be found from the early history of genetics onwards (it is for instance clearly expressed by Morgan, Sturtevant, Muller, and Bridges 1915), and continued to be of crucial importance throughout the history of Mendelian genetics. It was strongly embedded in the practice of Mendelian geneticists who carried out segregation and linkage analysis. And it played an important role for the evidential impact of results in Mendelian genetics. Whenever a trait mendelizes, a geneticist has to agree that is very likely due to the effect of a gene and thus that these experimental results have a bearing on her views on genetics — independent of whether the experimental studies were carried out by someone who has a different view of the material basis of the gene. To be sure, the understanding of the Mendelian gene needed to be refined once exceptions to the simple patterns of inheritance became clear due to position effects, variable expressivity, or overdominance. The inferential and explanatory potential of the term 'gene' as endorsed by individual scientists changed, but only slightly so that we can claim that these refinements in the way inheritance was explained do not amount to the emergence of a new concept and that instead change took place within the boundaries of the cluster concept of the Mendelian gene.

The role of the Mendelian gene concept in the explanation of phenotypic traits is as follows. Geneticists knew that the relation between genes and traits is many–many (Morgan, Sturtevant, Muller, and Bridges 1915). They could not explain the development of traits, because the large set of genes involved in a structure were unknown, and in particular they had no idea about how genes bring about their effects. So a real causal or mechanistic explanation of characters by genes was impossible. But Mendelian genetics could explain phenotypic *differences* by means of

genotypic *differences*, which was very useful for genetics. Despite the emergence of the molecular gene concept, something very much like the Mendelian gene concept is still used, for instance in population genetics. For the purposes of studying and explaining the change of character distributions in natural populations, it is sufficient to use the Mendelian notion of an allele and track their inheritance and the impact of genotype frequencies due to selection. In fact, many influential theoretical models of population genetics were developed well before the advent of molecular genetics. For some (though not all) purposes in current population genetics, a molecular gene concepts that tells you about the material structure of genes and how to get from genes to gene products is not important. In sum, *the inferential and explanatory role of the Mendelian gene concept is the prediction of patterns of inheritance and the explanation of phenotypic differences by means of genotypic differences*.

The transitional period between the Mendelian and the molecular gene concept is represented by those approaches in late Mendelian genetics that focused on the immediate causal products of genes. This is particular the case for biochemical understandings of Mendelian genes that viewed them as producers of biochemically active macromolecules. Well-known are Beadle and Tatum (1941) and Beadle (1946), who not only speculated that each gene produces exactly one enzyme, but offered relevant experimental evidence for this. Due to my cluster theory of concepts and the continuity of history there is often no matter of fact as to whether a geneticists in this transitional period really had a Mendelian or molecular (or rather an intermediate third) gene concept. Rather than giving a discussion of this transitional period, I focus on the contrast between the Mendelian gene concept and what is sometimes called the classical molecular gene concept, as we find it in the 60s.

According to the molecular concept, genes are stretches of DNA with a certain

structure and function. In contrast to what I reconstructed as the Mendelian gene concept, facts about the material and structural nature of genes are a necessary part of the inferential and explanatory potential of the molecular gene. Only in the context of the molecular gene do certain questions about genes make sense. The formulation of the colinearity hypothesis presupposes that the gene has a certain sequence which determines somehow the sequence of a polypeptide for which it codes. Similarly, the coding problem is a question about how the sequence of the molecular gene relates to the specific molecular substances it produces. Molecular biology is usually not interested in the explanation of patterns of inheritance. Instead, the explanatory focus is on understanding how genes influence and regulate biochemical, cellular, and developmental mechanisms. *The explanatory role of the molecular gene concept is to account for the production of molecular substances important for the cellular machinery such as RNA and polypeptides.* Once it was discovered that RNA (as for instance found in retroviruses) can be structurally and functionally analogous to DNA genes, the understanding of the gene slightly changed so that the inferential potential used by individual scientists came to include the statement that genes are DNA *or* RNA stretches (without creating a distinct group level abstraction of individual mental representations).

A structural understanding of genes is important for the way the molecular gene concept supports explanations. The molecular gene concepts refers to the way in which the structure of the DNA interacts with other molecular substances to synthesize its products. Molecular biology explains by means of the way in which various substances interact in mechanisms based on their structure-function relationships. The gene concept is by no means the only concept that is needed to give molecular explanations, but it is a necessary part of many explanations in molecular biology.

The crucial point for our purposes is that the Mendelian gene *concept* as such cannot fulfill this explanatory role (even though the entity referred to by this concept is the material substance that explains the phenomena molecular biology tries to account for), because the conceptual role of the Mendelian gene does not include a specification of the structure of genes and the way it functions in molecular mechanisms.

I view explanations in molecular biology as being based on general principles that I call 'explanatory strategies'. An explanatory strategy is any conceptual/theoretical schema that makes reference to certain structural entities (certain types of molecular entities such as genes or membrane channels) and their functional relationship such that this schema gives a general description and explanation of several concrete mechanisms or processes within and between cells. Thus, an explanatory strategy is a more abstract characterization that can be applied to concrete cases, it describes what several concrete mechanisms or pathways have in common. The *lac* operon model (Jacob and Monod 1961) is an example of an explanatory strategy. This model makes use of general molecular entities or concepts (operator, repressor) and gives an abstract characterization of their interaction so that a certain cellular process is explained. The crucial distinction is between coding regions and regulatory regions (such as promoters or enhancers). The other distinction is between structural and regulatory genes. The explanatory potential of the strategy consists in appealing to the products of regulatory genes that bind to regulatory sequences and thus influence the transcription of other genes (e.g., structural genes). A complete mechanism studied by molecular biology usually instantiates several different explanatory strategies. Explanatory strategies are combined and used to give explanations for concrete cases.[17] According to my conceptual role semantics account, the molecular

[17]My notion of an explanatory strategy is similar to Kitcher's (1993) notion of an explanatory schema. My account is more closely tied to the context of molecular biology be explicitly pointing out

gene concept makes reference to some of these explanatory strategies, for instance because it includes structural and functional properties of the gene such as the distinction between structural and regulatory regions and how they interact with other macromolecules to produce genetic products. In sum, *the molecular gene concept can be used to explain details about the molecular, physiological, and developmental effects of genes, whereas the Mendelian gene concept can only explain phenotypic differences.* Thus, this conceptual difference is an important instance of explanatory progress.[18]

This account was guided by the aim of contrasting the Mendelian with the molecular gene concept in order to discern conceptual progress. To this end, I delineated the Mendelian gene concept by abstracting from views about the material nature of Mendelian genes.[19] Historians of biology, however, are sometimes dissatisfied with this minimalist construal of the Mendelian gene, which characterizes it in functional terms only. The reason is that this account does not make intelligible why Mendelian genetics developed in a certain way and how molecular genetics could grow out of it in the first place. I indicated in the last section that there may be more than one way to individuate concepts, depending on the explanatory interests that underlie a particular study of conceptual change. If we are not primarily interested in the conceptual progress from the Mendelian to the molecular gene, but focus on the

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that explanatory strategies need not be quantitative and that many of them combine in explanations.

[18]In the recent past, some philosophers of biologists endorsed a strong, type-type reduction of the concepts of Mendelian genetics to molecular genetics, using Ernest Nagel's model of theory reduction (Gossens 1978; Schaffner 1993; Waters 1990), while others dissented (Hull 1976; Kimbrough 1978; Kitcher 1984). My account gives us a sense of progress from the Mendelian to the molecular genetics without having to assume that the Mendelian gene concept can be defined in terms of the concepts of molecular genetics.

[19]My account of the relation between the Mendelian and molecular gene is very similar to the account of Waters (1994). The difference is that Waters does not use a semantic theory or an account of what a concept it.

explanation of how Mendelian genetics developed, then another account has to be given along the following lines. Many Mendelian geneticists had strong views about the material nature of genes. These differences explain why these biologists chose to side with different research approaches and conduct different experiments. The exceptions to standard Mendelian patterns of inheritance that became known due to linkage, position effects, or variable expressivity, yielded insights into the structure and function of Mendelian genes and provided important clues for further experimental research. 'The' Mendelian gene concept as construed above abstracts from all these relevant differences. Thus if we are interested in explaining theoretical change, we have to make use of a more fine-grained scheme of individuation. Now we have to discern several Mendelian gene concepts, each of which embodies certain relatively specific views about the structure and function of genes. Each of these concepts was possessed only by a subset of Mendelian geneticists, and some individuals changed from one concept to another in their scientific career. This permits us to track the development of Mendelian genetics in a more fine-grained manner, and it puts us in a position to explain why certain historical developments occurred with reference to the different gene concepts used by different research groups. In sum, even if my discussion focused on the contrast between the Mendelian and the molecular gene, there may be other ways of individuating the Mendelian gene concept.

Conclusion

I criticized Kitcher's framework on conceptual change and progress that is based on the notion of reference potential. I questioned the practical applicability of Kitcher's notion to concrete cases. In addition, I challenged the idea that an account of conceptual change has to start out with an account of reference and that change in

reference potential captures the crucial aspects of conceptual change and progress. Kitcher's approach does not address how concepts contribute to scientific discovery, justification and explanation, and why novel concepts improve the ability to explain new types of phenomena. My rival approach used a conceptual role semantics that defines meaning and conceptual content in terms of the inferences and explanations supported by concepts. Conceptual change can be fruitfully studied by studying the change in the inferential and explanatory potential of concepts, and conceptual progress is to be evaluated in terms of the scientific significance of these new types of inferences and explanations. My case study delimited primarily two gene concepts, the Mendelian gene concept (which is still used in population genetics) and the molecular gene concept. Both differ in their explanatory potential. While the Mendelian gene concept can explain phenotypic differences by means genotypic differences, the molecular gene concept supports a direct explanation of characters by means of genes. Based on the idea that genes are stretches of DNA that figure in specific molecular processes, the molecular gene concepts offers an explanation of the developmental origination of traits (at least traits at the molecular level). Thus we get a clear sense of the type of progress that occurred from the transition of the Mendelian to the molecular gene concept. Note only did explanatory progress occur in the history of genetics, but the important point of my framework is that it permits us to say that this progress is due to the change of the gene concept.

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