CHAPTER 9

Lipton and Inference to the Best Explanation

Harman commented that, "When a scientist infers the existence of atoms and sub-atomic particles, he is inferring the truth of an explanation for various data which he wishes to account for." Since then, philosophers interested in IBE have frequently proposed that the use of IBE is pervasive in both everyday life and in science.[1] Despite the familiarity of this claim, there is typically little attention to details about how IBE relates to scientists, experiments, or journal articles. So, in theory, one can say that IBE is used in science without being committed to the details of IBE's relation to the scientific interpretation of the results of controlled experiments. Lipton does not take this path. He proposes that IBE is pervasive, but also commits to the applicability of his account to the scientific interpretation of experimental results. Section 9.1 will review Lipton's aspirations for his account of IBE.

Lipton's account of the scientific interpretation of the results of controlled experiments draws together views on contrastive explanations, Mill's Method of Difference, and controlled experiments. In using contrastive questions, Lipton draws on a tradition often traced to Bas van Fraassen's contention that explanation-seeking why-questions have the normal form, "Why P, but rather than Q?" Lipton links such questions to Mill's Method of Difference. Finally, Lipton proposes to link this to a controlled experiment in which a scientist asks, "Why is the mortality rate high in Clinic 1, rather than low?" Although many of the issues Lipton discussed have disappeared from more recent treatments of IBE, I review them here because they may serve as a useful foil for my views. At the very least, they may dull the suggestion that my account of the scientific interpretation of controlled experiments merely imports Lipton's account. Section 9.2 will review this.

On Lipton's version of IBE, scientists select the likeliest possible explanation of some phenomenon or phenomena based on which possible

[1] See, for example, Van Fraassen (1978), Lipton (2003), Bird (2005), McMullin (2013), Douven (2017a), McCain and Poston (2017b), and Park (2022).

explanation is the loveliest.[2] By the loveliest explanation, he means the explanation that provides the most understanding. By the explanation that provides the most understanding, he means the one that ranks best in terms of the explanatory virtues of mechanism specification, precision, scope, simplicity, fertility, and fit with background beliefs. One shortcoming of Lipton's discussion is that he essentially rehearses familiar ideas in the literature, rather than investigating how those familiar ideas are applicable to the scientific interpretation of an experimental result. Section 9.3 will review this.

9.1 The Scope of Lipton's IBE

In Chapter 1, I described the target of my theory: the scientific interpretation of certain experimental results given in the primary experimental literature. There I distinguished this project from that of describing the psychology of individual scientists as might be in play in the inferences scientists make in the laboratory. In the discussion of Harman (1965), I also distinguished this project from a project of theory choice, as illustrated in Chapter 8 with the example from Thagard (1978). Lipton (2003), however, has more sweeping aspirations for his theory of IBE than I have for my theory of singular compositional abduction. At times, his IBE model is intended to be an account of human psychology. At other times, it is a model for the scientific interpretation of individual experimental results. At still other times, it is a model of theory choice. Consider how these applications appear in his discussion.

In the introduction to his book, Lipton comments, "it is one thing to be good at doing something, quite another to understand how it is done or why it is done so well. It is easy to ride a bicycle, but hard to describe how it is done" (Lipton, 2003, p. 1).[3] Lipton's point is that it is hard to provide a philosophical characterization of IBE, because the mechanisms of IBE are part of a subconscious, maybe innate, human psychological endowment. In this regard, Lipton's view may be a precursor to Schurz's beachcomber story. In one particularly striking moment, Lipton comments, "We want to determine how illuminating that account is as a partial description of the mechanism inside the cognitive black box that governs our inductive practices" (Lipton, 2003, p. 71).[4]

[2] Lipton (2003, p. 60). [3] Cf., Lipton (2003, p. 12).
[4] See also Lipton (2003, pp. 19, 28, 61, 105, 109, 119f).

Maybe an infant's acquiring a grammar from primary linguistic data is in some respects like what Hodgkin and Huxley are doing in their interpretation of their experimental results in the experimental literature. Then again, it may differ in detail. Suppose that it is part of an infant's innate biological endowment to assume that it will be exposed to a natural language that contains nouns. In that case, the infant may never have to go through a process of weighing evidence to decide that the best explanation of the primary linguistic data is that there is a language containing nouns. Instead, the infant is "hard-wired" never to entertain the hypothesis that it is being exposed to a language without nouns. That might be labeled "IBE," but historians and philosophers of science should not simply assume that Hodgkin and Huxley are as innately "close-minded" in their thinking about the sodium hypothesis as are the infants in acquiring their natural language. Hodgkin was, for a time, resistant to the sodium hypothesis, but he did come around in time.

Although Lipton sometimes treats IBE as a psychological model, the preponderance of his discussion appears to be directed to what I mean by the scientific interpretation of the results of controlled scientific experiments. In chapter 3 of his book, he develops a theory of contrastive explanation that, in his chapter 5, he applies to Semmelweis's reasoning regarding childbed fever. A contrastive question has a fact-foil structure, "Why P, but not Q?" or "Why P, rather than Q?" In that chapter, Lipton relates such questions to controlled experiments by asking, "Why did the women in clinic 1 have a high mortality rate, whereas the women in clinic 2 did not?" This is the topic I will take up in Section 9.2.

Despite the attention Lipton pays to the application of IBE to controlled experiments in Lipton (2003), Lipton (1998) contains some comments that resist that interpretation. In an exchange that has received little attention in the abduction/IBE literature, Suppe (1998a) argued that IBE does not provide a good account of the structure of a scientific paper. In reply, Lipton (1998) comments that,

> First, the core idea that scientists argue for hypotheses by revealing their explanatory power does not require the consideration of alternatives, and in some cases we should not expect articulated alternatives to be on offer. . . . Second, even when there is more than one hypothesis on offer, we should not expect them all to appear in a single paper: different scientists may press the case for different hypotheses, without considering the opposition. (Lipton, 1998, p. 408)

I think that Lipton is exactly right on these two points. His reply is just what I was noting in my reply to Schurz's beachcomber example. Lipton

recognizes that, at a given moment in time, a scientist may only entertain one hypothesis. Further, he recognizes that the confirmation of some hypothesis over rivals might only emerge over a period of extended scientific investigation.[5] I also think that, in his rejoinder, Suppe aptly comments that, "This is a surprising move" (Suppe, 1998b, p. 423). The comment is apt since there is little in the rest of Lipton's work that would have suggested his reply.

Finally, Lipton suggests the possibility of applying IBE to theory choice when it comes to issues of the putative incommensurability of competing theories and the interpretation of so-called crucial experiments.[6] In a nutshell, a crucial experiment is one in which theory T_1 explains some result that theory T_2 does not. Incommensurability, however, threatens to block this interpretation, since the different theories have different results. Incommensurability proposes that T_1 explains one set of results, whereas T_2 explains a different, nonoverlapping set of results. Thus, both theories explain results that the other does not, so there is no one experimental result to serve as a crucial experiment. Lipton's proposed resolution is to look at T_1's explanation of its data and T_2's explanation of its data, and then pick the theory that offers the best explanation of its results.[7] Thus, Lipton has IBE applied to theory choice.

As suggested in Chapter 1 and in my discussion of Harman, I think it is important to critically examine the different possible applications of an IBE model. It might be a model of innate human psychology, or a model of the conscious interpretation of individual experimental results, or a model of theory choice. The bare idea of IBE is open-ended enough to apply to all these cases, but philosophers of science should not assume that one and the same version of IBE will apply in each of these cases. In what follows, I will examine Lipton's proposal for the application of IBE to the explanation of experimental results.

9.2 Causal Explanation, Contrastive Questions, and Controlled Experiments

In Chapter 3, there was an invitation to consider an explanation-seeking why-question:

[5] See also Lipton (2003, p. 90). [6] Lipton (2003, pp. 68–69).

[7] It is a surprising coincidence that Lipton's account here, like Thagard's in the last chapter, invites choosing the Hodgkin–Huxley model over the RGC theory (or vice versa), because the Hodgkin–Huxley model offers a better explanation of its phenomena than the RGC theory offers of its phenomena (or vice versa).

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(HH) Why did axon no. 15 *in seawater* display an initial *inward* current when depolarized by 65 mV, but *in a sodium-free medium* display an initial *outward* current when depolarized by 65 mV?

In that chapter, I called (HH) a "controlled experiment question." Referring back to the Y-maze experiment by Spence and Lippitt, one might also recognize a similar question.

SL (Spence-Lippitt): Why did the hungry rat go to the arm with the food, whereas the thirsty rat went to the arm with the water?

I proposed to recognize questions such as (HH) and (SL), because doing so helped articulate the reasoning the scientists gave in support of compositional hypotheses. I proposed that Hodgkin and Huxley and Tolman (who relied on the Spence–Lippitt experiment) were engaged in compositional abduction in the context of a controlled experiment.

Lipton (2003) provides a similar account. Lipton proposes that IBE be developed around a causal model of explanation: to explain an event is to give some information about its causal history.[8] But what information about an event's causal history provides a good explanation? Answer: Information that meets our interests. What are those? They are interests specified by contrastive questions, such as "Why did Adam eat the apple, rather than the pear?"[9] Further, Lipton proposes that contrastive questions can be linked to the interpretation of the results of controlled experiments.

Despite the similarities between my account and Lipton's, there are four significant differences. First, Lipton's proposal derives from the debate over contrastivism in the theory of explanation.[10] Mine does not. Second, origins aside, there are differences between contrastive questions as understood in the contrastivism debate and controlled experiment questions as I have described them. Third, although there are instances of contrastive questions that reasonably pair with controlled experiments, there are also instances where they do not. Fourth, following Lipton, one might suspect that there is a relationship between controlled experiments and a "method of compositional differences." This latter method is not any of Mill's methods but may be inspired by Mill's method of difference. Although Lipton might suggest a connection between controlled experiments and a method of compositional difference, there are clear cases where compositional controlled experiments do not fit the method.

Contrastivism. Contrastive questions, answered by contrastive explanations, are sometimes framed as a response to Sylvain Bromberger's theory of

[8] Lipton (2003, p. 30f). [9] Lipton (2003, p. 33f). [10] For a recent review, see Shan (2019).

explanation in terms of correct answers to why-questions.[11] For this, Bromberger proposed a normal form of why-questions. To a first approximation, such questions begin with "why" and contain an inner yes-no question, that is, a question whose answer is either yes or no. Thus, "Why does copper turn green when exposed to air?" is a why-question in normal form. The yes-no question is, "Does copper turn green when exposed to air?"

Bas van Fraassen was an early supporter of an alternative theory of the normal form of why-questions.[12] According to van Fraassen, the proper form of the question,

1) Why did Adam eat the apple?

is something like one of the following:

1a) Why was it *Adam* who ate the apple?
1b) Why was it the *apple* Adam ate?
1c) Why did Adam *eat* the apple? (Van Fraassen, 1980, p. 127)[13]

1a)–1c) are, thus, supposed to disambiguate what is meant by 1). Alternatively, 1a)–1c) are supposed to make explicit what a speaker implicitly means by 1). As van Fraassen states it, "The correct, general underlying structure of a why-question is therefore 'Why (is it the case that) P in contrast to (other members of) X?' where X, the contrast-class, is a set of alternatives" (Van Fraassen, 1980, p. 127). Subsequent work has sometimes glossed 1a)–1c) along the following lines:

1a') Why was it Adam, rather than Brad, who ate the apple?
1b') Why was it the apple, rather than the pear, Adam ate?
1c') Why did Adam eat, rather than discard, the apple?

or perhaps

1a'') Why did Adam eat the apple and Brad did not?
1b'') Why did Adam eat the apple and not the pear?
1c'') Why did Adam eat the apple and not discard it?

Sometimes these sentences are described in terms of facts and foils. Adam eating the apple is the fact, whereas Brad eating the apple, Adam

[11] Bromberger (1966) is the *locus classicus* for this theory. It is an especially careful and detailed discussion, but for present purposes, I will omit many of the details that are tangential to my concerns. In this regard, my discussion follows some of the important contributions to the contrastivism debate. See, for example, Van Fraassen (1980) and Temple (1988).

[12] Although discussions of contrastivism frequently refer to Van Fraassen (1980), this work draws upon what was, at the time, unpublished work by Jon Dorling, Alan Garfinkle, and Bengt Hansson. Garfinkle's work subsequently appeared as Garfinkel (1982).

[13] See also Temple (1988, p. 142) and Shan (2019, p. 2).

eating the pear, and Adam discarding the apple are foils.[14] The fact actually occurred, but the foils did not.[15]

While there are many points at which my project differs from the contrastivist project, let me mention just two before delving into further issues surrounding contrastivism. First, I note that, contrary to a long tradition in the philosophy of science, I do not take all explanations to be answers to why-questions. Some singular compositional explanations are answers to why-questions, but others are not. One might ask, "Why did this action potential overshoot zero?" This might be understood as a request for an account of how lower level processes bring about this process. This would be a request for a singular dynamic compositional explanation. One might ask, "Why is this axonal membrane capacitance 0.9 $\mu F/cm^2$?" This would be a request for a singular standing compositional explanation. Both of these questions are types of explanation-seeking why-questions, but there are other explanation-seeking questions. So, one might ask, "What is water made of?" Answer: Molecules of H_2O, OH^- ions, H_3O^+ ions, and H_2O oligomers. One might ask, "What are human retinas made of?" in the sense of "What cells make up human retinas?" Answer: Rods, cones, amacrine, bipolar, and horizontal cells. These would be requests for singular analytic compositional explanations. To understand explanation through the lens of why-questions is too narrow to do justice to scientific practice.

A second respect in which my project differs from van Fraassen's contrastivist project is that, even if one recognizes a wider range of questions with which to seek explanations, it is orthogonal to my concerns to determine whether there is, or is not, some canonical form for these questions. Maybe Bromberger's or van Fraassen's account of the normal form of explanatory why-questions is correct and maybe one could develop a normal form for compositional questions. Although that may be a worthy philosophical project, it is not my project.

Contrastive Questions versus Controlled Experiment Questions. Although my project differs from that of van Fraassen and his successors, it could, of course, happen that controlled experiment questions are just contrastive questions in van Fraassen's sense. So, let me point out three differences between "van Fraassen style" contrastive why-questions and compositional controlled experiment why-questions.

Consider, first, the fact that there is supposed to be a semantic or pragmatic relation between 1) and 1a)–1c). Whether the relation is

[14] See, for example, Carroll (1997) and Lipton (2003). [15] Lipton (2003, p. 34).

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semantic or pragmatic does not matter for my present point. 1a)–1c) are disambiguations of 1); 1a)–1c) make explicit what is implicit in 1). By contrast, the HH and SL questions are not disambiguations. The two conjuncts in the HH and SL questions relate to two empirically distinct explananda.

A second feature of contrastive questions, as commonly understood, is that the fact is supposed to have occurred, whereas the foil has not.[16] Adam ate the apple, but Brad did not eat the apple, Adam did not eat the pear, and Adam did not discard the apple. This central presupposition is incorporated into, for example, David-Hillel Ruben's treatment of contrastive explanation. Ruben (1987, pp. 35–36) proposes that explaining why Adam, rather than Brad, ate the apple, is a matter of explaining Adam's eating the apple and explaining how Adam's eating the apple "eclipses" (a more general concept than "causally prevents") Brad's eating the apple. In this case, the thought behind the eclipsing might be that if Adam has eaten the apple, there is nothing left of the apple for Brad to eat.[17] By contrast, in both the HH and SL questions, both conjuncts occurred. In the HH case, neuron no. 15 displayed an initial inward current when depolarized by 65 mV in a sodium-containing medium and it later displayed an initial outward current when depolarized by 65 mV in a sodium-free medium. In the SL case, one hungry rat went to the arm with the food, whereas another thirsty rat went to the arm with the water.

Of course, one might observe that, in the HH case, the axon's displaying an initial outward current means that it did not display an initial inward current. This might lead one to another explanation-seeking why-question,

> HH': Why did the axon display an initial inward current when depolarized by 65 mV in a sodium-containing medium, but did not display an initial inward current when depolarized by 65 mV in a sodium-free medium?

Similarly, one might observe that, in the SL case, the thirsty rat's going to the arm with the water means that it did not go to the arm with the food. This might lead one to the question,

> SL': Why did the thirsty rat go to the arm with the water, but not to the arm with the food?

[16] Temple (1988, p. 144), Lipton (2003, p. 34).
[17] Lipton (1987) presents three counterexamples to Ruben's analysis.

Moreover, it is perfectly reasonable to suppose that the HH experiment could prompt HH' and the SL experiment could prompt SL'. Finally, HH' and SL' might count as instances of the more familiar van Fraassen-style contrastive why-question. So, one might think that there is, after all, a connection between controlled experiment questions and contrastive questions.

All of the points in the last paragraph can be conceded. Controlled experiments may raise many questions. Some of these may be familiar contrastive questions. My point, however, is that controlled experiments also raise some questions that are not the familiar van Fraassen-style contrastive questions. Nothing in the last paragraph undermines the idea that HH and SL are not familiar contrastive why-questions. HH and HH' are different questions; SL and SL' are different questions. The second conjunct of HH contains a stronger presupposition than does the second conjunct of HH'. HH presupposes that there is an initial outward current when the axon is depolarized by 65 mV in a sodium-free solution, but HH' does not. HH is inconsistent with the axon displaying no current when the axon is depolarized by 65 mV in a sodium-free solution, whereas HH' is consistent with this. Similarly, *mutatis mutandis* for SL. For this response to work, one must recognize that there is no such thing as *the* question a controlled experiment may prompt.

A third feature of the contrastive questions, as often understood, is that they share what might loosely be described as common arguments, while differing in a single argument. So, for instance, Ia') and 1a") have the apple and eating as common arguments but differ in Adam and Brad. 1b') and 1b") have Adam and eating as common arguments but differ in the apple and the pear. Controlled experiment questions need not have this limitation. Hodgkin and Huxley voltage clamped neuron no. 15 first in a sodium-containing medium, and then in a sodium-free medium, and then in a sodium-containing medium, each time measuring the resulting currents. This experiment has neuron no. 15 as the common argument with the medium being the difference. But the two conjuncts of the HH question differ not just in one argument, but in two. The first conjunct asks why the axon displayed an initial *inward* current when depolarized by 65 mV in a *sodium-containing* medium and the second asks why an initial *outward current* when depolarized by 65 mV in a *sodium-free medium*? The direction of the current and the contents of the medium varied. The situation with the Spence and Lippitt experiment is the same. The hungry rat familiarized with the maze went to the arm with the food; the thirsty rat familiarized with the maze went to the arm with the water. The familiarity

with the maze was a common feature of both rats, but one was hungry and the other thirsty and one went one direction and the other went another.

It should be noted that in many discussions of contrastive questions, facts and foils are represented using sentential letters, such as P and Q.[18] For many purposes, this simple notation is perfectly adequate. Nevertheless, one unfortunate consequence of it is that it may obscure some of the structure implicit in contrastive questions. It may even lead some to suppose that I am misinterpreting the contrastive view. Fortunately, I am not alone in drawing attention to this implicit structure. Thomas Grimes uses differences among what I am calling "arguments" to distinguish different contrasts.[19] Taking a cue from Grimes, one might say that 1a') is a request for an explanation of an "S-fact" because it concerns the subject of the sentence; 1b') is a request for an explanation of an "O-fact" because it concerns the direct object of the sentence; and 1c') is a request for an explanation of an "A-fact" because it concerns the activity instance of the sentence. Those who consult Grimes (1993) will find that I am not simply taking over Grimes's scheme.[20] I am, instead, merely drawing attention to its recognition of the argument structure of contrastive questions. That is why I claim to be "taking a cue" from Grimes.

To this point, I have often qualified my claims by noting that contrastive questions *as commonly understood* have such and such features that my controlled experiment questions do not. I have also qualified my claim by referring to *van Fraassen-style* contrastive questions. Nothing, however, prevents a philosopher of science from abandoning earlier presuppositions about what counts as a contrastive question. Do that. Let controlled experiment questions be contrastive questions. For my purposes, it is enough that I have a correct account of what controlled experiments questions are. Whether they are, or are not, ultimately counted as contrastive questions is a tangential matter.

Lipton on Contrastive Questions and Controlled Experiments. I have proposed that controlled experiments raise what I have called controlled experiment questions. Hodgkin and Huxley's first experiment in Hodgkin and Huxley (1952a) provokes the HH question, "Why did the axon display an initial inward current when depolarized by 65 mV in a sodium-containing medium, but an initial outward current when depolarized by 65 mV in a sodium-free medium?" Similarly, Lipton proposed that controlled experiments raised van Fraassen-style contrastive questions,

[18] See, for example, Ruben (1987) and Temple (1988).
[19] Grimes (1993).
[20] Grimes (1993, p. 32).

such as "Why did Adam eat the apple, but not the pear?" A quick read of Lipton (1987, 1990, 1991) reveals that Lipton was quite familiar with van Fraassen-style contrastive questions. Moreover, Lipton is quite explicit in thinking that this analysis applies to Semmelweis's reasoning regarding childbed fever: "This case is a gold mine for inferences to the best contrastive explanation" (Lipton, 2003, p. 75). I now want to argue that Lipton is mistaken in aligning van Fraassen-style contrastive questions with controlled experiments. Lipton is right that Semmelweis relied upon controlled experiments in his efforts to determine the cause of childbed fever. Where he goes wrong, however, is in thinking that the reasoning behind controlled experiments is generally captured by van Fraassen-style contrastive questions/explanations. The analysis works sometimes, but it fails at others.

To begin with, let me show a case where it appears to work. One of the central observations for Semmelweis was the mortality rate due to childbed fever was higher in clinic 1 than in clinic 2. To deploy the van Fraassen-style contrastive question, let us begin with

(2) Why was the mortality rate high in clinic 1?

This might be disambiguated as follows,

(2a) Why was the mortality rate high in clinic 1 and not low in clinic 1?
(2b) Why was the mortality rate high in clinic 1 and not in clinic 2?

Let us assume that the relevant disambiguation is (2b), that (2b) gives an explanation-seeking why-question that concerned Semmelweis. Lipton proposes that atmospheric conditions would not explain why the mortality rate was high in clinic 1 and not in clinic 2, since atmospheric conditions were the same in both of the clinics. This example works as far as it goes.

A weak point in Lipton's analysis, however, is that scientists often make stronger commitments than are captured by van Fraassen-style contrastive questions. Semmelweis did not believe merely that the mortality rate was not high in clinic 2. The belief that the mortality rate was not high in clinic 2 is consistent with the belief that the mortality rate was low in clinic 2 and that the mortality rate was extremely high in clinic 2. Semmelweis clearly believed that it was low in clinic 2, or at least lower than in clinic 1. This belief shaped Semmelweis's experimental design. Semmelweis noted that many times a day a priest walked through clinic 1 ringing a bell and considered the idea that this might be the cause of the higher mortality rate in clinic 1. He, therefore, arranged to have the priest avoid passing through much of the clinic and to stop ringing the bell. This had no effect on the

mortality rate. Notice that if Semmelweis believed that the mortality rate was higher in clinic 2 than in clinic 1, his experiment with the priest would have likely involved the priest going to *both* clinic 1 and clinic 2. That experiment would have the hopeful outcome of reducing the mortality rate in clinic 2. Clearly, Semmelweis's reasoning was guided by the belief that the first clinic had a higher mortality rate than the second clinic, rather than the belief that the first clinic had a different mortality rate than the second. One may, of course, propose that Semmelweis had both this first belief and the second belief, since the first belief logically entails the second. Nevertheless, it was clearly the first belief that drove Semmelweis to conduct the experiment he did. The point here reiterates the importance of the distinction between the HH and HH' questions and SL and SL' questions introduced above.

Controlled Experiments and a Method of Compositional Difference. Lipton (2003) proposed that his version of IBE was an improvement over Mill's method of difference for causal inference. Rappaport (1996) and Scholl (2013, 2015), however, have argued that Lipton's account does not differ from Mill's methods. Further, Norton (2021) embraces Scholl's analysis.[21] This exchange, thus, raises the question of the extent to which my account of singular compositional abduction is, or is not, merely an instance of a difference principle for compositional, as opposed to causal, inference. Let the difference principle be the following:

> If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance save one in common, that one occurring only in the former; the circumstance in which alone the two instances differ, is a component of the phenomenon.

This difference principle is clearly inspired by John Stuart Mill's method of difference.[22] Here, I wish to emphasize that my term "inspired" is meant to leave open the extent to which Mill may or may not have accepted this difference principle. That is an issue I leave to specialists in Mill's philosophy of science. My concern is to argue that there are cases of singular compositional abduction that do not work according to a natural reading of this simple difference principle.

Recall that in Hodgkin and Huxley's first experiment they postulate that depolarizing axon no. 15 by 65mV increases the membrane's permeability to sodium. As a result, when there is excess extracellular sodium, sodium flows into the axon, whereas when there is no extracellular sodium,

[21] See also Barnes (1995). [22] Mill (1843, p. 455).

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sodium flows out of the axon. Following the compositional difference principle, one should expect to find Hodgkin and Huxley postulating something that differs between the case when the medium contains sodium and the case when the medium lacks sodium. And there is such a difference: the difference in the direction of the flow of sodium. But Hodgkin and Huxley also postulate something in common to the two cases: both involve an increase in the membrane's permeability to sodium.

Next, recall that, in support of the cognitive map hypothesis, Tolman (1948) cited an experiment performed by Spence and Lippitt. Recall that, in this Y-maze experiment, the hungry rats went to the arm with food and the thirsty rats went to the arm with water. Tolman, thus, postulated a difference between the two cases, hence acted in accordance with the compositional method of difference. Nevertheless, he also postulated something common: both the hungry and the thirsty rats formed a cognitive map of the Y-maze. The upshot of these two examples, therefore, is that singular compositional abduction is not simply the application of a Mill-inspired method of compositional difference.

9.3 Understanding and the Explanatory Virtues

Although Lipton's view is often described as inference to the best explanation, the somewhat more detailed formulation of his view is that scientists use inference to the loveliest potential explanation. A potential explanation is in all respects an explanation, except for truth. Lovelier explanations contribute more understanding. Further, explanations that are lovelier are explanations that are more likely. Finally, the explanatory virtues of mechanism, precision, scope, simplicity, fertility, and fit with background belief contribute to understanding. In advancing these claims, Lipton is rehearsing familiar ideas.

While there is nothing intrinsically wrong with these familiar ideas, they do not contribute much to the theory of singular compositional abduction. Some are not worked out in sufficient detail. Others are not applicable to a singular compositional explanation. Work through Lipton's list of virtues in order.

Mechanism. In a singular dynamic compositional explanation, one might suppose that insofar as a potential explanation of S Ψ-ing does not specify the x_i's or the φ_I-ings, one has failed to specify a mechanism. Yet there are two striking cases of widely accepted compositional explanations in which scientists are unbothered by explanations of S Ψ-ing that do not

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specify the x_i's or the φ_i-ings.[23] In Chapter 5, I noted that Hodgkin and Huxley believed that an internal microelectrode that touched the wall of the axon damaged the axon. How did they arrive at that conclusion? They hypothesized that the wall was damaged by the touching, since that would explain the subsequent electrical measurements. They had no other means of inspecting the membrane to determine in detail what had happened to membrane. There is a clear sense in which Hodgkin and Huxley could not specify the mechanism of the damage, yet this apparently did little to affect the acceptance of their explanation of the effects of touching the axonal membrane. A second case involves "run down." Hodgkin and Huxley believed that, over the course of an experiment, the axons were running down. They were dying. Yet, Hodgkin and Huxley did not specify what parts of the cells were changing or what activity instances of those parts were changing. Instead, they simply postulated changes in unspecified activity instances in unspecified individuals as responsible for the changes in axonal responses. What are philosophers of science to make of these cases vis-à-vis a "virtue for mechanism?" This is worth exploring, but Lipton offers nothing to lead the way.

Precision. Lipton proposes that "we understand more when we can explain the quantitative features of a phenomenon, and not just its qualitative ones" (Lipton, 2003, p. 122). Given that Lipton does not explain what it means to "understand more," the phrase is probably vague enough to be able to resist refutation. Nevertheless, even if the account is not wrong, it is also not illuminating. To get the point, recall that Hodgkin and Huxley's "Quantitative Description" paper, the fifth in their 1952 series, provided a quantitative model of their theory of the action potential. What did this quantitative model do for Hodgkin and Huxley? Did it help them understand more or did it merely provide them with greater confirmation of hypotheses they already understood? Lipton's comments are of no help here.

Scope, simplicity, and fruitfulness. The remaining virtues, as Lipton develops them, are not features of individual singular compositional explanations. They are features of an explanation in a sense in which an explanation is an explanation of many phenomena. (This issue was discussed in the context of Thagard's treatment of the consilience of

[23] Chapter 6 noted that Hermann's explanation of the grid illusion failed to provide a mechanism in the same sense that it failed to specify x_i's and ϕ_I's. This case differs from the HH case insofar as the HH explanation has been widely accepted as unproblematic, but the evidence that Hermann's explanation was acceptable is harder to come by.

abductions in Chapter 8.) Take a concrete case. Lipton claims that "An explanation that explains more phenomena is for that reason a lovelier explanation" (Lipton, 2003, p. 122). That is a common enough formulation of what scope is, but it is a sense of "explanation" in which one explanation explains multiple phenomena, that is, multiple explananda. It is not the sense of "explanation" that is in play in the explanation of one of the initial inward currents of Hodgkin and Huxley's axon no. 15. The explanation in the Hodgkin–Huxley case is of a single spatiotemporal occurrence. It involves a specific set of sodium ions moving into the axon at a single time and place. The explanans and explananda do not recur. This is just a feature of the explanation being singular. To get a general notion of an explanation that explains many phenomena, one might say that there is the general hypothesis – maybe what Bogen and Woodward would count as a phenomenon – of sodium carrying the initial inward current of the action potential. This general hypothesis might be said to have the scope it does in virtue of, say, the number of specific individual explanations in which it occurs.

Simplicity might be understood in many different ways. Lipton recognizes this but describes a conception of simplicity understood as a feature of the way in which a single hypothesis might be applied in the explanation of many explananda: "Again, simplicity, in its various guises, contributes to loveliness. For example, some forms of simplicity enable us to achieve one of the cardinal goals of understanding, namely to reveal the unity that underlies the apparent diversity of the phenomena" (Lipton, 2003, p. 122). The unity in the apparent diversity lies in one and the same general hypothesis – a Bogen-Woodward phenomenon? – occurring among the explanantia of multiple explanations. And while philosophers of science might accept this as a *bona fide* explanatory virtue, it is not an explanatory virtue of a singular compositional explanation.

The same point applies *mutatis mutandis* to Lipton's idea of fruitfulness, as borrowed from Thomas Kuhn. "Fruitfulness, the power of a theory to, in Kuhn's words, 'disclose new phenomena or previously unnoted relationships among those already known' [(Kuhn, 1977, p. 322)], is linked to scope and simplicity, and is again an explanatory virtue" (Lipton, 2003, p. 122). Part of what Lipton and Kuhn are pointing to under the heading of fruitfulness is straightforwardly described as the power of some hypothesis to explain multiple new phenomena. This, again, is not a feature of a singular compositional explanation. Disclosing previously unnoted relationships among already known phenomena might be a matter of showing how one compositional hypothesis might explain two

phenomena, thereby indicating that they are related, when this relation was not previously noted.

Another way of making the foregoing point about the scope, simplicity, and fruitfulness of a compositional explanation is that it presupposes an account of singular compositional explanations. To articulate what these virtues are, one wants an account of singular compositional explanation. Thus, just as a theory of singular compositional explanation might flesh out Thagard's account of the consilience of abductions, so it might flesh out Lipton's account of scope, simplicity, and fruitfulness.

Fit with the scientific background. Finally, there is what Lipton describes as an explanation's fit with the scientific "background." Chapter 4 provided a sketch of how pedestrian scientific beliefs play a role in compositional abductive arguments. Beliefs about the reliability of thermometers, the sodium concentration of seawater, and the existence of junction potentials are such pedestrian background beliefs. Lipton, however, has in mind something more global, namely, "background beliefs about what sorts of accounts are genuinely explanatory. For example, at given stages of science no appeal to action at a distance or to an irreducibly chance mechanism could count as an adequate explanation, whatever its empirical adequacy" (Lipton, 2003, p. 122). There is a lot to be said about this topic that is particularly germane to compositional abduction, although Lipton has nothing to say about it.

Consider some suggestive comments on this score. One of the more striking illustrations of a "Liptonian" background belief in cognitive science, neuroscience, and psychology concerns the legitimacy of mental representations and the legitimacy of postulating mental representations to explain behavior. Although the issue is not typically framed in these terms, skepticism about mental representations is often intertwined with skepticism about compositional abductive reasoning. One can see the interplay in B. F. Skinner's *Verbal Behavior*.[24]

> It has generally been assumed that to explain behavior, or any aspect of it, one must attribute it to events taking place inside the organism. In the field of verbal behavior this practice was once represented by the doctrine of the expression of ideas. An utterance was felt to be explained by setting forth the ideas which it expressed. If the speaker had had a different idea, he would have uttered different words or words in a different arrangement. If his utterance was unusual, it was because of the novelty or originality of

[24] Clearly, Skinner's book falls outside this book's focus on the primary experimental literature. Moreover, Skinner is self-conscious about this. See Skinner (1957, p. 11).

> his ideas. If it seemed empty, he must have lacked ideas or have been unable to put them into words. If he could not keep silent, it was because of the force of his ideas. If he spoke haltingly, it was because his ideas came slowly or were badly organized. And so on. All properties of verbal behavior seem to be thus accounted for. (Skinner, 1957, p. 5)
>
> The difficulty is that the ideas for which sounds are said to stand as signs cannot be independently observed. If we ask for evidence of their existence, we are likely to be given a restatement in other words; but a restatement is no closer to the idea than the original utterance. (Skinner, 1957, p. 6)
>
> There is obviously something suspicious in the ease with which we discover in a set of ideas precisely those properties needed to account for the behavior which expresses them. We evidently construct the ideas at will from the behavior to be explained. There is, of course, no real explanation. (Skinner, 1957, p. 6)

One of the principal features of compositional abduction, like abduction more generally, is that it is used as part of a case for the existence of things that are neither directly measured nor directly manipulated. This is what Skinner evidently has in mind in objections to what cannot be independently observed. The existence of a capacity current and ionic currents during Hodgkin and Huxley's voltage clamp experiments, the damage to an axon when a microelectrode touches the membrane, or the run down of an axon are all instances in which Hodgkin and Huxley postulated unseen goings on. And, when using abduction, the scientist does aim to construct hypotheses that are explanatory.

Verbal Behavior was, of course, published almost a decade after Tolman's "Cognitive Maps" paper. Nevertheless, Tolman was working within a broadly behaviorist context that was skeptical of his cognitive map hypothesis. To push back against behaviorist resistance, Tolman did not pursue a strategy of championing the explanatory value of compositional abduction. He did not pursue the kind of general strategy Skinner pursued in *Verbal Behavior*. Instead, he tried to provide experimental results that he thought could be explained by the cognitive map hypothesis but not by other stimulus–response hypotheses. In pursuing this strategy, he was evidently banking on his fellow psychologists sharing common ground on what counts as a good explanation in individual concrete cases. He was banking on his fellow psychologists recognizing the acceptability of compositional explanations invoking cognitive maps in concrete cases, even if, in the abstract, they rejected compositional explanations invoking the mental. This is an idea that probably merits attention as a case study in its own right.

So, Lipton is right that there is some philosophy of science story that should be told about background beliefs regarding abduction. Hodgkin and Huxley clearly shared any number of background beliefs with their fellow physiologists that informed their abductive reasoning. Moreover, Lipton, following tradition, is right that there is something one might informally or pre-theoretically describe as the background belief that compositional abduction is, or is not, an acceptable form of reasoning. How to describe this theoretically, when scientists may be thought to lack a concept of compositional abduction, is not trivial. However one theoretically works out the idea of the common ground that Hodgkin and Huxley shared with other physiologists, this theory leaves untouched large parts of the account of how Hodgkin and Huxley interpreted the results of their multiple controlled experiments in support of their theory of the action potential. Hodgkin and Huxley did not rely on the background they shared with other physiologists to carry the day in favor of their theory of the action potential. The crucial evidence came from the interpretation of specific experimental results. Articulating how they did that has been the goal of this book.

9.4 Summary

Any reader familiar with Lipton (2003) will know that there are large parts of Lipton's work that I have completely ignored. Lipton has chapters on Bayesian abduction, the Raven paradox, the putative connection between loveliness and truth, and the proposal that prediction of new data is more valuable than the explanation of old data. Lipton also devotes a lot of attention to arguing that the IBE account of confirmation is superior to the HD account. Many will also object that I have not devoted sufficient attention to replies that Lipton might give to my analyses.

Perhaps there is philosophical interest in a more complete examination of Lipton's book, but that is tangential to my concerns. I am trying to advance a science-first approach to the confirmation of compositional hypotheses. Further, in proposing that scientists sometimes use singular compositional abduction, I have a theory of singular compositional explanation and detailed scientific case studies to back it up. As this book prioritizes the development of the positive project, I have focused my attention on the features of Lipton's account that will best illuminate my own account and I have devoted less attention to an exhaustive historical review.