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## Three views of anomaly and their heuristic utility

Robert Rose

*Louisiana State University and Agricultural and Mechanical College*

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# THREE VIEWS OF ANOMALY AND THEIR HEURISTIC UTILITY

A Thesis

Submitted to the Graduate Faculty of the  
Louisiana State University and  
Agricultural and Mechanical College  
in partial fulfillment of the  
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in

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by

Robert Rose

B.A., Lamar University, 1997

M.A., Louisiana State University, 2001

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## ABSTRACT

This thesis presents three views of anomaly in explanation: the linguistic view, the perceptual view, and the mechanistic view. The linguistic view is based on the notion that an anomaly is an instance of logical inconsistency. According to the perceptual view, an anomaly is a perceptual event which consists of a phenomenon deviating from a paradigmatic set of expectations. Lastly, the mechanistic view defines anomaly as a phenomenon which reveals the predictive failure of a model of the mechanism underlying the phenomenon to be explained. Each view is evaluated in terms of its heuristic utility, in two ways: first, according to how well the view allows one to detect that there is an anomaly in explanation; and second, according to the resources it provides for exploring different kinds of anomaly. Three criteria are used to evaluate the heuristic utility of each view for anomaly detection: (1) Does the view allow one to distinguish between anomalies in explanation and non-explanatory anomalies? (2) Are there explanatory contexts in which a view cannot determine if there is an anomaly in explanation? (3) Given a view, what are the conditions of possibility for the appearance of an anomaly in explanation? The criteria for evaluating the heuristic utility of a view for engaging the question of which kind of anomaly there is are the following: (1) How well does the view allow one to localize, for heuristic purposes, an anomaly according to its kind? (2) What resources does the view provide for exploring theoretical anomalies? (3) For exploring phenomenal anomalies? (4) For exploring factual anomalies? The thesis argues that, as it stands, the mechanistic view is heuristically the most useful for anomaly detection and exploration. It also provides some suggestions as to how the linguistic view and perceptual views could be strengthened.

## § I. INTRODUCTION

### I.I. BACKGROUND AND CENTRAL ARGUMENT OF THESIS

I.I.I. FRIENDS OF DISCOVERY. In the post-positivist years, there has been a renewed interest in discovery among many philosophers of science.<sup>1</sup> Since the late seventies, much attention has been paid to Reichenbach's distinction between the "context of justification" and the "context of discovery," which was drawn primarily for the purpose of pointing out that the former, and not the latter, was the locus of scientific rationality (1938). Justification and explanation were thought to be tractable enough to be rationally reconstructed using the resources of formal logic; discovery was not. For many, Popper's *The Logic of Scientific Discovery* ([1934] 1961), despite its title, did nothing to challenge this view. Nonetheless, the "friends of discovery" have in recent years either attacked the distinction outright or called for an analysis of the rational aspects of the context of discovery. It has not been expected that such analysis would yield a *logic* of discovery, that is, a demonstration that discovery could be made to rely exclusively on deductively valid inferences. Instead, analysis of discovery processes has yielded descriptions of *methods* of discovery, which show that discovery can be rational. This kind of analysis has been carried out in various contexts: research on the traditional concepts of discovery, argued exposition of case studies from the history of science as well as contemporary science, and the development of computational models of discovery (Shrager and Langley 1990).

I.I.2. FRIENDS OF MECHANISMS. There has also been, during the post-positivist years, a resurgence of interest in *mechanistic* models of scientific explanation (Bechtel and Richardson 1993, Craver 2002, Darden 2002, Glennan 2002, Machamer et al. 2000, Wimsatt [1976] 1984, Woodward 2002). The crucial moment for many philosophers

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<sup>1</sup>Many or all the views mentioned in this paragraph can be found in Bechtel and Richardson (1993), Darden (1991), Gutting (1980), Hoyningen-Huene (1987), Laudan (1980), Nickles (1985, 1980), Schaffner (1993), Whitt (1990).



of science was the recognition that the biosciences are for the most part committed to explaining phenomena in terms of the mechanisms which produce them. Yet the models of explanation which have been the most discussed in 20th c. philosophy of science have been nomological: Hempel and Oppenheim's deductive-nomological model is frequently taken as a starting point (for an in-depth survey, see Salmon 1989). But in the biosciences, rarely, if ever, are new laws discovered; it could therefore seem unnecessary to describe their explanatory activities nomologically. Much more will be said about the distinction between nomological and mechanistic models of explanation in §§4 and 6.

1.1.3. FRIENDS OF BOTH. Many of the philosophers of science who are “friends of discovery” have also been proponents of mechanistic models of scientific explanation. The question arises if the interest in discovery and the interest in mechanistic explanation converge. Both interests are typically prefaced as reactions against positivistic requirements for scientific rationality: discovery is not necessarily without rational method and scientific explanation need not be nomological nor expressed in a formal language. But are mechanistic explanatory processes especially conducive to the development of methods of discovery? If so, how?

In this thesis, I propose to grapple with this question through analysis of the concept of *anomaly in explanation*. When compared with other accounts of explanation, mechanistic models of explanation, I will argue, are especially apt for the development of heuristics to detect and resolve scientific anomalies. In §1.2, I will introduce the concept of anomaly in explanation and discuss how it is relevant to the above question.

## 1.2. ANOMALY IN EXPLANATION

1.2.1. LIFECYCLE OF AN ANOMALY; ANOMALY RESOLUTION AND DISCOVERY. Anomalies can disrupt scientists' efforts to explain phenomena. Moreover, they do so over a period of time; they have, metaphorically speaking, a lifecycle. Take, for example,

perhaps the most famous anomaly in the history of modern science. In 1843, Le Verrier attempted unsuccessfully to explain Mercury's motion. He declared in 1859 that the rate of precession of Mercury's perihelion could not be explained at that time. The anomaly became history with the publication of the general theory of relativity in 1915, at which point it was known that the anomaly had been caused by a problematic theory, namely, the Newtonian theory of gravity.<sup>2</sup>

Anomalies in explanation occur when scientists rationally believe that certain theories *should* explain such-and-such a phenomenon which meanwhile remains inexplicable. Anomalies in explanation often result from failed predictions: certain theories predict a phenomenon (e.g., a perihelion advance of 527" /century), but a phenomenon appears which reveals that the prediction has failed (e.g., a perihelion advance 43" greater than predicted). Henceforth, the anomalous phenomenon, insofar as it stands in need of explanation, reveals an explanatory failure. Explanatory failure is the *first indication* that there is an anomaly in explanation. Le Verrier sought an explanation for Mercury's motion for 16 years before announcing that it was anomalous: while in 1843 there was clearly an explanatory failure, it was not until 1859 that it was known that the phenomenon which should have been explainable was not. One could say that early stages of an anomaly are dominated by attempts to answer the question *if* there is an anomaly.

Explanatory success is the *final indication* that there has been an anomaly in explanation. Successful explanation of the formerly inexplicable phenomenon entails not only the knowledge *that* the anomaly existed, but *what* the nature of the anomaly was. Le Verrier had thought that Mercury's orbit was being perturbed by unobserved bodies, either a planet in an inferior orbit, or a group of bodies of sufficient mass. None of these were discovered. Modified versions of Newton's theory of gravitation

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<sup>2</sup>For a precise historical account of this anomaly, see Roseveare (1982).

were proposed throughout the 19th c. to cope with the anomaly, none of which were accepted as a legitimate successor theory. General relativity of course did resolve the anomaly. Hence, the nature of the anomaly was known: it had been caused by a problematic theory. One could say that the mature stages of an anomaly are dominated by attempts to answer the question of *which* kind of anomaly it is.

This example shows that problematic theories can play a causal role in the appearance of anomalies. But at the same time, anomalies can play a causal role in *discovery* and *theory change*. For this reason, 20th c. philosophy of science, especially in the Kuhnian and the Popperian schools, has centered much attention on anomaly. More recently, the “friends of discovery” have taken a great deal of interest in it as well. The overarching question has been the following: how does attending to anomaly take scientists from explanatory failure to explanatory success? More specifically, are there methods of discovery, which would deserve to be called scientific and rational, in how scientists detect and resolve anomalies?

1.2.2. VIEWS OF ANOMALY. In this paper, I will lay out and compare three views of anomaly in explanation. Each view was developed in the context of a major account of scientific explanation in the past century. I have looked for prominent examples of discussions where anomaly is construed as an occurrence of explanatory failure and as an impetus for scientific discovery. While I have found three such examples on which to base my thesis, I have performed a certain amount of reconstruction in each case. Distinct accounts of scientific explanation should give rise to distinct accounts of anomaly in explanation. However, philosophers of science, while they have sought to distinguish their accounts of explanation, have not done the same for their views of anomaly.<sup>3</sup> For each view, my interpretive method proceeds as follows: first, I examine

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<sup>3</sup>At times this has lead to confusion: for example, Kuhn, who denies that successful explanation in science should presuppose regimentation in a formal language, nonetheless refers to anomalies as “inconsistencies” and “counterexamples.” While he uses these expressions only in a quasi-logical

an account of *explanatory success* in order to discern key characteristics of the *explanatory relation*; second, I define *anomaly* in terms of those characteristics.

In § 2, I reconstruct what I call the *linguistic* view of anomaly. The section presents some well-known attempts to define anomaly in explanation using deductive models. In § 3, I discuss the *perceptual* view of anomaly. The section is concerned with efforts in Kuhnian and post-Kuhnian science studies to adapt Kantian philosophy or gestalt psychology for the purpose of detecting and resolving anomalies. In § 4, I lay out the *mechanistic* view of anomaly. The section considers how anomaly in explanation is construed in recent accounts of mechanistic explanation.

1.2.3. OTHER SORTS OF EXPLANATORY FAILURE; OTHER SORTS OF ANOMALY; PREDICTIVE FAILURE WITHOUT EXPLANATORY FAILURE. In this thesis, I will examine explanatory failures which indicate the presence of an anomaly. I will also refer to anomalies as those which entail explanatory failure. But for the following reasons, I do not presume that this thesis provides an exhaustive account either of explanatory failure or anomaly.

First, not all explanatory failures are anomalies. Efforts at scientific explanation may fail for many reasons: for example, misapplication of accepted theories to a phenomenon outside of their domain (e.g., general relativity would fail to explain chemical synapses); use of discredited theories (e.g., Lashley's principle of mass action in the pre-striate cortex); attempts to explain phenomena which are exceedingly complex (e.g., consciousness); attempts to explain phenomena which are supernatural (e.g., resurrection). These and other sorts of explanatory failures will generally not give a positive first indication that there is an anomaly.

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sense, he could have avoided using them altogether; anomaly either entails reference to a semantic relation or it does not.

Second, not all anomalies are explanatory.<sup>4</sup> For example, failed predictions might be said to result in anomalies in cases where the predicted phenomenon is not in an explanatory relation with the theory. Consider laws which have no explanatory power but which have some predictive power and also occasion predictive failure. This is the case with the Titius-Bode Law, which was used in the 18th and 19th c. to calculate the semi-major axis of planetary orbits. The Titius-Bode Law makes use of the following equation:

$$a = 0.4 + (0.3)2^n$$

where  $n$  is an integer corresponding to the number of the planet (for Mercury,  $n = -\infty$ ; Venus,  $n = 0$ ; Earth,  $n = 1$ ; Mars,  $n = 2$ ; etc.) and  $a$  is given in astronomical units. On the one hand, the Titius-Bode Law was said to have successfully predicted a fifth planet between Mars and Jupiter: in 1801, Ceres, the largest asteroid of the Asteroid Belt, was discovered by Giuseppe Piazzi, and it was generally assumed that the asteroid belt was a destroyed planet. Uranus, once its orbit was calculated, also appeared to provide confirmation (i.e., for Uranus,  $n = 6$ ). On the other hand, the Titius-Bode Law failed to predict the semi-major axis of Neptune. (It turns out that the value  $n = 7$  corresponds better to Pluto.) Neptune's orbit might be considered anomalous for the Titius-Bode Law. However, we will not call it an anomaly in explanation insofar as the Titius-Bode Law should not be expected to explain Neptune's orbit.<sup>5</sup>

Another example of non-explanatory anomaly is the anomalous data point, measurement, or reading. These should be rigorously contrasted with unreliable data which lead to anomalies in explanation. In the latter case, data may seem reliable enough that scientists use them as evidence for a phenomenon. Yet it happens that the data are

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<sup>4</sup>For a general account of anomaly which encompasses the anomaly types described here and still others, see Elliott (2004).

<sup>5</sup>The question of how such anomalies are useful for scientific discovery is interesting and remains open to debate.

unreliable and the phenomenon inferred from them is non-existent. The phenomenon then stands in need of explanation, despite its non-existence, and an anomaly in explanation ensues. (See § 1.3.1 for an example of this kind of anomaly; for an in-depth discussion, see § 6.3.) In contrast, data are sometimes called anomalous precisely when they are *not* used as evidence for a phenomenon. Rejected data points may stand in need of explanation, but they do not cause the failure in explanation of a phenomenon for which they do not provide evidence.

For the remainder of the introduction, I will give a synopsis of my method for evaluating each view of anomaly with respect to its effectiveness within the discovery process.

### 1.3. HEURISTIC UTILITY OF A VIEW OF ANOMALY

In this thesis, the heuristic utility of a view of anomaly will be assessed in two ways: first, according to how well the view allows one to detect that there is an anomaly in explanation; and second, according to the resources it provides for the purpose of exploring different kinds of anomaly.

The question of how well a view allows one to determine if there is an anomaly will be addressed in the final section of the presentation of each view, §§ 2.3, 3.3, and 4.4. I have opted for this order because how well a view answers the question if there is an anomaly follows closely from the definition of anomaly in explanation that the view provides. Three criteria will be used to evaluate the heuristic utility of each view for anomaly detection: (1) Does the view allow one to distinguish between anomalies in explanation and non-explanatory anomalies? (2) Are there explanatory contexts in which a view cannot determine if there is an anomaly in explanation? (3) Given a view, what are the conditions of possibility for the appearance of an anomaly in explanation?

How well the views allow one to engage the question of which kind of anomaly there is will be discussed in § 6. Here the organizing principle shifts to the anomaly

kinds, and views will be compared directly with respect to the means they provide for exploring each one. The criteria for evaluating the heuristic utility of a view for engaging the question of which kind of anomaly there is are the following: (1) How well does the view allow one to localize, for heuristic purposes, an anomaly according to its kind? (2) What resources does the view provide for exploring theoretical anomalies? (3) For exploring phenomenal anomalies? (4) For exploring factual anomalies? Since I have not yet introduced these distinctions between kinds of anomalies in explanation, I will do so now.

1.3.1. KINDS OF ANOMALY IN EXPLANATION. Anomalies in explanation occur only when the theories, mechanisms, laws, initial conditions, relevant factors, phenomena, or data which are necessary for an explanation enter into an explanatory relation. Although this relation has frequently been characterized as one of consistency for successful explanations and of inconsistency for anomalies, I prefer not to limit the notion of explanatory relation to one which presupposes that the explanatory relata are linguistic expressions. In both the perceptual and the mechanical views, explanatory relations are conceived of between relata which are not necessarily sentential: for example, both use systems of visual perception and representation to describe explanatory relations or anomalies. More will be said in the respective sections; here, I simply want to suggest suspending judgment on the cognitive characterization of the explanatory relation.

When an anomaly first appears, scientists are usually unable to recognize which kind it is. While the kind of anomaly is unknown, scientists explore the anomaly as though it were a certain kind. The reliability of the data set may be doubted, competing interpretations of the data reconsidered, hypothesized mechanisms scrutinized, laws questioned, initial conditions re-examined, and the list of relevant factors checked for completeness.

In this paper, anomaly kind is defined according to the explanatory relatum which is primarily responsible for the inexplicability of the phenomenon. For example, the inexplicability of the advance of Mercury’s perihelion was caused by a problematic theory; this is an exemplar of a *theoretical anomaly*. A second kind of anomaly includes those which are caused by unknown relevant factors or erroneous initial conditions.<sup>6</sup> Uranus’ orbit was inexplicable because the perturbing force of Neptune was unaccounted for; this is a *factual anomaly*. The third kind of anomaly includes those which are caused by the phenomenon to-be-explained itself. Inadvertent reliance on bad data or misinterpretation of data can result in scientists seeking and failing to explain a phenomenon because it simply does not exist. For example, Neptune does not completely account for the perturbations in Uranus’ orbit. Neptune’s orbit as well has perturbations, which famously led to the discovery of Pluto. However, Pluto did not account for the perturbations. The unexplained perturbations in Uranus and Neptune’s orbits set astronomers scanning the Kuiper Belt for a new Planet X or sufficiently massive group of objects throughout much of the 20th century, until a study from the U.S. Naval Observatory was published in 1993 demonstrating that the phenomena of the perturbations in the two gas giants’ orbits in fact did not exist as they were based on incorrect measurements of Neptune’s mass (Standish 1993).<sup>7</sup> The resolution of the

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<sup>6</sup>In the context of the linguistic view of anomaly, this second sort of anomaly would be caused by at least one of the auxiliary hypotheses being false, or by a *ceteris paribus* clause failing to be corroborated.

<sup>7</sup>From the abstract of Standish’s article—

It is shown that the alleged “unexplained anomalies in the motion of Uranus” disappear when one properly accounts for the correct value of the mass of Neptune and properly adjusts to the observational data. Also, it is shown that each of the “irregularities in the measured positions of Neptune” has a complete explanation within the framework of the presently known solar system. There is still no evidence which requires or even indicates the existence of any planet-sized object; there remains no need to hypothesize the existence of a tenth planet in the solar system (1993, 2000).



anomaly has been generally accepted by astronomers. This case is typical of what I call a *phenomenal anomaly*.

The following table summarizes the anomaly kinds referred to in this thesis:

Anomaly Kind	Defining Relatum
Phenomenal	Non-existent phenomenon, viz., phenomenon based on unreliable or misinterpreted data
Factual	Unknown relevant factors or erroneous initial conditions
Theoretical	Problematic explanatory law, mechanism, paradigm, or theory

1.3.2. ANOMALY RESOLUTION. In this thesis, I will make use of the term “anomaly resolution”; the term is one I picked up from Darden, and my account of the heuristics involved in anomaly resolution is in many ways indebted to hers (1991). There she presents a four-step process of anomaly resolution:

- (1) Confirm that an anomaly exists.
- (2) Localize the problem.
- (3) Change the theory.
- (4) Assess the results. (1991, 270)

The focus of my thesis is, in a sense, on the first two stages. Answering the question if there is an anomaly runs parallel to confirming the anomaly; similarly, answering the question of which kind the anomaly is involves localizing the anomaly. However, there are two important differences between our accounts.

For Darden, confirming an anomaly involves eliminating the possibility that an anomaly is caused by faulty data or initial conditions (1991, 271); from there, the anomaly is resolved by first localizing the anomaly with respect to the domain of the theory, that is, by determining if the anomaly falls inside the scope of the theory. On my account, affirming that there is an anomaly does not entail eliminating the possibility that the anomaly is phenomenal or factual. On Darden’s account, both of these anomaly kinds

would refer to anomalies which really do not exist insofar as they are disconfirmed. This judgment on what constitutes an anomaly is too restrictive. Long-standing anomalies have occurred which turned out to be phenomenal: in astronomy, the anomaly of residual perturbations in Uranus' and Neptune's orbits provoked publications about a Planet X for well over a century (seven papers within just four years prior to the publication of Standish 1993); in visual neuroscience, the phenomenon of macular sparing has also been published on widely and has remained an anomaly for nearly a century (Leff 2004). Furthermore, there is an important distinction to be drawn between anomalies arising from explanatory facts and those arising from theories; for example, the difference between the anomaly of Uranus' orbit, which was caused by an unknown fact (the existence of Neptune), and the anomaly of Mercury's orbit, which was caused by a problematic theory. It seems methodologically impractical to assume that factual anomalies need to be eliminated or disconfirmed prior to exploring the possibility that the anomaly is theoretical. At the same time as Le Verrier and Adams were calculating the position of an eighth planet, others were proposing modifications to Newton's theory of gravity (notably, Airy, the Astronomer Royal; see Grosser 1962, 48). Thus, affirming that there is an anomaly, on my account, only commits one to its existence and not to a judgment of its kind. In addition, localizing an anomaly certainly involves determining if an anomaly is theoretical; but it also involves determining if an anomaly is factual or phenomenal.

The second difference worth mentioning is that my account does relatively little to describe heuristic strategies of theory revision or formulation, while this, of course, is the primary goal of Darden's account. Darden's heuristic strategies for anomaly resolution are directed towards improving a theory which is causing an anomaly; the object of discovery is a better theory. When I ask about the heuristic utility of a view of anomaly, I am inquiring to what extent a view of anomaly allows one to discover

anomalies (here the object of discovery is the anomaly itself) and to what extent a view of anomaly allows one to resolve different kinds of anomaly (here one would find a method of discovery). Even when I discuss the heuristic utility of a view of anomaly when exploring theoretical anomalies, my main purpose is to compare the three views. Thus, my account does not pretend to represent a comprehensive view of the heuristics related to anomaly resolution.

## § 2. THE LINGUISTIC VIEW OF ANOMALY

Scientific explanation and change were, in early 20th c. philosophy of science, predominantly analyzed by means of linguistic models. In these models, explanatory success is manifested as an instance of logical consistency. Anomalies appear as logical *inconsistencies*, typically (although not always) between theoretical statements and observation statements of some sort.

Here I will briefly outline two logical frameworks, both of which are frequently referenced in discussions of the traditional accounts of the explanatory process. The first is the *deductive-nomological model* (the D-N model), the goal of which, as it was articulated by Carl Hempel and Paul Oppenheim, was to provide the structure of a successful scientific explanation. The second is a series of models developed for theory falsification. These models have been chosen because they explicitly represent anomalies. Although models for theory falsification are most applicable to cases of predictive failure, they can also be used to demonstrate explanatory failure whenever the predicted phenomenon and the theory from which it is derived (in part) are in an explanatory relation. I will elaborate further on this point in § 2.2.

### 2.1. EXPLANATORY SUCCESS: THE D-N MODEL

Let's take an example. Suppose one sets out to explain the appearance of the moon on a particular night, or more precisely, why the illuminated portion of the Moon has the shape it does at a certain time. One might list a number of facts about the Moon's orbital motion with respect to the Earth, the role of the Sun as a light source, the reflective and spatial properties of the Moon, the refracting properties of the Earth's atmosphere, the position of the viewing apparatus, and so on. Hence, one might subsume all of these particular facts under general laws of geometrical optics to explain a certain gibbous form on a clear night.

The D-N model aims to generalize in logical terms scientific explanations such as why the moon has a gibbous shape. Thus, a set of statements of geometrical optics describing the law-like behavior of light rays, together with a sufficient set of particular facts, should have as a logical consequence a statement describing the visible shape of the moon.

Instantiating the D-N model begins by providing a statement of the “explanandum-phenomenon” (Hempel 1965, 336). The “explanandum-statement” should be deducible from two sets of statements, which serve as the explanans: (1) initial conditions (which must have empirical content) and (2) general and derivative laws and theories. Hence the schema—

$$\begin{array}{ccc} \text{Explanans} \left\{ \begin{array}{l} C_1, C_2, \dots, C_k \\ L_1, L_2, \dots, L_r \end{array} \right. & \frac{}{E} & \\ \text{Explanandum} & E & \text{(D-N model)} \end{array}$$

In addition, the D-N model requires that the explanandum statement not be deducible from a set of particular facts alone (Hempel 1965, 337). The explanation should be *nomological* as well as *deductive*. Thus, at least one law statement must be necessary for the inference. The point of this semantic stipulation is to prevent “explanations” which invoke laws either not at all or only in a trivial manner. For example, the fact that the Moon is waxing gibbous this evening follows from the conjunction of the facts that Venus is waxing gibbous and so is the Moon. But no law has been invoked in this “explanation.”

## 2.2. ANOMALY IN EXPLANATION: MODELS OF THEORY FALSIFICATION

The notion underlying theory falsification is that scientific theories need to be able to make predictions and thus be subject to empirical testing (Popper [1963] 2002, 47-48). The scientist endeavors to discern the predictive power of a scientific theory (i.e., a

*conjecture*) and to devise a *challenge* with the aim of *refuting* it. A failure to meet a challenge results in an instance of *inconsistency*.

The following presentation of theory falsification is based on Lakatos' *Falsification and the Methodology of Scientific Research Programmes* (1978). I have selected this text for several reasons. First, Lakatos clearly defines anomaly in terms of explanation: "An anomaly in a research programme is a phenomenon which we regard as something to be explained in terms of the programme" (1978, 72). Second, he clearly defines anomaly with respect to a deductive model. His question about the conditions under which a statement  $O$  can be taken to refute a theory is identical to the question about the conditions under which a statement  $O$  can be known to express a theoretical anomaly. Third, his discussion of theory falsification culminates with a concept of anomaly which has some of the desired features outlined in the introduction: that, for example, an anomaly's kind is known once it has been successfully explained and not before; or that an anomaly (e.g., a "monster anomaly") has a lifespan.

2.2.1. DOGMATIC MODEL OF THEORY FALSIFICATION. The first model of falsification has direct refutation for a goal, and is typical of what Lakatos calls "dogmatic falsificationism" (Lakatos 1978, 12ff.). It reflects a common view of scientific research according to which scientists confront theories with observations and throw out the former if the latter do not agree. Newton's "crucial experiments" were contrived for this theory-testing end. With the help of symbolic logic, Popper formulated a falsificationist model of theory-testing by expanding on this approach (see also Hempel 1966, 7). In its "dogmatic" form, falsification consists of a simple *modus tollens* between a theory statement  $T$  and an observation statement  $O$  that the theory should predict or explain:

$$\begin{array}{c|l}
1 & T \rightarrow O \\
2 & \neg O \\
\hline
3 & \neg T
\end{array} \quad (\text{Dogmatic model})$$

An anomaly, under the dogmatic model, is the true negation of an observation statement  $O$  such that  $T$  and  $\neg O$  form an inconsistent set of statements.

While each model of falsification varies, they all conform to the following schema:

$$\begin{array}{c|l}
1 & S \rightarrow O \\
2 & \neg O \\
\hline
3 & \neg T
\end{array} \quad (\text{Schema for falsification models})$$

where  $S$  stands for a conjunction of statements,  $S_1 \wedge \dots \wedge S_n$ . In the following sections, I will refer to an anomaly under the models of falsification as  $O_{\text{anomaly},S}$ .  $O_{\text{anomaly},S}$  is the true negation of an observation statement  $O$  such that  $S$  and  $\neg O$  form an inconsistent set of statements. Thus, an anomaly under the dogmatic model will be referred to as  $O_{\text{anomaly},T}$ .

**2.2.2. PROBLEMS WITH THE DOGMATIC MODEL.** The utility of this model for anomaly resolution would be limited to theoretical anomalies. Simply, if an anomaly were theoretical, one could plug a theory into the dogmatic model and refute it. It would not be necessary to develop a successful explanation of  $O_{\text{anomaly},T}$  to determine the anomaly kind. Indeed, anomalies would have virtually no lifespan. At the very moment an anomaly is discovered—the moment  $O_{\text{anomaly},T}$  is found inconsistent with  $T$ — $T$  is refuted and the anomaly becomes part of history. But this is counterintuitive, since the history of science contains anomalies which outlive generations of scientists.

A second major problem for the dogmatic model is a consequence of a key assumption on which it rests: that scientific theories *forbid* observable states of affairs which can be expressed by observation statements (i.e., the class of  $O_{\text{anomaly},T}$  state-

ments). The notion that a scientific theory should forbid certain states of affairs is critical to any account of theoretical anomaly. Those holding this claim believe that scientists, when formulating a theory, do so by circumscribing ahead of time all the possible states of affairs which could potentially give rise to anomalies for that theory. The connection between theoretical anomaly and scientific theory thus becomes fairly strong: as before, theoretical anomalies are defined in terms of theories; but now, scientific theories are also defined in terms of their *potential* theoretical anomalies.

A difficulty faced by those defining scientific theories in this way is that many prominent theories do not appear to forbid any states of affairs. Let's take for an example a well-known theory:

(LCLM) The total momentum of the system of two colliding particles remains unchanged by the collision, i.e., the total linear momentum is a conserved quantity. (French 1971, 310)

What exactly does the law of conservation of linear momentum *forbid*? Suppose two pool balls collide and then roll to a halt. To claim that total momentum is unchanged by the collision, we would need to factor in whichever forces brought the balls to a halt, such as friction from the felt and gravity. So indeed conservation of momentum only forbids that the total momentum of the system of two colliding particles decreases when every relevant factor affecting that system has been accounted for. Thus the theory alone does not forbid a state of affairs; a *ceteris paribus* clause is also needed. This requirement, which is implicit in conservation laws, that all relevant factors (e.g., causes of perturbation) have been accounted for before it can be made non-vacuously true severely limits the scope of application of dogmatic falsification.

2.2.3. MONOTHEORETICAL DEDUCTIVE MODEL. There are further complications for theory falsification. The major ones center around the following concern: isolation of a theory to be tested should not be taken for granted. The solitude of the *T* statement



in the antecedent of the dogmatic model may be a desideratum, but it is unlikely. Next, I will summarize three expansions on the dogmatic model: (1) the addition of auxiliary hypotheses, (2) the replacement of a single theory with a series of theories, and (3) the addition of a successor theory to  $T$ . Each of these expansions entails an increasingly sophisticated view of anomaly.

Rarely do interesting theories in isolation make predictions. A quick glance back to the D-N model should reveal a conspicuous oversight in the dogmatic model: if initial conditions should be among the explanatory premises of the D-N model, why would they not be conjoined with theory  $T$  in the theory-testing model? One explanation is simply that the initial conditions, presumably specified by observation statements, have already merged into the unproblematic background of theory  $T$ . Nonetheless, this can hardly be assumed at the *outset*. Initial conditions themselves are also hypothetical in nature. We can determine the mass and velocity for each colliding particle, but if linear momentum appears not to have been conserved as predicted, an obvious step for the scientist to take is to re-examine the values for mass and velocity expressed in the initial conditions, as well as the means by which those values were obtained, prior to declaring LCLM false. Troubleshooting procedures like this attest to the implicit (if not explicit) use of auxiliary hypotheses.

The monotheoretical deductive model is an instance of *modus tollens* involving (1) a material conditional whose antecedent consists of the conjunction of a theory  $T$  with various auxiliary hypotheses  $A_1, A_2, \dots, A_n$  and whose consequent is an observation statement  $O$  derived from the statements in the antecedent, and (2) the negation of  $O$ . The model is as follows:

1	$(A_1 \wedge \cdots \wedge A_n \wedge T) \rightarrow O$	
2	$\neg O$	
3	$\neg(A_1 \wedge \cdots \wedge A_n \wedge T)$	(Monotheoretical deductive model)

An anomaly, under the monotheoretical deductive model, is the true negation of an observation statement  $O$  such that  $(A_1 \wedge \cdots \wedge A_n \wedge T)$  and  $\neg O$  form an inconsistent set of statements.

Defined in terms of auxiliary hypotheses as well as the theory being tested,  $O_{\text{anomaly}, \bar{A}, T}$  can represent both theoretical and factual anomalies. Theoretical anomalies occur where  $O_{\text{anomaly}, \bar{A}, T}$  is inconsistent with theory  $T$ ; factual anomalies where  $O_{\text{anomaly}, \bar{A}, T}$  is inconsistent with one or more auxiliary hypothesis  $A_1, A_2, \dots, A_n$ .

2.2.4. PROBLEMS WITH THE MONOTHEORETICAL DEDUCTIVE MODEL. The monotheoretical deductive model reflects well the notion that when anomalies first appear, their kind is usually unknown. In this case, the model shows clearly that there is some sort of anomaly. But it also shows that a model of falsification provides no guide to determining which kind of anomaly has appeared: the syntax only tells us that the anomaly is either factual or theoretical. Unlike the dogmatic model, according to which anomalies have virtually no lifespan, anomalies under the monotheoretical deductive model seem, at least on logical grounds, unresolvable.

The monotheoretical deductive model has often shown up in the context of discussions of the Duhem-Quine thesis. The thrust of the Duhem-Quine thesis is primarily negative and amounts to a rejection of the dogmatic model (see Duhem [1906] 1954, 185 and Quine 1951, 39-40). Both Duhem and Quine argue that the direct refutation of a theory is impossible on the grounds that a single theory cannot be isolated. The main difference between Duhem and Quine concerns the problem of when, if at all, a theory

can indeed be refuted. For Duhem, *bon sens* is employed to determine when auxiliary hypotheses have been adequately enumerated and verified. (Thus, for Duhem, using *bon sens* could loosely be called a heuristic strategy.) Quine, in contrast, seems to believe that *definitive* refutation of a theory is unattainable since the auxiliary hypotheses include claims as basic as the validity of one's arithmetic and logic (which could always turn out to be false). (For more on the differences between Duhem and Quine in this regard, see Gillies [1993] 1998.)

In like manner, the *ceteris paribus* clause also poses a problem for the monotheoretical model. The dogmatic falsificationist seeks to ignore it, but is stuck in the predicament of being unable to tackle many important scientific theories. But if the theory being tested states, for example, that momentum is conserved if the system is closed, then a statement expressing the *ceteris paribus* clause (e.g., “the system is closed” or “there are no relevant but unaccounted for factors”) stands in need of verification. But it is not clear how such statements could be verified, since any number of true observation statements would be logically insufficient to prove a negative existential claim of this sort.<sup>8</sup> The consequences for anomaly are considerable: a given anomaly could potentially survive indefinitely unless the *ceteris paribus* premise can be established as true.

A second problem with the monotheoretical deductive model emerges when falsificationists employ a methodology for making decisions when the model itself does not compel rational choices. Lakatos considers the monotheoretical deductive model typical of “naive methodological falsificationism” (Lakatos 1978, 44). The shortcoming

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<sup>8</sup>Some would take a statement expressing the *ceteris paribus* clause to be an auxiliary hypothesis. See (Lakatos 1978, 98) for why this could be considered a “blunder.” Instead of expressing the *ceteris paribus* clause as an explanatory premise, one can simply note that the number of auxiliary hypotheses is indefinite. Noting that there could always be another, unaccounted for auxiliary hypothesis is the same as saying that a statement expressing the *ceteris paribus clause* is unverifiable in principle.

of the monotheoretical deductive model is that it can still manage to isolate a single theory. So when employed in conjunction with a methodology, the monotheoretical model converges on the dogmatic model. Consider the following methodological schema. At the moment an anomaly appears, one proceeds to verify rigorously one's initial conditions. One determines the truth-value of each of the the auxiliary hypotheses, one by one, and finally one decides if the *ceteris paribus* clause has been satisfied (see the "fourth decision" in Lakatos 1978, 26-28). If one decides that the auxiliary hypotheses have been exhausted and are all true, then one can then refute the theory, just as one would using the dogmatic model, that is, without a successful explanation of the anomalous phenomenon in hand. And we end up with an implausible view of anomaly like that of the dogmatic model. Scientists, as a community, have historically shown themselves unwilling to consider auxiliary hypotheses or a *ceteris paribus* premise well-corroborated solely for the purpose of refuting an important law if there is no successor theory available. Thus, scientists are unlikely to believe that an anomaly is theoretical (and not factual or phenomenal) if the anomalous phenomenon has not been successfully explained by the successor theory.

2.2.5. PLURALISTIC DEDUCTIVE MODEL AND SOPHISTICATED METHODOLOGICAL FALSIFICATION. In this section, I will discuss two revisions to the monotheoretical deductive model offered by Lakatos. The first, which he calls the *pluralistic deductive model*, incorporates a second expansion of the antecedent of the dogmatic model. It is important to note that the methodological purpose of the pluralistic model is different than that of the monotheoretical model: it is used not for theory refutation, but rather for showing how theories survive in the midst of anomalies which would seem to refute them and for showing how theories can be progressively modified without losing their identity. The second revision, however, does describe how explanatory theories are refuted: the result is a model for "sophisticated methodological falsification."

An anomaly may appear which, in the light of a model of falsification, seems to refute a theory; but the theory is neither rejected nor replaced as a result. In fact this is usually the case: as Laudan notes, the history of science does not readily provide examples of theories unaccompanied by anomalies; if scientists let anomalies directly refute their theories, we would have no theories left at all (Laudan 1977, 27-28). The example that Lakatos uses is Prout's theory ("a research programme progressing in an ocean of anomalies") (Lakatos 1978, 43-45, 53-55). According to Prout's theory, since all elements are compounds of hydrogen, the atomic weight of any pure chemical element is a whole number. However, according to Stas's experiments, the atomic weight of chlorine was 35.5. Stas's experimental setup was scrutinized and found to be adequate. Thus, the auxiliary hypotheses were thought to be true. Nonetheless, even though the Proutians had their critics, the theory was kept.

According to Lakatos, this case illustrates the inadequacy of the monotheoretical deductive model. There is not merely a single theory involved in a research programme, but rather a plurality of theories. The plurality of theories is divided into two kinds: *explanatory* and *interpretative* (or *observational*) theories. Prout's theory is an example of an explanatory theory. Interpretative theories, on the other hand, told chemists the conditions under which explanatory theories applied to their experimental results. Pure chemical elements, it was believed, were obtained through chemical separation; only later was it realized that pure elements were obtainable through physical separation. Thus, it was the interpretative theory which was inconsistent with the explanatory theory and the anomalous observation statement. Lakatos concludes:

In a monotheoretical model we regard the higher-level theory as an *explanatory theory to be judged by the "facts"* delivered from outside (by the authoritative experimentalist): in the case of a clash we reject the explanation. In a pluralistic model we may decide, alternatively, to regard the higher-level theory as an *interpretative theory to judge the "facts"* delivered

from outside: in case of a clash we may reject the “facts” as “monsters.” In a pluralistic model of testing, several theories—more or less deductively organized—are soldered together. ... It is not that we propose a theory and Nature may shout NO; rather, we propose a maze of theories, and Nature may shout INCONSISTENT. (Lakatos 1978, 45)

Lakatos’ revision to the monotheoretical deductive model conjoins an unspecified number of interpretative theories  $T_{I1}, \dots, T_{In}$  to the antecedent (which already includes auxiliary hypotheses and the explanatory theory  $T_E$ ). The following model is the result:

$$\begin{array}{l|l} 1 & (A_1 \wedge \dots \wedge A_n \wedge T_E \wedge T_{I1} \wedge \dots \wedge T_{In}) \rightarrow O \\ 2 & \neg O \\ \hline 3 & \neg(A_1 \wedge \dots \wedge A_n \wedge T_E \wedge T_{I1} \wedge \dots \wedge T_{In}) \end{array} \quad \text{(Pluralistic deductive model)}$$

It’s worth reiterating that the pluralistic deductive model is not suited for refutation of the explanatory theory. At this point, the farthest that theory falsification can proceed down a path towards refutation is to say that there is an inconsistency in the theory group, once methodological decisions are made which state that the truth conditions for the auxiliary hypotheses and for any *ceteris paribus* premise have been met. From there, the methodology typically calls for altering one of the interpretative theories to restore consistency in the theory group. Interpretative theories, unlike auxiliary hypotheses, cannot merge with an unproblematic background. It is for this reason, and not on syntactic grounds, that the pluralistic model cannot converge on a monotheoretical model.

For falsification of an explanatory theory to take place, a successor theory must be in hand. This is the second condition for “sophisticated methodological falsification.” A successor theory has the explanatory power of the theory it replaces, and can, in addition, explain some outstanding anomalies. Laws of Newtonian mechanics

are subject to falsification by an anomaly only after relativistic mechanics explains all of the phenomena explained by Newtonian mechanics, and moreover, explains the refuting anomaly (namely, the perihelion advance of Mercury). Of course, there could be anomalies under a theory which are not explained by the successor theory; but we could not say of these anomalies that they refute the theory. Moreover, if the successor theory does not explain an anomaly, then it remains unknown which kind of anomaly it is.

A model for “sophisticated methodological falsification” would include the successor theory to explanatory theory  $T$ . Furthermore, the successor theory  $T'$  must be consistent with the anomaly  $O_{\text{anomaly}, \bar{A}, \bar{T}_I, T_E}$ .

$$\begin{array}{c|l}
 1 & (T \rightarrow O) \leftrightarrow T' \\
 2 & \neg O \\
 3 & T' \\
 \hline
 4 & T \rightarrow O \\
 5 & \neg T
 \end{array} \quad \text{("Sophisticated falsification")}$$

An anomaly, under the pluralistic deductive model (using sophisticated falsification), is the true negation of an observation statement  $O$  such that  $(A_1 \wedge \dots \wedge A_n \wedge T_E \wedge T_{I1} \wedge \dots \wedge T_{In})$  and  $\neg O$  form an inconsistent set of statements. We stipulate first that a theoretical anomaly is an inconsistency in the conjunction of  $O_{\text{anomaly}, \bar{A}, \bar{T}_I, T_E}$  and the theory group  $T_E, T_{I1}, \dots, T_{In}$ . Secondly, an anomaly caused by a false explanatory theory statement is the true negation of an observation statement  $O$  such that  $T$  and  $\neg O$  are inconsistent while the successor theory  $T'$  and  $\neg O$  are consistent.

2.2.6. SOME SOLUTIONS FROM THE PLURALISTIC DEDUCTIVE MODEL (USING SOPHISTICATED FALSIFICATION). Many of the gains made by the pluralistic deductive model over the monotheoretical deductive model are heuristically useful. First, from the claim that explanatory theories cannot be refuted unless a successor theory is available,

it follows that the anomaly kind of an anomaly caused by false explanatory theories cannot be known prior to the successful explanation of that anomaly. Heuristic methods should take this epistemic limit about anomaly kinds into consideration. But neither the dogmatic nor the monotheoretical deductive model acknowledge this limit.

Second, theoretical anomalies can be approached with a new strategy. One can treat an anomaly as a “monster” and set about examining and adjusting one’s interpretative theories so that one’s theory group is consistent. Several kinds of discoveries can result from scrutiny of one’s interpretative theories: tacit interpretative theories are made explicit (e.g., that certain purification techniques yield pure samples); experimental procedures are questioned or improved (e.g., new purification techniques are adopted); the meanings of theoretical terms are redefined (e.g., that a “pure” sample need not result from chemical separation).<sup>9</sup>

Third, anomaly resolution no longer needs to be concerned with fulfilling truth-conditions for statements expressing the *ceteris paribus* clause. Without a methodological decision, anomaly resolution would never finish insofar as statements expressing the *ceteris paribus* clause would be negative existential claims. But such premises are redundant within sophisticated falsification (Lakatos 1978, 98): having a successor theory in hand is sufficient condition for the refutation of an explanatory theory.

Ending my account of the linguistic view here, I do not presume in any way that the pluralistic deductive model and sophisticated methodological falsification do not have problems of their own. Nor do I suppose that a stronger version of a linguistic view of anomaly could not be elaborated. My goal here has been simply to present a minimally compelling linguistic view; by which I mean, reasonably coherent as well as relevant to actual scientific practice. Furthermore, I have tried to clarify what is meant

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<sup>9</sup>For a more recent account of heuristic strategies concerning “monster-barring” and anomaly-based theory revision, see Darden 1990, *passim*, and 1991, 269-274.



when anomalies are called “inconsistencies”; as the linguistic view develops, it should become even clearer.

### 2.3. HEURISTIC UTILITY OF LINGUISTIC VIEW IN DETERMINING IF THERE IS AN ANOMALY IN EXPLANATION

To the extent that a model of explanation fails to distinguish explanations from non-explanations, the view of anomaly based on that model will also fail to separate anomalies in explanation from non-explanatory anomalies or non-anomalies. The standard counterexamples to the D-N model typically invoke instances of non-explanations which fit all the definitional requirements of the D-N model (Salmon 1989, 46-50). For example, one could conceive of a law whereby if the barometer gives a reading below a certain pressure, then a storm will occur. Such a law in conjunction with a statement signifying a sufficient drop in barometric pressure fulfill the requirements for an explanans under the D-N model, provided that a storm does indeed occur. Nevertheless, it is counterintuitive to say that a barometer reading and a law linking barometer readings to storm activity explain a storm. We expect an explanation to respect causality: the explanans should express the cause of the explanandum-phenomenon. In this case, the storm and the barometer reading have common atmospheric causes, but the latter does not regularly cause the other (Salmon 1989, 47).

The D-N model has been confronted with many other counterexamples which show it to be indiscriminate: a set of laws and facts could meet the requirements to be an explanans and yet fail to refer to one. The D-N model transmits this problem to the linguistic view of anomaly. Imagine that the storm was an anomalous explanandum-phenomenon. We would like to resolve the anomaly, and our heuristics would probably involve probing and revising the laws and the relevant facts that together *should* explain the explanandum-phenomenon (but do not). But it would be pointless to consider revising the “barometer” law above, because it never contributed to a satisfactory ex-

planans. Yet the D-N model left the door open to it. Just as the D-N model can accommodate non-explanations, the linguistic view has the potential to distract the resolution of anomalies by mistaking non-explanatory anomalies for anomalies in explanation.

Another set of difficulties for the D-N model concern legitimate types of scientific explanation which do not fit. Functional analysis is a well-known example (for details, see Salmon (1989, 26-32)). The D-N model often does not accommodate mechanistic explanation (this limitation will be examined in §§ 4.2). Thus, anomalies involving mechanistically-produced phenomena therefore may not be expressible in nomological terms, and therefore may not be detected by the linguistic view.

A third limitation of the linguistic view is that anomalies do not become apparent except as logical inconsistencies.<sup>10</sup> This means that any anomaly presupposes that the theoretical, factual, and phenomenal content has been adequately expressed in statements. Only then can they be placed in a deductive structure and a logical inconsistency arise. The language requirement could be questioned in two ways, linguistically and cognitively. First, it could be argued that scientific theories and phenomena are resistant to being adequately articulated in language. Second, there is the cognitive issue concerning how actual scientists recognize and deal with anomalies. It could be argued that anomalies are recognized by scientists prior to such articulation (see § 3).

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<sup>10</sup>Defining anomaly in terms of logical consistency is not merely a convenient way of looking at things. That science must proceed from a logically consistent foundation has been a widely held belief in 20th c. philosophy of science. Consider Lakatos' critique of Bohr for his willingness to leave anomalies unresolved in early quantum mechanics:

But *consistency*—in a strong sense of the term [Two propositions are inconsistent if their conjunction has no model, that is, there is no interpretation of their descriptive terms in which the conjunction is true. (note in original)]—must remain an important regulative principle (over and above the requirement of progressive problemshift); and inconsistencies (including anomalies) *must* be seen as problems. The reason is simple. If science aims at truth, it must aim at consistency; if it resigns consistency, it resigns truth. (Lakatos 1978, 57-58)

Moreover, scientists may be able to resolve anomalies without complete articulation of the anomaly in language. This last point will be discussed in §4.4.

The linguistic view also has a certain advantage related to the limitation imposed by the language requirement. An anomaly may very well be recognized prior to its linguistic articulation, but it may also escape the notice of many in the scientific community; but if one succeeds in expressing an anomaly as a logical inconsistency, then it is highly perspicuous to anyone with a grasp of rudimentary logic. Herein lies the primary heuristic utility of the linguistic view. The linguistic view, even if it falls short of providing an adequate account of anomaly, could provide effective tools for communication and thereby enable the scientific community at large to discover that there is an anomaly.

### § 3. THE PERCEPTUAL VIEW OF ANOMALY

During the historicist turn in the philosophy of science, many involved in science studies began to look at anomalies from the perspective of how they actually appear to historically-situated scientists. The perceptual view of anomaly differs from the linguistic view in that anomaly refers not to a class of observation statements but to a category of phenomena. Those who maintain the perceptual view need not deny that anomalies can be expressed as true observation statements giving rise to logical inconsistency within a deductive model. They simply deny that this is all that an anomaly is. Anomalies are at first *perceived* before they are expressed (if they are expressed) linguistically. Thus, the phenomenon to which  $O_{anomaly,S}$  refers is always already anomalous.

The most well-known exposition of the perceptual view of anomaly is found in the writings of Thomas Kuhn. Below, I will reconstruct the perceptual view based on Kuhn's, Hanson's, and post-Kuhnian philosophy of science. But I would like to caution the reader: endorsing a perceptual view of anomaly does not require acceptance of Kuhn's account of scientific change (e.g., paradigm shifts, incommensurability, and so on). I'll refer to the explanatory model on which the perceptual view is based as "explanation during normal science." This is merely for the sake of economy. As should become clear, one can adopt this explanatory model without committing to Kuhn's conception of normal science as a stage in the historical development of a scientific discipline.

#### 3.1. EXPLANATORY SUCCESS: NORMAL SCIENCE

The road to successful explanation for normal science differs from that for D-N explanation and Popperian falsification. D-N explanation begins with a phenomenon in need of explanation; the phenomenon is expressed by an explanandum-statement

which is assumed to be true; one or several *potential* explanantia are formulated; and then, according to the hypothetico-deductive method, potential explanantia are rejected until one remains which appears confirmed. Theory falsification runs roughly parallel: it begins with a conjecture which has both explanatory power and circumscribes the phenomena which it would fail to explain, should they appear; an indefinite theory-testing phase follows; should the conjecture survive a certain period of time, it may be accepted as corroborated.

Explanation in normal science begins with a theory—a *paradigm*—which is assumed to be true (a “disciplinary matrix”) and which is also present as a prototype of successful explanation (an “exemplar”); it continues with the search for another phenomenon which could potentially be explained under the paradigm (an unsolved “problem” belonging to the paradigm); a successful explanation is arrived at when such a phenomenon is found and is fit into one of the explanatory patterns of the paradigm (i.e., resembles one of the exemplary “problem-solutions”) (Kuhn [1962] 1996, 174-175).

In linguistic models of the explanatory process, a set of phenomena, expressible in true observation statements, are given at the outset, whereas theories may or may not be compatible with them. The activity of inventing theories to explain phenomena seems to be autonomous of the phenomena. Even for those like Popper and Lakatos, who reject theory justification, discovery remains mysterious; indeed, for them, it is even more causally disconnected from the regularities of natural phenomena than it was for the positivists. “Science does not rest on rock-bottom,” Popper is famous for saying, “the bold structure of its theories rises, as it were, above a swamp” ([1934] 1961, 111). In the same vein, Lakatos writes (with a touch of romanticism) that “the direction of science is determined primarily by human creative imagination and not by the universe of facts which surrounds us” (1978, 99).

In contrast, in normal science, it is given that a theory, or paradigm, has been fully accepted; it is rather the phenomena to which they potentially apply which begin partially undetermined. Moreover, paradigms are not causally disconnected from the phenomena appearing under them. A paradigm presents unsolved problems to scientists working under it. As a disciplinary matrix, a paradigm includes all the pedagogical means, linguistic and non-linguistic, needed to train scientists to solve outstanding problems. This training includes teaching scientists how to experience natural phenomena to which, untrained, they would have previously been insensible. For example, the paradigm of Newtonian mechanics is more than just a set of equations and laws; it also directs students through hands-on experimental situations so that they learn to recognize the phenomena referred to as mass, force, and acceleration. Change in paradigm entails change in the phenomenal world inhabited by the scientists.<sup>11</sup> Thus, without the paradigm, the phenomena to be explained would not even *appear* to them. In §§ 3.1.1 and 3.1.2, I will present two approaches to interpreting this provocative claim.

3.1.1. A KANTIAN INTERPRETATION. Kuhn's philosophy of science bears more than a passing resemblance to Kant's. Kuhn holds that the objects of perception with which a scientist is concerned are constituted by the paradigm. The constitutive role of the paradigm is similar then to that of a priori forms of experience (pure intuitions) such as Euclidean space. Unlike Kant, however, Kuhn held that that which constitutes (from the side of the subject) the scientists' phenomenal world changes; thus, the apriority of the paradigm does not entail invariance. Many have criticized Kuhn for the "plurality-of-phenomenal-worlds thesis,"<sup>12</sup> since it would seem to entail a relativism which would

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<sup>11</sup> "Paradigm changes do cause scientists to see the world of their research-engagement differently. In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world" (Kuhn [1962] 1996, 111).

<sup>12</sup> Hoyningen-Huene (See 1993, 36ff.).

undermine any account of scientific rationality. Whether rationality is sacrificed when affirming the thesis has been debatable. There is however a deeper problem plaguing Kuhn's Kantian metaphysics which is directly relevant to his account of anomaly.

The paradigm is the subject-sided factor constituting the phenomenal world; but what is the object-sided constitutive factor? For Kuhn, there must be some factor co-constituting phenomenal worlds other than paradigms, something which would remain unchanged by a paradigm shift or which would support multiple synchronous paradigms. This is made particularly evident by that fact that paradigms are not forcibly tied down to a particular historical moment; individuals living at the same time can work within different paradigms. This is almost certain to happen immediately after a major theory change: Newtonians remained in the midst of those working under the new paradigm of relativistic physics. Another example might be folk science: it seems that most of us, prior to any training in modern physics, inhabit a paradigm of vaguely Aristotelian conceptions of motion.

Moreover, what would the relation of a scientist to this world external to phenomena amount to? If empirical knowledge of a world independent of scientific paradigms should be possible, then scientists would have the means to judge which paradigm corresponds best to the external world. On the other hand, if access to it were not possible, knowledge about it might be obtained by inference from the differences between successive paradigms. Kuhn rejects both sorts of realism: first, because the paradigm is the condition of possibility of scientific experience, which is to say, a scientist cannot step outside of her paradigm, unless into another one; second, Kuhn's account of scientific progress and rationality does not depend on the convergence of paradigms on a true description of an external world, and he explicitly denies that the latter takes place (Kuhn [1962] 1996, 160-161). Kuhn is left, despite himself, with having

tacitly to adopt a concept of a world-in-itself, forever inaccessible to scientific inquiry, which strongly resembles the Kantian things-in-themselves.<sup>13</sup>

And finally, the clearest indication that phenomena are co-constituted by an external world to which scientists have no access is the existence of anomaly itself. As Hoyningen-Huene remarks—

The repeated occurrence of significant anomalies in the history of science can, in fact, prove one motive for assuming the existence of a theory-independent, and by its own determinate, proprietary features resistant, world-in-itself. (Hoyningen-Huene 1993, 227)

Interpreting Kuhn in light of Carnap, and Carnap in light of Neokantianism has reasonable support in the history of the philosophy of science (see Friedman 2001, 41-44; Boyd 1984, 56; Friedman 1999). However, no sooner does Kuhn say something with idealist overtones such as “when paradigms change, the world itself changes with them” ([1962] 1996, III), than he begins discussing the latest psychological theories of perception as would a naturalist. Hence, it’s also possible to consider explanation under normal science from a cognitive perspective, and leave aside the difficult metaphysical commitments entailed by the Kantian perspective.<sup>14</sup>

3.1.2. A GESTALTIST INTERPRETATION. Unlike the explananda-phenomena of D-N explanations which are simply *given*, a phenomenon to be explained during normal

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<sup>13</sup>Hoyningen-Huene comments on Kuhn’s position in 1979:

Kuhn now explicitly does without the world-in-itself, the “one real world, still unknown.” He describes his position as “also Kantian, but without the ‘things-in-themselves’” (and with temporally mutable categories). Though Kuhn claims his position to be a “realistic” one, the precise meaning of this claim remains, admittedly, unexplained (Hoyningen-Huene 1993, 60).

<sup>14</sup>Ron Giere makes a similar comment:

Kuhn’s official theory is the stage theory. But in fact, I think what he is groping toward is a cognitive theory. He picked Gestalt psychology because that was the best thing around in the late 1950s. (Callebaut 1993, 352)



science first needs to be constituted. Adapting a thesis articulated by Hanson, Kuhn describes this constitution process in terms of gestalt-switching. Consider an isometric line drawing of a cube (i.e., a Necker cube). Gathering the same information from the same visual field, one can perceive several distinctly different forms: a transparent cube as seen from below, a transparent cube as seen from above, a solid irregular polyhedron resembling a well-cut diamond, and so on (Hanson 1969, 86-87). Thus a single concrete drawing seems to have the potential to produce multiple phenomena in our perception. But that in virtue of which one of those phenomena is actualized and in virtue of which we see an actual geometrical shape is the paradigm. Hanson claims that learning to see as a scientist does is a similar process: a simple drawing of a cathode-ray tube may be meaningless to the non-physicist, but to the physicist would be immediately recognizable.

Moreover, not only do the same stimuli have the potential to produce different phenomena, but different stimuli have the the potential to produce the same phenomena. To exemplify this point, Kuhn refers to Stratton's experiments on the inversion of the retinal image. Test subjects were outfitted with inverting goggles over an extended period of time. Initially, their visual field appeared upside-down to them, and they experienced disorientation. In a matter of days, however, they had compensated such that they no longer perceived the inversion (Kuhn [1962] 1996, 112). Likewise, a scientist will see the same phenomena appear when immersed in different stimuli. Her ability to do so is conditioned by the paradigm in which she is operating.

The difficulty with accounting for the initial stages of the explanatory process in terms of gestalt experiments is that the cognitive processes studied in those experiments and those involved in scientific explanation may not be the same. Kuhn recognized this difficulty explicitly (Kuhn [1962] 1996, 113-114). However, the analogy between the experiments and his readings of the history of science proved provocative

enough for him to stretch the concept of gestalt enough to accommodate his views on scientific explanation.

Whenever a phenomenon to be explained is psychologically constituted under normal science, the subsequent explanatory process is somewhat less onerous than what it would be if, say, the hypothetico-deductive method were being pursued. The explanatory theory, after all, is assumed to be true. Moreover, because of the constitutive role of the paradigm, the phenomenon to be explained is, in a sense, already explicable, if not already partially explained. Scientific explanation as “puzzle-solving” should be taken quite literally in this context: the scientist is taught a pattern—the gestalt—ahead of time and the pieces—the phenomena—are laid out before her. She proceeds then to arrange the phenomena into the gestalt—to recognize how they fit the pattern—and thereby procures a successful explanation. Pieces, however, frequently do not fit.

### 3.2. ANOMALY UNDER NORMAL SCIENTIFIC EXPLANATION

An anomaly, under normal scientific explanation, is a perceptual event which fails to be adequately represented under the reigning theories (which nevertheless contribute in some sense to the possibility of the event). Philosophers of science have conceived of the nature of this contribution in several ways: metaphysically, as a priori conditions of possibility for the appearance of phenomena to scientists; linguistically, as a priori conditions—presuppositions—necessary for terms in scientific language to refer (Friedman 2001, 74); and cognitively, as psychologically-conceived sets of expectations which are subject to empirical study.

I would like to leave aside the aprioristic conceptions of anomaly if only because the Kantian metaphysics are highly problematic and the linguistic understanding in fact reveals nothing about the actual perception of anomalies. Instead, I would like to focus on the cognitive conception. Kuhn, at one point, defines anomaly as an

experience difficult to put into words, “a phenomenon, that is, for which his paradigm had not readied the investigator” ([1962] 1996, 57). In his discussion of the Bruner and Postman playing card experiment, Kuhn refers to the irregular cards as “anomalous, e.g., a red six of spades and a black four of hearts” ([1962] 1996, 63). When the anomaly first appeared, the perceiver could only say “that something had gone wrong” ([1962] 1996, 57). Anomaly is, however, “the prelude to discovery” ([1962] 1996, 57). Once the anomaly is understood—for example, once a test subject identifies that there are anomalous playing cards and what they are—then “the initially anomalous becomes the anticipated,” and “at this point, the discovery has been completed” ([1962] 1996, 64).

The question if there is an anomaly is answered when it is perceived *that something is wrong*.<sup>15</sup> The perceiver’s set of expectations has been upset by a phenomenon, but the perceiver need not be able to describe how her expectations have been contravened nor even how the phenomenon appears. By the time the perceiver is able to articulate the nature of the anomaly, the anomaly has been resolved. Since the description of the anomalous phenomenon merges into the set of expectations, one could say that it has, at that point, been successfully explained.

It is important to note that the appearance of anomaly rides entirely on the competence of the perceivers. During the playing card experiment, there were test subjects who never resolved the anomaly: that is, they could never revise their theory that a deck of cards contains only black spades and clubs, red hearts and diamonds. Curiously, such perceivers seem to lose basic observational competence altogether, which seems to imply that they in fact do perceive the anomaly, but they cannot recognize it since they cannot revise their theory. Kuhn recounts:

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<sup>15</sup>The moment when this answer is given marks “the beginning of an episode of discovery,” a requisite of which is “the individual skill, wit, or genius to recognize that something has gone wrong in ways that may prove consequential” (1977, 173).

A few subjects, however, were never able to make the requisite adjustment of their categories. ...One of them exclaimed: "I can't make the suit out, whatever it is. It didn't even look like a card that time. I don't know what color it is now or whether it's a spade or a heart. I'm not even sure what a spade looks like. My God!" (Kuhn [1962] 1996, 63-64)

### 3.3. HEURISTIC UTILITY OF PERCEPTUAL VIEW IN DETERMINING IF THERE IS AN ANOMALY IN EXPLANATION

One problem for the linguistic view is that the explanatory models on which it is based falls short of providing the sufficient conditions for a scientific explanation. Because those explanatory models have trouble distinguishing between explanations and non-explanations, the linguistic view inherits this trouble when distinguishing between anomalies in explanation and non-explanatory anomalies. It's not clear that the perceptual view fares better. On the one hand, successful explanation is guaranteed by the paradigm. There's no risk of explaining the height of a flagpole by the length of its shadow since the exemplary problem-solutions of the paradigm do not include such "explanations." On the other, it's not clear why a paradigm would not include such "explanations," provided they were accepted as legitimate. For example, the necessary conditions for successful explanation may vary according to paradigm. Kepler's laws as Kepler formulated them lack explanatory power under Newtonian physics since they provide only a geometrical, not a dynamical, description of orbital motion. (Einstein makes a similar claim vis-à-vis Newton's gravitational laws.) Therefore, the heuristic utility of the perceptual view is limited insofar as it could allow for a non-explanatory anomaly to be mistaken for an anomaly in explanation.

Regarding inclusiveness of explanatory types, the perceptual view is useful, as it does not have the limitations of the linguistic view. Any type of explanation which has been practiced in the history of science can give rise to anomalies in explanation. The only limiting factor would be the explanatory forms practiced under the paradigm in which one is operating.

The perceptual view also seems less likely to miss certain anomalies on account of their not appearing as logical inconsistencies. The perceptual view has no language requirement. Scientists first perceive anomalies *qua* anomalies and then can express them linguistically. However, there is an issue concerning whether or not the cognitive constraint might also be restrictive. Need a scientist recognize that a phenomena fails to fit into a paradigmatic gestalt in order to say if there is an anomaly? In a sense, the linguistic view might seem less restrictive than the perceptual view: a minimal competence in logic would allow someone to say that there is an anomaly (provided, of course, that the necessary theoretical and observation statements have been articulated). This is not to say that some form of a cognitive constraint should not be accepted. Only that any such constraint should allow for the possibility that a scientist may be convinced that there is an anomaly by force of logic in situations where he has difficulty perceiving it. A scientist's belief that there is an anomaly on logical grounds<sup>16</sup> might further be coupled with a sort of blindsight: he may in fact be able to respond to anomalies without consciously perceiving them.

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<sup>16</sup>In addition, other forms of external representation may be employed besides language (see 4.4).

## §4. THE MECHANISTIC VIEW OF ANOMALY

### 4.1. EXAMPLE: PUPILLARY REFLEX

To begin the discussion of the mechanistic view, I would like to give an example of a mechanistic explanation. Suppose one sets out to explain the pupillary reflex of the human eye. One might correlate the constriction of the pupil with the intensity of the light passing through it. One could further note that when only one eye is exposed to light, the pupil of the unexposed eye nevertheless constricts (*consensual response*). Moreover, one could regiment the correlation in a formal language and test it using a falsification model.

Still, intuitively, one might think that there is something lacking in this explanation. It may appear that we have done no more than specify more precisely which phenomenon needs to be explained. When we set out to explain the pupillary reflex, we are also interested in *how* the pupils constrict when light passes through one or both of them. In other words, how are pupillary reflexes *produced*?

To answer this question, one would attempt to isolate the mechanism responsible for causing the phenomenon in need of explanation. When light enters the pupil at time  $t_n$ , a constriction of the pupil occurs at a later time  $t_{n+1}$ . Hence, one would specify the sequence of operations which take place in the mechanism between  $t_n$  and  $t_{n+1}$ : the *transduction* of a light signal into an electro-chemical signal; the *relaying* of the electro-chemical signal towards both pupils; the *processing* of the signal for determining if the pupil is to constrict; the *innervation* of the muscles which constrict the pupil; and the observed *constriction* of the pupil. Furthermore, one would specify the parts of the mechanism which carry out these operations. From this perspective, one is looking primarily at spatial rather than temporal arrangement. One would construct a neuroanatomical map (see figure 1), beginning at the location where light enters the

pupil and strikes the retina, and ending where the pupils constrict: the *retinal ganglion cells* involved in pupillary reflexes project to the *pretectum* in the midbrain; the pretectal cells project bilaterally to both *accessory oculomotor nuclei*; from accessory oculomotor nucleus, *preganglionic parasympathetic neurons* project to the *ciliary ganglion*; and *postganglionic parasympathetic neurons* project from the ciliary ganglion to the *pupillary sphincter*.

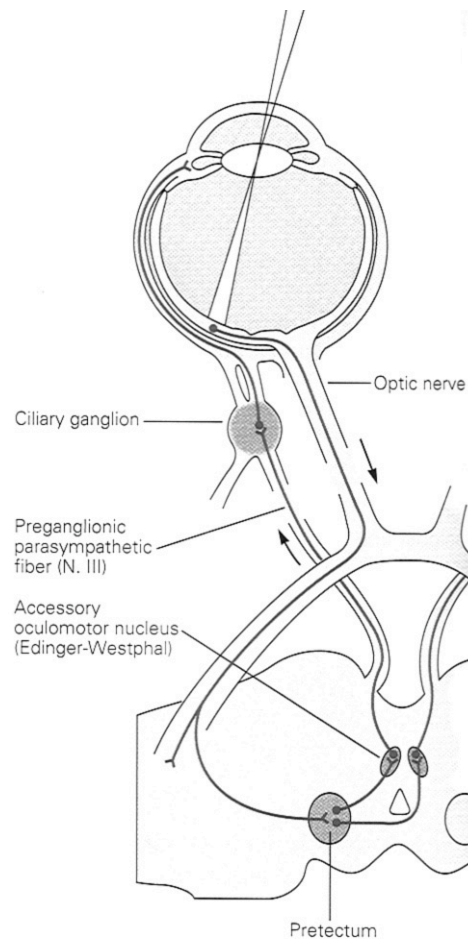


Figure 1: Diagram of pupillary reflex (Kandel et al. 2000, 528).

A finer-grained explanation, or one emphasizing particular features of the mechanism, can be made according to the level and character of explanation which is elicited

by the description of the phenomenon to be explained. For example, the explanation above would be serviceable for a clinician using pupillary reflexes to diagnose damage to the optic nerve, oculomotor nerve, or to the midbrain: a consensual pupillary response (i.e., from the eye not exposed to light) with no direct pupillary response (i.e., from the eye exposed to light) would indicate damage to the oculomotor nerve; bilateral pupillary response when one eye is exposed to light, but no response when the other eye is exposed would indicate damage to the optic nerve; complete lack of pupillary response may indicate damage to the midbrain, or damage to both optic nerves (Kandel et al. 2000, 528). However, the above explanation would need to be developed significantly if one were interested in explaining a particular feature, such as response time of the pupillary reflex.

#### 4.2. EXPLANATORY SUCCESS: MECHANISTIC EXPLANATION

4.2.1. MECHANISTIC MODELS. The mechanistic model aims to generalize scientific explanations in terms of the mechanisms underlying the phenomenon in need of explanation. In the above example, the phenomenon of pupillary reflex is explained in terms of the neuroanatomical parts and the orchestrated interactions of these parts within the reflex mechanism. The explanatory process consists of creating a model of the mechanism. A schematic model is derived from the phenomenon to be explained: light passing through the pupil is a *set-up condition*, and pupillary constriction is a result.<sup>17</sup> From there, the model goes through a process of *decomposition*: from set-up conditions to phenomenon to be explained, there are a number of mediating parts and activities to be discovered.

A mechanistic explanation includes a description of the spatial relations between the parts of a mechanism as well as a description of the temporal relations between

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<sup>17</sup>Although this mechanistic explanation of a pupillary reflex describes a linear process, mechanisms and their models are often non-linear. (Indeed, so is the pupillary reflex at a lower-level.) I have chosen a linear process for the sake of simplicity.



the interactions of these parts (Craver 2002, 387-388). In a mechanistic model, one aims to couple a description of a spatial relation (e.g., contiguity) between parts with a description of a temporal relation (e.g., succession) between the interactions of those parts. Above, a typical spatial relation is neuronal projection: e.g., the ciliary ganglion projects to the pupillary sphincter. A temporal relation between the interactions of these parts is also described: e.g., the preganglionic parasympathetic neurons synapse at the ciliary ganglion with the postganglionic parasympathetic neurons, which in turn innervate the pupillary sphincter. The coupling of descriptions of interactions with descriptions of parts of a mechanism is commonly called *localization*. Localization often involves some type of *interfield integration*: in this case, neuroanatomists define the parts and their layout while the electrophysiologists define the interactions and their sequence (Craver 2002, 388).

Accounts of mechanistic models of explanation come in several different metaphysical flavors, as can be seen from the labels mechanists give to complementary elements of a mechanism: let's assume we are referring to (1) a retinal receptor cell and (2) transduction. I have been referring to (1) as a part and (2) as an interaction. Bechtel, Richardson, and Abrahamsen would call both (1) and (2) a component which has two aspects: (1) is a component part and (2) is a component function or operation (Bechtel and Richardson 1993; Bechtel and Abrahamsen 2005, 423-424). In a dualistic vein, Machamer, Darden, and Craver would prefer calling (1) an entity and (2) an activity in order to emphasize the productive, processual character of (2) and the substantial, property-bearing character of (1) (2000, 3). Woodward and Glennan would call (2) an "invariant, change-relating generalization" to capture the notion that interactions are causally induced property-changes whose occurrence is law-like (Woodward 2002, S375; Glennan 2002, S344). As far as the overall functioning of a mechanism is concerned, Machamer, Darden, and Craver refer to *organization*, while Bechtel and Abrahamsen

refer, more lyrically, to *orchestration*. As Glennan remarks, all of these authors would consider a mechanism a *complex system*. (2002, S344); this is worth noting because although mechanists refer in their models of explanation to “set-up conditions” and “termination conditions,” mechanisms often consist of and participate in non-linear arrangements.

At this point, these differences in mechanistic metaphysics do not seem to me to be significant for the mechanistic view of anomaly. In this thesis, I use “parts” and “interactions” only because the terms have been in use for decades (Wimsatt [1976] 1984, 492-493) and I am not here trying to express my preferences in this dialogue. I think that other metaphysical conceptions of mechanism would substitute painlessly throughout this discussion.

4.2.2. MECHANISTIC V. NOMOLOGICAL EXPLANATION; LAWS IN MECHANISMS. As noted in § I, many are interested in mechanistic explanation these days because mechanisms are discovered in the biosciences far more readily than laws. This fact certainly makes mechanistic explanation relevant to philosophers of science and suffices to motivate those taking a naturalistic approach to their discipline (Darden and Craver 2005, 240-241). Nonetheless, there is more at stake for the proponents of mechanistic explanation than whether scientists in a certain discipline aim to discover mechanisms or laws. There is the question of whether certain nomological explanations can be replaced with mechanistic explanations, and vice-versa. On the one hand, several mechanists have, in recent years, developed their accounts of explanation in accordance with the physical sciences (which tend to explain phenomena in terms of laws): notably, Salmon (1984), Woodward (2002), and Glennan (2002). Unlike Galilean or Cartesian mechanistic philosophy, these accounts have been developed in light of mechanistically inexplicable phenomena in modern physics such as gravitation and electromagnetic fields. On the other hand, philosophy of biology has also seen efforts

to redescribe mechanisms in terms of nomological concepts (Schaffner 1993). Thus, there is a significant tension between the two models of explanation, which is especially visible in relatively new disciplines, such as cognitive science, where explanatory norms are still in the early stages of development. This tension has lead certain philosophers to propose mechanistic explanation as an *alternative* to nomological explanation (Bechtel and Abrahamsen 2005). I will discuss the question of mechanistic or nomological explanation again in § 4.4, as it is directly relevant to the heuristics of anomaly detection.

While mechanistic and nomological models of explanation may be in competition, laws and mechanisms can and do work together. As Bechtel and Abrahamsen note, promoting a mechanistic model of explanation does not preclude incorporating laws into one's models of mechanisms:

We leave laws in place as statements of particularly robust and general phenomena. However, we suggest that explanation is to be found in the mechanisms that account for these laws, not in the laws themselves. (In certain cases, laws may also be incorporated into the descriptions of parts of a mechanism.) (2005, 422)

Laws governing gravitational, electrical or magnetic behavior, for example, may describe the interactions between parts of a mechanism. One could say that a mechanistic model “bottoms out” when the interactions of its parts are determined completely by non-causal laws (for a discussion of “bottoming out”, see Machamer et al. 2000, 4, 13-14; on non-causal laws in mechanisms, see Glennan 2002, S348). I will return to this discussion when I consider how the presence of laws in mechanistic models affects the resolution of anomalies caused by those models in § 6.

#### 4.3. ANOMALY UNDER MECHANISTIC EXPLANATION

An anomaly, under mechanistic explanation, appears when a hypothesized model of a mechanism fails in a prediction. The anomaly is the phenomenon which directly

reveals that the prediction has failed; it is henceforth this phenomenon which stands in need of explanation.

Phenomenal anomalies occur when the anomalous phenomenon in fact does not exist; they are resolved when it is known that the predictive failure which lead one to conclude that there was an anomaly was based on unreliable data or the misinterpretation of data. Factual anomalies occur when the set-up conditions are not accurately specified or external factors affecting the mechanism are not adequately accounted for; otherwise, if they were, the hypothesized model would succeed in the prediction. Theoretical anomalies occur when the model of the mechanism requires revision or rejection before it can explain the anomalous phenomenon.

#### 4.4. HEURISTIC UTILITY OF MECHANISTIC VIEW IN DETERMINING IF THERE IS AN ANOMALY IN EXPLANATION

A disadvantage of both linguistic and perceptual views of anomaly is their ineffectiveness in discovering whether or not an anomaly is explanatory. The mechanistic view is more effective in this respect. Consider two famous counterexamples to D-N explanation: the barometer example, cited in § 2.3, and Bromberger's flagpole example (Salmon 1989, 47). To construct a mechanistic model of a storm wherein a barometer reading had explanatory significance, the barometer would have to constitute a component of the mechanism underlying the storm phenomenon. It is not physically impossible that a barometer plays a causal role in a particular storm. However, that a barometer indicates that a storm is brewing does not entail that that barometer plays a causal role in that storm or is a component in the mechanism producing that storm.

Bromberger's flagpole example makes it clear that it is possible to swap a statement expressing a particular fact in the explanans of a D-N explanation with the explanandum-statement without resulting in a deductively invalid inference. Thus, the height of a flagpole may explain the length of the shadow it casts under the D-N

model, but so might the length of the shadow “explain” the height of the flagpole. But mechanistic explanation would have no difficulty distinguishing the explanation from the non-explanation. It seems practically inconceivable that a flagpole’s cast shadow would be a component in producing the flagpole (with its properties); but it would be relatively straightforward to describe all the components of the mechanism, which would of course include the flagpole, which produce the cast shadow.

These counterintuitive “explanations” do not pose a problem for the perceptual view insofar as successful explanation is defined according to paradigmatic gestalts or sets of expectations and there does not seem to be any scientific community which would have considered these “explanations” successful. Nonetheless, the perceptual view does not give an account concerning why there has not been. Furthermore, scientific communities are known to have accepted non-explanations as explanations: for example, among ancient Greek astronomers, geometrical “explanations” with no known physical significance were common. (In fact, the flagpole “explanation” has all the virtues and vices of a geometrical “explanation” of the motion of a heavenly body.) And, as we discussed in § 3.3, what counts as a successful explanation under one theory often is in need of explanation under the successor theory: for example, Kepler’s laws lost explanatory power under Newtonian mechanics and indeed were themselves in need of explanation (whence Newton’s reformulation of them).

Thus, the mechanistic view also appears less likely than the perceptual view to confuse a non-explanatory anomaly with an anomaly in explanation. An analogy with the Kepler’s laws and Newtonian reformulation of them can be found in the example mechanistic explanation above. One could formulate a law correlating light influx to pupillary constriction; but it is precisely such a “law” which would stand in need of explanation from a mechanistic point of view. Kepler specified no underlying mechanism for the elliptical paths of planetary orbital motion. Newton’s dynamical explanation

can be understood as having provided a model of that mechanism. Thus, under the mechanistic view, a planetary orbit whose period is anomalous with respect to Kepler's Third Law would not constitute an anomaly in explanation, because no model of the mechanism producing the orbit is mentioned in Kepler's Third Law.

A comparison of the three views of anomaly is more complicated with regard to their capacity to encompass various types of scientific explanation. As noted in § 2.3, the linguistic view, which presupposes that successful explanation has the structure of the D-N model, will not, as it stands, be helpful in determining if there are anomalies arising from explanatory failures in functional analysis or in mechanistic explanation. The perceptual view, in contrast, seems capable of including any type of scientific explanation provided that it is exhibited in the exemplars of an actual scientific community.

A significant limitation for the mechanistic view appears when one asks if there are types of nomological explanation which fall outside its scope. Can it ultimately account for anomalous phenomena when those phenomena are law-governed but not mechanistically produced? Most would concede that the mechanistic view cannot. Examples such as Kepler's laws receiving a mechanistic explanation are inadequate to justify a positive answer. Some of the crowning achievements in modern science are explanatory laws so fundamental that a mechanistic explanation for them seems unattainable. A typical example would be Maxwell's equations. (This comparison between "mechanically explicable laws" and fundamental laws is an expansion on Glennan 2002, S348.) Thus, while the mechanistic view does comparatively well in not confusing non-explanatory anomalies for anomalies in explanation, it could still run the risk of categorizing certain anomalies in explanation as non-explanatory anomalies.

That said, none of the mechanists discussed hold that mechanistic explanations should replace nomological explanations generally. Concerning anomalies to funda-

mental laws, the mechanists are prepared to allow that the methods of discovery used in mechanistic explanation may not be able to resolve them. But what is more important is that there is still plenty of room for conflict between the mechanistic and the linguistic views. For example, things become especially interesting when mechanistic explanation and nomological explanation appear to compete in the same field, such as in cognitive science. The mechanist may, for example, deny that certain psychological laws have any explanatory power, whereas the nomologist disagrees. (For an analysis of this tension between theories of explanation in cognitive science, see Bechtel and Abrahamsen [forthcoming].) In such cases, someone holding the mechanistic view of anomaly may in fact believe that certain anomalies are not anomalies in explanation while someone holding a linguistic view of anomaly believes that they are.

The heuristic utility of the mechanistic view is relatively high with regard to the conditions under which an anomaly can appear. Unlike the linguistic view, the mechanistic view does not have a language requirement insofar as models of mechanisms generally do not have an inferential structure and their parts and interactions do not need to be represented sententially. The predictive failure can be represented in *modus tollens*; but the explanatory relation between the predicted phenomenon and the mechanistic model need not be expressible as an instance of logical consistency.

Of course, some form of external representation is needed for mechanistic explanation. Temporal and spatial relations, and the coordination between the two, can usually be depicted most clearly by using visuospatial representations. Thus, the principle component of a mechanistic model is often a diagram. As Bechtel and Abrahamsen note, some might consider the figures in a scientific textbook to be there as a “crutch” for the student. But one could not hold this opinion for long if confronted with scientific papers which present mechanistic explanations: there, language becomes the “crutch,” as the caption or the text serve as commentary to the figures and would be

unintelligible without the figures (2005, 426-427). The question here is not whether all the information which is represented diagrammatically can or cannot be represented linguistically. The question is whether the explanatory process can rely primarily on visuospatial representation rather than on linguistic representation. If so, then the mechanistic view of anomaly can allow that anomalies appear in virtue of diagrams instead of deductive models. See §5 for an example.

Bechtel and Abrahamsen argue for the importance of diagrams in mechanistic explanation for a second reason: so that the representation of a mechanism is isomorphic to the reasoning that we perform about that mechanism. Inferential reasoning is biased towards systems whose interactions take place sequentially and whose parts act separately (Hegarty 1992, cited in Bechtel and Abrahamsen 2005, 430), and indeed treats simultaneous interactions of parts as sequential and separate. This makes the task of reasoning backwards through a system particularly heavy and error-prone. Visuospatial reasoning, however, can cope more readily with systems involving multiple simultaneous processes and feedback loops. Likewise, complex systems of this sort can be represented more easily with diagrams and computer simulations than with language.

The mechanistic view, then, has an heuristic advantage over the linguistic view in that it can make use of both linguistic and visuospatial forms of explanation for the purpose of determining if there is an anomaly. It also has an advantage over the perceptual view in that it provides communicative means for persuading others in the scientific community that there is an anomaly. However, there is a way in which the perceptual view and the mechanistic view can complement one another at this point. The perceptual view places emphasis on anomaly as an internal cognitive representation and says little about the means of external representation by which other members of a scientific community are alerted to the anomaly (even though an anomaly only becomes



“significant” when the community recognizes it). At the same time, the perceptual view invokes a model of the explanatory process which privileges pattern recognition. One of the heuristic advantages of diagrammatic representation is that it facilitates pattern recognition (Larkin and Simon 1987, cited in Bechtel and Abrahamsen 2005, 430). Thus one holding the mechanistic view (or the linguistic view) is interested in explanation as an external representation,<sup>18</sup> from which it follows that anomaly too should depend on an external representation. One holding the perceptual view could still claim that an anomaly is a perceptual event involving pattern recognition; but she would have to allow that the event depends as much on the mechanist’s diagram, that is, on an external representation of the explanatory relation and of the anomaly, as on any internal, gestalt-like representation.

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<sup>18</sup>For a full argument for why explanation should be conceived of in terms of external representations (as opposed to Paul Churchland’s internalist conception) see Bechtel (1996).

## § 5. CASE STUDY: THE ANOMALY OF MACULAR SPARING IN HUMAN AND PRIMATE VISION



Figure 2: Japanese soldier wounded from a 7.6 mm bullet fired from a Moisin Nagent rifle with a muzzle velocity of 620 m/s. The localization of the lesion on the primary visual cortex was facilitated by the high velocity, small calibre ammunition used in the Russian army (Inouye 2000, 92).

In 1909, Tatsuji Inouye published in German a study of the effects of lesions to the primary visual cortex on visual perception. His study was among the first to investigate how the spatial relationship of the retinal photoreceptors was preserved in the primary visual cortex.<sup>19</sup> His data was gathered during his tour as a medical officer in the Russo-Japanese war from 1904-5. His duties included serving as an ophthalmologist and measuring the extent of vision loss to soldiers who had received gunshot wounds to the head (so as to calculate insurance payouts). Inouye's study encompasses thirty

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<sup>19</sup>My presentation of Inouye's contribution is largely indebted to Leff (2004).

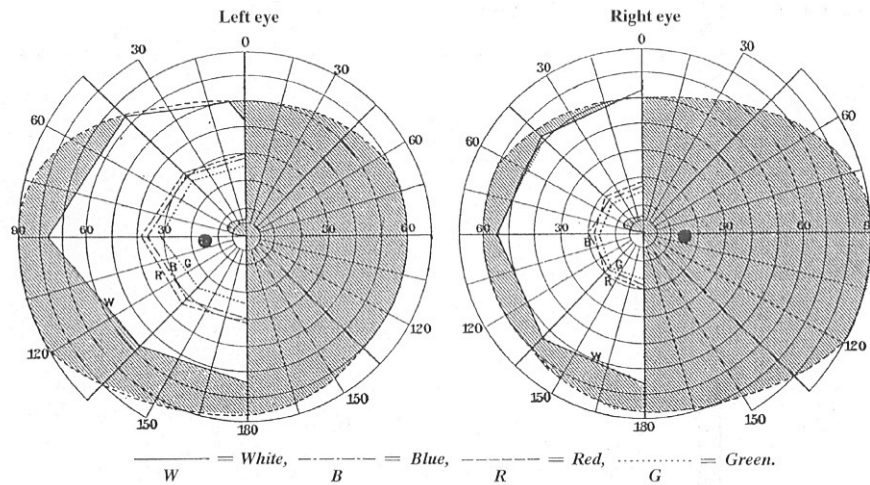


Figure 3: Vision loss (the dark areas) mapped onto the visual fields of each eye (Inouye 2000, 64).

such cases. For each case, he determined the location of the lesion on the cortex and traced the scotomata onto the visual field of each eye (figure 3).

In § 5.1, I describe the mechanism underlying the partial loss of vision suffered by this soldier. But before I do so, I would like to draw attention to a number of features of the phenomenon to be explained which are apparent from figures 2 and 3. I highlight these points in order to provide an example of the combined use of inferential and visuospatial reasoning commonly used in mechanistic explanation. First, the visual field of each appears split down the middle, as the scotoma from the right side of the visual field of each eye traces a midline. Second, the shape of the scotomata for each eye are very similar in shape. This suggests that the same lesion caused the near complete blindness to the right side of both visual fields (a condition commonly called *hemianopsia*); the same would hold for the scotoma in the left visual field, which has spared the central vision on that side. Thus, the right side of the visual field of both eyes would be represented in the same area of the cortex; likewise for the left side. Third, the trajectory traveled by the bullet appears to show that the occipital lobe,

where the primary visual cortex is found, was damaged more extensively on the left side than on the right. This could further suggest the location of the representation of the right visual field in the primary visual cortex. I will now turn to the model of the mechanism responsible for projecting a retinotopic map onto the primary visual cortex.

#### 5.1. SIMPLIFIED MODEL OF THE MECHANISM FOR REPRESENTING THE VISUAL FIELD ON THE PRIMARY VISUAL CORTEX

5.1.1. ELEMENTS OF A RETINOTOPIC MAP. As observed by Kepler, the eye functions like a pinhole camera, in that the eye's lens is instrumental in capturing and inverting an image which then falls onto a surface, the retina. (Here the analogy between a camera and the visual processing system stops.)

The distinction between retinal image and visual field is important for visual neuroscience (Kandel et al. 2000, 524). This is not only because the retinal image is an inverted representation of the visual field. A complete two-dimensional representation of the visual field incorporates two retinal images, left and right, and is obtained when both eyes fixate on a single point and do not change position. This fixation point divides the visual field into left and right hemifields. The retina is also divided into temporal and nasal hemiretinas. A line can be traced from the fixation point through the center of the cornea and lens back to the midpoint of the retina, found at the fovea.

A retinal image of the left hemifield can be found in both left nasal and right temporal hemiretinas; but the right temporal hemiretinal image contains no information about the extreme portion of the left hemifield; likewise for the left temporal hemiretinal image with respect to the right hemifield. The portion of the visual field corresponding to the overlap between the two retinal images is called the *binocular zone*. The extreme portions of the visual field which correspond to information contained

by only one of the two retinal images are called the *monocular zones*, or the *temporal crescents*.

5.1.2. THE OPTIC NERVES. Light from the visual field is transduced by receptor cells in the retina, which synapse with first order bipolar neurons. These in turn synapse with second order ganglion cells, whose axons gather and project out the optic disc (the blindspot), become myelinated and form the *optic nerve*.

5.1.3. THE OPTIC TRACTS. Sensory nerves *decussate* when they cross to the opposite side of the brain. Fish, amphibians, reptiles, birds, and some small mammals have optic nerves which decussate such that the entire optic nerve will project onto the contralateral brain hemisphere (Leff 2004, 269). Some such vertebrates also have binocular vision, particularly predators. They therefore have complete bilateral representation of the visual field within the binocular zone.

The two bilateral optic nerves in humans and most mammals *semidecussate*. The optic nerve semidecussates when only one side of the optic nerve crosses, while the other side projects to the ipsilateral brain hemisphere. Emanating out from the optic disc, the optic nerves of both eyes gather at a point in the hypothalamus called the optic chiasm (named by Galen for its  $\chi$  shape). There the nerve fibers from the nasal hemiretinas cross and are bundled with those from the temporal hemiretinas, which remain uncrossed. From there the right optic tract, which travels back to the thalamus and midbrain, is projected into by the left nasal hemiretinal and right temporal hemiretinal nerve fibers; the left tract is projected into by the right nasal hemiretinal and left temporal hemiretinal nerve fibers. Thus, a complete representation of each hemifield is projected to the contralateral brain hemisphere. It is because each brain hemisphere processes information in both retinas from the same hemifield that full stereoscopic vision is possible (Leff 2004, 269).

From the optic chiasm, each optic tract follows three pathways into the sub-cortical areas: in the midbrain, to the superior colliculus, which controls saccadic eye movement; and to the pretectum, which controls pupillary reflexes; and in the thalamus, to the lateral geniculate nucleus. The spatial relationship of the retinal photoreceptors is preserved in the LGN. From the lateral geniculate nuclei, the retinotopic map is projected back to the primary visual cortex. Ninety percent of the optic tract leads to the LGN. Damage to the pathway passing through the LGN directly will impact visual perception, causing either partial or total vision loss.

5.1.4. THE OPTIC RADIATIONS. After the LGN the retinotopic map is further split into quadrants. Fibers from the LGN relaying the superior visual field terminate in the superior bank of the primary visual cortex, above the calcarine fissure. The optic radiations relaying the inferior visual field (the *Meyer's loop*) terminate in the inferior bank, below the calcarine fissure.

Next, in § 5.2, I will discuss how the above mechanistic model does and does not account for how lesions to different parts of the mechanism which represents the visual field on the cortical retina cause vision loss.

## 5.2. MACULAR SPARING ANOMALY

As noted in § 4.4, because mechanistic explanation frequently relies on visuospatial reasoning, anomalies under the mechanistic view should also appear in the process of such reasoning. Here I have provided an example. I suggest that the reader attempts to discern the anomaly based on the diagram in figure 4. Even if this should prove too difficult, please note how my subsequent description largely assumes that certain spatial and temporal relationships have already been expressed in the diagram.

5.2.1. LESION TO OPTIC NERVE (I). Cutting the right optic nerve will prevent one from seeing out of one's right eye. However, monocular vision of the entire visual field (with the exception of the right temporal crescent) is retained.

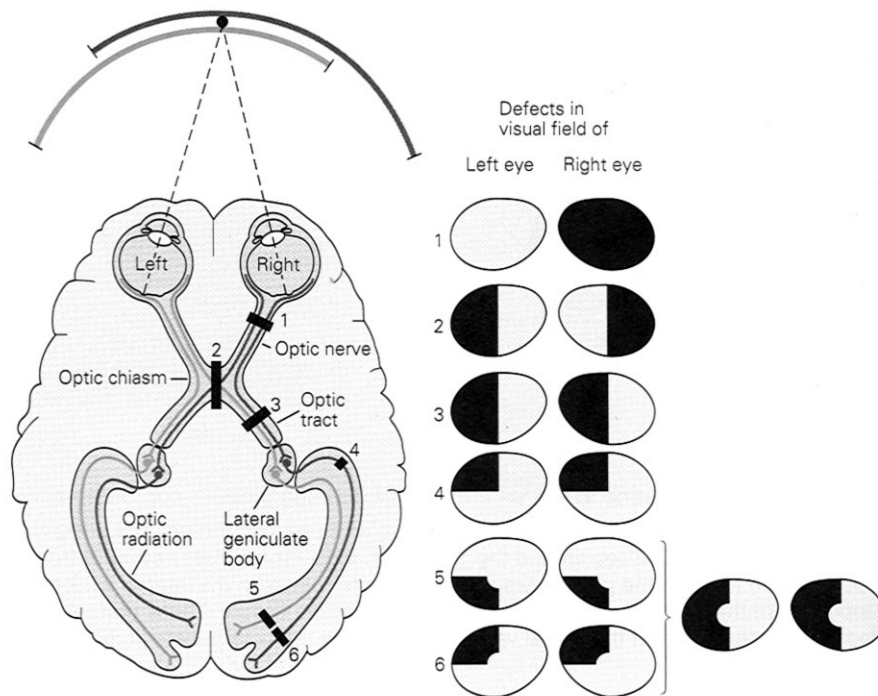


Figure 4: Types of vision loss caused by lesions to parts of the central visual pathway (Kandel et al. 2000, 544).

5.2.2. **LESION TO OPTIC CHIASM (2).** A lesion to the optic chiasm will impact the contralateral retinal projections but will have no effect on the ipsilateral projections. Therefore, the right eye sees only the left hemifield, whereas the left sees only the right hemifield. This condition is known as *nonhomonymous hemianopsia*, since the deficit occurs to a different side of the visual field for each eye.

5.2.3. **LESION TO OPTIC TRACT (3).** A lesion to the right optic tract will impact the ipsilateral and contralateral projections which relay the retinotopic map for the left hemifield. The condition is known as *homonymous hemianopsia*, since the deficit occurs to the same side of the visual field for both eyes.

5.2.4. **LESION TO OPTIC RADIATION (4).** Cutting the Meyer's loop on the right side will cause vision loss in the upper quadrant of the visual field, as the inferior half of

the retinotopic map (which is inverted) is relayed by the inferior optic radiations. The condition is known as *upper contralateral quadrantic anopsia*.

5.2.5. LESIONS TO VISUAL CORTEX (5) AND (6). These lesions are located on the upper and lower banks of the calcarine fissure, where the primary visual cortex is found. Lesions to the primary visual cortex should result in vision loss like that caused by lesions to the optic radiations. Notice though that the foveal area of the visual field has been spared. This phenomenon is known as *macular sparing*.

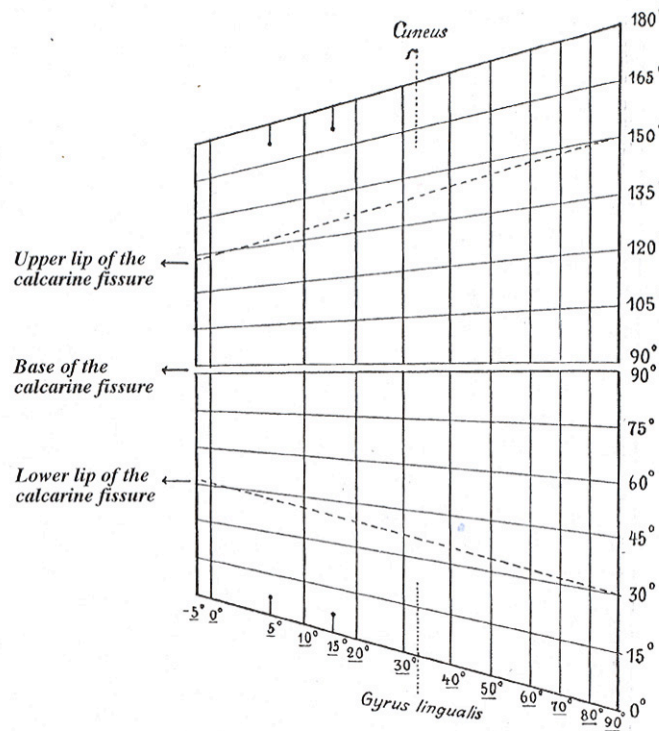


Figure 5: Bilateral representation of foveal region of visual field (Inouye 2000, 92).

5.2.6. EXPLORATION OF THE ANOMALY. Ever since Inouye's study, macular sparing has been an anomaly for the model of the mechanism for representing the retinotopic image on the primary visual cortex. Those who have explored the possibility that macular sparing is a theoretical anomaly have focused on revising the model above to



allow for bilateral representation of the foveal area of the visual field. For example, this, in fact, was the approach that Inouye took. Notice in figure 5 that his map of primary visual cortex in a single hemisphere includes an area of  $-5^{\circ}$  to  $0^{\circ}$ , which corresponds to the foveal area of the ipsilateral visual hemifield. Inouye's attempted resolution has not been accepted, however, because he largely underestimated the quantity of visual cortex devoted to the foveal area: nearly half of the primary visual cortex functions to represent this area. Others have explored the anomaly as a factual: for example, Leventhal et al. have sought to explain the "age-old problem of macular sparing and splitting"(1988, 67) in terms of a retinal mechanism. Nonetheless, many have rejected the notion that bilateral representation exists (in particular, see Leff 2004, 277), and a major textbook now considers it to be a phenomenal anomaly due to unreliable data, namely, incomplete cortical lesions (Kandel et al. 2000, 544).

## § 6. COMPARISON OF VIEWS WITH RESPECT TO THEIR HEURISTIC UTILITY IN DETERMINING WHICH KIND OF ANOMALY IN EXPLANATION THERE IS

### 6.1. ANOMALY LOCALIZATION

In the linguistic view of anomaly, the D-N and theory falsification models represent possible causes of the anomaly and are therefore useful in localizing the anomaly within the explanatory model. If the anomaly is theoretical, then one could look to the laws, explanatory theories, or interpretative theories used in the model. If the anomaly is factual, one could scrutinize the initial conditions, auxiliary hypotheses, or if need be, attempt to determine if the *ceteris paribus* clause is true. The major shortcoming of the linguistic view concerns phenomenal anomalies. Since one can claim to know that there is an anomaly iff the anomaly statement  $O_{\text{anomaly},S}$  is true, it seems as though phenomenal anomalies are no longer on the table under the linguistic view (unless the truth conditions of the anomaly statement allows for the possibility that it is true even while the phenomenon to which it refers can be proven not to exist).

In contrast, according to the perceptual view, one can claim to know that there is an anomaly as soon as one is ready to admit that the phenomenon one perceives does not fit one's set of expectations. Thus, phenomenal anomalies are by definition allowed, even if they are never considered "significant." Hence, in the perceptual view, anomaly localization seems to happen in stages, during which a scientific community overcomes its bias against recognizing theoretical anomalies. The scientist (and if not her, then the scientific community to which she belongs) has an acquired disposition towards believing, so the view goes, that phenomena which appear anomalous do not really exist. In other words, all anomalies at the outset are treated as phenomenal. Meanwhile, it is assumed that the paradigm is true. Only after a certain period of persistence will the possibility that the anomaly is theoretical be entertained. The

perceptual view is of little help, however, in localizing anomalies as either factual and theoretical: both kinds tend to collapse together as sets of expectations consisting indifferently of particular explanatory facts and theories. (See § 6.4 regarding the perceptual view of factual anomaly.)

The mechanistic view offers, in certain respects, a synthesis of the preceding two views. Like the perceptual view (particularly in its cognitive interpretation) the mechanistic view allows one to claim that there is an anomaly as soon as one recognizes as much. Thus, it is not necessary to rule out the possibility of a phenomenal anomaly in order for the claim that there is one to be true. And like the linguistic view, the mechanistic view allows one to make careful distinctions between factual and theoretical anomalies. Because mechanisms do not occur in isolation in nature, many additional factors come into play when we try to isolate them in order to model them. For this reason, among others, when resolving an anomaly in mechanistic explanation, it is crucial to be able to separate potential problems with one's model of the mechanism from erroneous set-up conditions or an inadequate list of relevant factors external to the mechanism. The heuristic utility of the mechanistic view is manifested in the fact that from the point in time when it is determined that there is an anomaly to the moment when a successful explanation is found for the anomalous phenomenon, every kind of anomaly is, in principle, on the table for exploration.

## 6.2. EXPLORATION OF THEORETICAL ANOMALIES

All three views offer some useful heuristics for resolving theoretical anomalies. In the linguistic view, the distinction between interpretative and explanatory theories opens up a couple of paths. Revisions to the interpretative theories which would make the theory group consistent could be suggested and compared. Furthermore, interpretative theories which have been tacitly assumed could be discovered and articulated.

In the perceptual view, another way of exploring theoretical anomalies is open. A theoretical anomaly may be resolvable by revising how a law-schema has been applied to a particular situation. Especially in complex situations, there may be several ways in which a law-schema could be adapted; in the case of a theoretical anomaly, one could try employing or discovering different instantiations of the law-schema (see Kuhn [1962] 1996, 101-102).

The mechanistic view includes a similar, but more elaborate distinction. Oftentimes several models, which are testable individually, share a common *mechanism-schema*. Mechanism schemata are abstract, and when they are instantiated they yield mechanistic explanations (Machamer et al. 2000, 15-18). Furthermore, there are schemata which are more abstract than others. For example, an abstract mechanism schema for protein synthesis could consist of three parts—DNA, RNA, and protein—and three activities—duplication, transcription, and translation; more detailed models would become progressively less abstract (Machamer et al. 2000, 15-16).<sup>20</sup> Resolution of theoretical anomalies will typically begin with revision of the least abstract model instantiated and continue with the more abstract as long as the anomaly persists. Ultimately the most basic form of the schema may be rejected in favor of another.

When a scientist encounters an anomaly caused by a mechanistic model, it is likely that she can search for the resolution of the anomaly in lower-level mechanisms. Mechanisms are often nested in one another hierarchically. The mechanistic model of pupillary reflex presented in § 4.1 does not describe individual neurons and their interactions, nor electrochemical phenomena and their underlying mechanisms. If the model of the

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<sup>20</sup>Abstraction should not be conflated here with generality. As Machamer, Darden, and Craver point out, a protein synthesis schema for RNA retroviruses (e.g., RNA→DNA→RNA→protein) could be just as abstract but less general than the schema for protein synthesis at the core of molecular biology (2000, 16).

pupillary reflex failed in a prediction, the scientist could attempt to resolve the anomaly at a lower level.

For the purpose of this comparison, I have not found a decisive criterion which selects one view as heuristically more useful for exploring theoretical anomalies. A possible reason is that views of anomaly are typically geared towards confronting theoretical anomalies. One might complain that the fact that the perceptual view does not cleanly localize factual and theoretical anomalies could lead to frustration for exploring theoretical anomalies, but this shortcoming should be less a problem in this context than in the context of factual anomalies. One might also point out that one has more options when revising mechanistic models than when revising laws; laws can easily be revised, but they can just as easily lose their explanatory power. Even if the claim is intuitively compelling, it is highly debatable. One could also claim that the mechanistic view provides little help for exploring theoretical anomalies to non-causal laws. This is true, but there are enough laws which are mechanically explicable that it is worth comparing the heuristic utility of the two views. But for the moment, it appears that all three views prove to have heuristic utility for exploring theoretical anomalies.

### 6.3. EXPLORATION OF PHENOMENAL ANOMALIES

In discussing phenomenal anomalies, beginning in §1.3.1, I have made use of a distinction between data and phenomena. Furthermore, I have listed “anomalous data points” as non-explanatory anomalies in §1.2.3. The distinction is one that I have adopted from Bogen and Woodward (1988); it would be helpful to explicate it further before moving on to consider the resources the views have for exploring phenomenal anomalies.

Bogen and Woodward distinguish between data and phenomena in terms of (1) observation and (2) prediction:

Data, which play the role of evidence for the existence of phenomena, for the most part can be straightforwardly observed. However, data typically cannot be predicted or systematically explained by theory. By contrast, well-developed scientific theories do predict and explain facts about phenomena. Phenomena are detected through the use of data, but in most cases are not observable in any interesting sense of that term. (1988, 305-306)

Let's consider an example of a phenomenon predicted by a group of theories, for which we have decent evidence, but which has never been observed: extra-solar planets. Theories of planetary system formation in conjunction with estimates of the size and makeup of the universe predict the presence of large numbers of extrasolar planets. But their size and distance from Earth make them extremely difficult to observe directly. Instead, indirect methods such as astrometry (measuring the visible wobble of the parent star) or radial velocity (measuring the Doppler effect caused by a wobbling parent star) are used in their discovery. Thus, the existence of extrasolar planets is a predictable phenomenon, one which has abundant evidence in its favor (154 likely candidates have been discovered), but which has never been observed with eye or telescope.

Bogen and Woodward also maintain their distinction with respect to phenomena which seem readily observable. For example, as Nagel claims, one might expect to be able to observe the melting point of lead: when one sees the lead sample melt, read the thermometer. However, what one observes is data point somewhere in the vicinity of 327°C. The facts and theories which would explain why lead melts at 327°C do not suffice to explain why the thermometer reads what it does. Bogen and Woodward remark:

It is easy to see that a theory of molecular structure which explains why the melting point of lead is approximately 327 degrees could not possibly explain why the actual data-points occurred. The outcome of any given application of a thermometer to a lead sample depends not only on the melting

point of lead, but also on its purity, on the workings of the thermometer, on the way in which it was applied and read, on interactions between the initial temperature of the thermometer and that of the sample, and a variety of other background conditions. [To measure the temperature at which a single sample of lead melts, the observer must take a reading just as the melting begins. A standard method is to put a small amount of finely powdered metal in a thin piece of capillary tube. A thermometer sensor (for example, a thin piece of wire connected to a thermocouple) is fixed to the outside of the tube. When the sample begins to melt, it vaporizes, changing the color of the tube. The observer records the thermometer reading as soon as he notices a color change. No matter how thin the capillary tube, there is always enough space between the lead and the sensor to guarantee a discrepancy between the temperature of the sample and the reading. No matter how attentive the observer may be, there is no guarantee that he will be able to tell precisely when the sample first begins to melt.] (1988, 309-310)

The data-phenomenon distinction is pertinent to our comparison of views of phenomenal anomaly in several ways. First, it recalls a deficiency in the heuristic prospects of the linguistic view. The linguistic view does not provide a coherent account of how  $O_{anomaly,S}$  should be verified. Yet it is precisely the move from data to phenomenon which should allow us to say that anomaly statements are true. Lakatos, with his distinction between interpretative and explanatory theories, comes close; still, he does not address himself to phenomenal anomalies. On his reading, the claim that chlorine had an atomic weight of 35.5 was not a phenomenal anomaly: the data were neither unreliable nor misinterpreted. Rather, the chlorine samples were impure because the theory of chemical purity had changed.

Second, the data-phenomenon distinction underlines a similar oversight in the perceptual view. The perceptual view is primarily focused on how a theory determines phenomena in such a way that they appear explainable, explained, or anomalous. But in doing so, it has managed to ignore the data-phenomenon distinction: phenomena are observable and data are predictable. One might defend the perceptual view by

pointing out, on the one hand, that a Kuhnian paradigm *qua* disciplinary matrix includes experimental techniques (so paradigms do predict data), and on the other, that we still have a lot to learn about what role perception plays in recognizing explanatory patterns (so phenomena are observable). Nevertheless, the perceptual view provides no clear means for exploring phenomenal anomalies.

Finally, the data-phenomenon distinction brings out a clear advantage of the mechanistic view. What Bogen and Woodward have provided in the passage above is a rough *mechanistic* explanation of a reading of the melting point of lead. In fact, they have differentiated between two different phenomena: one is the melting point of lead, the other is the measurement of the temperature of a lead sample. The second phenomenon is produced by a mechanism with all the parts listed above (thermocouple, sensor, capillary tube, lead, so on), which carry out certain activities or undergo certain changes in properties. Notice that if there is an underlying mechanism which produces the first phenomenon, the melting point of lead, then that mechanism is also a part of the mechanism which produces the second phenomenon.

To explore a phenomenal anomaly, one can treat data as mechanistically-produced phenomena, provide a mechanistic explanation of them, and thereby discover how the bad data were produced.<sup>21</sup> Here we are describing a routine aspect of scientific practice, troubleshooting the experimental set-up. A scientist, when confronted with an anomaly, will typically re-examine her instrumentation. To understand the point scatter, one should understand the mechanism which produces the points. Thus, the mechanistic view of anomaly, unlike the other views, allows one to use its explanatory resources as a method of discovery, namely, for exploring and resolving phenomenal anomalies.

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<sup>21</sup>It remains to be seen how a view of anomaly might prove useful when resolving phenomenal anomalies caused by misinterpretation of data.



#### 6.4. EXPLORATION OF FACTUAL ANOMALIES

As mentioned in § 6.1, the perceptual view tends to conflate factual and theoretical anomalies. This in fact is due to a deliberate interpretive choice on the part of Kuhn. He often refers to certain examples of “unexpected discoveries” which, he claims, blur the line between resolved factual and resolved theoretical anomalies: Priestley’s discovery of oxygen, Herschel’s discovery of Uranus, and Roentgen’s discovery of X rays (Kuhn 1977, 165-177). Since it is not clear to me that the discoveries of oxygen and of X rays were not simply resolutions of theoretical anomalies, I will focus on Herschel’s discovery. In 1781, Herschel was convinced that he had found a new comet and persisted in his belief until Lexell insisted that the data provided evidence not for a cometary orbit, but rather for a planetary orbit. It appears that Herschel and others were operating in a six-planet paradigm, one which had been in effect since at least the earliest astronomical records to which we have access. However, the claim that the solar system has seven planets instead of six refers to a particular fact, albeit one that is highly relevant to the explanations of a number of astronomical phenomena. But it does nothing to alter the prevailing paradigm, which was Newtonian mechanics. What makes Kuhn argue that the discovery of Uranus was theoretically significant was that it was *unexpected* or unpredictable given the current beliefs of the scientific community.<sup>22</sup> Hoyningen-Huene elaborates:

To be sure, as Kuhn notes in *SSR*, the distinction between the unexpected discovery of new phenomena or entities and revolutions in theory is artificial, for two reasons. First, superficial appearances to the contrary, *both* cases involve changes on the theory level. Second, the discovery of new facts may herald the replacement of a previously accepted theory, and conversely, the replacement of a previously accepted theory may lead to or be accompanied by unexpected discoveries. (1993, 228)

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<sup>22</sup>One explanation for Kuhn’s interpretation of these discoveries could be that *his* model of anomaly resolution was the Copernican revolution, an event encompassing the concomitant resolutions of both factual and explanatory anomalies (1957).

But neither Kuhn nor Hoyningen-Huene explain why unexpectedness should be a sufficient condition for the resolution of a factual anomaly to have theoretical import. This is important because the conflation of theoretical and factual anomalies limits the heuristic utility of the perceptual view when exploring factual anomalies. The obstacles scientists face when resolving theoretical anomalies would automatically turn up in the context of factual anomalies. But these obstacles cannot be reduced to nothing more than a paradigmatic set of expectations. Indeed, historically, many scientists have shown themselves entirely willing to revise or replace their theories; yet their willingness does little to attenuate the exceptional difficulty of resolving many theoretical anomalies.

The heuristic utility of the linguistic view for exploring factual anomalies is significant. To explore a factual anomaly, one holding this view begins by assuming that the theory statements are true. One then proceeds to examine one's auxiliary hypotheses, to see both if they are complete (a step which is equivalent to checking if the *ceteris paribus* clause has been corroborated) as well as if they are true. The well-known problem of verifying individual statements still hampers this process, but it is still far more practical to prove that an auxiliary hypothesis is false or that the *ceteris paribus* clause has not been satisfied than that all the auxiliary hypotheses are true. Thus, it does happen that one can demonstrate that an initial condition is erroneous, that some relevant factor has not been taken account, or that some other assumption which remained tacit needs to be explicitly interrogated.

The mechanistic view incorporates the same explanatory factual elements in conjunction with models of mechanisms as the linguistic view does with law statements. Mechanists refer as well to auxiliary hypotheses and the *ceteris paribus* clause. However, the explanatory resources available in the mechanistic view provide a definite advantage. It relates to Hoyningen-Huene's remark above. A discovery resulting from

an attempt to resolve a factual anomaly in the context of mechanistic explanation will frequently have theoretical consequences. Finding that an external factor has not been accounted for usually involves actually discovering that external factor, and that factor is typically a mechanism itself or a part of some mechanism. Here are two examples.

Research on blindsight can be understood as the exploration of a factual anomaly which has had theoretical consequences. The central visual pathway in humans and primates is arranged such there is a complete representation of the visual field in the primary visual cortex, which is found at the occipital lobe. The primary visual cortex projects to various anterior areas of the brain where specialized processing of that representation takes place. This is the mechanism which produces our vision. A small lesion to the primary visual cortex will result in blindness in a region of the visual field; a large enough lesion will result in complete blindness. Researchers have found cases, however, in which patients, who have received lesions in the primary visual cortex, are still able to respond to information in an area of their visual field where they are blind. One way of exploring this anomaly is to assume that the model of the mechanism whereby a retinal image is projected to the primary visual cortex does not need revision; instead, an external factor needs to be accounted for. In this case, we know that 10% of the optic tract does not project through the lateral geniculate nuclei back to the primary visual cortex; rather, it projects to the midbrain regions, the pretectum, which controls pupillary reflex, and the superior colliculus, which controls saccadic eye movement. Blindsight could be due to the retinal projections to the superior colliculus. Thus, these projections could constitute a relevant external factor, and moreover, a function for them may have been discovered as a result of testing the model of mechanism responsible for vision.

The second example is more of a thought experiment for comparing mechanistic and nomological models of explanation. Let's return to the discovery of Neptune.

Ever since 1846, it has been applauded first in newspapers and then in textbooks as a triumph for Newtonian physics insofar as the resolved anomaly and the successful prediction seemed to *confirm* the theory. Philosophers of science have made the point repeatedly that such beliefs are incoherent and theory confirmation is a unsalvageable enterprise. Thus, for the linguistic view, the resolution of this factual anomaly in fact has no theoretical significance. The number of planets in a solar system is a matter of indifference to the truth of gravitational laws.

But now suppose that Uranus' orbit was regarded not merely as a law-governed phenomenon, but as mechanistically produced phenomenon. Thus the various bodies in the solar system are parts which interact in various ways. Discovery of a new part would have theoretical significance insofar as a model of a mechanism including that part would require revision. And such a mechanism need not be conceived of as a sort of Cartesian clockwork. Instead we could model the mechanism which in fact produced Uranus and Neptune and their orbital motion. Let the set-up conditions for the model include a solar nebula, a rotating cloud of stardust and gas. The gravitational pull which the cloud particles exert on each other cause the entire cloud to contract towards the center and form a dense region known as a protosun. Cloud matter continues to be pulled in towards the protosun, which converts the gravitational energy into thermal energy according to Kelvin-Helmholtz contraction. As the cloud collapses around the protosun, its rotational velocity increases; the cloud also flattens out forming a disk. Eventually, the temperatures of the protosun become high enough that nuclear reactions take place and the protosun becomes a star. The K-H contraction ceases. Planets then form from the protoplanetary disk orbiting the Sun. Owing to the temperature differential across the inner and outer parts of the solar system (from 2000K to 50K), elements with high condensation temperatures remain near the sun whereas lighter elements liquify or solidify only in the outer solar system. Collisions be-

tween particles form planetesimals which in turn accrete to form protoplanets. Thus, the terrestrial planets and the Jovian planets differ according to chemical composition and size. But oddly, unlike Jupiter and Saturn, Uranus and Neptune have different chemical abundances of hydrogen and helium than the Sun, and they have a higher proportions of heavier elements. This is probably because these two planets formed much closer to the Sun than where they are located today, but the gravitational effects of Jupiter and Saturn pushed them out.

This could be a mechanistic explanation for Neptune's and Uranus' orbits. Unlike a gravitational law, it actually describes the sequence of causal interactions underlying the two gas giants now following their present orbits. While it is true that many of the interactions between the parts are law-governed, the phenomena of planetary formation, migration, even orbiting, are explained in terms of parts and their interactions.

The aim of this redescription of a law-governed phenomenon as a mechanistically produced phenomenon has been to compare the heuristic utility of the two views of anomaly. On the one hand, the discovery of Neptune, as we pointed out earlier, has no theoretical significance with respect to the nomological explanation of Uranus' orbit. On the other hand, the resolution of that factual anomaly does have theoretical consequences in relation to the mechanistic explanation of Uranus' orbit. Namely, an unknown relevant factor perturbing Uranus' orbit was also an unknown part of the solar system *qua* mechanism. As a result, the model of that mechanism undergoes a revision in response to the resolution of a factual anomaly.

## § 7. CONCLUSION

In presenting these three views of anomaly and comparing them with respect to their heuristic utility, my stated goal was to show that the mechanistic view, as it stands, has the most resources both for determining that there is an anomaly and which kind of anomaly it is. It should be clear though that the mechanistic view adopts many of the features of the linguistic and perceptual views. (It would have been awkward to present these views in the reverse order.) It would be interesting to see how the linguistic and perceptual views could be strengthened in light of the challenges presented by the mechanistic view. A linguistic view could be reconstructed with less deference to history and with a more sophisticated approach to the logic of explanation. Advances in that view will continue to be helpful even for those who do not believe that anomalies are always expressible as logical inconsistencies, since many are. Moreover, the linguistic view provides the most insight into anomalies appearing in the context of nomological explanations of mechanically inexplicable phenomena.

The perceptual view could clearly be improved by using more recent cognitive science. However, it seems to me that the perceptual view needs to be split into two research programs. One was suggested by Dr. Ramachandran, where he discusses a possible neurological basis for anomalies within paradigms:

The left hemisphere's job is to create a belief system or model and to fold new experiences into that belief system. ...When the anomalous information reaches a certain threshold, the right hemisphere decides that it is time to force a complete revision of the entire model and start from scratch. The right hemisphere thus forces a "Kuhnian paradigm shift" in response to anomalies, whereas the left hemisphere always tries to cling tenaciously to the way things were. (Ramachandran and Blakeslee 1998, 136)

Wherever Dr. Ramachandran's investigation into the neurological mechanisms underlying denial goes, he does isolate a prevalent symptom of working within an explana-

tory paradigm, namely, denial towards anomalies. This, it seems to me, deserves to be investigated all by itself, rather than being tied to a particular theory of scientific explanation. Someone can be in denial about the presence of an anomaly no matter which model of explanation he or she employs. One can confabulate a successful D-N explanation as well as a mechanistic explanation to avoid an anomaly. But scientists are not in denial every time they pursue a method of anomaly resolution which does not involve questioning their explanatory theories. The decision to explore an anomaly as though it were of a certain kind can be made based on many different factors which have nothing to do with feeling that an anomaly is threatening one's world view. Thus, the second research program suggested by the perceptual view would be to investigate further how cognitive agents, independently of any denial mechanisms, come to recognize that an anomaly is present. In this thesis, I have brought in cognitive notions such as pattern recognition, inferential reasoning, visuospatial and linguistic representation, and so on, but clearly this discussion of anomaly recognition should be taken much further.

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## VITA

Robert Rose was born in Howard County, Maryland, in 1973, and grew up in southeastern Connecticut. During his teenage years, he trained dressage horses and taught riding lessons on a family-run farm. Later, having received a scholarship to study computer art, he moved to Beaumont, Texas, where he was graduated with a Bachelor of Arts from Lamar University in 1997. For several years, he pursued his studies in philosophy within language and literature programs. In 2001, he was awarded a Master of Arts in French at Louisiana State University. Earlier that year, he moved to Paris, France, where he studied French, German, and Ancient Greek philosophy. Returning to the United States in 2003, he taught French at Louisiana State University before pursuing his Master of Arts in philosophy.