An Alternative to Kitcher's Theory of Conceptual Progress and His Account of the Change of the Gene Concept

Ingo Brigandt

Department of History and Philosophy of Science
University of Pittsburgh
1017 Cathedral of Learning
Pittsburgh, PA 15260
USA

E-mail: inb1@pitt.edu

September 25, 2005

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 classical to the molecular gene than Kitcher's theory.

Philosophical accounts of the change of scientific concepts 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 of 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 shall 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 used — the gene concept. My goal is to show that my approach offers a better account of the progress that occurred in the transition from classical 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. The framework of 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 in 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 the work of Kuhn and Feyerabend, in particular the incommensurability problem. 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" (p. 522).

Kitcher (1978) offers an improved account of reference by taking a token or an utterance of a term as the unit of analysis. He introduces the notion of a *mode of reference of a term token*, which is the way in which a term token refers or the way in which reference is fixed for a term token. The motivation is that 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. Thus, different individuals may use different modes of reference to refer to the same kind; and even a person may make use of different modes of reference when uttering a term on different occasions. 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

[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.

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 a particular utterance of a term, the referent of the term is that category 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 dominant intention is to refer to an object or natural kind that is present a causal mode of reference obtains. Similarly, if the speaker intends 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. So far I have focussed on the idea that different (though co-referential) modes of reference may be used for different tokens of a term. However, Kitcher explicitly maintains that modes of reference for a term need not always be co-referential, so that a term may refer to different entities on different occasions. For instance, the term 'phlogiston' as used by the phlogiston chemist Priestley was sometimes non-referential (nothing satisfies the associated description). On other occasions, when Priestley described the effects of him breathing 'dephlogisticated air', he referred to oxygen (in accordance with the causal theory of reference).

In the case of a scientific concept, there are many descriptions that can be used to pick out a certain referent, and 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

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

if scientists acquire new ways of referring to or picking out a known entity, or it may contract in case a former mode of reference is deemed to be problematic and abandoned. 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.

There are some unsolved internal issues that Kitcher's account raises, suggesting that his notion of reference potential has to be elaborated. One open issue arises from Kitcher's notion of descriptive modes of reference. Not every statement or description of an entity is reference-fixing. 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 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 them without potentially changing the reference of the term. Reference-analytic statements fix meaning in the sense of reference. The second type of statements 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 (or causal modes of reference), it is possible to make false statements that are still about the same referent, which is philosophically important. The distinction between reference-analytic and reference-synthetic statements is important for Kitcher, because on his account conceptual change is not just about changes in the set of statements endorsed. More specifically, conceptual change is change in reference potential, i.e., about the addition and deletion of *modes of reference*. So one has to be in a position to tell whether a statement is a descriptive mode of reference, i.e., reference-*analytic*. Furthermore, Kitcher's account of conceptual progress (to be discussed below) assumes that conceptual progress consists in the addition of new descriptive modes of reference and the deletion of problematic descriptive modes of reference. In my terminology, conceptual progress is the acceptance or abandonment of reference-*analytic* statements. The upshot is that Kitcher is committed to a distinction between reference-analytic and reference-synthetic statements. 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).[3]

The unsolved issue is 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 bear on what determines reference. 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 and their intentions may be based on empirical misconceptions. 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) previously criticized Kitcher's idea that in each situation there is a dominant intention of the speaker that settles the referent and the mode of reference. Psillos's point is that a scientist 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.[4] Likewise, a scientist may erroneously assume that different theoretical descriptions are identical and pick out the same kind. Due to such a misconception, a scientist's intention to use one and at the same time the other description is contradictory, though she is not aware of this. In this case, it is unclear which of the two descriptions is actually a mode of reference. Kitcher assumes that there is a clear-cut matter of fact whether or not a description is a mode of reference. But so far Kitcher's theory is not elaborated enough to yield

[3] 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 genuine analytic/synthetic distinction: it distinguishes meaning-constitutive from other statements.

[4] "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. ... 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 [about phlogiston]. 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)

a notion of reference-analyticity that adequately defines in virtue of which features a description 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 example, Kitcher concludes: "'What is in the speaker's head' does not therefore determine 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 reference 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.[5] 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 funda-

[5]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)

mentally 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 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 mode of reference cuts finer than extension. This strategy of resolving the tension has the drawback that one 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 19th century scientists. 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.[6]

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.

---

[6]A related issue is that a standard constraint on a theory of concepts is that concepts are shared by different persons. For concepts make propositional attitudes possible, and propositional attitudes have to be shared in order to underwrite intentional explanations of behavior (Fodor 1994). But given that Kitcher views causal modes of reference as involving various events outside the head that may differ between persons (scientists in different countries used different initiating events to introduce a concept), it is not fully clear how concepts can be shared on this approach.

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

Now I 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 then 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 with an account of reference. He supplements his theory to obtain 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 main concern for an account that is intended to explain why conceptual change rationally occurred and to assess whether it was progressive. 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). An account of reference 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 an account of reference and reference potential, but Kitcher hardly discusses such issues. He 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 divergent 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 brings about fundamental epistemic problems. Thus, 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 focusing on 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 differences in meaning (Shapere 1966; Burian 1985). But 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 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 meaning.

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

[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 one rejects the second step, then one 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.

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 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 classical to molecular genetics. The brief history of the gene that Kitcher discusses picks out very interesting points. But it is hardly an application of his philosophical framework. Kitcher mentions only

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

[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).

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 reference. 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 or causal modes of reference. In fact, these three statements are non-equivalent and pick out different categories (and they do not pick out genes as they are understood nowadays). Thus it is unlikely that *all* of them were descriptive modes of reference (unless one explicitly argues that the reference of the term gene constantly changed during history). At any rate, Kitcher does not explain why he views these descriptions as reference-fixing, and as long as we do not have a *prima facie* idea of why we are dealing in these concrete cases with modes of reference (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 classical 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 reference even at a particular point in history, then we need a detailed account of how several new modes of reference emerged rather than three isolated examples. In sum, Kitcher's framework that commits us to detect and study modes of reference (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* suggests that Kitcher's identification of concepts with reference potentials leaves out some important aspects of conceptual change and progress. Kitcher's account is designed rebut the incommensurability 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. One aim for the study of conceptual change is to track the emergence of novel concepts and to analyze if a term is used with differ-

ent meanings. For instance, it is widely recognized that the term 'species' nowadays corresponds to different concepts. In the next section I shall claim that currently we still use the classical 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 and pinpoint when one or several novel concepts emerged. Kitcher's notion of reference potential is his proxy for the notion of 'concept' or 'meaning of a term'. However, for Kitcher there is only a term and its reference potential at a particular time, which does not allow us to keep different concepts apart that correspond to the same term at a certain point in history. As his account stands, Kitcher can note that the contemporary reference potential of the term 'gene' (or 'species') is highly heterogeneous. But this ignores that some conceptual variation is best viewed as being due to the existence of several concepts or senses being associated with the same term. Kitcher would need a way of breaking a total reference potential down into different concepts. In sum, 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 a 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 classical gene concept could be viewed in this way. Recognizing substantial conceptual change (that justifies the postulation of a new concept) is important for *assessing conceptual progress*. On Kitcher's account, conceptual change, i.e., a change of the reference potential of a term, is progressive either if a new descriptive mode of reference is added to the reference potential, or if a problematic descriptive mode of reference is abandoned.[10] A minor drawback of this account of conceptual progress is that Kitcher counts any addition to a reference potential as progressive. However, not every change is equally important; and later I will later make use of an account of

[10]Conceptual progress occurs 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." (Kitcher 1993, p. 105)

concepts that can reserve the notion of progress to substantial conceptual changes that have an impact of the scientific discipline under consideration. Moreover, Kitcher's account of conceptual change and progress follows from and depends on his tenet that concepts are reference potentials. Descriptions that are not modes of reference are not part of the reference potential and thus *by definition* cannot 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? Thus, Kitcher does not have a complete defense of his particular definition of conceptual progress — independent of the idea that concepts are sets of modes of reference. Adding new modes of reference and eliminating problematic modes of reference may be all there is to progress regards the change of *reference potentials*. But my central critique of Kitcher's account of progress is that this does not exhaust conceptual progress in science.

What is crucial about conceptual change is that certain changes permit scientists to conduct discovery in a better manner, to justify new hypotheses, and to explain new ranges of phenomena. Kitcher is not interested in this dimension of conceptual change, his account of concepts is independent of how new or modified concepts contribute to scientific discovery, inference and explanation. Let me give some examples that illustrate why I view this feature as the main impact of conceptual progress on science. First, conceptual progress may occur in the case of the introduction of a completely new term, provided that this concept allows scientists 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 count the introduction of a new term as progressive. Moreover, 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 an evolutionary process (by means of causal modes of reference), but it played a fundamental role for the explanatory success of Darwinian evolutionary theory. Second, there are natural kind concepts. As is well known,

natural kinds have projectable properties. If some instances of a natural kind have a certain property, then other instances are likely to have the same property. Likewise, if an instance of a natural kind has a certain property, it is likely to have some other specific properties (Boyd 1991). For this reason, natural kind concept support important inductive inferences (inferences from some instances to other instances, or from some properties to other properties). *Semantic* accounts of natural kind terms, however, have focussed on their reference, in particular using the causal theory of reference. (Kitcher uses the notion of causal modes of reference.) But there is more to scientists possessing a natural kind concept than their ability to refer to a natural kind: in virtue of possessing natural kind concepts, scientists are able to carry out reliable and significant inferences. In this sense, scientific concepts support inferences, and improved concepts permit scientists to make new inferences and justify novel assumptions. Third, a similar point applies to how concepts support explanations. In my later case study on the gene concept I shall argue that the change from the classical to the molecular gene concept is progressive precisely because the molecular gene concept supports certain types of explanations that the classical gene concept cannot offer.

Kitcher defines concepts as sets of modes of reference, thereby viewing concept possession as an individual's *ability to refer to a category*. Consequently, conceptual progress is identified with scientists finding new ways of referring to an old referent. However, I emphasized that in addition to this, another crucial aspect of conceptual progress is that improved concepts enable scientists to carry out discovery in a more effective manner, to justify novel hypotheses, and explain new ranges of phenomena. Consequently, my suggestion is to take into account that concept possession also consists in the *ability to carry out inferences and explanations*. And I suggest studying conceptual change and progress in these terms. Kitcher (1993) focuses on two types of scientific progress: conceptual and explanatory progress (he characterizes the latter based on his notion of an explanatory schema). I agree with Kitcher that explanatory progress is of fundamental importance for science (and should be for the philosophy of science). 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: explanatory progress often occurs because novel or improved concepts become available. It is not easy to see how reference potentials can contribute to explanations, which ultimately stems from the fact that a referential account of concepts takes a 'meaning is outside the head' approach. (As mentioned in the previous section, it is unclear how concepts thus conceived can figure in scientific reasoning such as giving explanations and justifying hypotheses.) But concepts definitely do contribute to explanations, so the notion of reference potential is likely to miss an important aspect of concepts and conceptual progress. 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

The 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 framework is primarily designed to address the incommensurability problem — consequently he focuses on reference and views concepts as reference potentials. My own approach is not concerned with incommensurability; instead I focus on some further important tasks for the study of conceptual change. 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 in a rational fashion; 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 improving discovery, justification, and explanation. Consequently, I intend to work towards a framework of concepts that directly addresses the above three desiderata. Above I pointed out that while Kitcher views concept possession as the ability to refer to a category, concept possession also includes the ability to put forward inferences and explanations. For this reason, the account I present here is

a version of *conceptual role semantics*, which roughly defines conceptual content in terms of the inferences and explanations supported by concepts.

In this paper, I cannot offer a detailed account of my framework of concepts. Rather than offering a general semantic discussion and defense of this approach, my strategy is to illustrate the fruitfulness of the framework for the study of conceptual change by applying it to a concrete case in the next section — the history of the gene concept. This section presents the basic semantic features of my approach.[11]

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; McGinn 1982; Sellars 1953; Wedgwood 2001). 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

[11]I also cannot spell out the relationship between my and Kitcher's framework, and how reference figures in my conceptual role semantics. In a nutshell, I view my approach as complementary to but broader than Kitcher's referential framework. I follow Anil Gupta's (1999) critique of purely representational semantics. Gupta rightly stresses that the basic task of semantics is to account for how concepts underwrite successful practice; and that scientific concepts may figure in largely successful practice *even if* they involve misconceptions (e.g., if different empirically non-equivalent criteria are associated with the concept). Gupta points out that a representational semantics — which intends to assign referents to terms and truth-values to sentences — cannot assign determinate truth values to statements involving misconceptions, or in any case such an assignment does not explain how such statements still can be good guides to action. Consequently, Gupta suggests a prior step in semantic analysis. The first step is to assign something like a conceptual role to a term, which takes into account how a concept is used in actual practice. It is this first type of content which accounts for how concepts underwrite successful practice, while only in a second step is the representational dimension of content taken into account by assigning a referent (which may not be determinately possible in the case of a concept involving misconceptions). In a similar vein, I assume that taking conceptual role into account offers a broader semantic account, as it is the primary semantic property that explains how scientific concepts figure in successful research and how they can rationally change. I view referential features of concepts such as Kitcher's modes of reference as a secondary semantic property that contain less semantic information than conceptual roles. In what follows, I will focus on the first feature, without saying much about reference.

(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. I 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 one has 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*, 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 particular 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). 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 may be crucial for explaining a specific class of processes or situations, while in order to account for other phenomena different concepts have 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) argued that explanations are not arguments (neither deductive nor inductive), 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 shall argue that the crucial difference between the classical and the molecular gene concept is that the molecular gene concept supports explanations that the classical gene concept cannot give.[12]

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 holistic, 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 associate a different idiolect-meanings 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 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

[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. I actually assume that the conceptual role of a term also includes language-world relations, e.g., how a term is applied to objects. Furthermore, I assume that reference is also a semantic property of concepts. But since my goal is to study differences in certain scientific concepts rather than putting forward a general theory of content I cannot discuss these semantic issues at this point.

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

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. My version of conceptual role semantics does not endorse a holism about meaning *individuation* (it is not claimed that a concepts is a holistic conceptual role). Rather, I endorse a holism about meaning *determination* (the meaning of a concept supervenes on and is determined by conceptual role).

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 or not something is a mode of reference 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, in contrast, acknowledges that some concepts can be individuated in different ways. 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). I do not assume that there is a unique and principled distinction between real concepts and mere variants of a concept. Starting with the 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 may be legitimate. On this approach, the philosophical burden is not to defend a certain analytic-synthetic distinction. Rather, for each instance of concept individuation the philosophical task is to show that this way of individuation sheds light on conceptual change or progress. On my account, a central guideline is to individuate a scientific concept in terms of those inferences and explanations that account for the successful use of this concept in communication and especially in scientific practice. The following case study will illustrate this idea.[14]

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 scientists of a given community that account for the successful use of this concept). 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 the novel inferences and explanations supported by a new or modified concept contribute to science. Rather than studying additions and deletions of modes of reference, I focus on some features of scientific activity that directly matter to advance in science — inferring and justifying hypothesis and giving explanations. The final section applies this framework to the concept of the gene.

The classical 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.

[14] Another guideline is to individuate a concept based on the epistemic purposes for which it is used. A particular research program uses its concept to pursue certain explanatory goals and to obtain a particular epistemic product — characteristic type of inferences and explanations. In another paper (Brigandt unpubl.), I use this framework to *explain* how conceptual change of some biological concepts *rationally* occurred. Conceptual change is rational to the extent to which a modified concept is able to yield the intended epistemic product in a better fashion. The emergence of novel concepts can be rationally explained based on the emergence of new research approaches that have different goals. The current study focuses on assessing conceptual progress, though.

Instead, I will pick out one issue that illustrates how my approach can be applied in practice and what philosophical insights it can yield about the history of science. The focus is on characterizing the classical vs. the molecular gene concept. I will explain what the conceptual differences between the 'old' and the 'new' gene concept are and in what sense we have a clear instance of conceptual progress.[15]

During the history of classical genetics, especially its early history, the views about genes changed relatively rapidly. And even at a particular time, geneticists differed widely as regards their beliefs about the nature and material 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 rejected the idea that Mendelian genes are parts of the chromosome, and assumed that genes are abstract entities or — like Goldschmidt (1940) — that the chromosome is the unit of genetic function. 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 can be viewed as a difference between the mental representations each individual had for the term 'gene'. Individuals may differ in their conceptions of a phenomenon or an entity. In other words, due to the widespread disagreement about the material nature of genes, the conceptual role of terms such as 'gene', 'factor', and 'allele' — taken as a property of individuals — exhibited interpersonal variation.[16]

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 dif-

[15]My account of the difference between the classical and molecular gene concept is 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.

[16]Carlson (1966) 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).

ferent 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 genetic 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 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 smallest unit of physiological 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.[17] There are different ways in which a concept can change, and studying the conceptual role of a term suggests ways in which it might change.

Despite these between-person differences, the crucial step in my account is to delimit the classical gene concept by certain inferences and explanations in which the term 'gene' figured and that were widely endorsed by geneticists — so as to account for the successful use of this concept in biological practice. On my account, particular beliefs about the material nature of genes were not constitutive of the classical gene concept. Instead, classical genes were defined in terms of their role in *phenotypic transmission between generations*. The inferences that characterize the classical gene concept are those that specify how genes bring about *patterns of inheritance*. These are refined versions of Mendel's original principles of inheritance. Patterns of inheritance are predicted and explained by inferring from the genotypes of the parental generation the distribution of genotypes and thus phenotypes of the following generation. This prediction is possible because the inferential role of the classical gene concept includes an account of how genes behave in processes such as segregation and linkage (which entails the genotype distribution of 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 classical gene can be found from the

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

early history of genetics onwards (it is for instance clearly expressed by Morgan et al. 1915), and continued to be of crucial importance throughout the history of classical genetics.

I view these inferences as constitutive of the classical gene concept because they were strongly embedded in the practice of classical geneticists, who among other things carried out segregation and linkage analysis. In addition, these inferences yielded the epistemic and experimental access to classical genes. Whenever a trait mendelizes (yields a Mendelian pattern of inheritance), a geneticist has to agree that this 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. Whatever the material nature of genes, classical geneticists agreed on the fact that patterns of inheritance are to be predicted and explained by the inheritance of genetic factors or alleles. To be sure, the understanding of the classical gene needed to be refined once exceptions to the simple patterns of inheritance became clear due to position effects and variable expressivity. The conceptual role 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 classical gene.

Given this, the classical gene concept underwrites the prediction of patterns of inheritance. The role of the classical gene concept in the explanation of phenotypic traits is as follows. Geneticists knew that the relation between genes and traits is many–many (Morgan et al. 1915). They could not explain the development of traits, because the large sets 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 the development of characters by the action of genes was impossible. But classical genetics could explain phenotypic *differences* by means of genotypic *differences*. For instance, one can explain why a fruit fly has orange eyes (which differs from the normal eye color) with reference to the fact that a certain gene is mutated (the fly has a *cinnabar* rather than the wild-type allele).

Such an account does not causally explain how a certain eye color develops based on the interaction of various molecular substances produced by several genes. Rather, it explains phenotypic differences based on genotypic differences relative to a certain normal genetic, cellular and environmental background. While falling short of a direct explanation of how characters originate, these explanations are useful. For instance, population genetics originated long before the advent of molecular biology, and this field is an example of something very much like the classical gene concept still being used. For the purpose of studying and explaining the change of phenotype distributions in natural populations, it is sufficient to use the classical notion of genes and track changes in genotype frequencies due to selection and their phenotypic impact. The basic reason is that natural selection acts on phenotypic *differences* among individuals that are due to genetic *differences*. For some (though not all) purposes in current population genetics, a molecular gene concepts that contains information about the material structure of genes and how genes bring about their products is not important. In sum, *the inferential and explanatory potential of the classical gene concept consist in the prediction of patterns of inheritance and the explanation of phenotypic differences by means of genotypic differences.*

The transitional period between the classical and the molecular gene concept begins with those approaches in late classical genetics that focused on the immediate causal products of genes. This is particular the case for biochemical understandings of classical genes that viewed them as producers of biochemically active macromolecules. Well-known are Beadle and Tatum (1941), 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 classical 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 classical gene concept and the molecular gene concept, as we find it in the 70s.[18]

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

[18]Nowadays this often called the 'classical' as opposed to the 'contemporary' molecular gene concept. I cannot discuss the change of the molecular gene concept that occurred in the last two decades.

structure and function. In contrast to what I reconstructed as the classical gene concept, beliefs about the material nature of genes are a part of the inferences and explanations that characterize the molecular gene concept. Only in the context of the molecular gene concept 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. 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 role 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).

Unlike classical genetics, 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. A central epistemic aim pursued with the use of the molecular gene concept is to account for the production of molecular substances important for the cellular machinery such as RNA and polypeptides. A structural understanding of genes is important for the way the molecular gene concept supports explanations, as the molecular structure of genes explains which causal effects and molecular functions they have. On my account, the molecular gene concept 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 is that the classical gene *concept* as such cannot fulfill this explanatory role, because the conceptual role of the classical 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 or causal 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 made is between coding regions and regulatory regions (such as promoters or enhancers). Another important distinction keeps apart structural and regulatory genes. The explanatory impact 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.[19] According to my conceptual role semantics account, the molecular gene concept embodies several of these explanatory strategies, for instance because it includes inferences that specify the structural and functional properties of genes such as the distinction between structural and regulatory regions and how they interact with other macromolecules to produce genetic products.

This account of the content of the molecular gene concept shows that this concept supports the explanation of details about the molecular, physiological, and developmental effects of genes. In contrast, the classical gene concept can only explain phenotypic differences. Thus, this conceptual difference is an important instance of explanatory progress: the meaning of the term 'gene' changed in the transition from classical to molecular genetics so as to lead to a substantial increase in explanatory

[19]The notion of an explanatory strategy is similar to Kitcher's (1993) notion of an explanatory schema. My account is more general and more closely tied to the context of molecular biology by assuming that explanatory strategies need neither be deductive arguments nor quantitative. In addition, I stress that many of them combine in explanations.

potential.[20] While it is not my primary focus, I still want to conclude with a remark on reference. Genes do not form a single category and there is actually more than one kind to which biologists have referred to by using the term 'gene' (Burian et al. 1996). In particular, the classical and the molecular gene refer to overlapping, yet non-identical kinds in that there are classical genes that are not molecular genes and vice versa (Weber 2004). Consequently, the reference of the term 'gene' shifted somewhat with the origin of the molecular genetics. However, many accounts of conceptual change have assumed that stable reference is a necessary precondition for conceptual progress to occur, the idea being that only in this case can one say that later scientists have an improved conception of the very same entity that earlier scientists referred to. In spite of change of reference, I do maintain that the transition from the classical to the molecular gene concept is a genuine case of conceptual progress. For I view the shift in reference as a mere by-product of progressive meaning change. Change in reference occurred unbeknownst to geneticists, but it proved to be successful as it was a side-effect of an increase in the explanatory potential of the term 'gene'. Consequently, my suggestion is to make room for accounts of conceptual progress that do not necessarily presuppose unchanging reference. Both meaning change and reference change can be progressive if there are good reasons for such semantic changes and provided that such semantic changes advance scientific theory and practice.[21]

---

[20] This way of individuating gene concepts was guided by the aim of contrasting the classical with the molecular gene concept in order to discern conceptual progress. To this end, I delineated the classical gene concept by abstracting from views about the material nature of classical genes. However, another scheme of individuation may be necessary to explain why classical genetics developed in a certain way and how molecular genetics could grow out of it in the first place. If the aim of semantic analysis is to explain conceptual change (rather than to exhibit conceptual progress), then one has to discern several classical 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 classical geneticists. This permits the philosopher to track the development of classical genetics in a more fine-grained manner, and it puts her in a position to explain why certain historical developments occurred with reference to the different gene concepts used by different research groups. In this paper I cannot discuss this alternative way of individuating gene concepts.

[21] In the recent past, some philosophers of biology — largely unsuccessfully — argued for a strong, type-type reduction of the concepts of classical genetics to molecular genetics. My account gives us a sense of progress from classical to the molecular genetics without having to assume that the classical gene concept can be defined in terms of the concepts of molecular genetics.

Conclusion

I criticized Kitcher's framework of conceptual change and progress that is based on the notion of reference potential. In addition to questioning the practical applicability of Kitcher's notion to concrete cases, 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 all 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. While Kitcher's approach views concept possession as the ability to refer to a category, I stressed that concept possession also implies the ability to put forward inferences and explanations. My suggestion was to work towards a broader account of scientific concepts that takes this aspect of concepts into account, so as to account for how concepts underwrite successful scientific practice. My alternative 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 classical gene concept (which is still used in population genetics) and the molecular gene concept. Both differ in their explanatory potential. While the classical gene concept can explain phenotypic *differences* by means genotypic differences, the molecular gene concept supports a direct, causal-mechanistic 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 supports explanations of the developmental origination of traits (at least traits on lower levels of organization). Thus we get a clear sense of the type of progress that occurred from the transition of the classical to the molecular gene concept. Not only did explanatory progress occur in the history of genetics, but the important point is that my framework permits us to say that this progress occurred in virtue of change of the gene concept.

Acknowledgements

I would like to thank Dick Burian, Anil Gupta, Paul Griffiths, Jim Lennox, Alan Love, Bjørn Ramberg, and Nils Roll-Hansen for comments on earlier versions of this paper or discussions on conceptual change and the history of genetics.

Bibliography

Andersen, H. (2001). Reference and resemblance. *Philosophy of Science 68 (Proceedings)*, S50–S61.

Beadle, G. W. and E. Tatum (1941). Genetic control of biological reactions in Neurospora. *Proceedings of the National Academy of Sciences USA 27*, 499–506.

Benzer, S. (1957). The elementary units of heredity. In W. D. McElroy and B. Glass (Eds.), *A Symposium on the Chemical Basis of Heredity*, pp. 70–133. Baltimore: John Hopkins Press.

Block, N. (1986). Advertisement for a semantics for psychology. In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein (Eds.), *Studies in the Philosophy of Mind. Midwest Studies in Philosophy, Vol. 10*, pp. 615–678. Minneapolis: University of Minnesota Press.

Boyd, R. (1991). Realism, anti-foundationalism and the enthusiasm for natural kinds. *Philosophical Studies 61*, 127–148.

Boyd, R. (2002). Scientific realism. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy*. http://plato.stanford.edu/archives/sum2002/entries/scientific-realism/ (Summer 2002 Edition).

Brandom, R. B. (1994). *Making It Explicit: Reasoning, Representing, and Discursive Commitment*. Cambridge, MA: Harvard University Press.

Brigandt, I. (unpubl.). The role a concept plays in science — The case of homology. Manuscript, www.pitt.edu/~inb1/role.pdf.

Burian, R. M. (1985). On conceptual change in biology: The case of the gene. In D. J. Depew and B. H. Weber (Eds.), *Evolution at a Crossroads: The New Biology and the New Philosophy of Science*, pp. 21–42. Cambridge, MA: MIT Press.

Burian, R. M., R. C. Richardson, and W. J. Van der Steen (1996). Against generality: Meaning in genetics and philosophy. *Studies in History and Philosophy of Science 27*, 1–29.

Carlson, E. A. (1966). *The Gene: A Critical History*. Philadelphia: W. B. Saunders.

Devitt, M. (1979). Against incommensurability. *Australasian Journal of Philosophy 57*, 29–50.

Devitt, M. and K. Sterelny (1999). *Language and Reality: An Introduction to the Philosophy of Language*. Cambridge, MA: MIT Press.

Field, H. (1977). Logic, meaning, and conceptual role. *Journal of Philosophy 74*, 379–408.

Fine, A. (1975). How to compare theories: Reference and change. *Noûs 9*, 17–32.

Fodor, J. A. (1994). *The Elm and the Expert: Mentalese and its Semantics*. Cambridge, MA: MIT Press.

Goldschmidt, R. (1940). *The Material Basis of Evolution*. New Haven: Yale University Press.

Gupta, A. (1999). Meaning and misconceptions. In R. Jackendoff, P. Bloom, and K. Wynn (Eds.), *Language, Logic, and Concepts*, pp. 15–41. Cambridge, MA: MIT Press.

Hacking, I. (1983). *Representing and Intervening*. Cambridge: Cambridge University Press.

Harman, G. (1973). *Thought*. Princeton: Princeton University Press.

Harman, G. (1982). Conceptual role semantics. *Notre Dame Journal of Formal Logic 23*, 242–256.

Horwich, P. (1998). *Meaning*. Oxford: Clarendon Press.

Jacob, F. and J. Monod (1961). On the regulation of gene activity. *Cold Spring Harbor Symposia on Quantitative Biology 26*, 193–211.

Kitcher, P. (1978). Theories, theorists, and theoretical change. *The Philosophical Review 87*, 519–547.

Kitcher, P. (1982). Genes. *British Journal for the Philosophy of Science 33*, 337–359.

Kitcher, P. (1993). *The Advancement of Science: Science without Legend, Objectivity without Illusions*. Oxford: Oxford University Press.

Leplin, J. (1979). Reference and scientific realism. *Studies in History and Philosophy of Science 10*, 265–284.

Levin, M. (1979). On theory-change and meaning-change. *Philosophy of Science 46*, 407–424.

Martin, M. (1971). Referential variance and scientific objectivity. *British Journal for the Philosophy of Science 22*, 161–170.

McGinn, C. (1982). The structure of content. In A. Woodfield (Ed.), *Thought and Object: Essays on Intentionality*, pp. 207–258. Oxford: Oxford University Press.

Miller, R. (1987). *Fact and Method*. Princeton: Princeton University Press.

Morgan, T. H., A. H. Sturtevant, H. J. Muller, and C. B. Bridges (1915). *The Mechanisms of Mendelian Heredity*. New York: Henry Holt.

Muller, H. J. (1962 [1926]). The gene as the basis of life. In *Studies in Genetics: The Selected Papers of H. J. Muller*, pp. 188–204. Bloomington: Indiana University Press.

Newton-Smith, W. H. (1981). *The Rationality of Science*. Boston: Routledge & Kegan Paul.

Papineau, D. (1987). *Reality and Representation*. Oxford: Blackwell.

Psillos, S. (1997). Kitcher on reference. *International Studies in the Philosophy of Science 11*, 259–272.

Psillos, S. (1999). *Scientific Realism: How Science Tracks Truth*. London: Routledge.

Putnam, H. (1973). Explanation and reference. In G. Pearce and P. Maynard (Eds.), *Conceptual Change*, pp. 199–221. Dordrecht: Reidel.

Putnam, H. (1975). The meaning of 'meaning'. In H. Putnam (Ed.), *Mind, Language and Reality: Philosophical Papers, Volume 2*, pp. 215–271. Cambridge: Cambridge University Press.

Salmon, W. (1970). Statistical explanation. In R. G. Colodny (Ed.), *The Nature and Function of Scientific Theories*, pp. 173–231. Pittsburgh: University of Pittsburgh Press.

Sankey, H. (1994). *The Incommensurability Thesis*. Brookfield: Avebury.

Scheffler, I. (1967). *Science and Subjectivity*. Indianapolis: Bobbs-Merrill.

Schiffer, S. (1981). Truth and the theory of content. In H. Parret and J. Bouveresse (Eds.), *Meaning and Understanding*, pp. 204–222. Berlin: Walter de Gruyter.

Sellars, W. (1953). Inference and meaning. *Mind 1962*, 313–338.

Shapere, D. (1966). Meaning and scientific change. In R. G. Colodny (Ed.), *Mind and Cosmos*, pp. 41–85. Pittsburgh: University of Pittsburgh Press.

Stadler, L. J. (1954). The gene. *Science 120*, 811–819.

Waters, C. K. (1994). Genes made molecular. *Philosophy of Science 61*, 163–185.

Weber, M. (2004). *Philosophy of Experimental Biology*. Cambridge: Cambridge University Press.

Wedgwood, R. (2001). Conceptual role semantics for moral terms. *Philosophical Review 110*, 1–30.