The role of idealizations in the realism/anti-realism debate

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ABSTRACT

The thesis focusses on what impact the use of idealizations has on the realism/anti-realism debate concerning the fundamental laws of physics. My aim is modest. It is not to present an argument for either the realist or the anti-realist position but rather to show where the debate stands once we have considered recent arguments by Laymon and Cartwright which have made use of the notion of idealization assumptions. My intent is to point out the difficulties of Laymon's argument for realism in the hope of showing what must be accomplished in providing a more convincing argument for realism. I will also suggest that although Laymon's proposal is problematic, it still poses a serious problem for van Fraassen's form of anti-realism, constructive empiricism.

ABSTRACT

L'objectif de cette thèse est de s'interroger sur les conséquences qu'entraîne l'usage d'idéalisations en ce qui touch les lois physiques fondamentales dans le cadre du débat réalisme/anti-réalisme. Mon but est modeste, puisque je n'entends pas défendre l'une ou l'autre des positions en présence, mais bien plutôt démontrer où en est rendu le débat étant donné les positions avancées recémment par Laymon et Cartwright eu égard aux présupposés liés à l'usage des idéalisations. Il s'agira donc d'identifier les difficultés inhérentes à l'argumentation de Laymon, favorisant la position réaliste, de façon à pouvoir éventuellement indiquer ce qui favorisserait une meilleure défense de cette position. Par ailleurs en dépit du fait que la démonstration de Laymon soit problématique à certains égards, je compte souligner en quoi cette thèse pose de sérieux problèmes à celle, anti-réaliste, prônee par van Fraassen, soit un empirisme constructiviste.

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Introduction

Typically, the motivation for appealing to idealizations is to try to predict or explain a phenomenon through an understanding of it in a simpler form McMullin defines idealizations as that which signifies, 'a deliberate simplifying of something complicated (a situation a concept, etc) with a view to achieving at least a partial understanding of that thing ' The focus of discussion in this thesis will be the use of idealizations in explanations found within physics My primary aim is to investigate what role the use of idealizations has in the realism/anti realism debate concerning the fundamental laws of physics This aim will be accomplished by considering what problems the use of idealizations raises for standard models of confirmation and explanation such as the hypothetico-deductive model of confirmation and the D-N model of explanation From there, I will consider an alternative model of confirmation and explanation in the hope of defending realism

The thesis will begin by exploring the different kinds of idealizations we find in physical explanations I will then discuss why we must use idealizations in physical explanations From there, I will look at what consequences the unavoidability of using idealizations has on the realism/anti-realism debate The methodology which I will adopt in chapter 2 will be the following I will begin by considering some standard anti-realist arguments for the claim that either the fundamental laws are false or do not need to be true One of these arguments, I will show, rests crucially on the fact that idealizations are used in explanations of phenomena This argument poses a serious problem for the realist for two reasons The first is that this argument suggests that if we look at scientific practice, at least a particular part of scientific practice, we should be persuaded to be anti-realists The second is that since idealizations must be used in derivations, it means the realist must provide a response to the anti-realist which takes into account the use of idealizations In chapter 3, I will critically examine a possible realist response suggested by Ronald Laymon, in which he offers an alternative method of establishing the truth of fundamental laws which does precisely this It should be made clear that my aim in providing these criticisms, is not to argue for anti-realism but rather to make clear what tasks must be accomplished if one wants to be a realist.

Chapter 1

Some Terminology and Basic Setting Up

As I stated earlier, the focus of my discussion will be the use of idealizations in scientific explanations and scientific predictions My intent in this chapter is to first discuss what is meant by idealizations Part of this discussion will involve exploring the different kinds of idealizations we find in physical explanations I will, then, investigate why we use idealizations and discuss what their relation is to explanations. Before discussing these issues, it would be useful to go through some typical examples of scientific explanations and scientific predictions which include the use of idealization assumptions The first example is a case of scientific prediction used by applied engineers, and the second example is a case of a scientific explanation given by theoretical physics

In the calculation of the air speed[1], Ve, of a plane, applied engineers use the following equation for subsonic passenger and freight planes,

(i) $VE = [2((P_t - P_o)/\rho_s)]^{1/2}$

where P_t is the total air pressure at a particular altitude, P_o is the ambient pressure of the plane against the atmosphere, and ρ_s is the sea level density relating to the amount of air contained in a given volume. Equation (i), however, does not give exact values for the equivalent air speed. The reason is that the equation is obtained from another equation and the use of an idealization assumption The more precise equation for the air speed contains a Taylor series,

(ii) $VE = [2((P_t - P_o)/\rho_s) * (1/(1 + M^2/4 + M^4/40 + \ldots))]^{1/2}$

[1]Nancy Cartwright, **How the Laws of Physics Lie**. p.105

where M is a ratio of the plane's speed to the speed of sound under given conditions Thus, the difference between equation (i) and (ii) is that in equation (i), we ignore all of the factors containing M in the Taylor series The justification is that these terms are negligible when $M < 5$ As one can see, $(1/(1 + M^2/4 + M^4/40 + \quad))$ is very close to 1 when $M < 5$ Thus, equation 1 is merely an approximation of the actual velocity However, it is a good approximation for values when $M < 5$ since $(1/(1 + M^2/4 + M^4/40 + \quad))$ is close to 1 There is an important point worth noting about scientific practice in these cases. Even though it is required to use an idealization assumption to derive equation (i), thereby making it inaccurate, the equation is still often used to calculate the air speed The intuitive reason as to why we still consider this equation to be reliable is because we know that the factors which we are ignoring are negligible under particular circumstances. Note that in the above case, not only do we know that the factor which we are ignoring is negligible, but we also know why it is negligible because we possess the more precise equation which takes into account the idealized factor in equation (ii) Thus it seems that the real justification for the continued use of equation (i) lies not in the fact that the factor which we are ignoring is negligible, but rather in knowing how the ignored factor actually contributes to the calculation of the air speed

The second example concerns explaining the macroscopic properties of gases pressure, volume and temperature A commonly used equation for explaining these values is the ideal gas law, $(PV = nRT)$ Like the example above, this law only gives approximate results That is, the results which we derive from this equation do not correspond exactly to the experimentally derived results we obtain for a particular gas Since the innovation of statistical mechanics, we know that if we want to accurately derive the ideal gas law, several idealizing assumptions must be used For example, two of the idealizing assumptions are that the gas molecules are infinitesimal in size, and that there are no intermolecular forces There is a more accurate law which takes into account the factors which are ignored in the ideal gas law, Van der Waals equation $[(P + a/v^2)(v - b)] = RT$ where $b = 2/3N\pi r^3$ The two quantities, a, and , b, are constants which depend on the particular gas being discussed The appearance of the factor, a/v^2, however, is the result of taking into account the intermolecular forces of the molecules And similarly, the appearance of b in the equation is the result of taking into account the molecular sizes of the gas

There are two ways in which we can know that an explanation or prediction is only an approximation The first is through comparing the derived results with the values which we are trying to explain or predict, and the second is by looking at the derivation itself and noting that idealization assumptions are used. An important point to note, is that it is rare that theoretically derived results are actually equivalent to the results which we are trying to explain or predict More importantly, it does not seem that for an explanation to be good, it is required to be this accurate There is a simple practical reason as to why this is not a necessary condition which is, the equations that are required to accomplish this task are, in fact, impossible to derive What we find more often are explanations or predictions which are very close to the measured results The fact that the theoretically derived results are not equivalent to the measured results is often an expected result This is because physicists are aware that in the derivation of the equations which are used to explain a phenomenon, idealization assumptions are used Since most if not all derivations use idealization assumptions, theoretically derived results which are exact usually raise more suspicion than those that are not equivalent Take for example the first year physics student in a laboratory course. The purpose of some of these labs is to experimentally confirm some of the basic laws of mechanics and electromagnetism Lab instructors are very suspicious when students produce results which are perfectly accurate to the theoretically derived results The reason is the laws which are used for obtaining the theoretical results ignore several factors peculiar to the experiment being performed by the student in the lab These are factors such as the roughness of the plane, or the electromagnetic field produced by the earth Since these factors should affect the experiment, it would be unexpected that the experimental results should perfectly conform to the theoretical results

Idealizations and Their Different Forms

McMullin defines idealization as, "a deliberate simplifying of something complicated (a situation, a concept, etc.) with a view to achieving at least a partial understanding of that thing."[2] Moreover, he distinguishes four types of idealizations employed in the sciences. There are mathematical, construct, and two types of causal idealizations. Although these different forms of idealizations are conceptually distinct, they can overlap in many cases. Mathematical idealizations can be thought of as those idealizations which occur when a mathematical formalism is adopted to describe a physical situation. Two examples of this are the adoption of euclidean geometry in the seventeenth century and the adoption of statistical mathematics in statistical mechanics. The consequence of adopting a mathematical language, as McMullin notes, is that certain properties, namely qualitative properties, are either left out or neglected in the descriptions of objects or events within the theory. He writes,

> Geometry is an abstraction, an idealization. It leaves aside the qualitative detail that constitutes the physical singular *as* physical. A physics that borrows its principles from mathematics is thus inevitably incomplete *as physics*, because it has left aside the qualitative richness of Nature.[3]

Prior to modern physics, these idealizacions were quite substantial, in the sense, that when objects were represented mathematically, their substantitive quality was idealized away. An example of this is the representation of objects as geometrical points or figures in Newtonian physics.

Cartwright cites the adoption of statistical mechanics as another case of mathematical idealizations. In the nineteenth century, Kelvin and Maxwell adopted a continuum theory of space. By adopting such a theory, it meant that, "motion in the continuum would present essential

[2]Ernan McMullin, *Galilean Idealization* p.248

[3]Ibid p 249

instabilities and essential singular points, both incalculable and therefore beyond the scope of mechanical determination and of human knowledge" The solution to these incalculable problems was to introduce a statistical mechanics which involved representing certain aspects of the world as probabilities Thus, the introduction of probability theory resulted in a mathematical idealization Cases such as the representation of certain aspects of the world as probabilities are idealizations since quite obviously aspects of space are not really the probabilistic equations which we derive when we use probability theory Moreover, the idealizations are termed mathematical since they are a result of adopting a certain mathematics

It was clear that when mathematics was adopted as a language for physics, it was considered as a form of global idealization. In other words, although scientists realized that using mathematics would help our understanding, it was understood that there was a certain qualitative aspect of nature which could not be captured by the mathematics Recently, however, it has become ambiguous to what extent the use of mathematics in physics results in idealizations In other words, since the development of general relativity, it is unclear whether there is any aspect of reality which we cannot capture through the use of geometry What Einstein showed was that gravity and gravitational force could be accurately represented as simple contortions of space Thus, it would seem that mathematics is no longer merely a convenient language within which we formulate our physical theory since it now seems that we can describe an aspect of reality, gravitational force and gravity, in a mathematical language which does not involve any idealization in the sense which was described above There is no substantive quality which is idealized away The point is that although in previous theories, the adoption of mathematics as a language for the theory has resulted in idealizations, the innovation of general relativity has shown this to be not generally true

The primary difference between mathematical idealizations and the following construct and causal idealizations can be thought of in terms of a global/particular distinction That is, by adopting a certain type of mathematics as a language, there is a certain substantive property which cannot be captured when describing any event or object. Construct and causal idealizations which I will now discuss are more specific These idealizations are only those which are performed on *particular* events or concepts. These idealizations amount to either neglecting or abstracting away certain causal features or causal properties of a phenomenon or event

Causal idealizations involve the use of idealizing assumptions in either thought experiments or actual experiments. McMullin defines causal idealizations as those idealizations used in actual experiments, and subjunctive idealizations as those experiments performed in thought experiments. Although he terms the latter, 'subjunctive idealizations', they still fall under the category of being causal idealizations since they *idealize away causal factors in an experiment* The need for performing thought experiments often arises out of an impossibility to perform an actual experiment The way in which both of these idealizations occur is through what physicists call 'controlled experiments' What occurs in these experiments is the construction of an environment such that a certain property or set of properties is isolated For example, in a simple experiment to test the gravitational pull of the earth, an experimenter will try to create an environment with as little friction, or air resistance as possible. Thus, what will occur is a causal idealization since an actual experiment is constructed such that a certain property is ignored However, if we want to construct an experiment where there is no air resistance, we will have to invoke something which is called a thought experiment This is what Buridan did when he invoked thought experiments for spheres spinning in a frictionless media

I noted earlier that construct and causal idealizations involve neglecting or abstracting away causal lines or causal properties McMullin states that the primary difference between construct and causal idealizations is that the former involves a 'simplification on the conceptual representation of the object' while the latter involves a 'simplification on the object itself' The distinction can be made clearer if we consider the following example. Suppose that we are trying to explain the velocity of a ball rolling down a plane Examples of construct idealizations are neglecting the perturbations on the surface of the ball and neglecting the friction due to the plane These are construct idealizations since the idealizations act on the representations of the objects A causal idealization, on the other hand, involves an idealization on the actual objects or the actual event through the construction of an experiment Thus, a causal idealization would be something like constructing an experiment which involved objects which are more or less spherical and planes which are nearly frictionless. One might be led to think these really are not idealizations since what we have constructed through the experiment actually exists But if we consider McMullin's definition of an idealization, we see that these still are idealizations. Recall that McMullin defines an idealization as, "a deliberate simplifying of something complicated (a situation, a concept, etc) with a view to achieving at least a partial understanding of that thing "[4] One of the purposes of performing or constructing experiments is to try to create an environment which is similar but simpler to the environment in which a particular event, E, occurs The hope in this process is that the understanding which we gain through investigating how events proceed in the simpler environment will help us in our understanding of E.

Construct idealizations can be classed into two types, formal and material McMullin describes formal construct idealizations as follows 'features that are known (or suspected) to be relevant to the kind of explanation being offered may be simplified or omitted in order to obtain a result'. For example, in the derivation of Kepler's law, Newton assumed that the sun was at rest. This amounted to assuming that it was infinitely massive with respect to the other objects which are considered in the explanation Material construct idealizations, on the other hand, are when 'features that are deemed irrelevant to the inquiry at hand are left unspecified' For example, in the kinetic theory of gases, the internal structure of the molecules was left unspecified. The distinction between these two types of idealizations rests on the amount of knowledge we possess. McMullin writes,

[4]Ibid p 248

> Electron spin was not part of the original Bohr model Its omission was not a formal idealization, strictly speaking, because there was not reason to suppose the electron could possess spin "Has the electron spin?" was a question that simply had not been asked, and that could not be answered within the original model [5]

It is clear from this passage that the distinction McMullin wants to draw between material and formal construct idealizations rests on whether we actually know if the electron possesses spin For it seems from the passage, that had we had a theory which included electron spin, it would have been considered as a form of formal construct idealization Yet since we did not possess such a theory, we did not know exactly what was involved in our idealization. Thus, it was a form of material construct idealization

The distinction can be important for the following reason Since the distinction between the types of construct idealization rests on whether what is idealized is either known or unknown within the theory, it would seem that in the case where the idealized factor can be accounted for in the theory (formal construct idealizations), we are able to account for the factors which were idealized away. This can appear in two ways The first is that we possess a more accurate equation which takes into account the ignored factor An example of this case would be the possession of Van der Waals equation which shows how the factors, ignored in the ideal gas law, affect the pressure, volume, and temperature of gases in a container The second is that we possess an explanation although not in the form of an equation which tells us how the ignored factor contributes For example, suppose that we want to explain the results of an experiment I have been working on in my lab. However, since there was a fire in the lab yesterday, I got very poor results. Although I do not possess the equation which shows how the fire contributes to my experiment, I do possess the knowledge that it severely damaged the experiment These two cases are to be contrasted to the case where there is an idealization of an aspect for which we cannot account in the theory. In this case, clearly we do not possess a more

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[5] Ernan McMullin, *Galilean Idealization*, **Studies in the History and Philosophy of Science**, p 263

accurate equation which takes into account the ignored factor nor can we provide some explanation as to how the ignored factor contributes. The distinction between these two types of construct idealizations is relevant to when explanations which involve idealizations are considered to be good explanations or good predictions. For in the case, where we do use an equation which ignores some factor yet we do not know how this factor contributes, we cannot be justified in claiming that our explanation is good, since the ignored factor could be relevant to the explanation which we are trying to give. What I will discuss in the next section is how the notions of ad hoc/non-ad hoc correction factors are related to good explanations.

Although the primary difference between mathematical idealizations and construct or causal idealizations is one of generality, one might think that it is always possible to equate a mathematical idealization to a set of construct and causal idealizations. This is not so. There are certain mathematical idealizations which cannot be formulated in terms of either causal or construct idealizations.[6] If I understand McMullin correctly, the central element of mathematical idealizations is the adoption of a mathematical language for a science. Construct and causal idealizations, on the other hand, involve neglecting or abstracting away causal features or causal lines of phenomena or events. A result of mathematical idealizations, then, is that objects will get represented mathematically. If these representations were different from the real objects only in that they do not possess certain causal properties, the mathematical idealization would be equivalent to some set of causal or construct idealization, but this is not always the case. Thus, what I propose is that there are cases of mathematical idealizations which do not involve idealizing away any causal properties or factors.

Consider, for example, the distribution function used in statistical mechanics for deriving the entropy of a system. Sears and Salinger write,

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[6]This distinction was originally suggested to me through a discussion with Paul Pietroski.

> If a system consists of only a relatively small number of particles, as in Fig. 11-4 [diagram of the possible macrostates of an assembly of 6 particles obeying Bose-Einstein statistics], the average values of the occupation numbers of the energy levels can be calculated without much difficulty, when the total number of particles and the total energy are fixed. When the number is very large, as in the statistical model of a macroscopic system, direct calculations are impossible. We now show how to derive a general expression for the average occupation numbers when the total number of particles is very large. Such an expression is called a distribution function.[7]

The authors go on to show how the distribution function can be used to calculate the entropy of the total system. Note, however, that the distribution function represents the 'average occupation numbers'. Since it is only an average, it does not represent a property that any particular system is likely to possess. It is worth pointing out that a given system might conform exactly to the model since the relevant curve might be normal. The average in this case would correspond to the mode (the most likely state). The salient point here, however, is that the distribution functions are meant only to be an approximation because a direct calculation of the occupation numbers for very large systems is impossible. The idealization in this instance is not causal or construct, since we are not abstracting away any causal properties. However, it is still an idealization, in that the statistical method which we have adopted is supposed to represent only the approximate value for the occupation numbers, and not the actual values of the occupation numbers for the particular system.

Correction Factors and When Idealization Assumptions are Justified

The question which will be addressed in this section is under what conditions is an explanation good when idealization assumptions are involved? Physicists consider explanations or predictions which use idealization assumptions as good when either of the following two conditions are satisfied. The first is when we possess the theoretical knowledge to use some non-ad hoc correction factor to remove the

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[7]Francis Sears and Gerhard Salinger, **Thermodynamics, Kinetic Theory, and Statistical Thermodynamics**, p.327

idealization assumption, and the second is when the theoretically derived result falls within some derived error One might think that a possible condition is when the idealized factor is negligible There is some truth to this claim, in the sense that we can only really know that a factor is negligible when we possess the relevant theoretical knowledge However, if we do not possess the theoretical knowledge concerning the causal factor which we are ignoring, then there is no way of knowing that the factor is actually negligible It could be argued that we do know that the idealized factor is negligible by performing an experiment where the idealization assumption is employed, and thus we can tell that the idealized factor is negligible by comparing the value of this experiment to one in which the idealization is not employed It should be noted that if this form of justification is employed, it must be done so with caution. For if we do discover that a certain factor is negligible through the use of experiments and not through theoretical knowledge, then we can only conclude that the idealized factor is negligible under those conditions in which the experiment was performed The mistake would be to conclude that the factor is negligible under similar conditions to the ones in the experiment The reason why we cannot make this inference is because in these cases, we do not know how to account for the causal factor under any circumstances A simple but often used example will show why [8] Suppose that we set up two environments, one in which everyone smokes and exercises, and the other where no one smokes and everyone exercises. However, what is unknown to us are that smoking causes heart disease, and exercising prevents heart disease. Since exercising is a preventative of heart disease, it is possible that no one would get heart disease To return to the point which I made earlier, the causal factor of smoking in this circumstance has a negligible effect. However, if we alter the circumstances slightly such that no one exercises, then we would discover that smoking is no longer negligible Unless we possess the adequate theoretical knowledge (i e, that smoking did not cause lung cancer in the first environment because there was a preventative), there is no reason to

[8]Nancy Cartwright, **How the Laws of Physics Lie**, p.23

believe ignoring a particular causal factor will have the same effect under similar but different circumstances. The only reliable justification lies in our possession of the appropriate theoretical knowledge, and not with the fact that the idealized factor is small, since we only really know that a factor is negligible when we possess the appropriate theoretical knowledge. Furthermore, it is not always the case that idealization assumptions involve neglecting some small factor. Cartwright writes,

> Sometimes the omitted factors make only an insignificant contribution to the effect under study. But that does not seem to be essential to idealizations, especially to the idealizations that in the end are applied by engineers to study real things. In calling something an idealization it seems not so important that the contributions from omitted factors be small, but that they be ones for which we know how to correct. If the idealization is to be of use, when the time comes to apply it to a real system we had better know how to add back the contributions of the factor that have been left out.[9]

Thus in cases, where we do possess the adequate theoretical knowledge to use a correction factor for adding back in a particular idealization assumption, it is considered to be a non-ad hoc correction factor. One point worth noting before moving on to the second form of justification is that many of the explanations or predictions which we find in physics or in engineering use ad hoc correction factors. The explanations are most often found in cases when we require the use of theoretical knowledge from separate areas of physics. For instance, the derivation for calculating the radiation from a black hole involves the use of theoretical knowledge from both quantum mechanics and general relativity. Since there still is no theory which unifies these two theories, physicists resort to ad hoc correction factors in deriving theorems to explain the radiation. Moreover, physicists are unsatisfied with the use of these equations which contain ad hoc factors.

Physicists also consider explanations or predictions which use idealization assumptions as being good if the explanation or prediction falls within some predicted error. Error analysis is a common feature of scientific practice and the statistical analyses which are used to calculate

[9] Ibid. p.111

the allowable errors is considered to be quite reliable It is important to note, however, that in these cases the error is calculated within the theory That is, the error is calculated by first assuming that the theory is true or highly confirmed So if a result falls within some given error, the status of the given explanation or prediction will only be as good or correct as the theory

Ubiquity of Idealizations and Why Idealizations are Necessary

Laymon defines the ubiquity of idealizations claim as:

> Actual derivations will always (or nearly so) require the use of idealizations and approximations [10]

Since actual derivations are considered to be explanations within physics, we can reformulate the thesis in terms of explanation The reason for doing so is that I will be considering what consequences the use of idealizations has on a D-N model of explanation and the use of inference to the best explanation We, therefore, have,

> Scientific explanations will always (or nearly so) require the use of idealizations and approximations

There are several reasons why we must appeal to idealizations in physics. Laymon notes that scientists appeal to idealizations primarily for two reasons The first is because we do not possess the adequate computational abilities, and the second is because we not have the 'necessary data and required auxiliary theories' to provide a complete analysis of the phenomenon or event to be explained

A simple example of the first case of idealization would be an analysis of the earth's orbit Typically, an explanation of a planet's motion would appeal to Kepler's laws The derivation, however, of Kepler's laws from Newton's laws requires two idealizing assumptions. The first is that the sun is at rest and the second is that the forces due to the other planets is negligible For the moment, consider the second simplifying assumption.

[10]Ronald Laymon, *Cartwright and the Lying Laws of Physics*, p.357

Clearly, the second simplifying assumption is due to inadequate computational abilities since we do not have the computational abilities to calculate the motion of a body when three bodies are involved, let alone when a large number of bodies are involved. However, we do have all of the theory required. Thus, the simplifying assumption is not due to a lack of data or lack of auxiliary theories. This example, furthermore, shows that we must use idealizing assumptions when we are calculating the motion of any body since according to the gravitational law, every massive body exerts a gravitational force on every other massive body.

An example of an idealization due to a lack of data or auxiliary theories would be a case of what McMullin termed a material construct idealization. Consider as an example, the explanation of Uranus' orbit prior to the discovery of Neptune. What was curious about Uranus' orbit was that the data suggested that there was another planet beyond it which was affecting its orbit. However, this planet had not been visually discovered yet. Thus, scientists did not know the mass of the planet nor did they know the distance of the planet from the sun. Thus, in the explanation of Uranus's orbit, scientists were forced to ignore the effects of Neptune's influence on Uranus' orbit.

Another reason which I believe motivates but does not require scientists to use idealizations stems from the nature of a certain form of scientific explanation, causal explanation. Espagnat writes,

> Apart from experimental confirmation, however, something more is generally demanded of a theory. It is expected not only to determine the results of an experiment but also to provide some understanding of the physical events that are presumed to underlie the observed results. In other words, the theory should not only give the position of a pointer on a dial but also explaining why the pointer takes up that position [p 158, The Quantum Theory and Reality]

Note, that in the last line, Espagnat claims that what a theory must do is explain what causes a particular phenomenon or why it is a particular phenomenon came about. Although one of the primary disputes in the philosophy of science is what exactly characterizes a good scientific explanation, it is generally agreed that scientific explanations must not only describe and predict, but they must also provide a form of understanding In causal explanations, this form of understanding is achieved by showing how a particular event is the result of some causal process Although there is nothing wrong with identifying every single causal process associated with a particular event, scientists deem it unnecessary Salmon writes,

> It is important to note, in this connection, that particular facts do not necessarily embody all of the features of the phenomena which are involved [11]

Consider, for example, an archaeologist's explanation of why there is a particular worked bone at a site in Alaska Certain features of the bone are considered relevant to the explanation (the fact that the bone is thirty thousand years old, the fact that it was worked by a human artisan, and the fact that it had been deposited in an Alaskan site) However, other features of the bone are considered irrelevant (the size and shape of the bone, and the distance of the site from the nearest stream) The point of this example is that, in the explanation of this event (why there is a particular worked bone at a site in Alaska), we ignore certain features of the bone since they are not relevant to the particular explanation which is demanded

Of course, which causal process is sought after in the explanation and thus which features are deemed relevant or irrelevant to the explanation will depend on how the why question is formulated. For the purposes of my point however, this does not matter What is important is that scientific explanations do not require that we identify every single causal process associated with an event to be explained Therefore, any explanation of an event will always require an idealization which will depend on what is demanded for in the explanation. Thus, from a practical point of view, if our goal is to give causal explanations, we would want to use idealizations

[11] Salmon, W, **Scientific Explanation and the Causal Structure of the World**. p 273

Idealizations and Explanations

So far what I have discussed in chapter 1 are the different types of idealizations and why they are necessary in physical explanations. What I will do in this section is to relate idealizations to explanations through the notion of a model. The reason for doing this is mainly heuristic since what we often find in physical explanations of an event are the construction of models. That is, if we want to explain an event, we do this be constructing a model from our fundamental laws such that we can fit the event into our model. I should note that what I will mean by a 'model' is not the same as what is traditionally meant by models. Therefore, when I will be discussing models, I will not be referring to conceptual representations such as the Bohr model of the atom, or the Sommerfeld model.

Van Fraassen defines a model as "any structure which satisfies the axioms of a theory"[12] So, if we consider a theory as consisting of a set of axioms, then a structure is a model if the axioms of the theory are true of the structure. Van Fraassen's definition of a 'model', as he notes himself, is slightly different from the notion of 'theoretical model' defined by Achinstein. Achinstein characterizes a model as

> 1) consisting of a set of assumptions about some object or system,
> 2) describing a type of object or system by attributing to it what might be called an inner structure, composition, or mechanism, reference to which will explain various properties exhibited by that object or system,
> 3) being an approximation for useful purposes.

Van Fraassen calls this notion of a 'model' as a 'model-type'. Examples of model-types are the familiar cases such as the Bohr model of light, the billiard model of gases, the corpuscular model of light, the shell model of the atomic nucleus, and the free electron model of the atom. The important difference between a model and a model-type is that a model-type represents a certain class of structure or models in which certain parameters are left unspecified. Models, however, refer to 'specific structures in which all the

[12]Bas van Fraassen, *To Save the Phenomena*, **The Scientific Image** p 43

relevant parameters have specific values' For example, the billiard ball model for gases leaves all of the parameters of the ideal gas law unspecified However, if we were to construct a model of the billiard ball type, we would need to specify the amount of molecules in the container, and the relevant pressure, volume, and temperature The salient point of these definitions of model-type for Van Fraassen and model for Achinstein is that a model consists of a set of assumptions

There is an important point which should be brought out concerning the relation of different models According to Achinstein, a model-type consists of a set of assumptions about some object or system. Note, that which particular model-type is constructed depends on what set of assumptions are chosen From this, we can therefore relate different models through the relation of their relative sets of assumptions In other words, the way in which models are related will depend on the way in which the sets of assumptions are related Let us consider a few examples

Within Newtonian theory, one could construct several model-types concerning the behaviour of gases in a volume. Consider two models. the billiard ball model of gases for deriving the ideal gas law ($PV = nRT$), and the weakly-attracting rigid-sphere model for deriving Van der Waals equation $[(P + a/v^2)(v - b)] = RT$ where $b = 2/3N\pi r^3$ Sears and Salinger note that the derivation of the Ideal Gas Law involves five idealizing assumptions

> 1) Any macroscopic volume of a gas contains a very large number of molecules
> 2) The molecules are separated by distances that are large compared with their own dimensions and are in a state of continuous motion
> 3) The molecules exert no forces on one another except when they collide
> 4) The collisions of molecules with one another and with the walls are perfectly elastic
> 5) In the absence of external forces, the molecules are distributed uniformly [13]

Note that Van der Waals equation contains several additional terms. Recall that the factor, *a*, shows how the properties of pressure, volume, and

[13] Sears and Salinger, **Thermodynamics, Kinetic Theory, and Statistical Thermodynamics** pp 276-278

temperature depend on the electromagnetic forces, and the factor, b, shows how these properties depend on molecular sizes Thus, Van der Waals equation takes into account assumptions (2) and (3), and so does the corresponding model The set of assumptions associated with the weakly-attracting rigid-sphere model for deriving Van der Waals equation is only a subset of those idealizing assumptions used for the ideal gas law Although both models-types are constructed from the same theory (Newtonian theory), one model can embedded into the other More generally, we can say that if a model Θ consists of a set of assumptions, θ, and Ψ consists of a set of assumptions, ψ, and θ is a subset of ψ, then we can embed Ψ in Θ In our example, the billiard ball model can be embedded into the weakly attracting rigid-sphere model by setting certain parameters to zero This is due to the fact that the idealizing assumptions used to construct the weakly-attracting rigid-sphere model is a subset of the assumptions used to construct the billiard ball model

There are frequent cases in physics, however, where two sets of idealizing assumptions are not subsets of one another, or in other words, it is not possibleto embed one model-type into the other Consider the case of trying to explain the motion of a particle in which all four fields are involved, the gravitational field, the electromagnetic field, the field of the weak interaction, and the field of the strong interaction We could construct one model in which we neglect the field of the weak interaction, or we could construct another model which involved neglecting the field of the strong interaction. It would not be possible, however, embed one model in the other since the idealizing assumptions are not subsets of the other We would thus not expect the values obtained in one model to be isomorphic with any of the values obtained in the other

The next chapters will focus on what consequences the use of idealization assumptions has concerning the realism/anti-realism debate The point of the examples at the beginning of the chapter and the discussion of models was to show that scientific explanations and predictions which appeal to idealizations are still acceptable This might seem like a trivial point, since it would seem that all scientific explanations involve idealization assumptions What needs to be addressed is the implications their use has for our models of explanation One implication is that the use of idealization assumptions does not allow us to provide exact logical deductions of events or phenomena Or in other words, explanations which involve idealizing assumptions do not satisfy the constraints of the D-N model The fact is, however, that these explanations are still considered good explanations A possible conclusion then is that precise logical deductions is not a necessary condition for being a good explanation It should be made clear that this problem is quite different from the traditional asymmetry problems exemplified by the well-known flag-pole example This example showed that the constraints provided by the D-N model cannot be a sufficient condition for being an explanation So why should we not simply abandon the D-N model of explanation? If the use of idealizations requires us to give up the D-N model of explanation, then it also requires us to give up the hypothetico-deductive method of confirmation This poses a problem for the confirmation of fundamental laws What I will do in Chapter 2 is make explicit how this is relevant to the realism/anti-realism debate about the fundamental laws What I will then explore, in Chapter 3, is Cartwright's challenge to the realist, and consider an alternative model of confirmation which might save realism

Chapter 2

The last chapter has focussed on the different types of idealizations that are used in physics and why the use of idealization assumptions is necessary The focus of discussion in this chapter and the rest of the thesis will be the consequence the ubiquity of idealizations thesis has on the realism/anti-realism debate with respect to the fundamental laws The focus of discussion for the rest of the thesis will be the fundamental laws. I will define a scientific realist as one who believes that the aim of science is to discover a true theory, and more importantly for our discussion, true fundamental laws Thus, scientific realism will be defined in the following way to have good reason to accept a theory is to have good reason to believe that the fundamental laws are true Conversely, anti-realism will be defined as a, "a position according to which the aims of science can well be served without giving such a literally true story and acceptance of a theory may properly involve something less (or other) than belief that it is true."[14] The debate that I will be considering will focus on whether the fundamental laws are true, and whether the aim of science should be to seek true fundamental laws. The challenge that will be posed to the realist will be to offer justification for the claim that the aim of science should be to seek true fundamental laws. The realist methodology I will consider for offering this justification will be to try to argue that there are in fact such things as true fundamental laws. If this can be argued for successfully, then it would seem to provide some reason to accept that the aim of science should be to seek such laws. My primary concern will not be the realist's argument for the latter claim, but rather in trying to establish the former claim, that there are in fact true fundamental laws. Note, that the focus will not be on the existence of theoretical entities as is more often discussed in realism/anti-realism debates in the philosophy of science. However, I will briefly discuss this issue in distinguishing Cartwright's form of anti-realism from Van Fraassen's.

My intent in this chapter is to summarize Cartwright's argument for her particular form of anti-realism. In the first section, I will outline two of her arguments for the claim that the fundamental laws are false. The first argument will attack the realist view that the fundamental laws describe the facts. The second argument will be more general, in that it takes issue with one of the central tenets of the realist position, inference to the best explanation. In considering the rejection of this form of inference, I will also discuss Van Fraassen to show how his form of anti-realism differs from Cartwright's. I will then consider an account (the generic-specific account) which seems to respond directly to the anti-realist challenge concerning inference to the best explanation. From there, I will consider Cartwright's objection to this account which will bring out the role idealizations have in the realism/anti-realism debate. Before moving onto this discussion, it would be useful to clarify what exactly is meant by fundamental laws and how they differ from phenomenological laws.

[14]Bas Van Fraassen, *To Save the Phenomena*, **Journal of Philosophy** V 73, pp. 672-3

Typical examples of fundamental laws in physics are generalizations such as Newton's three laws, Schrodinger's equation, and Faraday's laws of electromagnetism These laws which we consider to be at the core of our theories are characterized by their general and abstract nature The usefulness of fundamental laws lies in their ability to explain a wide class of phenomena Phenomenological laws, on the other hand, tend to be very specific and detailed Thus, they are only used to explain a specific class of phenomena Typical examples of phenomenological laws are the exponential decay law in quantum mechanics and Airy's law in electromagnetism The fact that fundamental laws figure in explanations for a wide class of phenomena and phenomenological laws only figure in explanations for a restricted class is what I take to be the primary difference between these two forms of laws

Cartwright's Challenge

A predominant realist view in physics is that the fundamental laws describe the behaviour of real objects This is what Cartwright terms the 'facticity view of laws', and it is the primary aim of her book to show that this view is false Consider, for example, the gravitational law A common definition found in physics textbooks is,

> The Law of Gravitation is that two bodies exert a force between each other which varies inversely as the square of the distance between them, and varies directly as the product of their masses.

Taken literally, this definition is false if it is meant to describe the behaviour of objects. The reason is that the force is only equal to that described by the gravitational law if there are not any other forces interfering Thus, the law is true only in cases where there are no interfering forces. In conditions where there are other forces, the law is false if it is meant to describe the behaviour of real objects. Cartwright notes that one possible way to preserve the truth of the gravitational law is to claim that there is an implicit ceteris paribus clause in the law. Thus it should be read as,

> Under ideal conditions, two bodies exert a force between each other which varies inversely as the square of the distance between them, and varies directly as the product of their masses

The literal translation of ceteris paribus is 'all other things being equal' The problem with ceteris paribus clauses, which many recent philosophers have pointed out, is in providing some plausible interpretation as to what is meant by 'all other things being equal' At times, it has been construed as meaning 'under normal conditions' and at other times, it has been construed as 'under ideal conditions' For my purposes, I will not go into the problems of providing a non-circular or non-vacuous account when interpreting ceteris paribus in either of these ways Since even if these problems can be solved, the use of ceteris paribus clauses still does not help the realist for the following reason. Suppose that what is meant by ceteris paribus is 'under ideal conditions' Thus, if we read the gravitational law with a ceteris paribus clause, we have the following,

> Under ideal conditions, two bodies exert a force between each other which varies inversely as the square of the distance between them, and varies directly as the product of their masses

In this case, the 'under ideal conditions' is to be construed as, if there are not any other forces interfering The 'ideal' rather than 'normal' seems appropriate in this case since the conditions in which the law will be true never actually obtain The conditions, however, are closely approximated when there is an isolated particle in far out space There are, of course, law statements in which the conditions in the ceteris paribus clause actually do obtain For instance, Snell's law

> For any two media which are optically isotropic at an interface between dielectrics there is a refracted ray in the second medium, lying in the plane of incidence, making an angle θ_t with the normal, such that
>
> $$\sin \theta / \sin \theta_t = n_2/n_1$$

holds in any two media which are optically isotropic. What is meant by isotropic is that the properties of the material do not depend on the direction of transmission. For example, glass is isotropic with respect to refraction of light

At any rate, construing the fundamental laws as having an implicit ceteris paribus clause allows for some interpretation in which the laws come out true. By reading the laws with an implicit ceteris paribus clause, the suggestion is that we take a counter-factual rather than a subjunctive reading of the law.[15] That is, the law should be read as, "Were it the case that all other factors were absent, then, given certain initial conditions, certain resultant conditions would obtain." The idea is that the law holds in a possible world or set of possible of worlds in which the ideal conditions do obtain, and it is in virtue of these worlds that the law is said to be true. Although the use of implicit ceteris paribus clauses allows realists to hold onto the claim that the fundamental laws are true, this response raises problems for explanations if the realist wants to retain the covering-law model of explanation.

According to Hempel's original formulation of the D-N model, explanations are deductive arguments which take on the following form,

$$\frac{\begin{array}{l} L1, L2, L3, \ldots, Lr \quad \text{(Explanans)} \\ C1, C2, C3, \ldots, Ck \end{array}}{E \qquad \text{(Explanandum)}}$$

An explanation of an event E, therefore, consists of a deduction from a set of law statements and statements of initial conditions. A requirement for the argument to be an explanation is that both the law statements and the statements of initial conditions be true. The problem with understanding fundamental laws as containing an implicit ceteris paribus clause is that the law can only be used in an explanation when the ceteris paribus clause is satisfied. Cartwright writes, 'a law that holds only in restricted circumstances can explain only in those circumstances'. Thus by saving the truth of the fundamental laws by including an implicit ceteris paribus clause, the laws lose their explanatory power in those cases where the ceteris paribus clause is not satisfied. Consider, for example, trying to explain the trajectory of an electron when it is interacting with another electron. If the gravitational law is to be understood with an implicit ceteris paribus clause, then under those conditions when the ceteris paribus clause is not satisfied, the law cannot be used in the explanation of the particular event. Thus, if we abide by the restrictions of the D-N model, the

[15] Geoffrey Joseph, *The Many Sciences and the One World*, **The Journal of Philosophy V.87**, p 777

law cannot be used in the explanation of the trajectory of the electron. The fact, however, is that the law does and should figure in the explanation of this event, so we must either reject the D-N model of explanation, reject the idea that laws have an implicit ceteris paribus clause, or reject both. Before considering a possible response, let us consider Cartwright's second challenge to the realist.

Rejecting Inference to the Best Explanation

The point of Cartwright's objection is that the realist assumes that for a law to explain an event, it must be true. Moreover, the realist also assumes that if a law explains an event, we can infer that the law is true. This is precisely what the anti-realist denies. That is, the anti-realist denies that we can infer from that facts that x explains y, and y is true to the claim that x is true. This is simply denying that inference to the best explanation is a valid form of inference.

In Harman's original paper, he characterizes inference to the best explanation as,

> one infers, from the premise that a given hypothesis would provide a "better" explanation for the evidence than would any other hypothesis, to the conclusion that the given hypothesis is true.[16]

Thus, if x best explains y and y is true, we can infer that x is true. The relevance of this form of argument to our discussion is the following. The realist argues that our present theory which includes our fundamental laws best explains the phenomena. Therefore, we can infer that the fundamental laws are true. Note, that an important constraint on this form of argument is that the explanation which is to be inferred as being true *must be the best explanation*. The anti-realists I will consider reject inference to the best explanation by using arguments which focus on this criterion. Duhem and Van Fraassen, for instance, have argued that "for any given set of phenomena, in principle, there will always be more than

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[16]Gilbert Harman, *The Inference to the Best Explanation*, **Philosophical Review V.74** (1965), p 89

one equally satisfactory explanation, and some of these explanations will be incompatible" Van Fraassen's view is that the primary aim of science is to save the observable phenomena, and that there will always, in principle, be more than one theory which accomplishes this task He, furthermore, claims that we could always construct two theories that are incompatible His claim is that an explanation is not the best because it is true, but rather because it has more pragmatic virtues such as simplicity than another Thus, an explanation would be the best, on van Fraassen's account because it is the simplest and it has the most organizing power It is important to note that according to van Fraassen, these virtues are not indicators of truth, but rather indicators of what we are inclined to prefer Furthermore, it would be a mistake to think that our inclinations can reliably guide us towards the truth

Van Fraassen's argument, therefore, against the realist is that since we can always construct incompatible theories which save the same phenomena, we cannot infer the truth of any of the theories An important point concerning this argument is that we can only make the conclusion that there is no true theory if we construe incompatible as meaning there being a semantic incompatibility rather than merely only a syntactic incompatibility Realists have been quick to point out that it should not be taken for granted that such a task can always be accomplished Although Van Fraassen provides an example of what he means by two semantically incompatible theories to explain the same phenomena, he does not argue that we could always construct an incompatible theory nor does he offer any general methodology to do so. If Van Fraassen can offer such an argument, then it seems that there is never really any best explanation Thus, we could never infer the truth from a given explanation In other words, something can satisfy all the conditions for being an explanation and still not be true It thus seems that truth is an external characteristic of explanations

The anti-realist response to why we do in the end choose a particular theory as the 'best' is the result of pragmatic considerations Cartwright writes,

> Explanation (at least high level explanations of theoretical science which are the practical focus of the debate) organize, briefly and efficiently, the unwieldy, and perhaps unlearnable, mass of highly detailed knowledge that we have of the phenomena But organizing power has nothing to do with truth [17]

What we find in the sciences are that theories are chosen as 'best' for reasons of simplicity, efficiency, organizing power, and brevity But what do these notions have to do with truth? To think that these notions have anything to with truth is to presuppose that nature is itself simple and organized The anti-realist argument is simply to call this presupposition into question

To characterize Cartwright's position as only rejecting inference to the best explanation would be an over-simplification, since Cartwright is not really an anti-realist like Van Fraassen and Duhem She does not reject inference to the best explanation tout court, since she does not reject inference to causal explanations It is this distinction that she draws which makes her position particularly interesting The distinction allows her to be a realist about theoretical entities, but an anti-realist about the fundamental laws Let me explain

Cartwright notes that we observe two different forms of explanation in physics,

> Explaining in physics involves two quite different kinds of activities first, when we explain a phenomenon, we state its causes We try to provide detailed accounts of exactly how the phenomenon is produced Second, we fit the phenomenon into a broad theoretical framework which brings together, under one set of fundamental equations, a wide array of different kinds of phenomena The causal story uses highly specific phenomenological laws which tell what happens in concrete situations But the theoretical laws, like the equation of continuity and Boltzmann's equation, are thoroughly abstract formulae which describe no particular circumstances. [18]

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[17] Nancy Cartwright, **How the Laws of Physics Lie** p.87

[18] Ibid p 11

Thus, one form of explanation (fitting the phenomenon into a broad theoretical framework) involves explaining an event by relating it to other similar events through a fundamental law of nature The way in which this process often proceeds is through the construction of a model, and showing how the event fits into the model. Other explanations which involve citing the detailed causal story of an event, Cartwright claims, make use of phenomenological laws Thus, the objective in these forms of explanations is not to relate it to other similar events through a fundamental law, but simply to derive the event from some phenomenological law within the theory and a statement of initial conditions

As I stated earlier, the standard anti-realist argument against inference to the best explanation is to note that for every set of phenomena, there are several incompatible explanations or theories from which we can derive these explanations Since it appears that we can have incompatible explanations for the same phenomena, truth seems to be an external characteristic of explanations because if the explanations are incompatible, then they cannot both be true Recall, however, that the reason why we will choose one theory over another will be pragmatic, and these virtues, like simplicity and organizing power, are simply indications of our natural inclinations or preferences A point Cartwright notes, is that the redundancy which we find for theoretical explanations, is not found with causal explanations Thus, Cartwright agrees with Van Fraassen that in general there are or can be redundant explanations However, for causal explanations, she claims that this is not true. She writes,

> In physics, it is usual to give an alternative theoretical treatments of the same phenomena We construct different models for different purposes with different equations to describe them Which is the right model, which the 'true' set of equations? The question is a mistake One model brings out some aspects of the phenomena, a different model brings out others.. No single serves all purposes best Causal explanation is different We do not tell first one causal story then another, according to our convenience [19]

[19] Ibid p.11

Unlike theoretical explanations, Cartwright claims that we can infer the truth of causal explanations Thus, when one asks, 'what caused x?', the anti-realist cannot offer the same argument from redundancy as in the case of other types of explanation. She notes that we do not have the same tolerance for causal explanations as we do for theoretical explanations

Her argument, it seems, can be construed in two ways, either metaphysical or epistemological In the metaphysical way, the argument is the simple claim that for a single effect, there can only be one cause The problem with this claim is that even though our intuition seems to go in this direction, there is a serious difficulty which must be accounted for, overdetermination The problematic cases are those in which we have a single effect which seems to be the result of more than one cause For example, suppose that a piano falls on someone at the same time that the person is shot. Can we say that both the piano and the gunshot caused the death of the person? Of course, Cartwright could respond yes, since the causes in these cases are not incompatible Prima facie, at least, it seems that we could claim that it is true that the piano caused the death of the person and the gunshot also killed the person However, it should be noted that if Cartwright does respond yes, then the statement that the death of the person was the result of a single cause would be false, and thus the general statement, that for every single effect, there is a single cause would also be false This difficulty, however, is not fatal for Cartwright Recall that the argument against inference to the best explanation required that there not only be alternative explanations, but that the explanations also be incompatible Although the examples of overdetermination involving causation which are found in the standard literature provide us with cases of alternative causal explanations of the same event, these explanations are not incompatible

The second way of construing Cartwright's argument, as I stated earlier, is epistemological. A central feature of Cartwright's argument for the justification of inference to causal explanation concerns the tolerance that we have for different forms of explanation Cartwright's claim is that we tolerate alternative theoretical explanations, however, we do not tolerate alternative causal explanations. In other words, her claim is that once we accept a particular causal explanation for a phenomenon, then our epistemic commitment towards this explanation is the belief that it is true. However, in the case of theoretical explanations, once we accept these explanations, our epistemic commitment towards these explanations does not necessarily have to be the belief that it is true, we may merely accept that it succeeds in showing this particular phenomenon is similar to other phenomena accounted for in the theory The crucial part of this argument is the claim that for causal explanations, once we accept a particular causal explanation, we believe that it is true

Cartwright uses an example from quantum mechanics to illustrate her point If one asked 'what caused the line-width that we observe in a spectroscope for a radiating atom?', physics offers only one cause. In this case, the cause is the emission and reabsorption of real photons. Recall that since causal explanations compete with one another unlike other explanations, we can infer the truth of a particular explanation only once it has been accepted Cartwright believes that in claiming that the explanation is true, it requires that we believe in the existence of photons. The same type of reasoning can be used to argue for the existence of other theoretical entities

There is an important question which needs to be addressed which is relevant to our discussion: how can Cartwright be a realist about theoretical entities yet be an anti-realist about fundamental laws? Cartwright writes,

> What I invoke in completing such an [causal] explanation are not fundamental laws of nature, but rather properties of electrons and positrons, and highly complex, highly specific situation I infer to the best explanation, but only in a derivative way I infer to the most probable cause, and that cause is a specific item, what we call a theoretical entity But note that the electron is not an entity of any particular theory 20

From this passage, it seems that what we infer from a causal explanation is the existence of particular object or the causal capacity of a particular object However, as Cartwright notes, claims about the properties of that

[20] Ibid p 92

entity are particular to theories, however, the existence of the object or the causal capacities is not Thus, it seems that Cartwright must claim that the fundamental laws or at least some of them do not describe the causal capacities of theoretical entities. Cartwright's view, I believe, is that certain fundamental laws such as the gravitational law and the laws of electromagmetism describe the causal capacities of theoretical entities, however, other fundamental laws such as Boltzmann's equation do not A salient point of this is that Cartwright is clearly advocating the truth of causal laws In her recent book [1989], she explicates how a notion of causation can be formulated out of the idea of capacities Very briefly, Cartwright believes that entities, theoretical and non-theoretical, possess capacities, and the purpose of some fundamental laws is to describe these capacities This is quite different from the claim that the fundamental laws describe the behaviour of objects Thus, we should construe some fundamental laws as ascribing a capacity to objects The salient point is that the motivation for Cartwright's realism about theoretical stems from her realism about causation and causal powers

There are two points that should be noted about competing causal explanations. First, although causal explanations do not compete unlike other explanations, this does not preclude there being partial causal explanations. For example, in Lewis's example of explaining why the car swerved off the road, several causal explanations can be offered such as the faulty brakes, the wet road, the used tires, etc Which explanation is offered depends on the relevance relation or the context in which the why question is formulated. The reason is that these causal explanations do not conflict with one another since they are not responding to the same why question Furthermore, a theory can also provide competing causal explanations as long as it is recognized that the causal explanations are competing since we do not accept either of the explanations Thus, we would not believe or accept that both causal explanations are true

Cartwright claims that the intolerance that we have for accepting competing causal explanations is not exhibited for other forms of explanation. That is, within physics, it is acceptable to have more than one theoretical derivation of a phenomena which we take to be correct Her claim is that models which fit the phenomena into a theoretical framework do not compete with one another For example, unlike the causal story for the line-width, there exist six different mathematical treatments for deriving the shape and width of the line The six treatments are (1) the Weisskopf-Wigner method, (2) the Heitletr-Ma method, (3) Goldberger-Watson method, (4) Quantum statistical method, master equations; (5) Langevin equations corresponding to the master equation and a c-number representation and (6) the neoclassical theory of spontaneous emission Cartwright, furthermore, notes that,

> All of the approaches employ the basic format of quantum mechanics Each writes down a Schrodinger equation, but it is a different equation in each different treatment (Actually among the six treatments there are really just three different equations)[21]

The Schrodinger equations differ depending on which Hamiltonian is chosen to describe the system. One important point worth noting about this example is that the alternative treatments can be grouped into two classes, depending on which picture of quantum mechanics one adopts The two pictures are the Schrodinger picture of quantum mechanics and the the Heisenberg picture If the derivation is performed in the Heisenberg picture, it involves writing down a Langevin equation which describes the interaction between the field and the atoms The derivation in the Schrodinger picture involves writing down a density equation describing the interaction between the field and the atoms At this stage, the equations are considered to be exact In other words, no approximations or idealizations have been used With either of these two equations one can use different approximations or idealizations to derive the line-width which correspond to the different derivations given above

According to Cartwright, the fact that we do offer alternative theoretical treatments, namely that different Schrodinger equations are used, indicates that neither of them is actually true. She writes,

[21] Ibid. p.80

> Perhaps, contrary to my argument, the multiplication of theoretical treatments says more about this pragmatic orientation than it does about how explanatory laws ought to be viewed I disagree I think that it does speak about laws, and in particular show how laws differ from causes We do not have the same pragmatic tolerance of causal alternatives We do not use first one causal story in explanation, then another, depending on the ease of calculation, or whatever [22]

Cartwright's argument then is slightly different then the original anti-realist argument which I described above For it is not only the case that there are, *in principle,* alternative explanations, but rather that physics is actually in the practice of giving them The intolerance that we have for redundant causal explanations, Cartwright believes, shows that causal explanations carry truth on their sleeves Thus if x causally explains y and y is true, then we can infer that x is true This is what I take to be Cartwright's main argument for the claim that the fundamental laws are false. From the same set of fundamental laws, physics actually provides alternative incompatible explanations of the same phenomena

There are several things I want to note about this argument and the example The first concerns the ambiguity of the example As I stated earlier, which Schrodinger equations is chosen to describe the system depends on which treatment we use Furthermore, the treatments can be grouped into classes, depending on which interpretation of quantum mechanics is chosen, the Heisenberg picture or Schrodinger picture There is an ambiguity in the example because there are two senses of what is meant by alternative theoretical models in the example The two types are 1) constructing a model in the Schrodinger picture and constructing one in the Heisenberg picture and 2) constructing two models within one of the pictures Consider the first case

As I have just noted, there are two interpretations of quantum mechanics, the Schrodinger picture and the Heisenberg picture Louisell

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[22] Ibid p 81

notes that the two pictures are 'physically equivalent'[23] What he probably means by this is that we obtain the same results for the physically observable variables The two pictures differ in the following way According to the Schrodinger picture, the basis vectors are described as being stationary, and the state vector is described as moving, and according to the Heisenberg picture, the basis vectors are described as moving, and the state vector is stationary Basis vectors describe all possible states of the system That is, a particular state of any object in the system can be described as a linear combination of the basis states State vectors are simply some particular linear combination of the basis states which describe some physical property of the object. There are, however, transformation laws which allow us to move from one picture to the other. Cartwright's main claim of the example is that the treatments in both cases use different Schrodinger equations The important question is, how do they differ? There is a sense in which they only differ notationally In his discussion of the two methods for deriving the line-width, Louisell writes

> That is, the total hamiltonian in the HP is equal to the total hamiltonian in the SP [24]

where HP represents the Heisenberg picture and the SP represents the Schrodinger picture He, furthermore, writes,

> Also the reader should compare (6 3 38) with (6 3 7) and note that the functional forms of these two equations are identical just as they would be under the exact transformation from the SP to the HP This is the analog of thinking of a fixed coordinate system and rotating vectors as being equivalent to a rotating coordinate system and fixed vectors [25]

From the passage, it is unclear whether there is really any semantic difference in the two pictures If so, the fact that the two treatments use

[23] William Louisell, **Quantum Statistical Properties of Radiation**, p.55

[24] Ibid p 362

[25] Ibid pp 365-366

different Schrodinger equations may simply mean that both treatments are the same, but they are formulated in a different language. Thus, to claim that the basis vector is moving in the Heisenberg picture and the basis vector is stationary in the Schrodinger picture may not be a contradiction. If this is true that the two treatments differ only notationally, then it is unclear whether the treatments are actually incompatible. And if they are not in fact incompatible, then this example does not pose any problem for the realist. Alternative descriptions of entire areas of physics is not restricted solely to quantum mechanics. Recently, for instance, in an attempt to unify electromagnetism and general relativity, physicists have come up with a different way of interpreting electromagnetism which involves the use of different equations and laws, and even a different interpretation of a charge. Rather than interpreting the charge as a particle in the classical treatment, this new interpretation treats a charge "in terms of source-free electromagnetic-fields that (1) are everywhere subject to Maxwell's equations for free space but (2) are trapped in the "wormholes" of a space with a multiply-connected topology."[26] As I said earlier, this is one possible interpretation of what Cartwright means by *alternative theoretical treatments*, but it is not the only interpretation. In fact, I do not think that Cartwright intended her example to be understood in this way.

The other way of understanding what Cartwright means by alternative theoretical treatments is to construct two different models within the same picture. This simply means that we construct two different models within either the Schrodinger picture or the Heisenberg picture. Recall that in chapter 1, the term 'model' was used to represent some structure which satisfied the axioms of the theory. Thus we can derive different models depending on what assumptions are used. That is, from the same set of fundamental axioms or laws in a theory we could construct a model, M1 using idealization assumptions I1, and construct a different model, M2, using idealization assumptions I2. In such a case, when we use the term 'basis vectors' or say that the basis vector is moving,

[26]Misner, C. & Wheeler, J., *Classical Physics as Geometry*, **Annals of Physics, 2**, p.525

we mean the same thing in both models since both models are constructed from the same set of laws *with the same interpretation*. This is the way I believe Cartwright wishes to be understood when she writes,

> In physics, it is usual to give an alternative theoretical treatments of the same phenomena We construct different models for different purposes with different equations to describe them Which is the right model, which the 'true' set of equations? The question is a mistake One model brings out some aspects of the phenomena, a different model brings out others No single serves all purposes best

The reason why Cartwright's example has to be understood in this way is because it is unclear how to make sense of the claim that one model brings out some aspects of the phenomena that the others don't For according to the first way of understanding Cartwright's example, neither interpretation really brings out aspects that the other doesn't since we do have transformation principles such that we can freely move from one picture to the other. Furthermore, Cartwright cites another example in which it is clear that this is what she means by alternative theoretical treatments.

There are two standards treatments in the derivation of the exponential decay law in quantum mechanics, the Weisskopf-Wigner treatment and the Markov treatment In the Markov treatment, exponential decay is seen as a special case in the quantum theory of damping Louisell writes,

> Here we consider an abstract system weakly coupled to a reservoir The aim is to derive a general master equation for the evolution of the system. [27]

The idealizations and approximations involved in this treatment are

> 1)extending the time integrals which involve only reservoir correlations to infinity, on the grounds that the correlations in the reservoir are significant for only a short period compared to the periods over which we are observing the system, 2)letting t-to 0, on the grounds that the periods of time considered for the system are small compared to its lifetime.[28]

[27]Nancy Cartwright, **How the Laws of Physics Lie**, pp.113-114

[28]Ibid p 114

In the Weisskopf-Wigner treatment,

> we begin with the exact Schrodinger equations for the amplitudes, but assume that the only significant coupling is between the excited and the de-excited states

In this case, there are three approximations,

> 1) the rotating wave approximation,
> 2) the replacement of a sum by an integral over the modes of the electromagnetic field and the factoring out of terms that vary slowly in the frequency,
> 3) factoring out a slowly-varying term from the time integral and extending the limit on the integral to infinity

The two methods are constructed using the same interpretation of quantum mechanics. Therefore, what we have are two models of the same phenomena constructed from the same set of fundamental laws under the same interpretation which differ according to what approximations are used.

If the intent of Cartwright's line-width example is to be understood in this way, then it is not obvious how it argues against the claim that the fundamental laws are true. For it is not necessarily true that alternative mathematical treatments imply that there is not *one* correct mathematical treatment that captures the advantages of each of the accounts However, as Cartwright points out, the burden of proof rests on the side of the realist The realist must offer justification as to why we should believe that there exists a single theory which explains all the phenomena She writes,

> We should agree that the end of theoretical physics is in view only when it is clear they have done so.

Thus it is left up to the realist to show what it is about scientific practice that should make us want to be realists about the fundamental laws

At this point, it seems that we cannot retain the claim that the laws of nature describe the facts and also retain a D-N model of explanation The reason, recall, is that explanations which we consider to be good do not satisfy the constraints of the D-N model Furthermore, these explanations which do not satisfy the D-N model also underdetermine the event to be explained. Thus, the realist must not only offer an alternative model of explanation or an alternative interpretation for what the laws describe to show how the laws figure in the explanation, but the realist must also explain why it is that we allow alternative theoretical explanations For if it is the case that the theoretical explanations underdetermine the event to be explained, then it seems that the anti-realists are correct when they claim that truth is an external characteristic of explanation

So far, what I have discussed in this chapter are the arguments on the basis of which Cartwright claims that the fundamental laws are false. Even if the realist chooses not to adopt the facticity view of the fundamental laws, the realist must still respond to Cartwright's second challenge which rejects inference to the best explanation What I will do in the next section is to consider a realist position which seems to reply specifically to this challenge

Generic-Specific Account

A point worth noting before considering this account, however, is that although the anti-realists which I have been considering reject the truth of the fundamental laws on the grounds of rejecting inference to the best explanation, they grant the truth of the phenomenological laws for different reasons Recall that both Van Fraassen and Cartwright reject inference to the best explanation, a form of abduction Van Fraassen's reason unlike Cartwright's rests on an observable/non-observable distinction He writes,

> When the hypothesis is solely about what is observable, the two procedures amount to the same thing For in that case, empirical adequacy coincides with truth [29]

Thus, Van Fraassen does not reject enumerative induction where this is taken to mean, inferring from "observed regularity to universal regularity in the next instance."[30] Induction as a practice in general is not what Van

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[29]Bas Van Fraassen, **The Scientific Image** p.72

[30]Gilbert Harman, *The Inference to the Best Explanation*, **Philosophical Review V.74**, p 88

Fraassen objects to. Rather, he objects to this form of induction when we make conclusions about unobservable entities

Cartwright, on the other hand, believes that the phenomenological laws are true for a different reason She suggests that the crucial difference between the fundamental laws and the phenomenological laws is the role they play in scientific practice The role of the phenomenological laws, unlike the fundamental laws, is to describe the facts,

> I think that the basic laws and equations of our fundamental theories organize and classify our knowledge in an elegant and efficient manner, a manner that allows us to make very precise calculations and predictions The great explanatory and predictive powers of our theories lies in their fundamental laws Nevertheless the content of our scientific knowledge is expressed in the phenomenological laws [31]

It appears from the quotation that the phenomenological laws are true in virtue of the fact that the predictions which are derived from these laws accurately describe the facts When fundamental laws are used, sound derivations are not possible since we must use idealization assumptions for the reasons outlined in Chapter 1 Since accurate predictions cannot be derived by the use of fundamental laws, it would appear that they do not describe the facts, and thus they are not true There is, however, a serious problem with this reason Although predictions derived from phenomenological laws are almost accurate, they are not perfectly accurate As I stated in chapter 1, we must always use idealizations of some form for reasons of inadequate computational abilities If the predictions which are derived from phenomenological laws are not completely accurate, isn't the difference between derivations and phenomenological laws only a matter of degree?

There is perhaps another reason why Cartwright believes that the phenomenological laws are true, and that is because they mirror the causal story of phenomena She writes,

[31] Nancy Cartwright, **How the Laws of Physics Lie** p 100

> First, when we explain a phenomenon, we state its cause We try to provide detailed accounts of exactly how the phenomenon is produced The causal story uses highly specific phenomenological laws which tell what happens in concrete situations But the theoretical laws, like the equation of continuity and Boltzmann's equation, are thoroughly abstract formulae which describe no particular circumstances[32]

Thus, the phenomenological laws mirror the causal story from which we can infer the truth of the explanation and the truth of the phenomenological laws It is interesting that at this point Cartwright suddenly begins discussing Boltzmann's equation and the equation of continuity instead of the gravitational law Some realists may simply claim that these two laws simply aren't fundamental laws of nature for the following reason One characteristic of fundamental laws that both realists and anti-realists agree on is that fundamental laws causally explain why phenomenological laws and events are true This is one property that these two laws don't seem to possess The question then is, can Cartwright use the distinction I cited to distinguish phenomenological laws from our standard example of fundamental laws such as the gravitational law Most of our fundamental laws do tell some causal story of an event, but as Cartwright will be quick to point out, they don't tell the complete causal story The problem for Cartwright is neither do the phenomenological laws The problems she cites for the fundamental laws also exist for the phenomenological laws For the purposes of discussion, let us assume, however, that Cartwright is able to argue that the phenomenological laws are true

It may at first seem counter-intuitive to claim that the phenomenological laws are true, and the fundamental laws are false This is because there is a view that is firmly entrenched in the scientific community which places the priority of the fundamental laws over the phenomenological laws Cartwright labels this the 'priority thesis',

> A long tradition distinguishes fundamental from phenomenological laws, and favors the fundamental Fundamental laws are true in themselves, phenomenological laws hold only on account of more fundamental ones. This view embodies an extreme realism about the fundamental laws of

[32]Ibid p 11

basic explanatory theories Not only are they true (or would be if we had the right ones), but they are, in a sense, more true than the phenomenological laws that they explain.[33]

What I will consider in the rest of this chapter is a particular version of this thesis, the generic-specific account. According to this account, the fundamental laws are true if the laws can be used in sound derivations of phenomenological laws. Prima facie, this seems to respond directly to the anti-realist challenge of what explanation has to do with truth. Very briefly, the realist response is the following. Recall earlier, Cartwright, Duhem, and Van Fraassen grant the truth of the phenomenological laws. But the realist responds by claiming that the fundamental laws are truer than the phenomenological laws since they explain why the phenomenological laws are true. The challenge posed by the anti-realist is to ask what explanation has to do with truth. According to the generic-specific account, the way in which the fundamental laws explain the phenomenological laws is through logical entailment.

Cartwright claims that the primary difference between these two types of laws is that phenomenological laws are merely descriptive but fundamental laws are explanatory. If this is true, why not say that the fundamental laws explain why the phenomenological laws are true? This is the view which, I take, most physicists hold. What does it mean, however, to say that the fundamental laws *explain* the truth of the phenomenological laws? According to the generic specific account, the truth of phenomenological laws are explained through their derivation from fundamental laws. However, the derivation from fundamental laws to phenomenological laws need not represent a causal relation between these two forms of laws. For I do not think that the fundamental laws cause the phenomenological laws to be true in any way similar to the way that my hand causes this ball to move forward. A possible alternative, as Grunbaum suggests, is that the phenomenological laws are merely specific instances of fundamental laws,

[33]Ibid p.100

> It is crucial to realize that while (a more comprehensive law) G entails (a less comprehensive law) L logically, thereby providing an explanation of L, G is not the "cause" of L. More specifically laws are explained not by showing the regularities they affirm to be products of the operation of causes but rather by recognizing their truth to be special cases of more comprehensive truths.[34]

Cartwright calls this view the generic-specific account. In particular circumstances, both laws make the same claim, but the fundamental laws are superior since they are applicable to a wider range of phenomena. The fundamental laws are merely more general, abstract formulations of phenomenological laws. Thus, phenomenological laws can be derived by simply deducing them from a set of fundamental laws and a specification of boundary conditions. Furthermore, the model of explanation for particular actual events consists of two parts, both deductive. The first is a deduction of a phenomenological law from a set of fundamental laws and certain statements of boundary conditions and the second deduction involves the use of that phenomenological law and a statement of initial conditions to derive a statement of an event or phenomena. Whether the explanandum is an event or a phenomenological law should not matter in this debate since all three anti-realists grant that the phenomenological laws are true.

Cartwright's Two Objections

According to the generic-specific account, the truth of the fundamental laws derives from the fact that there exist sound derivations of true phenomenological laws. Cartwright raises two objections to this account. The first objection stems from the fact that theoretical derivations in physics are unsound, and the second objection stems from the fact that there are frequently several alternative derivations for the same phenomenological laws.

The first objection, Cartwright argues, is that the actual practice of physics does not support the claim that the fundamental laws are true because there are not many rigorous derivations for many of the

[34] A. Grunbaum, *Science and Ideology*, **The Scientific Monthly** (July 1954), p. 14.

phenomenological laws in physics and engineering The reason is that most derivations in physics involve the use of idealizing assumptions For example, the derivation of Kepler's laws contains two idealization assumptions The first assumption is that the force the earth exerts on the sun is negligible, and the second is that the force exerted by the other planets is also negligible The fact is that the statement of these idealizations assumptions are false since both of the assumptions requires giving a false description of the world. For example, the first would require assuming that the sun was infinitely massive The derivation of Kepler's laws is unsound since what is used is a false set of boundary conditions According to the D-N model, events get explained through a deduction from a set of true laws and true statements of initial conditions Similarly, this model suggests that laws are confirmed via these derivations The problem, therefore, is simple. If we don't have sound derivations of true phenomenological laws or events, fundamental laws cannot be confirmed on this model of confirmation For example, in the derivation of Kepler's law, the fundamental laws of Newtonian mechanics do not receive confirmation since the derivation is unsound

If one adopts the generic-specific account and the D-N model, explanations of phenomenological laws consist simply of sound derivations where, in these cases, they involve deductions from sets of true fundamental laws and true statements of boundary conditions Events can similarly be deduced from a set of phenomenological laws and a set of true statements of initial conditions Thus, we would expect,

TC1) If a theory or set of laws is true, then applying it (validly) to a true set of initial or boundary conditions must yield a true prediction

The statement of this truth-condition implies that the satisfaction of the consequent of TC1 provides confirmation for the antecedent and similarly if the consequent is not satisfied, it provides disconfirmation for the antecedent We, therefore, have the two following confirmation principles,

P1) If applying a true set of initial or boundary conditions to a set of laws yields a true prediction, then the the set of fundamental laws receives confirmation.

P2) If applying a true set of initial or boundary conditions to a set of laws yields a false prediction, then the set of fundamental laws receives disconfirmation

Thus, the confirmation of the fundamental laws rests on our ability to provide sound derivations of actual events or true phenomenological laws. The problem, recall though, is that these sound derivations do not exist Although, physicists provide derivations of phenomenological laws, the derivations involve the use of initial conditions which are false The reason why they are false is because the statements of initial conditions involves the use of idealization assumptions

The following picture illustrates why this is a problem for the confirmation of the fundamental laws if one adopts the generic-specific account and also accepts a D-N model of explanation On a very simple model where the fundamental laws strictly imply the phenomenological laws without the use of statements of initial conditions, fundamental laws get their confirmation or disconfirmation in the following way,

F → P	F → P
<u>not P</u>	<u>P</u>
not F	F

Note, that in this case all that is required for disconfirmation is a single instance of not P, but confirmation requires that there be many instances of P The relation between the two laws which we are investigating, however, is the one expressed by the generic-specific account. Thus the model of confirmation is more complicated Note that in the following case where the statements of the initial conditions are true, F is disconfirmed if P is false, and F is confirmed if there are many instances of P,

(F & I) → P	(generic-specific account)	(F & I) → P
I		I
<u>not P</u>		<u>P</u>
not F		F

where F represents one or a set of fundamental laws, I represents a set of statements of boundary condition or idealizing assumptions, and P represents a derived phenomenological law. Note, however, that when idealizing assumptions are used (that is, the initial conditions are false), we cannot infer anything concerning the truth of the fundamental laws.

(F & I) → P	(generic-specific account)	(F & I) → P
not I		not I
-P		P
F or not F		F or not F

In both cases where the phenomenological law is either derived or not, the fundamental laws are neither confirmed nor disconfirmed. The problem is that both disjunction statements are consistent with the premises of both arguments. Thus, since we cannot deduce the truth or falsity of the fundamental laws, "the falsity of I protects the theory against refutation".[35] If the use of idealizing assumptions is unavoidable in our derivations, the fundamental laws can not be either confirmed or disconfirmed. Therefore, realists cannot appeal to the actual practice of providing sound derivations of phenomenological laws or events to argue for the truth of the fundamental laws, since these derivations do not exist.

One might think that since the force of the objection rests on the actual practice of physicists not providing sound derivations, TC1 could be relaxed to something like the following,

TC2) If a set of fundamental laws is true then it is possible, *in principle* ,to construct a sound derivation of phenomenological laws and more generally, empirical data.[36]

[35]Ibid. p 148

[36]R. Laymon, *Cartwright and the Lying Laws of Physics*, p 357

The fundamental laws, on this account, only receive confirmation if the theory can, *in principle*, provided idealization free derivations. That is to say, if we had the requisite computational capabilities, we could produce these sound derivations. Thus, the truth of the fundamental laws would depend on our reasons for believing the following counter-factual: if we had the requisite computational abilities, we could produce the appropriate derivations. Without providing any reasons for believing this counter-factual, relaxing the condition for truth of the fundamental laws to being able, in principle, to provide sound derivations does not suffice to save realism. There is another problem even if we could provide some justification for believing in this counter-factual. Recall that in chapter 1, there are two reasons why, under certain circumstances, we must use idealization assumptions. The first is that we do not have the adequate computational abilities and the second is that we do not have the appropriate auxiliary theories.

Cartwright's main reason for believing that we can't provide the appropriate derivations of either events or complex phenomenological laws is that we do not have a unified field theory. For phenomena which require the use of laws from the different specialized areas of physics, we do not know how to combine these laws together. For instance, presently we do not know how to explain events which require considering both general relativity and quantum mechanics (radiation from black holes). In essence, her objection is that we do not have a unified field theory. So in such cases, even if we did have the adequate computational abilities, we still could not provide sound derivations of events.

If we did possess such a theory, it would dispel many of Cartwright's worries about physics. Cartwright acknowledges that if we do discover this theory then the fundamental laws will be shown to be true since we would then be able to provide rigorous derivations of actual events or phenomenological law, thus realism would be saved. Her repeated challenge, however, is what reason do we have to believe that such a theory exists?

rigorous solutions to exact equations might possible reproduce the correct phenomenological laws with no ambiguity 'when the right equations are found'. But the reason for believing in this gloss is not the practice itself, which we have been looking at, but rather the realist metaphysics, which I began by challenging.[37]

We should agree that the end of theoretical physics is in view only when it is clear they have done so. Our knowledge of nature, nature as we best see it, is highly compartmentalized. Why think nature itself is unified?[38]

It is true that there is a rather serious difficulty at this moment in trying to unify general relativity and quantum mechanics because of certain fundamental differences in each of the theories. Furthermore, one would hope to be able to find a defence of realism which does not rest on a claim which we cannot provide any reason to believe. For why should we believe that physics will one day discover a unified field theory?

The second objection Cartwright raises for the generic-specific account concerns the fact that we are able to offer alternative theoretical treatments of the same phenomena. It was noted earlier in this chapter that there are alternative derivations for the exponential decay law. The two derivations which I described were the Weisskopf-Wigner treatment and the Markov treatment. We shall consider only the Weisskopf-Wigner treatment since Cartwright's argument works for both treatments. Recall that in this treatment, the derivation involved three approximations,

1) the rotating wave approximation,
2) the replacement of a sum by an integral over the modes of the electromagnetic field and the factoring out of terms tha vary slowly in the frequency,
3) factoring out a slowly-varying term from the time integral and extending the limit on the integral to infinity

Cartwright notes that we can justify assumptions 2) and 3) by appealing to the physical characteristics of the atom-field pair. The details are unimportant except for the following point. In the justification provided for the approximations or idealizations, there is no reference made to the order in which the approximations must be performed. She writes,

[37]N. Cartwright, **How the Laws of Physics Lie** p.126-127

[38]Ibid p.13

> The second approximation is reasonable because the modes of the field are supposed to form a near continuum that is there is a very large number of very closely spaced modes This allows us to replace the sum by an integral The integral is over a product of the coupling constant as a function of the frequency, ω and a term of the form $\exp(-i\omega t)$ The coupling constant depends on the interaction potential for the atom and the field and it is supposed to be relatively constant in ω compared to the rapidly oscillating exponential Hence it can be factored outside the integral with little loss of accuracy The third approximation is similarly justified by the circumstances

In Bethe's original paper, he performed the derivation using the second approximation first and then the third approximation What physicists discovered later is that *the order in which the approximations are used is important* If the third approximation is performed before the second, the derivation predicts that there is a lamb shift which accords with the data However, if the order of approximations is reversed as originally done by Bethe, the derivation does not predict a lamb shift To summarize, what this example demonstrates is that two derivations which involve the same idealization assumptions result in two different equations, one which predicts a lamb shift and another which doesn't The reason is that the order in which the idealization assumptions are used affects what terms will be lost

An important point to note about this example is that it is not an argument from underdetermination Rather, it is an example of the construction of two models *where there is a qualitative difference*, the prediction of the Lamb shift So the data supports the derivation where the third approximation is performed before the second approximation The point of her example is to take issue with the generic-specific account She writes,

> Choices must be made which are not dictated by the facts I have already mentioned that is so with the choice of models But it is also the case with approximation procedures, the choice is constrained, but not dictated by the facts, and different choices give different incompatible results The generic-specific account fails because the content of the phenomenological

> laws we derive is not constrained in the fundamental laws which explain them.[39]

The argument then is that given only the knowledge of our fundamental laws, there is no reason to choose one approximation procedure over another. There is no theoretical reason for why we should choose one approximation procedure over the other.

What we have seen in this chapter is that if we want to be realists, we are going to have to alter the D-N model of confirmation and the hypothetico-deductive method of confirmation. In the next chapter, I want to consider a possible realist response by Ronald Laymon. He writes,

> My basic contention is that these arguments, though, highly suggestive, fail to take into account the relevance of the piecemeal improvability of idealizations and approximations and the corresponding improvements in predictive output.[40]

Thus, I will consider an alternative model of confirmation which takes into account the use of idealizations and approximations. What I will do in the next chapter is examine this proposal in detail and consider some difficulties which Laymon raises himself. I will also consider his response to Cartwright's second challenge in which he offers a methodology for choosing one set of approximation procedures or sets of idealization assumptions over another.

Chapter 3

The crucial point to the first objection posed in Chapter 2 is that the actual practice of physics (inability to provide sound derivations) does not support the claim that the fundamental laws are true. As I mentioned earlier, there are two reasons why we can not provide the derivations which would support the realist claim. The first is that we do not have the

[39]Ibid. p 107

[40]Ronald Laymon, *Cartwright and the Lying Laws of Physics*, p 353

requisite computational abilities, and the second is that even if we did, we don't have the appropriate fundamental laws that would provide sound deductive-nomological explanations. The response I will now consider, focuses on the problem of not possessing the adequate computational abilities. What I will do is consider an alternative model of confirmation which takes into account the use of idealizations and approximations. Having done this, I will consider the ways in which the anti-realists I have been considering would respond. More particularly, I want to raise the problems with this account by considering how I think Van Fraassen and Cartwright would respond, and then offer a problem which I find with the account.

Laymon's Proposal

In view of the fact that actual practice does not support realism. Laymon suggests that we adopt a weaker notion of the generic-specific account. Note that what is meant by actual practice is the fact that there are not many rigorous derivations of phenomenological laws or actual events. Laymon, therefore, suggests that we adopt a normative version of the generic-specific account which is,

(GSn) The goal of science should be to seek fundamental laws which are true, and can be used in the sound derivation of phenomenological laws.[41]

Thus, a realist would be one who believes that,

1) scientific practice can be appealed to in confirmational considerations and, in particular, that attempts to derive phenomenological laws (and more generally data) play a role in confirmational considerations,

2) the normative version of the generic-specific account is true.

The discussion has shifted slightly. Although realism about fundamental laws is still at issue, Laymon has also brought in realism about science itself. To be a realist about science is to believe the normative version of the generic-specific account. Clearly, however, one could not believe that there

[41] R. Laymon, *Cartwright and How the Laws of Physics Lie*, p 355

were no fundamental laws that were true and still believe that the goal of science should be to seek fundamental laws

Note that if the normative version of the generic-specific account is true, sound derivations of phenomenological laws are no longer required for the confirmation or disconfirmation of the fundamental laws. Sound derivations are taken to be a normative ideal which we may never achieve. The suggestion is that attempts to achieve sound derivations with a particular history are good reasons to believe that there are fundamental laws that are true. He suggests that,

TC3) If a set of fundamental laws is true, then we can make in principle sufficient corrections so as to yield better predictions.[42]

One salient feature of this statement of truth-conditions is that it accounts for the fact that idealizations and approximations are used in actual derivations of physics. Laymon's proposal also accounts for the fact that if either correction factors or more realistic initial conditions are used, we usually obtain more accurate predictions. The fact that our theories allow us to make these corrections which yield better predictions is supposed to be a reliable indicator of the truth of the laws. He writes,

> The relevance of the existence of such improvability is that it is inductive evidence in favor of the satisfaction of the consequent of R2[TC3], a necessary condition for truth. Similarly, failure to generate improved predictions on the bases of improved idealizations and approximations (as input to theory) is evidence against the satisfaction of the consequent of R2, and hence for the falsity of the theory in question.[43]

TC3, therefore, implies the two following confirmation principles,

C1) A set of fundamental laws receives confirmation if the use of more realistic specifications of initial or boundary conditions in fact leads to more accurate predictions.

[42] Ibid. p.359

[43] Ibid. p.359

(C2) A set of fundamental laws receives disconfirmation if the use of more realistic specifications of initial or boundary conditions does not lead to more accurate predictions.[44]

These two principles would imply that the fundamental laws must be 'monotonic towards the truth. This simply means that the use of more realistic statements of initial conditions will yield more accurate predictions and the use of less accurate statements of initial conditions will yield worse predictions. According to Laymon, if there is evidence for the monotonicity of a set of fundamental laws, then we can infer that the laws are true. To repeat, the advantage of this account is that actually providing sound derivations is not the only form of actual practice which can be used to confirm our fundamental laws. Cases in which we are able to get predictions closer and closer to the actual truth can also play a confirmational role. Thus, these two principles imply that we should be realists about science. Laymon writes,

> given the confirmation theses C1 and C2, it follows immediately that we should aim for increasingly more accurate derivations of phenomenological laws and individual predictions, since in that we confirm and disconfirm our law candidates.[45]

Laymon goes on to cite several cases in the history of physics which support his proposal. Lorentz, for instance, showed that more realistic descriptions applied to the aether theory produced worse predictions. The equipartition theory also yielded worse results as more accurate initial conditions were used. Both of these facts demonstrated to physicists that both theories must be false. Thus Laymon's proposal is supported by the actual practice of first using idealizations and approximations and then using correction factors or more realistic statements of boundary conditions to yield better predictions. The interesting point is that although realism is not supported by the actual practice of producing sound derivations (since the derivations do not exist) it is supported by the practice of producing derivations which

[44] Ibid. p.359-360

[45] Ibid. p.360

give results that are closer and closer to the truth. The proposal, then, is that we modify the D-N model of explanation and replace it with a model of explanation where a deduction which gives an approximate value is considered acceptable if we are able to produce further deductions with more realistic statements of boundary conditions that lead to more accurate predictions.

Some Objections

I would like to now consider a few objections to this proposal. My purpose in raising these objections should not be mistaken as an argument for anti-realism. The aim in offering these criticisms is to clarify what is at issue in the realism/anti-realism debate in the hope of clarifying how this dispute can be settled. Recall that the central claim to Laymon's proposal is that,

> If a set of fundamental laws is true, then we can make in principle sufficient corrections so as to yield better predictions.[46]

Thus, one can think of this realist response in the following way. Here is a piece of scientific practice that requires explaining. We have this regularity which is: if we use more realistic initial conditions, we get better predictions. The only thing that can seem to explain this fact is a true theory. I would like to now consider how the different anti-realists would respond to this proposal and offer a problem which I, myself, found with the proposal.

Van Fraassen and Duhem: The Strong Anti-Realist Response

Recall that Laymon's two confirmational principles state that a set of fundamental laws receive confirmation if the use of more realistic statements of initial conditions leads to more accurate predictions, and similarly, the laws receive disconfirmation if the use of more realistic specification of initial conditions leads to less accurate predictions. Hence, a set of laws will be highly confirmed if there are many instances of more

[46] Ibid. p. 359

accurate predictions when more realistic initial conditions are used This is right, but Laymon infers from the fact that the fundamental laws are highly confirmed to the claim that they are true The argument, therefore, is an inference to the best explanation But this is precisely what the strong anti-realist (Van Fraassen and Duhem) rejected in the first place What I will mean by strong anti-realist is the position that rejects inference to the best explanation tout court and weak anti-realism is the position that only rejects certain forms of inference to the best explanation Thus Cartwright is a weak anti-realist since she advocates inference to the best causal explanation

One could imagine Van Fraassen responding to Laymon's suggestion by simply modifying TC3 to

> If a set of laws are empirically adequate, then we can make in principle sufficient corrections so as to yield better predictions

Empirical adequacy, as normally understood, only requires that the theory be able to save the observable phenomena This means, in this case, that there is an additional constraint A theory is only empirically adequate when the use of more realistic statements of initial conditions leads to more accurate predictions We, thus, preserve Laymon's two confirmational principles unchanged In essence, this anti-realist response amounts to just denying the use of inference to the best explanation That is to say, what we infer from the fact that a theory is the best explanation is that it is the best explanation and not that it is true Or in Van Fraassen's terms, theories that satisfy the two confirmational principles are empirically adequate and do not have to be true

Empirical adequacy, of course, doesn't require that the theory produce accurate derivations Constraints as to what counts as an acceptable explanation are internal to the theory The first constraint is that the theory must provide some model in which the predictions of the theory are isomorphic to the data. Thus, an acceptable theory may contain derivations which give only approximate predictions as opposed to accurate ones The acceptable error within which the derivation can fall is relative to the theory Thus, if a theory does not provide predictions which are the same as the actual results, it does not mean that the theory is not empirically adequate. For it could be that the theory requires only that predictions fall within a given error where the error factor is defined within the theory. For example, our present theory is considered empirically adequate even though it does not provide perfectly accurate predictions. These explanations are acceptable since they fall within a certain error. The error which the predictive results can fall within is itself derived within the theory.

Note, that with this response, the anti-realist does not really give a response to the realist's challenge, which is to explain why when we use more realistic initial conditions do we get better predictions? Recall, that part of the anti-realist response was the addition of another constraint, namely, that if we accept a theory, it is required that the use of statements of more realistic initial conditions will lead to better predictions. Thus, in some sense, the strong anti-realist responds by denying that there is a fact that needs explaining, and uses the constraint as a virtue for choosing one theory over another. Therefore, the fact that we get better predictions given more realistic initial conditions is considered as a pragmatic virtue similar to simplicity and explanatory power.

The problem is that this condition is similar to pragmatic virtues like simplicity and explanatory power, however, it is different in an important respect. Van Fraassen writes in his discussion of the pragmatic virtues,

> In so far as they go beyond consistency, empirical adequacy, and empirical strength, they do not concern the relation between the theory and the world, but rather the use and usefulness of the theory, they provide reasons to prefer the theory independently of questions of truth.[47]

Note that unlike the other pragmatic virtues, the constraint of more realistic initial conditions leading to more accurate predictions *does concern the relation between the theory and world*. The theory plays an intermediary role for the inputs (the statements of initial conditions) which we obtain from the world and the results (predictions) that we obtain which are to be compared with the real world. So it seems that Laymon's

[47] Bas Van Fraassen, **The Scientific Image**, p. 88

condition does not fall under the category of pragmatic virtues Moreover, this piece of evidence or condition does not fall under what van Fraassen means by phenomena either since this condition is not a claim only about predictions, but rather about what happens *if we use more realistic initial conditions* It seems that Van Fraassen's response to why it is rational to consider simplicity and explanatory power as pragmatic virtues cannot be applied to the constraint discussed above, nor can he say that it is a phenomena like any other which is to be saved

Clearly, the strong anti-realist must either offer some rational justification for considering more realistic initial conditions which lead to more accurate predictions as a virtue or else respond to the challenge In other words, the anti-realist must explain why successful theories are those that when more realistic initial conditions are used, more accurate predictions are obtained The evidence cited by Laymon seems to support the side of the realist since, in the past, those theories which we have considered to be successful are those that satisfy this condition There are phenomena which cry out to be explained, and this seems to be one of them So I do not think that the anti-realist can just claim that a good theory must be one that satisfies the condition that when more realistic initial conditions are used, we will obtain more accurate predictions without offering any justification

Furthermore, if the anti-realist does not choose to answer the challenge, it seems to leave the realism/anti-realism dispute at a stand-off. By this, I mean that I believe, like Cartwright, that the debate over realism about the fundamental laws is an empirical issue Thus, what will decide which position is correct will be scientific practice itself The idea is that observations about scientific practice act as evidence for whether we should be realists or anti-realists This is an important point since it forces the anti-realist to provide some explanation as to why we get better predictions when we use more realistic initial conditions More generally, this means that any position, realist or anti-realist, is held accountable to scientific practice If the anti-realist, however, denies this, it is hard to see how the debate can be settled at all since we would only be left with a priori arguments, and I cannot imagine what such an argument would look like

Thus, it seems that either the anti-realist must allow that there is something to be explained or else be committed to claiming that the realism/anti-realism debate is no longer an issue really worth discussing. These alternative conclusions, however, rest on the assumption that the realism/anti-realism debate is an empirical issue, and Van Fraassen may reject this assumption. For recall that the challenge raised by Van Fraassen was to ask what truth has to do with explanation. How to respond to this challenge clearly goes beyond the bounds of this thesis, but one moral that could be drawn from this is that if one wants to respond to Van Fraassen and Duhem, one should not look at scientific practice for any support.

Cartwright's Response

The second point that I want to make concerns the truth of TC3. In essence, Laymon's claim is that monontonic behaviour of fundamental laws, i.e. successive cases of more realistic statements of initial conditions which lead to more accurate predictions, is a reliable indicate their truth. Before proceeding onto the second objection, we should first be clear as to what is meant by 'a reliable indicator'. The use of 'reliable' is important because Laymon is willing to grant that there may be cases where we have a true theory yet the use of more realistic statements of initial conditions leads to worse predictions. He states,

> What this means at the least is that C1 and C2 must be interpreted as being in some sense prima facie inductive confirmation principles. In particular, the computational specifics (how the theory is computationally implemented) may override the prima facie confirmational value of existing samples of improvement or nonimprovement.[48]

Thus the confirmational principles provide good evidence, but not decisive evidence for the truth of the fundamental laws. Since it is only a reliable indicator, Laymon does not expect that for every case where we have a true set of laws, the use of more realistic statements of initial conditions will always yield better predictions. In these cases, one should seek

[48]Ibid. p 360-361

explanations as to why the confirmational principles were not satisfied. If, an explanation cannot be found, then we would have a counter-example to Laymon's proposal.

Let us consider an actual case in physics where Laymon's confirmational principles appear not to hold. As I stated earlier, Cartwright unwittingly provides an apparent counter-example to Laymon's proposal. The exponential decay law in quantum mechanics has received a wealth of empirical support. That is to say, the law yields predictions which accord with actual results. The interesting fact about this law is that it is accurately derived from the fundamental laws of quantum mechanics *even with the use of idealizations and approximations*. In the Weisskopf-Wigner derivation of the exponential, the assumptions involve 1) assuming that the only significant coupling is between the excited and the de-excited states and 2) neglecting terms which are slowing varying in omega. The result is odd since what we would expect is that a derivation from a true set of fundamental laws and a statement of idealized initial conditions would yield a result that would not be exact but only close to the actual result. What is even more interesting is that if more realistic statements of initial conditions are used, the derivation yields results which *detract* from the actual results. Thus, in this case, if we use more realistic statements of initial conditions, we derive results which get farther and farther away from the actual results we observe. Thus, it is a case where,

> 1) no empirically determined corrections are required if appropriate approximations and idealizations are used, and
>
> 2) improvements in the realism of the analysis will lead to divergence from the desired prediction.[49]

No empirically determined corrections are necessary, of course, since the derived result is the same as the actual result. It appears that this example may be a problem for Laymon since what we have is a case in which the use of more accurate statements of initial conditions leads to worse predictive results. Thus C1 would not be satisfied.

[49] Ronald Laymon, Cartwright and the Lying Laws of Physics, p 364

There is a problem with the use of this example since the evidence available to us now provides as much confirmation for the rigorous derivation(i e the derivation which does not use idealization assumptions) as much as it does for the exponential decay law The additional terms that are included in a rigorous derivation are very small and only concern the rate of decay for very large times Thus, predictions for the derived law and the exponential decay law are the same for short time periods but diverge for very large times As Cartwright notes, there have been many experiments concerning the exponential law, but none of these experiments have given any results concerning the rate of decay for very large times Thus at the moment, there is no evidence that favors either the rigorous derivation or the phenomenological law (the exponential decay law) Thus, it could be that the phenomenological law is false if we could obtain data about radioactive decay over long periods of time However, I will assume, as Laymon does, that the phenomenological is not false and see what problems arise

There are several conclusions that one can adopt concerning this example. The first is that it should be treated as an anomaly Treated as such, we should look for explanations as to why we did not get better predictions. For example, we should be to looking to see whether there is something like a fortuitous cancellation of idealizing assumptions Thus, in cases when accurate results are obtained even when idealization assumptions are used, the first thing that one should look for is a cancellation of idealization assumptions If, however, such an explanation cannot be found, then it would appear that we have a counter-example to Laymon's principle There are, however, two other conclusions that one can adopt from this example, reject quantum mechanics or reject realism

Laymon's response to the example is that what it shows is not that realism is false, but rather that quantum mechanics is false He writes,

> If decay could be determined to be truly exponential, and if my first interpretations of Cartwright were the correct way to view the case (i e , all improvements will lead to a worsening of predictions), this would be at best only sufficient to refute quantum mechanics I do not see how this refutation would affect realism

If we have good reason to believe that the idealization assumptions are ineliminable, the example does provide evidence for the claim that quantum mechanics is false. However, I find it rather odd that Laymon claims that the example has no implications for realism. The exponential decay law provides an example where more more accurate statements of initial conditions yields worse results. Thus it appears that we have a counter-example which shows that Laymon's proposal is false. If his proposal is false, then this argument cannot stand as a defence of realism. One might respond by saying that we have alot of other evidence for realism. This may be true, but we also have alot of confirming evidence for quantum mechanics also.

Furthermore, recall that what is in dispute is realism about the fundamental laws. The original challenge was to show how the actual practice of physics can provide evidence for the claim that the fundamental laws are true. As Cartwright pointed out, sound derivations cannot provide evidence for the truth of the fundamental laws since they do not exist. Laymon then suggested that we consider another form of actual practice as confirming evidence for the fundamental laws (the practice of producing better predictions when more realistic statements of initial conditions are used). Thus, Laymon's whole defence of realism rests on the claim that better predictions due to more realistic statements of initial conditions is a reliable indicator for the truth of the laws. If, as Laymon claims, actual counter-instances do not serve to refute realism or at least his defence of realism, then I do not see how he can use positive instances of his principles such as the aether example and the equipartition example to affirm his defence of realism. It could be that Laymon does recognize this point since several paragraphs later, he retracts his claim that it does not affect realism,

> The idea here is that the existence of such a set means that the presence or absence of actual monotonic behaviour is not a reliable indicator of long range or in-principle behaviour. Although this approach saves quantum mechanics from refutation, it has nasty consequences for realism.

Thus, it seems that if no explanation can be found as to why an accurate result was derived even with the use of idealization assumptions, then either we must reject realism or conclude that quantum mechanics is false.

A form of evidence which would point to which conclusion we should adopt is the following. If we had several of these cases only within quantum mechanics, but did not have instances in other areas of physics, then I believe we would have good reason to reject the fundamental laws of quantum mechanics. However, if we have many of these cases, in all of the other areas of physics, then one may have reason to reject realism. The reason is that an abundance of these cases only within quantum mechanics would show that there is something peculiar about quantum mechanics. Thus, it would seem that more accurate statements of initial conditions which lead to more accurate predictions would still be a reliable indicator since in all of the other specialized areas of physics Laymon's principle still holds. However, if we obtained cases which appeared in all of the areas of physics, it would appear that the only conclusion open would be to reject Laymon's principle.

As it stands, the exponential decay law is not a counter-example since we do not possess the empirical evidence which would show whether the exponential decay law is correct. For this example to count as a counter-example, we will have to wait for physics to produce an experiment which shows what happens to decay over very large period of times.

Laymon's Own Objection

A peculiar point about Laymon's paper, in which he proposes his two confirmational principles as a defence of realism, is that at some points, he writes as if he has succeeded in defending realism However, as he himself points out there are several serious difficulties for his own proposal I pointed out earlier that the anti-realist must explain why we accept theories which when we use more realistic initial conditions, we get better predictions When I suggested that this would pose serious difficulties for Van Fraassen, I assumed that a true theory can explain why we get better predictions when more realistic initial conditions are used At the outset of his paper, Laymon assumed that a true theory does explain why we get better predictions when we use more realistic initial conditions without really offering any explanation, however, later on, he realizes that such an explanation is not so simple

According to Laymon, many instances of using more realistic initial conditions which lead to better predictions is inductive evidence for the truth of the fundamental theories What is the justification for this conclusion? For it is not obvious Laymon writes,

> My conclusion then is that the principles C1 and C2 along with R2 are sufficient for establishing realism (as defined above) The stronger R1 is not necessary There does seem, however to be some connection between R1 and R2 I claimed above that C1 and C2 derive their plausibility from R2 But, if now ask what justifies R2, we run into some difficult questions One possible realist strategy would be to claim that there exists some continuity condition such that R1 and R2 are equivalent given that condition That is, that theories should be continuous in the sense that monotonic improvability yields true output in the limit [50]

Laymon's idea, then, is that the presence of monotonic behaviour of the laws provides inductive evidence that there exists some in principle derivation which is sound That is, there exists a derivation in which the statements of initial conditions is true, and the prediction is accurate Why should we think that this monotonic behaviour should justify this

[50]Ronald Laymon, *Cartwright and the Lying Laws of Physics*, p 362

conclusion? Laymon's response, as can be seen in the above quotation, is that there might exist some continuity condition which would justify the induction. As Laymon points out, this is where the realist and anti-realist positions clash. Laymon himself does not offer a defence of this form of induction in the form of explicating what this continuity condition might look like. But presumably, it would involve investigating the nature of laws and why in fact they are monotonic. Such an investigation goes beyond the bounds of this thesis, but what is important to point out is that until such an explication is provided, Laymon's condition does not stand as a defence of realism.

Another Objection

Another important point worth noting about Laymon's suggestion is that it is unclear whether we can make any sense out of what it means for one set of idealization assumptions to be more realistic than another. Laymon recognizes this, and tries to offer an account of deciding which is more realistic than the other. He writes,

> Let T be the law or conjunction of laws whose truth we wish to ascertain. And assume that I1 and I2 are two competing combinations of idealization and approximation whose relative realism cannot be determined according to background standards in place. Assume, finally, that T & I1 → P1 and T & I2 → P2, and the experiment reveals that prediction P2 is more accurate than P1. In such a case, we would (as shown by Cartwright's own cases) claim (perhaps only tentatively) that I2 is, for the situation considered, call it s, the more accurate description.[51]

Laymon then goes on to claim that we should expect that the set of statements of idealizations will yield better predictions under similar but different circumstances than s. He writes,

> More accurately stated, those features of I2 which carry over to the new calculation should ceteris paribus remain relatively more realistic than the corresponding features of I2 which are carried over to the new cases. So,

[51]Ronald Laymon, *Cartwright and the Lying Laws of Physics*, p 369

> new calculations based on I 2 should continue to produce better results than ones based on I1.[52]

There is clearly an intuitive appeal to Laymon's suggestion in that if a set of statements of initial conditions is more realistic than another then we would expect the former to yield better results when applied under different circumstances. For example, when physical theory began taking into account the inner structure of the atom, more accurate results were obtained in *all* circumstances than in those cases when the derivations did not take into account the inner structure. This, however, is a simple case, and I imagine that most decisions concerning which set of statements of idealization assumptions are more realistic will not be so simple.

In cases where galilean idealizations assumptions are involved, we could have two cases. Suppose that we have two sets of idealization assumptions I1 and I2, then we could have the simple case where one of the sets is a subset of the other. If I1 is a subset of I2, then clearly I1 is the more realistic set of idealizations since I2 involves abstracting away more causal properties then I1. Thus, the derivation which involves the use of I1 will be more accurate since it involves a more accurate description of the initial conditions. However, can we make sense of saying that one set of assumptions is more realistic then the other if neither is a subset of the other? Suppose that we do have two sets of idealization assumptions, and then proceed to try to give explanations of an event via the construction of models which use these idealization assumptions. Cartwright writes,

> In physics, it is usual to give alternative theoretical treatments of the same phenomenon. We construct different models for different purposes with different assumptions to describe them. Which is the right model, which the 'true' set of equations? The question is mistake. One model brings out some aspects of the phenomenon, a different model brings out others. Some equations give a rougher estimate for a quantity of interest, but are easier to solve. No single model serves all purposes best [p 11]

Cartwright's point is that we will use different models to explain different aspects of the same event. Thus, we will use different idealization

[52] Ibid p 369

assumptions depending on which aspect of an event we are trying to explain. That is, if we are trying to explain property X and we have a choice of two sets of idealization assumptions, it could be that the derivation which involves the idealization assumptions, I1, gives us abetter result then the derivation which involves the idealizations, I2 However, if we want to investigate property Y, then the derivation which involves the idealization assumptions, I2, may give us a better result than the derivation which involves the idealization assumptions,I1 How can this happen?

If the idealization assumptions that we have employed are galilean or more specifically causal idealizations, then I1 and I2 involve ignoring causal properties of an event The case which causes problems for Laymon's suggestion is the following Suppose that two sets of idealizations differ simply by a single idealization assumption More formally, let I1= I0 + S1 and I2= I0 + S2, where each S1 and S2 amount to abstractions of causal lines In such a case, we would not want to say that one set of idealization assumptions is more realistic than the other, even though one gave a better prediction than the other for a particular phenomenon Claiming that one set of idealization assumptions is more realistic than the other would amount to claiming that abstracting away one causal line is more realistic than abstracting away another This would be absurd An important point worth noting about this objection is that it is one that Cartwright may find appealing since the objection only makes reference to the existence of causal lines which she grants exist

Consider, for example, models which are used in electronics to calculate the gain of amplifiers Depending on whether one wants to investigate the small signal properties or the large signal properties of the amplifier, electrical engineers will use different models The difference in models is, in part, due to the difference in what idealization assumptions are used. It is, however, possible to use either of the models to measure both small and large signal properties The problem is that the model which is useful for small signal properties will give very poor predictions for large signals, and the the model useful for large signal properties will give very poor results for small signals Thus, it seems we have a counter example to Laymon's proposal in deciding which statements of initial conditions are more realistic than another. The point of the example, however, is not merely to provide a counter-example to Laymon's methodology of determining which set of initial conditions are more realistic than another, but to point out that in some cases, it does not make sense to claim that one is more realistic than another. The point is metaphysical and not epistemological. If I am right, then there will be no way of devising a general methodology for determining which set of initial conditions is more realistic since neither is. The more general problem that this poses for Laymon or the realist is that Laymon's confirmational principles cannot be used as a defence of realism if we cannot determine which set of initial conditions are more realistic. Now, it is true that we can determine which are more realistic for *some* cases, but what will determine the usefulness of Laymon's project will be those cases which the anti-realist uses against the realist. In other words, in cases where physics provides alternative theoretical derivations of the same phenomena, are the idealization assumptions associated with each of the models related in the way I described above? The amplifier example showed that there exists at least one example of this in physics. Whether other derivations are similar to this case will have to be determined.

What I have tried to do in this chapter is to consider an alternative model of confirmation which takes into account the use of idealizations and approximations, and also consider some possible problems for the proposal. In the first section where I considered Van Fraassen's response, I questioned whether the anti-realist can explain why when we use more realistic initial conditions, do we get better predictions. Here, I pointed out that this condition is unlike the pragmatic virtues which Van Fraassen considers, and I also pointed out that it is a piece of scientific practice that the anti-realist must explain. The anti-realist cannot simply respond by claiming that there is nothing to be explained, since, as Laymon has noted, in the history of physics we have rejected theories on the basis that more realistic initial conditions has led to worse predictions. Moreover, we consider it a virtue of a theory if it does yield better predictions when more realistic initial conditions are use. There are, however, as I have noted other ways in which an anti-realist could respond. The first is to come up with an actual case in which the use of more realistic initial conditions will lead to worse predictions which is perhaps what is occurring in the case of the exponential decay law We will have to wait until physics can provide evidence concerning the decay of an atom over long periods of time to see whether it actually is a counter-example to Laymon's proposal Such cases, in the end, will decide whether Laymon's proposal is a reliable indicator for the truth of the fundamental laws A perhaps more serious difficulty, however, with Laymon's proposal is to provide a general account of how to determine the ranking of statements of initial conditions Furthermore, even if such an account can be offered, it is unclear whether a true theory or realism can explain why it is that when we use more realistic initial conditions, we get better predictions

Chapter 4

Some Concluding Remarks

In the previous three chapters, I have considered several arguments for the claim that the fundamental laws are false I did this by considering two anti-realist positions, Van Fraassen's and Cartwright's in chapters 1 and 2 In chapter 3, I considered a possible realist response which focussed on a particular piece of scientific practice What was considered was that it appears that we accept theories which yield better predictions when more realistic initial conditions are used Thus, the focus of discussion has been whether the use of idealization assumptions can be used in a defence of realism At the end of chapter 3, I offered several criticisms of Laymon's proposal which may lead one to believe that realist position is hopeless What I would like to make clear in this chapter is where these positions, realism and anti-realism, stand in light of the arguments which take into account the use of idealization assumptions

In considering this debate, I examined two anti-realist positions, one briefly, Van Fraassen's, and the other, in more detail, Cartwright's In proposing these two positions, I considered several arguments for the claim that the fundamental laws are false It is important to note the logic of the debate which I considered in chapter 2 The purpose of chapter 2 was to try to establish the claim that fundamentals are false in Cartwright's case or do not need to be true in the case of Van Fraassen, and from there I tried to consider ways in which the realist could respond What I would like to do now is summarize each of the positions individually and investigate whether the use of idealizations can be used in a defence of realism

The primary argument I considered from Van Fraassen was the rejection of one of the realists' central tenets, inference to the best explanation If the aim is to argue for anti-realism, the simple rejection of inference to the best explanation is not enough The anti-realist must also show how the whole scientific project is rational yet still maintain that theories or the fundamental laws need not be true This is Van Fraassen's aim in **The Scientific Image** Van Fraassen begins by claiming that the primary aim of science is to 'save the phenomena' Recall that in Van Fraassen's attack on inference to the best explanation, he claims that the phenomena underdetermines the theory In other words, it is, in principle, possible to construct two empirically equivalent yet incompatible theories Ironically, his argument against inference to the best explanation quickly raises a problem for his own position The problem is that if it is possible to construct more than one theory which 'saves the phenomena', why is it that we settle on one theory? Van Fraassen's response is to claim that the reasons why we choose theories that are simpler and have more organizing power are pragmatic He proceeds by arguing that although it is only for pragmatic reasons that we choose theories that are simpler, it is still rational to do so

There are several ways in which realists have attacked van Fraassen's position One way is to try to defend inference to the best explanation As I stated earlier in the discussion on inference to the best explanation, van Fraassen's rejection of this principle rests on an observable/unobservable distinction Many realists have responded by claiming that such a distinction is untenable, and thus there is no real difference in using the inference in either case Another way of responding to van Fraassen is to attack his claim that it is, in principle, possible to construct more than one theory which 'saves the phenomena' such that the two are incompatible As I noted earlier, it is questionable whether in fact this can be done since van Fraassen never provides any general argument that we can construct two incompatible theories which 'save the phenomena' Another possible way of responding to van Fraassen is to not try to defend inference to the best explanation, but rather to argue that under the anti-realist's construal of what the aim of science is, the whole or parts of scientific practice is irrational The response which I considered from Laymon falls under this category

I noted, earlier, that for van Fraassen, the aim of science is to construct a theory which 'saves the phenomena' rather than discovering a theory which is true The response by Laymon which I considered in chapter 3 can be viewed as citing a piece of scientific practice which must be explained The laws of our present theory seem to satisfy the following condition when we use more realistic initial conditions, we get better predictions Moreover, in the past, we have rejected theories because they have not satisfied this condition Thus, it seems that the anti-realist must offer some explanation The problem as I noted in chapter 3 was that van Fraassen cannot consider this condition as a pragmatic virtue nor can he consider it as a phenomena like other phenomena such as the trajectory of a planet If the anti-realist cannot provide some rational justification for why we accept theories on this basis, where does this leave realism?

To answer this question, it is important to recall one of the final criticisms I raised concerning Laymon's proposal Here I claimed that it was unclear whether a true theory actually explains why, when we use more realistic initial conditions, we get better predictions So it is unclear, whether the realist like the anti-realist can explain why we get better predictions when we use more realistic initial conditions If such an account can be offered through something like a continuity condition, then realism seems to be a tenable position at least with respect to van Fraassen, since there seem to be several ways of defending inference to the best explanation, and furthermore the realist can offer a piece of scientific practice which the anti-realist cannot explain

Van Fraassen's form of anti-realism, however, is not the only form of anti-realism that the realist must deal with Although I suggested that Cartwright's argument for anti-realism was similar to van Fraassen's, there are important respects with which they differ. First, Cartwright's rejection of inference to the best explanation does not rest on an observable/unobservable distinction, and second Cartwright does not claim that it is, in principle, possible to construct more than one theory which 'saves the phenomena'. Her rejection of the principle rests on a theoretical/non-theoretical distinction, and it is in the case of the theoretical explanations that we are unwarranted to infer truth. Unlike van Fraassen who claims that it is, *in principle*, possible to construct incompatible explanations, Cartwright appeals to scientific practice and claims that we actually provide alternative theoretical explanations for the same phenomena. Note, however, that in describing Cartwright I used 'alternative', and for van Fraassen I used 'incompatible'. So Cartwright is not even claiming that the alternative derivations are incompatible. In fact, she would not claim that they are, since she admits that it is possible to construct a more general derivation which would capture the alternative derivations.

Since Cartwright's argument against inference to the best explanation differs in this way from van Fraassen's, arguments which attempt to defend this principle against van Fraassen's challenge such as those provided by Churchland[53] and Glymour[54] cannot be used against Cartwright. We should first summarize and make clear what exactly Cartwright's argument is against inference to the best explanation. Recall that Cartwright claims that in physics we tolerate alternative theoretical explanations, but we do not tolerate alternative causal explanations. Thus, she claims we are warranted to conclude the truth of causal explanations, but not theoretical explanations. There are two important ways in which this argument differs from van Fraassen's. The first is that Cartwright is not only claiming that it is, in principle, possible to construct alternative explanations but rather that we actually do construct alternative

[53]Paul Churchland, *The Ontological Status of Observables: In Praise of the Superempirical Virtues*, **Images of Science, Essays on Realism and Empiricism**, pp 35-47

[54]Clark Glymour, Explanation and Realism, **Images of Science, Essays on Realism and Empiricism**, pp 99-117

explanations of the same phenomena. The second is that Cartwright does not claim that these explanations are incompatible. The first difference is important because the realist cannot appeal to the arguments used against van Fraassen concerning whether we can, in principle, come up with incompatible theories or explanations for the same phenomena. The second difference is important because since the explanations are not incompatible, this does not imply that there is no true explanation. How then is Cartwright's anti-realist conclusion justified?

Recall that Cartwright never explicitly expresses what she means by "alternative theoretical models", but instead she offers an example concerning the exponential decay law. On the most plausible interpretation of this example, the alternative theoretical models do not actually turn out to be incompatible. "Alternative theoretical models" should be understood as alternative models which differ according to what idealization assumptions are used in the construction of the models. On this construal of what is meant by "alternative theoretical models", the models are constructed within the same interpretation of the theory whether it be the Heisenberg picture or the Schrodinger picture. This is to be contrasted to alternative models which are constructed via different interpretations of the whole theory itself, i.e. the Heisenberg and Schrodinger picture of quantum mechanics.

If the models only differ according to what idealization assumptions are used, is it not possible that there is one model that accurately explains or predicts phenomena which does not contain any idealization assumptions at all? It is, of course, *possible* that there is such a model or theory, but it is clear that we do not possess such a theory or model at this moment. Cartwright's question to the realist is simply to ask why we should we be warranted to believe that there is such a theory or set of laws. Laymon's proposal responds directly to this question. The fact that when we use more realistic initial conditions, we get better predictions gives inductive evidence for the claim that if we had the adequate computational abilities and the appropriate auxiliary theories, we could produce the accurate prediction.

At the end of chapter 3, I offered several criticisms of this proposal Cartwright could respond by citing the exponential decay law, which appears to be a counter-example to Laymon's proposal The law is peculiar because even though idealization assumptions and approximations are used in its derivation, it still gives accurate predictions Furthermore, when we use more realistic initial conditions, we get worse predictions The question which remains to be answered is whether there is a fortuitous cancellation of idealization assumptions in its derivations Since we do not have the answer to this question, this example, at the moment, cannot be used as a counter-example to Laymon's proposal The two other difficulties which I mentioned in chapter 3, however, should pose more serious problems One of these difficulties was that for Laymon's principle to be applicable, we must be able to make sense of what it means for a set of initial conditions to be more realistic than another In that section, I offered an example of a case in which we could not say that one set of initial conditions is more realistic than another. Although I grant that there are many cases in which we can make sense of when one set is more realistic than another, I do not grant that we can in the important cases What do I mean by the important cases?

I take it that the purpose of Laymon's proposal is to respond to Cartwright's challenge since he makes several references to Cartwright's arguments in the processes of developing his proposal If so, then the focus of attention is on those cases in which we find alternative theoretical explanations, and we still do not have one explanation which captures the advantages of both explanations since it is these cases which Cartwright uses to argue for her form of anti-realism If these examples are similar to the one I described for which we could not say that one set of idealization assumptions is more realistic than the other, then it would seem that Laymon's suggestion cannot be used against Cartwright However, if we can tell which set of idealizations is more realistic than the other, then perhaps there is a response to Cartwright. I say, perhaps, because as I mentioned earlier in discussing van Fraassen, until the realist can explain how a true theory explains why when we use more realistic initial conditions, we get better predictions, Laymon's proposal cannot be used to defend realism. Where does this leave realism with respect to Cartwright? Recall that the argument which I summarized above is not the only argument which Cartwright provides against the realist. In her other argument, she concluded that the fundamental laws cannot describe the facts. Therefore, the realist must not only rebut the argument concerning there being alternative theoretical explanations, but the realist must also rebut the argument against the facticity view of the fundamental laws. Creary[55], for instance, provides a way in which we could interpret the fundamental laws which seems to responds to Cartwright's challenge. The point is that if the realist is to provide a full response to Cartwright, she must respond to both of her challenges.

On a final note, although I have spent the majority of this thesis attacking realism and investigating the role which idealizations play in this debate, my intent was not to argue for anti-realism. Instead, the intent was to try to make clear what must be done to argue for realism, and more specifically what must be done in terms of appealing to the use of idealization assumptions in a defence of realism.

[55]Lewis Creary, *Causal Explanation and the Reality of Natural Component Forces*

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