The Application of Physical Theories to Nature

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ABSTRACT
Two examples of applying physical theories to objects are considered and argued to support different models of theory application and theory evaluation. One of the examples confirms Carl Hempel's model of theory application whereas the other requires a different type of model. Nancy Cartwright's criticism of Hempel's model is considered and argued to misdiagnose the problem with this model.

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1 Introduction

Suppose we have some theory T and we wish to apply it to object O so as to predict something about O. The theory might be Newtonian mechanics, the object a ball suspended by a spring, and the aim to use Newtonian mechanics to predict the motion of the ball. Or the theory might be the kinetic theory of gases, the object a sample of some gas, and the aim to use the former to predict the heat capacity of the latter. The question I address in this paper is the following: does the application of Newtonian mechanics (the kinetic theory of gases) to the suspended ball (a sample of some gas) imply specific predictions about the ball's motion (the heat capacity of the gas)? This question can be rephrased as follows. There are certain laws, principles, equations, and so forth, which must be understood and accepted if one can be said to understand and accept a theory. Let us call these basic assumptions the principles of the theory or its *theoretical principles.* Newton's three laws of motion are obvious candidates for theoretical principles of Newtonian mechanics. In addition, in accepting a theory we often must also accept some mathematics, e.g., algebra, geometry, and vector calculus. The question which will concern us can now be restated as follows: does the application of the principles and mathematics of Newtonian mechanics (the kinetic theory of gases) to a ball suspended by a spring (a sample of some gas) imply specific predictions about the ball's motion (the heat capacity of the gas)?

According to Carl Hempel (1966, pp. 72-75), the above question should be answered with an affirmative. Hempel construes theories as consisting of two types of principles, namely, bridge principles and internal principles.

> Broadly speaking, the formulation of a theory will require the specification of two kinds of principles; let us call them internal principles and bridge principles for short. The former will characterize the basic entities and processes invoked by the theory and the laws to which they are assumed to conform. The latter will indicate how the processes envisaged by the theory are related to empirical phenomena with which we are already acquainted, and which the theory may explain, predict, or retrodict. (1966, pp. 72-73)

Theory application, on the other hand, is modeled in terms of three stages. In the first stage a description of the object, expressed only in terms of a pretheoretical vocabulary, is converted into a description using theoretical terms. Pretheoretical terms, or antecedently available terms, are "terms that have been introduced prior to the theory and can be used independently of it" (1966, p. 75). One advantage of assuming a pretheoretical vocabulary is that it avoids the problematic assumption of an observational vocabulary. A second advantage is that a pretheoretical description provides a starting point from which theory application in its entirety can be traced since, by definition, these descriptions can be arrived at and understood without using the theory. In the first stage of theory application, a theoretical description is deduced from a pretheoretical description through the use of the theory's bridge principles. In the second stage the internal principles of the theory are used to predict what happens to an object falling under the theoretical description inferred in the first stage. And finally, in the third stage bridge principles are used again so as to connect these predictions with predictions stated in terms of the pretheoretical vocabulary. A schematic illustration of Hempel's model of theory application is as follows (BP

$$\begin{array}{l}\text{pretheoretical description} \xrightarrow{BP} \text{theoretical description} \\ \text{theoretical description} \xrightarrow{IP} \text{predicted theoretical description} \\ \text{predicted theoretical description} \xrightarrow{BP} \text{predicted pretheoretical description}\end{array} \qquad (1)$$

indicates a set of bridge principles and IP indicates a set of internal principles):

Hempel answers the questions stated above with an affirmative because, according to the above model of theory application, predictions about objects are deduced from the application of theoretical principles to these objects. For example, predictions about the motion of a ball suspended by a spring (the heat capacity of a sample of gas) would be deduced from the application of the bridge principles and internal principles of Newtonian mechanics (the kinetic theory of gases) to a pretheoretical description of the ball and spring (the sample of gas). What could be wrong with these conclusions? One obvious answer to this question is that theories simply do not provide enough theoretical principles to accommodate Hempel's view. For example, a theory might not supply the bridge principles needed to link a pretheoretical description of some object with a theoretical description which is sufficiently detailed to yield the desired prediction. This does not, however, imply that the theory cannot be used to make such predictions; rather, predictions might be obtained by supplementing the principles of the theory with assumptions which do not qualify as principles of the theory. This type of argument is developed after considering the "sample of gas" example in the second section. The first section provides a discussion of an example of theory application which does fit Hempel's model (the "ball and spring" example).

In the third section Nancy Cartwright's (1980) criticism of Hempel's model is considered. Cartwright criticizes Hempel's construal of theory appliction by arguing that very often there is a gap in the alleged chain of deductive inferences characterized by Hempel. I share this view but disagree with Cartwright about the location of the gap. Cartwright misdiagnoses the problem with Hempel's view. She argues that a theory provides few bridge principles for linking theoretical descriptions with the sorts of descriptions to which the "theory can match an equation". In terms of Hempel's picture she therefore locates the gap somewhere in the second stage. On the basis of the "sample of gas example", I argue that the gap occurs in the first stage, that is, in between the initial pretheoretical description and the theoretical description. The existence of this gap is attributed to the absence of bridge principles. Gaps might exist in other places as well but I argue that Cartwright finds a gap where there is none. And finally, the last section provides a summary of the basic conclusions of the paper.

2 Principle Governed Theory Application

In this section Newtonian mechanics is applied to a ball suspended by a spring. The question which concerns us here is whether the application of the principles and mathematics of Newtonian mechanics to the ball and spring imply a prediction about, for example, the location of the ball in five seconds. In order to simplify the example, we shall assume that there are no frictional forces acting on the ball or spring—this makes the example somewhat artificial but nonetheless I will argue that the example serves to illustrate an instance of theory application which fits Hempel's model. In order to capture the complete course of theory application the starting point must be a description of the ball and spring stated in terms of a pretheoretical vocabulary. The general plan is to begin with a pretheoretical description of the object, apply the theory to this description, and derive a prediction about the motion of the ball.

A ball is attached to a spring. The ball is for the most part spherical and about the size of a marble. It is connected to a spring made of some type of wire which is wound into a helical shape. The helix has a certain length and a certain diameter and the wire has a certain thickness. The spring hangs vertically from a ceiling beam and the ball hangs at the bottom of the spring. We observe that if we take hold of the ball, move it downwards and release it, then the ball moves up and down. We can also measure the ball's position at various times and plot its position as a function of time.

All of the terms used in the above description can be understood independently of Newtonian mechanics. But the description is only a partial description and the extent to which certain features of the objects are described whereas others are not might be indicative of some underlying understanding of Newtonian mechanics. The description's focus on the kinematic and geometric features of the objects, as opposed to, for instance, their colors, might be due to the describer's awareness that geometric features as opposed to colors enter into a Newtonian account of the motion of the ball.

Therefore, if our aim is to illustrate the complete process of theory application, it is better to add to the above description everything else one knows about the objects which does not presuppose any knowledge of Newtonian mechanics.

Before applying Newtonian mechanics to the ball and spring as described above it is necessary to introduce an assumption about the composition of Newtonian mechanics. Like Hempel, I assume that Newtonian mechanics consists of a set of theoretical principles which are interpreted in standard ways. These principles include Newton's laws of motion, the principles of conservation of momentum and energy, and Newton's law of gravitation. There are other principles as well but what is characteristic of all of the principles of a theory is that in accepting the theory one must accept them. This assumption is supported by the fact that textbooks always introduce these principles as central to the theory and by the fact that advocates of the theory apply these principles without the need for any special justification. The question before us can now be phrased as whether the application of these principles and the theory's mathematics to the pretheoretical description of the ball and spring imply a prediction about the ball's location at future times.

Theory application might take the following course. First, the theory informs us that the ball has some quantity of mass. Here we rely on a theoretical principle of Newtonian mechanics, often left unstated, which asserts that all bodies have some quantity of mass. The quantity of mass of the ball can be measured with a pan balance. The mass is determined by placing the ball on one arm of the balance and placing masses of known magnitude on the other arm until the two arms are balanced. Next, given that the motion of the ball is nonuniform, Newton's first law implies that an unbalanced force acts on the ball, i.e., every material body persists in its state of rest or of uniform, unaccelerated motion in a straight line, unless it is compelled to change that state by the application of an external, unbalanced force. This raises the question of what is the magnitude and direction of the force which acts on the ball. Suppose one takes the ball in his hand and moves it towards the spring so as to compress the spring. In that case, he feels a force exerted by the spring against his hand. Similarly, if one moves the ball so as to stretch the spring he feels a force pulling his hand towards the spring. Pushes and pulls like these provide some of the criteria for identifying forces. Understanding the Newtonian concept of 'force' assumes, among other things, an understanding of these criteria for identifying the presence of forces. Finally, the magnitude of the force due to the spring can be determined by suspending the spring from a beam and attaching various weights of known magnitude to the end of the spring. The magnitude of these weights versus the distance they displace the spring from its relaxed position can then be plotted. The resulting plot provides a graph of f(x), that is, the magnitude of the force exerted by the spring on a body attached to it when the spring is displaced from its relaxed position by a distance x. Furthermore, suppose that over the range of x for which f(x) is measured that f(x) fits the line $f(x) = -kx$, where k is 160; better yet, suppose that as far as the eye can discern the measured values of f(x) exactly fits the line

$f(x) = -kx$.

There are several gaps in the above reasoning, that is, places in which conclusions are reached which are not apparently the result of applying theoretical principles to the pretheoretical description. Why, for instance, does the pan balance provide a measure of mass? To see why it does we would need to consider the application of additional theoretical principles to the pan balance, e.g., Newton's second law of motion and the law of gravitation. Similarly, the method for measuring the spring constant assumes some additional theoretical principles such as Newton's third law. But for present purposes it is not necessary to show how every step in the derivation is warranted by principles of Newtonian mechanics. Instead, it is sufficient to point out that masses and forces are measured this way and that, if one understands and accepts Newtonian mechanics, he or she must accept the results of such measurements. We cannot understand and accept Newtonian mechanics and still doubt that the values for m and f(x) as determined above are the correct values. If one consults any textbook on Newtonian mechanics or any authority on the subject, applies what he learns to the ball and spring as described, and still denies the assumed values for m and f(x), then he has not yet learned Newtonian mechanics. Without filling in the necessary steps and digressing further into elementary Newtonian mechanics, we may safely conclude that the following theoretical description of the ball can be deduced from the application of theoretical principles to the assumed pretheoretical description:

> A mass of 10 grams is acted upon by a force $-kx$ where k is 160 and x is the displacement of the ball from its equilibrium position.

If we accept the above conclusions, as I think we must, then this example confirms Hempel's view of the first stage of theory application. Recall that according to Hempel theory application begins with a description of an object stated in terms of a pretheoretical vocabulary. In the first stage of theory application bridge principles are applied to this description and a description of the object in terms of the theoretical vocabulary is deduced, i.e., the above theoretical description. This description is deduced because, if we premise the pretheoretical description and Newtonian mechanics, the theoretical description can only be denied at the price of inconsistency, i.e., denying at least one of the premises. Newtonian mechanics therefore provides a set of bridge principles which enable us to deduce the above theoretical description from the pretheoretical description. For example, Newton's first law of motion functions as a bridge principle when it is used to infer the presence of an unbalanced force from a description of a nonuniform motion. And in less obvious ways, additional principles of Newtonian mechanics, as interpreted in standard ways, are used to infer the remainder of the theoretical description from the pretheoretical description.

According to Hempel, in the second stage of theory application internal principles of the theory are used to predict what happens to an object falling under the theoretical description deduced in the first stage. In the second stage, Newton's second law of motion functions as an internal principle since it can be used to predict what happens to an object falling under the above theoretical description. Newton's second law states that the net unbalanced force acting on a body is proportional to the product of its acceleration and mass; therefore, it follows that $ma = -kx$. Assuming some initial conditions (that the ball is released from rest at a point 5 cm from its equilibrium position) and some of the mathematics assumed by Newtonian mechanics, the following equation of motion is derived:

$$m\frac{d^2x}{dt^2} = -kx \tag{2}$$

The general solution to this equation of motion is:

$$x(t) = A\cos(\omega t + \phi) \tag{3}$$

where $\omega = \sqrt{k/m}$ and ϕ is the phase factor. Since $m = 10$ grams and $k = 160$, $\omega = 4$ radians/second. And finally, application of the initial conditions (at $t = 0$ $x = 5$ cm and v $= 0$) implies that $A = 5$ cm and $\phi = 0$. The final solution to the equation of motion is therefore:

$$x(t) = 5\cos(4t) \tag{4}$$

In the final stage of Hempel's picture of theory application bridge principles are used again so as to infer a prediction about the ball, stated in terms of a pretheoretical vocabulary, from the theoretical description of the object which was deduced with internal principles. A bridge principle would link the predicted motion of the mass with the predicted motion of the ball. Bridge principles like this one are often left unstated in textbooks, but in order to show how the present example fits Hempel's view, they must be explicitly stated.

In summary, if we premise the assumed pretheoretical description of the ball and spring and Newtonian mechanics we have no choice but to predict that the ball's motion will be accurately described by figure 1. In this case, we are not free to argue that, for instance, this is the motion of the ball predicted with Newtonian mechanics by John Smith but Fred Brown has used the same theory to predict a different motion. There are no choices here—if John and Fred arrive at different predictions then one of them has not correctly applied Newtonian mechanics to the pretheoretical description. And finally, these facts have clear consequences for the related issue of theory evaluation. If the ball's motion does not approximate the motion depicted in figure 1 then, if we assume that the pretheoretical description is accurate, then the theory is in trouble, i.e., at least one of its theoretical principles must be modified or rejected. The blame cannot be placed elsewhere as it might be if we could argue that there are *other* ways of applying the theory which might accurately predict the motion.

3 When Bridge Principles are Lacking

In this section I develop an example of theory application which does not support Hempel's view. In this example the kinetic theory of heat is applied to a sample of some gas so as to derive the heat capacity of the gas. What distinguishes this example from the "ball and spring" example is that the application of the principles of the

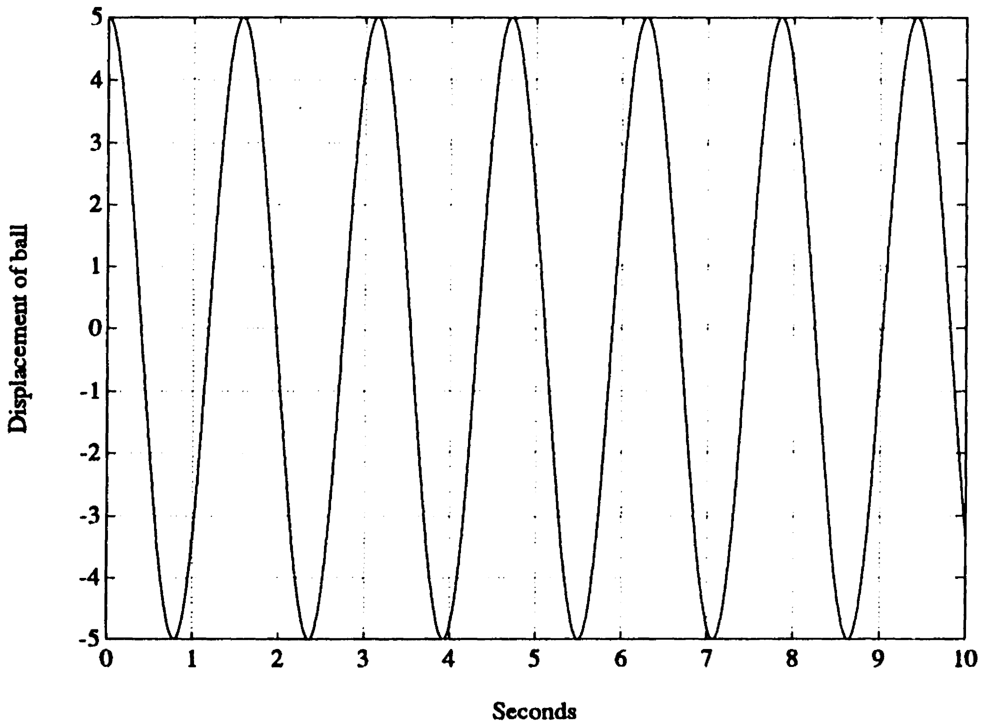

Figure 1: Theoretically predicted motion of ball.

kinetic theory of heat to the gas does not result in a theoretical description which is sufficiently detailed to yield a prediction about the heat capacity of the gas. But of course, this theory was used to make predictions about the heat capacities of various gases; therefore, theory application involves something besides applying theoretical principles. The additional component consists of hypotheses which are not mandated by the application of any of the theory's principles. In this example, these hypotheses consist of conjectures about the molecular structure of the gas, and because different advocates of the theory may favor different hypotheses, the theory can be used to make several conflicting predictions about the heat capacity of the gas. The "right" hypothesis is not determined by theoretical principles, rather, it is determined with the hindsight gained from comparing predictions based on alternative hypotheses with observation.

As was the case with Newtonian mechanics, the kinetic theory of heat is assumed to povide several theoretical principles. These principles include Newton's laws of motion, the principles of conservation of energy and momentum, and Newton's law of gravitation. In addition, the theory assumes certain bridge principles such as that asserting that the temperature of a gas is proportional to the mean kinetic energy of its molecules. Another principle of the theory is the principle of equiparition of energy. This principle states that for a substance in thermal equilibrium its energy is equally divided between the degrees of freedom of its molecules, that is, its energy is equally divided between the translational, rotational, and vibrational degrees of freedom of is molecules. Some advocates of the theory questioned this principle, such as Maxwell, but for present purposes I will treat it as a principle of the theory. And finally, the theory provides several definitions which pertain to heat capacities. The heat capacity C, or specific heat, of a substance is defined as the amount of heat required to raise a mole of the substance by one degree of temperture. Expressed differently, the heat capacity is the capacity of a body to absorb heat for a given rise in temperture. Two types of heat capacity are distinguished. C_v is the heat required to raise the temperture of some amount of a mole of a substance by one degree when the volume is held constant. In this sense, C_v is said to measure the true heat capacity because when the volume is held constant no work is done. C_v is defined as

$$C_v = \partial E / \partial T \tag{5}$$

where E is the heat energy used to raise the temperture T. On the other hand, C_p represents the heat required to raise the temperture of a mole of substance one degree with the pressure is held constant:

$$C_p = C_v + R \tag{6}$$

where R is the gas constant. From these two equations it follows that:

$$\gamma = C_p / C_v = 1 + R / C_v \tag{7}$$

Therefore, once C_v is known, equations 6 and 7 can be used to infer C_p and γ. Since $C_v = \partial E/\partial T$, determining C_v requires knowing $E(T)$, that is, the dependence of the energy per mole of gas on T. In order to determine $E(T)$ one additional result will be assumed. Advocates of the kinetic theory of heat determined that a mole of gas has $RT/2$ units of energy per degree of molecular freedom.

The above assumptions consist of theoretical principles, definitions, and some "results" which do not fit in either category. Whether or not all of these assumptions are essential to the kinetic theory of heat is open to dispute, but let us give Hempel the benefit o: the doubt, and simply assume that the theory includes all of these assumptions and that they are all similar to theoretical principles in that accepting the theory presupposes the acceptance of them. Given these assumptions, the problem of determining C_v for a gas is greatly simplified. Since the total energy E per mole of a gas is the sum of the energies per degree of freedom of translation, rotation, and vibration, and each of these degrees contributes $RT/2$ energy, it follows that:

$$E = (t + r + 2v)RT/2 \tag{8}$$

where t, r, and v represent the number of translational, rotational and vibrational degrees of freedom and the factor of 2 before v is attributed to the fact that vibrational energy consists of both kinetic and potental energy, whereas translational and rotational energy are purely kinetic. And finally, since $C_v = \partial E/\partial T$,

$$C_v = (t + r + 2v)T/2 \tag{9}$$

To further simplify the problem of evaluating Hempel's view, suppose we now simply assume that equation 9 is one of the internal principles of the kinetic theory of heat; hence, we can forget about its derivation and simply assume it as fundamental. Hempel's view will therefore be vindicated if the theory also provides bridge principles which connect pretheoretical descriptions of gases with theoretical descriptions of the degrees of freedom of their molecules.

In order to appraise Hempel's view let us assume that a group of advocates of the kinetic theory of gases is confronted with a sample of gas and asked to use the theory to predict its heat capacity. The names of the hypothetical members of this group are Krönig, Clausius, Jeans, Rankine, and Maxwell.

Krönig models (describes) the gas as consisting of N point-masses, where N is Avagadro's number. Because point-masses only have translational motions, and there are three degrees of translational freedom, $E(T) = 3RT/2$, and therefore, $C_v = 3R/2$, and γ is $1\frac{2}{3}$. The physicist Krönig derived this result in the nineteeth century. Following Krönig, Clausius argues that molecules might have motions in addition to that of translation, such as rotational and vibrational motions. He reasons as follows:

> Krönig assumes that the molecules of a gas do not oscilliate about definite positions of equilibrium, but that they move with constant velocity in right lines until they strike against other molecules, or against some

surface which is to them impermeable. I share this view completely, and I also believe that the expansive force of the gas arises from this motion. On the other hand, I am of the opinion that this is not the only motion present ...In the first place, the hypothesis of a rotary motion as well as a progressive motion of the molecules at once suggests itself; for at every impact of two bodies, unless the same happens to be central and rectilinear, a rotary motion ensues ...I am also of the opinion that vibrations take place within the several masses in a state of progressive motion. Such vibrations are conceivable in several ways. (1857, p.109)

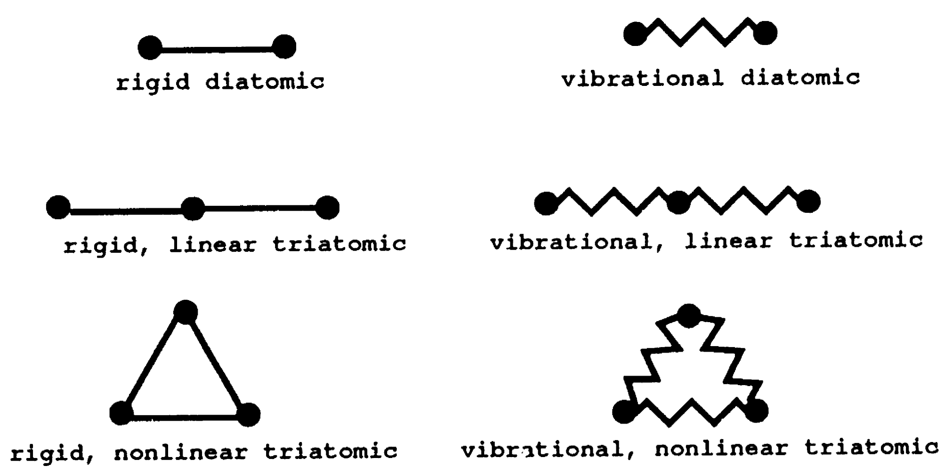

Figure 2: Illustration of several molecular models.

Some of the models suggested by Clausius are illustrated in figure 2. The C_v associated with each of these models can be derived by simply determining the number of translational, rotational, and vibrational degrees of freedom allowed by the model and substituting these values into equation 9. The following table lists the names of these models and the values of t, r, v, C_v and γ associated with them.

Suppose that the measured value of γ is 1.5. Krönig predicts a value of 1.67 for γ whereas Clausius uses a variety of models, the best of which predicts a value of 1.40 for γ. Because neither of these results agree with the measured result, Jeans considers

Model Type	t	r	v	C_v	γ
point-mass	3	0	0	3R/2	5/3
rigid diatomic	3	2	0	5R/2	7/5
vibrational diatomic	3	2	1	7R/2	9/7
rigid, linear triatomic	3	2	0	5R/2	7/5
rigid, nonlinear triatomic	3	3	0	6R/2	8/6
vibrational, linear triatomic	3	2	4	13R/2	15/13
vibrational, nonlinear triatomic	3	3	3	12R/2	14/12

Figure 3: Summary of values of t, r, v, C_v and γ for several molecular models.

the implications of assuming that molecules can aggregate with one another, and after some derivation, shows how molecular aggregation will enter into the determination of C_v C_p, and hence γ:

> For in raising the temperture of the gas work is done not only in increasing the energy of the various molecules, but also in seperating a number of molecules from one another's attractions. This work will involve an addition to the values of C_v and C_p such as was not contemplated in the earlier analysis... We should therefore expect the values of C_p and C_v to be in excess of the values obtained from our eariler formulae, throughout regions of pressure and temperture in which molecular aggregation can come into play. (1962, p.293)

But suppose Jeans reasoning cannot make the theory accommodate the measured value of γ since, as Krönig and Clausius point out, the pressure and temperture of the gas are not in regions in which molecular aggregation comes into play—the measured γ therefore remains an anomaly for the theory. Rankine now takes up the challenge and introduces a new type of model which might account for the measured γ. Rankine considers a vortex model of the molecules which he describes as follows:

> That each atom of matter consists of a nucleus, or central physical

point, enveloped by an elastic atmosphere, which is retained in its position by forces attractive towards the nucleus or centre.

That the elasticity due to heat arises from the centrifugal force of revolutions or oscillations among the particles of the atomic atmospheres; so that the quantity of heat is the *vis viva* [kinetic energy] of those revolutions or oscillations. (1851, p.234)

Rankine shows that the expressions for C_v derived with this type of model take the following form:

$$C_v = \partial E/\partial t = f(k) \tag{10}$$

Here, C_v is expressed simply as a function of the coefficient k which Rankine uses to represent the ratio of the kinetic energy due to the vortical motion of an elastic atmosphere to the total energy of an atom (which also includes the kinetic energy of its nucleus, and possibly, the energy due to "oscillations of expansion and contraction, or of rectilinear vibration about a position of equilibrium" (1851, p. 240). Different values for k, and hence for C_v and γ can be inferred from different vortex models. But suppose that after imagining a variety of vortex models, Rankine is not able to find one which leads to a value of γ equal to 1.5.

Finally, Maxwell steps forward and shows how the theory can be used to predict a continuous range of values for γ ranging from 1.3 to 1.6. He obtains this result by assuming that the gas might consist of various mixtures of spherical and non-spherical particles:

By considering the effect of collisions of bodies of any form not spherical it appears that the *vis viva* of rotation tends to become equal to that of translation so that the whole energy in unit of volume is not $\rho v^2/2$ as in the case of perfect spheres, but ρv^2. In a medium of perfect spheres and partly of other bodies the energy will be $\beta \rho v^2/2$ where

$$\beta = \frac{\rho_1/2 + \rho_2}{\rho_1 + \rho_2}$$

(ρ_1 being the weight of the spheres and ρ_2 of the other bodies in unit of volume) where q is the ratio of the mass of the non-spherical particles to that of the whole mass. If γ is the ratio of the specific heat under constant pressure to that under constant volume

$$\gamma = 1 + \frac{2}{3\beta}$$

If the particles are all spherical with their centres of figure and mass coincident than $q = 0$, $\beta = 1$, and $\gamma = 1\frac{2}{3} \approx 1.6$. If none of the particles fulfill these conditions then $q = 1$, $\beta = 2$, and $\gamma = 1\frac{1}{3} \approx 1.3$. These are the two extreme cases. (1986, p. 341)

Maxwell then concludes that the gas consists of a mixture of particles in which the number of spherical particles is greater than the number of non-spherical particles and shows how a value for γ of 1.5 can be derived.

Before turning to the implications of this example it should be pointed out that Krönig, Clausius, Maxwell, Jeans, and Rankine accepted the kinetic theory of gases. They all accepted Newton's laws of motion, the assumption that the mean kinetic energy of a gas is proportional to its temperature, and various additional principles of the kinetic theory. In addition, although the scenerio assumed in this example is artificial, the above discussion accurately reflects the molecular models developed by these individuals and the values of C_v and γ they derived with these models. With these conclusions in mind let us now turn to the question of whether Hempel's construal of theories and theory application fits this example.

This example presents a problem for Hempel's view because of the absence of bridge principles which connect a pretheoretical description of the gas with a description of its molecules. The theory does provide some bridge principles such as the general principle which states that the temperature of a gas is proportional to the mean kinetic energy of its molecules. But the application of these principles to the gas does not produce a description of the molecules which is sufficiently detailed to imply specific values for C_v and γ. On the other hand, one might argue that Krönig, for example, introduced such a bridge principle which linked a pretheoretical description of the gas with a detailed molecular model when he described the gas as a collection of point-masses—but surely this is not a principle of the kinetic theory since Clausius, Maxwell, Jeans, and Rankine might have modeled the gas differently even though they were still applying the kinetic theory.

The "correct" model of the gas is not determined by the application of principles to a pretheoretical description of the gas but by the conjectures of individuals and trial and error methods. Krönig modeled the gas as a collection of point-masses and predicted a value of 1.67 for γ. Because the measured value of γ is 1.5, Clausius, Jeans, Rankine, and Maxwell put forth alternative hypotheses about the molecules of the gas and derived different results for γ. If in the end we conclude that Maxwell found the "correct" model then it is not because his description of the gas was dictated by the application of principles of the theory but because his model predicts that γ is 1.5. And here is where theory application involves not so much the application of theoretical principles as the imagination of its advocates. The role of imagination becomes particular important when a theory is confronted with an anomaly, for as this example illustrates, the advocates of a theory often concoct a diverse group of models in their attempts to find the "correct" model which makes the theory accommodate the anomaly.

The present example, therefore, supports a view of theory application quite different from that supported by the example of the previous section. The two examples also corroborate sharply contrasting views of theory evaluation. In the discussion of the last section I argued that everyone who understands and accepts Newtonian mechanics, reasons correctly, and premises the assumed pretheoretical description of the ball and stone must reach the same prediction about the ball's motion. The application of the theory's principles did not leave room for choices which, for instance, would allow John Smith and Fred Brown to make different predictions about the ball's motion. On the other hand, on the basis of the example of this section I have argued that the application of the principles of the kinetic theory to a gas requires that individual conjectures be made if our aim is to predict values for C_v and γ. Because of the variety of possible conjectures, the theory can be used to predict many values for C_v and γ. Two different views of theory evaluation emerge from these two examples. If the motion of the ball does not agree with the motion predicted with Newtonian mechanics then the theory is in trouble—one of its principles must be modified or rejected. But if Krönig uses the kinetic theory to predict a value for γ of 1.67 when in fact γ is 1.5 then it does not follow that one of the principles of the kinetic theory must be rejected or modified. Rather, in cases like this one, the blame can be placed on the individual as opposed to the theory. Krönig failed to find a model which would make the theory accommodate the phenomenon, but subsequently, Maxwell vindicated the theory by showing how it could predict 1.5 for C_v.

Similarly, these two examples argue for different conclusions about whether a distinction between a "context of evaluation" and a "context of discovery" can be maintained. In the case of the kinetic theory, theory evaluation and theory discovery (articulation) occur simultaneously and in the same context. In order to evaluate the kinetic theory in accordance with its agreement with measured values of C_v and γ it is necessary to imagine ways in which gases can be modeled. If none of these models accounts for a given heat capacity then the process of evaluation is not complete since new models might be discovered which accommodate the heat capacity. On the other hand, if Newtonian mechanics does not correctly predict the ball's motion then the theory is in trouble and we are not free to discover new ways of describing the ball and spring since a unique theoretical description is deduced from the application of the theory's principles. Therefore, in this case, theory evaluation occurs in a context into which theory discovery does not enter.

The above discussion of the kinetic theory suggests a different picture of theories and the way in which their empirical content is determined. Hempel's construal of theories in terms of bridge principles and internal principles provides a simple answer to the question of how a theory is related to its empirical content, i.e., how it is related to its testable predictions. The content of a theory can be deduced by applying its bridge principles and internal principles to pretheoretical descriptions of the objects in its domain. Whereas Hempel's picture provides a straightforward way of delineating the content of a theory, the above discussion of the kinetic theory suggests that a more complicated picture is needed. In order to address this issue let us assume that every aspect of the above derivations of values for C_v fits Hempel's picture except for those steps in which a molecular model was assumed. Given this assumption, the C_v content of the theory is fixed by the theoretical principles in conjunction with all of the molecular models allowed by the theory. This suggests that the kinetic theory be construed in terms of bridge principles, internal principles, some mathematics, and models. The crucial question which must then be resolved in determining the C_v content of the theory is then—how is the class of models allowed by a theory determined?

There is no clear-cut way of specifying the models of a theory, nonetheless, some clarity can be gained by drawing attention to the following two points. First, models used in one application are often borrowed from other applications. All of the models discussed in applying the kinetic theory of heat to a gas originated in applying Newtonian mechanics to balls, springs, and similar objects. More generally, many of the models used in applying the classical theories of heat, sound, and light originated when applying Newtonian mechanics to objects within its domain. Second, these "borrowed" models also function as generative models because they are suggestive of a cluster of models. Let us call the description of molecules in terms of balls and springs a *root model.* The root model is a generative model because it suggests a large group of models which are variations of it. These variations stem from choices which result from thinking about the root model. Some of these choices pertain to the mass, size, shape, and number of the "balls"; the length of the spring and its spring constant; and the arrangement of the balls and springs (linear or nonlinear).

The above considerations place some constraints on the class of models which can be used in applying the kinetic theory. These models must be based on models developed in applying Newtonian mechanics to its domain or they must be generated from models developed in applying Newtonian mechanics, e.g., combinations or variations of these models. These constraints might not be very satisfying since they leave us with a "fuzzy" characterization of the kinetic theory and its empirical content. On the other hand, a more rigid characterization may result in a misunderstanding of the kinetic theory and its empirical content.

4 Cartwright's Criticism of Hempel's View

Nancy Cartwright (1980) has also criticized Hempel's construal of theory application, although, her objection is quite different from the objection raised in the previous section. These two types of objections are similar because, stated too generally, they both assert that there is often a gap in the alleged Hempelian chain of deductions which occurs before a prediction is reached. But these two forms of criticism differ when it comes to locating the gap. I have argued that theories often do not provide the bridge principles needed to connect a pretheoretical with a sufficiently detailed theoretical description. To remedy this problem, the principles of the theory are supplemented with hypotheses authored by individuals. This type of reasoning places the gap somewhere in the first stage of the Hempelian model of theory application—in between the pretheoretical description and a sufficiently detailed theoretical description. On the other hand, Cartwright's construal of theory application overlooks the first stage and simply starts with a theoretical description. She then locates a gap somewhere in the second stage by arguing that a theory provides very few principles for converting a theoretical description into a description to which the "theory can match an equation". In general, I will argue that Cartwright overlooks the stage of theory application in which gaps occur and argues that there is a gap where there is none.

Cartwright describes theory application as having two endpoints, namely, theory entry and theory exit. Theory exit is what happens at the end of theory application when we "exit" the theory and conclude with a prediction. The problem with Cartwright's view which is alluded to above, however, arises in her account of theory entry:

> ...I think theory entry proceeds in two stages. We start with an *unprepared* description which gives as accurate a report as possible of the situation. The first stage converts this into a *prepared* description. (1980, p. 15)

We shall shortly see what a prepared description is; for now it is only important to note that her first stage of theory entry *starts* with an unprepared description. But now notice what she allows as an unprepared description:

> Theory entry proceeds in two stages. I imagine that we begin by writing down everything we know about the system under study, a gross exaggeration, but one which will help to make the point. This is the *unprepared description* ... The unprepared description contains any information we think relevant, in whatever form we have available. There is no theory-observation distinction here. We write down whatever information we have: we may know that the electrons in the beam are all spin up because we have been at pains to prepare them that way ... and we may also know that that the cavity [of a helium-neon laser] is filled with three-level helium atoms. The unprepared description may well use the language and the concepts of the theory, but it is not constrained by any of the mathematical needs of the theory. (1980, p. 133)

The above unprepared description of the laser describes the laser cavity as containing three-level atoms, i.e., atoms which have three energy levels. On the other hand, the theory which Cartwright applies to the laser is quantum theory (1980, p. 132). But Cartwright's account of theory entry *starts* with an unprepared description. One might therefore argue that Cartwright overlooks the first stage of theory application since her account starts with descriptions which presuppose some previous theory application—quantum theory was used in inferring that the laser cavity contains three-level atoms. But Cartwright would deny this and argue that the reasoning which implies that there are three-level atoms in the cavity does not assume the quantum theory. This conclusion is supported by Cartwright's realism about causal entities and anti-realism about theories. She believes in electrons and atoms but she denies that our theories about electrons and atoms are true; hence, there must be a way of ascertaining their existence which does not premise some theory about them. Cartwright argues that causal strategies provide the answer. By manipulating a cause (an electron) and checking to see if its effects change in the appropriate manner the nature of the causal entity (the electron) can be determined. This is done through experimentation and "intervention" and in such a way that no theory about electrons is presupposed—it is not the Bohr electron, the Rutherford electron, or the Lorenz electron, rather, "it is the electron, about which we have a number of incomplete and sometimes conflicting theories" (1980, p. 92).

The manipulation of causal entities is one method which informs our understanding of the microscopic world, but I have also emphasized the way in which theories are used to make inferences about electrons. Inferences about electrons are made in a way similar to the way in which the hypothetical Maxwell of the last section inferred that the sample of gas consisted of a mixture of spherical and non-spherical particles. In this case, the kinetic theory was presupposed in making this inference. This type of inference is called "inference to the best explanation" as opposed to "inference to the most probable cause" as characterized by Cartwright. I doubt that these two types of inference come apart as easily as Cartwright suggests, but without further digression, let us gather what is needed from these considerations and return to the main line of argument. Suppose that Cartwright is right and that just as experimentation and intervention implied that the laser cavity consists of three-level atoms it now implies that the gas consists of nonlinear triatomic molecules.

Theory entry therefore simply begins with an unprepared description which describes the molecules of the gas. In this way, Cartwright circumvents those steps in theory application for which I argued that Hempel's view runs into problems. Where then does Hempel's view go wrong according to Cartwright? The problem Cartwright points to is reflected by her use of the phrases *unprepared description* and *prepared description*. Why does Cartwright call certain descriptions *unprepared description*, or expressed differently, what are these descriptions not prepared for? According to Cartwright, they are not prepared for the "mathematical needs of the theory":

> At the first stage of theory entry we prepare the description: we present the phenomenon in a way that will bring it into the theory. The most apparent need is to write down a description to which the theory matches an equation. (1980, p. 133)

The unprepared description must be converted into a prepared description because of the limited number of descriptions to which a theory can match an equation. In support of this argument we might appeal to textbooks on Newtonian and quantum mechanics. In the first case, we find a limited number of equations for representing balls attached to springs—there are the standard equations for the simple harmonic oscillator, the damped oscillator (with underdamping, critical damping, and overdamping), the driven oscillator, the damped and driven oscillator, and so forth. In the case of quantum mechanics Cartwright appeals to textbooks and cites a handful of what she calls "model Hamiltonians" which include the linear harmonic oscillator, the free particle in one dimension, the particle in a box, all of which, are associated with a Hamiltonian which when substituted into Schrödinger's equation gives the allowed energy levels for that model (1980, pp. 136-137). These models are examples of Cartwright's prepared descriptions, that is, descriptions to which the theory matches an equation. But nature is messy and complicated and the descriptions which accurately describe it are rarely prepared descriptions: "I claim that in general we will have to distort the true picture of what happens if we want to fit it into the highly constrained structures of our mathematical theories" (1980 p. 139). And finally, this presents a problem for Hempel's view because a theory provides very few principles for going from a unprepared description to a prepared description: "there are few formal principles for getting from 'true descriptions' to the kind that entails an equation. There are just rules of thumb, good sense, and, ultimately, the requirement that the equation we end up with must do the job" (1980, p. 133).

> This first stage of theory entry is informal. There may be better or worse attempts, and a good deal of practical wisdom helps, but no principles of the theory tell us how to prepare the description. We do not look to a bridge principle to tell us what is the right way to take the facts from our antecedent, unprepared description, and to express them in a way that will meet the mathematical needs of the theory. The check on correctness at this stage is not how well we have represented in the theory the facts we know outside the theory, but only how successful the ultimate mathematical treatment will be. (1980, p. 134)

So as to evaluate Cartwright's argument, consider the claim that prepared descriptions are required because of the mathematical needs of the theory. Cartwright argues that a theory only attaches equations to a limited number of descriptions. She gives the example of quantum theory and the limited number of Hamiltonians one finds in a textbook on the theory. Similarly, in textbooks on Newtonian mechanics one finds only a handful of equations of motion for balls attached to springs. Students are taught to somehow describe the situations characterized in their problem sets in a way that leads to one of these stock equations. The result might be a distorted description of the situation but it has the virtue of leading to equations that they have learned how to solve. But the limited number of "stock" equations provided by standard textbooks reflects a pedagogical strategy and not a limitation of the theory.

A theory can match equations to a vast number of theoretical descriptions. Suppose, for example, that theory entry begins with a very complicated unprepared description of the molecules of a gas. The molecules are triatomic; the interatomic forces within a molecule are complex damping and restoring forces; and the intermolecular forces vary as a complicated function of the intermolecular spacing. If all of these parameters are known then we can in principle determine the equation of motion of each atom. The equation would be extremely complicated, but nonetheless, the theory does associate an equation of motion with each atom; therefore, the assumption of a prepared description which idealizes or in some other way misrepresents the atoms is not necessary.

On the other hand, it is highly unlikely that all of the atomic parameters would be known. If the kinetic theory is used in an attempt to fill in these details then, as I have argued in the last section, we find that it does not provide the needed bridge principles. The resulting gap must be filled with conjectures which are not implied by the theory. So there might be a gap here, but it is not the one argued for by Cartwright. According to Cartwright these parameters are not determined through theory application. They are not determined through "inference to the best explanation", i.e., by inferring the "correct" model from its role in accounting for the heat capacity, viscosity, etc., of the gas. Rather, if these parameters are unknown they will be determined with experimentation and causal reasoning all of which takes place outside of the context of Cartwright's construal of theory entry; hence, any gaps or missing details which arise here are not reflective of theory entry.

In order to determine whether theory entry consists of a gap-free series of deductions we should therefore start with a description which completely characterizes the molecules of the gas. This assumption enables us to focus on Cartwright's thesis, namely, that a theory provides few principles for converting such descriptions into the sorts of descriptions to which the theory matches an equation. But if the masses of the atoms, their interatomic forces, and so forth, are fully specified then, in principle, the relevant equations can be determined. One possible explanation of Cartwright's thesis is that she erroneously characterizes problems which arise outside of the context of her construal of theory entry as problems which arise because of the "mathematical needs of the theory". If a description of an object is sufficiently detailed, stated with the terms of the theory, then in principle the relevant equations can be determined. On the other hand, if these details are lacking, or if the description is not cast in the vocabulary of the theory, then we will certainly have problems in determining the relevant equations. But determining a sufficiently detailed description which is expressed with the concepts of the theory takes place outside of the context of Cartwright's construal of theory entry.

5 Conclusions

In summary, I have argued that the "ball and spring" example and the "sample of gas" example support different views of theory application. The first example was argued to fit Hempel's model of theory application since predictions about the ball's motion were deduced from a pretheoretical description of the ball and spring. Theory application left no room for choices which would enable practitioners of the theory to reach different predictions about the ball's motion. The second example supports a different view since the application of the principles of the kinetic theory did not imply a description of the gas which was sufficiently detailed to imply values for C_v and γ. Predictions of C_v and γ were only obtained through the additional use of conjectures about the molecular composition of the gas which were not implied by any of the theory's principles. Because individuals could advocate conflicting hypotheses about the molecualr composition of the gas while still remaining committed to the same theory, the kinetic theory was used to make a variety of conflicting predictions about the heat capacity of the gas.

The relation between Newtonian mechanics and what it predicts about the motion of the ball could therefore be specified with a Hempelian model. But the relation between the kinetic theory and its C_v content was not so easily specified. The theory was construed in terms of theoretical principles and root models which function as generative models. The root models generate a "fuzzy" class of models, and since these models typically lead to different values for C_v the C_v content of the theory was loosely characterized.

Similarly, these two examples were argued to support two different views of theory evaluation. If the ball's motion does not agree with the motion indicated in figure 1 then the theory is in trouble—the theory cannot be defended on the grounds that this is just one of many predictions about the ball's motion which can be implied with the theory. The application of the theory to the pretheoretical description left no room for such choices. But when Krönig used the kinetic theory to predict a value for γ of 1.67 when in fact γ is 1.5 it did not follow that one of the principles of the theory must be rejected or modified. Rather, the example only showed that Krönig failed to find the right way of applying the theory to the gas.

And finally, I argued that Cartwright misdiagnoses the problem with Hempel's construal of theory application. Cartwright's argument was described as resting on the assumption that a theory provides very few principles for converting a description into a description to which the "theory can match an equation". But I argued that if a description is sufficiently detailed and expressed in terms of the theory then in principle we can determine the relevant equations. Both of these conditions might not be satisfied, but because the place in which they might be satisfied lies outside of the context of Cartwright's construal of theory application, whether or not they are in fact satisfied has no bearing on her view of theory application.

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